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ABSTRACT

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

This study examines the impact of receiving one additional week of paid vacation on labor market attachment among Norwegian workers aged 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.

JEL Classification: H8, I12, J22, J26

Keywords: paid vacation, older workers, labor supply, triple-differences, public policy

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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 in the year and turning 60 before and after the reform in a triple-difference strategy, we can pin down the causal effects of extra 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.

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. 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 (2015) used survey data on firms' additional leave policies for older workers coupled with administrative data to estimate differential retention responses across firms. While the study found that retention rates increased in firms introducing extra vacation relative to firms that did not introduce it, these firms may also have implemented other supportive policies or 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 gained the right to eight additional vacation days from age 62, the retainment rate among state employees in their 60s increased relative to that of employees in the same age group in other sectors. It is

hard to evaluate whether this was due to the extra vacation days or whether other factors were responsible for the differential trends.

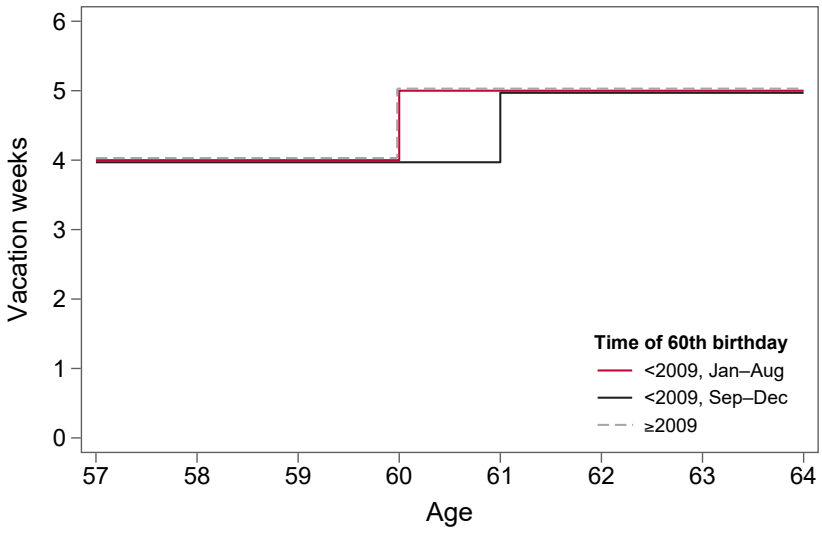
This paper relates more broadly to the growing literature on workplace amenities and labor supply (see, e.g., Akerlof et al., 1988; Cassar and Meier, 2018; Maestas et al., 2023; Sockin, 2022). Scholars have investigated the effects of various forms of non-pecuniary compensation, including flexible hours (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] Funding was initially covered by the Norwegian social insurance scheme, but this responsibility was shifted to employers in 1988. For many years, eligibility for the extra week in the year of turning 60 depended on the individual's birthday, with those born on or after September 1st not becoming eligible until the subsequent 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. Self-employed individuals 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



[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)

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

We do not observe uptake of paid leave in our data. However, Norwegian law stipulates that employees utilize all their vacation days. In a 2015 survey, over 70% of employees aged 60-70 responded that they actually used the extra week of vacation and more than 50% answered that the extra week provided some or a large motivation to postpone retirement (Svalund and Veland, 2016). As we do not observe any information on vacation uptake, our results will be reduced form.[2]

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 income of at least one 'basic amount' in the year they turned 59.[3] 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

[2]Assuming that uptake is random, estimates can be multiplied with 1.43 (1/0.7) to uncover the full effect.

[3]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.[4] 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

[4]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 (%)	9.5	29.3	9.0	28.6
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.1 shows stable trends within cohorts over time, with differences emerging primarily across cohorts.

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 of taking this approach could take the form:

$$\begin{aligned} Y_{ia} = \alpha_1 + \sum_{a \neq 59} \beta_{1,a} \mathbb{1}_{age=a} + \gamma_1 [Late]_i \\ + \sum_{a \neq 59} \delta_{1,a} \mathbb{1}_{age=a} \cdot [Late]_i + \mu_1 X_i + \varepsilon_{1,ia}, \end{aligned} \tag{1}$$

where Y_{ia} is outcome Y for individual i at age a, $\mathbb{1}_{age=a}$ is an indicator for the age of person i equaling a, and $[Late]_i$ equals 1 for individuals born between September and December. X_i is a vector of individual characteristics that, unless otherwise specified, includes birth year and calendar year fixed effects. $\delta_{1,60}$ 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. As suggested by Table 1, the age gap and other differences between individuals born early and late in the year could be significant enough to bias the results.

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 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:

$$\begin{aligned} Y_{ia} = \alpha_2 + \sum_{a \neq 59} \beta_{2,a} \mathbb{1}_{age=a} + \gamma_2 [Reform]_i \\ + \sum_{a \neq 59} \delta_{2,a} \mathbb{1}_{age=a} \cdot [Reform]_i + \mu_2 X_i + \varepsilon_{2,ia}, \end{aligned} \tag{2}$$

where $[Reform]_i$ 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 all else 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 $\delta_{2,60}$ 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.[5]

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:

$$Y_{ia} = \sum_{l=0}^{1} \sum_{p=0}^{1} \sum_{\substack{a=52 \\ a \neq 59}}^{61} \theta_{a,p,l} \mathbb{1}_{age=a} \mathbb{1}_{[Late]=l} \mathbb{1}_{[Reform]=p} + \mu_3 X_i + \varepsilon_{3,ia}. \tag{3}$$

The causal effect of an additional week of vacation at age 60 is given by $\theta_{1,1,60}$ which identifies the differential effect for those who turned 60 after the reform and were born late in the year.

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

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 using five outcomes meant to capture labor market attachment: (i) employment (0/1), defined as annual wage earnings totaling 1G or higher; (ii) annual wage earnings in EUR 1000s[6]; (iii) any doctor-certified sickness absence days (0/1); (iv) the effective number of sickness absence days per year (0-365), with partial absences weighted proportionally; and (v) any disability insurance receipt (0/1), including both temporary and permanent benefits. Individuals are included in the sample from the year they turn 52 until the year they turn 61.[7] 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.[8] Standard errors are clustered at the individual level to allow for arbitrary correlation within individuals over time.

4 Results

4.1 Main Results

The estimated impact of an additional week of paid vacation at age 60 is presented in Table 2. Panel A summarizes results for the five main outcomes, with estimates of $\theta_{1,1,60}$ reported in the bottom row.[9] Overall, the effects of extra vacation eligibility are negligible. In column

[6] Wage income is top-coded above the 99.5th percentile.

[7] 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 $\theta_{1,1,a}$ in Equation 3 for these outcomes is 53.

[8] 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.

[9] Table 2 reports only the interaction effects. For an expanded version that includes the full set of coefficients at age 60, see Appendix Table A.2. For completeness, we also report estimates obtained using

(1), the triple-differences estimate for employment is essentially zero, and the 95% confidence interval rules out an impact smaller than -0.24 and larger than 0.3 percentage points. This suggests that, on net, the additional week does not make a difference for labor market participation.

In column (2), the point estimate of 0.13 indicates a modest increase of about 130 EUR in annual income. However, this estimate is only significant at the 10% level and may reflect chance. There are also small or negligible effects on sickness absence and the probability of receiving disability insurance. In particular, column (4) shows that we can rule out a reduction in annual sick leaves 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. Thus, even for this group of workers, with a relatively high initial sickness absence level, an extra week of vacation does not seem to have an impact on sickness absence.

4.2 Mechanisms

Despite the overall null results, the result of a 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

the two estimation strategies specified by Equations 1 and 2 in Appendix Table A.3.

Table 2: Effects of Extra Paid Vacation

A: Main outcomes	(1) Employed	(2) Earnings	(3) Sickness	(4) Sick days	(5) DI receipt
Age = 60 × Late	-0.0006	-0.0411	0.0010	0.0970	0.0002
	(0.0010)	(0.0579)	(0.0033)	(0.4189)	(0.0011)
Age = 60 × Reform	0.0068***	0.3000***	0.0023	-0.0508	-0.0020*
	(0.0010)	(0.0613)	(0.0032)	(0.4031)	(0.0011)
Age = 60 × Late × Reform	0.0003	0.1346*	-0.0035	-0.2181	-0.0022
	(0.0014)	(0.0786)	(0.0045)	(0.5686)	(0.0015)
Observations	2,468,131	2,468,131	2,113,686	2,113,686	2,468,131
Individuals	247,966	247,966	247,966	247,966	247,966
Mean DV	0.97	48.84	0.39	21.44	0.07

B: Additional outcomes	(1) New main employer	(2) Primary earnings	(3) Multiple employers	(4) Suppl. earnings
Age = 60 × Late	0.0005	-0.0522	0.0009	-0.0709*
	(0.0019)	(0.0986)	(0.0023)	(0.0407)
Age = 60 × Reform	0.0071***	0.4826***	-0.0000	-0.0825*
	(0.0020)	(0.0978)	(0.0023)	(0.0428)
Age = 60 × Late × Reform	-0.0016	0.0414	-0.0006	0.0992*
	(0.0028)	(0.1329)	(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. Panel A considers our five main outcomes, while Panel B considers additional outcomes. Only the 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 differencing for this outcome. Standard errors clustered at the individual level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.*

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

be induced to seek out new employment. On the other hand, many already have multiple employments, and column (4) shows a positive effect on total supplementary earnings. 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.

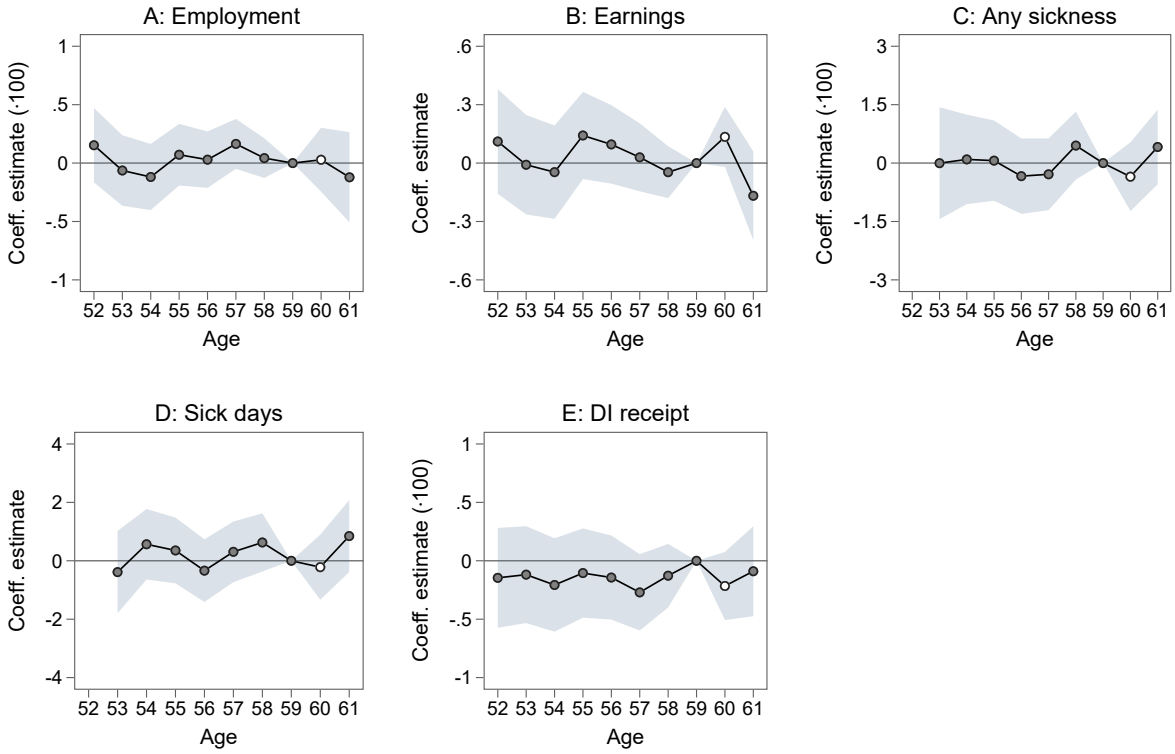
4.3 Dynamic responses

Figure 2 plots the full set of triple-differences estimates for the five 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 $\theta_{1,1,60}$ (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 effect on income observed at age 60 eliminated at age 61. Similar results are shown in Appendix Figure A.2 for the four additional outcomes considered in Panel B of Table 2.

4.4 Robustness Checks

We perform several robustness checks. First, estimates corresponding to those presented in Table 2 and Figure 2, but obtained from regressions that incorporate individual-level fixed effects, are reported in Appendix Table A.4 and Appendix Figure A.3. This does not affect results. Second, Appendix Figures A.4 and A.5 provide estimates of Equation 3 without the intertemporal component, comparing outcomes in the year of turning 60 only across cohorts and months of birth. This alternative estimation strategy relies on a stronger assumption of balance between the treatment and control samples. The results remain generally consistent

Figure 2: Effects of Extra Paid Vacation, Event Study



Notes: *Each panel presents coefficient estimates of $\theta_{1,1,a}$ in Equation 3 for individuals aged 52 to 61. Age 59 serves as the reference category. White-filled coefficients denote estimates of $\theta_{1,1,60}$. Shaded areas represent 95% confidence intervals. Standard errors are clustered at the individual level.*

regardless of bandwidth used.

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 $\theta_{1,1,60}$ 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).

Table 3: Effects of Extra Paid Vacation by Occupation Type

	(1) Employed	(2) Earnings	(3) Sickness	(4) Sickdays	(5) DI receipt
High-skilled white-collar [123, 240]	0.0004	0.1052	-0.0005	0.7045	-0.0028
	(0.0016)	(0.1290)	(0.0063)	(0.6864)	(0.0018)
Low-skilled white-collar [73, 006]	0.0009	0.1785*	-0.0039	-0.6470	-0.0011
	(0.0029)	(0.1001)	(0.0085)	(1.1525)	(0.0030)
High-skilled blue-collar [20, 160]	0.0038	0.1716	0.0017	0.0167	-0.0012
	(0.0056)	(0.2739)	(0.0163)	(2.2222)	(0.0060)
Low-skilled blue-collar [31, 465]	-0.0037	0.1325	-0.0175	-2.6295	-0.0028
	(0.0050)	(0.2069)	(0.0130)	(1.9173)	(0.0051)
Equal effects (p-val.):	0.78	0.98	0.68	0.36	0.96

Notes: *Each cell represents a separate regression of Equation 3, conducted on different subsamples defined by occupation category at age 59. The number of individuals in each subsample is reported in brackets. Y is as denoted by the column headers. Only the interaction effects at age 60 – $\theta_{1,1,60}$ in Equation 3 – are reported. P-values from tests of equal effects, derived from saturated regressions with the occupation groups interacted, 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. *** p<0.01, ** p<0.05, * p<0.1.*

The results reveal no clear occupational patterns. The only statistically significant coefficient estimate appears on earnings in column (2) for low-skilled white-collar occupations – namely, 'Clerical support workers' and 'Service and sales workers' – but the magnitude of about 180 EUR is comparatively similar to (and not significantly different from) the other groups. The bottom row reports p-values from tests of equal effects for each of the five outcomes, obtained from separate saturated regressions with the occupation groups interacted. None of the null hypotheses are rejected.

Appendix Table A.5 additionally examines heterogeneity by income, gender, and sector

of employment. There is no evidence that estimates vary significantly across any of these categories, although the earnings effect appears to be driven mainly by male, private-sector workers who earn less than the median.

5 Discussion

Mandating additional 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. 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. We find some evidence of increased earnings, which we trace to greater intensive-margin labor supply among workers with secondary employment. It therefore appears that, if anything, some workers use the additional paid leave as a work subsidy to increase supplemental earnings, revealing that they prefer increased income to paid leave. The cost of the extra vacation is borne by employers, and back-of-the-envelope calculations suggest that this amounts to approximately EUR 350 million per year (3.5 billion NOK). In light of the policy's stated aim of ensuring sufficient rest, this substantial cost with little return casts doubt on its effectiveness.

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. We 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.[10] 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.

A key limitation of our study is the focus on effects measured within one year of eligibility. Potential effects that takes place over many years, such as delayed retirement or cumulative health benefits, are not captured in our analysis. Future research could explore longer time horizons to assess the sustained impact of additional paid vacation.

As life expectancy has risen and health has improved, many age thresholds in the Norwegian pension system have been adjusted. 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.

[10]This estimate is derived from: $W = \sum_{ik} w_{ik}(1 + \tau_k) \cdot \frac{5}{230} \cdot 0.75$, 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 fact that not all eligible workers take the leave. Earnings are taken from 2022 and deflated to 2025 prices using the Norwegian Basic Amount Index.

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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 (%)	9.2	29.0	9.1	28.7
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: Effects of Extra Paid Vacation (Expanded Table)

	(1) Employed	(2) Earnings	(3) Sickness	(4) Sick days	(5) DI receipt
Late	0.0000*	-0.0555	-0.0070**	-0.6767**	-0.0048***
	(0.0000)	(0.1439)	(0.0028)	(0.3413)	(0.0017)
Reform	0.0008	-2.5529	-0.0557**	-6.6386***	-0.0113
	(0.0069)	(1.8886)	(0.0216)	(1.8597)	(0.0164)
Age = 60	-0.0348***	-2.0788***	-0.0101***	-0.1461	0.0232***
	(0.0011)	(0.2250)	(0.0034)	(0.3616)	(0.0021)
Late × Reform	-0.0000*	0.2989	0.0045	0.3364	0.0032
	(0.0000)	(0.2057)	(0.0040)	(0.4646)	(0.0023)
Age = 60 × Late	-0.0006	-0.0411	0.0010	0.0970	0.0002
	(0.0010)	(0.0579)	(0.0033)	(0.4189)	(0.0011)
Age = 60 × Reform	0.0068***	0.3000***	0.0023	-0.0508	-0.0020*
	(0.0010)	(0.0613)	(0.0032)	(0.4031)	(0.0011)
Age = 60 × Late × Reform	0.0003	0.1346*	-0.0035	-0.2181	-0.0022
	(0.0014)	(0.0786)	(0.0045)	(0.5686)	(0.0015)
Observations	2,468,131	2,468,131	2,113,686	2,113,686	2,468,131
Individuals	247,966	247,966	247,966	247,966	247,966
Mean DV	0.97	48.84	0.39	21.44	0.07

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.3: 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
Age = 60	-0.0363***	-1.6433***	-0.0057**	0.5530*	0.0252***
	(0.0008)	(0.0695)	(0.0024)	(0.3031)	(0.0010)
Late	0.0000	-0.0566	-0.0070**	-0.6798**	-0.0048***
	(0.0000)	(0.1439)	(0.0028)	(0.3413)	(0.0017)
Age = 60 × Late	-0.0005	-0.0402	0.0010	0.1019	0.0002
	(0.0010)	(0.0579)	(0.0033)	(0.4189)	(0.0011)
Observations	1,203,473	1,203,473	849,028	849,028	1,203,473
Individuals	120,807	120,807	120,807	120,807	120,807
Mean DV	0.97	48.36	0.40	23.03	0.07
B: Born late in the year	(1) Employed	(2) Earnings	(3) Sickness	(4) Sick days	(5) DI receipt
Age = 60	-0.0337***	-2.2111***	-0.0086**	-0.1913	0.0226***
	(0.0014)	(0.3228)	(0.0043)	(0.4347)	(0.0029)
Reform	0.0150	-2.8900	-0.0431	-7.1007***	-0.0144
	(0.0101)	(2.7253)	(0.0310)	(2.6626)	(0.0237)
Age = 60 × Reform	0.0067***	0.4829***	-0.0019	-0.3234	-0.0042***
	(0.0011)	(0.0698)	(0.0034)	(0.4281)	(0.0013)
Observations	1,175,799	1,175,799	1,005,682	1,005,682	1,175,799
Individuals	118,132	118,132	118,132	118,132	118,132
Mean DV	0.97	48.86	0.39	21.25	0.07

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

A: Main outcomes	(1) Employed	(2) Earnings	(3) Sickness	(4) Sick days	(5) DI receipt
Age = 60 × Late	-0.0006	-0.0316	0.0008	0.0786	0.0002
	(0.0011)	(0.0605)	(0.0035)	(0.4451)	(0.0012)
Age = 60 × Reform	0.0067***	0.2782***	0.0020	-0.0839	-0.0022*
	(0.0011)	(0.0640)	(0.0034)	(0.4284)	(0.0012)
Age = 60 × Late × Reform	0.0003	0.1298	-0.0033	-0.1998	-0.0021
	(0.0015)	(0.0822)	(0.0048)	(0.6042)	(0.0016)
Observations	2,468,131	2,468,131	2,113,686	2,113,686	2,468,131
Individuals	247,966	247,966	247,966	247,966	247,966
Mean DV	0.97	48.84	0.39	21.44	0.07

B: Additional outcomes	(1) New main employer	(2) Primary earnings	(3) Multiple employers	(4) Suppl. earnings
Age = 60 × Late	0.0006	-0.0433	0.0009	-0.0715*
	(0.0020)	(0.1037)	(0.0024)	(0.0429)
Age = 60 × Reform	0.0072***	0.4594***	-0.0001	-0.0819*
	(0.0021)	(0.1024)	(0.0024)	(0.0451)
Age = 60 × Late × Reform	-0.0016	0.0379	-0.0006	0.0979
	(0.0030)	(0.1396)	(0.0034)	(0.0628)
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

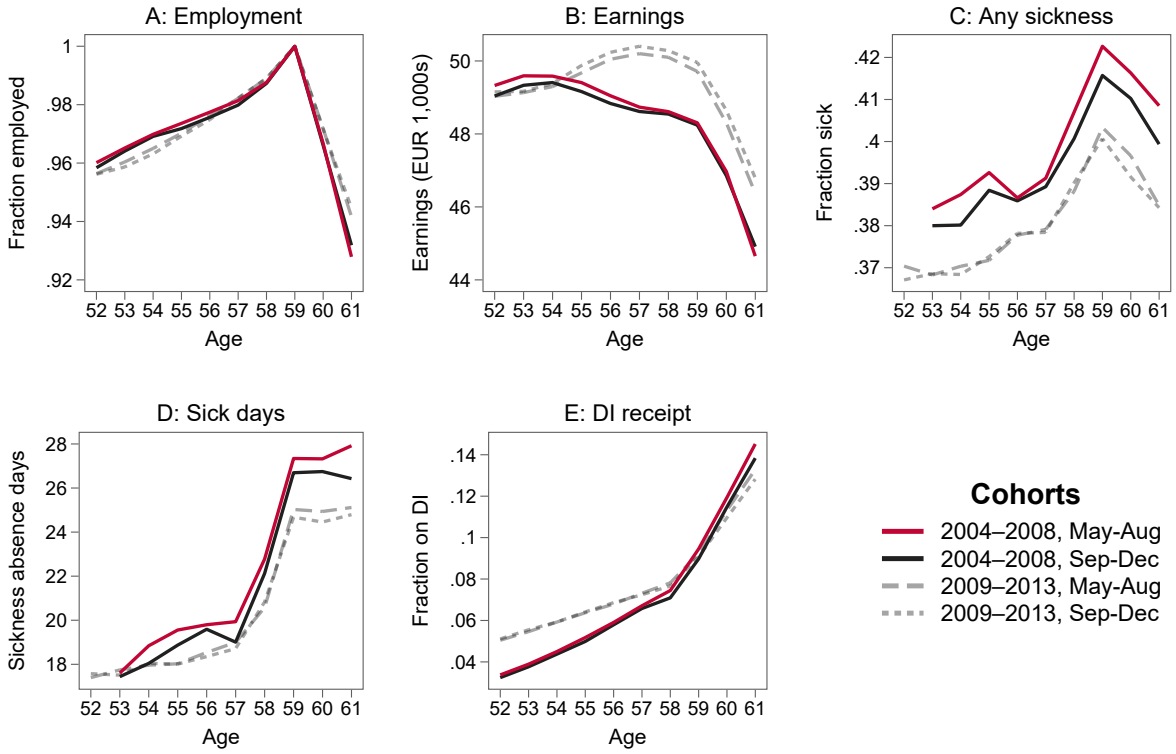
Notes: *Each column represents a separate regression of Equation 3. Y is as denoted by the column headers. Only the interaction effects at age 60 are reported. All models substitute the birth year fixed effects in Equation 3 with individual-level fixed effects. Standard errors clustered at the individual 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
Below median [124, 118]	-0.0013	0.2205***	-0.0081	-0.8755	-0.0023
	(0.0024)	(0.0812)	(0.0065)	(0.9093)	(0.0024)
Above median [123, 595]	0.0015	0.0702	0.0008	0.4157	-0.0017
	(0.0015)	(0.1336)	(0.0063)	(0.6829)	(0.0018)
Equal effects (p-val.):	0.30	0.34	0.32	0.26	0.83
B: By gender	(1) Employed	(2) Earnings	(3) Sickness	(4) Sickdays	(5) DI receipt
Women [119, 281]	-0.0002	0.0794	-0.0059	-0.2420	0.0009
	(0.0021)	(0.0768)	(0.0067)	(0.8666)	(0.0023)
Men [128, 685]	0.0007	0.1931	-0.0013	-0.2007	-0.0049**
	(0.0019)	(0.1334)	(0.0061)	(0.7455)	(0.0020)
Equal effects (p-val.):	0.76	0.46	0.61	0.97	0.05
C: By sector, age 59	(1) Employed	(2) Earnings	(3) Sickness	(4) Sickdays	(5) DI receipt
Public [104, 450]	-0.0006	0.0246	-0.0069	-1.1651	0.0001
	(0.0019)	(0.0816)	(0.0070)	(0.8752)	(0.0022)
Private [143, 516]	0.0009	0.2197*	-0.0013	0.4645	-0.0038*
	(0.0020)	(0.1219)	(0.0059)	(0.7485)	(0.0020)
Equal effects (p-val.):	0.59	0.18	0.54	0.16	0.19

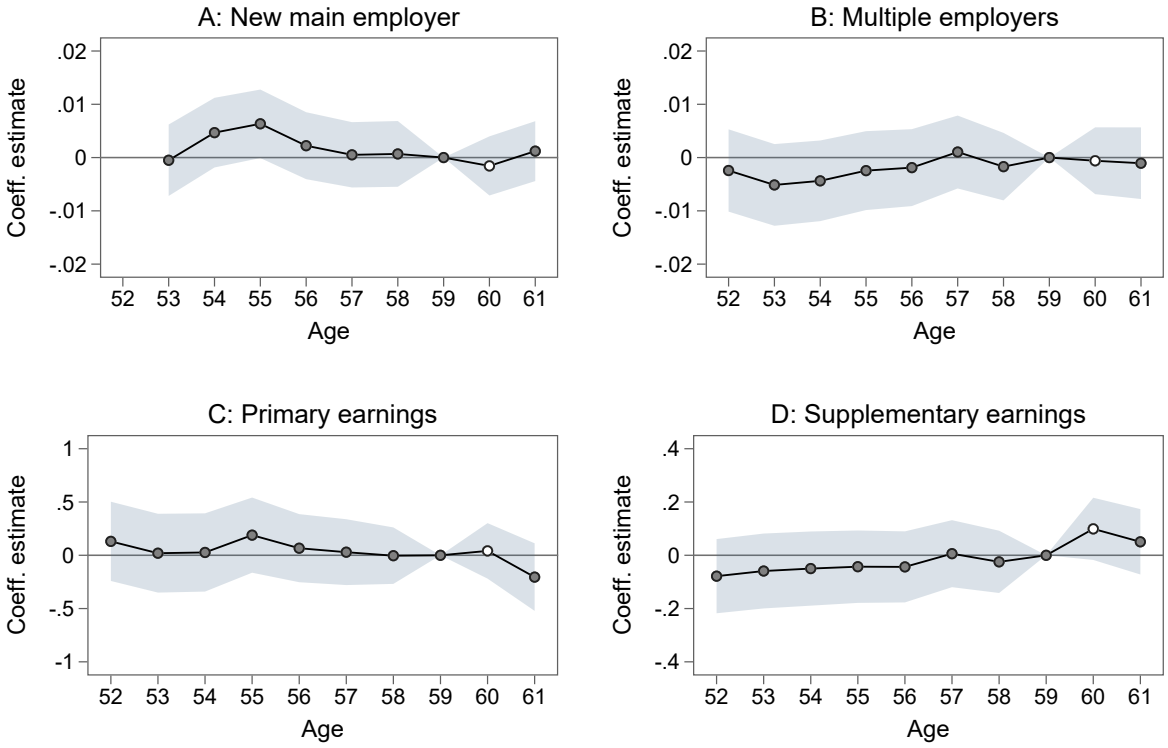
Notes: *Each cell represents a separate regression of Equation 3, conducted on different subsamples defined according to the criteria to the left. The number of individuals in each subsample is reported in brackets. Y is as denoted by the column headers. Only the triple interaction effects at age 60 – $\theta_{1,1,60}$ 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: Raw Data Trends by Cohorts and Month of Birth



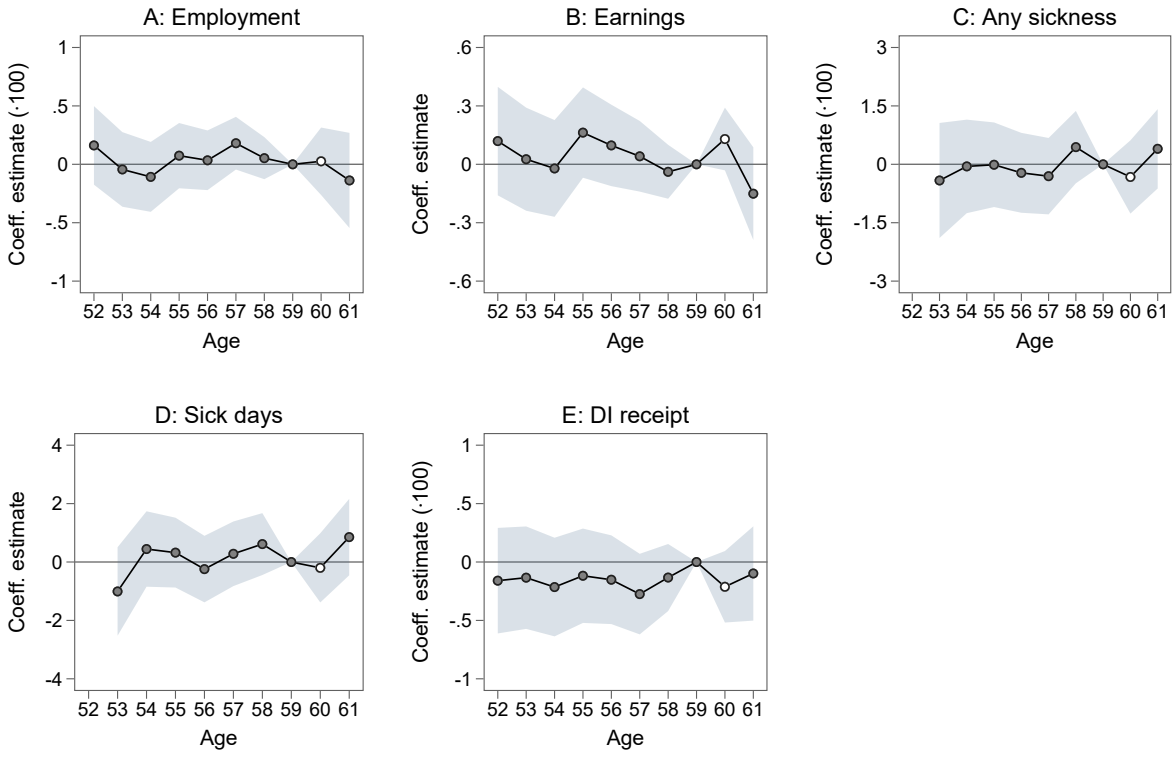
Notes: *Each panel depicts the average outcome across ages 52 to 61. Red and black solid lines represent individuals turning 60 in May–August and September–December in 2004–2008, respectively. Dashed and short-dashed gray lines represent those turning 60 in the same months of 2009–2013, respectively.*

Figure A.2: Effects of Extra Paid Vacation – Additional Outcomes, Event Study



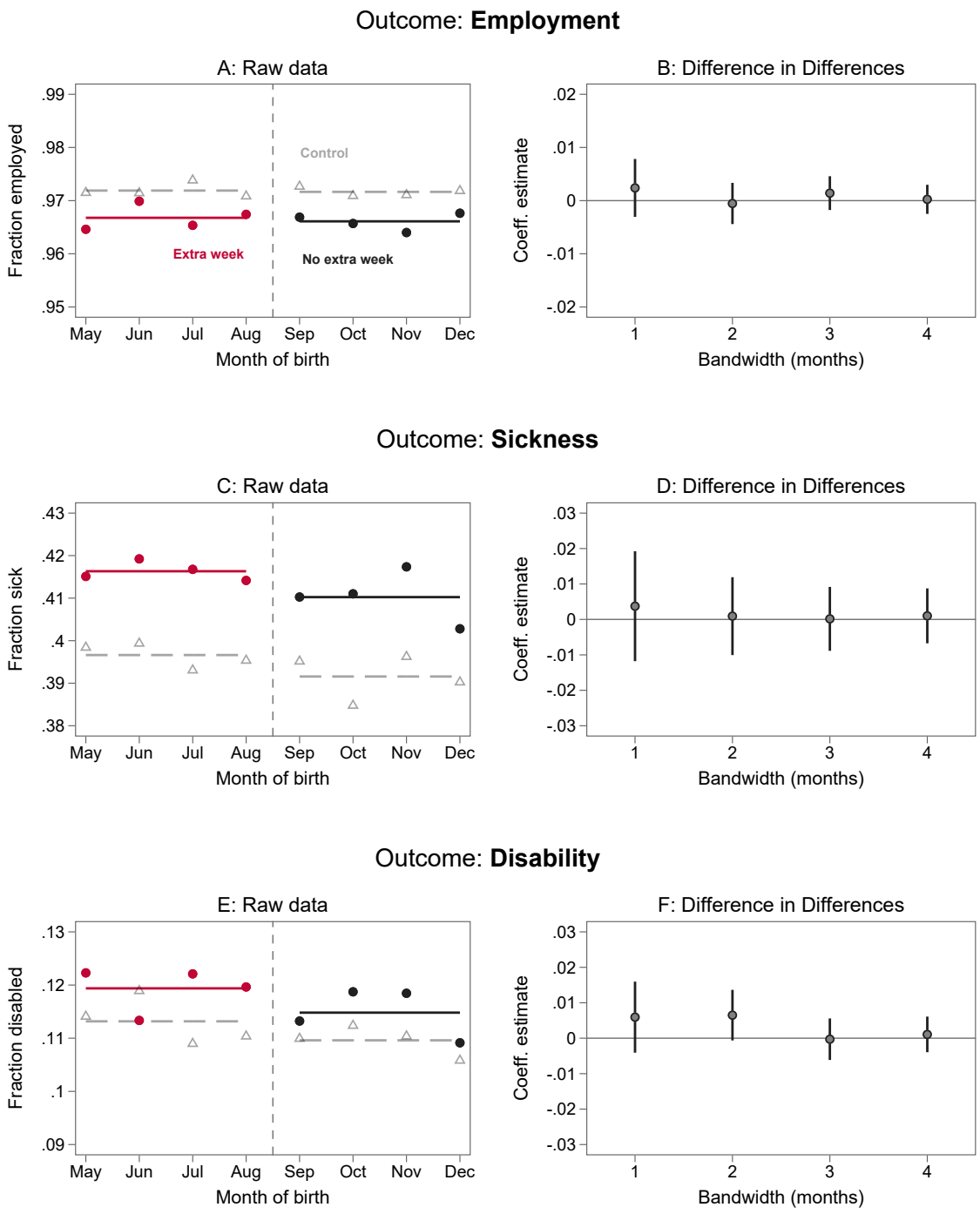
Notes: *Each panel presents coefficient estimates of* $\theta_{1,1,a}$ *in Equation 3 for individuals aged 52 to 61. Age 59 serves as the reference category. White-filled coefficients denote estimates of* $\theta_{1,1,60}$*. Shaded areas represent 95% confidence intervals. Standard errors are clustered at the individual level.*

Figure A.3: Effects of Extra Paid Vacation, Event Study (Individual FEs)



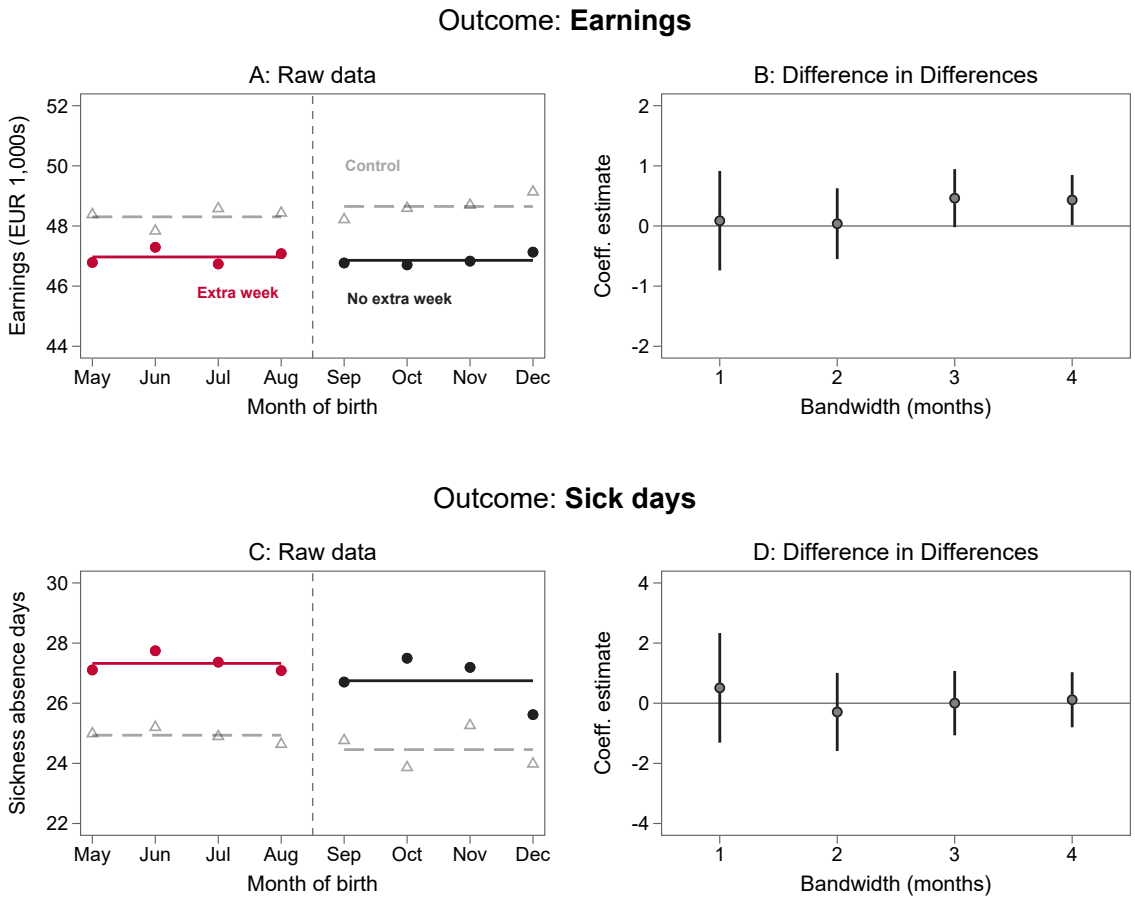
Notes: *Each panel presents coefficient estimates of $\theta_{1,1,a}$ in Equation 3 for individuals aged 52 to 61. Age 59 serves as the reference category. White-filled coefficients denote estimates of $\theta_{1,1,60}$. Shaded areas represent 95% confidence intervals. All models substitute the birth year fixed effects with individual-level fixed effects. Standard errors are clustered at the individual level.*

Figure A.4: 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.5: Alternative estimation strategy – Intensive Margin Outcomes



Notes: *The left panels (A, C) 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) 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:

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