Supplemental Appendix

Deadwood Labor? The Effects of Eliminating Employment Protection for Older Workers

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A Additional Model Discussion

Dynamic considerations. Our model above is written in a quasi-static way, with treatment and control groups. In practice, we have treatment and control *ages*, and we focus on excess separations occurring at the narrow focal ages where workers lose employment protection.

Formally, we can represent this dynamic context by considering four ages $a = 0, 1, 2, 3$, and measure separations between age pairs. Age 0 is our background age. Since firing costs $f_0 = f_1$ do not change between ages 0 and 1, separations between those two periods emerge only because of shifts in the job's non-regulatory fundamentals $(S^W, \widetilde{S}^F)$. We think of these types of baseline separations as "normal churn," and they correspond to our control period. Age 2 is our treatment age, when employment protection is eliminated, and hence $f_2 = 0$. Comparing separations at age 1 (control age) with those at age 2 (treatment age) identifies the treatment effect of the elimination of f on separations in the form of *excess separations*. Identifying those excess separations at age 2 is the main focus of our paper. Finally, at age 3, the firing cost in our context persistently stays eliminated, and hence $f_3 = 0$, so that the third period provides either a window into persistent (compositional) effects or another control group.

In principle, employment protection may have dynamic effects before and after the anticipated elimination of employment protection cost f. First, employers may retime separations to earlier ages, strategically delaying separations until after f drops to zero (i.e., separations from age 0 to 1 should be lower than those from another age -1 to 0). Our results do not suggest such dynamics, as we do not find declines in separations right before the elimination of employment protection—perhaps because of imperfectly persistent surplus shocks or due to employment protection rules, including advance notice. When we exploit a reform that shifts the age cutoff for employment protection in an empirical extension, we also do not see any response to separations prior to the age cutoff.

Second, separations after, rather than before, the employment protection cutoff age may also be affected. On the one hand, in a given job, surplus will stay persistently lower, increasing the chances that surplus shocks end in a dismissal. On the other hand, exactly in the presence of an initial spike in separations, positive composition effects among surviving jobs may curb separations. Our analyses of raw data do not suggest clear evidence for such a level shift in separations following the elimination of employment protection.[30]

[30] Modeling the law as $f_3 > 0$, i.e., a window to dismissal individuals (see Section II.B for the discussion of the regulatory context), would generate additional dynamic incentives to dismiss the worker in period 2

Third, employment protection rules typically strengthen in tenure, and hence employment protection *differentials* between the original job and the potential next job will generate strong surplus to the worker, who enjoys maximal protection in the current job. This dynamic will curb quits (in the form of job-to-job transitions) up to 67 (see Gielen and Tatsiramos, 2012, for cross-country evidence on this mechanism), but this differential will disappear as a consideration after 67, as employment protection is eliminated in *both* the current job and subsequent new jobs. Below, we also preview that most of the excess separations will go into permanent nonemployment (retirement), which we discuss next.

Separations into retirement vs. to other employers. There are two cases to distinguish regarding the worker's trajectory following the layoff. The worker may move to another employer, or leave the labor force and retire. Ignoring search frictions, a worker would move to the next job that gives her the highest worker surplus $S^{W\prime} = \text{argmax}_{j \in J | \widetilde{S}_j^F \geq 0} S_j^W$, where the set of jobs J is defined as those that also fulfill the participation constraint of the next employer. If that job gives the worker positive surplus, she will accept it and be employed. If the best job offer does not make the cut, the worker will go into retirement.

To the degree that wages in the next job can be set flexibly, separations into retirement following the elimination of employment protection hence raise a stronger notion of deadwood: the worker's reservation wage exceeds his productivity everywhere.[31]

In the data, we will find that most workers that our strategy identifies as separating due to the elimination of employment protection experience permanent nonemployment, i.e., retirement. These results therefore do not speak to the role of employment protection for a broader set of workers at younger ages, who will be more likely to go back into reemployment following a dismissal.

Wage effects, and flexible wages. We briefly also discuss a notion of deadwood absent wage rigidities—although our empirical analysis finds no evidence of wage adjustment as workers age out of employment protection. An alternative model would assume flexible wages in the original job. However, this mechanism is difficult to conceptualize in a realistic way exactly because firing costs f are only due upon a one-sided (firm-initiated) separation in practice. Some notion of wage rigidity or bargaining friction is needed as otherwise the parties could always eliminate f if the worker were willing to agree to label the separation as a quit, in exchange for a side payment (see Carry and Schoefer, 2024, for a direct empirical assessment of such mechanisms in another context). To understand the initial layoffs, we therefore favor the previous setup, with fixed wages and a clear notion of a layoff, to read the evidence.[32]

in the presence of shocks between period 2 and 3.

[31] It is possible that wage rigidity is active also in newly formed jobs (e.g., due to regulatory wage floors), which may lead the worker to retire in an involuntary sense (i.e., she would accept the wage if a firm were willing to employ her); wage rigidity may also mean that her productivity does exceed her reservation wage.

[32] Under efficient bargaining of wages, the firm and worker find a wage within the bargaining set of the parties' reservation wages (respectively defined as the wage that would make each party's participation constraint hold with equality) to avoid an inefficient separation. Viable jobs have then gross-of-employment protection *joint* surplus (the sum of worker and firm surplus, with the bilaterally efficient wage cancelling out) above the firing costs: $\widetilde{S} = (J^W - O^W) + (J^F - O^F) \geq -f$. In this setting, employment protection fosters a notion of bilaterally efficient deadwood: jobs for which $-f \leq \widetilde{S} < 0$, i.e., that are viable (carry weakly positive net surplus) only because of the presence of employment protection but would have negative

Extensive margin and intensive margin adjustments. Our model features an extensive margin only, with no room for adjusting or rebargaining hours or other aspects of the job. When wages are rigid, firms may be able to bargain for higher effort or lower hours (under diminishing marginal products) when firing costs fall. Or, only some tasks of the worker may carry positive gross surplus to the firm, and the firm may effectively dismiss the worker and rehire the worker for only this subset of her tasks. Indeed, such patterns occur in Sweden in universities (as we discuss below), where a professor crossing the employment protection cutoff age may cease to be paid for research activities but paid for teaching courses on a case-by-case basis when recalled after retirement. One can view those intensive-margin adjustments as revealing deadwood labor units, and we document substantial evidence for their relevance in Sweden, in the form of hours and earnings reductions (at fixed wages) among continuing workers as they lose employment protection.

B Regression-Based Heterogeneity Analysis

Regression analysis. In the 2019 monthly micro-data, we define a separation indicator $s_{ima} = 0, 1$ for individual i observed in month m separating at age a (in months) for the sample of individuals who were working in the previous month. Each individual is observed up to 12 times in 2019 data depending on how many months she works during 2019. For the univariate analysis, we regress s_{ima} on monthly age dummies and monthly age dummies interacted with the dummy variable D_{ima} (being a public sector worker when we analyze public vs. private sector workers for example) as follows:

$$\text{Univariate specification: } s_{ima} = \alpha_a + \beta_a \cdot D_{ima} + \varepsilon_{ima}. \tag{A.1}$$

We then conduct the bunching analysis on the basis of the interaction-age coefficients—with the same age windows. We compute standard errors in the same way of for our benchmark excess separation estimates as described in Figure 7. This differential bunching analysis essentially formalizes the estimation of the contrast in excess separations across two groups that we discussed in Figures 11 and 12 at the end of Section IV.B above.

An important issue is that the various dimensions of heterogeneity might be correlated. For example, public sector workers may have longer tenure on average than private sector workers and the greater excess separations in the public sector might simply be a consequence of longer tenure and not public sector per se. To address this issue, we extend our univariate method to a multivariate approach that measures excess separations when a specific dummy is switched on while controlling for the other variable dummies as follows. That is, the multivariate regression follows the model in Equation (A.1) but regresses s_{ima} on monthly age dummies α_a and monthly age dummies interacted with all dummy variables of interest $D^1_{ima}, D^2_{ima}, ...$:

$$\text{Multivariate specification: } s_{ima} = \alpha_a + \beta^1_a \cdot D^1_{ima} + \beta^2_a \cdot D^2_{ima} + .. + \varepsilon_{ima}. \tag{A.2}$$

surplus absent employment protection—and hence separate when employment protection is eliminated.

Again, we conduct the bunching analysis and report excess separation effect estimates on the interactions of a given heterogeneity variable with the age coefficients.

Results. We summarize our heterogeneity analysis in Appendix Table A.1. This table lists in each row a specific characteristic (e.g., sick ≥ 3 weeks). The first column "Share group" displays the fraction of our estimation sample with the characteristic. The second column reports the excess separation estimate for the group with the characteristic. The third column reports the excess separation for the complement (i.e., individuals without the characteristic). The difference between columns 2 and 3 is reported in column 4. This is the univariate difference from the specification in Equation (A.1). The last column reports the additional excess separation when the dummy variable is equal to one (relative to the dummy variable equal to 0) when controlling for all the other dummy variables listed in the table as in the multivariate specification in Equation (A.2).

Appendix References

Carry, Pauline and Benjamin Schoefer. 2024. "Conflict in Dismissals: Evidence from "Separations by Mutual Agreement" in France." Working paper.

Gielen, Anne and Konstantinos Tatsiramos. 2012. "Quit Behavior and the Role of Job Protection." *Labour Economics* 19 (4): 624-632.

OECD. 2022. *OECD Reviews of Pension Systems: Slovenia*, OECD Reviews of Pension Systems, OECD Publishing, Paris, `https://doi.org/10.1787/f629a09a-en`.

Table A.1: Excess Separation Estimates: Heterogeneity

	Share in group	Excess Separations Group	Excess Separations Complement	$\Delta_{\text{univariate}}$	$\Delta_{\text{multivariate}}$
	(1)	(2)	(3)	(4)	(5)
Sick > 3 weeks	0.094	0.199	0.079	0.120	0.103
		(0.016)	(0.004)	(0.017)	(0.016)
High tenure	0.724	0.099	0.019	0.079	0.053
		(0.004)	(0.009)	(0.009)	(0.010)
Firm > 10 empl.	0.809	0.097	0.021	0.076	0.044
		(0.004)	(0.009)	(0.010)	(0.012)
High earner	0.364	0.114	0.054	0.061	0.041
		(0.005)	(0.006)	(0.008)	(0.009)
Public sector	0.471	0.108	0.050	0.058	0.037
		(0.005)	(0.006)	(0.008)	(0.010)
Immigrant	0.151	0.112	0.072	0.039	0.043
		(0.010)	(0.004)	(0.011)	(0.011)
Manufacturing	0.048	0.102	0.077	0.024	0.031
		(0.016)	(0.004)	(0.018)	(0.019)
High education	0.445	0.085	0.073	0.012	-0.011
		(0.006)	(0.005)	(0.008)	(0.009)
Male	0.494	0.081	0.076	0.005	0.020
		(0.006)	(0.005)	(0.008)	(0.009)
Observations				387,858	359,632

Notes: This table shows excess separation estimates by various subgroups, displayed in rows in column 1. All heterogeneity analysis are based on binary variables, and column 2 reports the share of the estimation sample for whom the binary variable equals one. For example, in our estimation sample around age 67, 9.42 percent of the workers experienced a sick leave of more than three weeks in 2018 (and hence 90.58 percent did not). In columns 3 and 4, we report the excess separation estimates separately for the target group (those with the value one on the dummy variable; e.g., the sick) and its complement (e.g., the non-sick). These bunching estimates are obtained just as in the baseline analysis described in Figure 7. In column 5, we report the coefficient difference between the target group and the complement group with its associated standard error. This is the univariate difference from specification (A.1). For example, sickly workers experience an additional excess separation probability at age 67 of 12.0 percentage points relative to other workers (19.9 percent vs. 7.9 percent). Column 6 estimates report the same differences in excess separations but controlling for all other dummy variables listed in all rows as in specification (A.2). In the case of sickness status, controlling for all the other variables reduces slightly the differential excess separation of sickly workers from 12 points down to 10 points. The sample in the last column differs slightly from the main analysis sample, because we remove observations where some characteristics cannot be uniquely determined.

C Additional Results

Summary statistics. Appendix Table A.2 shows summary statistics for the population as a whole in Panel A and for workers in Panel B for 2019.

All statistics are computed for the year 2019 and in Panel B a worker is defined as having positive earnings during the year. The first column shows averages among workers aged 25-61, which represent the working-age population outside of our analysis. The second column shows corresponding means for ages 62-70, our baseline sample used in most of our graphical analysis. The third column zooms in on individuals around the 67 age threshold (a fifteen month age window from 67 minus 7 months to 67 plus 7 months), the age window of the sample we use to estimate excess separations in our bunching methodology introduced below. Overall, the 62 to 70-year-olds are less likely to be working: 37 percent vs. 78 percent for the younger group. Around age 67, the fraction working is 25 percent. Correspondingly, unconditional wage earnings are substantially lower for the older groups. However, when conditioning on working in Panel B, the difference in earnings between older and younger cohorts decreases. The remaining difference is mainly driven by an intensive-margin hours difference. Around 56 percent of older workers aged 62-70 work full-time and only 27 percent do so around age 67, in contrast to the younger workers for whom the average is around 78 percent. Full-time-equivalent monthly wages among older workers are slightly higher so that the differences in earnings are due to labor supply both along the extensive and intensive margins. The elderly naturally have longer tenure and are slightly more likely to work in the public sector. The two groups score quite similarly in terms of other demographic characteristics, with the exception that older workers are less likely to be immigrants.

Table A.2: Summary Statistics in Year 2019

	Working Age Ages 25-61 (1)	Graph sample Ages 62-70 (2)	Estimation sample 15 months around 67 (3)
Panel A: **All individuals**			
Share working	0.78	0.37	0.24
Years of education	12.61	11.81	11.78
Annual wage earnings	328.37	131.98	65.83
Age	42.51	65.96	67.04
Individuals	4,877,308	990,200	233,797
Panel B: **Working individuals**			
Annual wage earnings	384.80	273.26	177.52
Monthly wage (FTE)	35.24	36.44	37.03
Share full-time	0.78	0.56	0.27
Years of education	12.86	12.31	12.43
Tenure (years)	7.43	12.46	11.68
Public sector	0.41	0.47	0.44
Manufacturing	0.11	0.09	0.06
Share women	0.49	0.49	0.49
Share immigrants	0.25	0.16	0.15

Notes: This table shows summary statistics for all individuals in Panel A and for working individuals in Panel B. All statistics are computed for the year 2019 and in Panel B a worker is defined as having positive earnings during the year. The first column shows averages among individuals aged 25-61, which represent the working-age population outside the scope of our analysis. The second column shows corresponding means for ages 62-70, our baseline sample used in our graphical analysis. The third column zooms in on individuals around the 67 age threshold (a fifteen month age window from 67 minus 7 months to 67 plus 7 months), which corresponds to the sample we use to estimate excess separations in our bunching methodology (described in Figure 7 and applied throughout). Note that the administrative data include 12 monthly earnings observations for each individual. The number of individuals is reported in the table for each sample. The first row—Share working—is defined as follows. For each individual, we compute the share of months during the calendar year that the individual has positive earnings and we take the average of this share across all individuals in the corresponding age sample. Wage earnings in the table are reported at the annual level (not monthly) while wages (FTE) represent the monthly full-time equivalent wage, measured using the Structure of Earnings Survey (SES) in one month (typically October or November). Share full-time is also measured in the SES. The number of observations that we observe monthly wages from the 2019 SES in the samples from left to right are 2,190,293; 188,491 and 21,312, respectively. Monetary values are expressed in nominal 1000 SEK (with $1 = 10 SEK approximately). Tenure is the number of years the individual has worked with the main employer (i.e., the one with the highest earnings) of 2019.

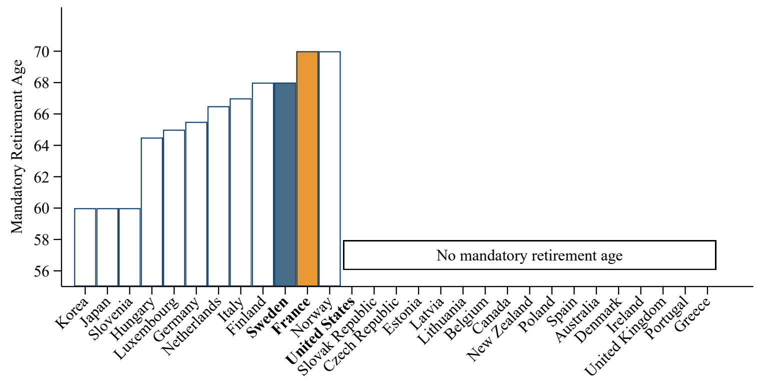
Table A.3: Excess Separation Estimates

	2019 (1)	2020 (2)	2021 (3)	2022 (4)
Panel A: **Threshold age 67**				
Excess separation	**0.084**	0.038	0.027	0.025
	(0.004)	(0.004)	(0.003)	(0.003)
Observations	387,858	371,293	378,709	413,184
Panel B: **Threshold age 68**				
Excess separation	0.002	**0.006**	**0.036**	**0.038**
	(0.004)	**(0.004)**	**(0.004)**	**(0.004)**
Observations	294,432	271,050	290,971	317,815
Panel C: **Placebo threshold age 69**				
Excess separation	-0.002	0.009	0.001	0.001
	(0.004)	(0.005)	(0.004)	(0.004)
Observations	254,826	218,633	227,246	258,037

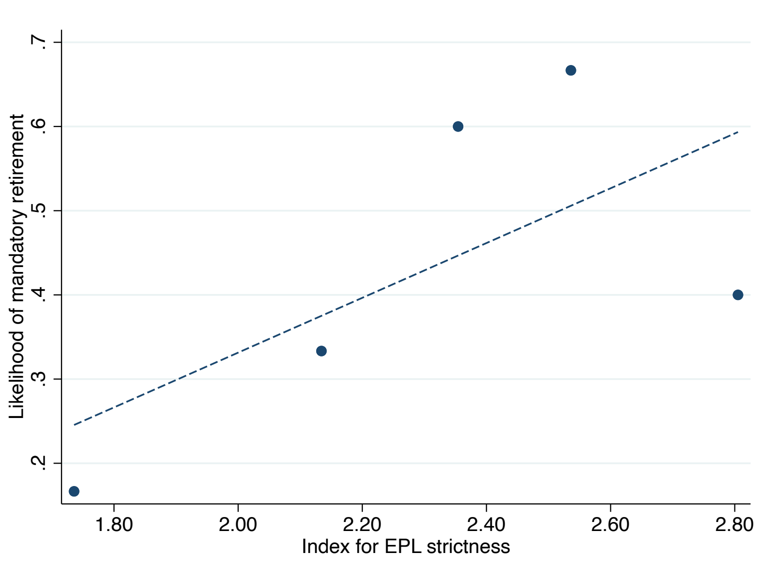
Notes: This table shows excess separation estimates across age thresholds and over years. The columns focus on different years while the panels zoom in on different age thresholds (67 in Panel A, 68 in Panel B). We include a placebo age threshold of 69 in Panel C. All estimates are obtained using the same bunching method described in Figure 7. The graphical analysis underlying these estimates is presented in Figure 9. Standard errors are computed using the delta method. The estimates are bolded when corresponding to the true legal thresholds when employment protection ends. Employment protection is eliminated at age 67 up to 2019 and at age 68 in 2020-2022. Excess separations at age 67 are sharply reduced in 2020 and after. Excess separations at age 68 start appearing in 2021 and 2022. There is no spike at age 68 in 2020 because that cohort's "deadwood" jobs could be laid off by employers at 67 in 2019. By 2021, more than half of the spike has migrated to age 68. This demonstrates that the spike we observe at age 67 in 2019 is indeed driven by the elimination of employment protection. None of the placebo estimates for age 69 are significant, validating our identification assumption that, absent employment protection ending, there would be no bunching at ages 67 or 68. Observations count the number of months times individuals, including only months with positive earnings as separations are always defined relative to the working population, for the ages 67-7/12 to 67+7/12. See also Figure 10 for the time series of excess separations by age cutoff.

Figure A.1: Mandatory Retirement and Link with Employment Protection Strictness

(a) Mandatory Retirement Ages in 2021



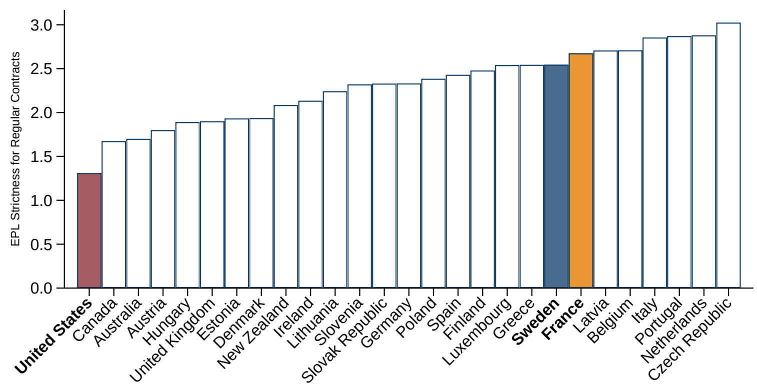
(b) Mandatory Retirement and Employment Protection Strictness



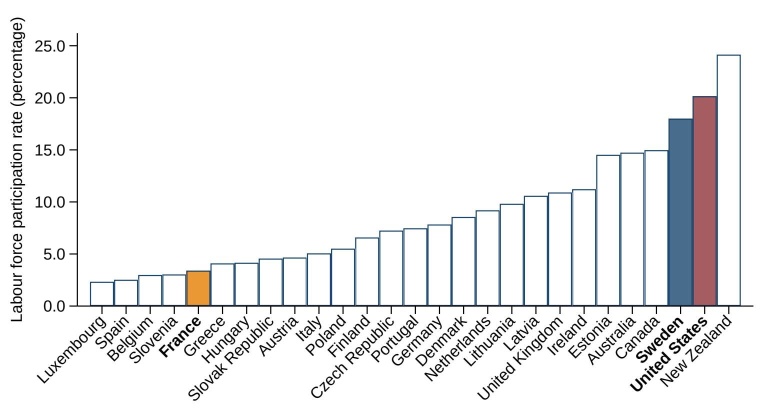
Notes: Panel (a) depicts mandatory retirement ages in OECD countries as of 2021 (source is OECD 2022). Countries are ordered by mandatory retirement age when such a mandatory age exists and applies to all workers in both the private and public sectors. Countries with no across the board mandatory age are listed on the right side (a number of these countries do have mandatory age in the public sector but not the private sector). Panel (b) shows that there is a positive correlation between the likelihood of having a mandatory retirement age across the board and the strictness of employment protection legislation (employment protection). OECD countries from Panel (a) are ranked in five bins of employment protection strictness using the data depicted on Appendix Figure A.2 Panel (a). For each bin, the compute the fraction of countries with a mandatory retirement age across the board using the data depicted in Panel (a). The regression line is depicted and is upward sloping.

Figure A.2: How Sweden Compares on Employment Protection and LFP at Age 65+

(a) Stringency in Employment Protection Legislation



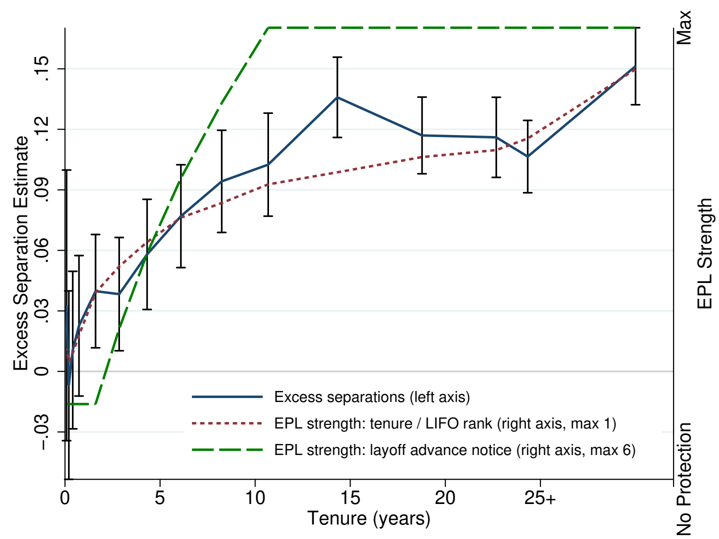
(b) Labor Force Participation at Age 65+



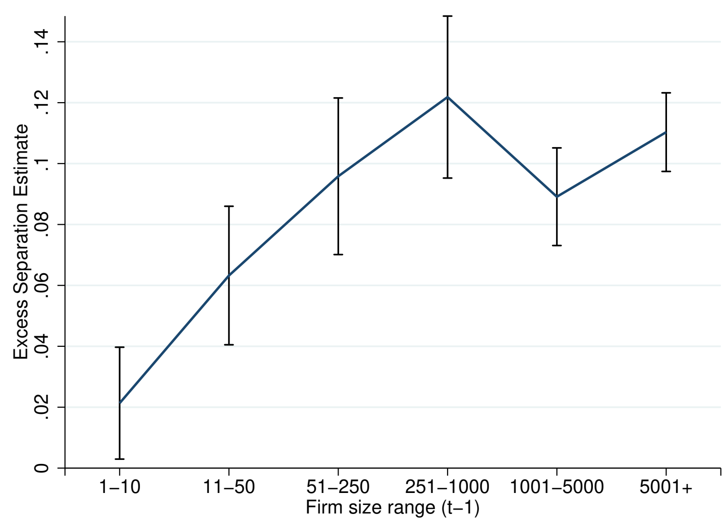
Notes: This figure uses OECD statistics to compare Sweden vs. EU countries and Anglo-American countries in terms of stringency of employment protection legislation (employment protection) in Panel (a) and labor force participation of the population aged 65 and over in Panel (b) in 2019. Sweden has a stringent employment protection comparable to France and much stricter than the United States. Sweden has high labor force participation at older ages, much higher than France and comparable to the United States.

Figure A.3: Heterogeneity by Tenure and Firm Size

(a) Excess Separations by Tenure



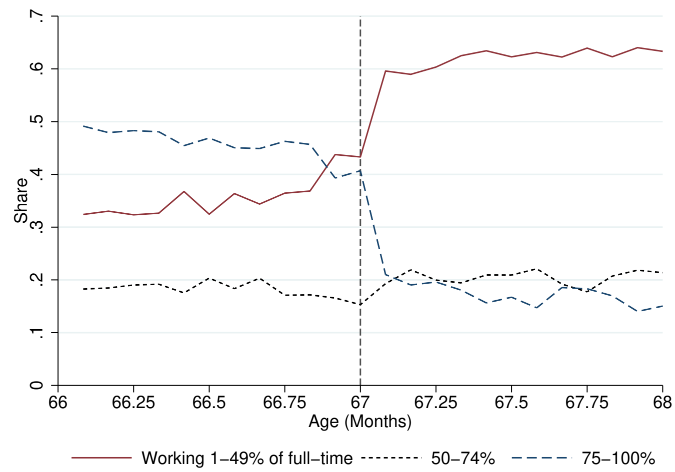
(b) Excess Separations by Firm Size



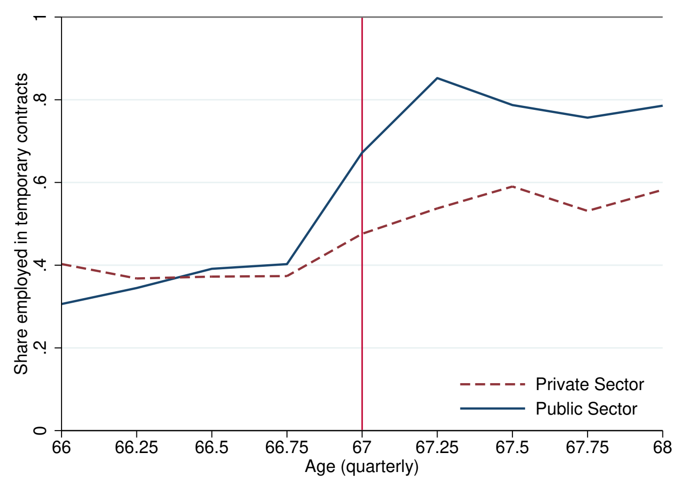
Notes: Panel (a) depicts the excess spike, estimated using the bunching procedure, by tenure decile among those with tenure of at most 12 years, and then five equally sized groups for the higher tenure groups. The x-axis captures the average tenure in each of these quantiles, except for the top group (which is above censored for data reasons, and hence we cannot calculate that group's mean). The graph shows that the excess separations estimate grows fairly smoothly with tenure. It becomes flat at higher tenure values, plausibly because EPL strength is maxed out. Indeed, we additionally plot two proxies for EPL strength as in Figure 3, for tenure rank (for LIFO rules) and advance notice (in months), where the max values are 1 (top of the tenure distribution relevant for LIFO) and 6 months, respectively. Panel (b) depicts the excess spike, estimated using the bunching procedure, by firm size measured as number of employees in the year before. The graph shows that the excess separations estimate grows with firm size, in particular at the lower end of the firm size distribution (recall that firms with 10 or fewer employees are partially exempt from LIFO rules for layoffs).

Figure A.4: Impact of Employment Protection Elimination on Contracts

(a) Full-time vs. Part-time Jobs in Public Sector



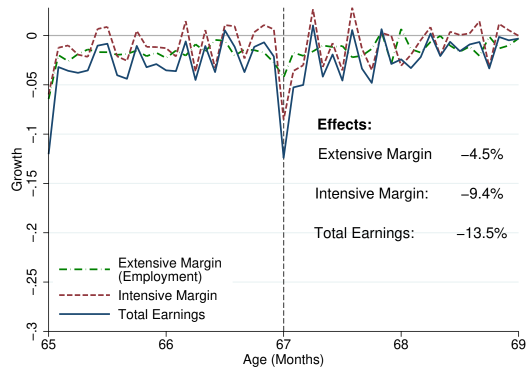
(b) Temporary vs. Permanent Contracts



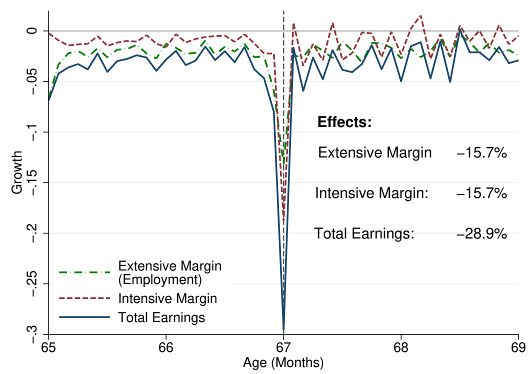
Notes: Panel (a) considers the panel of stayers (with the same employer on both sides of 67) among public sector workers (in the 2018-2019 Structure of Earnings Survey waves as in Figure 13), but then breaks it down into hours categories, displaying the fraction of workers working less than half of full-time, between half-time and less than 75 percent of full-time, and 75 percent of full-time or more. The figure shows a large decline in fraction working at least 75 percent of full-time and a corresponding increase in the fraction working less than half of full-time. This shows that the main margin of intensive response in the public sector is to shift workers from (close to) full-time positions toward part-time positions. Panel (b) uses the Labor Force Survey pooling years 2010-2019 to plot the fraction of workers in temporary contracts (as opposed to permanent contracts) among private sector workers and among public sector workers by age (in quarters). The figure shows a large increase from 40 percent to 80 percent in temporary contracts surrounding the employment protection cutoff age 67 among public sector workers and a much more muted increase for private sector workers. In the Labor Force Survey, the definition of stayers (see main text) is among employed workers on both sides of 67, as we cannot identify employers in this dataset. Panel (b) is based on 1,693 observations.

Figure A.5: Earnings per Capita: Private vs. Public Workers, and Professors

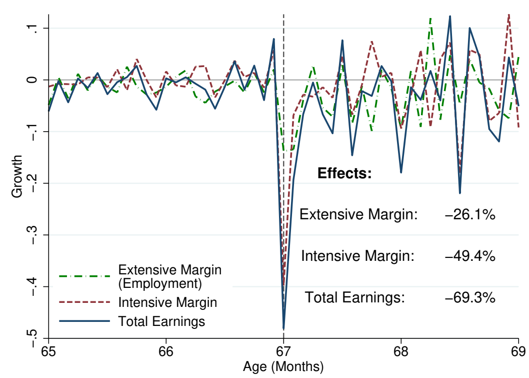
(a) Private Sector Workers



(b) Public Sector Workers

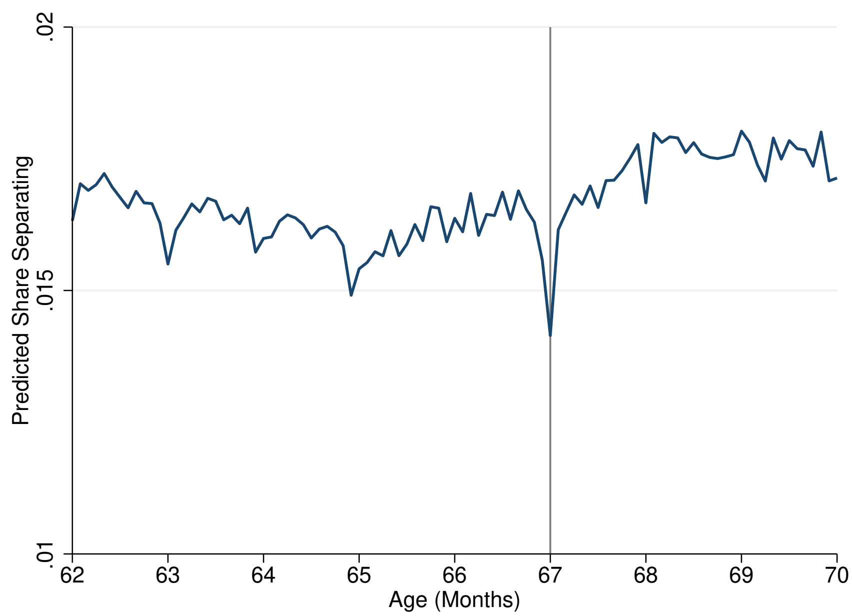


(c) Professors



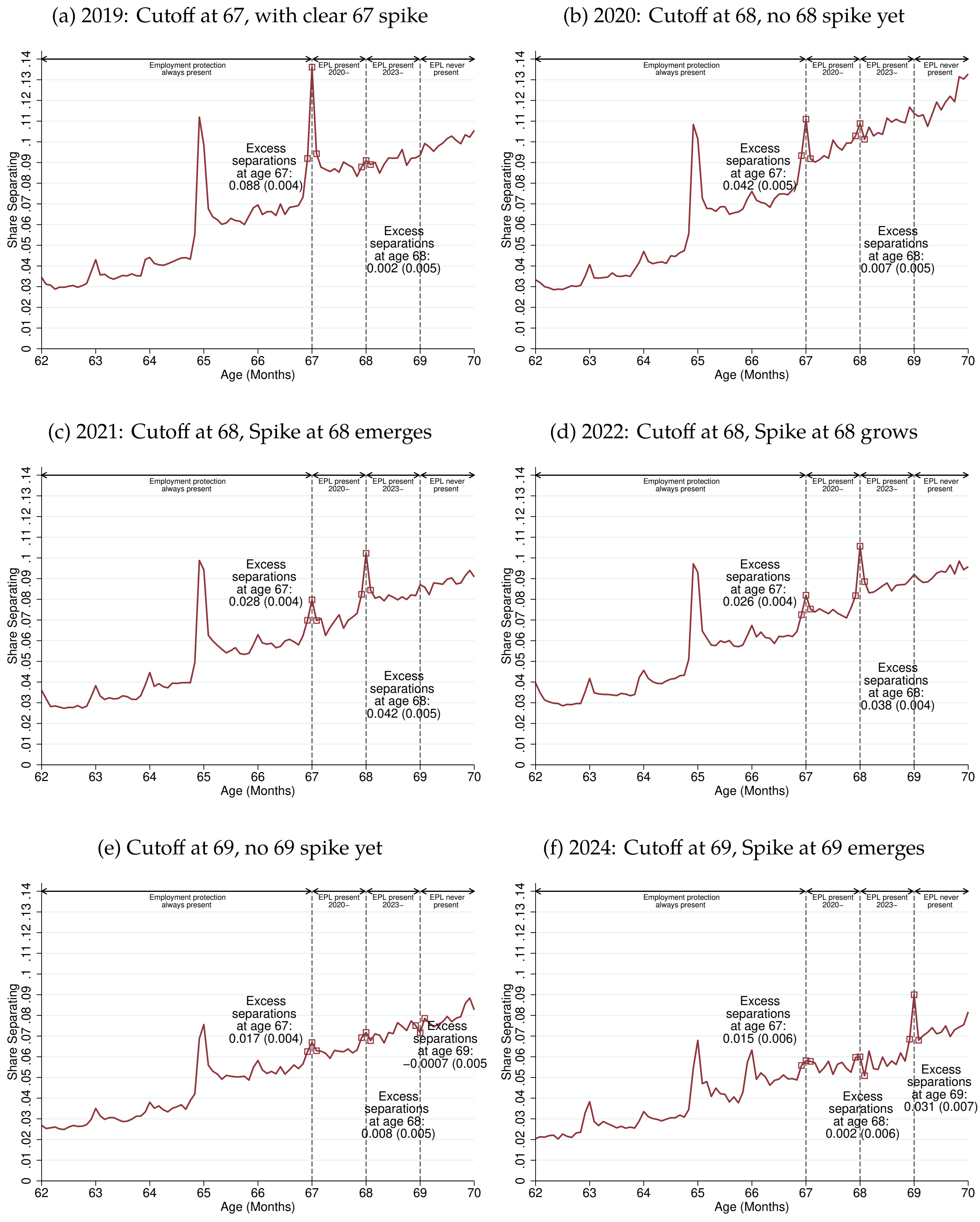
Notes: The figure repeats the analysis of percent changes in monthly earnings per capita (including zeros) presented in Figure 4 Panel (b) but broken down by private sector workers (Panel (a)), public sector workers (Panel (b)), and professors (Panel (c)). For each group, the figure also decomposes the total earnings per capita changes into an extensive margin (employment changes in green dotted-dashed line) and an intensive margin (earnings conditional on working in red dashed line). The earnings drop are twice as large in the public sector than in the private sector, and are considerably larger for professors. In the overall public sector, the extensive margin employment drop and the intensive margin of earnings conditional on working contribute about half to the total earnings drop. In the private sector, the intensive margin accounts for more, about two thirds, similarly for professors. In 2019 and with public/private defined by ownership, and professors by occupational codes, the shares are 51 percent public, 49 percent private, and 0.72 percent professors.

Figure A.6: Suggestive Test on Compositional Effects Among Separators Based on Predicted Separation Shares, if Anything Pointing to Lower Rather than Higher Separation Rates Among Age-67 Separators



Notes: The figure reports the empirical analyses described in Footnote 22 in the main text: a suggestive direct test on compositional effects. Specifically, we constructed separation probabilities predicted by observables (fed into a linear probability model trained on data from ages 59-61 in 2019, using public/private sector, gender, immigrant status, ten tenure categories, and all their interactions, as well as a cubic polynomial in previous earnings, and fixed effects for education-industry indicators). We then plot the average predicted separation rate among separators and find a sharp but modest decline rather than increase at 67. Hence, the test suggests that the excess separations appear to stem from jobs with if anything modestly longer rather than shorter predicted subsequent duration. This finding does not lend support to the view that the spike of separation at 67 has limited effects on lifetime employment by terminating jobs that were about to end.

Figure A.7: Reform Robustness: Change in Employment Protection Age Cutoff



(a) 2019: Cutoff at 67, with clear 67 spike

(b) 2020: Cutoff at 68, no 68 spike yet

(c) 2021: Cutoff at 68, Spike at 68 emerges

(d) 2022: Cutoff at 68, Spike at 68 grows

(e) Cutoff at 69, no 69 spike yet

(f) 2024: Cutoff at 69, Spike at 69 emerges

Notes: This figure repeats Figure 9 but defines a separation as being employed by a specific employer in a given month, but not working with that employer during any of the next 6 months–instead of 12 months in our baseline. The 2024 panel is identical to Figure 9(f) and based on the first 4 months of the year (instead of full year) because our data ends in October 2024 (and we need 6 months of post-separation data to measure separations). 2023 is full year in this figure while it was based on the first 9 months in Figure 9(e). Shifting the definition from 12 months to 6 months has only a minimal impact on excess separations at cut-off ages with nearly identical estimates. This validates the approach of Figure 9 of showing 2024 with a different definition due to data availability.