The Scarring Effect of Short-Lived Sickness Spells for Temporary Workers

Jonathan Rossi, Pierre Koning, Paul Muller, Carlos Riumallo Herl*

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Abstract

Existing research documents wage scarring after job loss, but little is known about the long-term effects of sickness leave. We study how sickness leave duration affects subsequent labor market outcomes for temporary workers in the Netherlands. Using administrative population-wide data on temporary workers who called in sick between 2010 and 2015, we first estimate descriptive event studies matching sick workers to observationally equivalent controls. We find that even short sickness spells are persistently associated with lower employment, wages, hours and permanent contract probability. Because the duration of sick leave is mechanically correlated with health, these estimates cannot be interpreted as causal. To isolate causal effects, we exploit a 2013 reform introducing a health assessment after 12 months of sickness leave, which substantially shortened sickness spells. Implementing an augmented regression discontinuity in time design, we find that shorter spells increase employment by about five percentage points for up to nine years, accompanied by reduced reliance on sickness and disability benefits. Conditional on employment, there is no effect on wages, hours, contract type, or sectoral mobility.

Keywords: sick leave, employment, health, flexible work

JEL codes : J24, J30, J41

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*Affiliations Koning and Muller: Vrije Universiteit Amsterdam and Tinbergen Institute. Affiliations Riumallo Herl and Rossi: Erasmus University Rotterdam and Tinbergen Institute. Corresponding author: Rossi (rossi@ese.eur.nl). This paper has benefited from feedback from seminar audiences at ... Muller gratefully acknowledges financial support from Vidi grant from the Dutch Science Foundation (NWO). Results in this paper are based on non-public microdata from Statistics Netherlands (CBS). In principle, part of these microdata are accessible for statistical and scientific research. For further information: microdata@cbs.nl.

1 Introduction

A large share of employees experience sickness leave periods during their career (Hemmings and Prinz, 2020), and the risk of illness is greater among workers in precarious employment arrangements (Koning et al., 2025). At the same time, the consequences of illness are manyfold: not only do they place a burden on social security systems, but they also potentially have long–term career trajectory consequences. This is particularly evident in cases where the illness persists until the point where an application for disability insurance (DI) is filed. Naturally, the literature has focused extensively on this particular outcome, while attempting to design DI systems that avoid excessive inflow to disability benefits for workers with remaining capacity to work.

In many countries, the period of illness preceding a DI application is substantial. Applicants are typically eligible only after at least one year of sickness (e.g. UK, Norway, Spain, Belgium), eighteen months of sickness (e.g. Germany, Japan, Austria) or even two years of sickness (e.g. the Netherlands and Sweden) (Koning, 2016). As a result, lengthy sickness spells occur regardless of DI applications, and these may have implications for labor market trajectories. Prolonged absences may weaken labor market attachment, potentially causing loss of human capital, reduced career progression, stigma, and, in the case of temporary workers, increased risk of unemployment after contract expiration. These mechanisms are probably most relevant for temporary workers whose fixed-term contracts expire during the sick leave spell. At the same time, sickness spells are obviously associated with worse health – likely persisting into the future – complicating the identification of the causal impact of sickness leave spells on long-term labor market outcomes.

In this paper, we study the relationship between the duration of sickness leave and long-term labor market outcomes. Our approach is twofold. First, we estimate event-studies where we identify temporary workers that experience a sickness leave period and match them with observationally equivalent temporary workers who do not experience sickness (or only very short sickness spells), using a rich set of individual and job characteristics. We estimate the event-studies using administrative, population-wide data from the Netherlands (2010-2015), focusing specifically on all employees with fixed-term contracts.[1] We hypothesize that the consequences of sickness leave spells are more severe for temporary workers than for permanent workers, whose contracts cannot be dissolved before the disability application point after two years of sickness. We estimate event-studies for four groups of sick workers defined by the duration of their sickness spell: one week to one month, one to six months, six to thirteen months, and more than thirteen months.

[1] In the spirit of Ahammer et al. (2024), our sampling strategy ensures that the distribution of these characteristics in the control group perfectly replicates the one of the treatment group, allowing for a more accurate counterfactual.

These analyses show that even short sickness spells are associated with a persistent decline in employment, with effects growing in magnitude as spell duration increases. Conditional on employment, sickness leave also leads to sustained reductions in wages, hours worked, and likelihood of having a permanent contract in the future, accompanied by displacement from previous jobs, firms, and sectors. We also identify a peak in unemployment insurance (UI) uptake at the end of the sickness leave (and consequently the end of the Sickness Insurance (SI) entitlement), suggesting substitution across social programs.

To shed light on underlying mechanisms, we use information on healthcare expenditures as a proxy for changes in health over time. Even when health conditions were not severe enough to result in DI entitlement, we find a persistent health deterioration following the initial sickness spell. The higher long-term healthcare use and continued reliance on health-related social programs partly explain the estimated adverse labor market effects. Overall, the negative association between prolonged sickness leave and long-run labor market outcomes likely reflects a combination of health deterioration and human capital loss. Consequently, we cannot interpret the differences as causal evidence of scarring effects.

Our second approach proceeds beyond associations and attempts to estimate the *causal* effects of sickness duration leveraging a natural experiment. We exploit a reform in the Netherlands that introduced an assessment of illness after one year of sickness leave. Performed by the public Employee Insurance Agency (UWV), the assessment was intended to screen out individuals who were deemed unlikely to qualify for DI benefits if they would apply after two years of sickness. This assessment thus encouraged many workers on sickness leave to return to the labor market after 12 months, rather than waiting until, potentially, 24 months. The reform generates exogenous variation in the sickness leave spell duration, allowing us to estimate the causal impact of sickness spell length on long-term labor market outcomes (we can follow workers until 2024).

Exploiting the exact timing of the screening reform, we apply a regression discontinuity in time estimator (RDiT, e.g. Hausman and Rapson (2018)). Identification is based on the fact that individuals who fall ill just before the reform date (January 1^{st}, 2013) do not face the 12-months assessment, while those falling ill just afterwards are assessed after 12 months. While seasonal variation might generate differences between those falling ill in December and January, we can use earlier years to filter out such variation using the augmented specification as described by Hausman and Rapson (2018). We find that the reform has a strong first-stage impact: sickness leave spells starting after the reform are significantly shorter, especially when conditioning on a sample workers that is sick for at least 6 months. This reduction in sickness leave duration leads to a persistent five percentage point higher employment rate relative to those unaffected by the introduction of

the 12-month assessment, with the effect lasting up to nine years after illness onset. Conditional on employment, shorter sickness leave spells however do not appear to affect other dimensions of career trajectories, including wages, hours worked, the likelihood of holding a permanent contract, or changes in employer or sector relative to those at the onset of the sickness leave spell. Overall, these findings suggest that the duration of sickness leave spells can influence long-run labor market trajectories. While the likelihood of entering DI programs is the only mechanism examined so far, future analyses are needed to explore additional channels, such as employer discrimination and human capital depreciation.

The remainder of this paper is structured as follows. First, we provide an overview of the institutional framework and details on our data sources in Section 2. Next, we present the empirical strategy and report the results of the event study and regression discontinuity analyses in Section 3 and Section 4. We conclude in Section 5.

2 Institutional Setting and Descriptive Statistics

2.1 The Dutch Sickness Benefit Scheme

Upon falling sick, employees in the Netherlands are required to report their incapacity to work to their employer by the second day of illness. By law, both permanent and ongoing fixed-term contracts cannot be dissolved during illness. Workers with permanent contracts are entitled to continued wage payments by their employer for up to two years (referred to as *the waiting period*), after which they may apply for disability insurance (DI) benefits (see Koning and Lindeboom (2015) for further details). Benefit applications are only admissible when both the employer and the worker have followed all required reintegration obligations as stipulated by the *Gatekeeper Protocol* (Koning et al., 2025). Full DI benefits are set equal to 70% of the pre-disability earnings, and partial benefits are proportional to the assessed loss of the applicants' earnings capacity. The earnings loss threshold for partial benefit receipt equals 35% of pre-disability earnings.

During the waiting period, employers bear full responsibility for permanent workers' wage continuation. They are required to report short sickness spells only to their occupational health service or company doctor. When the sick leave spell exceeds 42 weeks, they are also required to notify the Employee Insurance Agency (UWV). In contrast, for temporary workers, employers must report their sickness to UWV immediately. The employer continues to pay wage payments until the contract expires, after which UWV assumes responsibility by providing social insurance benefits under the Sickness Benefits Act (In Dutch: *Ziektewet*). SI benefits replace up to 70 per-

cent of pre-illness earnings and are granted for a maximum duration of 104 weeks. The level of SI benefits is adjusted according to the employees' earnings during the sickness period, as working is allowed with an implicit tax of 70% on extra earnings. The length of entitlement of SI benefit is usually determined after the medical assessment with the UWV physician, which occurs no later than 15 weeks after the worker has reported sick, and when the UWV caseworker draws up a return-to-work plan (as part of the Gatekeeper Protocol). SI recipients are also obliged to report their health recovery, which leads to an immediate termination of the benefit entitlement.

Against the backdrop of persistent increases in DI applications from fixed-term contract workers, the Dutch government implemented a set of reforms in 2013 specifically aimed at this group (Koning et al., 2025). In addition to expanded employer monitoring and financial responsibilities during the waiting period, it introduced a mandatory first-year medical evaluation for sick temporary workers. While being more rudimentary than the assessment at DI application, the first-year evaluation aimed to screen out workers with expected losses of earnings capacity below 35% of the pre-disability earnings. As will be shown later on, the implementation of the first-year evaluation led to a significant outflow of workers 12 months after the onset of sick leave, and most likely also a shift in temporary workers' sickness leave spell durations from two to one year (see Figure A1 in the Appendix). In section 4, we exploit this exogenous reduction in expected sickness duration to isolate the causal effect of a longer sickness spell (the 'scarring effect') from health-based selection (cross-sectionally, those with longer spells tend to have worse health in the future as well).

2.2 Data Sources

Our analysis combines three sources of administrative records to construct an individual level monthly panel dataset including information on labor market performance and health trajectories. Our data covers the universe of temporary workers who reported sick at least once between 2010 and 2015.

First, we use data from the Dutch Employee Insurance Agency (UWV) on the sickness leave spells of temporary workers as reported by Dutch employers between 2010 and 2015. The data contain the start and end dates of each spell, where the start date is the day the worker reported being sick and the end date reflects either the recovery date or the expiration of the two-year waiting period. The data also contain the reason for the end of the sickness leave, the date and outcome of the first year evaluation, and whether the sickness leave spell resulted in a subsequent DI application and entitlement.

Second, we use tax records from Statistics Netherlands (CBS) containing linked employer-

employee data for all jobs in the Netherlands between 2010 and 2024. The data include unique identifiers for workers, jobs, and firms, as well as detailed information on sector codes and contract start and end dates, contract type (permanent vs. temporary), monthly earnings, working hours, and working days (Centraal Bureau voor Statistiek, 2025). We complement these data with information from the System of Social Statistical Files (SSB) on individuals' socio-economic category, which records at the monthly level (from 1999 to 2023) individuals' entitlement to social benefits.

Lastly, we use administrative data on healthcare expenditures from health insurance claims to proxy an individuals' health status. These costs cover the annual healthcare costs covered and reimbursed by the basic health insurance. Since the Healthcare Insurance Act (In Dutch: *Zorgverzekeringswet*, Zvw) legally requires all individuals residing or working in the Netherlands to take out basic private health insurance, we observe the patterns of healthcare expenditure for the full Dutch population from 2009 and 2022. The data also includes a breakdown of total healthcare expenditures across several provider categories, including general practitioners, prescription drugs, hospitalizations and mental health treatments.

2.3 Descriptive Statistics

By combining information on the start date of sickness leave spells with the start and end dates and type of job contract, we identify workers who reported sick while employed on a temporary job. We exclude sickness leave spells that lasted fewer than eight calendar days, as such short-lived events are unlikely to generate long-lasting effects in workers' labor market trajectory. Our analysis sample comprises all temporary workers that experience such a sickness spell within our observation window, and we focus on the first spell per individual in case there are multiple.

As our analysis focuses on the scarring effects of temporary health shocks on long-term labor market outcomes, we distinguish between individuals whose sickness leave spell did not lead to subsequent DI benefit receipt and those for whom it did. This distinction will provide a benchmark to assess how labor market trajectories differ between transitory and more persistent health conditions.

Our final sample contains 251,316 individuals (including individuals who eventually receive DI benefits). Column (1) and (2) of Table 1 report their descriptive statistics. To characterize the composition of our sample, Columns (3) and (4) show the corresponding means for the broader populations of permanent and temporary workers, respectively, as defined in January 2010. Compared to the full population of temporary workers, those experiencing sickness leave (without DI, column 1) are less likely to be Dutch natives, female, or highly educated, and are typically em-

ployed in lower-paying jobs in the month they report sick. Similar differences exist for the group that does eventually claim DI, although on average they are more similar to the general population in column 3. In line with the timing of the sickness leave spell, annual healthcare expenditures in the respective year are consistently higher across all cost categories sick temporary workers (columns 1 and 2). Those without eventual DI are, on average, 2.8 years older than the general population. But this age gap does not seem to fully account for the striking difference, as their healthcare expenditures also significantly exceed those of the population of permanent workers, who are on average 7.5 years older (column 4).

Table 1: Descriptive Statistics of Full Population and Selected Subsample of Temporary Workers

	(1)	**(2)**	**(3)**	**(4)**
	Sick Temp. Workers	**DI Recipient, Temp. Workers**	**Temp. Workers**	**Permanent Workers**
A. Socio-demographics				
Female	0.44	0.52	0.51	0.46
Age	35.29	40.88	32.52	42.73
Dutch Native	0.68	0.70	0.76	0.84
Bachelor Degree	0.16	0.15	0.33	0.25
B. Healthcare Expenditure (per year)				
Total Costs (euros)	3,970.51	12,758.58	1,294.56	1,457.37
Hospitalizations (euros)	2,307.61	7,184.13	651.48	827.68
GP Visits	59.38	94.50	29.30	30.55
Drugs (euros)	255.10	689.12	166.91	224.47
Mental Health Treatments	981.07	4,048.58	208.73	134.58
C. Labor Market Outcomes				
Hourly Wage (euros)	11.25	12.94	13.43	19.14
Monthly Hours Worked	98.07	116.12	108.48	133.60
Monthly Days Worked	18.40	21.60	19.81	24.86
D. Sick Leave Spell				
Duration				
1 week - 1 month	0.35	-	-	-
1-6 months	0.29	-	-	-
6-13 months	0.19	-	-	-
>13 months	0.17	-	-	-
Type of Recovery				
Self-Reported	0.58	-	-	-
After Consultation	0.20	-	-	-
Termination Waiting Period	0.07	-	-	-
Other Reasons	0.15	-	-	-
Number of Individuals	226,434	24,882	1,843,500	5,171,272

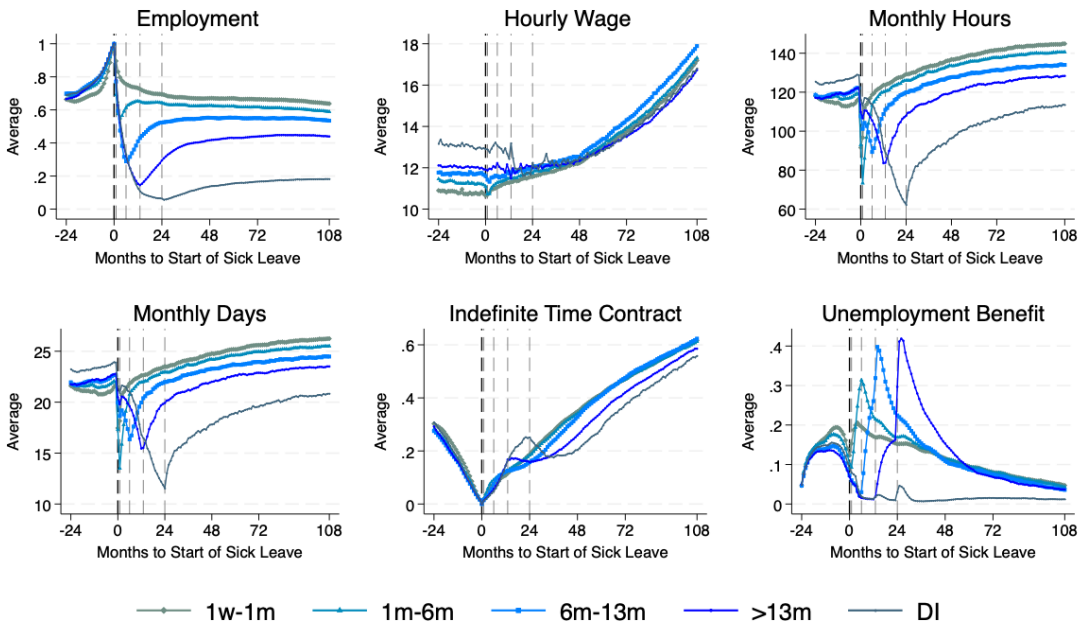
Notes: In Column (1) and (2) of this table, we present the mean value of individuals' characteristics measured in the calendar year in which temporary workers called in sick. In Column (3) and (4) we present the corresponding mean values for the entire population of temporary and permanent workers of 2010 who did not call in sick. These two groups of workers are defined according to their contract type on January 2010. Healthcare expenditure and wages are measured in 2010 euros.

Panel D of Table 1 summarizes the longest observed sickness leave spell for each individual, detailing its duration and recorded recovery type. Approximately 35% of individuals experienced a longest sickness leave spell of one month or less, while a comparable proportion had spells exceeding six months. Among the latter, roughly half exceeded thirteen months. In Appendix Figure A1 we show the entire distribution of the sickness leave duration for individuals calling in sick before and after January 2013, when the first-year evaluation was introduced. The figure illustrates a striking shift in the distribution of sickness leave spell durations for individuals reporting sick from 2013 onwards, with density mass moving from the end of the waiting period (i.e., two years after sickness onset) to approximately month thirteen, the month immediately following the first-year evaluation. Note that we also see the consequence of the reform by comparing the shifts in the recorded reasons for the termination of the sickness leave spells for individuals reporting sick before and after 2013. Specifically, Appendix Table A1 shows that the share of sickness leave spells reaching the end of the two-year waiting period decreased by 6.8 percentage points, while the share that ended after a consultation with the return-to-work counselor rose by 4.5 percentage points.

We stratify the sample into four subgroups based on the duration of the longest sickness leave spell. As shown in Figure 1, individuals exhibit distinct labor market trajectories that persist well beyond the end of the sickness period. Those with longer sickness leave spells experience markedly worse labor market outcomes even nine years after the onset of their spell, by which point they most likely have recovered from the underlying health condition.

At the extensive margin, only 44% of individuals sick for more than thirteen months remain employed seven years after the end of the two-year waiting period. The corresponding employment rates are approximately 53% for those with spells lasting six and thirteen months and 64% for those those with spells lasting between one week and one month. Similar negative correlations with sickness leave spells occur at the intensive margin, the monthly hours worked, number of working days, the likelihood of holding a permanent contract, and hourly wages. Patterns in unemployment insurance (UI) benefit receipt around the termination of sickness leave spells are suggestive of social support substitution, as individuals transition to UI benefits when SI entitlement is exhausted.

Figure 1: Average patterns in labor market outcomes, by sickness leave spell duration and DI status



Note: This figure illustrates the patterns in average labor market outcomes centered around the month in which the temporary workers in our estimation sample called in sick. We present these patterns for five different subgroups, separately, categorized according to the duration of the characterizing sickness leave spell and the DI benefit entitlement status in its aftermath. Only individuals categorized as "DI" have been awarded DI benefit in the aftermath of their sickness leave spell.

Alongside these labor market patterns, the trajectories of healthcare expenditures and benefit entitlements shown in Figure A2 in the Appendix point to a health channel that may contribute to these disparities. Workers with longer sickness leave spells consistently exhibit higher healthcare utilization and higher likelihood of qualifying for DI or SI benefits, indicating that those with poorer underlying health both endure longer spells and suffer from larger and persistent labor market penalties. Given the well-established interplay between health and labor-market performance, these health differentials likely account for a substantial share of the observed gap in career outcomes.

3 An Exploratory Event Study Approach

In Section 3.1 we outline the event study approach with one-to-one matched controls, which enables us to isolate the long-term dynamics on individuals' labor market outcomes following sickness leave spells of varying duration. Section 3.2 presents the corresponding estimates for a broad set of labor market outcomes, while Section 3.3 discusses their interpretation and outlines further analysis.

3.1 Event-study with one-to-one matched controls

In the previous section, we descriptively uncovered that longer sickness leave spells are associated with long-lasting worsening of labor market outcomes. However, these groups with distinct sickness leave duration also differ on various demographic and labor market characteristics. The first part of our empirical analysis aims to address these differences by matching the respective individuals one-to-on with observationally equivalent controls, consisting of temporary workers who did not experience any sick spell (exceeding seven days). Clearly, this approach remains a descriptive exercise, as there may be time-variant unobservables – particularly related to the evolution of health outcomes – that extend the sick leave spell and also are likely to impact labor market outcomes. As we will argue later on, estimates from this approach can be interpreted as an upper bound of the effect of human capital depreciation and reduced access to the labor market. To obtain a truly causal impact, section 4 exploits the earlier mentioned policy reform.

The treatment group comprises temporary workers who reported sick for a spell longer than seven calendar days (the same group as described in Section 2.3). The control group is drawn by conditional-random sampling from the entire population of temporary workers who did not call in sick for a spell longer than seven days in the same period. For each treated individual i, we construct a sub-sample comprising all temporary workers in the same calendar month who exactly match i on key observable characteristics. The matching variables include gender, age, highest educational attainment, 12-month labor market history (the share of months employed and the earnings bracket), and the decile of healthcare expenditures in the calendar year preceding the month in which individual i reported sick. Subsequently, we randomly draw from this individual-specific sub-sample one individual for the control group. In the same spirit of Ahammer et al. (2024), this sampling strategy ensures that the distribution of observed characteristics in the control group perfectly replicates that of the treatment group.

We partition the treatment group into the same duration-based categories used in the previous section: sickness spells lasting one week to one month; one to six months; six to thirteen months; and more than thirteen months. We add an additional treatment group defined by individuals whose sickness leave spell resulted in DI benefits entitlement, which will serve as a benchmark to compare how labor market trajectory differs between transient and more long-lasting health conditions. For each of these five groups, we repeat the aforementioned conditional random sampling of the control group, allowing for the same individuals to serve as control across multiple sub-treatment groups.

To formalize matters, let $\tilde{m}_i$ denote the month in which individual i called in sick, and m as the

month relative to $\tilde{m}_i$. For individuals in the control group, m is computed relative to the month of the placebo sickness leave spell used for the matching procedure discussed above. For each of the five treatment groups (indexed by s), we estimate the following event study model:

$$y^s_{i,m} = \sum_{\substack{k=-24 \\ k\neq -6}}^{108} \beta^s_k \cdot \mathbf{I}[k=m] \times T^s_i + \gamma^s_m + \gamma^s_i + \epsilon^s_{i,m}, \tag{1}$$

where $y^s_{i,m}$ is the labor market outcome of interest for individual i, m months away from the month they called in sick, T^s_i is an indicator for treatment, and γ^s_m and γ^s_i capture relative months and individual-fixed effects, respectively.[2] The full dynamics of the effect of sickness leave spells up until nine years (108 months) after its occurrence is captured by the coefficient estimates of β^s_k, representing our coefficients of interest. The model is estimated for different sub-treatments, s, separately, allowing for heterogeneous treatment effects for different sickness duration groups. All model estimates are computed relative to month $t = -6$, which serves as the pre-sickness baseline. Because individuals who report sick in month $t = 0$ are – by construction – employed under temporary contracts, and employment status and contract type exhibit serial correlation, we use this relatively distant baseline to mitigate upward bias in the estimation of the full dynamic effects on employment and the probability of having an indeterminate-time job. Because of perfect matching between the aforementioned potential confounding factors, we cannot control for any of the matching dimensions in our model specification.

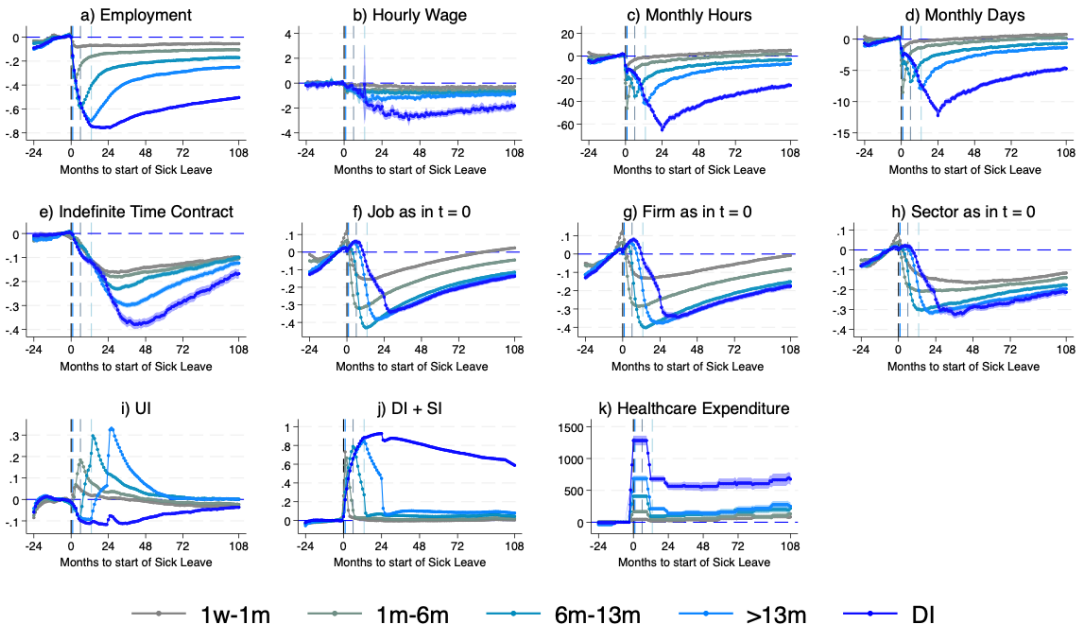
The identification assumption underlying our model corresponds to the conventional parallel trends assumptions for DiD models. In our case, it implies that trends in outcomes for temporary workers experiencing a sickness period and those for the non-sick controls that we conditionally matched with them would have moved parallel absent the sickness leave spell. By matching on average employment rate, earnings and healthcare expenditure in the twelve months before the sickness, we mechanically impose equal pre-sickness trends in the second half of the pre-sickness period for these outcomes. However, underlying differences in health trajectories may still generate divergent outcome dynamics even in the absence of sickness leave. Consequently, as noted above, our estimates should be interpreted as upper bounds on the causal effect of sickness leave on subsequent labor market outcomes.

[2] It should be noted at this point that, due to left-censoring at 2010, we have a panel which is unbalanced for the pre-sickness period ($m < 0$). Specifically, we partially observe the two-year pre-sickness period for those reporting ill between January 2010 and December 2012.

3.2 Results

Figure 2 plots the full dynamic of the effect of sickness on labor market outcomes and healthcare expenditure, estimated according to the event study model described in Section 3.1 (β_k^s in equation 1). The effect is estimated separately for the four different sub-groups (defined by sickness leave spell duration), as well as for the group whose health conditions led into DI entitlement (dark blue line).

Figure 2: Full Dynamic Trajectory of the Effect of Sickness Leave, by Sickness Leave Duration



Note: This figure plots full dynamic trajectory of the effect of sickness leave on several labor market outcomes for different sub-groups of the population of temporary workers who called in sick between 2010 and 2015. These subgroups are defined according to the duration of the characterizing sickness leave spell and the DI entitlement in its aftermath. Each dot represents a β_m point estimate from the model outlined in Equation (1), estimated relative to the baseline pre-sickness leave month $m = -6$. The estimates are plotted together with their 95% confidence intervals from clustered standard errors at the individual level.

There are four key takeaways from these graphs. First, Panel (a) illustrates persistent drops in labor market participation. This holds true also for the group of individuals who experienced the shortest sickness leave spell duration (between seven and thirty days), who are 10 percentage points less likely to be employed than their matched healthy counterpart even nine years after the start of their sickness leave spell. As expected, this negative effect increases with sickness leave duration, reaching about 25 percentage point decrease for those sick for more than thirteen months. Second, conditional on employment, the intensive margin and contract terms are also affected. Employed sick workers suffer from a long-term decrease in wage, hours worked and the likelihood of being

on a permanent job. Similarly to extensive margin results, longer sickness leave spells go together with larger negative effects on these outcomes. Interestingly, sick workers are less likely to stay in the same job, firm and sector where they reported being sick, suggesting that these non-permanent health shocks can divert them from their established career paths and tenure trajectories. Third, for all sub-groups, we see UI benefits peaking in the months corresponding to the sickness benefit exhaustion date, and consequently of the termination of SI benefits entitlement. This temporal alignment is suggestive of social support substitution, whereby individuals transfer from SI to UI without re-entering the labor market. Such substitution behavior, also documented in the Dutch context by Borghans et al. (2014), highlights the interconnectedness of social insurance programs and the potential for spillover effects when one program entitlement ends.

Lastly, we consider the evolution of health expenditures. Given that this information is recorded annually, sickness spells starting late in a calendar year may lead to an underestimation of expenditures in the first months of sick leave. We therefore limit our analysis to spells beginning between January and March, and convert to monthly values by dividing by 12 for this sub-sample.[3] We find relatively small, but persistent differentials in healthcare expenditure by sick leave duration. After nine years, the monthly healthcare expenditures are around 200 euros (60%) higher for sick leave spells lasting longer than 13 months compared to the full sample of sick-listed temporary workers).

3.3 Interpretation

The results from our event study are suggestive of long-term substantial adverse effects of sickness leaves, which are exacerbated by the length of it and, consequently, by the severity of the underlying health condition. Although the underlying conditions are not severe enough to result in DI benefit entitlement, sick workers exhibit long-term declines on both the extensive margin – becoming significantly less likely to be employed – and the intensive margin – earning lower wages, working fewer hours, and holding fewer permanent contracts. Moreover, they are more prone to switch jobs, firms, and sectors, suggesting that a sickness leave spell can divert individuals from their original career trajectories.

While the magnitude of the estimated effects is sizable and relevant for all the considered labor market outcomes, these results cannot be interpreted as the causal impact of sick leave spells. As was illustrated in Panel k) of Figure 2, sick workers' health does not re-converge to levels of

[3] As shown in Figure A3, the estimated patterns for the other labor-market outcomes in this January–March sub-sample overlap closely with those from the full sample, reassuring us about its representativeness.

the pre-sickness period. As a result, the long-term impact of sick leave periods on labor market outcomes should be considered as an upper bound of genuine duration effects due to stigma and losses of human capital. Supported by abundant evidence in the literature on the tight link between health and labor market outcomes (Currie and Madrian, 1999; García-Gómez et al., 2013), the identified long-run downturn in performance may reflect persistent health deficits — i.e., selection into sickness—rather than a pure scarring effect of sickness absence.

To shed more light on the distinct effects of scarring effects and those induced by persistent changes in health, we next investigates the effect of *exogenous* changes in the length of sick leave spells. For this, we exploit the 2013 reform that imposed a mandatory 12-month screening within the two-year waiting period. Since the implementation of this reform exogenously shortened sickness leave spells for temporary workers calling in sick after that date, a RDiT design will allow us to hold health constant for the *compliers* – i.e, those removed from the SI program by the first-year evaluation who would otherwise have remained – and thus isolate non-health channels of the sickness penalty. We will discuss this approach and the results emerging from its implementation in detail in the next section.

4 Regression Discontinuity Analysis

4.1 Specification

To obtain causal estimates of scarring effects of sick leave on various long-term labor market outcomes, we use an augmented RDiT design. Specifically, we use the two-step procedure proposed in Hausman and Rapson (2018), referred to as augmented local linear regression discontinuity. Since the introduction of the first-year assessment affects sickness leave duration only for temporary workers who actually undergo it — or expect to — we restrict our analysis to long sickness leave spells. To account for potential anticipation effects to the 12-months assessment, we include individuals with a minimum sickness leave duration of 180 days.

In the first stage, we estimate seasonality on the considered labor market outcomes by considering the entire population of temporary workers who called in sick between 2010 and 2015 for all the calendar months in our dataset. We do this by regressing the considered outcomes on both months and year fixed effects. Subsequently, we compute and store the residuals as seasonality-adjusted outcomes. In the second stage, a local linear specification is then estimated on these seasonality-adjusted outcomes for just a sub-sample of the entire population of sick workers whose sickness leave spells started within a narrow bandwidth around the discontinuity threshold of January 1^{st},

2013. Specifically, we estimate the equation:

$$\tilde{y}_{it} = \alpha^t + \beta^t D_i + f(date_i) + \epsilon_{it} \tag{2}$$

where $\tilde{y}_{it}$ is the seasonality-adjusted outcome, D_i is an binary variable for calling in sick after January 1^{st}, 2013, and $date_i$ is the start date of the sickness leave. Following Gelman and Imbens (2019), who advise against using high-order polynomial $f(\cdot)$, we use a local linear polynomial, such that we can rewrite the equation as:

$$\tilde{y}_{it} = \alpha^t + \beta^t D_i + \gamma_1^t date_i + \gamma_2^t D_i \cdot date_i + \epsilon_{it} \tag{3}$$

We estimate the model across a wide range of labor market outcomes and for multiple time periods t. Specifically, we consider both elapsed time – ranging from $t = 13$ to $t = 109$ months after the start of the sickness leave spell – and fixed calendar time, covering each month from January 2014 to December 2024. This approach allows us to document the full dynamic effect of sickness leave duration over several years after its occurrence. By analyzing both elapsed and fixed time periods, we also can rule out residual seasonality as a potential confounder. For all these outcomes, we estimate Equation (3) using a 60-day bandwidth around the threshold, corresponding to January 1^{st}, 2013. We also test the sensitivity of our estimates by varying bandwidths around the cutoff date, spanning from 30 to 210 days. This allows us to assess the bias-variance trade-off, characterizing regression discontinuity designs, and evaluate the robustness of our results to different definitions of the running variable window.

Intuitively, our two-step procedure leverages observations away from the cutoff only to estimate seasonality in the labor market outcomes, while restricting identification of the local average treatment effect to observations near the cutoff. This allows the two local linear polynomials to be estimated using only the relevant bandwidth around the threshold, while accounting for broader seasonal patterns in the outcome variables. If the typical RD assumption holds – i.e. ϵ_{it} does not change discontinuously around the threshold – and the first stage fully captures seasonality in the outcomes, the local average treatment effect estimated by $\hat{\beta}^t$ will be unbiased.

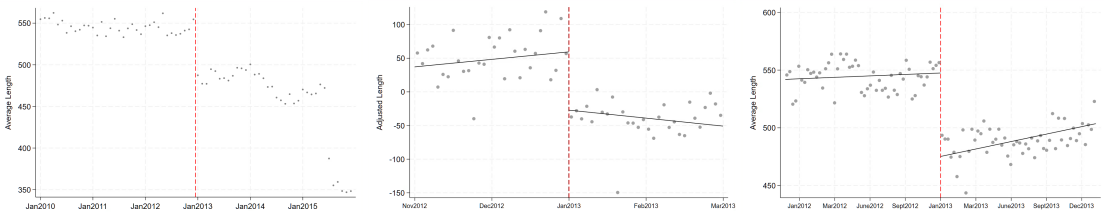
4.2 First-stage and assumptions

To graphically examine the presence of direct cutoff reform effects on SI benefit receipt, the panels in Figure 3 plots the average sickness leave spell length for individuals reporting sick from 2010 to 2015. Recall that we condition on sickness leave spells for at least 180 days. To start with, Figure 3a plots the raw average sickness leave spell duration over the full sample period, revealing a sharp discontinuity at the January 1, 2013 threshold marking the introduction of the first-year assessment.

At the cutoff, the average duration declines by around 77 days. In panel Figure 3b we run the augmented local linear RD design using sickness leave duration as the outcome variable, controlling for reporting-month fixed effects and considering a symmetric 60-days bandwidth. Similarly to what observed for the raw averages, a strong discontinuity appears for this specification, with a statistically significant drop by 86.51 days. Similar results are obtained using a more traditional RD model with a 13-month bandwidth, where we simultaneously control for reporting-month fixed effects, as illustrated in Figure 3c. The identified discontinuity in sickness leave spells' length is also robust to different bandwidth and polynomial orders, as reported in Table A2 in the Appendix.

Figure 3: Decline in sickness leave spell duration after the 2013 reform

(a) Raw Average **(b)** Augmented Local Linear **(c)** 13-Month Bandwidth



Note: This figure plots the sharp decline in sickness leave duration at the January 1, 2013 cutoff following the introduction of the first-year assessment. The running variable is the start date of the sickness leave spell. Panel (a) shows raw average durations at the calendar-month level. Panel (b) plots adjusted averages and fitted values from the augmented local linear RD specification with a 60-day bandwidth, controlling for start-month fixed effects using the full sample of temporary workers with sickness leave spells from 2010 to 2015. Similarly, Panel (c) presents results from an alternative traditional RD design specification using a 13-month bandwidth, which likewise allows to account of start-month fixed effects.

In cross-sectional RD designs, the McCrary (2008) density test is commonly used to detect bunching around the cutoff and to rule out endogenous manipulation into treatment. With time as a continuous running variable in our case, however, we indirectly evaluate sorting around the January 1, 2013 threshold by testing for discontinuities in predetermined characteristics of individuals reporting sick around the cutoff. We test a broad set of potential confounders of sickness leave duration and long-term labor market outcomes (including age, education, gender, tenure, and past work experience), and find no evidence of discontinuities at the threshold in variables other than the treatment, as reported in Table 2. This lends credence to the identifying assumption of non-manipulation into treatment around the cutoff date.

Table 2: Test for sorting around the cut-off based on pre-determined individual characteristics

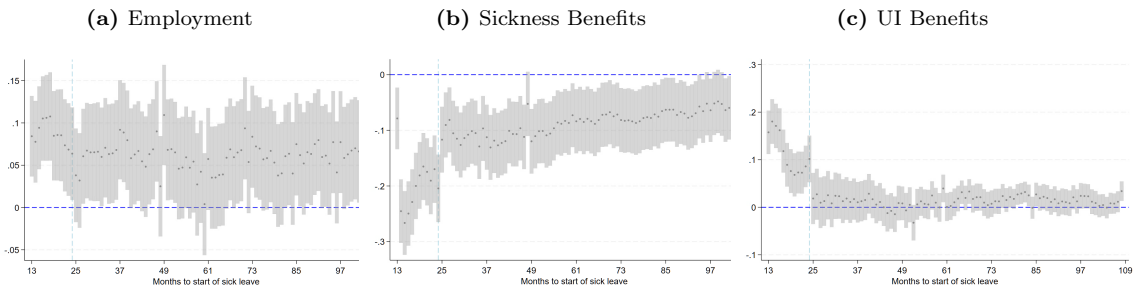
	(1) Female	(2) Degree	(3) Age	(4) Tenure	(5) Employment
Y_0	0.009	0.033	0.001	-0.845	0.007
	(0.031)	(0.023)	(0.718)	(0.612)	(0.015)
Number of observations	5,590	5,590	5,590	5,590	5,590

Note: This table reports the estimated discontinuity around the January 1st, 2013 start date cutoff for five different pre-determined individual characteristics, separately. Estimates are obtained from a sharp RD design using local linear regressions with a symmetric 60-day bandwidth on either side of the threshold. *Female* is a binary indicator for being a woman. *Degree* indicates possession of a university degree. *Age* is individuals' age at the start of the sickness leave spell. *Tenure* denotes years worked at the employing firm prior to the sickness leave spell. *Employment* measures months worked in the 12 months preceding the spell. Standard errors are reported in parentheses.

4.3 Main results

Figure 4 plots the dynamic effects of shorter sickness leave spells on employment outcomes, measured up to nine years after the start of the sickness leave spell. Estimates are obtained from the augmented local linear specification in Equation (3), estimated separately for each month from $t = 13$ to $t = 109$.

Figure 4: Dynamic of effect of shorter sickness leave spells on employment outcomes



Note: Estimates for the augmented local linear RDiT specification with a 60-day bandwidth. For each outcome, point estimates are obtained separately for each elapsed month between 13 and 109 months from the start of the sickness leave spell. Employment is defined as having at least one active job contract in a given month. Sickness benefits refers to receiving either disability or sickness insurance benefits during that period. UI recipiency is defined as receiving unemployment insurance benefits. Shaded areas represent 95% confidence intervals.

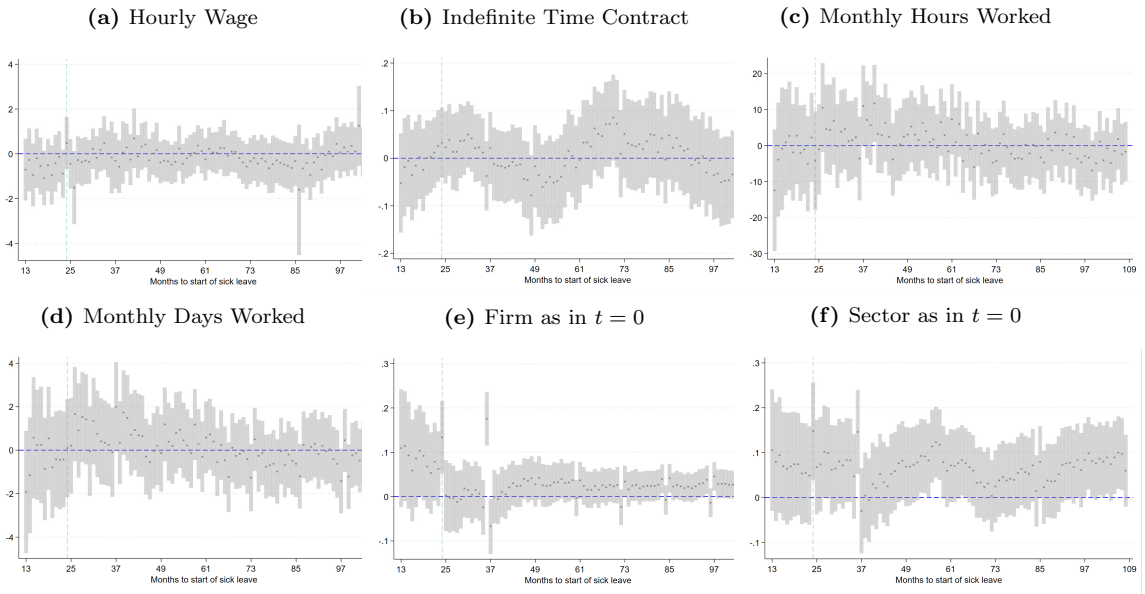
Our estimates reveal a direct and persistent positive effect on the likelihood of being employed, even after the expiration of the two-year waiting period for the control group. The magnitude of the effect is stable at roughly five percentage points. This long-term positive effect on employment is not mirrored by a long-term decline in UI recipience, which does not differ from zero after the

end of the waiting period of the control group (see Figure 4c). In contrast, Figure 4b shows a persistent and statistically significant decline in sickness benefit receipt, encompassing both SI and, subsequently, DI. Alongside these findings for employment and SI and UI benefit outcomes, Figure 5 shows no statistically significant long-run effects on job characteristics, including wage, the likelihood of holding a permanent contract, hours and days worked, and the likelihood of being employed at the same firm or in the same sector as when the sickness leave spell began.

Similar patterns emerge for our outcome variables when using fixed rather than elapsed months and when varying the bandwidth around the January 1, 2013 cutoff (see Figure A4 and Figure A5 in the Appendix). Moreover, the placebo test for individuals who reported sick around January 1, 2012 (Figure A6) rules out the possibility that the estimated effect is driven by unobserved differences between individuals calling in sick at the end versus the beginning of calendar years.

Because the RD design identifies a local average treatment effect for individuals whose sickness leave spells would have been extended had they fallen ill immediately before the reform, this gap cannot be attributed to pre-existing differences in health between treatment and control groups. One explanation could be that there was a positive pure health effect of shorter sickness spells potentially facilitated by earlier labor market reintegration. Given the immediate employment effect, however, it is more likely that the reform screened out relatively healthy workers. These workers may in turn have responded to the loss of SI benefits by adjusting their labor supply (see e.g. Garcia-Mandicó et al. (2020)).

Figure 5: Dynamic of effect of shorter sickness leave spells on conditional labor market outcomes



Note: These figures plot the dynamic effects of shorter sickness leave spells on a set of labor market outcomes conditional on being employed, estimated using the augmented local linear RDiT specification with a 60-day bandwidth. For each outcome, point estimates are obtained separately for each elapsed month between 13 and 109 months from the start of the sickness leave spell. Shaded areas represent 95% confidence intervals.

To conclude, the RDiT estimates suggest a persistent long-run positive effect of shorter sickness leave spells on employment. This effect appears to be driven by a direct screening effect on long-term sick leave spells of relatively healthy workers who resume work. Given that this effect is immediate and persistent, it is unlikely that the 12-months assessment prevented subsequent losses of human capital and increases in stigma for this group.

5 Conclusion

In this paper we investigate the long-run scarring effects of sickness leaves on workers' labor market outcomes. In doing so, we focus on the Dutch population of temporary workers In the Netherlands, whose sickness leave spells are tracked from the Dutch Employment Insurance Agency (UWV) from the day they call in sick, which also administers SI benefits once the wage continuation duties of the employers are terminated and the temporary job contract expires.

We construct a monthly panel of administrative records around the calendar month in which sickness leave spells start, tracing at the individual level patterns on employment, earnings, hours, contract type, and healthcare expenditures for workers calling in sick between 2010 and 2015. Using an event-study design with one-to-one matched never-sick controls, we trace dynamic effects

up to nine years after the beginning of the sickness leave spell. We document that even short spells (7–30 days) reduce employment by 10 percentage points, with larger declines—up to 25 points – for spells exceeding thirteen months and not resulting into DI entitlment. Conditional on employment, workers also experience persistent wage and hours losses, lower permanent-contract probabilities, and elevated rates of job, firm, and sector switching. Despite the non-permanent nature of their health condition, sick workers also exhibit elevated long-term healthcare use and continued SI/DI reliance, indicating persistent health deterioration beyond the initial illness. These health deficits likely drive much of the observed downturn in labor market performance, complicating causal interpretation. Accordingly, our event-study cannot disentangle health-mediated effects from other mechanisism potentially operating via labor demand frictions or human capital depreciation. To isolate the latter, we exploit a 2013 reform introducing a twelve-month evaluation aimed at reassessing benefit eligibility and return-to-work potential. A regression discontinuity in time (RDiT) around this cutoff induces exogenous variation in sickness leave spell duration while holding underlying health constant. This analysis shows that shorter sickness leave spells increase employment by about 5 percentage points for up to nine years after illness onset. The rise in employment is accompanied by a persistent decline in SI and DI benefit receipt, which may promote employment by reducing financial resources. Conditional on employment, no effects are found on wages, hours, contract type, or employer/sector mobility. These results suggest sickness leave duration shapes long-run labor market trajectories, with DI entry as the primary mechanism examined to date. In the next steps, we will examine other potential driving mechanisms, such as employers' discrimination and human capital depreciation. These insights will inform the optimal design of benefit durations and reintegration policies.

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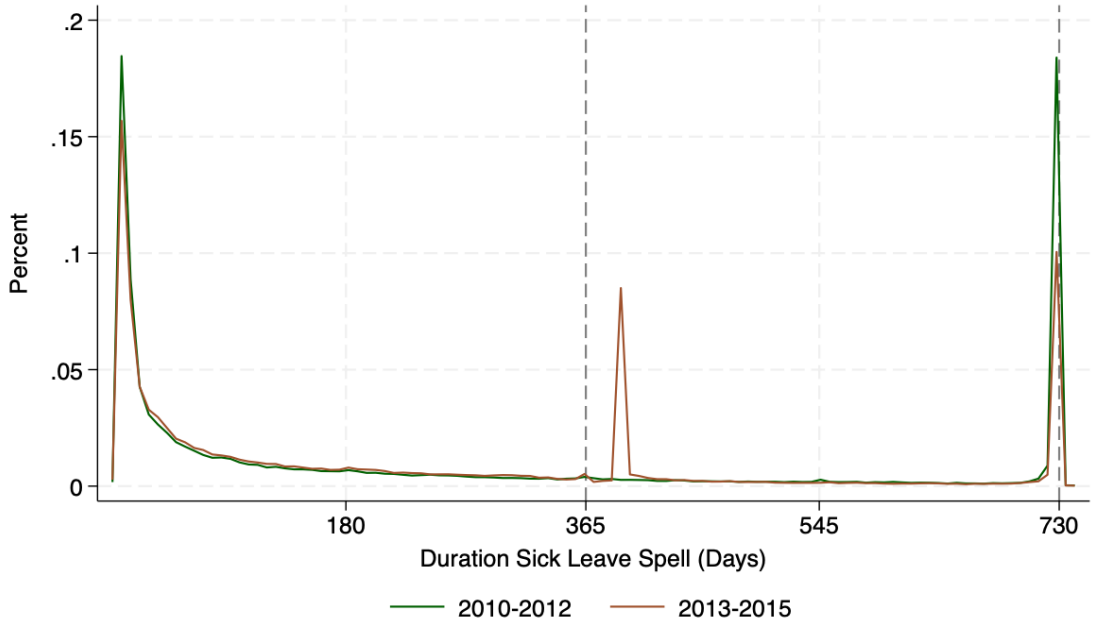
Appendix

Figure A1: Distribution of sickness leave duration before and after 2013



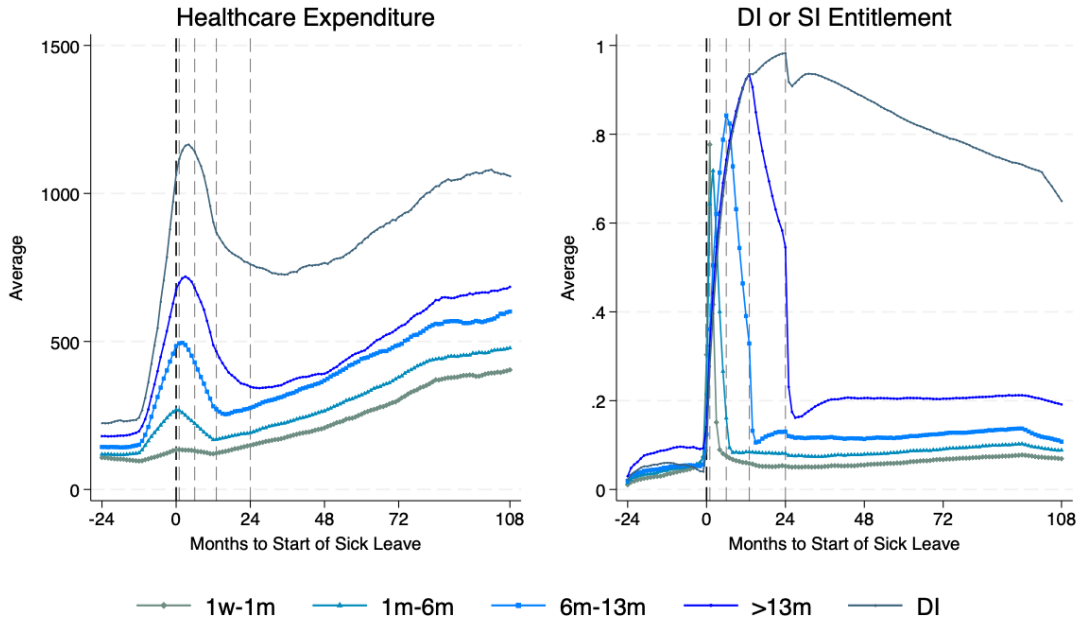
Note: This figure illustrates the distribution of sickness leave duration for temporary workers who called in sick before and after the introduction of the first-year assessment during the two-year waiting period, implemented for sickness spells starting from January 1st 2013 onwards.

Table A1: Sickness Leave Spells before and after the introduction of the first-year assessment

	(1) Sick before 2013	(2) Sick from 2013
Sick Leave Spell Duration (Days)	251.36	221.19
First-year Assessment	0.00	0.26
Self-Reported	0.49	0.56
After Consultation	0.16	0.21
Termination Waiting Period	0.09	0.03
DI Entitlement	0.10	0.08
Other Reasons	0.15	0.11
Number of Individuals	140,427	110,889

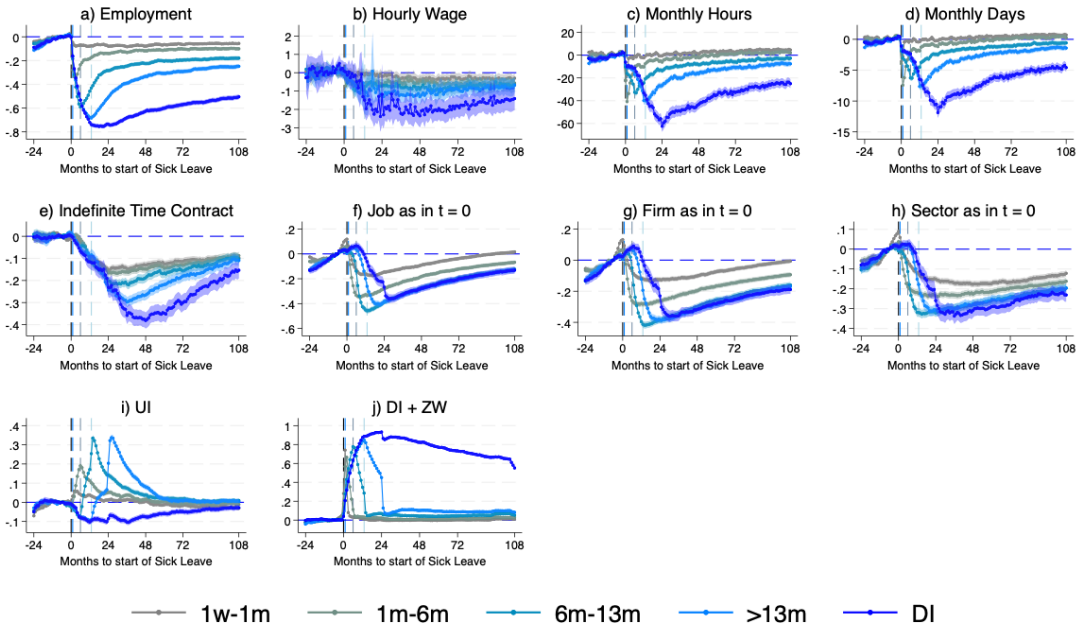
Notes: This table shows the mean characteristics of sickness leave spells of temporary workers that started before and after the introduction of the first-year assessment during the two-year waiting period, implemented on January 1st, 2013.

Figure A2: Average patterns in health-related outcomes, by sickness leave spell duration and DI status



Note: This figure illustrates the patterns in monthly healthcare expenditure and prevalence of DI and SI benefits centered around the month in which the temporary workers in our estimation sample called in sick. We present these patterns for five different sub-groups, separately, categorized according to the duration of the characterizing sickness leave spell and the DI benefit entitlement status in its aftermath. Only individuals categorized as "DI" have been awarded DI benefit in the aftermath of their sickness leave spell.

Figure A3: Full Dynamic Trajectory of the Effect of sickness leave, by sickness leave duration



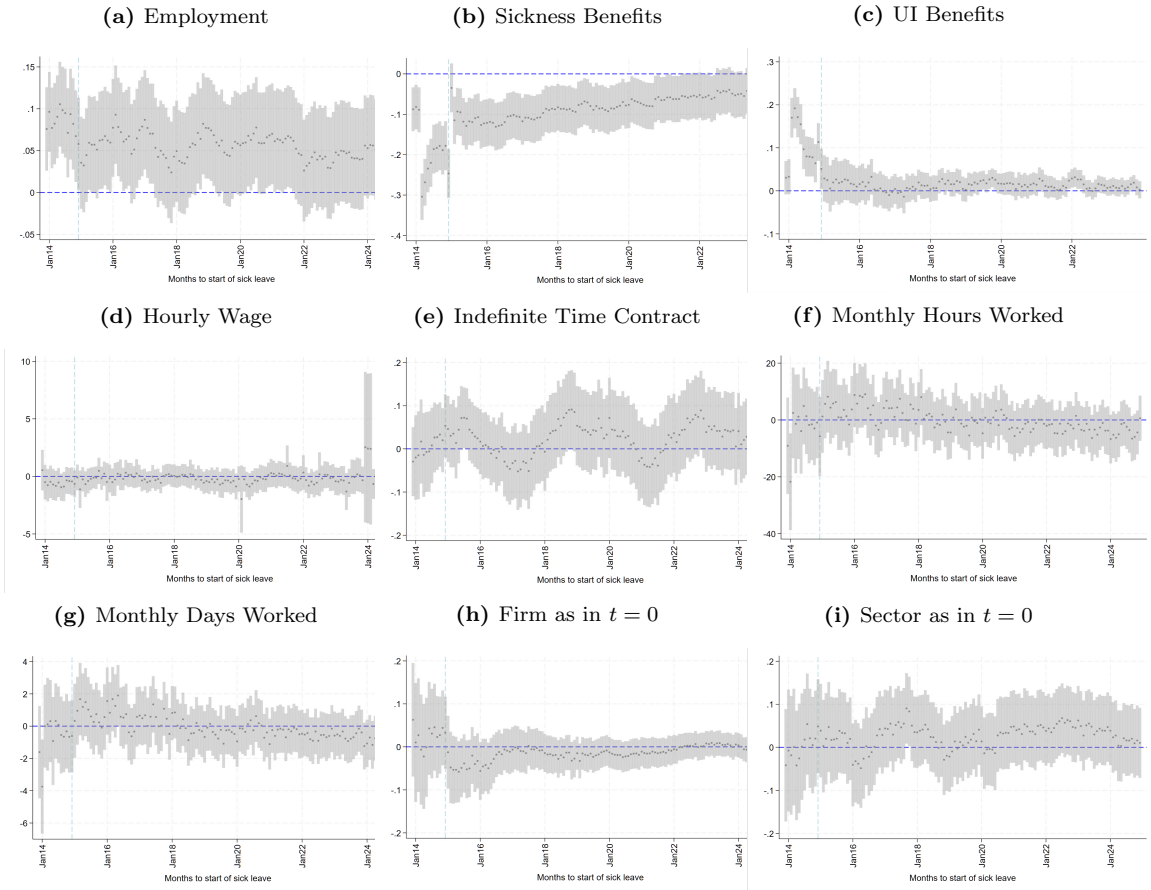
Note: This figure plots full dynamic trajectory of the effect of sickness leave on several labor market outcomes for different sub-groups of the population of temporary workers who called in sick between 2010 and 2015 between January and Match. These subgroups are defined according to the duration of the characterizing sickness leave spell and the DI entitlement in its aftermath. Each dot represents a β_m point estimate from the model outlined in Equation (1), estimated relative to the baseline pre-sickness leave month $m = -6$. The estimates are plotted together with their 95% confidence intervals from clustered standard errors at the individual level.

Table A2: Robustness of first stage across RDiT specifications

	Augmented Local Linear				13-month Bandwidth			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Duration	−88.918***	−86.511***	−88.483***	−88.827***	−72.351***	−75.790***	−78.150***	−78.479***
	(18.903)	(12.605)	(10.050)	(8.558)	(4.614)	(6.860)	(9.157)	(11.588)
Polynomial Order	1	1	1	1	1	2	3	4
Bandwidth	30	60	90	120	396	396	396	396
N. of Observations	2,709	5,590	8,493	11,658	37,761	37,761	37,761	37,761

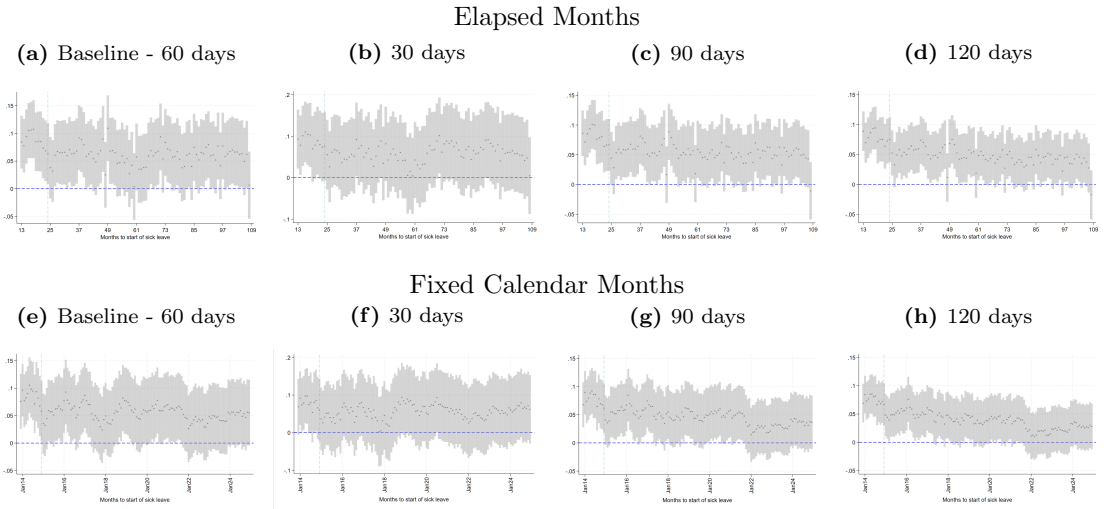
Note: This table reports the estimates from several RD designs testing the effect on sickness leave duration at the January 1, 2013 cutoff following the introduction of the first-year assessment. The running variable is the start date of the sickness leave spell, while the outcome variable is sickness leave spell duration.

Figure A4: Dynamic of effect of shorter sickness leave spells on labor market outcomes, conditioning on calendar months



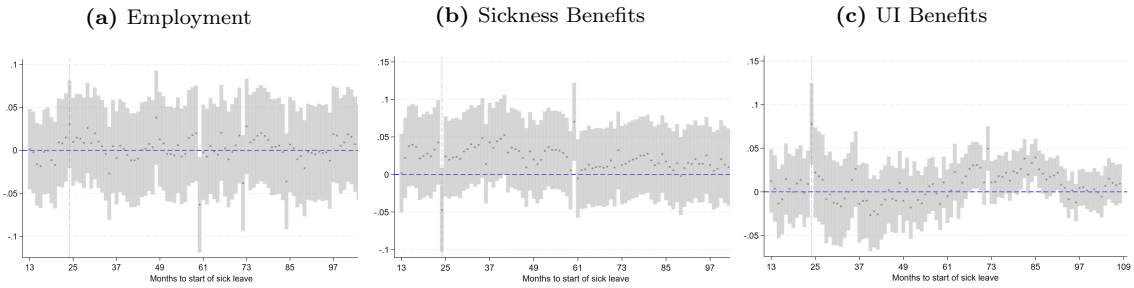
Note: These figures plot the dynamic effects of shorter sickness leave spells on a set of labor market outcomes, estimated using the augmented local linear RDiT specification with a 60-day bandwidth. For each outcome, point estimates are computed separately for each calendar month from January 2014 to December 2024, except for sickness and unemployment benefits, which are observed only through December 2023. Since the analysis compares treated and control groups over calendar time, the first stage does not require conditioning on calendar month fixed effects, unlike the elapsed-time specification. Instead, the first stage of the augmented local linear RDiT design includes fixed effects for the start month of the sickness leave spell to account for potential unobserved differences between individuals calling in sick in November and December vs January and February. Shaded areas represent 95% confidence intervals.

Figure A5: Dynamic of effect of shorter sickness leave spells on employment status across different specifications and bandwidths



Note: These figures plot the dynamic effects of shorter sickness leave spells on employment status, estimated using the augmented local linear RDiT specification with varying bandwidths and time definitions. The running variable is the start date of the sickness leave spell, and the bandwidth ranges from 30 to 120 days around the January 1, 2013 cutoff, marking the introduction of the first-year assessment. When time is defined in elapsed months, it refers to the number of months since the start of the sickness leave spell. For each elapsed month between 13 and 109, we estimate the RDiT model separately, including calendar month fixed effects in the first stage. Whereas, when time is defined in calendar months, we estimate the model separately for each month from January 2014 to December 2024. In this case, because the treated and control groups are observed at the same point in time, calendar month fixed effects are not necessary. However, we include fixed effects for the start month of the sickness leave spell to account for potential unobserved differences between individuals calling in sick early versus late in the year. Shaded areas represent 95% confidence intervals.

Figure A6: Placebo Analysis on employment outcomes



Note: These figures plot the dynamic effects from a placebo analysis estimated using the augmented local linear RDiT specification with a 60-day bandwidth. We consider as placebo event a sharp drop in sickness leave spell duration after January 1st 2012. For each outcome, point estimates are obtained separately for each elapsed month between 13 and 109 months from the start of the sickness leave spell, considering temporary workers who called in sick around the placebo treatment date. Employment is defined as having at least one active job contract in a given month. Sickness benefits refers to receiving either disability or sickness insurance benefits during that period. Similarly, UI recipiency is defined as receiving unemployment insurance benefits. Shaded areas represent 95% confidence intervals.