Moral Hazard in Short-Term Disability: Benefit Cuts, Work, and Health

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PRELIMINARY AND INCOMPLETE: DO NOT CITE OR CIRCULATE

Abstract

In this paper we estimate the effect of reducing entitlements in short term disability on subsequent labor market and health outcomes. Using high quality Danish administrative data and a reform of the Danish short term disability system we document that exposing individuals to the threat of lower entitlements leads to a speedier exit from short term disability, increases the likelihood that they return to work, and makes it more likely that the individuals return to the same employer that they were sicklisted from. We find no evidence that the marginal returning worker is more sick or less productive. Neither do we find any effects on health care utilization on individuals exposed to the reform.

1 Introduction

In recent years, there has been a significant uptick in the number of employees availing themselves of sick leave or temporary disability benefits, imposing hefty costs on both corporate entities and public treasury. Central to this discussion is the moral

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hazard inherent in claims for benefits, as well as the wider effects of adjustments in replacement rates on both the frequency and duration of workplace absences. While moral hazard is an intrinsic element of any insurance mechanism, tweaking replacement rates to shorten or extend absence periods highlights critical efficiency dilemmas. The duration of an absence is crucial since an employee's human capital deteriorates during periods of sick leave. A prolonged absence can not only reduce a worker's efficiency upon their return but may also compel employers to end the employment relationship, thus disrupting a previously productive match and erasing job-specific human capital. On the flip side, incentivizing early return to work risks inadequate recovery and raises the chance of further absences. However, empirical data on these matters are notably scarce.

This paper studies whether introducing threats of lowering benefits to individuals on short term disability affects the duration of absence and hazard back to work. We utilize a major reform in the Danish sickness and temporary disability insurance system implemented in the summer of 2014. Employers receive compensation from the government when workers take-up temporary disability but with the reform, employers faced a lower compensation for workers on temporary disability spells that extended beyond 26 weeks. The drop ranged from 9% of compensation for workers with dependent children to 28% for workers without. As such, this policy change was in line with the suggestions from Autor and Duggan (2010) where it has been suggested to transfer some responsibility for monitoring and accommodating disabled workers from taxpayers to employers, incentivizing workplace adaptations to reduce transitions from employment to benefit schemes.

We study individuals entering short term disability from employment. We detect an increase in the hazard back to work of around 40% around the time of the reduction in compensation (weeks 26), indicative of substantial moral hazard in take-up. We find no evidence of ex ante threat effects of the reform: the reduction did not appear to affect the hazard back to work prior to week 26 of temporary disability spells nor does it impact who takes up STDI. We find no evidence that the marginal worker returning to work is less productive as the reform neither affects the sickness absence

nor the earnings conditional on returning to work.

Our findings contribute to several strands of literature. First we contribute to a literature on incentives in short term disability. The literature on the importance of economic incentives, duration of entitlements, and training policies for the unemployed is extensive going back decades; e.g. Heckman et al. (1999). Yet much less is known about similar issues concerning sick-listed workers. Using American data Cabral and Dillender (2023) documents how increased benefit generosity leads to longer income benefit durations and increased medical spending. European empirical studies that do exist are mainly Swedish, Norwegian, or German. Johansson and Palme (2005) study a Swedish reform from 1991 where the replacement rate while on sick leave was changed from a permanent level to a duration dependent system. The reform resulted in fewer workers sick-listing, a quicker return early on in the sickness spell, but a substantial drop in the transition rate for those sickness spells with a longer duration. The latter results are interpreted as a moral hazard response to the higher costs of reporting back fit for duty after a sickness spell and illustrates the complexity in the behavioral responses to reforms of the type studied in this paper. Henrekson and Persson (2004) find similarly large and persistent responses to changes in the replacement rate of the sickness benefits on sickness absence based on time series analysis of data spanning 1955 to 1999 in Sweden.

Furthermore, our study contributes by investigating the health implications of the reform. Recent work by Ahammer and Packham (2023) investigates the impact of unemployment duration on physical and mental health. The increased incentive for workers to expedite their return to work carries the inherent risk of inadequate recovery time, potentially leading to premature re-entry into the labor market. Such a scenario could result in increased instances of sickness relapse, suggesting a need for careful evaluation of the balance between economic incentives and health outcomes. While we find substantial impacts of the reform on the probability of returning to work, we find no difference between the treatment and comparison groups in earnings or sickness absence for those returning to work. We do not find any impacts of the reform on health care utilization.

In addition, future versions of this paper will also investigate heterogenous responses and the role of the firm.

This paper is organized in the following way. Section 2 describes the institutional setting and the 2014 reform in the Danish sick leave system. Section 3 presents the data and descriptive evidence while section 4 presents the econometric evaluation framework. Section 5 holds the results of our study.

2 Short term disability and the 2014 reform

2.1 The Danish STDI system

Three major systems of temporary income support exist in Denmark: A voluntary unemployment insurance system covering around 70% of the labor force, a means-tested welfare system, and a short term disability benefit system. The latter is universal and covers both wage earners, self-employed, and workers on UI. Except for 2007 and 2008 – at the height of the last business cycle – the short term disability benefit program is the smallest of the three.

Sick-listed workers receive compensation equivalent to that in the UI benefit system. This benefit depends on prior earnings with a cap at €1,600 per month. The average compensation level among sick-listed workers prior to the reform was €1,460. A time limit was in place prior to the reform such that injured or sick workers could receive the benefit for 12 months within an 18 months period. Only workers satisfying certain requirements were eligible to continue receiving sickness benefits beyond this period. In the years leading up to the reform, around two in three of the workers who reached the 12 months maximum would satisfy one of the requirements and continue receiving the sickness benefits. Non extended workers would end up without either an earned income or qualify for either UI benefits or welfare.

The employer is responsible for paying out the benefits for all employees with more than eight weeks of tenure and at least 74 hours of work at the employer. The government (i.e. the tax-payer) will cover benefit expenses for the employer after the

first four weeks of the sickness spell. Very small employers with a total yearly payroll below €967,414 (2015 limit) can insure themselves against the risk of having to cover the first four weeks by buying an insurance from the government. All public sector employers are covered after the first day, however.

To minimize the moral hazard risk of unemployed workers calling in sick just to avoid having to participate in active labor market programs, sick-listed workers are required to participate in job counseling, educational training programs and similar measures in much the same way as unemployed workers.

2.2 The 2014 reform

In December 2013 a political agreement was reached between the major political parties in Denmark to change the sickness benefit system. The reform would take effect the following summer by July 1 2014 and essentially traded off two financial incentives. On the one hand, the reformed aimed at making sure that no sick-listed worker should find himself without any income support after the 12 month of sickness benefit recipiency. This was accomplished by removing the means testing of the lower welfare payments. In effect, all sick-listed workers having exhausting the 12 months cap and no longer satisfying one of the requirements to continue on sickness benefits, would after July 1 2014 qualify for cash benefits irrespective of personal assets or spousal income. The amount of cash benefits were independent of previous earnings and equal to €1,048 per month for workers without dependents and €1,330 for workers with dependents. On the other hand, the reform aimed at lowering moral hazard by moving the maximum number of months workers were eligible for sickness benefits forward from 12 to five months. This meant that any sick-listed worker with less than five months on sickness benefits by July 1 2014 and who was not deemed eligible for continued recipiency would be moved to the lower cash benefits. For the average sick-listed worker without dependents this amounted to a 28% drop in benefit level and for a worker with dependents, the drop was 9%.

The timing of the events surrounding the reform – the six months lag between the agreement and implementation – and the way the reform was implemented – affecting

both the inflow and stock of sick-listed workers in July 2014 when the changes took effect – have important implications for our ability to evaluate the reform. The early announcement and extensive media coverage of the reform made sure that no sick-listed worker was caught off guard by July 2014. By having the two changes apply to both the stock and inflow of sickness benefit recipients by July 2014, some sick-listed workers ended up being affected only by the increased generosity of the system. This was the case for all workers with more than five months of tenure in the system when the reform took place. They could all continue under the old rules until they met the 12 months cap but by then no one would faced the risk of seeing their benefits removed altogether as they would in the pre-reform regime. Worst-case scenario, they would receive the lower cash benefits if they did not live up to any of the requirements for extension. Anyone with five months or less of tenure in the system by July 2014, would have to meet at least one of the requirements by week 22 of their sickness spell to be continue on sickness benefits. Failure to meet one of the requirements would result in a reduction in benefits.

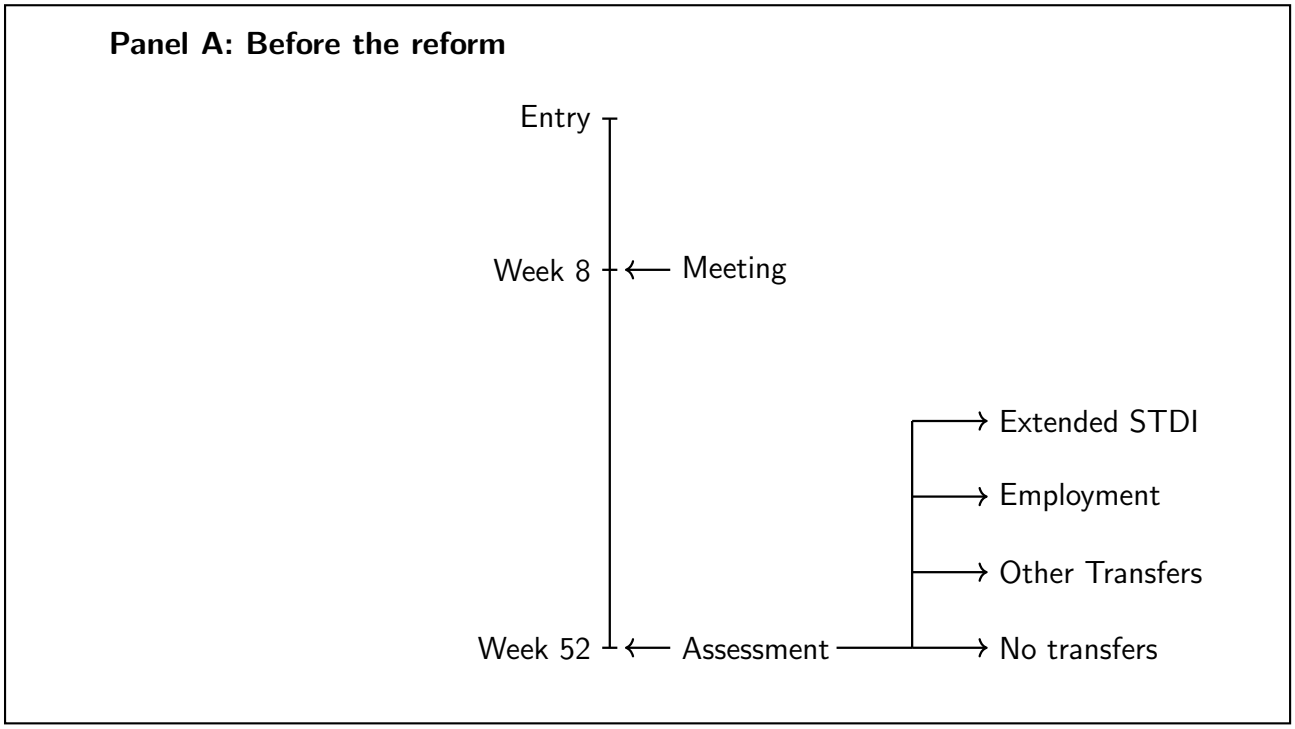
Figure 1 sums up the reform of the Danish sickness benefit system that took effect by July 1 2014. In summary, the reform removed the existing 12-month time limit but did so by lowering the benefit level by an average of nine or 28% depending on family status for less deserving sick-listed workers after five months. Anyone sick-listed for more than five months when the legislative change took effect would see the time limit disappear but would only see the risk of having his benefit reduced after 12 months.

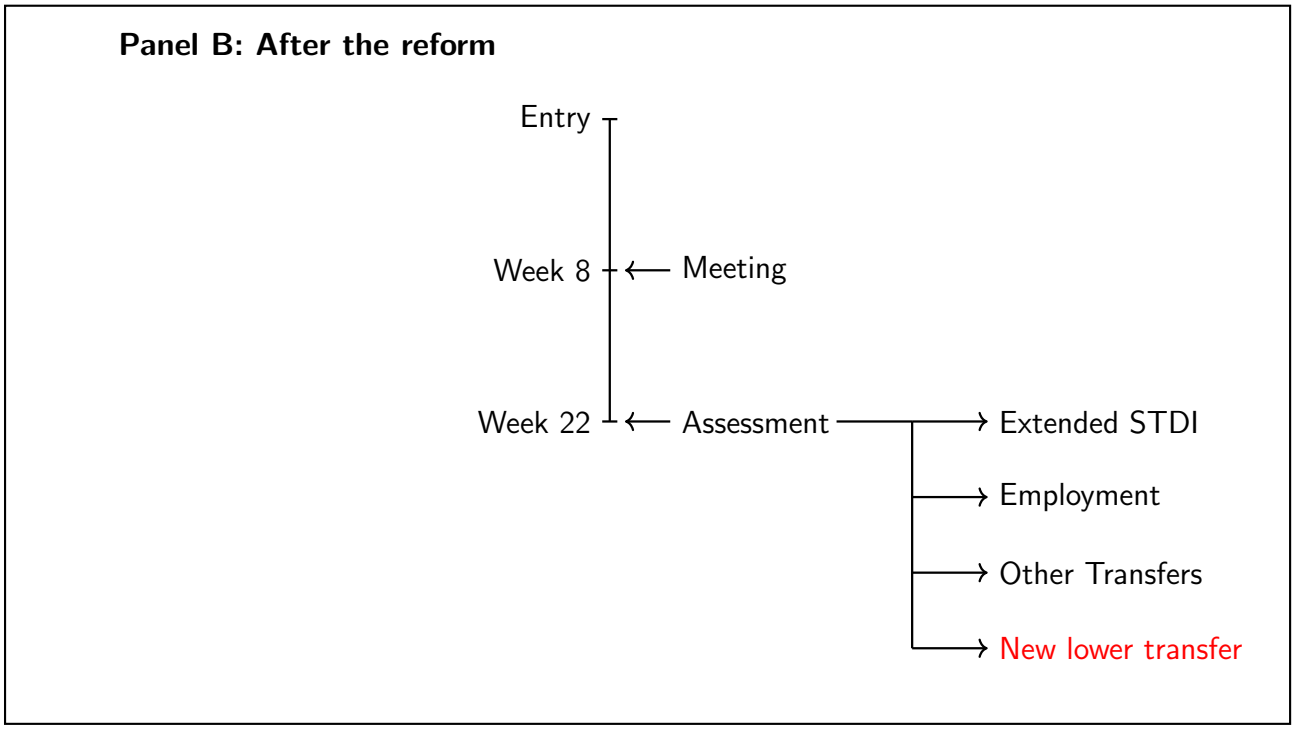
3 Empirical Strategy

3.1 The effect of the reform on the exit from STDI and return to work

To estimate the impact of the reform we estimate hazards and a combined effect. We start by estimating hazards for exiting STDI (θ_1), returning to work(θ_2) and

Figure 1: Illustration of the reform





returning to the same employer as the individual was with when he/she entered STDI (θ_3). Let $w_i > 0$ denote the week individual i exits STDI and let $T_i \in \{0, 1\}$ denote whether the individual enter STDI under the new ($T_i = 1$) or the old ($T_i = 0$) regime.

Hazard for exiting STDI:

$$\theta_i^1(t) = P(w_i = t | w_i \geq t)$$

Hazard for exiting STDI and returning to work:

$$\theta_i^2(t) = P(w_i = t | w_i \geq t, empl_i = 1)$$

Hazard for exiting STDI and returning to work for the same employer:

$$\theta_i^3(t) = P(w_i = t | w_i \geq, empl_i = 1, E_0 = E_1)$$

where $empl_i$ indicates that individual i is employed in the month following return from STDI, and E_0 and E_1 indicate the employer individual i was employed at the month before entering STDI (E_0) and the month after returning from STDI (E_1). We estimate the hazard using OLS:

$$P(w_i = t) = \alpha_t + \gamma_t T_i + \varepsilon_i \text{ for } w_i \geq t \tag{1}$$

Then the expected hazard for the treament group is

$$E[\theta^1(t)] = \alpha_t + \gamma_t$$

and the hazard in period t for the comparison group is

$$\theta^1(t) = \alpha_t$$

To estimate a combined impact of the reform we estimate impacts on exiting before (and including) week 30 conditional on not exiting before week 20:

$$Y_i = P(w_i <= 30 | week_i \geq 20),$$

that is Y_i denotes e.g. whether individual i exits employment before (or including) week 30 conditional on not having left STDI before week 20). In practice we estimate the following linear model by OLS:

$$Y_i = \beta_0 + \beta_1 T_i + X_i \beta_2 + \varepsilon_i \tag{2}$$

We estimate the model with and without individual level covariates X_i. While the individuals who do not exit STDI before week 20 is fundamentally different from those that do, we verify that there are no differences in individual characteristics and pre-entry health care utilization for the treated and comparison conditional on not exiting before week 20.

3.2 The effect of the reform on productivity for the returning workers

A natural concern would be that the increased incentive to return to work would leave the marginal returnee in poorer health post-reform than pre-reform. This would manifest itself in lower productivity and increased sickness absence. To check for such productivity losses we focus on the individuals returning to work before week 30 conditional on remaining on short term disability after 20 weeks. We investigate whether individuals in the treatment group have more absence, A_i by estimating the following equation by OLS:

$$A_i = \beta_0 + \beta_1 T_i + X_i \beta_2 + \varepsilon_i \tag{3}$$

only for individuals how return to work before week 30 cond. on surviving until

week 20 in STDI and appear in our absence data[1]. As earlier T_i indicates belonging to the treatment group and X_i are individual characteristics.

To investigate whether they there is an earnings impact we estimate equation 3 with monthly earnings in the first full month of employment after return as outcome. In addition to the individual controls we also control for earnings prior to entering sickness absence.

3.3 The effect of the reform on health care utilization

To investigate the impact on healthcare utilization we estimate linear regressions using OLS to measure the difference in hospital admissions between the treatment and comparison groups. Let H_{it} be an indicator for having an hospital admission in the t months since entering short term disability, and H_{i0} be an indicator of having an hospital admission in the period $\pm$ 3 months around the time of entry into sickness absence.

$$H_{it} = \beta_0 + \beta_1 T_i + X_i \beta_2 + H_{i0} + \varepsilon_{it} \tag{4}$$

4 Data and Descriptives

Our analysis includes the universe of Danish workers sick-listed between January 2013 and June 2015. For this population, we have their earnings and employment histories on a daily basis, exact information about type and timing of all income transfers from the government, and information on all programs and job counseling interviews the sick-listed might have participated in. With this information, we are able to follow the population of sick-listed workers through their spell of sickness and study the transition rate back into their old jobs or possibly new ones, as well as the transition rates into other income support programs.

We limit our analyses below to all spells of sickness initiated from Januar 1st 2013

[1]We find no difference btw comparison and control in the probability of appearing in the absence data. For details on the absence data see section 4

to December 31st 2014. We include only workers without any sickness spells longer than four weeks in the 18 months prior to initiation. This ensures that all spells will be fresh in the sense that the tenure clock is zero at onset of the spell under study. We also limit ourselves in the subsequent sections to study sickness spells with duration of at least four weeks. This latter limitation is imposed since larger private sector employers do not receive a government refund covering the first four weeks; see section 2 above. We conjecture that this limitation is without importance as the risk of receiving the lower benefits only materializes after week 26 and we find no evidence of any ex ante response to this risk.

We end up with a total of 189,356 sickness-spells. 83,502 of these spells are initiated after January 2014 and will therefore potentially be exposed to the risk of the lower benefit level should the spell extend beyond July 1st of that year. The remaining 105,854 spells will all potentially have crossed 26 weeks of duration by July 1 2014 and will only be exposed to the removal of the time limit of 52 weeks: they will qualify for the more lenient system.

Table 1 shows descriptive statistics for the full population of our selected sickness spells as well as a distinction made between those covered by both the of the reform effects and those only covered be the latter. Column 5 reports the differnces and column 6 reports the standardized mean differnce. Across all characteristics these are small - e.g. all SMDs (with the exception 1 education category) are below 5%. Table 2 present the descriptive statistics for our sample conditional on not exiting STDI before week 20. Here we also do not find any meaningful differences.

In terms of outcomes we measured whether an individual exits the short term disability system, whether the individual exits the STDI system to any employment and whether the individual returns to the same employer as the individual was sicklisted from. The two latter outcomes are measured by 1) whether the individual have any labor market income in the month after exiting STDI, and 2) whether this income is paid by the same employer as before the sicklisting. We also use hospitalizations in the months 3-6,3-12,3-18 and 3-24 as outcomes in our analysis. We do separate analysis for various types of hospitalizations.

Table 1: Descriptives for the Treated and the Comparisons

	Treatment		Comparison		Difference	
	Mean	SD	Mean	SD	Difference	SMD
Age -25	0.16	0.37	0.15	0.36	0.01	0.02
Age 25-35	0.18	0.39	0.18	0.38	0.00	0.01
Age 35 - 45	0.23	0.42	0.23	0.42	-0.00	-0.01
Age 45 - 55	0.25	0.43	0.26	0.44	-0.01	-0.01
Age 55 -	0.18	0.38	0.18	0.38	-0.00	-0.01
Female	0.56	0.50	0.55	0.50	0.00	0.01
Immigrant	0.08	0.28	0.08	0.27	0.01	0.02
Descendent	0.01	0.12	0.01	0.11	0.00	0.02
Edu Pri	0.22	0.41	0.23	0.42	-0.01	-0.02
Edu Sec	0.09	0.29	0.08	0.28	0.01	0.03
Edu Voc	0.34	0.47	0.36	0.48	-0.02	-0.04
Edu Short	0.22	0.41	0.21	0.41	0.01	0.03
Edu Long _ter	0.06	0.24	0.06	0.23	0.01	0.03
Married	0.44	0.50	0.46	0.50	-0.02	-0.03
Pre Earnings	27474.81	15918.59	26981.68	16062.13	493.13	0.03
Observations	92428		94828			

Notes: Descriptives for all entrants

We also include information on sickness absence. This data contains information on all sickness absence spells for public employees as well as all private firms with more than 100 employees. Private firms with number of employees between 10-100 are surveyed and surveyweights are available to us.

5 Results

In this section we present our results. We will present the effects of the reform on exit from STDI, the return to employment and the return to the same employer. We begin by presenting hazards out of STDI, exit out of STDI into employment and exit out of STDI into employment with the same employer the individual was sicklisted with. In addition we also document the impact of the reform on hospitalization utilization.

Table 2: Descriptives for the Treated and the Comparisons

	Treatment		Comparison			
	Mean	SD	Mean	SD	Difference	SMD
Age -25	0.05	0.22	0.05	0.23	-0.00	-0.02
Age 25-35	0.13	0.34	0.13	0.34	0.00	0.01
Age 35-45	0.26	0.44	0.25	0.44	0.01	0.02
Age 45-55	0.31	0.46	0.32	0.47	-0.01	-0.02
Age 55-	0.24	0.43	0.24	0.43	0.00	0.00
Female	0.60	0.49	0.60	0.49	0.00	0.00
Immigrant	0.10	0.29	0.09	0.29	0.01	0.02
Descendent	0.01	0.09	0.01	0.09	0.00	0.00
Edu Pri	0.18	0.38	0.20	0.40	-0.02	-0.06
Edu Sec	0.06	0.23	0.05	0.22	0.01	0.03
Edu Voc	0.35	0.48	0.36	0.48	-0.01	-0.02
Edu Short	0.26	0.44	0.24	0.43	0.02	0.04
Edu Long	0.08	0.27	0.06	0.25	0.01	0.05
Married	0.54	0.50	0.54	0.50	-0.00	-0.01
Earnings	28971.42	14982.22	28205.20	17344.57	766.22	0.05
Observations	16930		17358			

Notes: Descriptives for entrants conditional on not exiting before week 20

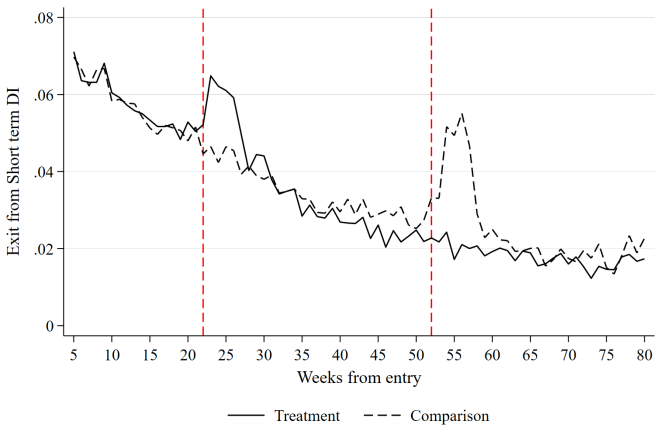
5.1 Exit from STDI and Return to employment

5.1.1 Hazards from Short Term Disability Insurance

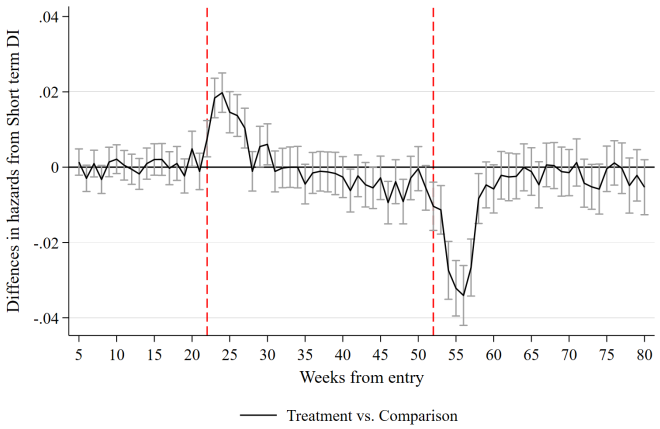
In figure 2 we present hazards out of STDI. In panel 2a we report the hazard for exiting short term disability insurance for each week of the sickness spell duration. It is clear that the treatment group behaves very similar to the comparison group leading up to week 22. During the weeks 22-30 the hazard is elevated for the treated group, but seemingly unchanged for the comparison group. After 52 weeks in STDI the comparison group are faced with the prospect of receiving either UI benefits or social assistance. This lead to a large hazard out of short term DI. in panel 2b and panel 2c we respectively present differences in hazards and relative differences in hazards between treated and comparison individuals along with 95% confidence bands. Consistent with panel 2a we see a jump immediately after week 22 of around 2 percent, or almost 50% relative to the hazard of the comparison group.

Figure 2: Transitions out of Short Term Disability Insurance

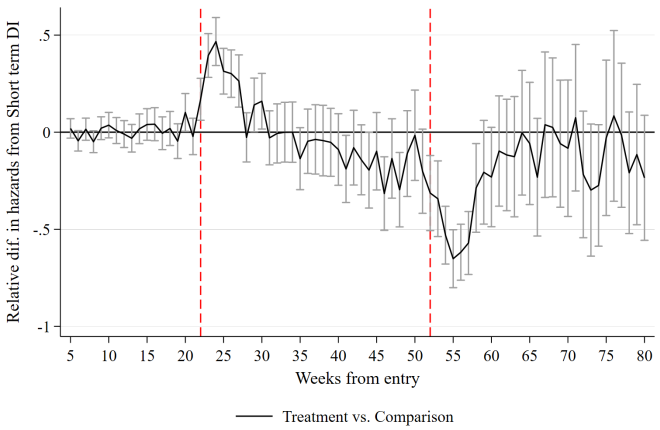
(a) Exit from Short Term Disability Insurance



(b) Exit from Short Term Disability Insurance



(c) Exit from Short Term Disability Insurance

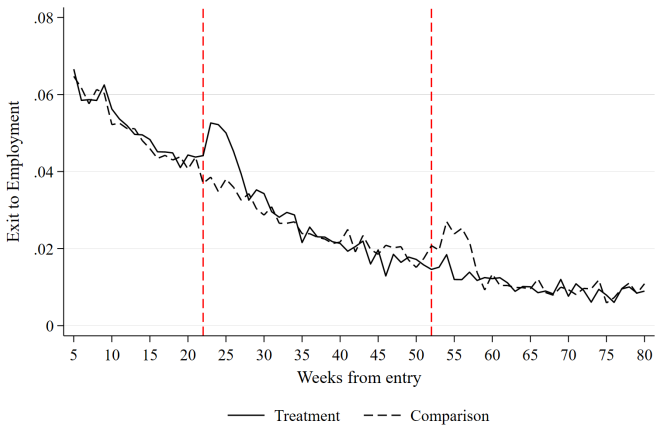


The transition into employment mimics the transition out of STDI very closely.

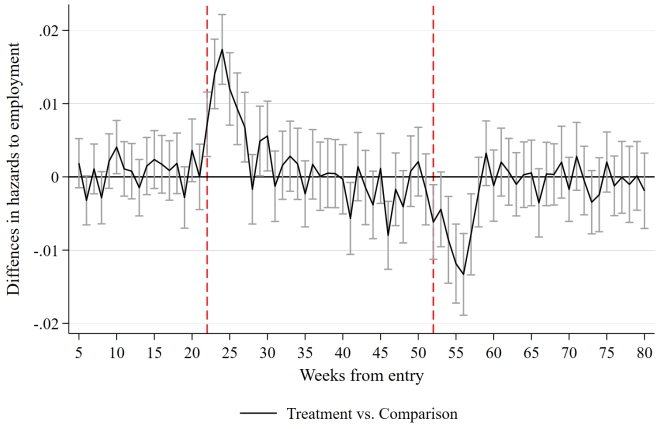
In figure 3 we present hazards out of STDI into employment. In panel 3a we report the hazard for exiting short term disability insurance into employment for each week of the sickness spell duration. It is clear that the treatment group behaves very similar to the comparison group leading up to week 22. Immediately after week 22 the the hazard is elevated for the treated group, but seemingly unchanged for the comparison group. After 52 weeks in STDI the comparison group are faced with the prospect of receiving either UI benefits or social assistance. This lead to a large hazard of transitioning into employment. In panel 2b and panel 2c we respectively present differences in hazards and relative differences in hazards between treated and comparison individuals along with 95% confidence bands. The increase in transistions into employment relative to the comparison group seems to fade out sooner than was the case for exits out of STDI in figure 2. Consistent with panel 3a we see a jump immediately after week 22 of a little less than 2 percent, or almost 50% relative to the hazard of the comparison group.

Figure 3: Transitions from Short Term Disability Insurance into employment

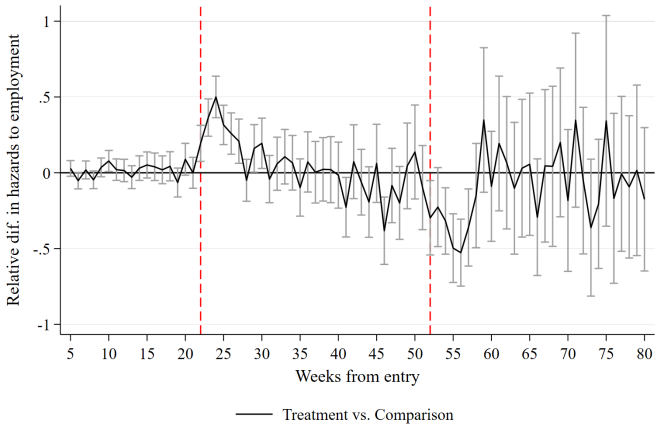
(a) Hazard for transition into employment



(b) Difference Exit to employment



(c) Relative impact in exit to employment



For hazards into employment with the same employer, our results are very similar

albeit a little more muted as expected. These can be found in appendix table A1.

To estimate the combined effect of the reform on exits out of STDI, and transitions into employment we estimate equation 2 by OLS. That is, we estimate the difference in probability of exiting STDI by week 30 conditional on not exiting before week 20. We present our results in table 3, where all standard errors are clustered at the municipality level. For each outcome we estimate three models one without controls, one adding municipality fixed effects and one further adding individual controls. The results for exit from STDI is presented in columns (1) - (3). With the full set of controls we find that the probability of exiting STDI before week 30 conditional on not exiting before week 20 is 5.7 pp. higher for the treated than the comparisons. This is app. 15% of the comparison mean. The probability of exiting to employment is 4.4% higher for the treated than the comparison or 14% relative to the mean outcome in the comparison group. The probability of exiting STDI to the same employers as one was sicklisted from increases for the treated compared to the comparison individuals. The probability of exiting STDI to your former employer before week 30 conditional on not exiting before week 20 increased by 4.2 percentage points which is app. 15% relative to the outcome mean of the comparison.

Table 3: Effect of the reform on Exit from STDI, Exit to employment and Exit to the same employer

	Exit STDI			Exit to employment			Exit to same employer		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment	0.060***	0.060***	0.057***	0.048***	0.048***	0.044***	0.047***	0.047***	0.042***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Constant	0.390***	0.390***	0.398***	0.321***	0.321***	0.305***	0.286***	0.286***	0.280***
	(0.005)	(0.003)	(0.019)	(0.004)	(0.003)	(0.016)	(0.004)	(0.003)	(0.015)
Observations		33,965			33,965			33,965	
R-squared	0.004	0.010	0.017	0.003	0.007	0.031	0.003	0.007	0.039
Municipal FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Ind. Characteristics	No	No	Yes	No	No	Yes	No	No	Yes
Outcome Mean Comparison		0.390			0.321			0.286	

Robust standard errors in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes:

Table 4: Effect of the reform on sickness absence and earnings upon return

	(1)	(2)	(3)	(4)	(5)	(6)
	Fraction absent		Any sickness absence		Earnings (DKK)	
Treat	-0.014	-0.005	-0.026**	-0.022*	741.0*	778.4*
	(0.013)	(0.009)	(0.012)	(0.012)	(408.7)	(437.5)
Observations	6611	6366	6611	6366	11778	10571
Outcome Mean Comparison	0.098	0.090	0.572	0.575	32987.1	33990.2
Robust standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$						

Notes:

5.2 Impact on earnings and sickness absence conditional on returning to employment

Here we present evidence that the marginal individual returning to work is not less productive measured by sickness absence and earnings. We estimate 3 using OLS and present the results are presented in table 4[2]. columns (1), (3) and (5) the sample is those that return to work irrespectively of which employer they return to, and in columns (2), (4) and (6) we study those that return to the same employer as they were sicklisted from. For sickness absence we find no effect on the fraction of time spent on sickness absent and small and precisely estimated impact of the reform on having any sickness absence. Importantly though, the estimated impacts are all negative, which implies less sickness absence upon return.

In terms of earnings conditional on returning to work, we find no evidence of a negative impact - on the contrary we find a small positive and marginally significant impact of the reform. Together this implies that even though the reform expedited the return to employment for treated individuals there was no negative impact on their productivity.

[2]As information on sickness absence is not available for all individuals we start off by checking whether the reform had any effect on being in the sample. We find that there is no difference in the probability of being present in the absence data

5.3 Impact on Health care utilization

In this subsection we present our results for health care utilization. In figure A2 we focus on hospitalization admissions. In panel A2a we plot the probability of having any hospital admission around the entry into take up of STDI for the treatment and comparison groups respectively. We observe individiuals 18 month prior to the take up, and follow them for two year after entry. The lines are virtually indistinguishable from each other, which indicate that in terms of hospital admissions, the reform did not lead to any differences among those who enter STDI. Neither do we find any indications of differences after entry. Panel A2b zooms in on the $\pm$ 6 months around the entry into STDI. Panel A2c present the probability of having any hospital admission around the entry into take up of STDI for the treatment and comparison groups respectively, but also by whether or not the duration of the STDI spell is above or below 20 weeks. While it is evident that these groups (above/below) are different from each other we confirm that conditional on having a during above 20 weeks, there are no differences between the treated and the comparisons. We interpret the similar pretrends as supporting evidence of the validity of our analysis, and the apparent lack of differences post entry to indicate no effect of the reform on hospital admissions.

To estimate the impact of the reform on health care utilization we estimate equation 4 by OLS for individuals remaining in STDI after 20 weeks. The results are presented in table 5. The outcome is any hospitalization admission, and we investigate two time horizons: 12 and 24 months. Regardless of whether we focus on the difference between the treated and comparison after 12 months or 24 months after entry, we do not find any economical nor statistically significant effects. Future versions of this paper will delve deeper into the health impacts of the reform. All in all our findings on the impact of hospital admissions implies that treated individuals did not differ from the comparison group before entering STDI, nor did they have more hospital admissions throughout the first two years after the entry.

Table 5: Impact of the reform on health care utilization

	Any Hospitalization			
	12 months	12 months	24 months	24 months
Treatment	0.004	-0.003	-0.006*	-0.005
	(0.003)	(0.004)	(0.003)	(0.003)
Individual Controls	Yes	Yes	Yes	Yes
Pre reform admissions	No	Yes	No	Yes
Observations	34,055		34,055	
Outcome mean comparison	0.77		0.87	

6 Conclusion

Our analysis provides evidence on the labor market dynamics and health outcomes following a cutback in short-term disability benefits. Utilizing comprehensive Danish administrative data, coupled with the legislative reform of the disability system in Denmark, we have established that reduced entitlements serve as an incentive for quicker transitions out of short-term disability. This effect is not only observed in the accelerated exit from disability status but also manifests in the increased probability of individuals rejoining the workforce. We find that the likelihood of returning to their previous employer is heightened. Despite the potential concerns associated with curtailing benefits, our findings do not indicate any detrimental impact on productivity for the returning workers, nor do we see any increased health care utilization.

Looking ahead, we aim to extend our analysis into the interplay between firm behavior and the observed labor market outcomes of sicklisted workers. The firm's role, especially heterogeneity in whether the firm receives the payment, is a critical aspect that we will devote more attention to in the future. Moreover, we plan to undertake a welfare analysis using sufficient statistics to provide a nuanced understanding of the welfare implications of the reform.

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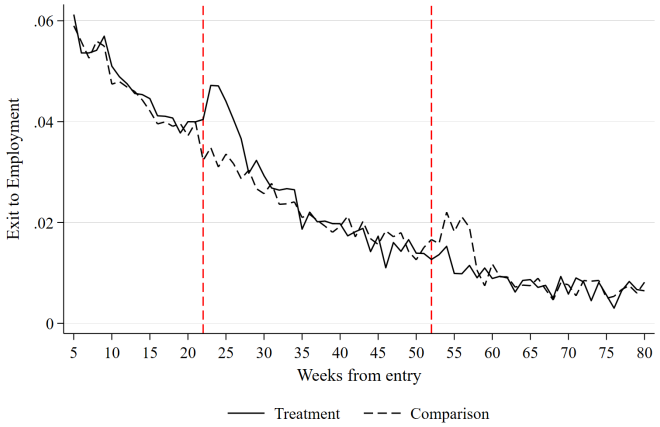
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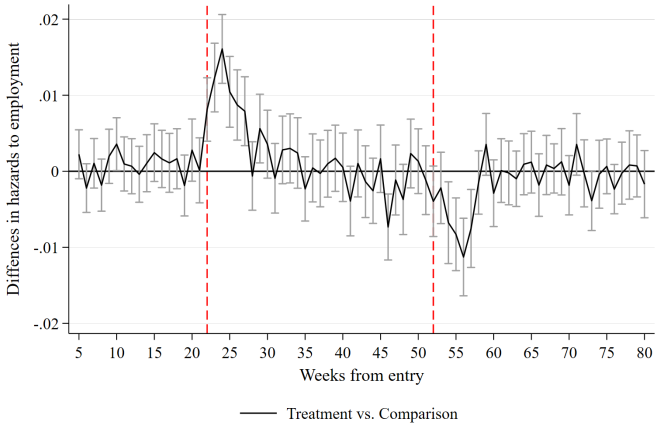
A Additional Material

Figure A1: Transitions into employment with previous employer

(a) Difference in Exit from Short Term Disability Insurance



(b) Difference Exit to employment



(c) Exit to same employer

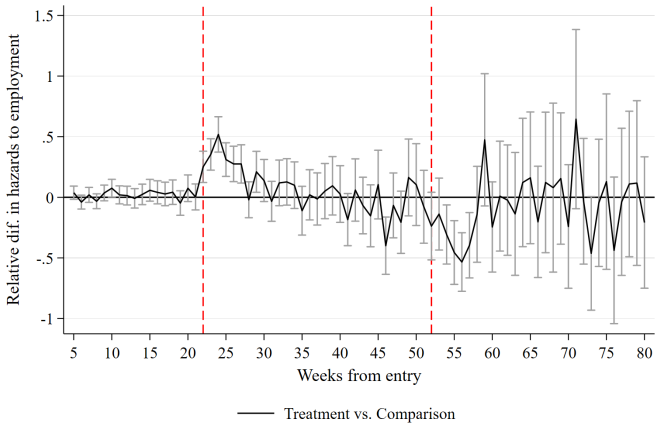
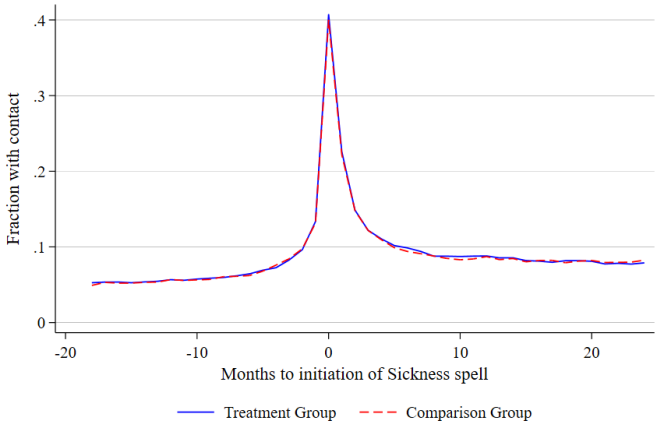
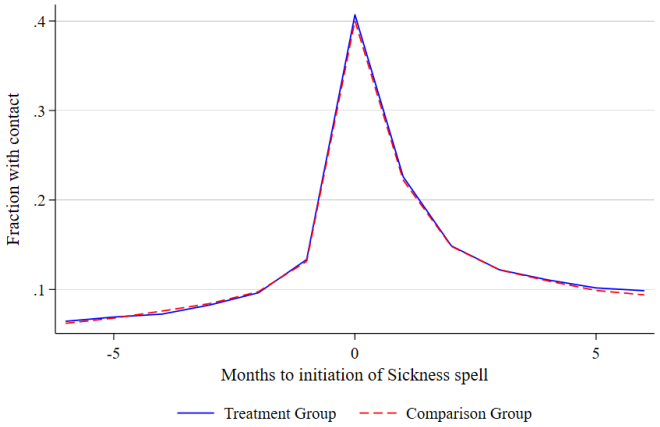


Figure A2: Probability of hospital admission

(a) Any admission by treatment status



(b) Any admission treatment and comparison group ± 6 months



(c) Any admission by treatment status and STDI duration spell

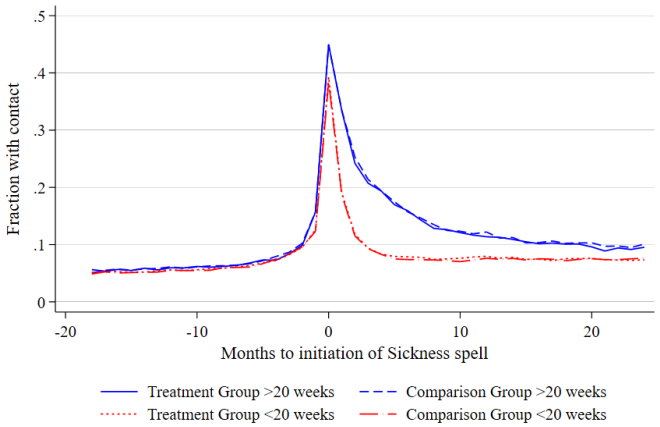
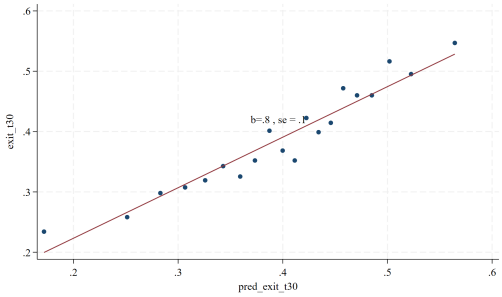


Figure A3: Exit before week 30

(a) Realized vs. Predicted Exit



(b) Distribution of Predicted Exit

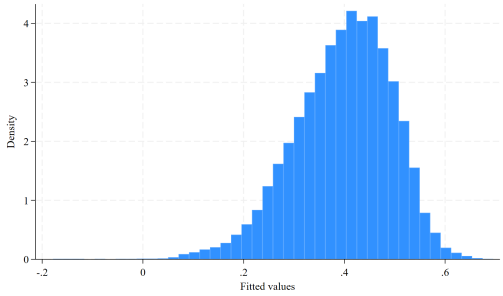
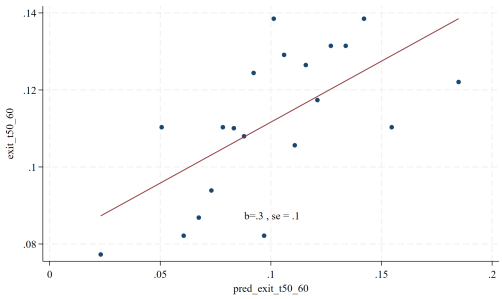


Figure A4: Exit between weeks 50 and 60

(a) Realized vs. Predicted Exit



(b) Distribution of Predicted Exit

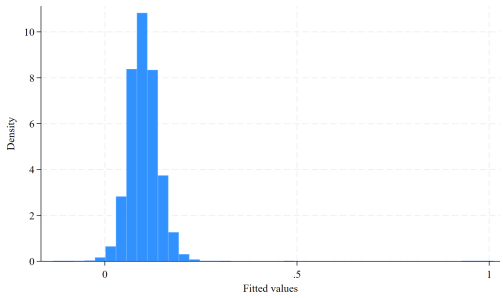
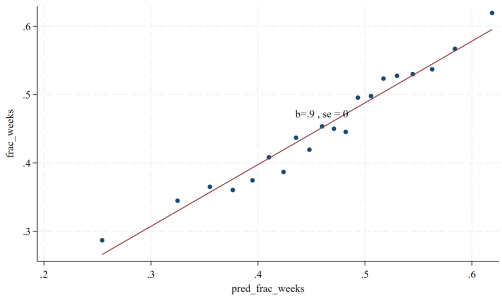


Figure A5: Exit before week 30

(a) Realized vs. Predicted Exit



(b) Distribution of Predicted Exit

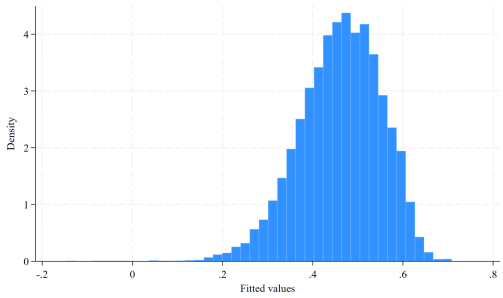
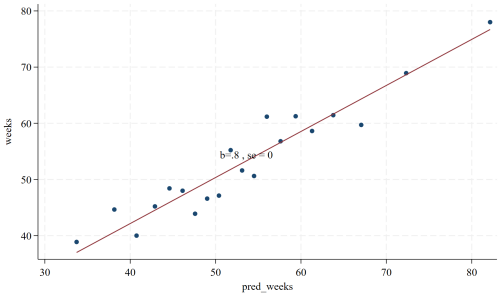


Figure A6: Exit before week 30

(a) Realized vs. Predicted Exit



(b) Distribution of Predicted Exit

