No pay-off from time-off? Effects of Extra Paid Vacation for Elderly Employees*

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Abstract

This study examines the impact of an additional week of paid vacation on sickness absence and employment among Norwegian workers aged 60 and above. Utilizing a difference-in-differences-in-differences design, we exploit age-based eligibility thresholds before and after a 2009 policy change. Our findings indicate that the extra week of paid vacation 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. While the policy may improve well-being of already employed workers, it does not seem to improve labor market attachment or sickness absenteeism at the current age threshold.

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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 recreation (OECD, 2021). In nations with aging populations, paid vacation is also viewed by some as a policy instrument to enable workers to postpone retirement and maintain their attachment to the labor force Van Dalen et al. (2009).

This article addresses the question of whether extra paid vacation for 60-year old employees affects their labor market behavior. We do so by exploiting a 2009 reform that altered the timing of extra-vacation eligibility. Prior to 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 turning 60 before and after the reform, born at different times in the year, we can pin down the causal effects of extra leave on labor market outcomes at age 60.

Using detailed register data that covers five complete cohorts of workers in Norway both before and after the reform, we find no meaningful impact of extra vacation on any of our five main labor market outcomes. This is the case both in the year workers turn 60 and the year after. The results hold consistently across different samples and estimation methods, and generally do not vary among subgroups – even those for which we might have anticipated more pronounced responses.

In theory, additional paid leave can affect worker outcomes both through the the labor supply from affected workers and labor demand from firms. On the supply side, older workers who require more recovery time to remain productive, may use the extra vacation to sustain their labor supply and potentially reduce their take-up of health-related social insurance benefits. Alternatively, the extra vacation time can simply be spent on leisure or used to supplement income through secondary employment.

On the demand side, extra paid leave for older workers effectively increases the labor costs for those employees, since earnings remain 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. As most workers are unaffected, one should therefore expect that some firms employ a younger workforce. The degree to which firms do this depend both on the changes in relative prices and the flexibility in the labor market.

Overall, we expect supply-side effects—such as improving older workers' job satisfaction and encouraging them to remain employed—to push outcomes in a positive direction, while

demand-side effects—substitution away from older workers—push them in the opposite direction. Given these opposing forces, the net impact of additional paid vacation on older employees' labor market outcomes is theoretically ambiguous.

Empirical evidence on how such policies influence older employees' labor outcomes remains limited. In a Norwegian context, Hermansen (2015) used survey data on firm policies on additional leave for elderly workers and estimated a difference-in-difference model with retention as the outcome. While the study found higher retention rates in firms adopting extra vacation, these firms may also have implemented other supportive policies or adopted extra vacation endogenously, e.g. through demands from elderly workers who knew that they were going to work longer. Midtsundstad et al. (2017) analyzed the same survey data and found that the retirement rate among state employees in their 60's declined relative to employees in their 60's in other sectors in the period after state employees got the right to 8 days of extra vacation from age 62. It is hard to evaluate whether this was due to the extra vacation days or whether other factors were responsible for the differential trends in the state versus other sectors in this period. Given the costs associated with extra paid vacation and the theoretically ambiguous nature of its effects, the scarcity of rigorous empirical research in this area is noteworthy.

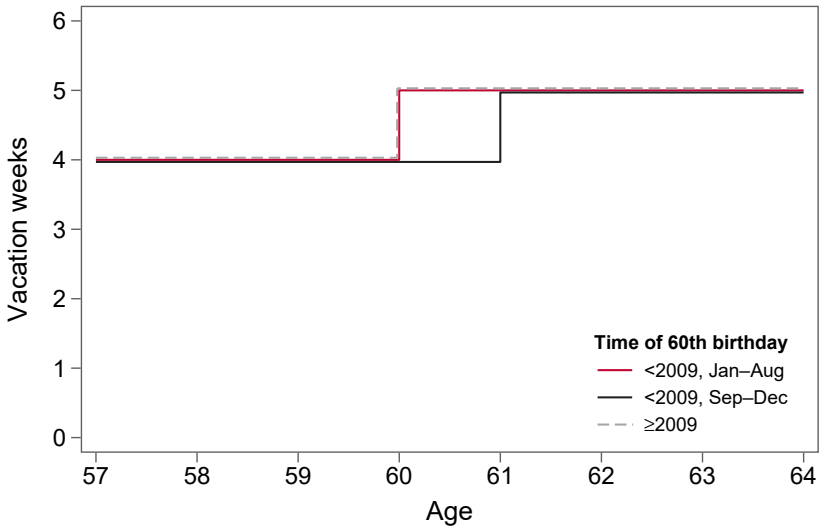
This paper provides new evidence on the causal effect of additional paid vacation on labor market outcomes for elderly workers. Previous studies have found that vacations can lead to immediate improvements in well-being, but that these tend to fade out over the course of a few weeks (de Bloom et al., 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, these findings may not generalize to older workers or those in different occupational settings.

Our study contributes to the literature by offering estimates of the effect of additional vacation at a point in the life cycle when the value of additional leave from work is believed to be the largest. Understanding these effects is crucial 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.

2 Norwegian Paid leave Policies

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

Figure 1: Illustration of the 2009 Vacation Reform



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.

[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 Data and Descriptive Statistics

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, residence area, family links, immigration status, tax returns, labor market data and data one sick leave and benefits including disability. Labor market data are primarily gathered from the employer-employee registry. This registry contains information on start and stop dates on job spells as well as corresponding labor income, occupation, sector and contracted hours. Data on sick leave and benefits include start and stop dates for spells as well as degree if relevant.

We can 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 turn 60 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] We then narrow our focus to a symmetric sample window around the vacation eligibility threshold, excluding anyone born before May 1st in any year from the sample. We also condition on individuals being registered inhabitants in all years of observation.

The main argument for an additional vacation week is to make staying in work more attractive relative to leaving the workforce for elderly workers. 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 comprise a shift from labor market earnings to one or more type of benefits. If the exit is

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

[3]The Basic Amount is a part of the Norwegian National Insurance Scheme, and is adjusted each year approximately in line with the average wage growth. In 2024, the Basic Amount was equal to NOK 124 028, or approximately EUR 10 600.

due to health related reasons, this would be disability benefits. We there define indicators for employment and disability insurance receipt as the two main outcome variables.

The other margin is degree of labor market attachment. Extra vacation may make workers less likely to cut down on work, which we measure using earnings.[4] To assess whether additional vacation provides work-related health benefits, we also examine sickness-absence days as an outcome. 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. Finally, workers may chose to increase hours with other employers. There is no oversight that ensures workers do not attend a secondary job while vacationing from another.

Any effects on labor market attachment will include potential demand side responses from firms and not separable from worker responses. For instance, some workers may be less inclined to leave the work force due to the vacation week leading to an increase in the employment rate. At the same time, firms may be less likely to employ any workers who become unemployed because of the vacation week causing a reduction in the employment rate. We will only detect the net effect of these counteracting forces.

3.2 Descriptive Statistics

Table 1 presents descriptive statistics for the pre-reform sample. All outcomes are measured in the year individuals turn 59. Corresponding statistics for the post-reform sample are reported in Appendix Table A.1. For both samples, the raw data trends from ages 52 to 61 across five labor market outcomes, are presented in Appendix Figure A.2.

4 Empirical Framework

4.1 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 can therefore be to compare individuals who are born early and late in the same calendar year across ages. The event study specification using this variation takes the form:

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

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	31.7	68.1	31.0	67.4
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.

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

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 have 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. Given a symmetrical sample window around the eligibility threshold, there is an average age difference of four months between treated and non-treated individuals in our sample. This age gap 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 can then

estimate an event study specification comparing outcomes for individuals born on or after September 1st in older cohorts to those born on or after September 1st in younger cohorts. 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, 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 have parallel trends in the absence of treatment. This may not be a credible assumption. For example, we know that younger cohorts are of better health, and 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-in-differences 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. Using this estimator we net out effects of an increasingly healthy population as well as differences in health 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:

[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.

(i) employment, defined as annual wage earnings totaling 1G or higher, (ii) annual wage earnings[6] (in G), (iii) any sickness absence days, (iv) number of sickness absence days, and (v) disability insurance receipt. 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 assumption and accommodates the possibility that effects might not materialize in the first year.[8] Standard errors are clustered at the individual level.

5 Results

5.1 Main Results

Table 2 presents the estimated impact of eligibility for the additional week of paid vacation in the year of turning 60.[9] Consider first the results in Panel A, which examine our five main outcomes. The effect of extra vacation eligibility on all five outcomes is negligible. In column (1), the triple-difference estimate on Employment is approximately zero, with 95% confidence intervals suggesting that we can rule out an impact of more than a quarter of a percentage point. In column (2), the 0.13 point estimate suggests a slight intensive margin increase of around 130 EUR in annual income. We note that this estimate is only statistically significant at the 10% level and may be due to chance. We analyze the effect on earnings in more detail below and find that additional earnings from secondary employers is the driving force behind the estimate.

There are also small or no effects on sickness absence and the probability of receiving disability insurance. For example, we can confidently rule out that receiving an additional week of vacation leads to a reduction in annual sickness absence of more than 1.5 days, ref. column (4). This is less than 6% of the average number of sickness absence days in the sample.

Figure 2 presents estimates of $\theta_{1,1,a}$ in Equation 3 across the full range of ages 52 to 61. Age 59 serves as the reference category. This presentation enables us to assess the possibility that effects do not materialize until the subsequent year. We start by observing that the

[6] Wage income is censored 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] Estimating effects further into the future is not feasible due to Norway's 2011 pension reform, which comprehensively altered retirement and labor market participation incentives. In addition, 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.

[9] Table 2 reports only the main interacted 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 the two estimation strategies specified by Equations 1 and 2 in Appendix Table A.3.

differences in trends are statistically indistinguishable from zero in all pre-treatment periods, supporting our identifying assumption. There is also no evidence that the effects differ in the subsequent period, with even the marginally statistically significant effect on income observed at age 60 eliminated at age 61.

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	0.1156	0.0002
	(0.0010)	(0.0579)	(0.0033)	(0.4776)	(0.0011)
Age = 60 × Reform	0.0068***	0.3000***	0.0023	-0.2839	-0.0020*
	(0.0010)	(0.0613)	(0.0032)	(0.4656)	(0.0011)
Age = 60 × Late × Reform	0.0003	0.1346*	-0.0035	-0.2270	-0.0022
	(0.0014)	(0.0786)	(0.0045)	(0.6551)	(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	25.52	0.07

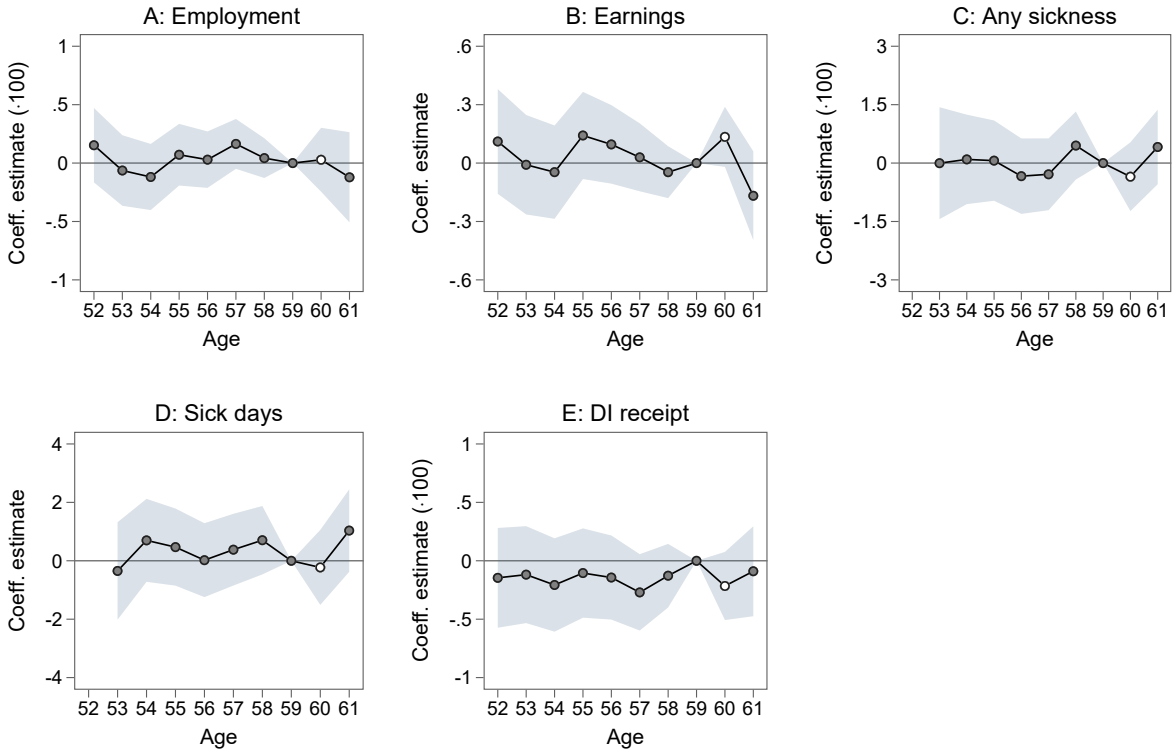
B: Additional outcomes	(1) New main employer	(2) Multiple employers	(3) Primary earnings	(4) Suppl. earnings
Age = 60 × Late	0.0005	0.0009	-0.0522	-0.0709*
	(0.0019)	(0.0023)	(0.0986)	(0.0407)
Age = 60 × Reform	0.0071***	-0.0000	0.4826***	-0.0825*
	(0.0020)	(0.0023)	(0.0978)	(0.0428)
Age = 60 × Late × Reform	-0.0016	-0.0006	0.0414	0.0992*
	(0.0028)	(0.0032)	(0.1329)	(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	0.19	42.99	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.*

5.2 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

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.*

effects, are reported in Appendix Table A.4 and Appendix Figure A.4. This does not affect results. Second, Appendix Figures A.5 and A.6 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 consistent regardless of bandwidth used.

5.3 Other Outcomes

Additional paid leave is an expense for employers, who have to pay the same amount per worker, while workers reduce their yearly work hours. As such, the additional paid leave will cause some workers to cost more than their marginal product in a competitive market. One may therefore expect some workers to change their main employer. We test this hypothesis by estimating our main model using a dummy for changing main employer as an outcome.

The result is reported in the first column of Panel B in Table 2. We see no effects on this margin, and the point estimate is actually of opposite sign that what theory predicts for firm responses.

Employers are required by law to make sure all employees spend all their mandated days of paid leave. However, workers may choose to spend the additional leave working for other employers. Columns (2) and (3) of Panel B in Table 2 show that additional paid leave does not cause any change in whether workers have multiple employers or whether they change their working hours at the intensive margin with their main employer.

Column (4) suggests that workers slightly increase their earnings through secondary employers, with the effect roughly matching our main earnings estimate. This pattern suggests that employees holding multiple jobs boost their hours in secondary roles. While this is not inconsistent with some workers prolonging their labor market attachment or abstaining from taking sick leave or benefits, it indicates that some of the affected workers opt to use leave to secure additional income rather than increasing rest. For these workers, extra paid leave appears less valuable than higher earnings. In light of the policy's goal to provide sufficient rest, this finding alone raises doubts about the effectiveness of the extra week of vacation.

5.4 Heterogeneous Effects

Even if there are no overall effects of the extra vacation on labor market outcomes, there may still be individual differences that are masked in the full sample. For example, it may be that individuals in physically demanding labor categories benefit more from the policy than others. Table 3 presents main effects by occupation category. [10] We exclude individuals with missing occupations or categories with a low number of observations (<1% of the sample). Each coefficient represents a separate linear regression of Equation 3, conducted on the subsample of employees belonging in the respective occupation category at age 59.

The results reveal few clear patterns based on occupation. Interestingly, the borderline significant effect on earnings in our main results are here statistically significant only for 'Service and sales workers', an occupation group where it is relatively easy to adjust hours. For 'Managers', the results indicate a substitution from disability to employment with more sick leave. At the same time we find reduced employment rates for 'Professionals' and 'Plant and machine operators and assemblers'. Reduced employment goes against the economic incentives for workers but are in line with firm incentives. However, the share of workers who are employed at 59 and need to find new work at age 60 is very low and unlikely to drive these estimates. With this many estimates, one should be cautious in over-interpreting all

[10] Occupation is based on the Norwegian STYRK-98 classification at the 1-digit level. STYRK-98 is an adaptation of ISCO-08.

of them, as some are likely to show up as statistically significant due to chance. The bottom row reports a the p-value from a test of equal effects for each of the five outcomes. The statistic is obtained from a saturated linear regression with all occupation groups interacted. Only for the first outcome is the null hypothesis of equal effects rejected.

Appendix Table A.5 additionally investigates heterogenous effects across the dimensions income, gender and sector of employment. There is no evidence that effects vary systematically across any of these categories. The effect on earnings only show up for workers paid less than the median.

6 Discussion

A policy of increased mandated paid leave has unclear effects ex ante, as incentives for workers and firms should induce opposite effects on employment. While the policy aims to enhance the situation of older workers, it imposes costs on employers. Providing an extra week of paid vacation increases the labor costs associated with employing older workers. This makes older employees more expensive relative to their younger counterparts, potentially discouraging employers from hiring or retaining them. Such unintended consequences could offset any intended benefits of the policy, particularly if employers adjust by reducing other forms of compensation or limiting employment opportunities for older workers.

Our findings suggest that the additional week of paid vacation does not have a significant impact on labor market attachment for elderly workers in the first calendar year they receive it. Rather, we find some evidence that earnings increase when workers are given additional paid leave. We trace this to increased labor supply at the intensive margin among workers with secondary employers. It therefore appears that, if anything, some workers use additional paid leave as a work subsidy to increase supplemental earnings, revealing that they prefer increased income to paid leave. Consistent with this, we find suggestive evidence of increased earnings among low-paid workers and workers in service and sales occupations.

While our identification strategy only allow us to estimate effects in the relatively short run, 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 age 60 or 61. We find some significant results for particular sub-populations, but we generally cannot rule out that these are under-rejection when considering estimates jointly. The results aligns with the results of Hofmarcher (2021), who also found negligible effects 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

Table 3: Effects of Extra Paid Vacation by Occupation

	(1) Employed	(2) Earnings	(3) Sickness	(4) Sickdays	(5) DI receipt
Managers [293, 098]	0.0062**	0.3995	0.0143	3.6259**	-0.0070**
	(0.0029)	(0.3148)	(0.0123)	(1.5231)	(0.0033)
Professionals [366, 825]	-0.0063**	-0.2939	-0.0072	1.3049	-0.0024
	(0.0026)	(0.2284)	(0.0114)	(1.4370)	(0.0030)
Technicians and associate professionals [567, 191]	0.0019	0.2204	-0.0038	-0.9121	-0.0010
	(0.0026)	(0.1719)	(0.0095)	(1.3092)	(0.0028)
Clerical support workers [226, 081]	-0.0002	-0.1329	-0.0114	-0.1104	-0.0067
	(0.0047)	(0.1957)	(0.0150)	(2.1553)	(0.0048)
Service and sales workers [501, 675]	0.0013	0.3150***	-0.0005	-1.2922	0.0015
	(0.0036)	(0.1153)	(0.0102)	(1.6491)	(0.0038)
Craft and related trades workers [190, 747]	0.0039	0.2156	-0.0020	0.4028	-0.0010
	(0.0056)	(0.2828)	(0.0167)	(2.5235)	(0.0062)
Plant and machine operators and assemblers [195, 509]	-0.0123**	0.0318	-0.0210	-2.7620	-0.0028
	(0.0061)	(0.2883)	(0.0164)	(2.5763)	(0.0063)
Elementary occupations [116, 968]	0.0106	0.2043	-0.0128	-3.5266	-0.0025
	(0.0087)	(0.2680)	(0.0213)	(3.5490)	(0.0089)
Equal effects (p-val.):	0.02	0.26	0.79	0.21	0.77

Notes: *Each cell represents a separate regression of Equation 3, conducted on different subsamples defined by occupation category at age 59. The number of observations 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. The p-value from a test of equal effects, derived from a saturated regression with all occupation codes interacted, is provided in the bottom row. Occupation category is defined according to the Norwegian 1-digit STYRK-98 classification. 1012 individuals belonging to occupations 'skilled agricultural, forestry, and fishery workers', 'armed forces', or those with missing or undisclosed occupations, are omitted from the sample due to their low number of observations. Standard errors clustered at the individual level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.*

health benefits, are not captured in our analysis. Future research could explore longer time horizons to assess the sustained impact of additional paid vacation.

This study examines the effects of an extra week of paid vacation for Norwegian workers aged 60 and above. Our analysis finds small and statistically insignificant effects on employment, earnings, sickness absence, and disability insurance receipt in the year of eligibility for the extra week of vacation.

While the policy aims to improve well-being and prolong labor force participation among

older workers, our findings suggest that its immediate impact is limited. As life expectancy has increased, many age thresholds in the pension system have been recalibrated. Applying a similar approach to the age cutoff for extra vacation could ensure that its benefits target those who need it most, while avoiding costly distortions. By adapting the threshold in line with improvements in longevity and health, policymakers could maintain the original intent of preserving 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 the aging workforce.

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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	30.1	66.4	29.6	65.9
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**	-0.7915**	-0.0048***
	(0.0000)	(0.1439)	(0.0028)	(0.3903)	(0.0017)
Reform	0.0008	-2.5529	-0.0557**	-7.6586***	-0.0113
	(0.0069)	(1.8886)	(0.0216)	(2.2644)	(0.0164)
Age = 60	-0.0348***	-2.0788***	-0.0101***	-0.2908	0.0232***
	(0.0011)	(0.2250)	(0.0034)	(0.4231)	(0.0021)
Late × Reform	-0.0000*	0.2989	0.0045	0.3462	0.0032
	(0.0000)	(0.2057)	(0.0040)	(0.5388)	(0.0023)
Age = 60 × Late	-0.0006	-0.0411	0.0010	0.1156	0.0002
	(0.0010)	(0.0579)	(0.0033)	(0.4776)	(0.0011)
Age = 60 × Reform	0.0068***	0.3000***	0.0023	-0.2839	-0.0020*
	(0.0010)	(0.0613)	(0.0032)	(0.4656)	(0.0011)
Age = 60 × Late × Reform	0.0003	0.1346*	-0.0035	-0.2270	-0.0022
	(0.0014)	(0.0786)	(0.0045)	(0.6551)	(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	25.52	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**	0.5050	0.0252***
	(0.0008)	(0.0695)	(0.0024)	(0.3468)	(0.0010)
Late	0.0000	-0.0566	-0.0070**	-0.7941**	-0.0048***
	(0.0000)	(0.1439)	(0.0028)	(0.3903)	(0.0017)
Age = 60 × Late	-0.0005	-0.0402	0.0010	0.1208	0.0002
	(0.0010)	(0.0579)	(0.0033)	(0.4776)	(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	27.00	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**	-0.3724	0.0226***
	(0.0014)	(0.3228)	(0.0043)	(0.5144)	(0.0029)
Reform	0.0150	-2.8900	-0.0431	-8.5208***	-0.0144
	(0.0101)	(2.7253)	(0.0310)	(3.2455)	(0.0237)
Age = 60 × Reform	0.0067***	0.4829***	-0.0019	-0.5708	-0.0042***
	(0.0011)	(0.0698)	(0.0034)	(0.4940)	(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	25.27	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	0.0958	0.0002
	(0.0011)	(0.0605)	(0.0035)	(0.5077)	(0.0012)
Age = 60 × Reform	0.0067***	0.2782***	0.0020	-0.3188	-0.0022*
	(0.0011)	(0.0640)	(0.0034)	(0.4950)	(0.0012)
Age = 60 × Late × Reform	0.0003	0.1298	-0.0033	-0.2092	-0.0021
	(0.0015)	(0.0822)	(0.0048)	(0.6964)	(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	25.52	0.07

B: Additional outcomes	(1) New main employer	(2) Multiple employers	(3) Primary earnings	(4) Suppl. earnings
Age = 60 × Late	0.0006	0.0009	-0.0433	-0.0715*
	(0.0020)	(0.0024)	(0.1037)	(0.0429)
Age = 60 × Reform	0.0072***	-0.0001	0.4594***	-0.0819*
	(0.0021)	(0.0024)	(0.1024)	(0.0451)
Age = 60 × Late × Reform	-0.0016	-0.0006	0.0379	0.0979
	(0.0030)	(0.0034)	(0.1396)	(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	0.19	42.99	2.63

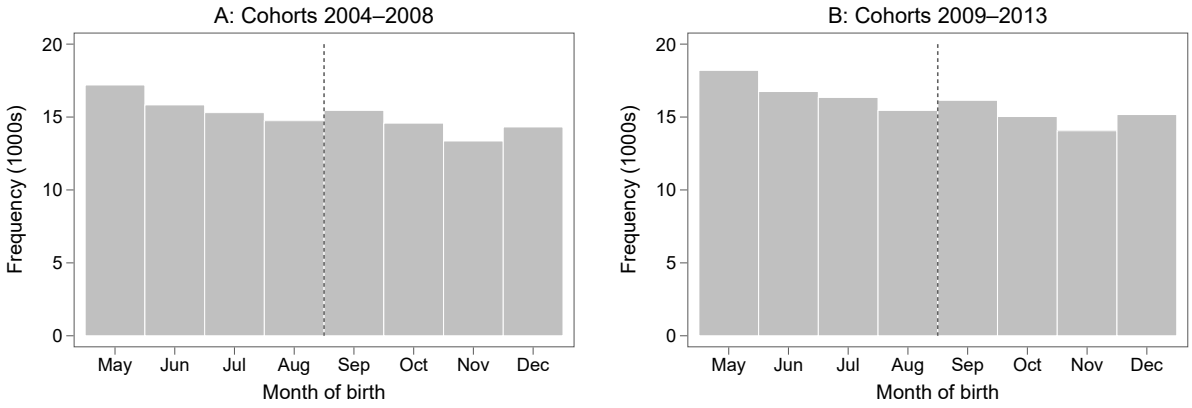
Notes: *Each column represents a separate regression of Equation 3. Y is as denoted by the column headers. Only the interaction effects at age 60 are reported. All models substitute the birth year fixed effects in Equation 3with individual-level fixed effects. Standard errors clustered at the individual 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
Below median [1, 233, 747]	-0.0013	0.2205***	-0.0081	-1.0076	-0.0023
	(0.0024)	(0.0812)	(0.0065)	(1.0355)	(0.0024)
Above median [1, 233, 653]	0.0015	0.0702	0.0008	0.5165	-0.0017
	(0.0015)	(0.1336)	(0.0063)	(0.8029)	(0.0018)
Equal effects (p-val.):	0.30	0.34	0.32	0.24	0.83
B: By gender	(1) Employed	(2) Earnings	(3) Sickness	(4) Sickdays	(5) DI receipt
Women [1, 188, 884]	-0.0002	0.0794	-0.0059	-0.4365	0.0009
	(0.0021)	(0.0768)	(0.0067)	(1.0179)	(0.0023)
Men [1, 279, 247]	0.0007	0.1931	-0.0013	-0.0386	-0.0049**
	(0.0019)	(0.1334)	(0.0061)	(0.8388)	(0.0020)
Equal effects (p-val.):	0.76	0.46	0.61	0.76	0.05
C: By sector, age 59	(1) Employed	(2) Earnings	(3) Sickness	(4) Sickdays	(5) DI receipt
Public [1, 040, 982]	-0.0006	0.0246	-0.0069	-1.3050	0.0001
	(0.0019)	(0.0816)	(0.0070)	(1.0378)	(0.0022)
Private [1, 427, 149]	0.0009	0.2197*	-0.0013	0.5514	-0.0038*
	(0.0020)	(0.1219)	(0.0059)	(0.8438)	(0.0020)
Equal effects (p-val.):	0.59	0.18	0.54	0.17	0.19

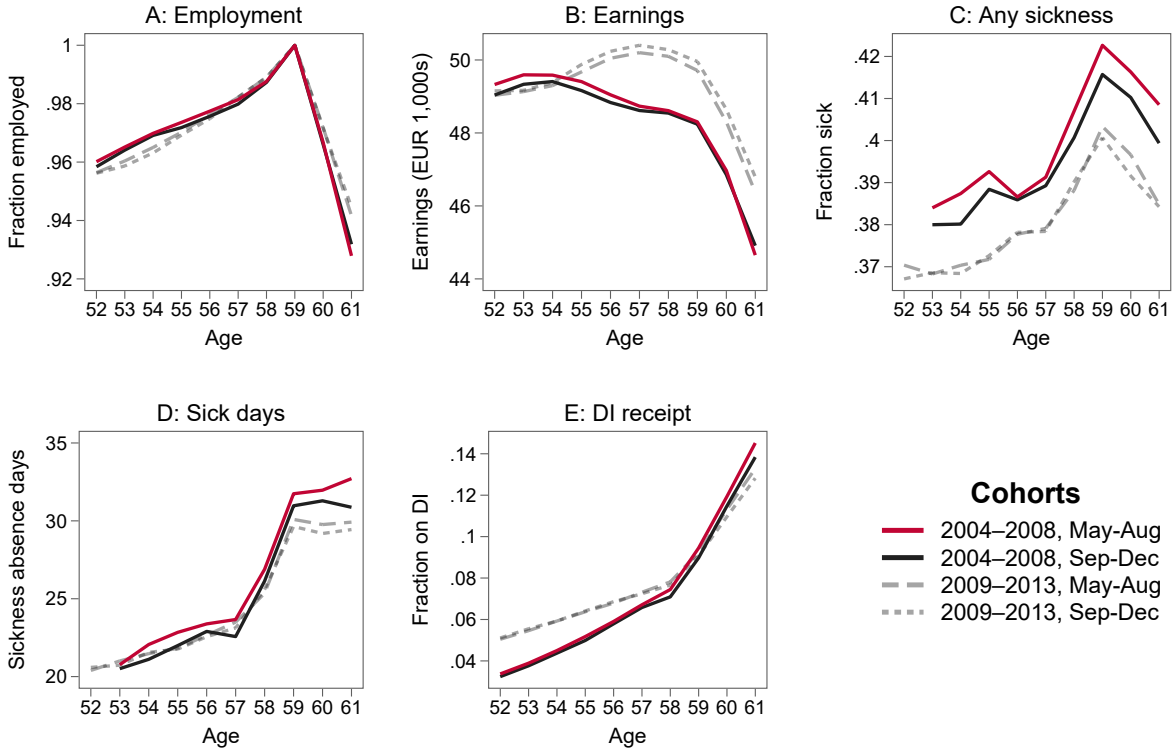
Notes: *Each cell represents a separate regression of Equation 3, conducted on different subsamples defined according to the criteria to the left. The number of observations in each subsample is reported in brackets. Y is as denoted by the column headers. Only the triple interaction effects at age 60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



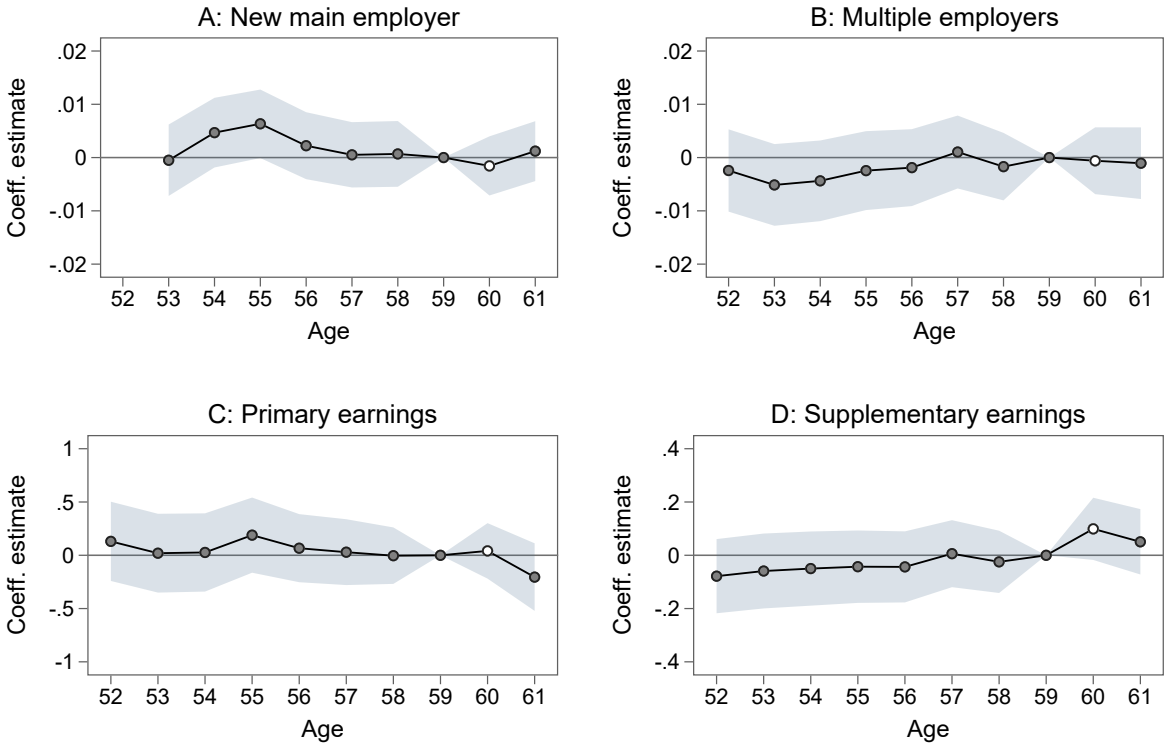
Notes: *Each panel depicts the sample distribution by month of birth, respectively for the pre-reform (turns 60 in 2004–2008) and post-reform (turns 60 in 2009–2013) cohorts. The vertical dashed line denotes the cutoff for extra vacation eligibility prior to the 2009 reform.*

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



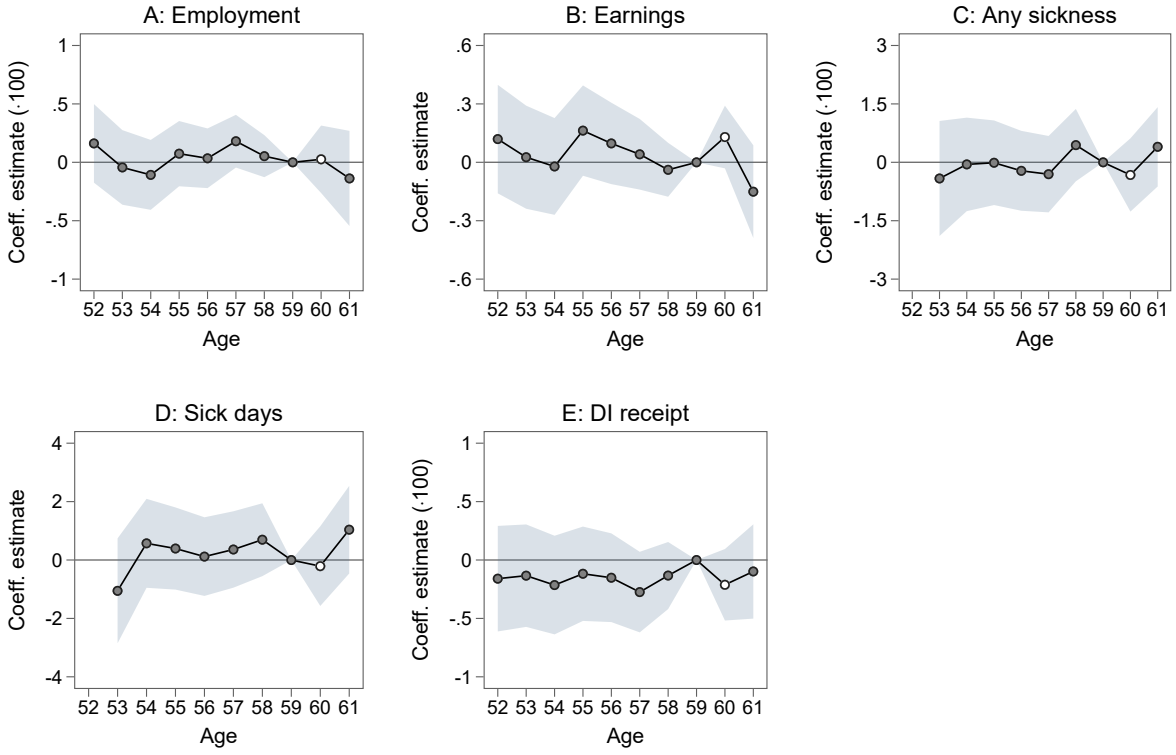
Notes: *Each panel depicts the average outcome across ages 52 to 61. Red and black solid lines represent individuals turning 60 in May–August and September–December in 2004–2008, respectively. Dashed and short-dashed gray lines represent those turning 60 in the same months of 2009–2013, respectively.*

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



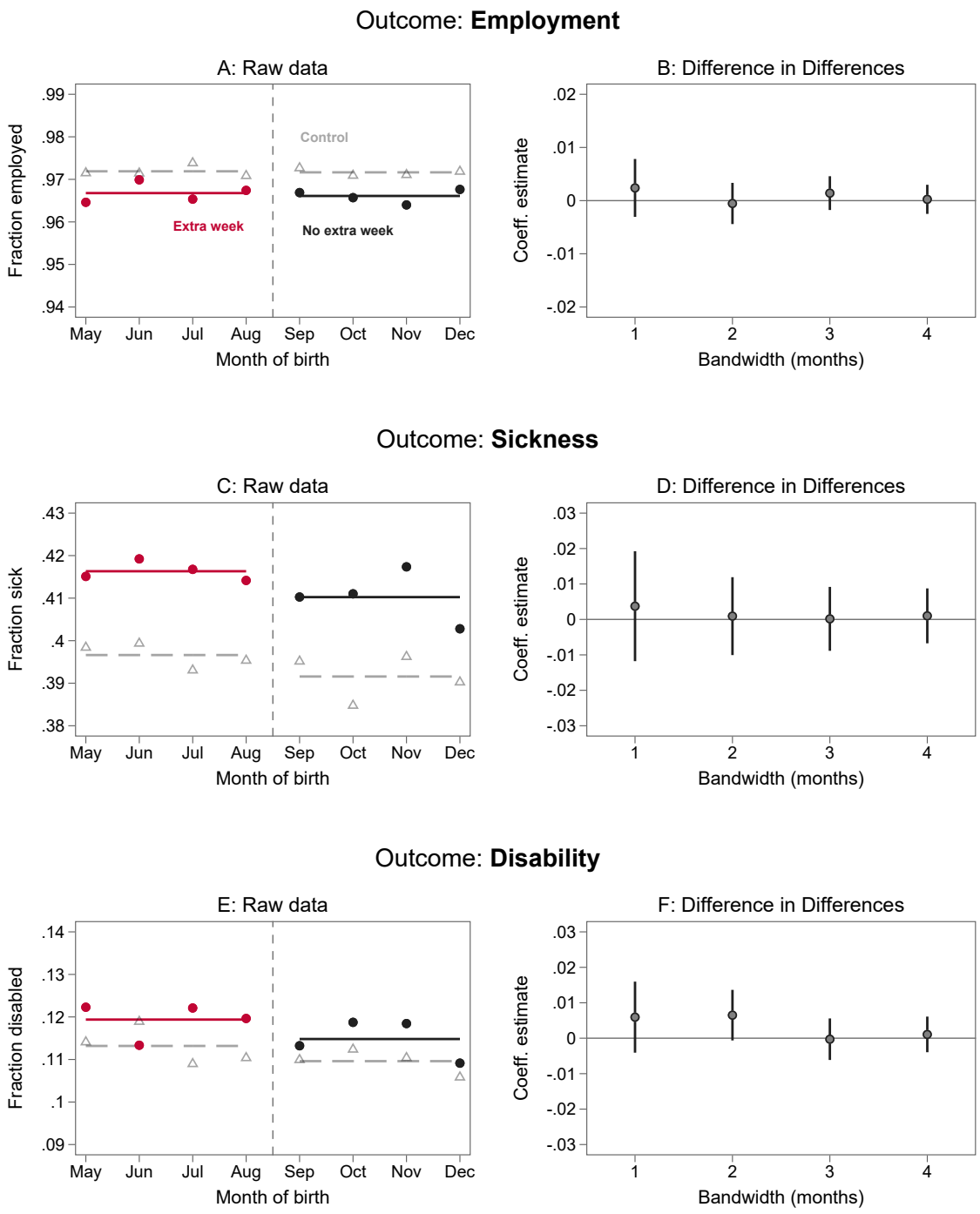
Notes: *Each panel presents coefficient estimates of $\theta_{1,1,a}$ in Equation 3 for individuals aged 52 to 61. Age 59 serves as the reference category. White-filled coefficients denote estimates of $\theta_{1,1,60}$. Shaded areas represent 95% confidence intervals. Standard errors are clustered at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

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



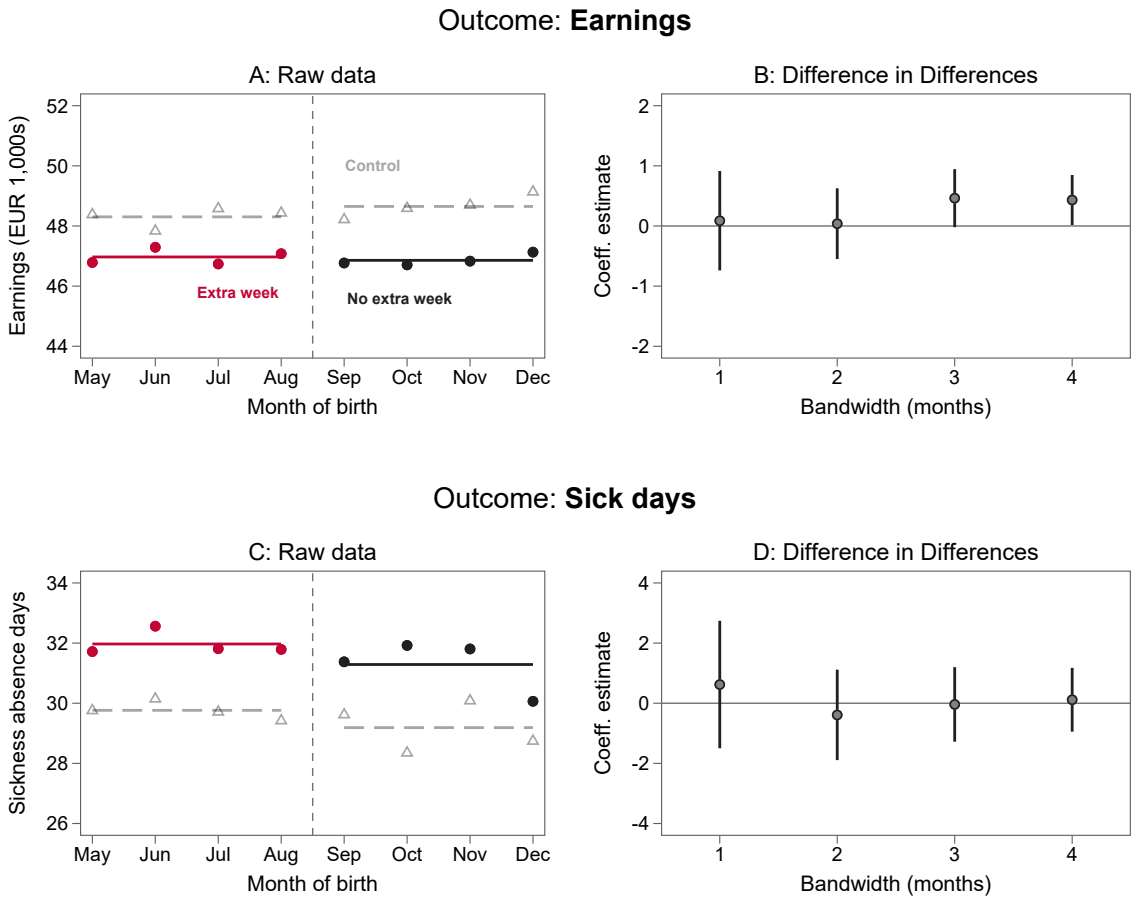
Notes: *Each panel presents coefficient estimates of $\theta_{1,1,a}$ in Equation 3 for individuals aged 52 to 61. Age 59 serves as the reference category. White-filled coefficients denote estimates of $\theta_{1,1,60}$. Shaded areas represent 95% confidence intervals. All models substitute the birth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

Figure A.5: 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.6: 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.*