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Temporary Layoffs and Long-Run Labour Market Scarring: Evidence from Austria

Rainer Eppel^{1*}, Thomas Horvath¹, Helmut Mahringer¹, Christine Zulehner^{1,2}

Abstract: Temporary layoffs – employment separations followed by recall to the same employer – are widely used to buffer short-term demand fluctuations, yet their long-term worker consequences remain poorly understood. Using Austrian administrative data and propensity-score matching, we estimate their long-run effects by comparing workers experiencing short, non-seasonal interruptions during their first employment year with similar workers whose employment continues uninterrupted over the same first-year period. Outcomes are measured from the second year onward. Over ten years, affected workers accumulate about 3.5 months less employment and three months more unemployment, while cumulative earnings decline by more than €6,000. Monthly earnings during employment are also slightly lower, providing no evidence of compensating wage differentials. The results indicate substantial worker welfare costs and fiscal externalities and suggest that current institutional arrangements may encourage firms to shift adjustment costs to workers and the unemployment insurance system.

Keywords: Temporary layoffs, Recalls, Employment scarring, Unemployment insurance, Labour market policy

JEL classification: J63, J64, J65, J68

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1. Introduction

Temporary layoffs – employment separations followed by recall to the same employer – represent a widespread but understudied labour-market phenomenon. Firms use them to reduce labour costs during demand troughs while retaining access to trained workers. For workers, however, they constitute involuntary job loss with uncertain reemployment prospects, typically bridged by unemployment insurance benefits.

International evidence documents substantial prevalence. Recall rates – the share of workers returning to their previous employer after unemployment – range between 30% and over 40% across many countries (see, for example, Fujita & Moscarini, 2013; Jensen & Svarer, 2003; Røed & Nordberg, 2003). For Austria, approximately one-third of all transitions from employment to unemployment result in recall to the previous employer, and over 10% of all hires represent returns (Böheim, 2006; Nekoei & Weber, 2015). Temporary layoffs contribute roughly one percentage point to the unemployment rate and impose annual costs of approximately €600-700 million on the unemployment insurance system (Eppel & Mahringer, 2025).

Despite this prevalence, systematic causal evidence on long-term worker consequences remains limited. Existing studies focus primarily on immediate earnings or consumption effects (Del Bono & Weber, 2008; Nekoei & Weber, 2015; Lepage-Saucier, 2016). Recent work by Gertler et al. (2026) documents substantial "loss-of-recall" risk in the US, whereby initially temporary layoffs unexpectedly become permanent – particularly during recessions. However, the broader question of how temporary layoffs affect employment stability, earnings trajectories, and welfare for the affected workers remains unaddressed.

This gap is notable given extensive evidence that involuntary job loss generates persistent scarring effects. The job-displacement literature shows that workers losing stable jobs through plant closures or mass layoffs experience substantial, long-lasting earnings losses – often 10-30% annually, persisting for decades (Jacobson et al., 1993; Davis & von Wachter, 2011; Couch & Placzek, 2010; Schmieder et al., 2020). These losses reflect both lower reemployment wages and reduced employment stability. The unemployment-scarring literature similarly demonstrates that unemployment spells generate lasting negative effects on subsequent employment probabilities and wage rates, even after successful reemployment (Arulampalam, 2001; Gangl, 2006; Gregg, 2001).

Temporary layoffs share fundamental features with these phenomena – often involuntary separation, earnings interruption, and unemployment exposure – but differ in the expectation of recall to the same employer. Whether this distinction generates fundamentally different long-run trajectories, or whether temporary layoffs produce attenuated versions of displacement scarring, remains an open empirical question.

We fill this gap by providing causal evidence on temporary layoffs' medium- and long-run effects on workers' labour market outcomes. Using rich linked employer-employee data combined with propensity-score matching, we compare employment and earnings trajectories of workers experiencing a temporary layoff in the first employment year with similar workers whose employment continues uninterrupted. Our findings show that temporary layoffs generate persistent negative effects on employment stability, cumulative employment, and earnings,

alongside increased unemployment insurance receipt and reduced tax contributions, implying substantial long-term externalities.

2. Data and Methods

We use Austrian Social Security Database (ASSD) records linked with Public Employment Service (PES) data, covering all employment spells, earnings, unemployment episodes, and benefit receipt since 1972 on a daily basis, and merge these data with regional characteristics obtained from Statistics Austria. The sample comprises job starts from registered unemployment (2013-2017) in established firms outside seasonal sectors (tourism, construction, agriculture) and temporary-agency work, aged 25-55, observable ≥ 12 months ($N=252,183$ job starts).

A temporary layoff (TLO) occurs when employment (1) ends, (2) is followed by at least one day registered unemployment, and (3) resumes with the same employer within 62 days. This threshold captures short-term interruptions that plausibly reflect temporary demand fluctuations rather than seasonal employment cycles, which typically involve longer interruptions. Classical seasonal industries (agriculture, construction, tourism) and temporary agency work are excluded because temporary layoffs in these sectors are often part of established seasonal employment strategies. Our analysis instead focuses on sectors where temporary layoffs occur but are not structurally embedded in seasonal production cycles.

Treatment is defined within a fixed one-year risk window after job start. Employment relationships enter the treatment group if they experience a TLO during this period but continue beyond the first year ($N=15,743$). Controls are same-month job starts that remain uninterrupted throughout the first year ($N=236,440$). This symmetric design ensures both groups include only relationships lasting at least one year, differing solely in the presence of a first-year interruption. Our estimand is the average effect for workers who experience a temporary layoff during the first employment year and are subsequently recalled by the same employer. Outcomes are measured exclusively from year two onward, ensuring a clean separation between treatment and outcomes.

Identification relies on conditional independence given a rich set of pre-treatment covariates (>150 variables), including detailed employment and earnings histories (15 years), demographics, education, health status, unemployment duration, firm characteristics (industry, size, wage structure, turnover, recruitment patterns), and regional labour-market conditions. This allows us to compare workers with similar observable characteristics entering comparable jobs. We estimate the propensity score as the probability that a job is interrupted within the first year and match treated and control observations accordingly. This ensures that treated and control jobs have comparable ex ante probabilities of experiencing a temporary layoff. Restricting the sample to unemployment-to-employment transitions improves comparability of initial conditions. Moreover, temporary interruptions are unlikely to be anticipated at job start, as they depend on subsequent firm-level demand fluctuations. Additionally, job choice among unemployed workers is constrained by benefit rules requiring the acceptance of suitable employment.

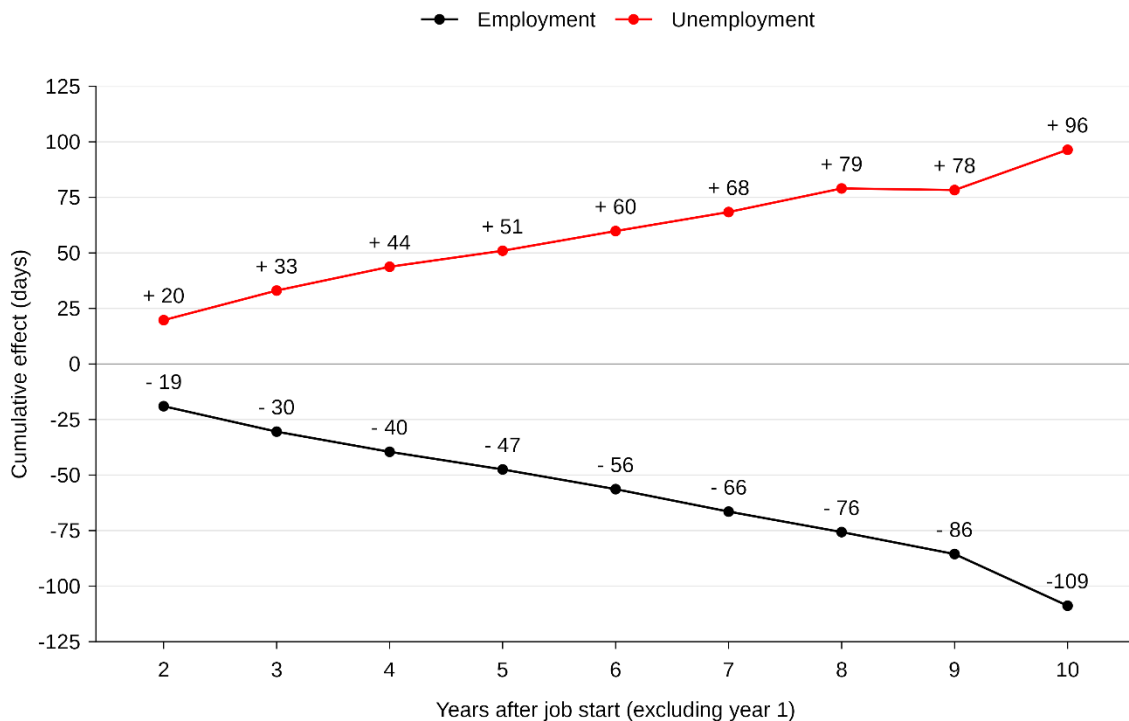
We estimate propensity scores via probit and apply nearest-neighbour 1:3 matching with caliper (0.01), with exact matching on job-start month-year. Matching diagnostics confirm excellent balance (median standardized bias <1%, pseudo- $R^2 \rightarrow 0$, likelihood-ratio test $p=1.000$ post-matching). The final matched sample comprises 13,612 treatment cases. Inference uses analytic standard errors; regression adjustment confirms robustness.

Outcomes include employment status at annual cut-offs, cumulative days in employment/unemployment/inactivity, remaining tenure with original employer, cumulative and monthly earnings, unemployment benefit receipt, and tax/contribution revenues. Follow-up extends up to ten years. As data are available until 2024, estimation samples are defined separately by outcome horizon, with longer horizons based on earlier entry cohorts. To avoid contamination from retirement transitions, we restrict each horizon-specific sample to individuals who remain below statutory retirement age throughout the observation period. All TLO events occur pre-COVID-19.

3. Results

Temporary layoffs lead to substantial and persistent differences in cumulative labour-market outcomes. Over years 2-10 after job start, affected workers accumulate 109 fewer employment days than matched controls (2,640 vs. 2,748 days), corresponding to roughly 3.5 months less employment (Table 1 and Figure 1). At the same time, they experience 96 additional days of registered unemployment (409 vs. 313 days). Economic inactivity remains largely unchanged, indicating that temporary layoffs primarily shift workers between employment and unemployment rather than inducing labour-force withdrawal.

Figure 1: **Cumulative Effects of Temporary Layoffs**



Notes: Points show cumulative differences between treated workers and matched controls. All effects are statistically significant at the 10% level or better.

Temporary layoffs also weaken long-term attachment to the original employer. After the first-year risk window, remaining tenure with the initial employer averages 944 days for treated workers compared with 1,201 days for controls, implying a reduction of 258 days (about 21%). This suggests that TLOs reduce the stability of employer-employee matches beyond the interruption itself.

Reduced employment stability translates into substantial earnings losses. Over years 2-10, cumulative earnings are €6,133 lower for workers experiencing temporary layoffs. Annual earnings differences range from €343 in year two to about €870 in year ten, indicating persistent gaps throughout the observation period. Average monthly earnings during employment are also slightly lower for workers experiencing temporary layoffs (€2,322 versus €2,357, a difference of about €35 or 1%), implying that earnings losses arise both from reduced employment continuity and modestly lower wages during employment.

Table 1: **Effects of Temporary Layoffs on Labour Market Outcomes**

Outcome	Treatment group	Control group	Difference (SE)	Relative (%)
Remaining tenure with original employer after year 1 (days)	944	1,201	-258*** (11)	-21
Cumulative employment days, years 2–10	2,640	2,748	-109*** (13)	-4
Cumulative unemployment days, years 2–10	409	313	96*** (8)	31
Cumulative earnings, years 2–10 (€)	205,783	211,916	-6,133*** (1,672)	-3
Average monthly earnings, years 2–10 (€)	2,322	2,357	-35*** (13)	-1
Cumulative UI benefits, years 2–10 (€)	14,076	11,192	2,884*** (261)	26
Cumulative income tax, years 2–10 (€)	21,735	23,655	-1,920*** (366)	-8
Cumulative social security contributions, years 2–10 (€)	80,849	83,218	-2,369*** (668)	-3

Notes: Treatment group: temporary layoff in the first employment year. Control group: continuous employment during the first year. Outcomes are measured over years 2–10 following job start. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

This pattern provides no evidence for compensating wage differentials. Standard labour economics predicts that jobs with higher layoff risk or earnings volatility should offer wage premiums to compensate workers for bearing these risks (Abowd & Ashenfelter, 1981; Topel, 1984; Cahuc & Zylberberg, 2004).

Several explanations may account for the absence of such premiums. First, if temporary interruptions are unanticipated at hiring, workers cannot demand compensation ex ante. Second, workers entering from unemployment possess limited bargaining power and outside options, reducing the scope for wage negotiation. Third, in thick labour markets with abundant applicants, firms may face little pressure to offer such premiums. Fourth, collective wage-setting institutions may constrain firm-level wage adjustments. Finally, workers may underestimate the risks associated with temporary interruptions – for example if recall commitments are perceived as reliable – reducing incentives to demand wage compensation.

Temporary layoffs also generate substantial fiscal effects. Over years 2–10, affected workers receive €2,884 more in unemployment insurance benefits. At the same time, reduced employment and earnings lower public revenues through €1,920 less income tax and €2,369 less in social security contributions.

Effects are observed across all demographic and occupational groups. Magnitudes are somewhat larger for women and white-collar workers, although the overall pattern remains similar across subgroups (Appendix Table A1).

Results prove highly robust. Treatment effects remain stable when applying regression adjustment, excluding large firms or tourism-intensive regions, restricting to direct unemployment-to-employment transitions, extending the risk window to two years, or excluding COVID-affected observation periods (Appendix Tables A2-A3).

4. Conclusion

This paper provides novel causal evidence on the long-term consequences of temporary layoffs for workers' employment trajectories and earnings. Using detailed administrative data and a matching design that ensures comparability between workers in similar jobs and with similar *ex ante* risks of temporary interruptions, we show that even short temporary layoffs early in an employment relationship generate persistent negative effects extending far beyond the interruption itself.

Three main findings emerge. First, temporary layoffs substantially destabilize employment relationships: tenure with the original employer declines by 21%, and affected workers accumulate about 3.5 months less employment over the following decade. Second, cumulative earnings fall by more than €6,000, reflecting both slightly lower monthly wages and reduced employment continuity. We find no evidence of compensating wage premiums that might offset the risks associated with temporary interruptions. Third, temporary layoffs impose substantial fiscal externalities through increased unemployment benefit receipt and reduced tax and social security contributions.

These results connect to the broader literature on labour-market scarring. While displacement studies document large and persistent losses following plant closures or mass layoffs (Jacobson et al., 1993; Davis & von Wachter, 2011), and unemployment-scarring research shows lasting effects of unemployment spells (Arulampalam, 2001; Gangl, 2006), our findings indicate that even short-lived interruptions followed by recall can generate persistent employment and earnings losses. Temporary layoffs therefore appear to produce attenuated but qualitatively similar scarring effects.

Our analysis conditions on successful recall within two months. Workers who experience layoffs but do not return – analogous to the "loss-of-recall" documented by Gertler et al. (2026) – are excluded, as we cannot observe whether recall was intended at separation. If such cases experience worse outcomes, our estimates likely represent lower bounds of the overall effects of temporary layoffs.

Institutional context may shape these patterns. Austria combines relatively low dismissal costs with unemployment insurance that provides partial income replacement during unemployment and flat-rate employer contributions independent of layoff behaviour (i.e., no experience rating). These features may facilitate temporary layoffs by allowing firms to shift part of the adjustment costs associated with demand fluctuations to workers and the unemployment insurance system.

Overall, the findings suggest that temporary layoffs – although often perceived as short-term and reversible – can generate persistent worker-level costs and fiscal externalities. Policy instruments such as experience rating, which links employer contributions to layoff behaviour, have

been proposed to internalize such externalities and may reduce excessive use of temporary layoffs, as documented in evidence from the United States (Eppel & Mahringer, 2025).

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Declarations

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Competing interests

The manuscript has not been previously published elsewhere nor is it currently under review by another journal. There are no conflicts of interest to report.

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A. Institutional Background: Temporary Layoffs in Austria

Austria's institutional framework creates conditions conducive to temporary layoffs. Dismissal protection is moderate by European standards, particularly during initial employment months. Unemployment insurance provides universal coverage with replacement rates around 55% of previous net earnings, financed through flat-rate employer and employee contributions independent of individual layoff behaviour. Notably, workers with firm recall commitments ("Einstellungszusage") face reduced job-search requirements while retaining full benefit eligibility, lowering recall costs for employers.

Approximately one-third of unemployment entries result in recall within 12 months (Böheim, 2006; Nekoei & Weber, 2015). Over 10% of all hires represent returns to previous employers. Roughly one-quarter of hiring firms use temporary layoffs at least annually (Eppel & Mahringer, 2025). These patterns indicate that temporary layoffs constitute a quantitatively significant labour-market phenomenon.

Beyond traditional seasonal sectors, temporary layoffs primarily buffer short-term demand shocks. By temporarily ending employment relationships and relying on unemployment insurance to bridge low-demand periods, firms reduce labour costs while maintaining access to trained workers, effectively externalizing business-cycle risk.

B. Data, Sample Construction and Variables

B.1 Data Sources and Linkage

The analysis draws on two linked administrative datasets:

1. Austrian Social Security Database (ASSD): Complete matched employer-employee register containing daily labour-market states since 1972, monthly earnings (up to contribution ceiling), occupational and sectoral information, and demographic characteristics.
2. Public Employment Service (PES) Unemployment Register (AUR): Records all registered unemployment spells, benefit receipt types and amounts, jobseeker characteristics, caseworker interactions, and programme participation.

Datasets are linked at the individual level via encrypted personal identifiers. Regional labour-market indicators (unemployment structure, employment composition, wage levels, GDP, population density) are merged at the labour office district level (101 districts).

B.2 Sample Selection

The evaluation sample includes individuals who:

- Were aged 25-55 at job start,
- Started non-subsidized, fully social-security-covered employment from registered unemployment between 2013-2017,
- Worked in established firms (continuously employing workers in the two years before job start),
- Were observable for at least 12 months following job start,
- Started jobs outside traditional seasonal sectors (tourism, construction, agriculture/forestry) and temporary-agency work.

Exclusions:

- Asylum seekers or subsidiary protection status
- Subsidized employment starts
- Marginal employment and self-employment
- Very small or newly established firms
- Private households and extraterritorial organizations
- Missing key characteristics

B.3 Treatment and Control Group Definition

Treatment group: Employment relationships that (1) experience a temporary layoff during the first year (end of employment → at least one day registered unemployment → rehiring by same employer within ≤62 days), and (2) continue beyond the first year.

Control group: Employment relationships starting in the same calendar month that continue uninterrupted throughout the first year without any layoffs.

Both groups include only employment relationships lasting at least one year, differing solely in whether the first year includes a temporary interruption. This symmetric design ensures comparability and clean separation of treatment from outcome.

B.4 All Covariates Used in Propensity-Score Estimation

We condition on over 150 pre-treatment variables, all measured before or at the day preceding the job-start month:

Personal characteristics:

- Age (linear and quadratic), gender, education, nationality, migration background, marital status, number and age of children (women), family-related career returner, health limitations, legal disability status

- PES registration status and benefit receipt (type and daily amount) immediately before job start
- Net unemployment duration, days in registered unemployment/training during current spell
- Sickness benefit days during unemployment (last 2 years)

Detailed employment history (up to 15 years):

- Binary indicators for employment and unemployment status in each of last 12 months and 10 years
- Cumulative days by state (unsubsidized employment, subsidized employment, self-employment, unemployment, PES training, other unemployment status, non-insurance) in years 1, 2, 3-5, 6-10, 11-15 before job start
- Cumulative earnings in years 1, 2, 3, 4, 5, 6-10 before job start
- Time since last employment, earnings in last job, sector and occupation in last job

Firm characteristics:

- Industry (NACE 2-digit)
- Firm age (log)
- Firm size, gender composition, age structure (median and IQR), median wage (log) – measured in year before job start
- Employment growth, hiring rate, separation rate, share of hires from unemployment – averaged over two years before job start, leave-one-out

Regional characteristics:

- Federal state, economic region type, population density
- Unemployment rate and structure (age, education, status), long-term unemployment share, share with hiring promise, vacancy pressure, wages, employment growth, labour supply growth – measured at job start
- Regional turnover rate and TLO rate – averaged over two prior years, leave-one-out

All variables are measured strictly pre-treatment to avoid post-treatment bias.

B.5 Outcome Variables

Employment status at annual cut-off dates:

- Employment (dependent and self-employment)
- Registered unemployment (including training)
- Economic inactivity

Cumulative volumes (years 2-10):

- Days in employment, unemployment, and inactivity
- Days in unsubsidized active dependent employment
- Remaining tenure with original employer

Earnings and income:

- Average monthly earnings from dependent employment (during employment months only)
- Cumulative earnings from dependent employment
- Cumulative unemployment insurance benefits

Fiscal variables:

- Income tax (derived using 2016 tax schedule with standard deductions)
- Employee and employer social security contributions

B.6 Follow-up horizons and sample restrictions

As administrative data are available until the end of 2024, not all entry cohorts can be observed over identical post-treatment horizons. We therefore construct horizon-specific samples. For longer follow-up periods (e.g., 8 or 10 years after the first employment year), only earlier entry cohorts (e.g., 2013–2014) can be included, whereas shorter horizons allow the inclusion of later cohorts.

In addition, we account for transitions into retirement. To avoid conflating treatment effects with retirement behaviour, each horizon-specific sample is restricted to individuals who remain below the statutory retirement age (60 for women, 65 for men) throughout the respective observation window. The baseline sample includes individuals aged 25–55 at job start.

C. Identification Strategy and Matching Procedure

C.1 Formal Framework

Let D denote temporary layoff experience in the first employment year, and Y_1 and Y_0 denote potential outcomes with and without TLO. The observed outcome is $Y = DY_1 + (1-D)Y_0$.

Our parameter of interest is the average treatment effect on the treated (ATT):

$$ATT = E[Y_1 - Y_0 \mid D=1] = E[Y_1 \mid D=1] - E[Y_0 \mid D=1]$$

Because $E[Y_0 \mid D=1]$ is unobserved for treated individuals, we estimate it using matched non-participants.

Identification relies on conditional independence:

$$Y_0 \perp D \mid X$$

and common support:

$$0 < P(D=1 \mid X) < 1$$

where X denotes the rich set of pre-treatment covariates.

We estimate the propensity score $P(D=1 \mid X)$ using a probit model and apply nearest-neighbour 1:3 matching with caliper within common support. The ATT estimator is:

$$ATT = (1/N_T) \sum_{i \in T} [Y_i - (1/J(i)) \sum_{j \in J(i)} Y_j]$$

where $J(i)$ denotes the set of matched controls for treated unit i .

Inference relies on analytic standard errors. Robustness checks include regression adjustment, controlling for residual imbalances post-matching.

C.2 Plausibility of CIA

CIA appears plausible because:

1. **Limited anticipation:** Workers entering from unemployment cannot reliably foresee whether their job will experience temporary interruption, as this depends on future demand shocks and short-term firm decisions.
2. **Restricted choice:** Unemployed jobseekers face limited discretion and must accept suitable offers to maintain benefits.
3. **Comprehensive controls:** Linked data provide detailed employment histories, earnings, firm characteristics, and regional conditions mirroring both TLO determinants and outcome predictors.
4. **Homogeneous population:** Restriction to unemployment-to-employment transitions in non-seasonal sectors creates comparable starting conditions.
5. **Excellent balance:** Post-matching diagnostics confirm statistical indistinguishability of treated and control groups.

C.3 Matching Diagnostics

Across all specifications, propensity-score matching achieves excellent covariate balance:

- Median absolute standardized bias falls below 1%
- Pseudo- R^2 drops from ~ 0.34 to < 0.01
- Likelihood-ratio tests for joint significance are clearly insignificant post-matching ($p = 1.000$)
- Rubin's B ranges 15-19, Rubin's R near 1
- Loss from common-support restrictions: $\sim 14\%$ of treatment cases

Placebo test using employment status one year before job start yields very small, insignificant effects, confirming parallel pre-treatment trajectories.

D. Additional Results

D.1 Subgroup Effects

Appendix Table A1 reports effects by population subgroups. All groups experience significant employment losses, ranging from -43 to -57 days over five years and -75 to -130 days over ten years. Effects are slightly stronger for women and white-collar workers. The unemployment response also varies: men and blue-collar workers experience primarily increased unemployment (+84 and +100 days over ten years), while women and white-collar workers additionally show modest increases in economic inactivity (+31 days).

Appendix Table A1: **Estimated Effects by Population Group**

	5-year employment loss		10-year employment loss	
	Difference (SE)	Relative (%)	Difference (SE)	Relative (%)
Total average	-47*** (3)	-4%	-109*** (13)	-4%
Women	-57*** (6)	-5%	-130*** (22)	-5%
Men	-47*** (4)	-4%	-75*** (15)	-3%
Age 25–39	-50*** (4)	-4%	-94*** (15)	-3%
Age 40–55	-43*** (5)	-4%	-86*** (21)	-3%
Blue-collar workers	-44*** (4)	-4%	-97*** (15)	-4%
White-collar workers / civil servants	-57*** (6)	-4%	-104*** (23)	-4%

Notes: Cumulative employment days in years 2-5 and 2-10 following job start. *** p < 0.01.

E. Robustness Tests

E.1 Alternative Estimators and Specifications

Appendix Table A2 reports robustness tests varying sample composition and estimator. Results remain highly stable:

Appendix Table A2: **Robustness Tests – Employment Effects**

	5-year effect (days)		10-year effect (days)	
	Difference (SE)	Relative (%)	Difference (SE)	Relative (%)
Main specification	–47*** (3)	–4%	–109*** (13)	–4%
Entry cohorts 2013–2015 only	–44*** (4)	–3%	—	
Entry cohorts 2016–2017 only	–48*** (5)	–4%	—	
Direct unemployment-to-employment only	–47*** (3)	–4%	–92*** (13)	–3%
Classical unemployment status only	–45*** (3)	–4%	–101*** (12)	–4%
Excluding large firms (≥250 employees)	–50*** (3)	–4%	–96*** (13)	–3%
Excluding tourism-intensive regions	–47*** (3)	–4%	–96*** (14)	–4%
First employment with employer ≥1 year	–45*** (4)	–4%	–99*** (14)	–4%
Two-year risk window	–44*** (3)	–4%	–87*** (13)	–3%
With regression adjustment	–49*** (4)	–4%	–117*** (14)	–4%

Notes: Cumulative employment days in years 2-5 and 2-10. *** p < 0.01.

E.2 Excluding COVID-Affected Periods

Appendix Table A3 compares estimates with and without COVID-affected observation periods. Effects remain virtually identical, confirming that pandemic-related disruptions do not bias our estimates.

Appendix Table A3: **Comparison With and Without COVID Period**

	Including COVID (days)		Excluding COVID (days)	
	Difference (SE)	Relative (%)	Difference (SE)	Relative (%)
Employment days, years 2–5	–47*** (3)	–4%	–55*** (5)	–4%
Employment days, years 2–6	–56*** (4)	–4%	–57*** (9)	–4%
Unemployment days, years 2–5	+51*** (2)	+35%	+53*** (4)	+34%
Unemployment days, years 2–6	+60*** (3)	+33%	+62*** (7)	+31%

Notes: COVID-excluded specification restricts all outcomes to December 2019. *** $p < 0.01$.

F. Welfare and Efficiency Analysis

F.1 Private Costs: Worker Welfare

From workers' perspective, temporary layoffs impose substantial private costs through three channels:

1. Direct income losses during interruption

Workers lose earnings during unemployment spells, partially offset by unemployment benefits (typically 55% of previous net earnings). For a two-month interruption at median earnings, this represents approximately €1,500–2,000 in foregone income relative to continued employment.

2. Long-term earnings losses

The persistent reduction in cumulative employment (–109 days over ten years) and slightly lower monthly wages (–€35) generate cumulative earnings losses of €6,133, reflecting sustained reductions in employment and wages rather than the mechanical earnings loss during the interruption itself. Workers do not recover these losses through compensating premiums or accelerated wage growth in subsequent employment.

3. Increased earnings volatility and uncertainty

Beyond average losses, workers face heightened income volatility through more frequent unemployment spells. This volatility imposes welfare costs even for risk-neutral individuals (through liquidity constraints, consumption smoothing difficulties) and larger costs for risk-averse workers. The absence of compensating wage differentials indicates workers do not receive compensation for bearing this additional risk.

4. Total private welfare loss

Combining direct interruption losses, long-term earnings reductions, and uncompensated risk exposure, the total private welfare cost substantially exceeds the measured €6,133 earnings

gap. Including utility losses from unemployment itself (beyond income effects) and accounting for risk aversion would further increase estimated welfare costs.

F.2 Fiscal Externalities

Temporary layoffs generate substantial fiscal externalities through two mechanisms:

1. Increased unemployment insurance expenditures

Over ten years, TLO-affected workers receive €2,884 more in unemployment benefits (+26%), driven by persistently higher unemployment incidence after the interruption. Critically, these costs are not borne by the separating employer but socialized across all contribution-payers through flat-rate financing.

2. Reduced tax and contribution revenues

Lower earnings generate substantial revenue losses:

- Income tax: -€1,920 (-8%)
- Social security contributions: -€2,369 (-3%), comprising -€1,096 employee and -€1,273 employer contributions

Total fiscal externality per TLO-affected worker:

Net fiscal cost = +€2,884 (benefits) - €1,920 (income tax) - €2,369 (contributions) = -€1,425

This calculation understates total fiscal impact, as it excludes potential second-order effects (e.g., reduced VAT from lower consumption, higher social assistance in vulnerable subgroups, administrative costs).

For Austria's approximately 200,000 annual TLO cases, even conservative per-person fiscal costs of €1,000-1,500 imply system-wide costs of €200-300 million annually from long-term effects alone, beyond immediate interruption costs.

F.3 Efficiency Implications

The documented patterns are consistent with the presence of fiscal externalities associated with temporary layoffs:

Firms using temporary layoffs generate costs for workers (earnings losses, employment instability) and the unemployment insurance system (benefit payments, reduced contributions) without directly bearing these costs.

Flat-rate contribution financing implies that employer contributions do not vary with layoff behaviour, which may weaken incentives to internalize such costs.

From a welfare perspective, these patterns suggest that temporary layoffs may be used more intensively than would be the case under full cost internalization. However, a full welfare evaluation is beyond the scope of this paper.

Firms facing temporary demand reductions can adjust along several margins, including internal flexibility (e.g. working-time accounts, overtime reduction), short-time work schemes, temporary layoffs, or by retaining workers despite temporary underutilization (labour hoarding).

Temporary layoffs may be less desirable from a welfare perspective if they generate persistent employment and earnings losses, as documented in this paper.

F.4 Limitations and Caveats

Three important caveats apply to this welfare assessment:

1. Conditioning on successful recall:

Our analysis conditions on workers who return to the same employer within 62 days. Workers experiencing loss-of-recall – i.e., becoming permanently separated despite initial temporary layoff – cannot be reliably distinguished from standard separations in the data and are therefore excluded. If this group experiences worse outcomes, total welfare costs exceed our estimates. Our results should thus be interpreted as lower bounds of the effects of temporary layoffs.

2. Firm-side benefits not quantified:

While we document worker and fiscal costs, we do not quantify firms' private benefits from temporary layoff flexibility. Identifying optimal policy requires comparing social costs (documented here) with private adjustment benefits. If firm benefits are small – particularly when less disruptive alternatives exist – inefficiency is clear. If benefits are large, the optimal policy may involve cost-sharing rather than prohibition.

3. General equilibrium effects:

Our partial-equilibrium analysis estimates effects on TLO-affected workers holding labour-market aggregates fixed. If policy reforms substantially reduce TLO use, general-equilibrium adjustments may occur – potentially affecting wage levels, unemployment duration, or job-finding rates. The direction of such effects is theoretically ambiguous.

Despite these limitations, the documented private and fiscal costs are substantial, robust, and policy-relevant. They provide clear evidence that current institutional arrangements fail to internalize the full social costs of temporary layoff decisions.