Do UI cuts Accelerate the Transition from Graduation to Employment?

Evidence from a Danish Policy Reform*

By

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Abstract

In 2023 UI generosity was reduced for recent graduates in Denmark. The benefit period was shortened from two years to one, and a new two-tier structure imposed benefit reductions on non-parents after three months of unemployment. This paper examines the impact of these changes. I use a DiD strategy to estimate the reduction in benefit levels and find increased exits around the three-month mark. Additionally, I identify positive effects of the shortened benefit period, particularly after benefit exhaustion. I estimate an employment effect of 1,550 FTE – substantially below pre-reform projections – and compare my findings to the literature, discuss potential discrepancies and relate them to insights on optimal UI design.

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1. Introduction

Unemployment Insurance (UI) provides compensation for income loss during periods of involuntary unemployment. A distinct feature of the Danish UI system is the inclusion of recent graduates under a separate scheme. Graduate benefits are independent of previous earnings, and provided at lower amounts and for shorter duration than general UI.

In recent years, Danish economic policy has prioritized increased labor supply, leading to several major UI reforms. A 2010 reform reduced the potential benefit duration (PBD) for all UI recipients from four years to two, while reforms in 2017 and 2023 introduced changes specifically targeting graduate UI. The 2017 reform reduced benefit levels for non-parents by 12.8 %, while the 2023 reform introduced further reductions after three months of unemployment – 13.1 % for non-parents over 30 and 31.2 % for those under 30. In addition, the 2023 reform shortened the PBD from two years to one independent of parental status.

This paper examines how the UI adjustments introduced with the 2023 reform have affected job-finding behavior among graduates in Denmark.

Standard job search theory, as outlined by Mortensen (1977), predicts how job seekers respond to lower UI benefit levels. A reduction in benefits influences job finding rates through two channels: First, it increases the marginal benefit of job search relative to its cost by lowering the utility of being unemployed. This incentivizes job seekers to intensify their search efforts, raising the probability of receiving job offers. Second, the reduced utility of unemployment relative to employment compels job seekers to demand lower compensation to exit unemployment. This decline in the reservation wage leads job seekers to accept a larger proportion of job offers. Consequently, job finding rates increase as job seekers both receive more offers and accept a higher share of them.

This paper focuses exclusively on the unemployment exit effects of the UI reform. However, reductions in UI generosity can have broader welfare implications. As emphasized by Baily (1978), public UI contributes to overall welfare by providing consumption smoothing, which is difficult to replicate in private markets due to asymmetric information. Moreover, Diamond (1981) and Acemoglu and Shimer (1999, 2000) argue that UI enables workers to search more selectively, leading to improved job matching and long-term productivity gains. Finally, a growing body of empirical evidence documents negative effects of UI reforms on both physical and mental health outcomes (Ahammer and Packham 2023; Chen et al. 2023; Cylus et al. 2015; Kuka 2020). These broader welfare considerations must be weighed against the potential gains in job-finding incentives when policymakers determine the appropriate level of UI generosity.

The relationship between UI generosity and unemployment duration has been extensively studied in the empirical literature (H. L. Andersen and Arendt 2015; T. M. Andersen et al. 2015; Chetty and Finkelstein 2013; Cohen and Ganong 2024;

Krueger and Meyer 2022; Le Barbanchon et al. 2024)[1]. A recurring finding is that increased UI generosity has an adverse effect on job finding rates, though results vary significantly over studies (see figure 8). These differences may be attributed to cultural variations, structural differences in UI systems, or diverse estimation methodologies. They may also reflect differences in the business cycle, as the effects of UI reforms and active labor market policies are known to be state-dependent (Schmieder et al. 2012).

In a Danish context, the impacts of the UI reforms of 2010 and 2017 have been subject to empirical evaluation. Dagpengekommissionen (2015), Hermansen (2014), and Jonassen (2014) evaluate the impact of a reduction in the PBD from four to two years in 2010 in Denmark. In the years following this reduction, several temporary benefits were introduced, such as »Særlig uddannelsesordning I & II«, »Midlertidig arbejdsmarkedsydelse«, and »Kontantydelse«. These benefits were provided at lower rates than the standard UI benefits, leading to a decrease in replacement ratios.

Against this background, Hermansen (2014) and Jonassen (2014) investigate how the shorter PBD influenced job-finding behavior. Both studies apply a simple pre-post design, comparing unemployment exits for cohorts with a four-year entitlement to those for cohorts with a two-year entitlement, neither of which were directly affected by the temporary benefit schemes. Their results point to a clear increase in unemployment exits as benefit exhaustion approaches. However, the timing of these effects differs: Hermansen (2014) find significant responses as early as 1.5 years before exhaustion, while Jonassen (2014) identify such effects only within the last four months. This discrepancy most likely reflects differences in how the two studies define exits into employment. Moreover, while Jonassen (2014) do not provide elasticities, Hermansen (2014) report a duration elasticity between 0.15 and 0.24

Building on these earlier analyses, Dagpengekomissionen (2015) leverages the variation in treatment intensity stemming from the introduction of temporary lower-rate benefits to estimate the effects of reduced benefit levels on job-finding rates. Their findings suggest significant positive anticipatory effects on unemployment exits up to 1.5 years before benefit reduction and extending to six months afterward. These effects are particularly pronounced in the eight weeks before and after benefit decline. A separate analysis restricted to recent graduates produces similar results. The study is significant in a Danish context since the estimated elasticities are incorporated into the UI model (Dagpengemodellen), which is used to project the employment effects of UI adjustments, including the 2023 revision of the graduate UI scheme (Dagpengekommissionen 2021; Finansministeriet 2021).

1. Additionally, jobeffekter.dk compiles and synthesizes research on various labor market interventions, including UI reforms, providing an excellent overview of the existing literature.

However, the magnitude of anticipatory effects following the 2010 reform has been contested. While Dagpengekommissionen (2015) reports large effects appearing long before benefit exhaustion, more recent evidence by Arbejderbevægelsens Erhvervsråd (2024) attributes part of these to confounding factors, suggesting that the true employment effects are substantially smaller.

Whereas the 2010 reform primarily reduced the length of benefit eligibility for all UI recipients, the 2017 UI reform introduced policies targeting graduates. Most notably, the benefit level for recently graduated non-parents was reduced by 12.8 %, while parents remained unaffected. The PBD remained unchanged at two years. Ahle and Skafte-Larsen (2018) and STAR (2022) exploit the differential treatment by parental status to evaluate the reform's impact using a Difference-in-Differences approach. Both studies report comparable findings, with STAR (2022) estimating a duration elasticity of 0.44 and Ahle and Skafte-Larsen (2018) reporting an elasticity of 0.77. Moreover, Ahle and Skafte-Larsen (2018) investigates the dynamic effects throughout the unemployment spell, identifying significant impacts during the first six months of unemployment and in the period immediately preceding benefit exhaustion.

The empirical strategies employed in this paper draw directly on the methodologies established in these prior evaluations from Denmark. In line with Ahle and Skafte-Larsen (2018) and STAR (2022) I use a Difference-in-Differences design to estimate the impact of lower benefit levels for non-parents, using parents as a control. The results show that non-parents under 30 increased exits to employment around the three-month mark, while no significant effect was found for those over 30. To assess the effect of the shortened PBD, I compare unemployment exit paths for parents enrolled before and after the reform. This strategy closely aligns with Hermansen (2014) and Jonassen (2014). I find a positive response, with anticipatory effects emerging up to 35 weeks before benefit exhaustion and the strongest effect occurring immediately afterward. However, this design is considerably weaker, as no clear control group is available, and the results should therefore be interpreted with caution.

Bringing these findings together, I estimate the reform’s total employment effect at 1,550 full-time equivalents – substantially below the 6,330 projected by Dagpengemodellen prior to implementation (BEU 2022). I compare the results with the existing literature, discuss potential discrepancies, and relate them to theoretical insights on optimal UI design.

The paper is structured as follows: Section 2 outlines the theory of UI and job search. Section 3 describes the 2023 reform of graduate UI, and section 4 presents the data. Section 5 sets out the empirical strategy, with the results reported in section 6. Section 7 evaluates the impact of the reform and discusses the paper’s contribution to the literature. Section 8 considers the welfare implications of UI, including the optimal benefit level and profile, and section 9 concludes.

2. Theory

The modern theory of unemployment is grounded in a search theory framework developed in the 1960s and 1970s (Gronau 1974; Lippman and McCall 1970; Mortensen 1970; Stigler 1961, 1962). In this framework, job seekers randomly sample wage offers and decide whether to accept or continue searching by weighing the costs and benefits of further search. Unemployment in this framework arises due to search frictions. A key contribution, examining the relation between UI and job search, is Mortensen (1977). In his approach, search is modeled as a sequential process where a worker optimizes search effort and a reservation wage to maximize lifetime utility. Mortensen (1977) shows that UI has a direct impact on the selection of these two variables, and thereby also on the exit of unemployment.

2.1. The Mortensen (1977) model of UI and unemployment

This section presents a concise introduction to the Mortensen (1977) model followed by an application in relation to the 2023 graduate UI reform. See Le Barbanchon et al. (2024) for more details.

The model describes a worker who becomes unemployed at time $t = 0$. From that point onward, in each subsequent period $t = 1,2,3, \dots$, the worker chooses a search effort e_t. The probability of receiving a job offer in a given period is assumed to be proportional to the search effort so, $s_t = e_t$. The cost of searching is a convex function $c(e)$, which satisfies $\frac{dc}{de} > 0$ and $\frac{d^2c}{de^2} > 0$

Each job offer comes with a wage w drawn from a continuous distribution with CDF $F(w)$. While unemployed, the worker receives UI benefits b_t in each period. Future utility is discounted by a factor δ, and the worker derives utility from both the wage w and the UI benefits b_t through a concave utility function $u(x)$, satisfying $\frac{du}{dx} > 0$ and $\frac{d^2u}{dx^2} < 0$

If a job is accepted, the worker retains it for all future periods. The value of accepting a job offer at time t is therefore the discounted utility of receiving a constant wage w indefinitely:

$$V_t^E = \frac{u(w)}{1 - \delta} \tag{1}$$

Since V_t^E increases monotonically with w, there exists a unique reservation wage, denoted by ϕ_t, at which the worker will accept any job offer exceeding this threshold. The reservation wage represents the minimum wage that makes accepting a job offer preferable to continuing the job search: $V_t^E(\phi_t) = V_t^U$. Using this in combination with equation 1, one gets:

$$u(\phi_t) = (1 - \delta)V_t^U \tag{2}$$

The value of being unemployed at time t consists of the utility derived from receiving unemployment benefits, minus the cost of job search, plus the discounted expected value of receiving a job offer in the next period $t+1$. The latter can be interpreted as the value of remaining unemployed plus the expected value of receiving a wage offer that exceeds the reservation wage:

$$V_t^U = \max_{s_t,\phi_{t+1}} \underbrace{u(b_t)}_{\text{Benefit utility}} - \underbrace{c(s_t)}_{\text{Search costs}} + \underbrace{\delta\left(V_{t+1}^U + s_t \int_{\phi_{t+1}}^{\infty} (V_{t+1}^E(w) - V_{t+1}^U) dF(w)\right)}_{\text{Expected value of receiving a job offer in next period}} \quad (3)$$

Taking the first order condition of equation 3 with respect to s_t and inserting equation 1 yields the optimal search effort as function of ϕ_{t+1}

$$s_t^* = c'^{-1}\left(\frac{\delta}{1-\delta}\left(\int_{\phi_{t+1}}^{\infty} \big(u(w) - u(\phi_{t+1})\big) dF(w)\right)\right) \quad (4)$$

However, since the model involves two endogenous variables – search effort and the reservation wage – it remains incomplete. To close the model, combine equations 1, 2 and 3 to obtain:

$$u(\phi_t) = (1-\delta)\big(u(b_t) - c(s_t^*)\big) + \delta u(\phi_{t+1}) + \delta\left(s_t^* \int_{\phi_{t+1}}^{\infty} \big(u(w) - u(\phi_{t+1})\big) dF(w)\right) \quad (5)$$

Equations 4 and 5 form a system of equations that jointly determines the optimal search effort s_t^* and the reservation wage ϕ_t over time. These values depend on changes in benefit levels throughout the unemployment spell.

A steady state is reached in period S when b_t remains constant for all $t \geq S$. This is effectively the period where the PBD has expired. In steady state, the optimal search effort and reservation wage stabilize at $s_t^* = s_{t+1}^* = s_S^*$ and $\phi_t = \phi_{t+1} = \phi_S$. The steady-state values of search effort and the reservation wage are implicitly determined by the following equations derived from equations 4 and 5:

$$s_S^* = c'^{-1}\left(\frac{\delta}{1-\delta}\left(\int_{\phi_S}^{\infty} \big(u(w) - u(\phi_S)\big) dF(w)\right)\right) \quad (6)$$

$$u(\phi_S) = u(b_S) - c(s_S^*) + \frac{\delta}{1-\delta}\left(s_S^* \int_{\phi_S}^{\infty} \big(u(w) - u(\phi_S)\big) dF(w)\right) \quad (7)$$

Once the steady-state values s_S^* and ϕ_S are computed, the system can be solved recursively for earlier periods with equations 4 and 5.

2.2. UI and job search

UI generosity directly affects job search decisions, as workers weigh the utility of continued unemployment against employment when choosing their search intensity s_t and reservation wage ϕ_t. The effects of a change in benefit levels can be observed from equations 6 and 7:[2]

Lower UI benefits ($b_S \downarrow$) reduce the utility of being unemployed, leading workers to lower their reservation wage ($\phi_t \downarrow$) as they become willing to accept lower compensation to exit unemployment. A lower ϕ_t expands the integral term in equation 6, since a larger share of job offers now exceeds the reservation wage. This raises the expected return to job search and thereby increases optimal search effort ($s_S^* \uparrow$). The intuition is straightforward: If the range of acceptable job offers expand, it becomes more beneficial to search.

The derivation of optimal values of s_t^* and ϕ_t allows for the calculation of the hazard rate, which represents the probability of exiting unemployment at time t conditional on being unemployed for the previous $t - 1$ periods. It is written:

$$\underbrace{h_t}_{\substack{\text{Probability of} \\ \text{unemployment exit}}} = \underbrace{s(e_t)}_{\substack{\text{Probability of} \\ \text{receiving a job offer}}} \times \underbrace{1 - F(\phi_t)}_{\substack{\text{Probability of} \\ \text{accepting a job offer}}} \tag{8}$$

Consequently, a reduction in UI benefits exert a positive influence on the hazard rate through two channels:

- Increased search effort which raises the probability of receiving a job offer, and
- Reduced reservations wage which increases the probability of accepting a job offer

2.3. Simulation

Figure 1 shows simulations of the Mortensen model under the graduate UI regimes before and after 2023. The exact assumptions are listed in appendix B. Theory predicts that hazard rates follow an exponential pattern: initially low and stable, increasing sharply as benefit exhaustion approaches, after which search stabilizes due to perpetual social assistance. The pattern reflects the fact that workers respond to an increased risk of running out of benefits as the duration of unemployment increases.

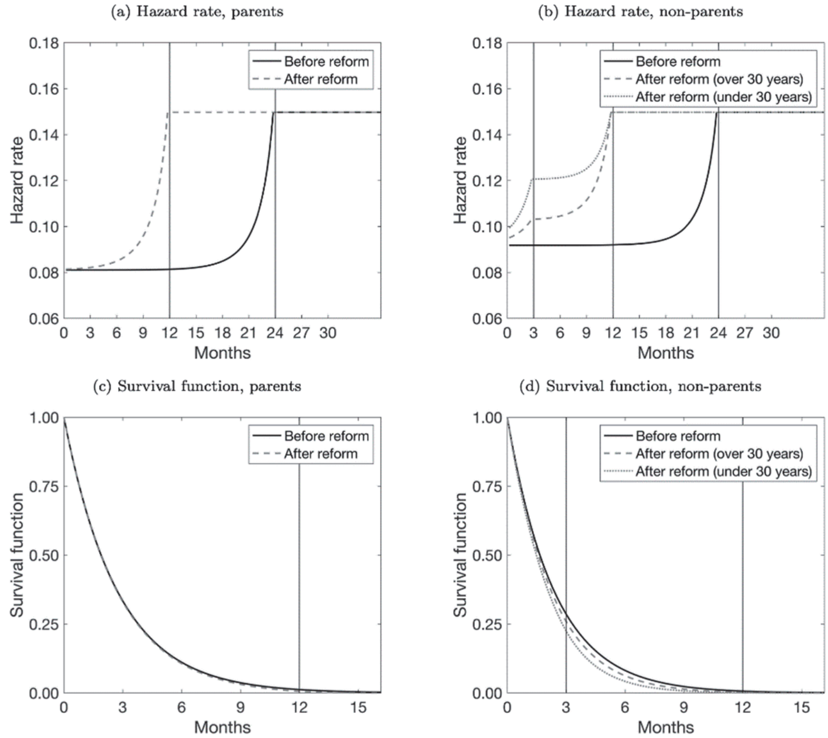
As shown in panel a, the shortened benefit period has minimal immediate effects on unemployment exit for parents. After all, at the start of unemployment, whether a worker has 52 or 104 weeks of benefits remaining has little bearing on their initial actions. The effect emerges around six months, accelerating job exits as the new expiration point nears. Panel b shows a stronger effect for non-parents, who face benefit reductions after three months. The effect is most pronounced for non-parents under 30 years, who face a larger reduction. Their hazard rate rises

2. See appendix A for a formal comparative statics derivation.

rapidly early in the spell before following a new trajectory toward benefit expiration at 12 months. This reflects the new two-tier structure of UI benefits for non-parents with a reduction in benefits after 3 months of unemployment.

Panels c and d summarize the effects using survival functions, showing that benefit level reductions have a greater cumulative impact than PBD cuts. Since PBD reductions primarily affect later stages of unemployment, their influence is limited, whereas lower benefit levels impact a larger share of job seekers earlier in the spell.

Figure 1: Simulation of the Mortensen (1977) model



Note: Vertical lines indicate the timing of a benefit reduction.
Source: Own calculations.

3. The 2023 reform of graduate UI in Denmark

A key feature of the Danish UI system is that graduates qualify for benefits immediately upon completing their studies.[3] These benefits, known as graduate UI (dimittenddagpenge), are provided at reduced levels and for shorter durations compared to standard UI.

3. Eligibility criteria are detailed on a-kasser.dk

In May 2023, reductions were made to the level and duration of UI benefits for graduates (see figure 2). Prior to the reform, parents (graduates with dependent children under 18) received 16,177 DKK per month, while non-parents received 14,106 DKK, both for up to 24 months.

Following the reform, the PBD was cut to 12 months for all graduates. This change affects graduates who applied for benefits on or after May 1, 2023. Earlier applicants retained the 24-month period.

Benefit levels remained unchanged for parents but were adjusted for non-parents. For the first three months of unemployment, benefit rates remained at pre-reform levels. Thereafter, non-parents over 30 years old saw their benefits reduced to 12,253 DKK, while those under 30 received 9,700 DKK per month. This represents a 13.1 % and a 31.2 % decrease respectively. The reduction in benefit levels applied retroactively implying that all unemployed non-parents as of May 2023 became subject to the new lower rates, regardless of their enrollment date.

A reduction in UI generosity for recent graduates was first proposed by the government in the fall of 2021. A political agreement was reached in January 2022, but implementation was delayed until the spring of 2023 due to an unexpected general election in the fall of 2022. This timeline may have generated announcement effects, as some students could have accelerated their graduation to qualify for the more generous pre-reform benefit level and duration. However, as the reform took effect in May and most programs graduate students in either January or June, doing so would have required advancing their study plans by half a year on relatively short notice.

Figure 2: Unemployment insurance for graduates before and after reform

DKK per month 2023-level

Note: Unemployment insurance benefit levels and duration for graduates enrolled before (dotted line) and after (solid line) May 1, 2023.

Source: Beskæftigelsesministeriet (2022).

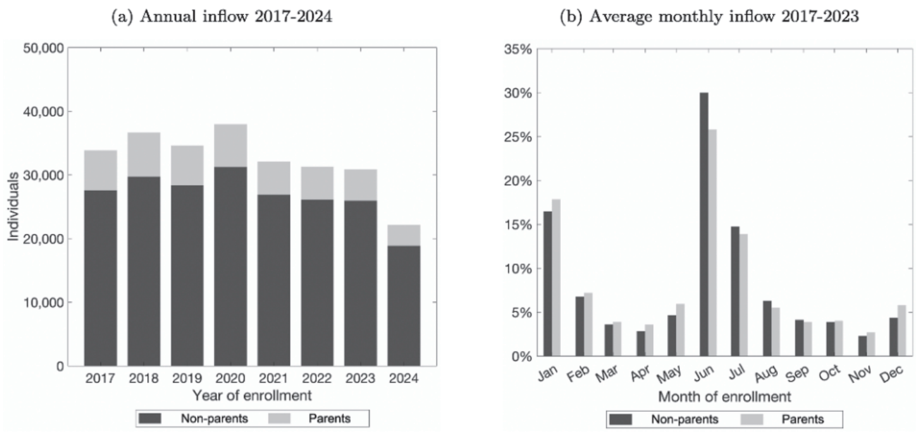
4. Data

4.1. Graduate unemployment inflow

The estimation relies on administrative data from the Danish Agency for Labour Market and Recruitment (STAR) and Statistics Denmark. The dataset spans from early 2017 to August 2024, to avoid individuals affected by the 2017 reform. I identify recent graduates entering UI immediately after completing their studies and track unemployment spells using STAR's DREAM longitudinal database. DREAM provides weekly records of public benefit recipients since 1992, with the latest version covering data up to week 35 of 2024. Each unemployment spell is merged with relevant Statistics Denmark registers to obtain background characteristics.

Between January 2017 and August 2024, 259,155 graduates entered the UI system. Of these, 82.8 % were non-parents, and 17.2 % were parents. Figure 3 shows a slight decline in annual entries and pronounced spikes in January, June, and July – typical graduation months.

Figure 3: Graduate unemployment inflow



Note: Inflow is lower in 2024 because data only extends to week 35. Average monthly Inflow Is only calculated on enrollments between 2017 and 2023.
Source: Own calculations based on DREAM and registers from Statistics Denmark.

4.2. Unemployment exit

Each identified spell is tracked until exit from unemployment. An exit occurs in week t if no UI benefits are received for five consecutive weeks from $t+1$ to $t+5$. This avoids misclassifying temporary UI disruptions as exits. An exit to employment requires at least three of five weeks in employment, while exits to other benefits require three weeks on another benefit. Self-sufficiency exits occur when neither employment nor other benefits are recorded. If no exit is observed by the end of the study period, the unemployment spell is right-censored. Table 1 shows that

78.1 % transition to employment, 9.8 % to other benefits, and 7.3 % to self-sufficiency. The remaining unemployment spells are right censored.

Because the data period only extends to week 35 of 2024, many 2024 spells are censored (46 % versus 6 % in 2023). Some of these may be incorrectly right-censored if an exit occurs between weeks 31 and 35 of 2024, which would not allow for the consecutive five-week period without benefit receipt. This is particularly problematic for graduates, who typically enroll between weeks 23 and 26 (June). For these individuals, an exit to employment will only be observable if their unemployment spell is shorter than 5 to 8 weeks; otherwise, it will be right-censored, effectively being counted as no exit. This issue is important to consider in relation to the reduction in benefit levels after 13 weeks. Including 2024 enrollments may therefore introduce a downward bias in estimating the effect of the reduction in benefit levels. I therefore exclude all spells beginning in 2024 from the main estimations.

Table 1: Unemployment exit

	In total		Non-parents		Parents	
Employment	202,308	78.1%	167,879	78.3%	34,429	77.1%
Maternity/Paternity	7,954	3.1%	4,508	2.1%	3,446	7.7%
Sickness	6,918	2.7%	4,901	2.3%	2,017	4.5%
Self sufficiency	18,818	7.3%	16,937	7.9%	1,881	4.2%
State educational support (SU)	4,842	1.9%	4,252	2.0%	590	1.3%
Others	5,698	2.2%	5,301	2.5%	397	0.9%
Right censored	12,617	4.9%	10,722	5.0%	1,895	4.2%
Individuals	259,155		214,500		44,655	

Note: Exit for graduates enrolled in the UI system between January 2017 and August 2024.
Source: Own calculations based on DREAM and registers from Statistics Denmark.

5. Empirical strategy

As outlined, the UI reform affected graduates differently depending on their parental status. The reduction in benefit levels applied exclusively to non-parents, while the reduction in PBD applied to all graduates. This variation in treatment assignment creates a quasi-experimental setting, allowing for an estimation of the reform's impact on unemployment exits. The objective is to assess the two policy changes separately, and the analysis is structured accordingly. The first part examines the effect of the reduction in benefit levels for non-parents, while the second evaluates the impact of the shortened PBD.

5.1. Reduction in benefit level for non-parents

A simple before-after comparison of unemployment exit for non-parents would confound reform effects with broader labor market changes. Similarly, comparing non-parents to parents *post-reform* mitigates business cycle effects but introduces selection bias, as unemployment incentives may differ by parental status.

To address these concerns, I use a Difference-in-Differences (DiD) approach. Unemployed graduates are grouped into parents (control) and non-parents (treatment), with only the latter experiencing a benefit level cut. The treatment effect is identified by comparing changes in unemployment exit rates between groups before and after the reform. This approach mitigates the issues of both business cycle fluctuations and selection bias, provided that the parallel trends assumption holds.

The parallel trends assumption requires that, in the absence of the benefit reduction, unemployment exit rates for parents and non-parents would have followed similar trajectories over time. This assumption has two key implications. First, both groups are expected to respond similarly to macroeconomic fluctuations. Second, they are assumed to exhibit comparable responses to the reduction in PBD, despite non-parents being subject to an additional treatment – the cut in benefit levels.

The empirical analysis is conducted in a discrete-time duration framework, where the weekly probability of exiting unemployment is modeled using a linear probability model (LPM) (Jenkins, 1995). Each unemployment spell is divided into weekly intervals, and the dependent variable equals one in the week of exit and zero otherwise. Spells that remain open by the end of the observation period are right-censored, and these observations contribute information for all completed weeks prior to censoring.

This approach differs from the more common proportional hazards models, such as the Cox model or the complementary log–log model. I use the LPM approach for direct interpretation of coefficients as percentage-point effects on weekly exit probabilities, avoiding the tie-handling complications of continuous-time models such as Cox and the less transparent log hazard ratio interpretation of complementary log–log models (Wooldridge 2010).

In practice I estimate equation 9 with OLS:

$$\begin{aligned} h_{is}(t) &= \alpha + X_i'\beta + c_\tau'\gamma + \delta(t) + \lambda(t)D_\tau^{post} \\ &+ \sum_{g\in(U30,O30)} \left[\theta_g(t)D_{sg}^{treat} + \xi_g(t)\left(D_{sg}^{treat} \times D_\tau^{post}\right)\right] + \epsilon_{ist} \end{aligned} \tag{9}$$

where $h_{is}(t)$ is a binary variable indicating exit to employment after t weeks of unemployment. Subscript s and τ denote parental status and calendar time respectively. Covariates X_i include gender, descent, age, marital status, region, financial wealth, education, and job experience. I also add time-varying macroeconomic variables and calendar dummies encapsulated in c_τ. This includes the unemployment rate and the number of open vacancies on Jobnet.dk.

No parametric restrictions are placed on the baseline hazard shape. Time dependence is captured flexibly via week dummies: $\delta(t)$, $\theta(t)$, $\lambda(t)$, $\xi(t)$.

Specifically, $\delta(t)$ estimates the pre-reform hazard rates for parents, while $\delta(t) + \lambda$ estimates the post-reform hazard rates of the same group.

$\delta(t) + \theta_g(t)$ estimates the hazard rates for non-parents (aged under 30 or 30 and over at enrollment)[4] before the reform, and $\delta(t) + \lambda(t) + \theta_g(t) + \xi_g(t)$ estimates the post-reform rates.

Consequently, given that the assumptions of the DID-estimator are fulfilled, $\xi(t)$ estimates the treatment effect of the reduction in benefit levels for non-parents. Inspired by Hermansen (2014), I use a piecewise constant specification for the implementation of the hazard functions allowing for arbitrary shapes of the exit profile.

5.2. Shortening of the potential benefit duration

The second part of the analysis concerns the effect of the shortening of the PBD. This part of the reform applied to both non-parents and parents; however, since non-parents were also affected by the benefit level reduction, the analysis focuses on parents to isolate the effect of PBD shortening. This avoids confounding influences from changes in benefit rates.

The identification strategy follows a pre-post design similar to Hermansen (2014) and Jonassen (2014), who analyzed the 2010 UI reform that reduced the benefit period from four to two years. These studies compared unemployment exits before and after implementation to assess how the reform influenced job search behavior over the unemployment spell. Once again, the analysis is conducted in a discrete-time duration framework estimated using an LPM. Specifically, I estimate:

$$h_{is}(t) = \alpha + X_i'\beta + c_\tau'\gamma + \delta(t) + \lambda(t)D_\tau^{post} + \epsilon_{ist}, \quad s = parents \tag{10}$$

The difference across time within parents potentially cancel out the selection bias discussed above. On the contrary, time effects remain an issue. If one is willing to assume that the time effects are identical before and after the reform, then the causal effect of the shortening of the PBD is isolated and captured in $\lambda(t)$. This is a more strict assumption than the one required for the DiD analysis which only required the trends in the time-effects to be parallel. In practice, I pick a treatment and control period with similar macroeconomic environments and seasonal effects (see section 6.2). In addition to this, I test the robustness of my estimates by varying the estimation period. However, the results should be interpreted with much care.

4. I also restrict this to individuals aged under 29 or 30 and over to avoid misclassifying those who turn 30 during their unemployment spell. This restriction has a negligible impact on the results.

6. Empirical results

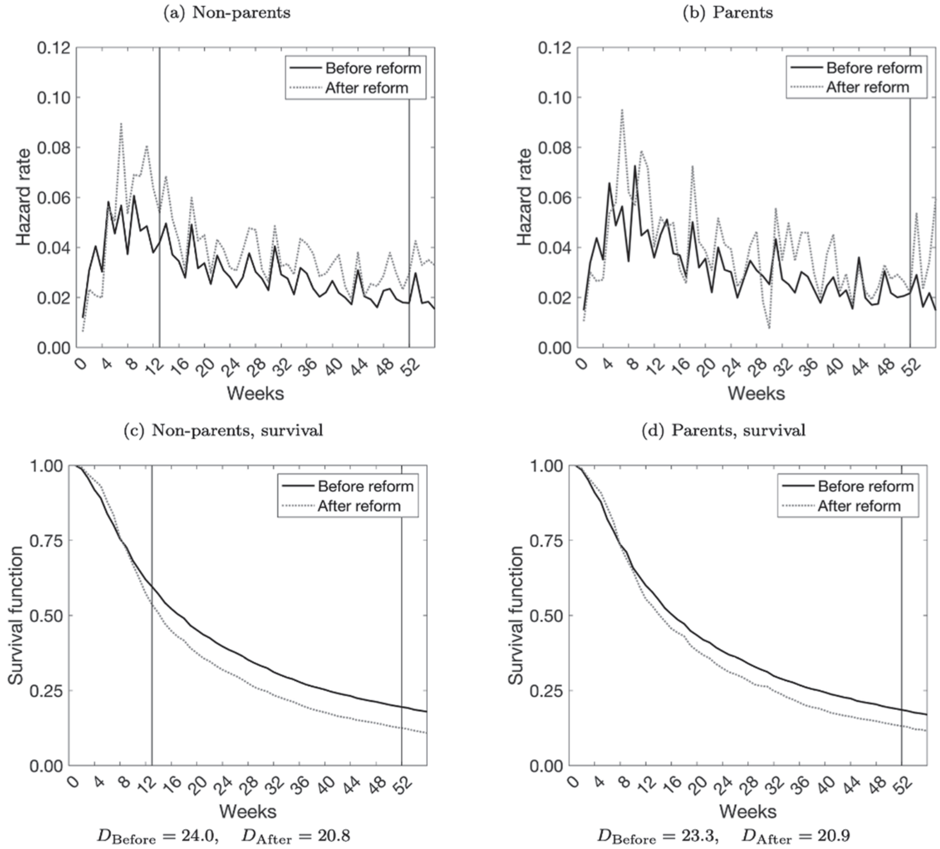
This section presents the estimated effects of the reform of graduate UI in 2023. To contextualize the results, it is useful to first visually explore job-seeking behavior before and after the reform. This is done by examining the empirical hazard rates for exits to employment alongside their corresponding survival functions, as illustrated in figure 4.

With the exception of the first six weeks, the hazard rates for non-parents after the reform consistently exceed those observed before the reform. This effect is particularly notable between weeks 8 and 16 of unemployment, where the post-reform hazard rate is approximately 3 percentage points higher than the pre-reform rate. This pattern indicates an increased likelihood of unemployment exits around the 13-week (3 months) threshold, coinciding with the benefit reduction.

For parents, the post-reform hazard rates exhibit greater variance due to the smaller sample size. Nonetheless, the hazard rates generally appear higher after the reform, particularly during the early weeks of unemployment and following the expiration of benefits at 12 months.

Unlike the exponential pattern of the theoretical hazard rates presented in section 2, the empirical hazard rates appear to display a U-shape, with higher exit rates at both the beginning and end of the PBD. As noted by Le Barbanchon et al. (2024), the initially high exit rate may reflect the rapid re-employment of high-skilled workers with lower search costs. Over time, this leaves behind a population increasingly composed of lower-skilled workers with lower individual hazard rates which reduces the aggregate hazard rate. Toward the end of the PBD, however, job search effort tends to intensify, which raises individual hazard rates and contributes to the observed rise in the aggregate hazard rate.

Figure 4: Empirical hazard rates before and after reform



Note: The vertical lines indicate the timing of the benefit reduction and the exhaustion of the benefit period. The »before reform« cohort consists of individuals enrolled between January 2017 and April 2022, while the »after reform« cohort includes those enrolled between May 2023 and December 2023.

Source: Own calculations based on DREAM and registers from Statistics Denmark.

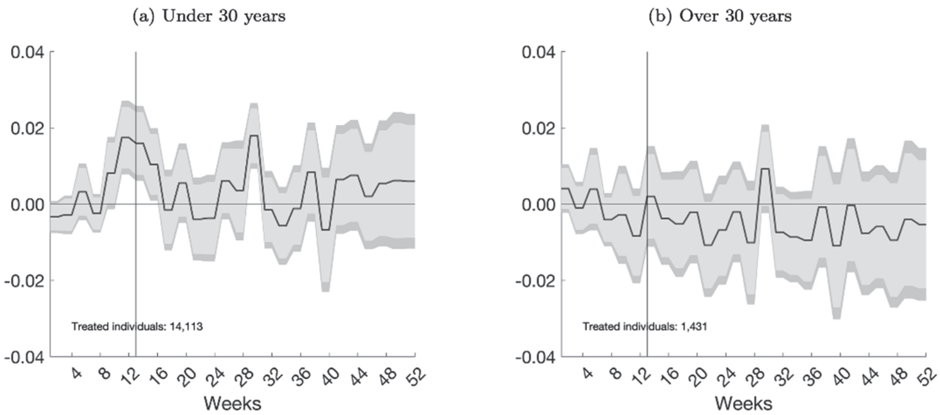
The survival functions shown in panels c and d align with these observations. For non-parents, there is a substantial post-reform increase in unemployment exits beginning around week 10. Parents also show a post-reform increase in exits after 10 weeks, though the magnitude is smaller. Non-parents exhibit a reduction in expected unemployment duration of 3.2 weeks, compared to 2.4 weeks for parents. This difference aligns with the distinct exposure to the reform: non-parents were affected by both the reduction in benefit levels and the shortened benefit period, while parents were impacted only by the latter.[5]

5. These estimates are derived under the constraint that unemployment spells are censored at 56 weeks due to data limitations after the reform. Consequently, the actual expected durations are likely higher. For reference, the uncensored expected duration of unemployment for non-parents before the reform was 32 weeks compared to 24 when censored at 56 weeks.

6.1. Estimation results of the effects of a reduction in benefit levels for non-parents

Visual inspection showed suggestive evidence of a reform effect, but a proper econometric evaluation is needed. For a formal econometric investigation, I estimate equation 9. I use individuals enrolled within January 2017 to April 2022 as the control period and May 2023 to December 2023 as the treatment period. These periods align with those used for deriving the empirical hazard rates shown in figure 4. I exclude the period from May 2022 to April 2023 to prevent individuals in the control group from being inadvertently treated if they remain unemployed after the reform's implementation. Furthermore, I exclude individuals enrolled in 2024 as a large fraction of these are right censored, and thus no exit is observed by the end of the study period. Inclusion of these observations would cause a downward bias to the estimates. The estimated treatment effects for non-parents below and above 30 years, respectively $\xi_{U30}(t)$ and $\xi_{O30}(t)$, are presented in figure 5.

Figure 5: Effect of a reduction in benefit levels for non-parents



Note: OLS estimates of $\xi_{U30}(t)$ and $\xi_{O30}(t)$ from equation 9. The vertical lines indicate the timing of the benefit reduction after 13 weeks of unemployment. Dark gray (light gray) area marks a 95 % (90 %) confidence interval.

Source: Own calculations based on DREAM and registers from Statistics Denmark.

For non-parents under 30 years, the treatment effect appears statistically insignificant and close to zero during the first 8 weeks of unemployment. It increases to 0.8 percentage points in weeks 9-10 and becomes borderline significant at 10 %. A substantial effect of the reform emerges between weeks 11 and 14, where the weekly hazard rate for non-parents rises by 1.5 percentage points, significant at the 1 % level. The effect then declines to 0.9 percentage points during weeks 15-16, significant at the 5 % level. Beyond week 16, the treatment effect fluctuates around zero without achieving statistical significance, except for a peak of 1.9 percentage points in weeks 29-30. This late increase lacks a clear economic rationale, and as depicted in figure 4 it is caused by a concurrent drop in hazard rates for parents enrolled after the reform rather than increased exit for non-parents. I suspect this increase is caused by parents enrolled over the summer of 2023 who for some reason have trouble finding employment in January of 2024. New analyses with a larger data foundation will likely not encounter this issue.

Contrary to non-parents below 30 years, no significant effects are observed for non-parents above 30 years. In fact, the results indicate a negative but statistically insignificant response to the reform for this group. The lack of significance may be partly due to the smaller reduction in benefit levels faced by non-parents above 30 years: non-parents below 30 years experienced a 31 % reduction compared to a 13 % reduction for those above 30 years. Additionally, the small size of this treatment group, comprising less than 10 % of unemployed non-parent graduates, may contribute to the lack of precision in the estimates.

6.1.1. Robustness tests

I conduct a series of robustness checks and placebo tests to assess the validity of the econometric design. For simplicity, in the robustness tests I pool non-graduates under and over 30 years into a single treatment group, so that equation 9 becomes a DiD with one treatment group. Running the tests separately for each age group produces similar results.

First, I estimate equation 9 while progressively incorporating additional control variables to assess robustness to omitted covariates. The results, presented in table 2, show minimal variation in the estimates of $\xi(t)$ across specifications. I also include exits to self-sufficiency to capture potential missed exits to employment; this too does not affect the results.

Second, I assess the robustness of the estimation results by varying the control and treatment periods. These results are presented in table 3 and should be compared with column 4 of table 2, which contains the full specification estimated on the complete sample.

Column 1 of table 3 excludes UI enrollments during 2020–2021, the years most affected by COVID-19; results are nearly unchanged, indicating little impact from including the pandemic period. Column 2 extends the control period to April 2023, which, due to retroactive benefit reductions, introduces treatment into the control group and slightly lowers the effect, though it remains significant and positive.

To address potential seasonality, I align control and treatment periods. Column 3 restricts both to May-December enrollments, yielding smaller effects; column 4 uses May-July enrollments, producing stronger effects, consistent with greater responsiveness among summer graduates from long-cycle programs.

Finally, column 5 reverts to the original control period but extends the treatment period to July 2024. As discussed in section 6.1, 2024 spells may be misclassified as right-censored, and the estimated effect between weeks 11-14 falls from 1.5 to about 1 percentage point, consistent with negative bias.

Finally, I conduct placebo tests by shifting the reform date to May 1 in each of the years 2018-2022 to further test the validity of the design. The results, shown in Appendix C reveal no significant effects in these placebo regressions, as expected.

Table 2: Robustness test, reduction in benefit level for non-parents

	(1) Time effects	(2) Personal characteristics	(3) Job experience	(4) Macroeconomic variables	(5) Incl. exit to self-sufficiency
Week 1-2	-0.003	-0.002	-0.003	-0.003	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Week 3-4	-0.003	-0.002	-0.003	-0.003	-0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Week 5-6	0.003	0.004	0.003	0.003	0.003
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Week 7-8	-0.004	-0.003	-0.004	-0.003	-0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Week 9-10	0.007	0.007	0.007	0.007	0.007
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Week 11-12	0.015***	0.015***	0.014***	0.015***	0.014***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Week 13-14	0.014***	0.014***	0.014***	0.014***	0.013**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Week 15-16	0.008*	0.009*	0.008*	0.009*	0.009*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Week 17-18	-0.003	-0.002	-0.002	-0.002	-0.001
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Week 19-20	0.004	0.005	0.004	0.004	0.005
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Week 21-22	-0.005	-0.004	-0.005	-0.005	-0.007
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)
Week 23-24	-0.004	-0.003	-0.004	-0.004	-0.003
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Week 25-26	0.006	0.006	0.005	0.005	0.006
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Week 27-28	0.002	0.003	0.001	0.002	0.002
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Week 29-30	0.017***	0.018***	0.016***	0.017***	0.018***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Week 31-32	-0.002	-0.002	-0.003	-0.003	-0.003
	(0.005)	(0.005)	(0.005)	(0.004)	(0.005)
Week 33-34	-0.006	-0.006	-0.006	-0.006	-0.007
	(0.004)	(0.005)	(0.005)	(0.005)	(0.004)
Week 35-36	-0.002	-0.003	-0.003	-0.003	-0.003
	(0.006)	(0.005)	(0.005)	(0.005)	(0.006)
Week 37-38	0.007	0.007	0.007	0.007	0.009
	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)
Week 39-40	-0.007	-0.008	-0.008	-0.008	-0.006
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Week 41-42	0.006	0.005	0.005	0.005	0.007
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Week 43-44	0.006	0.005	0.005	0.005	0.001
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)
Week 45-46	0.001	0.000	0.000	0.001	0.003
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Week 47-48	0.003	0.002	0.003	0.003	0.005
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Week 49-50	0.005	0.004	0.004	0.004	0.002
	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)
Week 51-52	0.005	0.005	0.004	0.004	0.006
	(0.009)	(0.009)	(0.009)	(0.009)	(0.011)
Month and year fixed effects	x	x	x	x	x
Demography & geography		x	x	x	x
Net wealth		x	x	x	x
Education		x	x	x	x
Previous job experience			x	x	x
Macroeconomic variables				x	x
Treatment period	May 23-Dec 23	May 23-Dec 23	May 23-Dec 23	May 23-Dec 23	May 23-Dec 23
Control period	Jan 17-Apr 22	Jan 17-Apr 22	Jan 17-Apr 22	Jan 17-Apr 22	Jan 17-Apr 22
Individuals	203,238	203,238	203,238	203,238	203,238

Note: OLS estimates of $\xi(t)$ from equation 9. Non-graduates under and over 30 years are pooled into a single treatment group. Robust standard errors in parentheses. $* \, p < 0.10$, $** \, p < 0.05$, $*** \, p < 0.01$.

Source: Own calculations based on DREAM and registers from Statistics Denmark.

Table 3: Robustness test, reduction in benefit level for non-parents

	(1) Excluding 2020-2021	(2) Including 2022	(3) May-December	(4) Summer cohorts	(5) Including 2024
Week 1-2	-0.002	-0.002	-0.004*	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Week 3-4	-0.002	-0.002	-0.006**	-0.001	0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
Week 5-6	0.005	0.004	0.002	0.002	0.001
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
Week 7-8	-0.003	-0.002	-0.003	-0.003	-0.002
	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)
Week 9-10	0.005	0.007	0.008	0.006	0.004
	(0.005)	(0.005)	(0.005)	(0.006)	(0.003)
Week 11-12	0.015***	0.014***	0.013***	0.017***	0.010***
	(0.005)	(0.005)	(0.005)	(0.006)	(0.003)
Week 13-14	0.015***	0.014***	0.014***	0.014**	0.011***
	(0.005)	(0.005)	(0.005)	(0.006)	(0.004)
Week 15-16	0.010**	0.008*	0.008*	0.006	0.009**
	(0.005)	(0.005)	(0.005)	(0.006)	(0.004)
Week 17-18	-0.002	-0.002	-0.002	-0.000	-0.001
	(0.005)	(0.005)	(0.005)	(0.006)	(0.004)
Week 19-20	0.004	0.004	0.005	0.004	0.005
	(0.005)	(0.005)	(0.005)	(0.006)	(0.004)
Week 21-22	-0.005	-0.005	-0.002	-0.005	-0.004
	(0.006)	(0.005)	(0.005)	(0.006)	(0.004)
Week 23-24	-0.003	-0.004	-0.002	-0.008	-0.001
	(0.006)	(0.006)	(0.005)	(0.007)	(0.005)
Week 25-26	0.007	0.006	0.002	0.006	0.003
	(0.005)	(0.005)	(0.005)	(0.006)	(0.004)
Week 27-28	0.002	0.001	0.000	0.004	-0.004
	(0.007)	(0.007)	(0.006)	(0.008)	(0.006)
Week 29-30	0.018***	0.016***	0.015***	0.012**	0.015***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)
Week 31-32	-0.003	-0.003	-0.000	-0.004	-0.001
	(0.005)	(0.004)	(0.005)	(0.006)	(0.004)
Week 33-34	-0.005	-0.006	-0.006	-0.008	-0.006
	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)
Week 35-36	-0.002	-0.003	-0.002	-0.006	-0.002
	(0.005)	(0.005)	(0.004)	(0.07)	(0.005)
Week 37-38	0.007	0.007	0.004	0.002	0.006
	(0.007)	(0.006)	(0.006)	(0.008)	(0.006)
Week 39-40	-0.006	-0.008	-0.006	-0.011	-0.005
	(0.008)	(0.008)	(0.007)	(0.010)	(0.007)
Week 41-42	0.006	0.005	0.005	0.000	0.004
	(0.007)	(0.007)	(0.007)	(0.009)	(0.007)
Week 43-44	0.005	0.004	0.005	0.002	0.005
	(0.007)	(0.007)	(0.007)	(0.009)	(0.007)
Week 45-46	0.001	0.001	-0.002	0.001	0.001
	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)
Week 47-48	0.005	0.003	0.004	0.002	0.003
	(0.008)	(0.008)	(0.008)	(0.010)	(0.008)
Week 49-50	0.005	0.004	0.001	-0.002	0.004
	(0.009)	(0.009)	(0.009)	(0.011)	(0.009)
Week 51-52	0.004	0.004	0.004	0.009	0.004
	(0.009)	(0.009)	(0.009)	(0.010)	(0.009)
Month and year fixed effects	x	x	x	x	x
Demography & geography	x	x	x	x	x
Net wealth	x	x	x	x	x
Education	x	x	x	x	x
Previous job experience	x	x	x	x	x
Macroeconomic variables	x	x	x	x	x
Treatment period	May 23-Dec 23	May 23-Dec 23	May 23-Dec 23	May 23-Jul 23	May 23-Jul 24
Control period	Jan 17-Dec 19 & Jan 22-Apr 22	Jan 17-Apr 23	May-Dec, 17-21	May-Jul, 17-21	Jan 17 - Apr 22
Individuals	131,023	235,453	142,778	95,341	225,870

Note: OLS estimates of $\xi(t)$ from equation 9. Non-graduates under and over 30 years are pooled into a single treatment group. Robust standard errors in parentheses. $*\,p < 0.10$, $**\,p < 0.05$, $***\,p < 0.01$

6.2. Shortening of the potential benefit duration

This section analyzes the effect of shortening the unemployment benefit period from 24 to 12 months by comparing unemployment exit for parents before and after the reform. As discussed, the identification strategy employed here is weaker than the Difference-in-Differences approach used to analyze the reduction in benefit levels, as it relies on stricter assumptions. The key assumption is that, in the absence of the reform, the hazard rate for parents would have remained unchanged. For this assumption to hold, specific criteria must be met regarding the control and treatment periods.

First, macroeconomic conditions and labor market dynamics should be similar in the control and treatment periods to avoid confoundings by business cycles effects. Additionally, the control and treatment periods should cover the same months of the year to account for any potential seasonal variation. Temporal proximity is also essential to ensure that the characteristics of individuals in the control and treatment groups remain comparable. Lastly, these requirements should be met while maintaining a sufficiently large sample size, which can be challenging given that parents represent a small subset of unemployed graduates.

To address the challenges discussed above and assess the sensitivity of the results to period selection, I estimate equation 10 under several specifications. Results are presented in table 4. First, I use the full sample spanning January 2017 to December 2023 as a baseline in column 1 of table 4. However, this specification is unlikely to produce reliable results due to substantial business cycle fluctuations over the period. Next in column 2, I restrict the sample to enrollments in 2023, defining January-April as the control period and May-December as the treatment period. While this specification reduces the impact of business cycle effects, it fails to account for seasonal differences, leading to unrealistically high estimates likely driven by elevated summer exit rates.

I then narrow the control period to match the treatment period in terms of months covered. In column 3, I define it as May-December in 2021 and 2022. In column 4 I restrict the control period further to May-December in 2022, ensuring closer temporal proximity to the treatment period. Finally, in my preferred specification column 5, I extend the treatment period to May 2023-April 2024 and define the control period as May 2022-April 2023. This specification aligns the control and treatment periods in terms of months covered, reducing seasonal bias, while spanning an entire calendar year, which addresses potential differences in the timing of graduate entries into the unemployment insurance system. Moreover, the macroeconomic conditions during this period were relatively stable (Beskæftigelsesministeriet 2025; De Økonomiske Råd 2024; Nationalbanken 2024; Økonomiministeriet 2024). A potential drawback is that enrollments occurring in 2024 may be subject to right-censoring, which could introduce a downward bias in the estimates.

Table 4: Effect of a shortening of the benefit period for parents

	(1) Full sample	(2) Only 2023	(3) May-Dec, 2021-2023	(4) May-Dec, 2022-2023	(5) May 2022 - May2024
Week 1-4	-0.011***	0.053***	0.004*	0.019***	0.004**
	(0.002)	(0.006)	(0.002)	(0.005)	(0.002)
Week 5-8	0.007**	0.059***	0.005	0.019***	0.003
	(0.003)	(0.006)	(0.003)	(0.005)	(0.004)
Week 9-12	0.004	0.045***	0.001	0.012**	0.000
	(0.003)	(0.006)	(0.004)	(0.006)	(0.004)
Week 13-16	-0.002	0.039***	-0.001	0.012**	0.000
	(0.003)	(0.006)	(0.004)	(0.006)	(0.004)
Week 17-20	0.005	0.040***	0.008*	0.022***	0.009**
	(0.003)	(0.006)	(0.004)	(0.006)	(0.004)
Week 21-24	0.008**	0.042***	0.012***	0.023***	0.010**
	(0.004)	(0.006)	(0.005)	(0.006)	(0.004)
Week 25-28	0.005	0.037***	0.009**	0.014**	0.006
	(0.004)	(0.007)	(0.005)	(0.006)	(0.004)
Week 29-32	-0.005	0.022***	0.003	0.010*	-0.003
	(0.004)	(0.008)	(0.005)	(0.006)	(0.004)
Week 33-36	0.014***	0.034***	0.021***	0.022***	0.009*
	(0.005)	(0.009)	(0.006)	(0.007)	(0.006)
Week 37-40	0.007	0.033***	0.012**	0.013*	0.005
	(0.005)	(0.008)	(0.006)	(0.007)	(0.005)
Week 41-44	0.001	0.024***	0.010*	0.011*	0.008
	(0.005)	(0.008)	(0.005)	(0.006)	(0.005)
Week 45-48	0.002	0.020**	0.004	-0.000	0.002
	(0.005)	(0.008)	(0.006)	(0.007)	(0.006)
Week 49-52	0.007	0.014	0.007	0.009	0.009
	(0.006)	(0.009)	(0.007)	(0.007)	(0.007)
Week 53-56	0.022**	0.040***	0.021**	0.020*	0.025**
	(0.009)	(0.011)	(0.010)	(0.010)	(0.010)
Treatment period	May 23-Dec 23	May 23-Dec 23	May 23-Dec 23	May 23-Dec 23	May 23-Apr 24
Control period	Jan 17-Apr 23	Jan 23-Apr 23	May-Dec, 21-22	May 22-Dec 22	May 22-Apr 23
Individuals	41,077	4,597	9,574	6,036	9,019

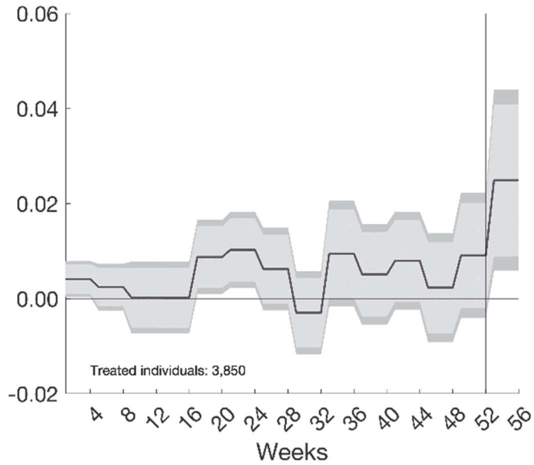
Note: OLS estimates of $\lambda(t)$ from equation 10. Robust standard errors in parentheses. $*$ $p < 0.10$, $**\, p < 0.05$, $***\, p < 0.01$.

Source: Own calculations based on DREAM and registers from Statistics Denmark.

Table 4 reveal considerably more variation in the estimates compared to the analysis of benefit level reductions. This highlights a major limitation of the identification strategy – namely, that the estimated effects are highly sensitive to the choice of control group. Nevertheless, despite the large variation all five specifications consistently estimate an increase in the hazard rate of approximately 2 percentage points during weeks 53-56, the period immediately following the expiration of benefits under the reform. This finding suggests that the shortening of the benefit period increased exits to employment after benefit exhaustion. Unfortunately, due to data limitations, I cannot estimate treatment effects beyond 56 weeks, which restricts the ability to analyze longer-term behavioral changes.

The estimates from my preferred specification column 5, presented in figure 6, reveal no noticeable effect on the hazard rate during the first 16 weeks of unemployment. Between weeks 17 and 52, the weekly hazard rate increases by approximately 1 percentage point, periodically significant. At weeks 53-56, corresponding to the period immediately after benefit expiration under the reform, the hazard rate shows a significant increase of 2.5 percentage points, indicating a substantial impact of the reform on employment exits after exhaustion.

Figure 6: Effect of a shortening of the benefit period in prefered specification



Note: Plot of column 5 in table 4. The vertical line indicates the timing of the benefit exhaustion. Dark gray (light gray) area marks a 95 % (90 %) confidence interval.

Source: Own calculations based on DREAM and registers from Statistics Denmark.

7. Employment effect

Despite some limitations in the design, the estimated effects of both reform initiatives point to a positive impact. To assess the reform's implications for graduate unemployment, I follow Hermansen (2014) and compare predicted hazard rates under the reform with counterfactual hazard rates that simulate outcomes had the reform not been implemented. This approach maintains comparability by using the same observable characteristics of the unemployed and controlling for macroeconomic conditions.

The analysis of reduced benefit levels focuses exclusively on non-parents under 30 years old, as no significant effects were observed for non-parents over 30. Using the DiD model outlined in equation 9, I calculate the predicted hazard rate for each non-parent under 30 enrolled after the reform. These rates are conditional on individual characteristics, macroeconomic factors, and seasonal conditions at the week of exit. I aggregate the individual hazard with an arithmetic average to obtain a predicted aggregate hazard rate:

$$\bar{h}_{U30}^{post}(t) = \frac{1}{N_{U30}^{post}} \sum_{i=1}^{N_{U30}^{post}} \left[\hat{\alpha} + X'^{post}_{i}\hat{\beta} + c'_{\tau}\hat{\gamma} + \hat{\delta}(t) + \hat{\theta}_{U30}(t) + \hat{\lambda}(t) + \hat{\xi}_{U30}(t)\right] \quad (11)$$

Based on this I calculate the counterfactual hazard rates by subtraction of the treatment effect $\hat{\xi}_{U30}(t)$.

Notably, the counterfactual hazard rates do not reflect a scenario in which the reform was entirely absent. Instead, they represent a situation where only the benefit period was shortened. This approach allows for isolating and evaluating the specific impact of the reduction in benefit levels.

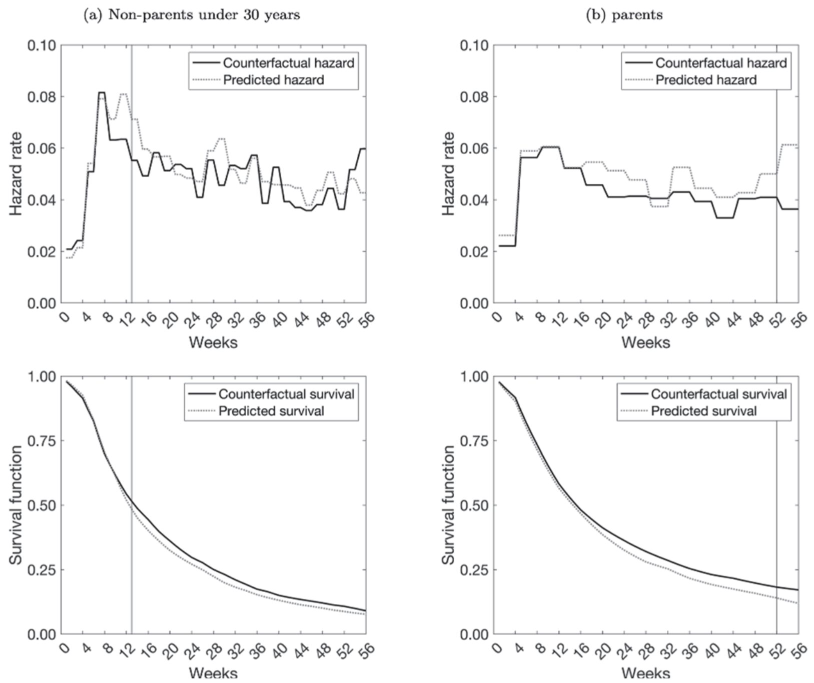
Similarly, in the analysis of the shortened benefit period the predicted hazard rate for parents after the reform is calculated as:

$$\bar{h}_{parent}^{post}(t) = \frac{1}{N_{parent}^{post}} \sum_{i=1}^{N_{parent}^{post}} \left[\hat{\alpha} + X'^{post}_{i}\hat{\beta} + c'_{\tau}\hat{\gamma} + \hat{\delta}(t) + \hat{\lambda}(t)\right] \tag{12}$$

Counterfactual hazard rates, representing a scenario where the benefit period was not shortened, are derived by subtraction of $\hat{\lambda}(t)$

Figure 7 illustrates these simulated hazard rates alongside their corresponding survival functions. The expected duration of unemployment is then estimated by integrating the area under the survival functions. These calculations are used to quantify the reform's impact on employment, with the results summarized in table 5.

Figure 7: Predicted and counterfactual hazard rates and survival functions



Note: Vertical lines indicate the timing of a benefit reduction or a benefit exhaustion.
Source: Own calculations based on DREAM and registers from Statistics Denmark.

The analysis reveals that the reduction in benefit levels shortened the expected duration of unemployment by 1.1 weeks, resulting in an estimated employment increase of 521 full-time equivalents (see column 1a in table 5). Similarly, the shortening of the benefit period reduced the expected unemployment duration for parents by 1.7 weeks. Assuming a similar response among non-parents, this corresponds to an additional employment increase of 1,030 full-time equivalents (column 2a in table 5).

Sensitivity analyses were conducted to test the robustness of these findings. In columns 1b and 2b, insignificant treatment effects are restricted to zero. This has minimal effects for the estimated effects of the reduction in benefit levels, while the estimated effects of the shortening of the PBD falls by 0.6 weeks. Furthermore, column 1c removes the unexplained positive treatment effect observed during weeks 29 and 30 of unemployment. Once again, it appears to have a small impact on the aggregate effect.

It is important to note that this preliminary impact analysis is subject to considerable uncertainty. This is particularly true for the evaluation of the shortened benefit period, as the reform is expected to have significant effects on hazard rates in the weeks following benefit exhaustion. For example, previous studies on the shortening of the general UI benefit period from four to two years have found positive effects on hazard rates lasting up to 16 weeks post-exhaustion (Hermansen 2014), and up to 26 weeks (Dagpengekommissionen 2015). This study is limited to 4 weeks after exhaustion due to data availability which introduces a downward bias in the estimated employment effect. This limitation is less pronounced for the analysis of reduced benefit levels, as no significant hazard effects are expected beyond 52 weeks of unemployment.

Table 5: Estimated employment effects of the 2023 graduate UI reform

	Reduction in level			Shortening of PBD	
	(1a)	(1b)	(1c)	(2a)	(2b)
Expected duration, weeks[a]	18.2	18.3	18.4	20.8	21.5
Counterfactual duration, weeks	19.4	19.4	19.4	22.6	22.6
Net effect, weeks	-1.1	-1.0	-1.0	-1.7	-1.1
Graduates affected annually[b]	24,348	24,348	24,348	30,832	30,832
Effect, FTE [c]	-521	-479	-459	-1,030	-648
Duration elasticity[d]	0.18	0.17	0.16	0.15	0.10

a) The predicted expected duration of unemployment for individuals enrolled after the reform. It is calculated based on survival functions censored at 56 weeks, and thus does not represent the actual duration of unemployment.

b) The number of non-parents below 30 years and the total number of graduates enrolled in 2023 respectively.

c) The full-time equivalent (FTE) effect is calculated as: $\frac{\text{Net effect}\times\text{Graduates affected}}{52.14}$

d) Duration elasticity wrt. level is given as: $\frac{\text{Net effect/Counterfactual duration}}{(9{,}700-14{,}106)/14{,}106}$.

Duration elasticity wrt. PBD is given as: $\frac{\text{Net effect/Counterfactual duration}}{(52-104)/104}$

Source: Own calculations based on DREAM and registers from Statistics Denmark

Following the announcement of the reform, it was projected by Dagpengemodellen that changes to the graduate UI system would result in total employment outflow effects equivalent to 6,330 full-time equivalents. This estimate includes 4,370 from the reduction in benefit levels and 3,840 from the shortening of the potential benefit period, with 1,880 deducted to account for interaction effects (see table 6). In contrast, this study finds a total employment effect of 1,551 FTE, which is significantly lower.

The projections by Dagpengemodellen are based on findings from the event study by Dagpengekommissionen (2015). However, the model has faced criticism for overstating the employment effects of changes to public benefits. A central criticism concerns the magnitude of the anticipatory effects, which appear unusually large given that they are observed up to 1.5 years before benefit exhaustion. A recent study by Arbejderbevægelsens Erhvervsråd (2024) revisits the 2010 reform and suggests that confounding factors during the period may have biased the original estimates. After accounting for these confounders, significant anticipatory effects emerge only 3-4 months before benefit exhaustion. Consequently, the employment effects identified by Arbejderbevægelsens Erhvervsråd (2024) are 40-70 % lower than those projected by Dagpengekommissionen (2015).

In that light, an estimated employment effect of 1,551 FTE does not appear unreasonably low.

Table 6: Estimated employment effects of the 2023 graduate UI reform

	Dagpengemodellen		Present study
	Inflow effects	Outflow effects	Outflow effects
Reduction in benefit level	540	4,370	521
Shortening of benefit period	290	3,840	1,030
Interaction effects[a]	-20	-1,880	-
Sum	810	6,330	1,551

a) The shortening of the benefit period and reduction in levels for non-parents interact with each other, such that the combined effect of the initiatives is smaller than the sum of the isolated effects of each individual proposal.

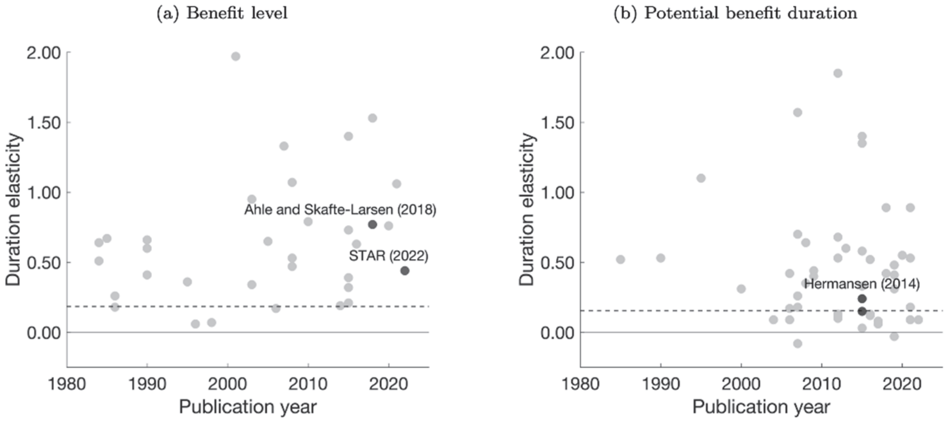
Source: BEU (2022) and own calculations based on DREAM and registers from Statistics Denmark.

7.1. Comparison with the literature

I estimate that a 1 % reduction in benefit levels corresponds to a 0.18 % decrease in unemployment duration, while a 1 % reduction in the potential benefit duration results in a 0.15 % decrease in unemployment duration. These estimates are relatively modest compared to findings from similar studies of UI reforms in other countries. A meta-analysis by Cohen and Ganong (2024) synthesizes results from 55 studies published between 1980 and 2022, converting them into duration elasticities to allow for consistent comparison. Figure 8 illustrates this extensive literature review, with the present study's results indicated by a dashed line.

Regarding benefit levels, the estimated elasticity is notably lower than those reported by Ahle and Skafte-Larsen (2018) and STAR (2022) (highlighted in dark gray), who examined the 2017 reform of the Danish graduate UI system and found elasticities of 0.77 and 0.44, respectively. A key factor that likely explains this discrepancy is the timing of benefit reductions. The 2023 reform lowered benefit levels only after 13 weeks of unemployment, whereas the 2017 reform introduced reductions at the onset of unemployment. As shown in figure 7, in a counterfactual scenario, approximately 50 % of the unemployed would have exited by week 13, with 40 % leaving by week 10, when anticipatory effects emerge. As a result, only 50-60 % of the unemployed population is directly affected by the benefit reduction, which in turn lowers the estimated elasticity, as it is calculated over the entire unemployed workforce.

Figure 8: Elasticity estimates of unemployment duration with respect to benefit level and potential benefit duration



Note: The dashed lines indicate the estimates reported in the present study. Dark gray dots mark studies from Denmark. In panel a these are Ahle and Skafte-Larsen (2018) and STAR (2022), while in panel b this is Hermansen (2014).

Source: Cohen and Ganong (2024), Ahle and Skafte-Larsen (2018), STAR (2022), Hermansen (2014) and own calculations.

The estimated duration elasticity with respect to PBD is more in line with Hermansen (2014), who analyzed the 2010 reduction in Denmark's general PBD from four to two years, reporting elasticities between 0.15 and 0.24. While this range is consistent with the present study's results, the estimated elasticity remains on the lower end compared to the broader literature. One possible explanation lies in data limitations, as the available data allow for observation of treated individuals only up to week 56 of unemployment—just four weeks beyond benefit exhaustion. Related Danish studies suggest that the effects of a shortened benefit period can persist for up to 26 weeks after exhaustion (Dagpengekommissionen 2015). This constraint likely leads to an underestimation of the full impact of the reform.

It is also important to recognize that the litterature review summarized in figure 8 draws upon a broad set of policy reforms implemented across highly diverse macroeconomic contexts – significant as the responsiveness of job search behaviour to changes in UI generosity is known to be state-dependent. As highlighted by Schmieder et al. (2012) moral hazard effects of UI benefits tend to diminish during recessions and intensify during economic booms. This stems from the cyclical variation in job-finding rates: when labor demand is strong, unemployed workers have more opportunities to benefit from intensified search, raising the relative gain of reduced benefit receipt. By contrast, in downturns, the marginal returns to search effort are limited, dampening the behavioral response to changes in UI generosity.

8. Unemployment Insurance and welfare implications

The empirical results of this paper reveal relatively modest behavioral responses to the 2023 reform of graduate UI. Estimated duration elasticities of 0.18 with respect to benefit levels and 0.15 with respect to benefit duration suggest that job-finding behavior reacts only moderately to changes in UI generosity. These findings should be interpreted in light of the broader theoretical literature on optimal unemployment insurance, which seeks to balance the incentive effects of UI against its welfare-enhancing roles in consumption smoothing and job matching.

8.1. Job matching and productivity considerations

The focus of this paper has been to quantify the moral hazard effects associated with UI: by reducing the benefit of job search while simultaneously increasing the value of unemployment, the availability of UI can lead to frictional unemployment, inefficient labor allocation, and overall output losses.

On the contrary, some studies challenge this view, arguing that UI can enhance productivity. Notably, Diamond (1981) highlights that UI allows workers to be more selective in their job search, leading to better job matches. In the long run, this improves productivity and reduces the likelihood of repeated unemployment spells. Building on this idea, Acemoglu and Shimer (1999, 2000) propose that UI not only influences worker behavior but also shapes firm incentives. They argue that UI encourages workers to seek higher-productivity jobs and incentivizes firms to create such jobs. Under the assumption of risk-averse agents, their findings suggest that the output-maximizing level of UI is not zero but moderately positive.

Empirical evidence of the relation between UI and job match quality has been found by e.g Nokoei and Weber (2017) and Tatsiramos (2014). In a Danish context, Draborg (2022) and Ebdrup (2022) examine the effects of the 2017 reduction in graduate UI benefits on job match quality. Their findings indicate that the benefit reduction led to lower subsequent earnings and a decreased likelihood of full-time employment, suggesting a productivity loss. These potential productivity costs should therefore be weighed carefully against the potential moral-hazard gains when policymakers consider to introduce UI cuts (or UI extension).

8.2. Balancing moral hazard and welfare protection

Beyond labor market efficiency, UI also serves a critical role in providing consumption smoothing for risk-averse workers. While, in theory, both public and private insurers could offer such coverage, private markets often fail to do so effectively due to asymmetric information, leading to moral hazard (Arrow 1963) and adverse selection (Akerlof 1970) distortions. These market failures justify public intervention in UI provision.

A well-developed theoretical literature examines the optimal level of UI, balancing the moral hazard effects against the welfare benefits of consumption smoothing. In a static principal-agent framework, Baily (1978) derives the optimal UI level by maximizing worker utility subject to search intensity choices. His model yields the well-known Baily-Chetty formula, which relates optimal UI to workers' risk aversion and the elasticity of unemployment duration with respect to benefits. A higher risk aversion implies greater welfare losses from consumption drops, favoring a higher benefit level. Conversely, a higher duration elasticity suggests that UI provision carries a greater cost, warranting a lower benefit level. The Baily-Chetty formula thus captures the trade-off between welfare gains and moral hazard costs.

Applying this logic to the modest elasticities estimated in this study suggests that the 2023 cuts may have reduced benefits more than what is welfare-optimal, especially for younger graduates with limited savings capacity and strong consumption-smoothing needs. In other words, the small behavioral responses estimated in this paper point to a low marginal efficiency cost of public insurance.

8.3. Benefit profiles and time-dependent incentives

A key policy feature of the 2023 reform was the introduction of a two-tier benefit profile for non-parents, with benefit reductions after three months of unemployment. Theoretical work by Shavell and Weiss (1979) characterizes the optimal benefit profile over time. In a dynamic setting, they show that if workers are liquidity-constrained, benefits should decrease monotonically throughout the unemployment spell. The intuition is that forward-looking agents anticipate future declines in benefit levels and, consequently, intensify job search efforts. This structure helps mitigate moral hazard issues.

Extending this framework, Hopenhayn and Nicolini (1997) introduce a wage tax upon reemployment, which varies with unemployment duration. Their results still favor a decreasing benefit profile, though of a lesser magnitude than in Shavell and Weiss (1979). In their calibration with US data, without a tax, the optimal replacement rate declines from 86 % in week 1 to 13 % in week 52. When incorporating a wage tax, the decline is much smaller, from 99 % to 94 %. The rationale is that a declining benefit structure incentivizes job search, while the wage tax mitigates consumption fluctuations upon reemployment.

The empirical results in this paper are broadly consistent with these models: exits increased sharply in the weeks leading up to the benefit reduction, suggesting that job seekers responded to time-dependent incentives. However, the overall elasticity was small, and the response was short-lived—indicating that, while the two-tier system effectively raised search intensity around the three-month mark, it did not fundamentally change average unemployment durations. From an optimal-policy perspective, a smoother and more continuous benefit decline might achieve comparable incentives with smaller welfare losses than the discrete reduction in the current profile. However, such a design would likely be more costly to implement and administer.

Empirical studies on the welfare effects of front-loaded UI reforms are surprisingly limited and have yielded mixed results. Lindner and Reizer (2020) analyze a 2005 Hungarian UI reform, which increased benefits by 50 % during the first 90 days of unemployment but reduced them by 25 % for the subsequent 180 days. They estimate a 6 % reduction in unemployment duration and a 1.4 % increase in reemployment wages. The authors argue that the reform was quasi-Pareto optimal, as the higher initial UI costs were offset by faster reemployment and higher tax revenues from increased productivity. Their results suggest that front-loaded UI structures can benefit both taxpayers and unemployed workers and provides empirical evidence in favor of a declining UI benefit profile.

By contrast, Kolsrud et al. (2018) find opposite results in their study of Sweden's UI system. They estimate that job search responses to benefit reductions are larger early in the unemployment spell than later, contrary to the predictions of forward-looking models. They suggest that long-term unemployed individuals may respond less to benefit reductions due to duration dependence. Additionally, they find that long-term unemployed workers experience greater drops in consumption expenditure than short-term unemployed workers, implying potential welfare gains from providing more generous benefits later in unemployment spells. These findings challenge the conventional idea that decreasing UI profiles are optimal and instead suggest potential advantages of an increasing benefit structure.

9. Conclusion

Unemployment insurance for recent graduates in Denmark underwent significant reductions in 2023, including a one-year cut in the PBD for all graduates and lower benefit levels for non-parents after three months of unemployment.

I employ a Difference-in-Differences strategy to analyze the effects of the benefit level reduction, assuming that parents and non-parents responded similarly to the PBD-shortening. I estimate a duration elasticity of 0.18 for non-parents under 30, with no significant effect for those over 30. The results indicate increased exit to employment around the three-month mark, coinciding with the benefit reduction. The findings generally appear robust to the exclusion of control variables and changes in the sample period, while placebo tests support the validity of the identification strategy.

To assess the impact of the shortened PBD, I employ a pre-post design, comparing unemployed parents before and after the reform. This yields a duration elasticity of 0.15 and indicates anticipatory effects up to 35 weeks before benefit exhaustion. Unlike the DiD strategy, the pre-post design does not control for time-related effects, and consequently the results are sensitive to changes in the sample period. Hence, the results should be interpreted with care.

The estimated employment impact is 1,550 full-time equivalents, significantly below the pre-reform projection of 6,330. This lower estimated impact may stem from data limitations and the fact that benefit reductions for non-parents take effect only after three months of unemployment. Conversely, Dagpengemodellen has faced criticism for overestimating the effects of UI changes, and in that view the results obtained are not unreasonably low.

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Appendix A:
The effect of higher UI benefits on search effort and reservation wage

The comparative statics analysis is performed in the steady state environment (see Le Barbanchon et al. (2024) for details). First, I examine how the reservation wage affects search effort by differentiating equation 6 with respect to the reservation wage, ϕ_S:[6]

$$
\begin{aligned}
c''(s_S^*)\frac{\partial s_S^*}{\partial \phi_S} &= \frac{\delta}{1-\delta}\int_{\phi_S}^{\infty}\frac{\partial}{\partial \phi_S}\big(u(w)-u(\phi_S)\big)dF(w) \Leftrightarrow \\
\frac{\partial s_S^*}{\partial \phi_S} &= \frac{1}{c''(s_S^*)}\frac{\delta}{1-\delta}\int_{\phi_S}^{\infty}\big(-u'(\phi_S)\big)dF(w) \\
&= \frac{-u'(\phi_S)}{c''(s_S^*)}\frac{\delta}{1-\delta}\int_{\phi_S}^{\infty}dF(w) \\
&= \underbrace{\frac{-u'(\phi_S)}{c''(s_S^*)}}_{-}\underbrace{\frac{\delta}{1-\delta}\big(1-F(\phi_S)\big)}_{+} < 0
\end{aligned}
\tag{A1}
$$

This result indicates that search effort decreases as the reservation wage increases.

Next, to analyze the effect of higher UI benefits on the reservation wage, take the total derivative of equation 7:[7]

$$
\begin{aligned}
u'(\phi_S)d\phi_S &= u'(b_S)db_S - \frac{\delta}{1-\delta}u'(\phi_S)\big(1-F-(\phi_S)\big)d\phi_S \Leftrightarrow \\
\frac{d\phi_S}{db_S} &= \frac{u'(b_S)}{u'(\phi_S)+\frac{\delta}{1-\delta}u'(\phi_S)u'(\phi_S)\big(1-F(\phi_S)\big)} > 0
\end{aligned}
\tag{A2}
$$

This demonstrates that the reservation wage increases with higher UI benefits. The reasoning is that an increase in benefits raises the utility of being unemployed, prompting the job seeker to demand higher compensation to leave unemployment.

The effect of UI benefits on search effort can be determined by combining the results of equations A1 and A2:

$$
\frac{\partial s_S^*}{\partial b_S} = \underbrace{\frac{\partial s_S^*}{\partial \phi_S}}_{-}\underbrace{\frac{\partial \phi_S}{\partial b_S}}_{+} < 0 \tag{A3}
$$

It follows that an increase in UI benefits leads to a reduction in search effort. This occurs because the marginal benefit of job search declines relative to its cost, as the increase in UI raises the utility of remaining unemployed.

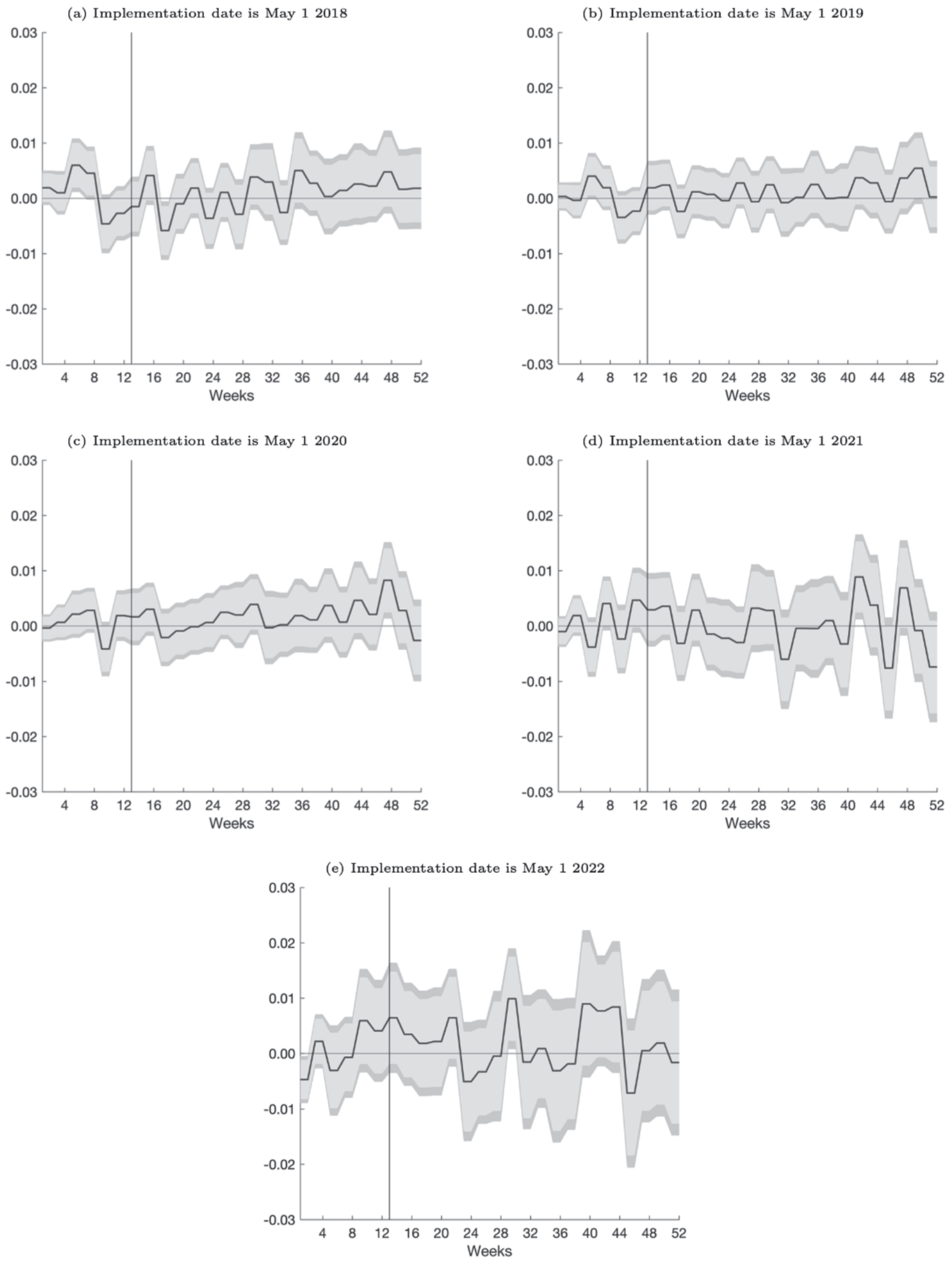
6. First, rewrite as $c'(s_S^*) = \frac{\delta}{1-\delta}\left(\int_{\phi_S}^{\infty}\big(u(w)-u(\phi_S)\big)dF_S(w)\right)$ and then take the derivative with respect to ϕ_S
7. Assuming $ds_S^* = 0$ as optimal search behaviour is determined by the first-order condition

Appendix B: Assumptions behind simulation of the Mortensen (1977) model

- Utility is specified as log utility, $u(x) = \log(x)$
- The search cost function is given by $c(s) = k\frac{s^{1+\gamma}}{1+\gamma}$, with parameters $k = 10$ and $\gamma = 5$.
- Wage offers follow a lognormal distribution, $f(w) \sim Lognormal(\mu, \sigma^2)$, with parameters $\mu = 8$ and $\sigma = 0.225$. This results in an expected weekly wage offer of approximately 6,150 DKK, with a stadard deviationof around 1,400 DKK.
- A discount factor of $\delta = 0.99$ is used.
- I simulate for two groups: parents and non-parents under both the pre- and post-reform UI regimes:[8]
  - Pre-reform regime (before May 1, 2023):
    - Parents: b_t = 3,733 DKK per week (16,177 DKK per month) for 104 weeks, followed by b_t = 1,500 DKK per week indefinitely.
    - Non-parents: b_t = 3,255 DKK per week (14,106 DKK per month) for 104 weeks, followed by b_t = 1,500 DKK per week indefinitely.
  - Post-reform regime (after May 1, 2023):
    - Parents: b_t = 3,733 DKK per week for 52 weeks, followed by b_t = 1,500 DKK per week indefinitely.
    - Non-parents: b_t = 3,255 DKK the first 13 weeks (3 months). From week 14 to 52, b_t = 2,828 DKK (12,253 DKK per month) for those over 30 years old and 2,238 (9,700 DKK per month) for those under 30 years old. After 52 weeks, b_t = 1,500 DKK.

8. See section 3 for an elaborate description of the reform of graduate UI in 2023

Appendix C:
Placebo tests, reduction in benefit levels for non-parents



Note: OLS estimates of $\xi(t)$ from placebo tests. Non-graduates under and over 30 years are pooled into a single treatment group. The vertical lines indicate the timing of the benefit reduction. Dark gray (light gray) area marks a 95 % (90 %) confidence interval.