No Payoff from Time Off? Mandated Paid Vacation and Late-Career Employment*

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Abstract

This study examines the effect of an additional week of paid vacation on labor market attachment among Norwegian workers at age 60. Employing a triple-differences estimation strategy, we exploit age-based eligibility thresholds before and after a 2009 reform to identify causal effects. Our findings indicate that the extra leave has negligible effects on both employment, sickness absence and disability benefit receipt in the year workers first receive it. If anything, some workers use the additional vacation time to increase earnings from secondary employers. The results imply that policymakers should consider alternative measures to mandated leave to support an aging workforce.

Keywords: Paid vacation; Older workers; Labor supply; Triple-differences; Public policy

JEL Classification: H8, I12, J22, J26

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1 Introduction

Most developed countries mandate a minimum amount of statutory paid leave to provide workers time off for extended rest and recovery (OECD, 2021). In some aging societies, governments have extended this principle and mandated extra paid vacation to older employees to sustain productivity and postpone retirement (Lester, 2010; Nagarajan and Sixsmith, 2023). Despite the costs of such policies, evidence on whether they deliver measurable benefits in the labor market is scarce.

This article examines whether granting extra paid vacation to 60-year-old employees in Norway influences their labor market behavior in that year. We exploit a 2009 reform that altered the timing of extra-vacation eligibility. Before the reform, workers born before September 1 received an additional week of vacation in the year they turned 60, while those born in September–December had to wait until the following year. After the reform, all workers received the right to the extra vacation week in the year they turned 60, regardless of birth month. This policy shift created a natural experiment: by comparing workers born at different times of the year who turn 60 before and after the reform, we can identify the one-year causal effect of the additional leave on labor market outcomes.

In theory, additional paid leave can influence outcomes through both the labor supply of affected workers and the labor demand of firms. On the supply side, workers who require more recovery time to remain productive may use the extra vacation to sustain their labor supply and potentially reduce their reliance on health-related social insurance benefits. They may also use the freed-up time to supplement their income, as there is no oversight preventing workers from taking secondary jobs while on vacation from their primary employer. Additional vacation may also incentivize some workers to seek employment rather than staying unemployed.

On the demand side, additional paid leave raises effective labor costs, since total compensation remains fixed while working hours decline. Standard economic theory predicts that firms will then re-optimize and substitute towards factor inputs whose price become relatively lower. Mandated benefits function in practice like a payroll tax, as discussed already in (Summers, 1989). We can therefore expect reduced labor demand and potentially increased involuntary unemployment if workers value the benefit (Gruber, 1994). We may also observe lower wages if wage setting is sufficiently flexible. Overall, we expect supply-side effects to push labor-market outcomes for older workers in a positive direction, whereas demand-side effects push them in the opposite direction. The net impact is theoretically ambiguous.

Using detailed register data that cover five complete cohorts of workers in Norway both before and after the reform, we find no evidence that extra vacation affects employment, sickness absence or disability benefit receipt. This is the case both in the year workers turn 60 and the year after. The results hold consistently across estimation methods, and generally do not vary among subgroups – even those for which we might have anticipated more pronounced responses. There is some indication that a group of workers use the freed-up time to increase their labor supply in secondary employment, consistent with economic models in which workers re-optimize their allocation between consumption and leisure following the provision of additional leave.

Empirical evidence on the influence of vacation policies is limited. Previous studies have established that vacations can lead to immediate improvements in well-being, but that these tend to fade out over the course of a few weeks (de Bloom et al., 2009; Kühnel and Sonnentag, 2011; de Bloom et al., 2011). Evidence on effects over a longer horizon and on labor market outcomes is scarce. A notable exception is Hofmarcher (2021), who analyzed additional paid vacation days for younger Swedish government employees and found no significant health effects. However, extra vacation may plausibly be much more important for workers who are older and work outside the government sector.

In the Norwegian context, Hermansen (2014) used survey data on firms' leave policies for older workers coupled with administrative data to estimate differential early retirement responses across firms. The study found that early retirement stayed roughly constant in firms that introduced extra vacation for 62 year-olds and increased in firms that did not introduce it. However, the firms that introduced extra vacation also increased other supportive policies in the same period, while the control firms decreased their number of such policies. The study is also not able to rule out that firms may have adopted extra vacation endogenously, e.g. through demands from older workers who knew that they were going to work longer. Midtsundstad et al. (2017) analyzed the same data and found that, after state employees in 2006-2008 got the right to at least eight additional vacation days from age 62, the retainment rate among state employees aged 61-66 increased relative to that of employees in the same age group in other sectors. Also in this case, other concurrent changes challenge a causal interpretation of the results. In particular, the share of organizations reporting that they have a 'policy for seniors' increased by around 40 percentage points in the public sector and less than 15 percentage points in the private sector (Midtsundstad, 2014). In contrast, we study a reform affecting extra vacation only, and that is without any scope for endogenous policy adoption by firms or reverse causality.

This paper relates more broadly to the growing literature on workplace amenities and labor supply (see, e.g., Akerlof et al., 1988; Cassar and Meier, 2018; Maestas et al., 2023; Sockin, 2022). Scholars have investigated the effects of various forms of non-pecuniary compensation, including flexible hours (Chen et al., 2019; He et al., 2021; Mas and Pallais, 2017; Wiswall and Zafar, 2017), the option to work from home (Angelici and Profeta, 2024; Bick et al., 2023; Bloom et al., 2024, 2015; Emanuel and Harrington, 2024), and autonomy at work (Saragih, 2011). The empirical findings are mixed; for example, field experiments by Chen et al. (2019) and He et al. (2021) find that workers value flexible hours, while Mas and Pallais (2017) reach the opposite conclusion. Of particular relevance to our study are early surveys by Best (1978) and Nealey and Goodale (1967) suggesting that workers are willing to trade higher pay for additional paid vacation days. This view frames vacation primarily as a worker-valued benefit rooted in a preference for leisure. Instead, the policy we evaluate in this paper was introduced under the premise that leisure is necessary for senior workers to maintain their ability to work. This perspective aligns with studies like Filer and

Petri (1988), Hayward et al. (1989) and McLaughlin and Neumark (2018), which find that physically demanding jobs are associated with early retirement. Our results suggest that, at least in this context, this rationale does not hold.

2 Norwegian Vacation Policy

Norway mandates a statutory paid annual leave of 21 working days, aligning with the median among OECD countries when combined with public holidays (OECD, 2021). In 1976, the country introduced an additional week of paid vacation for employees over the age of 60. The act was aimed at reducing the workload for senior employees to maintain their work ability in their final years of employment.[1] Initially, funding was provided by the Norwegian social insurance scheme, but the responsibility was transferred to employers in 1988. Back-of-the-envelope calculations indicate that firms today bear an annual cost of about EUR 62 million (NOK 720 million) to cover the week for 60-year-olds alone.[2]

Until 2009, the first year an individual could take out the extra week depended on the their date of birth: those born before September 1 received it in the year they turned 60, whereas those born on or after September 1 became eligible only in the following year. The resulting stagger in within-cohort eligibility is illustrated by the solid red and black lines in Figure 1. In 2009, the criterion was expanded to include all employees who turned 60 at any point during the calendar year. Eligibility for this group is illustrated by the gray dashed line in Figure 1. These regulations are stipulated in the *Act Relating to Holidays* and apply to all employees except those in shipping and fishing industries.[3] Self-employed individuals

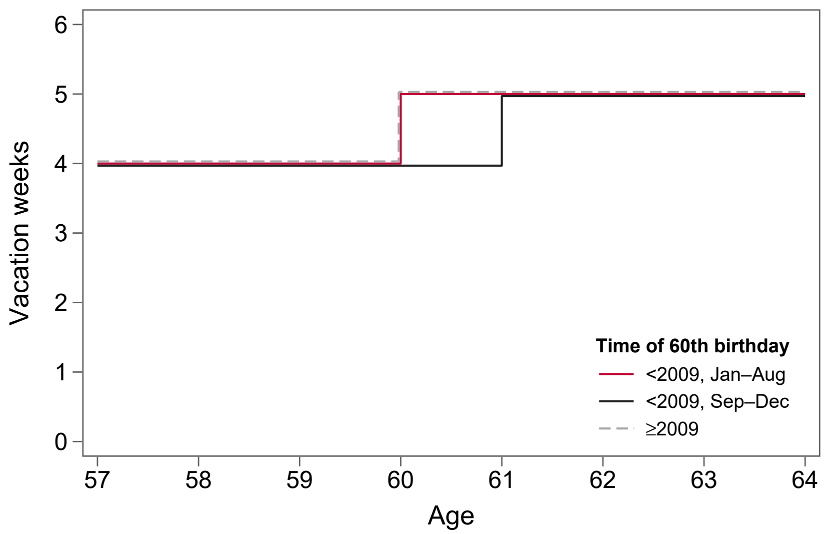
[1]A 1975 feasibility study references other reports, stating: 'Phasing down working hours for people over a certain age can likely be very beneficial for maintaining the work capacity and motivation of older adults'. It also quotes: 'This is because older people need more time to recover than younger ones.' (Halvorsen, 1975)

[2]This estimate is based on: $W = \sum_{ik} w_{ik}(1 + \tau_k) \cdot \frac{5}{230} \cdot 0.75$, where w_{ik} denotes the total annual wage earnings of 60-year old employee i in region k, τ_k is the employers' National Insurance contribution rate (which varies by region), $\frac{5}{230}$ accounts for one additional week of vacation relative to a 230-day work year, and the multiplier 0.75 conservatively adjusts for the possibility that not all eligible workers take the leave. Earnings are measured in 2022 and deflated to 2025 prices using the Norwegian Basic Amount, which is adjusted each year roughly in accordance with annual wage growth.

[3]The September 1 cut-off was often criticized as unfair, for example in the national daily Dagsavisen

are not covered by this act. In the public sector, in some firms and for members of some unions, there are additional days or weeks of paid leave. Importantly, this additional paid leave does not vary systematically with the policy change we leverage for identification. Yet, the total amount of available paid leave we use in the analysis is a lower bound.

Figure 1: Illustration of the 2009 Vacation Reform



3 Methodology

3.1 Data Sources and Sample Selection

All inhabitants in Norway receive a national identification number upon birth or immigration. These identification numbers are used in all administrative registries, allowing us to collect data on all individuals who turned 60 in the years surrounding the reform. We combine data from several of these registries, including birth and death date (at the month-year level), education level, family links, immigration status, earnings, employment, sick leave

(2005) and labor movement online newspaper FriFagbevegelse (2008). The criticism was later repeated by labor organizations LO, YS, Unio, Akademikerne, Nito and the Norwegian Union of Journalists, who argued for equal treatment, while employer circles opposed the change (otp, 2008). The government's proposal was adopted in autumn 2008 and entered into force on 1 January 2009. It was widely announced in the national press, circulars, and industry journals.

and disability benefit receipt. Our primary source of labor market data is the employer-employee registry. This registry contains information on job spells, including start and stop dates, wage earnings, occupation, sector and contracted hours. Information on sick leave and benefits include start and stop dates and degree if relevant.

We do not observe actual vacation take-up in our data. As a result, our estimates should be interpreted as intention-to-treat effects of eligibility for the additional vacation week (a reduced-form effect). However, the fact that Norwegian law stipulates that employees utilize all their vacation days suggests a high rate of compliance. Using information from the Norwegian employer-employee register available from 2015 onward, Appendix Figure A.1 shows that individuals receive about 16% more holiday pay from the year they turn 60, consistent with widespread take-up of the extra week.[4] Survey evidence is also consistent with substantial use. In 2015, 75% of employees aged 60–61 reported using the extra week of vacation, and more than 50% stated that it provided some or strong motivation to postpone retirement (Svalund and Veland, 2016). Taken together, these pieces of evidence suggest that non-take-up is likely to be small. If we accept a take-up rate of 75%, the reduced-form estimates can be scaled by 1/0.75, or approximately, 1.33, to obtain the average treatment effect of taking the extra week.

Our sample consists of ten complete cohorts of Norwegian employees who turned 60 years old between 2004 and 2013, encompassing five years before and five years after the 2009 vacation reform. We restrict data to individuals who were employed and earned a wage earnings of at least one 'basic amount' in the year they turned 59.[5] This group comprises about two-thirds of the population. We then narrow our focus to a symmetric window around the vacation eligibility threshold of September 1st, and exclude anyone born before May 1st from the sample. We also condition on individuals being registered inhabitants in

[4]In Norway, holiday pay replaces regular wages during vacation and is accrued as a fixed percentage (10.2% at a minimum) of the previous year's wage earnings. Individuals aged 60 or older who take the additional week of vacation are entitled to a 2.3 percentage points higher accrual rate.

[5]The Basic Amount is a part of the Norwegian National Insurance Scheme, and is adjusted each year in line with the average wage growth. In 2025, the Basic Amount was equal to NOK 130,160, or approximately EUR 11,000.

their years of observation.

If the policy has the intended effect, we expect that workers respond along two margins. The first is the extensive labor supply margin – workers who are eligible should be less likely to exit the labor market. Aged-based pension is not available for workers aged 60. Exiting the labor market may therefore involve a shift from labor market earnings to one or more types of benefits. If the exit is due to health related reasons, this would be disability benefits. We therefore define indicators for employment and disability insurance receipt as two of our main outcomes.

The other margin is the degree of attachment to the labor market. Extra vacation may make workers less likely to cut back on work, which we measure using earnings.[6] They may also use the freed-up time to supplement their income, as the Norwegian Act Relating to Holidays does not regulate *how* workers spend their vacation. We additionally examine sickness absence to assess whether extra vacation provides work-related health benefits. If the policy is effective, it should reduce the number of sick-leave days taken by eligible workers, indicating a better balance between work and recovery time.

Any effects on labor market attachment will include potential demand side responses from firms that are not separable from worker responses. For instance, while some workers may be less inclined to leave the workforce when granted additional vacation, firms at the same time face incentives to substitute away from older, more expensive, workers. We will only detect the net effect of these counteracting forces, though Norway's strong employment protection rules will likely limit the impact on the demand side.

3.2 Descriptive Statistics

Table 1 reports descriptive statistics for the five cohorts in our estimation sample who turn 60 in the years preceding the reform. Outcomes are shown separately for individuals born between May–August and September–December, measured one year before the difference in

[6] Data on contracted hours are of poor quality until 2015.

vacation eligibility takes effect.

Table 1: Descriptive Statistics at Age 59, 2004–2008 Cohorts

	Born May–Aug		Born Sep–Dec	
	Mean	SD	Mean	SD
Female (%)	47.5	49.9	47.7	49.9
Has children (%)	90.5	29.3	90.4	29.4
Higher education (%)	30.5	46.1	29.8	45.7
Married (%)	73.6	44.1	73.4	44.2
Divorced (%)	14.8	35.5	14.8	35.5
Immigrant (%)	3.9	19.3	4.1	19.8
Public sector (%)	41.5	49.3	40.7	49.1
Earnings (EUR 1,000s)	48.3	25.1	48.2	24.9
Any sickness (%)	42.3	49.4	41.6	49.3
Sickness absence days	27.3	59.6	26.7	59.0
DI receipt (%)	10.9	31.1	10.4	30.5
DI amount (EUR 1,000s)	1.4	4.7	1.4	4.6
N	63,088		57,719	

The table reports summary statistics for all individuals turning 60 in the years 2004–2008. Outcomes are measured in the year they turn 59.

Given their age and the inclusion criteria, our sample consists of relatively high earners who also exhibit relatively high rates of sickness absence and disability insurance receipt (including partial or temporary benefits). For example, they have about 40% more absence days than is typically observed for physician-certified sick leave in the overall Norwegian labor force. Comparing individuals born earlier and later in the year suggests that they are largely similar, though slight differences are present in educational attainment, sickness absence, and disability insurance receipt. On average, individuals in the two groups differ in age by about four months. We return to the implications of this difference for our identification strategy below.

Corresponding statistics for the five cohorts turning 60 after the reform are reported in Appendix Table A.1. The patterns are broadly similar to those observed in Table 1. In addition, Appendix Figure A.2 shows stable trends within cohorts over time, with differences emerging primarily across cohorts.[7]

3.3 Research Design

Our setting allow us to identify the effect of additional vacation using two sources of variation. The first comes from the fact that within each cohort that turned 60 before 2009, access to the extra vacation week was determined by birth month. A simple estimation strategy exploiting this source of variation, could therefore be to compare individuals born early and late in the same calendar year. An event study specification using this approach takes the form:

$$\begin{aligned} Y_{ia} = \alpha_1 + \sum_{s \neq 59} \alpha_{2,s} \mathbb{1}[a = s] + \alpha_3 \mathbb{1}[Late]_i \\ + \sum_{s \neq 59} \alpha_{4,s} \mathbb{1}[a = s] \cdot \mathbb{1}[Late]_i + \alpha_{5,t(ia)} + \varepsilon_{ia}, \end{aligned} \tag{1}$$

where Y_{ia} is outcome Y for individual i at age a, $\mathbb{1}[a = s]$ is an indicator equal to one when individual i is age s, with s indexing ages (in years) from 52 to 61, and $\mathbb{1}[Late]_i$ equals 1 for individuals born between September 1st and December 31st in any year. $\alpha_{5,t(ia)}$ denotes calendar-year fixed effects. $\alpha_{4,60}$ is the coefficient of interest. In order for this estimand to have a causal interpretation, we need that individuals born early and late in the year would have had similar outcomes in the absence of the extra vacation week. This assumption may hold close to the threshold, however, since our data only allows us to observe individuals' month of birth – not the day – we are unable to exploit the discontinuity fully.

A second source of identifying variation comes from the difference in access to extra vacation for those born late in the year before and after the 2009 reform. We could exploit

[7] The cohort differences in levels reflect broad time trends. With regards to the higher earnings of the later cohorts, there was substantial real wage growth in the study period (Statistics Norway, 2025). With regards to the lower sickness absence levels of the later cohorts, Andersen and Hernæs (2025) document strong reductions in sickness-benefits receipt 2004-2019 among elderly employees in particular and emphasize the role of education, occupation and health trends.

this by estimating an event study specification comparing the evolution of outcomes for individuals born on or after September 1st in older cohorts, who did not have access to the extra vacation the year they turned 60, to those born on or after September 1st in younger cohorts, who did have access to the extra vacation. In econometric terms:

$$
\begin{aligned}
Y_{ia} = \beta_1 &+ \sum_{s \neq 59} \beta_{2,s} \mathbb{1}[a = s] + \beta_3 \mathbb{1}[Reform]_i \\
&+ \sum_{s \neq 59} \beta_{4,s} \mathbb{1}[a = s] \cdot \mathbb{1}[Reform]_i + \beta_{5,t(ia)} + \eta_{ia},
\end{aligned}
\tag{2}
$$

where $\mathbb{1}[Reform]_i$ is equal to 1 for individuals who turn 60 in the years after the 2009 reform, i.e., those born in 1949 or later, and the rest is as defined for Equation 1. Observe that unlike the previous specification, identification of Equation 2 requires using persons both from the pre- and post-reform cohorts, but only those born late in the year. In order for $\beta_{4,60}$ to give causal estimates, we need that individuals of different cohorts would have had the same evolution of outcomes from age 59 in the absence of the extra vacation. This may not be a credible assumption; for example, we know that younger cohorts are of better health. Thus, by itself this source of variation is not suitable to estimate the effect of the additional vacation week.[8]

In sum, we consider both difference-in-differences methods to have drawbacks, as they rely on strong assumptions about potential outcomes. Identification in the first specification is threatened by the age difference for individuals born in different months of the year. Identification in the second specification is threatened by trends in outcomes across cohorts. We therefore combine these two approaches to circumvent both threats, and estimate a triple difference estimator of the form:

[8] It is possible to include those born before September 1st in Equation 2 to net out cohort effects, but we omit them in this discussion to emphasize the source of variation.

$$
\begin{aligned}
Y_{ia} = \; & \gamma_1 + \sum_{s \neq 59} \gamma_{2,s} \mathbb{1}[a = s] + \gamma_3 \mathbb{1}[Late]_i \\
& + \sum_{s \neq 59} \gamma_{4,s} \mathbb{1}[a = s] \cdot \mathbb{1}[Late]_i + \gamma_5 \mathbb{1}[Reform]_i \\
& + \sum_{s \neq 59} \gamma_{6,s} \mathbb{1}[a = s] \cdot \mathbb{1}[Reform]_i + \gamma_7 \mathbb{1}[Late]_i \cdot \mathbb{1}[Reform]_i \\
& + \sum_{s \neq 59} \gamma_{8,s} \mathbb{1}[a = s] \cdot \mathbb{1}[Late]_i \cdot \mathbb{1}[Reform]_i + \gamma_{9,t(ia)} + \nu_{ia}.
\end{aligned} \tag{3}
$$

The causal effect of an additional week of vacation at age 60 is given by $\gamma_{8,60}$ which identifies the differential effect for those who turned 60 after the reform and were born late in the year. This estimator nets out effects of an increasingly healthy population as well as differences within cohorts across birth months. In other words, the specification allows for differential outcomes across ages for those born early and late in the year when these are common across cohorts, as well as a separate effect for cohorts by age as long as these are constant across cohorts.

We estimate Equation 3 for six outcomes meant to capture labor market attachment, grouped into three broad categories. The first category is employment, measured extensively by an indicator for having annual wage earnings of at least 1 'basic amount', approximately EUR 11,000, and intensively by total annual wage earnings in EUR 1,000s (top-coded at the 99.5th percentile). The second category is sickness absence, measured extensively as an indicator for having any doctor-certified sickness absence during the year and intensively as the effective number of sickness absence days per year (0–365), where partial absences are converted to full-day equivalents. The third category is disability insurance receipt, measured extensively as an indicator for receiving any DI benefits, including both temporary and permanent benefits, and intensively as the total amount of DI benefits received in EUR 1,000s.[9] Individuals are included in the sample from the year they turn 52 until the year they

[9]Disability insurance receipt refers to benefits recorded in the annual wage and tax statements (LTO) under the following codes: 218 disability pension from the National Insurance Scheme, 221 provisional

turn 61.[10] This range allows us to observe sufficient pre-trends to validate the identifying assumptions and accommodates the possibility that effects might not materialize in the first year.[11] Standard errors are clustered at the individual level to allow for arbitrary correlation within individuals over time.

4 Results

4.1 Main Results

Panel A of Table 2 shows the age-60 effect of receiving the extra week of paid vacation on our six main outcomes.[12] We report only the coefficient of interest, $\gamma_{8,60}$; the expanded Appendix Table A.3 reports $\gamma_{2,60}$ through $\gamma_{8,60}$. Romano and Wolf (2005a,b) step-down adjusted p-values, computed using 1000 bootstrap replications over the full set of reported coefficients, are shown in brackets. For the extensive-margin outcomes in columns (1), (3), and (5), the point estimates are all close to zero, but the precision is insufficient to draw firm conclusions about the policy's effectiveness. For instance, in column (1), the 95% confidence interval for employment ranges from –0.24 to 0.30 percentage points. Since about 3% of those employed at age 59 are unemployed at age 60, this implies that we can rule out effects larger than a 10% reduction in the probability of exiting the workforce, but not smaller ones.

For the intensive-margin outcomes in columns (2), (4), and (6), however, the results allow for a clearer interpretation. In column (2), the point estimate of 0.13 implies a modest annual income gain of about EUR 135, compared to the average weekly wage in the sample

disability pension from the National Insurance Scheme, 222 rehabilitation benefits from the National Insurance Scheme, 233 time-limited disability pension from the National Insurance Scheme, 239 work assessment allowance (AAP), and 247 disability pension outside the National Insurance Scheme.

[10] Our data on sickness absence spells do not cover the years prior to 2001. This implies that the earliest age for which we can estimate $\gamma_{8,s}$ in Equation 3 for these outcomes is 53.

[11] Any identifiable effects are likely to be short lived given that individuals not granted the additional week at age 60 did receive it starting at age 61. Extending the timeline further into the future is also not feasible due to Norway's 2011 pension reform, which comprehensively altered retirement and labor market participation incentives.

[12] For completeness, we also report estimates obtained using the two estimation strategies specified by Equations 1 and 2 in Appendix Table A.2.

of EUR 1,062. The 95% confidence interval extends up to EUR 287. While this could suggest an adjustment in working hours in response to receiving the week, we show below that an alternative explanation is more likely. In column (4), the estimate on sickness absence days shows that we can rule out a reduction of more than 1.33 days, about 6% of the mean. Adjusting for the typical Norwegian full-time work year (230 out of 365 days) yields a lower bound of 0.84 workdays per year. In column (6), the confidence interval for the amount of DI receipt extends down to EUR –84. One way to interpret the magnitude of this effect is by comparing it to the cost of the vacation week, estimated at EUR 62 million for 60-year-olds per year. Multiplying the lower bound by the number of 60-year-olds in Norway (69,826 in 2025) yields a best-case saving in DI payments of EUR 5.9 million, less than 10% of the total cost.

4.2 Mechanisms

Despite the overall null results, the slight increase in earnings may suggest that some workers respond to the mandatory paid leave by increasing their intensive-margin labor supply.

There are three possible channels through which an increase in earnings could materialize. First, even though overall employment is unchanged, it is possible that the increase in earnings is driven by job switches among workers. We test this hypothesis by re-estimating our main model with a dummy for changing the main employer as the outcome. The result, reported in column (1) of Panel B in Table 2, shows no effect. Second, extra vacation could enable some workers to maintain their working hours relative to not getting the leave. Since we do not have a good measure of actual hours worked, we measure this by earnings obtained with an individual's primary employer. Column (2) of Panel B shows no evidence that the effect operates through primary earnings.

Finally, although employers are legally required to ensure that employees take their leave, some workers may choose to use the additional time for work elsewhere. Column (3) shows no change in the likelihood of holding multiple jobs, thus affected workers do not seem to

Table 2: Effects of Extra Paid Vacation

A: Main outcomes	(1) Employed	(2) Earnings	(3) Sickness	(4) Sick days	(5) DI receipt	(6) DI amount
Age = 60 × Late × Reform	0.0003	0.1348*	-0.0035	-0.2182	-0.0018	-0.0267
	(0.0014)	(0.0786)	(0.0045)	(0.5686)	(0.0015)	(0.0291)
	[0.99]	[0.54]	[0.95]	[0.99]	[0.85]	[0.93]
Observations	2,468,131	2,468,131	2,113,686	2,113,686	2,468,131	2,468,131
Individuals	247,966	247,966	247,966	247,966	247,966	247,966
Mean DV	0.97	48.84	0.39	21.44	0.09	1.23

B: Additional outcomes	(1) New main employer	(2) Primary earnings	(3) Multiple employers	(4) Suppl. earnings
Age = 60 × Late × Reform	-0.0016	0.0416	-0.0006	0.0993*
	(0.0028)	(0.1329)	(0.0032)	(0.0596)
	[0.99]	[0.99]	[0.99]	[0.55]
Observations	2,222,196	2,468,131	2,468,131	2,468,131
Individuals	247,966	247,966	247,966	247,966
Mean DV	0.09	42.99	0.19	2.63

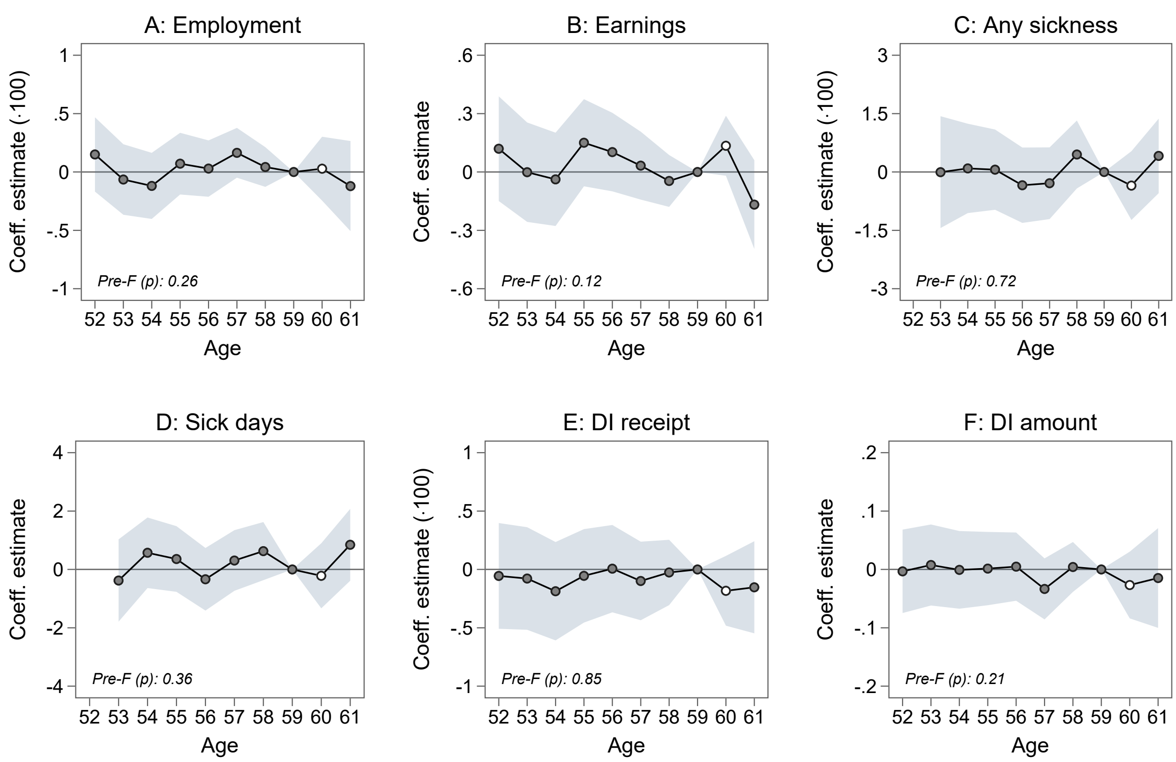
Notes: *Each column represents a separate regression of Equation 3. Y is as denoted by the column headers. Panel A considers our six main outcomes, while Panel B considers additional outcomes. Only the three-way interaction effects at age 60 are reported. In Panel A, there are fewer observations in columns (3) and (4) because sickness data is only available from 2001. There are also fewer observations in the first column of Panel B because the initial period is lost when taking the difference. Standard errors clustered at the individual level are reported in parentheses. Romano-Wolf step-down adjusted p-values, computed using 1000 bootstrap replications over the full set of reported coefficients, are shown in brackets. Significance stars refer to conventional (unadjusted) p-values: *** p<0.01, ** p<0.05, * p<0.1.*

be induced to seek out new employment. At the same time, many already have multiple jobs. Column (4) shows a positive effect on total supplementary earnings, defined as the sum of annual wage earnings from all but the highest-ranked (by wages) employer. The positive point estimate of approximately EUR 100 is numerically similar to the main earnings estimate in Panel A. In sum, our results provide some indication that employees for whom increasing labor supply is relatively easy, use the extra leave to generate additional income rather than rest. Appendix B presents a simple model of individuals' labor supply that rationalizes this behavior.

4.3 Dynamic responses

Figure 2 plots the full set of triple-differences estimates for the six main outcomes across ages 52–61. This presentation allows us to assess both the validity of the parallel trends assumption and the possibility of effects not emerging until the following year. Estimates of $\gamma_{8,60}$ (reported in Table 2) are highlighted by white markers. We first note that trends are statistically indistinguishable from zero across all outcomes in the pre-treatment periods. This supports our identifying assumption. There is also no evidence of delayed effects, with even the marginally statistically significant (with conventional p-values) effect on income observed at age 60 eliminated at age 61. Similar results are shown in Appendix Figure A.3 for the four additional outcomes considered in Panel B of Table 2.

Figure 2: Effects of Extra Paid Vacation, Event Study



Notes: *Each panel presents coefficient estimates of* $\gamma_{8,s}$ *in Equation 3 for individuals aged 52 to 61. Age 59 serves as the reference category. White-filled coefficients denote* $\gamma_{8,60}$*. Shaded areas represent 95% confidence intervals. Standard errors are clustered at the individual level. The p-value from an F-test of joint significance for the pre-period (age* $<$ *59) is reported in the lower-left corner of each plot.*

4.4 Robustness Checks

We perform several robustness checks. First, estimates obtained from regressions that incorporate individual-level fixed effects are reported in Appendix Table A.4 and Appendix Figure A.4. This does not affect the results. Second, Appendix Figure A.5 reports cohort-specific estimates for the post-reform period to assess whether low compliance immediately after the reform could have influenced our results. We find no evidence of effects emerging in later cohorts. Finally, Appendix Figures A.6 and A.7 present results from a difference-in-discontinuities-type approach, comparing outcomes across the eligibility threshold at age 60 directly rather than tracking them over time. This estimation strategy relies on a stronger assumption of balance between the treatment and control groups, but here too there is no evidence of any effects.

4.5 Heterogeneous Effects

Even if the overall effects of the extra paid vacation on labor market outcomes are negligible, there may still be individual differences that are masked in the full sample. For example, it is conceivable that individuals in physically demanding labor categories benefit more from the policy than others. Table 3 presents coefficient estimates of $\gamma_{8,60}$ in Equation 3 by occupation category. The four categories broadly correspond to ISCO 1–3 ('high-skilled white-collar'), ISCO 4–5 ('low-skilled white-collar'), ISCO 6–7 ('high-skilled blue-collar'), and ISCO 8–9 ('low-skilled blue-collar'), respectively. Each cell represents a separate linear regression, conducted on the subsample of employees belonging in the respective group at age 59. A few individuals with missing or military occupations are omitted (<1% of the sample).

The results reveal no clear occupational patterns. Using conventional p-values, the only significant coefficient appears for earnings in column (2) for low-skilled white-collar occupations, but the magnitude of about 180 EUR is comparatively similar to (and not significantly different from) the other groups. After applying the Romano-Wolf correction, none of the coefficients are statistically significant. The bottom row reports p-values from F-tests of

Table 3: Effects of Extra Paid Vacation by Occupation Type

	(1) Employed	(2) Earnings	(3) Sickness	(4) Sickdays	(5) DI receipt	(6) DI amount
High-skilled white-collar (n = 123,240)	0.0004	0.1052	-0.0005	0.7044	-0.0022	-0.0363
	(0.0016)	(0.1290)	(0.0063)	(0.6864)	(0.0019)	(0.0389)
	[0.98]	[0.80]	[0.98]	[0.80]	[0.72]	[0.80]
Low-skilled white-collar (n = 73,006)	0.0009	0.1793*	-0.0039	-0.6481	-0.0005	-0.0314
	(0.0029)	(0.1001)	(0.0085)	(1.1525)	(0.0030)	(0.0513)
	[0.97]	[0.29]	[0.97]	[0.97]	[0.97]	[0.97]
High-skilled blue-collar (n = 20,160)	0.0038	0.1709	0.0017	0.0182	0.0002	0.1069
	(0.0056)	(0.2740)	(0.0163)	(2.2221)	(0.0061)	(0.1255)
	[0.95]	[0.95]	[1.00]	[1.00]	[1.00]	[0.92]
Low-skilled blue-collar (n = 31,465)	-0.0037	0.1364	-0.0175	-2.6244	-0.0048	-0.0746
	(0.0050)	(0.2069)	(0.0130)	(1.9173)	(0.0052)	(0.0949)
	[0.77]	[0.77]	[0.63]	[0.63]	[0.77]	[0.77]
Equal effects (p-val.):	0.78	0.97	0.68	0.36	0.88	0.70

Notes: *Each cell represents a separate regression of Equation 3, conducted on different subsamples defined by occupation category at age 59. Y is as denoted by the column headers. Only the three-way interaction effects at age 60 –* $\gamma_{8,60}$ *in Equation 3 – are reported. P-values from F-tests of equal effects are provided in the bottom row. The occupation categories are defined according to the Norwegian STYRK-98 classification, which correspond to ISCO 1–3 ('high-skilled white-collar'), ISCO 4–5 ('low-skilled white-collar'), ISCO 6–7 ('high-skilled blue-collar'), and ISCO 8–9 ('low-skilled blue-collar'), respectively. Standard errors clustered at the individual level are reported in parentheses. Romano-Wolf step-down adjusted p-values, computed using 1000 bootstrap replications over the set of coefficients in each row, are reported in brackets. Significance stars refer to conventional (unadjusted) p-values: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.*

equal effects for each of the six outcomes, obtained from separate saturated regressions with the occupation groups interacted. We fail to reject the null in all cases.

Appendix Table A.5 additionally examines heterogeneity by income, gender, and sector of employment. There is no evidence that the estimates differ significantly across these categories, although the results suggest that the marginal earnings effect discussed earlier is driven primarily by low-income men in the private sector.

5 Discussion

The effects of mandated benefits on labor market equilibria has been a heavily discussed topic in economics at least since Summers (1989) influential article. However, most empirical studies have concerned unemployment and health insurances and benefits excluding mandated vacation which is the topic of this paper. Similar to other mandated benefits, additional mandated paid leave for a subset of the workforce has ambiguous effects ex ante, as incentives for workers and firms may induce opposite effects on employment. From a theoretical perspective, additional paid mandated vacation will have the same effects as a payroll tax on firms and raise the cost of labor. For workers we can expect much of the same effect as from an increased payroll tax, but increased mandatory leave also makes work relatively more attractive compared to unemployment which may raise labor supply,

Our study estimates causal effects of additional vacation at a point in the life cycle when the value of leave may be particularly salient. Understanding these effects is important for informing policies aimed at promoting cost-effective and sustainable employment and well-being among older workers. By shedding light on the implications of mandated vacation policies, our findings can inform policy discussions aimed at promoting sustainable and efficient labor market outcomes.

In Norway, workers aged 60 and above have received one additional week of paid vacation since 1976. Our analysis of the causal effect of this policy shows no significant impact on workers' labor market attachment in the first year of eligibility. On the extensive margin outcomes employment, sickness absence, and disability benefit receipt, the point estimates are close to zero. Although the confidence intervals rule out large effects (e.g. more than a 10% reduction in workforce exit at age 60), smaller effects cannot be excluded. The intensive margin outcomes provide a more informative picture: sickness absence reductions are limited to no more than 1.3 days per year (0.8 workdays after adjustment) while for disability benefits, the 95% confidence interval extends down to EUR –84 per year. We arguably find some evidence of increased earnings, which we trace to greater intensive-margin labor supply among workers with secondary employment. It therefore appears that some workers use the additional paid leave as a work subsidy to increase supplemental earnings. The cost of the extra vacation is borne by employers, and back-of-the-envelope calculations suggest that this amounts to approximately EUR 62 million per year (720 million NOK) for 60-year-olds alone. In light of the policy's stated aim of ensuring sufficient rest, this substantial cost with little return casts doubt on its effectiveness.

While our identification strategy only allows us to estimate effects in a one-year time frame, it seems unlikely that any cumulative effects from an additional week of paid leave at age 60 should materialize at age 62 when not present either at ages 60 or 61. We also find no substantial results for particular sub-groups. Our results align with Hofmarcher (2021), who found negligible effects of a similar policy among younger government employees in Sweden.

Norway's generous sick-leave scheme could attenuate the marginal 'recovery-time' value of an extra vacation week. This channel has a direct empirical implication: if the extra vacation week primarily substitutes for recovery that would otherwise be taken as sick leave, we should observe a reduction in sickness absence in the treated year; we do not. It should also be mentioned that generous paid sick leave is not unique to Norway. Across the OECD, most countries provide long paid leave for personal illness and a majority have replacement rates of 80 percent or more (Raub et al., 2018; Lee et al., 2025). Nonetheless, sick-leave generosity varies meaningfully across countries, so the effects of mandated senior vacation could plausibly differ in settings with shorter duration and/or lower replacement.

A key limitation of our study is the focus on effects in the calendar year individuals turn 60. Potential effects that materialize over many years, such as delayed retirement or cumulative health benefits, are not captured in our design. Future research in other settings could explore longer time horizons to assess the sustained impact of additional paid vacation.

Our results therefore do not rule out meaningful effects of a permanent increase in vacation entitlements over multiple years, nor does it imply that senior vacation policies are ineffective in general. Nevertheless, the absence of short-run effects at age 60 raises the policy-relevant question of whether the current age threshold targets the margin where additional recovery time is most valuable. As life expectancy has risen and health has improved, age thresholds in pension systems have generally been adjusted upwards. A similar recalibration of the age cutoff for extra vacation could help ensure that benefits reach those who need them most, while limiting costly distortions. Aligning the threshold with improvements in longevity and health would preserve the policy's original intent of supporting older workers' well-being, without inadvertently subsidizing those who have not yet experienced substantial declines in work capacity. Policymakers should also consider whether alternative measures are better suited to support an aging workforce than mandated vacation.

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A Additional Tables and Figures

Table A.1: Descriptive Statistics at Age 59, 2009–2013 Cohorts

	Born May–Aug		Born Sep–Dec	
	Mean	SD	Mean	SD
Female (%)	48.7	50.0	48.4	50.0
Has children (%)	89.5	30.7	89.5	30.6
Higher education (%)	34.3	47.5	33.3	47.1
Married (%)	69.9	45.9	69.4	46.1
Divorced (%)	16.7	37.3	17.2	37.7
Immigrant (%)	5.0	21.8	5.3	22.4
Public sector (%)	43.7	49.6	42.5	49.4
Earnings (EUR 1,000s)	49.7	26.1	50.0	26.3
Any sickness (%)	40.3	49.1	40.1	49.0
Sickness absence days	25.0	56.4	24.7	55.9
DI receipt (%)	10.9	31.1	10.7	30.9
DI amount (EUR 1,000s)	1.4	4.8	1.4	4.7
N	66,746		60,413	

The table reports summary statistics for all individuals turning 60 in the years 2009–2013. Outcomes are measured in the year they turn 59.

Table A.2: Diff-in-Diff Estimates of Effects of Extra Paid Vacation

A: Pre-reform cohorts	(1) Employed	(2) Earnings	(3) Sickness	(4) Sick days	(5) DI receipt	(6) DI amount
Age = 60	-0.0361***	-1.6689***	-0.0054**	0.5617*	0.0250***	0.6878***
	(0.0008)	(0.0659)	(0.0024)	(0.3009)	(0.0010)	(0.0191)
Late	-0.0000	-0.0586	-0.0070**	-0.6746**	-0.0043**	-0.0724***
	(0.0000)	(0.1439)	(0.0028)	(0.3413)	(0.0018)	(0.0269)
Age = 60 × Late	-0.0005	-0.0399	0.0010	0.1015	-0.0003	-0.0096
	(0.0010)	(0.0579)	(0.0033)	(0.4189)	(0.0011)	(0.0220)
Observations	1,203,473	1,203,473	849,028	849,028	1,203,473	1,203,473
Individuals	120,807	120,807	120,807	120,807	120,807	120,807
Mean DV	0.97	48.36	0.40	23.03	0.08	1.16

B: Born late in the year	(1) Employed	(2) Earnings	(3) Sickness	(4) Sick days	(5) DI receipt	(6) DI amount
Age = 60	-0.0365***	-1.9444***	-0.0033	0.5865*	0.0242***	0.6578***
	(0.0008)	(0.0673)	(0.0025)	(0.3093)	(0.0010)	(0.0186)
Reform	-0.0111***	-0.6095*	-0.0010	-0.7462	-0.0030	0.0223
	(0.0011)	(0.3242)	(0.0046)	(0.4749)	(0.0035)	(0.0538)
Age = 60 × Reform	0.0068***	0.3670***	-0.0012	-0.3673	-0.0048***	-0.1513***
	(0.0011)	(0.0815)	(0.0035)	(0.4284)	(0.0014)	(0.0258)
Observations	1,175,799	1,175,799	1,005,682	1,005,682	1,175,799	1,175,799
Individuals	118,132	118,132	118,132	118,132	118,132	118,132
Mean DV	0.97	48.86	0.39	21.25	0.09	1.21

Notes: *Each column represents a separate regression of Equations 1 (Panel A) and 2 (Panel B). Y is as denoted by the column headers. In Panel A, only those turning 60 during 2004–2008 are included in the sample. In Panel B, only those born on or after September 1st (regardless of year) are included. Standard errors clustered at the individual level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.*

Table A.3: Effects of Extra Paid Vacation (Expanded Table)

	(1) Employed	(2) Earnings	(3) Sickness	(4) Sick days	(5) DI receipt	(6) DI Amount
Late	0.0000	-0.0526	-0.0070**	-0.6698**	-0.0043**	-0.0723***
	(0.0000)	(0.1439)	(0.0028)	(0.3413)	(0.0018)	(0.0269)
Reform	-0.0113***	-0.9627***	-0.0047	-1.0033**	-0.0039	0.0004
	(0.0008)	(0.2411)	(0.0037)	(0.3970)	(0.0027)	(0.0417)
Age = 60	-0.0360***	-1.8816***	-0.0042*	0.4999*	0.0246***	0.6650***
	(0.0007)	(0.0545)	(0.0023)	(0.2933)	(0.0009)	(0.0167)
Late × Reform	-0.0000	0.2949	0.0044	0.3266	0.0027	0.0338
	(0.0000)	(0.2057)	(0.0040)	(0.4646)	(0.0025)	(0.0378)
Age = 60 × Late	-0.0006	-0.0412	0.0010	0.0970	-0.0003	-0.0091
	(0.0010)	(0.0579)	(0.0033)	(0.4189)	(0.0011)	(0.0220)
Age = 60 × Reform	0.0068***	0.1562**	0.0030	-0.0771	-0.0024*	-0.1114***
	(0.0010)	(0.0678)	(0.0032)	(0.4032)	(0.0012)	(0.0228)
Age = 60 × Late × Reform	0.0003	0.1348*	-0.0035	-0.2182	-0.0018	-0.0267
	(0.0014)	(0.0786)	(0.0045)	(0.5686)	(0.0015)	(0.0291)
Observations	2,468,131	2,468,131	2,113,686	2,113,686	2,468,131	2,468,131
Individuals	247,966	247,966	247,966	247,966	247,966	247,966
Mean DV	0.97	48.84	0.39	21.44	0.09	1.23

Notes: *Each column represents a separate regression of Equation 3. Y is as denoted by the column headers. This table is an expanded version of Table 2, showing the full set of coefficient estimates at age 60 in addition to the interacted effects. Standard errors clustered at the individual level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.*

Table A.4: Effects of Extra Paid Vacation (Individual FEs)

A: Main outcomes	(1) Employed	(2) Earnings	(3) Sickness	(4) Sick days	(5) DI receipt	(6) DI amount
Age = 60 × Late × Reform	0.0003	0.1298*	-0.0033	-0.1998	-0.0018	-0.0273
	(0.0014)	(0.0779)	(0.0045)	(0.5677)	(0.0015)	(0.0290)
Observations	2,468,131	2,468,131	2,113,686	2,113,686	2,468,131	2,468,131
Individuals	247,966	247,966	247,966	247,966	247,966	247,966
Mean DV	0.97	48.84	0.39	21.44	0.09	1.23

B: Additional outcomes	(1) New main employer	(2) Primary earnings	(3) Multiple employers	(4) Suppl. earnings
Age = 60 × Late × Reform	-0.0016	0.0379	-0.0006	0.0979
	(0.0028)	(0.1324)	(0.0032)	(0.0596)
Observations	2,222,196	2,468,131	2,468,131	2,468,131
Individuals	247,966	247,966	247,966	247,966
Mean DV	0.09	42.99	0.19	2.63

Notes: *Each column represents a separate regression of Equation 3. Y is as denoted by the column headers. Only the interaction effects at age 60 are reported. All models substitute the birth year fixed effects in Equation 3with individual-level fixed effects. Standard errors clustered at the individual level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.*

Table A.5: Effects of Extra Paid Vacation by Income, Gender and Sector of Employment

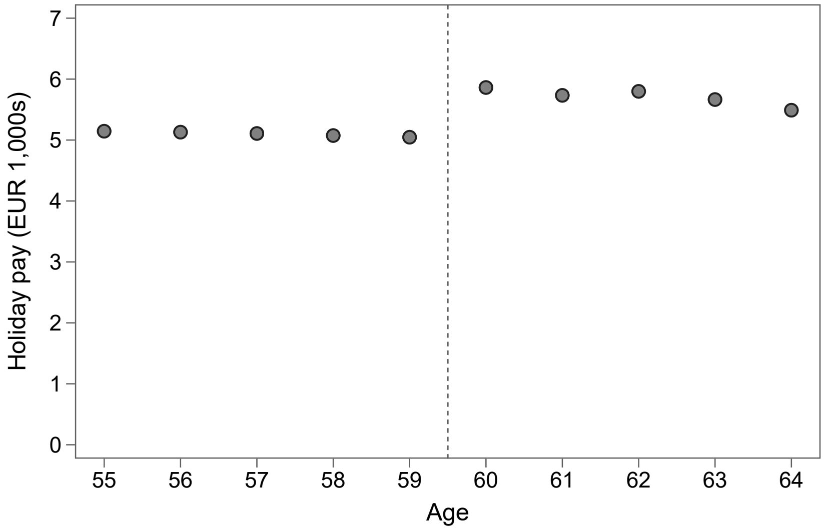
A: By earnings, age 52–59	(1) Employed	(2) Earnings	(3) Sickness	(4) Sickdays	(5) DI receipt	(6) DI amount
Below median (n = 124,118)	-0.0014	0.2208***	-0.0082	-0.8761	-0.0017	-0.0430
	(0.0024)	(0.0812)	(0.0065)	(0.9093)	(0.0024)	(0.0416)
Above median (n = 123,595)	0.0015	0.0704	0.0008	0.4162	-0.0017	-0.0053
	(0.0015)	(0.1336)	(0.0063)	(0.6829)	(0.0018)	(0.0408)
Equal effects (p-val.):	0.30	0.34	0.32	0.26	1.00	0.52

B: By gender	(1) Employed	(2) Earnings	(3) Sickness	(4) Sickdays	(5) DI receipt	(6) DI amount
Women (n = 119,281)	-0.0002	0.0797	-0.0059	-0.2420	0.0009	0.0104
	(0.0021)	(0.0768)	(0.0067)	(0.8666)	(0.0023)	(0.0393)
Men (n = 128,685)	0.0007	0.1929	-0.0013	-0.2002	-0.0043**	-0.0617
	(0.0019)	(0.1334)	(0.0061)	(0.7455)	(0.0020)	(0.0428)
Equal effects (p-val.):	0.76	0.46	0.61	0.97	0.09	0.21

C: By sector, age 59	(1) Employed	(2) Earnings	(3) Sickness	(4) Sickdays	(5) DI receipt	(6) DI amount
Public (n = 104,450)	-0.0006	0.0242	-0.0069	-1.1651	0.0001	-0.0203
	(0.0019)	(0.0816)	(0.0070)	(0.8752)	(0.0023)	(0.0425)
Private (n = 143,516)	0.0009	0.2200*	-0.0013	0.4645	-0.0033	-0.0320
	(0.0020)	(0.1219)	(0.0059)	(0.7485)	(0.0020)	(0.0397)
Equal effects (p-val.):	0.59	0.18	0.54	0.16	0.27	0.84

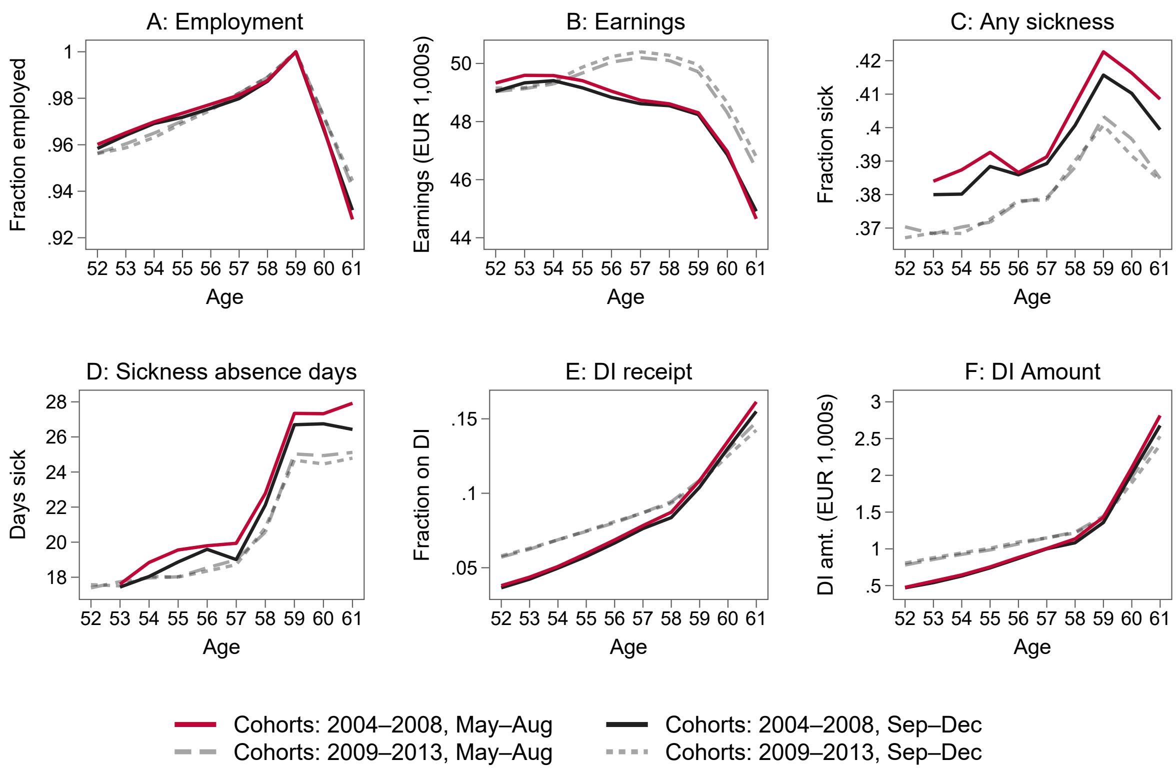
Notes: *Each cell represents a separate regression of Equation 3, conducted on different subsamples defined according to the criteria to the left. The number of individuals in each subsample is reported in brackets. Y is as denoted by the column headers. Only the three-way interaction effects at age 60 – $\gamma_{8,60}$ in Equation 3 – are reported. The p-value from a test of equal effects, derived from a saturated regression with interacted categories, is provided in the bottom rows of all panels. Institutional sector is defined according to the 1987 Norwegian Classification of Institutional Sector, where 'Public' includes categories 110 'Central government', 510 'County municipalities' and 550 'Municipalities', and 'Private' contains all other categories. Standard errors clustered at the individual level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.*

Figure A.1: Annual Holiday Pay, by Age



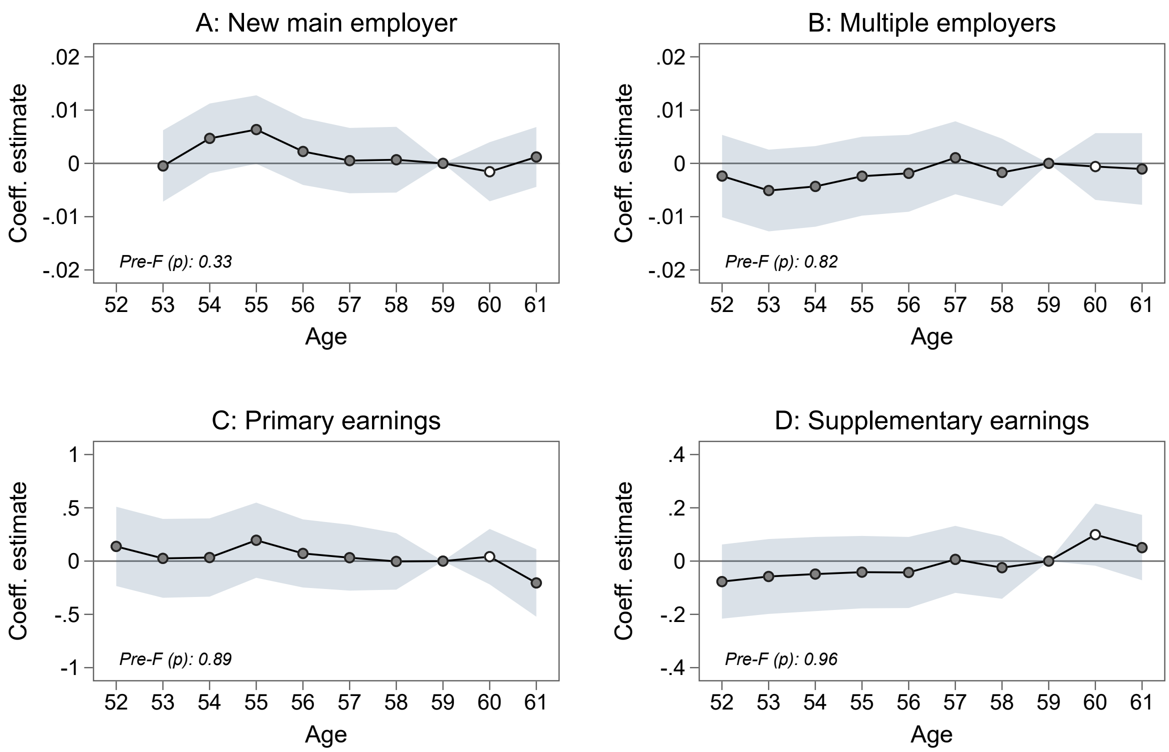
Notes: *The figure shows average annual holiday pay by age, based on a pooled cross-section of all individuals aged 55–64 who received holiday pay between 2015 and 2023. The dashed vertical line denotes the cutoff when individuals would be eligible for one additional week. Information on holiday pay transfers is available in the employer-employee register (A-ordningen) from 2015 onward.*

Figure A.2: Raw Data Trends by Cohorts and Month of Birth



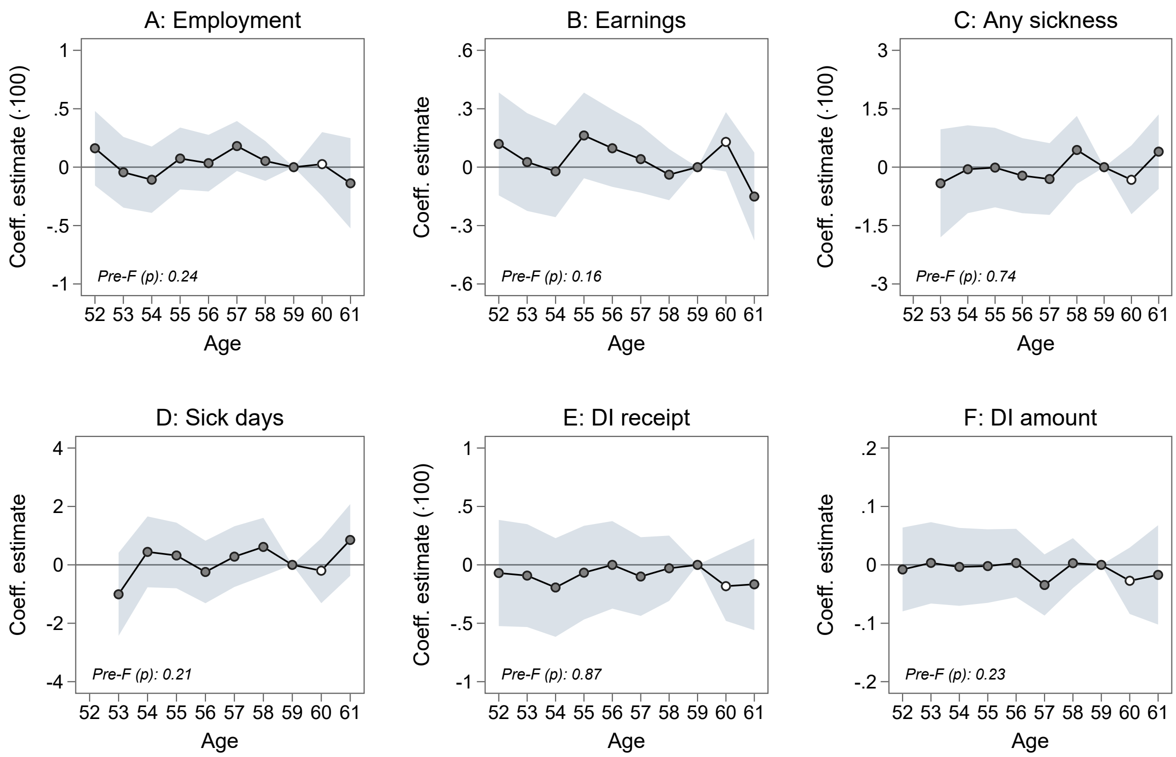
Notes: *Each panel depicts average outcomes across ages 52 to 61. Solid red and black lines represent individuals turning 60 in May–August and September–December of 2004–2008, respectively. Dashed and short-dashed gray lines represent those turning 60 in the same months of 2009–2013, respectively.*

Figure A.3: Effects of Extra Paid Vacation – Additional Outcomes, Event Study



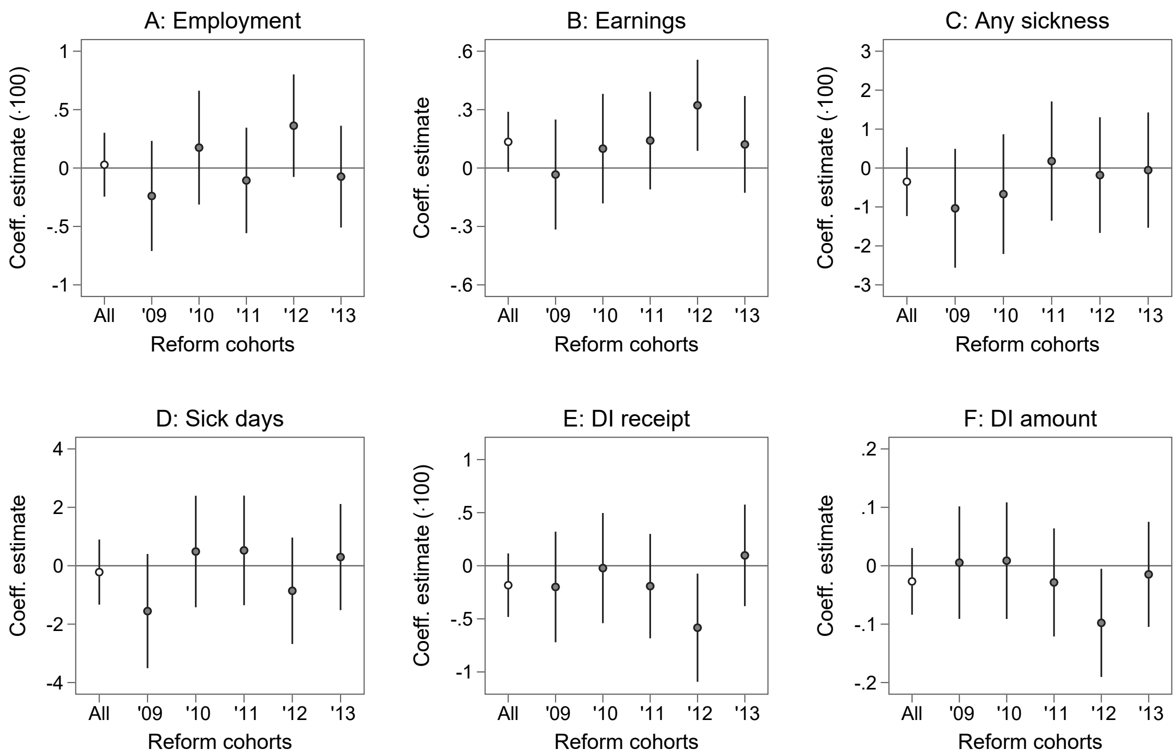
Notes: *Each panel presents coefficient estimates of* $\gamma_{8,s}$ *in Equation 3 for individuals aged 52 to 61. Age 59 serves as the reference category. White-filled coefficients denote* $\gamma_{8,60}$. *Shaded areas represent 95% confidence intervals. Standard errors are clustered at the individual level. The p-value from an F-test of joint significance for the pre-period (age* < 59*) is reported in the lower-left corner of each plot.*

Figure A.4: Effects of Extra Paid Vacation, Event Study (Individual FEs)



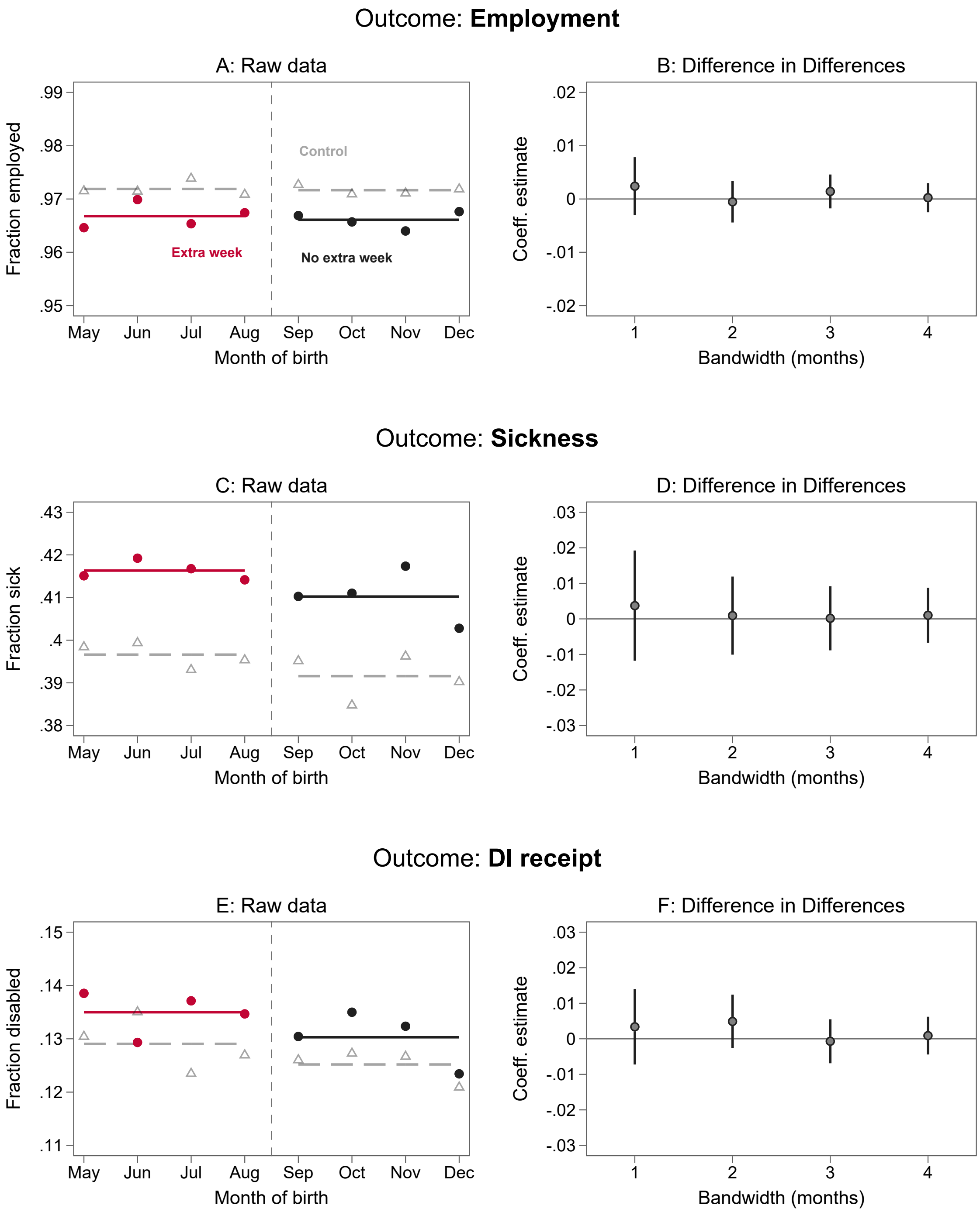
Notes: *Each panel presents coefficient estimates of* $\gamma_{8,s}$ *in Equation 3 for individuals aged 52 to 61. Age 59 serves as the reference category. White-filled coefficients denote* $\gamma_{8,60}$*. Shaded areas represent 95% confidence intervals. All models substitute the birth year fixed effects with individual-level fixed effects. Standard errors are clustered at the individual level. The p-value from an F-test of joint significance for the pre-period (age* < 59*) is reported in the lower-left corner of each plot.*

Figure A.5: Effects of Extra Paid Vacation, Separate Reform Cohorts



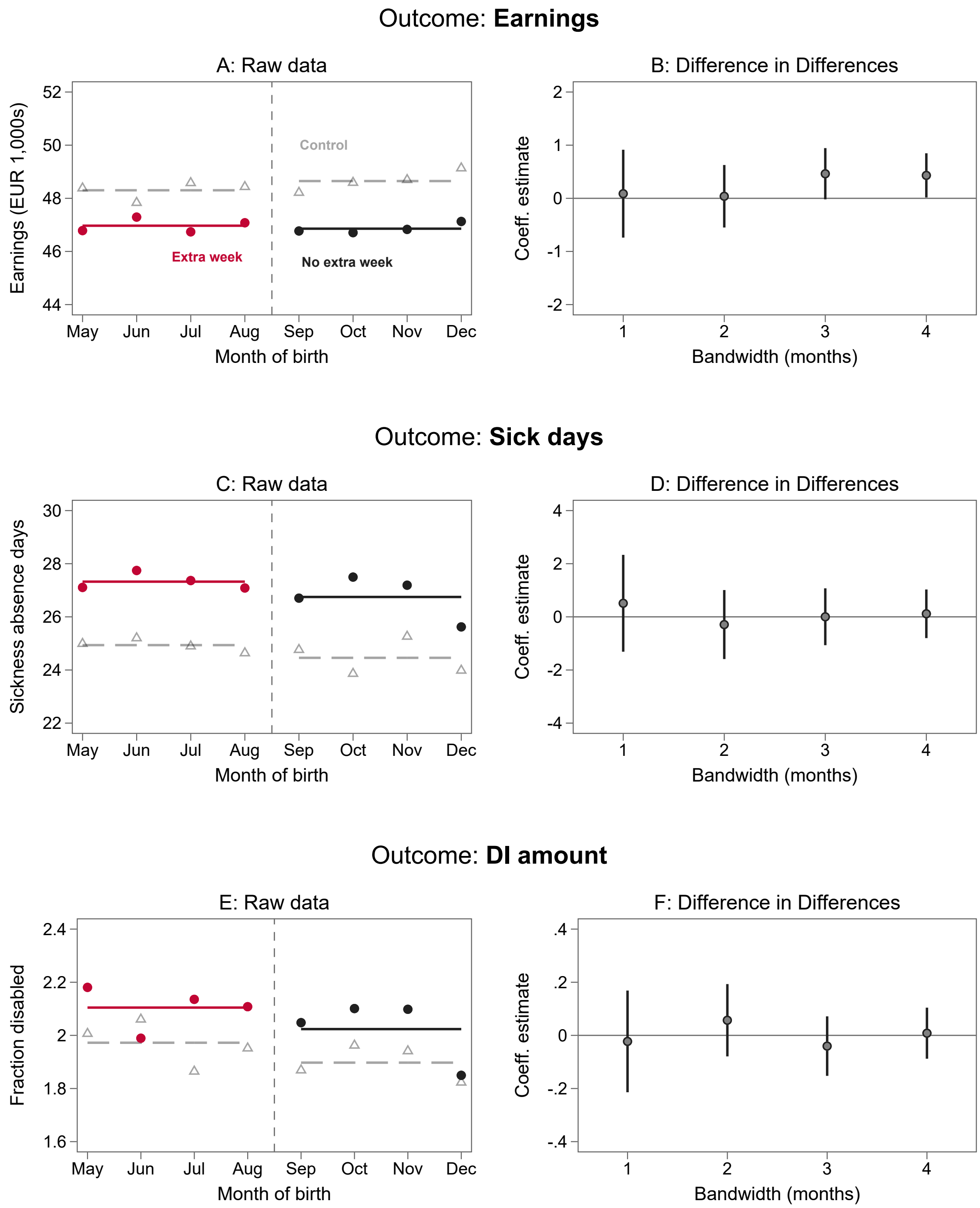
Notes: *Each panel reports coefficient estimates of* $\gamma_{8,60}$ *in Equation 3, estimated using different cohorts from the post-reform period. Cohort labels (e.g., '09') refer to the calendar year in which individuals turn 60. Lines represent 95% confidence intervals. White-filled markers correspond to the full-sample estimate reported in Table 2. Standard errors are clustered at the individual level.*

Figure A.6: Alternative estimation strategy – Extensive Margin Outcomes



Notes: *The left panels (A, C, E) plot the average outcome in the year of turning 60, by month of birth. Solid dots represent cohorts that turned 60 in the years 2004–2008 (extra week/no extra week), while hollow triangles represent cohorts that turned 60 in the years 2009–2013 (control). Sample-wide averages are represented by horizontal lines. The right panels (B, D, F) presents coefficient estimates of the difference in differences effects, along with 95% confidence intervals, using observations from an increasing number of birth months on either side of the threshold. Regression models include indicator variables that control flexibly for each year of birth in the sample.*

Figure A.7: Alternative estimation strategy – Intensive Margin Outcomes



Notes: *The left panels (A, C, E) plot the average outcome in the year of turning 60, by month of birth. Solid dots represent cohorts that turned 60 in the years 2004–2008 (extra week/no extra week), while hollow triangles represent cohorts that turned 60 in the years 2009–2013 (control). Sample-wide averages are represented by horizontal lines. The right panels (B, D, F) presents coefficient estimates of the difference in differences effects, along with 95% confidence intervals, using observations from an increasing number of birth months on either side of the threshold. Regression models include indicator variables that control flexibly for each year of birth in the sample.*

B Mandatory paid leave in a model of labor supply

A mandated paid leave should in general cause workers to re-optimize their chosen levels of consumption and leisure such that leisure (labor supply) is higher (lower) than without mandated leave, but lower (higher) than without any adaptive behavior. To illustrate, we turn to the canonical labor supply model using a closed-form Cobb Douglas utility function with consumption C, leisure l, working hours h, time endowment T, and non-labor income V. The utility function is then:

$$C + wl = wT + V = M, \qquad h \geq 0, 0 \leq l \leq T, C = wh, T = h + l. \tag{B.1}$$

Using the budget restriction, the utility maximization problem implies

$$C^*(V) = \alpha\ M, \tag{B.2}$$
$$l^*(V) = \frac{(1-\alpha)M}{w}. \tag{B.3}$$

The mandatory vacation week strictly reduces l for workers within their main employers such that $l' = l + \Delta l$ and working hours decline to $h' = h - \Delta l$. The reduction is compensated with a new transfer $V' = V + v$ such that consumption is fixed. Using the budget constraint, we have a net zero change in consumption:

$$\begin{aligned} C^*(V') &= w\big(T - l'\big) + V' \\ &= w\big(T - (l + \Delta l)\big) + (V + v) \\ &= \underbrace{w(T - l) + V}_{C} + \big(v - w\Delta l\big) \end{aligned} \tag{B.4}$$

with $v = w\,\Delta l = w\,(l' - l) = w\,(h - h')$. If workers are allowed to re-optimize their choice set of leisure and consumption, they will decrease leisure relative to l'. To see this, consider the optimal choice of leisure; substituting the new budget constraint yields:

$$l^*(V') = (1-\alpha)\left(T + \frac{V+v}{w}\right) \;=\; l + (1-\alpha)\,\frac{v}{w} \tag{B.5}$$

$$= l + (1-\alpha)\Delta l \;<\; l' = l + \Delta l, \tag{B.6}$$

and, equivalently, for working hours:

$$\begin{aligned} h^*(V+v) &= T - l^*(V+v) \\ &= h - (1-\alpha)\Delta l \;>\; h' = h - \Delta l. \end{aligned} \tag{B.7}$$

The conclusion also holds for a general utility function under standard assumptions. Forcing an increase in leisure with given consumption leads to an increase in utility:

$$U(C, l') - U(C, l) > 0. \tag{B.8}$$

If the worker had optimized prior to the mandated leave the marginal rate of substition (MRS) was given by:

$$\text{MRS}(C, l) := \frac{U_l(C, l)}{U_C(C, l)} = w. \tag{B.9}$$

Decreasing the marginal return of leisure implies that the MRS no longer equals w, absent re-optimization:

$$\text{MRS}(C, l') \;<\; \text{MRS}(C, l) \;=\; w. \tag{B.10}$$

In order to achieve equilibrium the worker needs to increase consumption and decrease leisure from l' such that:

$$\text{MRS}(C^*, l^*) := \frac{U_l(C^*, l^*)}{U_C(C^*, l^*)} = w. \tag{B.11}$$