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ABSTRACT

Why Do Temporary Workers Have Higher Disability Insurance Risks Than Permanent Workers?*

Workers with fixed-term contracts typically have worse health than workers with permanent contracts. We show that these differences in health translate into a substantially higher (30%) risk of applying for disability insurance (DI) in the Netherlands. Using unique administrative data on health and labor market outcomes of all employees in the Netherlands, we decompose this differential into: (i) selection of workers types into fixed-term contracts; (ii) the causal impact of temporary work conditions on worker health; (iii) the impact of differential employer incentives to reintegrate ill workers; and (iv) the differential impact of labor market prospects on the decision to apply for DI benefits. We find that selection actually masks part of the DI risk premium, whereas the causal impact of temporary work conditions on worker health is limited. At the same time, the differences in employer commitment during illness and differences in labor market prospects between fixed-term and permanent workers jointly explain more than 80% of the higher DI risk.

JEL Classification: J08, I1, J22, H53

Keywords: disability insurance, temporary work, employer incentives, worker health

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1 Introduction

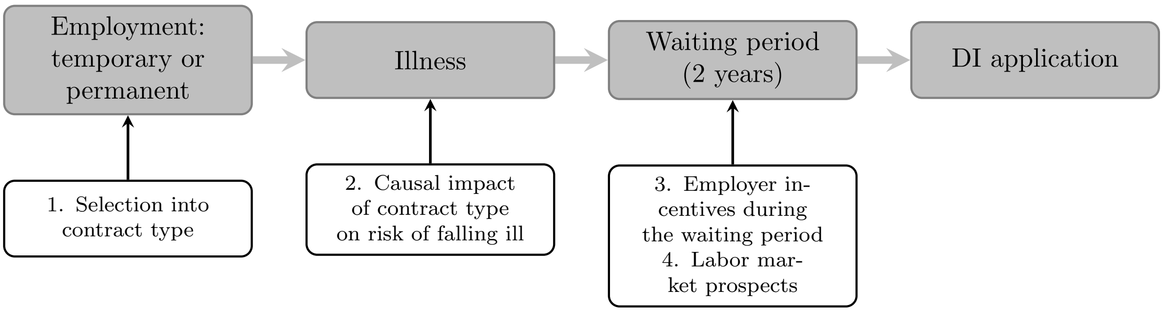
In most OECD-countries, workers with fixed-term contracts show worse health conditions than workers with permanent contracts (OECD, 2010).[1] This negative association is found both in studies based on cross-sectional data and panel surveys – see Kim et al. (2012), Virtanen et al. (2005) and Benach et al. (2014) for survey studies – and pertains to both mental and non-mental health problems (Bardasi & Francesconi, 2004). From a public finance perspective, however, evidence on differences in the enrollment of temporary and permanent workers into Disability Insurance (DI) is scarce.[2] When temporary workers have higher DI risks, a crucial question is whether temporary work arrangements causally lead to higher DI risk or whether there is a segmentation of the labor market in which relatively unhealthy workers with weaker labor market prospects end up in fixed-term contracts. The latter has been suggested for the US, where vulnerable workers with low productivity levels entail an increasing fraction of the Social Security Disability Insurance (SSDI) benefit recipients (Maestas, 2019; Autor & Duggan, 2003; Deshpande & Lockwood, 2022). These workers have either worse health conditions ('health disability') or limited prospects on the labor market abstain them from work ('work disability') (Benítez-Silva et al., 2010).

In this paper, we study the mechanisms behind DI application risk differentials between temporary and permanent workers. We use large-scale administrative data for the Netherlands on labor market histories, disability application records and health consumption and health treatments. In the time period under investigation (2010-2015), the prevalence of fixed-term contracts increased, and the DI risk for this group has been approximately 30% higher than for workers with permanent contracts – and the difference is even larger when correcting for differences in demographics.

[1]Note that 'flexible work' may refer to various employment constructions in the literature. In this paper we focus on fixed-term contracts, which can be considered the most widespread type of flexible employment.

[2]The existing literature on the impact of non-standard work arrangements tends to focus on the wage effects of specific types of non-standard work arrangements, such as temp work agencies (Drenik et al., 2020; Katz & Krueger, 2019; Goldsmith & Schmieder, 2017).

Figure 1: Timeline from employment to DI application: potential mechanisms explaining the gap in DI risk between fixed-term and permanent workers



We hypothesize that there are four potential explanations for the DI risk premium that are relevant at consecutive stages before DI application, as illustrated by Figure 1. These include: (i) ex-ante compositional differences in characteristics between permanent and temporary workers; (ii) the causal impact of the contract type on the probability of falling ill; (iii) the impact of the differential employer role during illness (but prior to DI application); and (iv) the role of outside options in the labor market for the decision to file a DI application. By unifying an analysis of all of these explanations, we provide a comprehensive picture of the drivers of the higher DI risk of temporary workers. Such an analysis is essential for guiding policy: if there is a causal impact of contract type, policies to reduce DI risk might focus on discouraging the use of temporary work arrangements. But if not, structural changes to employer incentives or improvements in the labor market prospects of vulnerable workers would be required to reduce DI inflow rates.

In brief, we find that selection of workers into contract types cannot explain the DI risk differential. Also the risk of falling ill is not substantially larger for temporary workers (once controlling for observables). Rather, the employer efforts during illness and the difference in outside options in the labor market explain most of the higher DI risk for temporary workers. Put differently, it is not the fact that temporary work is associated with more frequent illness, but that *given illness* temporary workers face different support and incentives that makes them more likely to ultimately file a DI application.

Following the timeline in Figure 1, we first regress the DI application risk on contract type and sequentially add a wide range of demographic, employment, occupational and prior health controls. Our initial focus is on selection into fixed-term contracts that stems from specific worker types and specific jobs with higher or lower a-priori disability risks. Once controlling for these factors, we find that the monthly DI application risk is about 50% higher for workers with a fixed-term contract (compared to the raw difference of 30%). Even with additional sets of control variables (such as prior health measures), this gap remains almost constant, suggesting that workers with worse health are not more likely to select into fixed-term contracts. These results are confirmed by non-parametric weighted DI risks. While we cannot exclude that permanent and temporary workers differ on dimensions still unobserved to us, the robustness of the risk premium estimate suggests that selection is unlikely to explain the difference.

Second, we consider differences in the likelihood of falling ill between temporary and permanent workers. We interpret these differences as direct effects of contract type on health deterioration, due to higher occupational hazard or increased stress due to lack of job security.[3] To proxy the occurrence of falling ill, we define health shocks as substantial increases in medical consumption for physical costs (both hospitalizations and medication) and/or the start of mental health treatment. We then find that the risk of mental health shocks is about 10% higher for temporary workers than for permanent workers. As to the risk of DI application conditional on being ill, the gap in relative risk decreases to approximately 40%. The risk of a physical health shock is about 2% lower for temporary workers, and the gap in DI risk conditional on such a shock remains approximately 50%.

We next focus on what happens *after* a worker falls ill. At this stage, the employer faces weaker incentives to facilitate rehabilitation for workers whose contract will expire anyway (i.e., explanation 3). This divergence is strengthened

[3] Note that these effects may be dampened if fixed-term contracts are probation periods and workers therefore have incentives not to report absent (Ichino & Riphahn, 2005; Riphahn & Thalmaier, 2001; Engellandt & Riphahn, 2005).

by financial incentives inherent with continued wage payments and experience rating being targeted at permanent workers only. In light of these considerations, we exploit a policy reform in 2013 that increased the monitoring obligations and the financial consequences for the employer if their temporary worker enters DI. Using a difference-in-difference strategy on the sub-sample of workers that have experienced a health shock, we find that about half of the conditional DI risk differential is explained by differences in employer incentives. As such, we also add to earlier findings in the literature that experience rating affects fatality and injury rates (Koning, 2009; Kyyrä & Tuomala, 2013; Tompa et al., 2012; De Groot & Koning, 2016).

We finally change our focus to differences in *incentives* for ill temporary and permanent workers during the waiting period. We hypothesize that employees with temporary contracts are more susceptive to bad labor market prospects, as their contract is likely to end during their waiting period. When their outside options are limited, applying for DI becomes a more attractive option. We assess the relevance of this channel by comparing temporary workers in occupations with high and low labor market tightness (Autor & Duggan, 2003; Benítez-Silva et al., 2010). Conditional on illness, we then find that the DI application risk for permanent workers does not vary systematically with market tightness. This contrasts to temporary workers, for whom the risk is substantially smaller in tight sectors. Specifically, in sectors with tight labor markets, the DI risk premium for temporary workers – conditional on being ill – shrinks to only 8%. Combing our four sets of results, we are able to explain more than 80% of the DI risk premium for employees with fixed-term contracts.

The remainder of this paper is organized as follows. In Section 2 we present the relevant institutions in the Netherlands and show descriptive evidence on DI risk and healthcare expenditures. Section 3 illustrates a decomposition framework which will be used as a guideline for the empirical analyses. Section 4 lays out our empirical strategy and presents the results for the various mechanism. Finally, Section 5 compares our results to other studies, and Section 6 concludes.

2 Institutional setting and descriptive statistics

In this section we provide a brief overview of the institutional setting in the Netherlands regarding DI benefits and the distinction between fixed-term and permanent contracts. We provide definitions of the key terminologies in our analysis and show relevant descriptive statistics.

2.1 The DI scheme in the Netherlands

Implementation of the DI system in the Netherlands is carried out by the Employee Insurance Organization (UWV).[4] DI benefit applications follow after two years of illness; this is referred to as the 'waiting period' (see also Figure 1). DI applications consist of a medical assessment by a medical expert and an assessment of remaining earnings capacity by a labor market expert. If the loss in earnings capacity is below 35% of pre-application wages, the application is rejected. Benefits amount to 70% of the loss in earnings – relative to pre-disability earnings –, although further financial incentives exist to stimulate making use of one's remaining earnings capacity. For further details, see Koning et al. (2022). Applicants with a loss in earnings capacity between 35% and 80% are awarded partial benefits, and applicants with a loss in earnings capacity above 80% are awarded full benefits.

During the waiting period permanent and temporary workers face different employer support and commitment. For *permanent workers*, employers are obliged to continue wage payments during illness for the entire waiting period. Concurrent with this, they are obliged to actively monitor the health of the employee and provide support to stimulate rehabilitation, for example through facilitating adjusted working conditions. Moreover, awarded DI benefits are experience-rated for permanent contracts. For workers with *fixed-term contracts*, the employer continues their wage payments during illness up to the point where

[4]Note that in the years prior to 2006 a range of reforms was implemented to reduce DI inflow after the number of DI recipients had been growing substantially in the 80s and 90s. For further details and evaluations of these reforms we refer to Koning & Lindeboom (2015), Van Sonsbeek & Gradus (2012), Godard et al. (2019) and Hullegie & Koning (2018).

the contract expires. Next, these workers receive illness benefits through social insurance. Until 2013, the employer's (financial) responsibility for the fixed-term worker ended at that point. In case the ill worker entered the DI system, the extra DI benefit costs were not experience rated.

As the share of DI applications of fixed-term contracts steadily increased since 2006, the government introduced a reform in 2013 that extended monitoring and financial obligations of the employer to ill-listed temporary workers. Since then, employers remain financially responsible for their ill-listed employees after their contract expires. This means that employer premiums of sick-pay and DI benefits for fixed-term workers are experience rated. To alleviate the financial risks that might arise for small employers, the premium is averaged within sectors for employers with less than 10 employees.[5] Together with this change, a one-year medical assessment was introduced for ill-listed individuals whose contract had expired. A similar assessment was already in place for ill-listed employees with a permanent contract. Overall, the 2013 reform made both employer obligations and incentives more comparable between temporary and permanent workers.

It should be noted that fixed-term contracts are limited in duration in the Netherlands. An employer is allowed to hire a worker for at most three consecutive fixed-term contracts, with a joint maximum duration of three years. The fourth contract needs to be permanent.[6] While permanent contracts offer substantial job protection, both permanent and fixed-term contracts cannot be dissolved during illness. Temporary workers therefore remain employed during illness as long as their contract lasts.

2.2 Data sources

Our analysis is based on three administrative datasets which are merged at the individual level. The combination of these three data sets allows us to construct

[5] For employers with more than 10 and less than 100 workers, the DI premium is a weighted average of the individual and the sector-averaged premium. For this group, the weight of the individual premium linearly increases from 0% to 100% with respect to firm size.

[6] The counting is reset after a break of at least three months, meaning that only with short breaks lengthy durations of fixed-term employment are possible with the same employer.

the employment and health status trajectories for all employed Dutch individuals. First, we use tax records provided by Statistics Netherlands, containing detailed descriptions of all employment contracts in the Netherlands between 2010 and 2015. It includes the commencement and end dates of contracts, identifiers for the individual and the firm, the type of contract (fixed-term or permanent), industry code, weekly hours and paid salary.

Second, we use administrative healthcare expenditures data. The data concerns total annual individual healthcare expenditures as covered by the basic health insurance system, as well as a breakdown by healthcare type (17 categories). Basic health insurance is mandatory for all Dutch adults; consequently, the data cover the entire Dutch population. We extend these data with even more detailed data on mental health treatment trajectories for which the exact start and end date are available, as well as the number of treatment-minutes per month covering 2011–2016. These data are also provided by Statistics Netherlands.

Third, we use data describing all Disability Insurance applications between 2010 and 2015, provided by the Employee Insurance Agency (UWV). These data contain details on filed applications regarding the health impairments assessment and the subsequent labor market assessment by vocational experts that determines the remaining earnings potential and the corresponding degree of disability. Both rejected and approved applications are included in our data.

2.3 Descriptive statistics

Column (1) of Table 1 shows descriptive statistics for our full sample of employed individuals. The sample contains over 10 million individuals that are employed in at least one month in our observation window (2010–2015). The top panel shows demographics and education, as measured in January 2010. The middle panel describes health care measures averaged over the period 2010–2015. Employed individuals have on average €948 non-mental healthcare costs and €187 mental healthcare costs per year. The lower panel shows employment measures, again

Table 1: Descriptive statistics of the full sample and selected subsamples

	Employed population[a]	Temporary workers[b]	Permanent workers[b]	DI applicants
Demographics[c]:				
Age	37.4	32.2	42.5	43.0
Female	46.5%	50.1%	46.3%	52.5%
Dutch native	77.0%	74.9%	84.0%	72.7%
Education unknown	34.9%	17.4%	42.0%	25.1%
Education (if known):				
Low	17.5%	14.1%	15.1%	34.3%
Middle	41.5%	44.5%	37.7%	44.7%
High	41.0%	41.5%	47.2%	21.1%
Annual health measures[d]:				
Mental healthcare expenditures (in €)	187	216	123	1252
Physical healthcare expenditures (in €)	948	846	1032	3709
Mental health treatment (in minutes)	60.0	81.6	43.2	477.6
Employment measures[e]:				
Permanent contract[f]	47.8%	29.4%	81.1%	49.4%
Fixed-term contract[f]	18.7%	50.8%	7.6%	15.7%
Hourly wage	23.5	19.1	26.2	21.2
Monthly number of working hours	77.1	87.5	107.0	75.0
Awarded DI benefits[g]				43.2%
Number of individuals	10,583,956	1,785,327	5,184,711	253,628

(a) All unique individuals who are employed at some point in time between 2010 and 2015, (b) Reference date for contract type is January 2010, (c) Demographics in January 2010, (d) Health measures are averages computed over the time window 2010–2015, (e) Employment measures are averages computed over the time window 2010–2015, (f) Percentage of months with a certain type of contract, (g) Percentage of DI applicants who have been awarded DI benefits.

averaged over 2010–2015. On average, individuals have a permanent contract for 48% of the months in our observation window, while in 19% of the months they have a fixed-term contract. In the remaining months they are not employed.

Columns (2) and (3) of Table 1 show the separate statistics for temporary and permanent workers, respectively. Due to the longitudinal nature of the data, a single individual can change contract type over time. We therefore classify individuals by their contract types as measured in January 2010. Temporary workers are substantially younger, less likely to be Dutch natives, and lower educated than permanent workers. They also have lower physical healthcare expenditures, but higher mental healthcare expenditures, which might be driven by the age difference. In the lower panel we see that workers with a fixed-term contract in January 2010 have a permanent contract in 29.4% of the months in 2010–2015. The reverse pattern is much less common: permanent workers only spend 7.6% of the next five years in fixed-term contracts. Permanent workers also have higher hourly wages and work more hours per week. Finally, column (4) of Table 1 shows statistics of the subsample of DI applicants, of which there are over 250,000 in our observation window. The most striking differences with the working population are their higher age, lower level of education, higher healthcare use (on all measures) and slightly lower wage level. Note that these descriptives conceal the DI risk premium for temporary workers: among DI applicants the share with a fixed-term contract is similar to the share in the employed population, but to a large extent this is due to the average age of DI applicants (older individuals are more likely to hold permanent contracts).[7]

2.4 DI application risks

To derive DI application risks by contract type, it is key to account for the two-year waiting period that precedes DI applications. Specifically, the population at risk for DI-application in month t consists of all individuals employed in month $t-24$. The populations at risk for permanent and temporary workers between 2010 and 2015 are shown in panel (a) of Figure 2. While the population at risk with permanent contracts decreases steadily, the prevalence of fixed-term contracts increases with similar magnitude. Note also that the number of unemployed increases sharply due to the recession, though this group is an order of magnitude smaller than the population of employed individuals. UI recipients can also file a DI application if they fall ill.

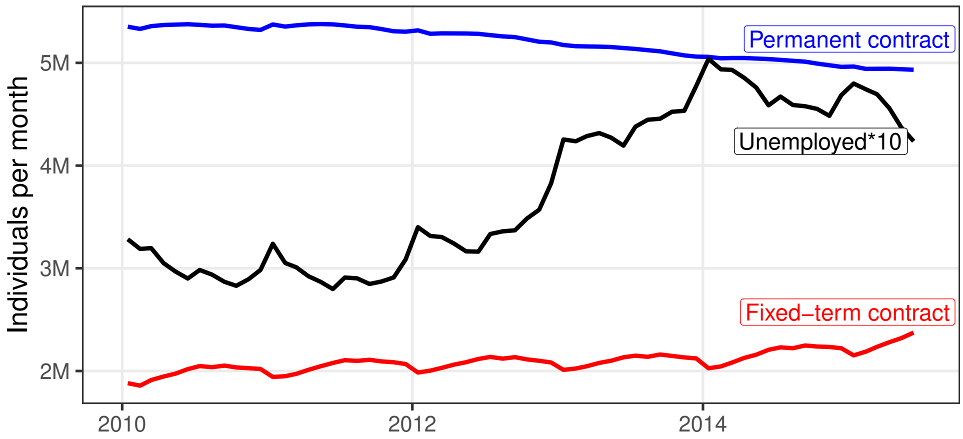
We classify all DI-applicants by their contract type 24 months prior to their application date. Most DI applicants can be classified into one of these three groups (permanent, temporary or unemployed), but a small share of approximately 1% cannot.[8] We assign all those without either an employment contract

[7]Note that in addition, the sampling in the table makes it infeasible to directly observe relative DI risks because the presented shares of permanent fixed-terms contracts in column (1) represent the number of months while in column (4) they represent number of people.

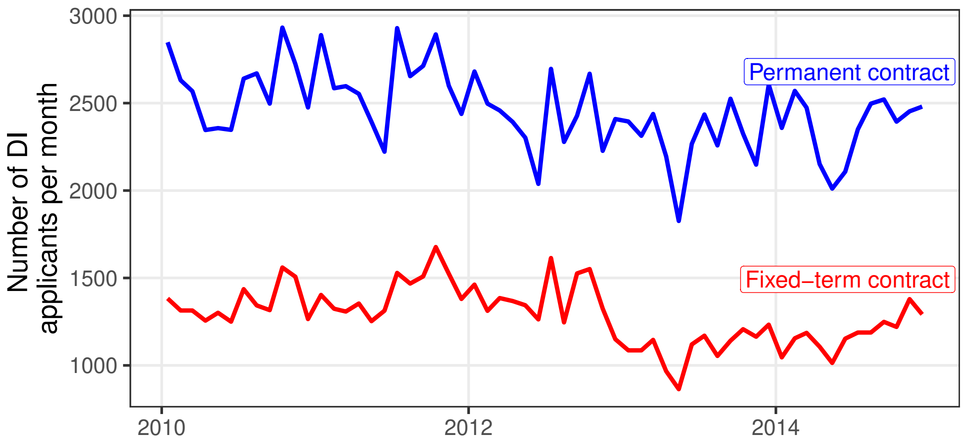
[8]There are several reasons for the remaining unclassified applicants. There are exceptions to the length of the waiting period, meaning that $t-24$ is not always the relevant month to consider. Furthermore, if a worker falls ill shortly after their contract ended, they are still eligible for DI benefits two years later. In this case it is difficult to identify the relevant month.

Figure 2: Number of employed, DI applicants and DI risk

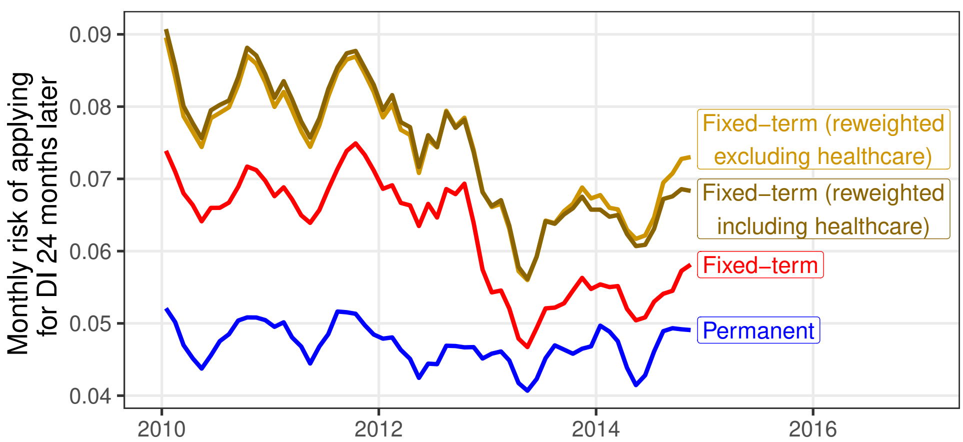
(a) Risk populations (number of employed)



(b) DI applicants



(c) DI risk



or UI benefits to the fixed-term contract category, as they do not have an active employer during their illness spell. The numbers of DI applicants per category between 2010 and 2015 are shown in panel (b) of Figure 2. The largest group of DI applicants are those with a permanent contract, but the difference with the group of temporary workers is considerably smaller than the corresponding difference in the populations at risk.

The DI risk equals the number of DI applicants at time t divided by the population at risk in month $t-24$. Panel (c) of Figure 2 shows the pronounced difference in DI risk between the groups. Workers with fixed-term contracts face a risk about 1.5 times as high as those with permanent contracts. The difference is fairly stable until 2013, after which it becomes considerably smaller.

Previewing our analysis in the next section, we briefly explore whether the substantial DI risk difference between permanent and temporary workers stems from compositional differences shown in Table 1. We do so by reweighting the DI risk of fixed-term contract workers using the distribution of characteristics of the permanent contract workers.[9] We reweight using 48 cells defined by interacting age groups, gender and education levels, yielding results that are depicted by the semi-dashed line in panel (c) of Figure 2. Most notably, conditioning on these basic demographics leads to an even larger DI risk premium for temporary workers; it is now almost twice as large (before 2013). Also after the reduction in the risk after 2013, the DI risk of temporary workers remains well above the risk for permanent workers. The most important explanation is that both the likelihood of securing a permanent contract and the DI risk increase with age.

[9] Reweighting is performed at the monthly level. For month t, define the population share of group j within the population of permanent workers by

$$\alpha_{tj} = \frac{\sum_i^{N_t} I_t(i \in j)}{N_t}$$

A group j is defined by an interaction of characteristics. Subscript i refers to individuals and N_t is the total number of permanent contract workers in month t. The reweighted DI-risk for fixed-term contract workers is the weighted sum of DI-risk for each group:

$$\tilde{R}_t^T = \sum_j \alpha_{tj} R_{jt}^T$$

Since the DI risk clearly correlates with health status, difference in healthiness – prior to falling ill – between temporary and permanent workers might explain the risk premium. Our large number of observations allows reweighting further using health care expenditures. We interact the earlier-defined cells with five levels of health care expenditures (measured in the calendar year prior to month $t-24$) and show reweighted DI risks as indicated by the second semi-dashed line in panel (c) of Figure 2. Surprisingly, the resulting risk is almost identical to the DI risk conditional on basic demographics. So once we control for demographics, any remaining health differences are unable to explain the higher DI risk.

3 Decomposition framework

We now turn to a formal analysis of the DI risk premium decomposition. Throughout the empirical analysis, we focus on the relative DI risk: the ratio of the probability of applying for DI with a fixed-term contract to a permanent contract. To guide the empirical analysis, we first show that the total relative DI risk can be decomposed into a weighted average of relative risks under various scenarios. These scenarios are based on the four mechanisms potentially causing the DI risk gap. We present the main findings here, while formals proofs and intermediate steps can be found in Online Appendix B.1. As a starting point, we consider the observed 'raw' relative DI risk:

$$\lambda_{raw} = \frac{P(DI|FT)}{P(DI|P)} \tag{1}$$

with FT denoting fixed-term contracts and P denoting permanent contracts. To account for selection, we condition on all observable characteristics. This corresponds to the first step in our decomposition, which results in a reweighted relative DI risk with the weights depending on the distribution of observables for those with a fixed-term contract and a permanent contract.

$$\lambda_{raw} = \alpha_x \cdot \frac{P(DI|FT,x)}{P(DI|P,x)} = \alpha_x \cdot \frac{\pi_{DI}^{FT}}{\pi_{DI}^{P}} = \alpha_x \cdot \lambda_{cond} \tag{2}$$

where α_x reflects the impact of selection on the relative risk due to differences in composition. For notational convenience, we define π_{DI}^{FT} and π_{DI}^{P} as the respective risks of applying for DI conditional on having a fixed-term or permanent contract, conditional on all observable characteristics. The corresponding relative DI risk is λ_{cond}.[10]

To analyze differentials stemming from the causal impact of contract type, we next decompose the conditional DI risk into the risk of experiencing a health shock (π_S) and the risk of applying for DI conditional on such a health shock ($\pi_{DI|S}$).[11]

$$\begin{aligned}\lambda_{cond} &= \frac{\pi_S^{FT} \cdot \pi_{DI|S}^{FT}}{\pi_S^{P} \cdot \pi_{DI|S}^{P}} \\ &= \lambda_{shock} \cdot \lambda_{DI|S} \\ &= \lambda_{shock} \cdot \tilde{\lambda}_{DI} \qquad (3)\end{aligned}$$

Note that from now on we denote probabilities and relative risks that are *conditional on health shocks* as $\tilde{\pi}$ and $\tilde{\lambda}$, respectively.

We next isolate the third contributor to the DI risk differential, which is due to differences in employer commitment. For this, we further split the conditional probabilities by whether employers are fully responsible for the worker (R) or not ($\not{R}$) during illness. Recall that the employers' responsibility for fixed-term workers during illness changed during the time period under investigation. The probability that employers are responsible for fixed-term workers is denoted by $\tilde{\pi}_R^{FT}$ (note that $\pi_R^P = 1$ and hence cancels out, see Appendix B.1).

$$\tilde{\lambda}_{DI} = \tilde{\pi}_R^{FT} \cdot \tilde{\lambda}_{DI|R} + \tilde{\pi}_{\not{R}}^{FT} \cdot \tilde{\lambda}_{DI|\not{R}} \qquad (4)$$

[10] Given that all remaining analysis is conditional on observables, we omit conditioning on observables in the notation.

[11] Note that our measures of health shocks are imperfect, and we may observe applications where we observe no prior shock ($\not{S}$). In Online Appendix Section B.1 we show that DI applications for which we observe no prior health shock drop out from λ_{cond} under the assumption that the relative risk after a shock is equal to the relative risk after no shock ($\lambda_{shock} \cdot \lambda_{DI|S} = \lambda_{no\ shock} \cdot \lambda_{DI|\not{S}}$). We test this assumption and find that it holds approximately.

As a final step in the decomposition analysis, we also split the conditional probabilities with respect to a discrete measure of outside options in the labor market. We denote good labor market prospects with L and bad prospects with $\not{L}$. The probability that labor market prospects are good is denoted by $\tilde{\pi}_L$. We assume labor market prospects are always good for permanent employees ($\tilde{\pi}_L^P = 1$ as they have an ongoing employment contract), but can be good or bad for temporary employees depending on the tightness in their sector (see Section 4.4).

$$\tilde{\lambda}_{DI} \quad = \quad \tilde{\pi}^{FT}_{R,L} \cdot \tilde{\lambda}_{DI|L,R} + \tilde{\pi}^{FT}_{\not{L},R} \cdot \tilde{\lambda}_{DI|\not{L},R} + \tilde{\pi}^{FT}_{L,\not{R}} \cdot \tilde{\lambda}_{DI|L,\not{R}} + \tilde{\pi}^{FT}_{\not{L}|\not{R}} \cdot \tilde{\lambda}_{DI|\not{L},\not{R}} \quad (5)$$

When combining (2), (3) and (5), we can summarize the full decomposition framework:

$$\begin{aligned} \lambda_{raw} \quad = \quad & \underbrace{\alpha_x}_{\text{Selection}} \cdot \underbrace{\lambda_{shock}}_{\substack{\text{Causal impact} \\ \text{on falling ill}}} \cdot \\ & \Bigg[\underbrace{\tilde{\pi}^{FT}_{R,L} \cdot \tilde{\lambda}_{DI|L,R}}_{\substack{\text{Responsible employer,} \\ \text{good prospects}}} + \underbrace{\tilde{\pi}^{FT}_{\not{L},R} \cdot \tilde{\lambda}_{DI|\not{L},R}}_{\substack{\text{Responsible employer,} \\ \text{bad prospects}}} + \\ & \underbrace{\tilde{\pi}^{FT}_{L,\not{R}} \cdot \tilde{\lambda}_{DI|L,\not{R}}}_{\substack{\text{No responsible employer,} \\ \text{good prospects}}} + \underbrace{\tilde{\pi}^{FT}_{\not{L}|\not{R}} \cdot \tilde{\lambda}_{DI|\not{L},\not{R}}}_{\substack{\text{No responsible employer,} \\ \text{bad prospects}}} \Bigg] \end{aligned} \quad (6)$$

Equation (6) shows that the observed 'raw' relative DI risk can be decomposed into the effect of selection, multiplied by the relative risk of experiencing a health shock and the relative risk of applying for DI conditional on a health shock. The latter term can in turn be written as the average of the relative DI risks in four scenario's defined by employer responsibility and labor market prospects, weighted by the relative prevalence of each scenario.

4 Empirical analysis

In what follows, we perform regression analyses to decompose the components that potentially contribute to the overall relative DI risk for fixed-term workers. In line with the sequential nature of this procedure, we first consider the role of selection into contract type in Section 4.1, building supportive evidence for the reweighting results from the previous section and estimating λ_{raw}, λ_{cond} and thereby indirectly α_x. Second, in Section 4.2 we estimate the effect of contract type on the probability of falling ill and the subsequent probability of applying for DI (λ_{shock} and $\tilde{\lambda}_{DI}$). Third, we investigate the role of the employer during the two-year waiting period ($\tilde{\lambda}_{DI|R}$ and $\tilde{\lambda}_{DI|\not{R}}$) in Section 4.3 by exploiting the 2013 reform that implemented employer responsibility for temporary workers. Finally, we estimate whether the probability of DI application depends on labor market prospects in Section 4.4 thereby completing the full decomposition as shown in Equation (6).

4.1 Selection into contract type

Empirical specification

In the first step of our analysis, our aim is to assess the extent to which observable (pre-illness) differences between temporary and permanent workers explain the DI risk premium. For this, we estimate both the observed relative DI risk (λ_{raw}) and the relative DI risk conditional on observables (λ_{cond}) using linear regression models.[12] Compared to the reweighting results that were shown earlier, we substantially extend our set of covariates. All employed individuals are included in a monthly panel. Since workers that apply for DI in a given month are no longer part of the population at risk, we only exclude their monthly observations less than 24 months prior to application. The key explanatory variable comprises a set of four employment status dummies E^j_{it}, equal to one if individual i's contract

[12]As a robustness check on our baseline model, we will replace the linear specifications with logistic regressions.

in month t equals j and is zero otherwise, where j can be: (i) a fixed-term contract, (ii) a permanent contract, (iii) unemployment benefits after a fixed-term contract or (iv) unemployment benefits after a permanent contract. We include individuals on UI benefits, as these have substantially higher DI risks. UI benefits might represent an important pathway from employment to DI, which would be ignored if we excluded this group.[13] The regression model is:

$$DI_{it} = \sum_{j}^{4} \beta_j E_{it}^{j} + \delta X_{it} + \varepsilon_{it}, \tag{7}$$

with DI_{it} as an indicator dummy that is equal to one if individual i applies for DI in period $t+24$, meaning they fell ill in month t. Differences in DI risk between the contract types are denoted by β. We sequentially add control variables X_{it} to assess to what extent they explain differences between contract types. We first add demographic controls, which we then extend with job characteristics and two-year lagged healthcare expenditures. To minimize omitted variable bias, we include all relevant cross-products by using a double LASSO specification following Belloni et al. (2014).[14] Finally, we estimate an upper bound on potential remaining bias from selection on unobservable characteristics using the methods suggested by Oster (2019).

While our data allows for the use of individual fixed effects to exploit individual variation in contract type, we choose not to do so. In that case, we would effectively lose all individuals that never apply for DI benefits, which is the vast majority. Related to this, contract type effects (β) would be identified solely from individuals that switch between fixed-term and permanent contract. This raises the concern that previous contract types may have spillover effects to current ones.[15] We therefore focus on cross-individual variation in contract type

[13]To test for the robustness of this choice, we also estimate a specification where we ignore UI spells and simply classify individuals receiving UI benefits after a fixed-term contract as fixed-term contracts and likewise for permanent contracts. Results are similar.

[14]See Appendix Section B.2 for discussion on double LASSO specification.

[15]To study the effect of switches in more detail, we will estimate a robustness specification in which the subsample of individuals who recently switched contract types are included as separate employment contract groups.

and DI risks. Following Abadie et al. (2017), we choose not to adjust standard errors for clustering, as we consider the full Dutch population.

Results

Regression estimates for the monthly DI risk cf. Equation (7) are presented in Table 2. As a reference point, we find that a fixed-term contract increases the monthly DI application risk with 0.013 percentage points, which amounts to a relative risk (λ_{raw}) of 1.3. Additional controls are included in subsequent columns. The inclusion of age, gender, nationality, education level, family composition and population density[16] as controls in column (2) *increases* the relative DI risk to 1.68. This is mainly due to age differences: younger individuals are more likely to have fixed-term contracts and less likely to apply for DI. Controlling for job characteristics (wage, working hours and sector) reduces the difference in DI risk to 43% (column (3)).

In column (4) we include 10 dummies for the level of lagged healthcare expenditures to control for differences in healthiness. The effect of lagged health on DI risks is substantial: individuals with lagged healthcare expenditures in the top 10% of the distribution have a DI risk 40 times higher than those in the lowest 10% (see Appendix Table A.2 for healthcare coefficients from the regression of column(5)). The effect of large healthcare expenditures is ten times as large as the effect of the contract type. Nevertheless, the contract type difference in DI risk remains unchanged. Selection based on health appears limited and does not explain the difference in DI risk between contract types.

Columns (6) and (7) in Table (2) show that our key regression results remain constant with more extensive sets of controls. This holds for the inclusion of 408 municipality dummies, as well as adding interactions between sectors (70) and education levels (10) that aim to control for more specific occupational characteristics that may drive disability risks. We also consider a specification that

[16] Population density is measured at the municipality level and consists of 6 categories ranging from "non-urban" (less than 500 individuals per km^2) to "very urban" (more than 2,500 individuals per km^2).

Table 2: Regression results monthly DI application risks for temporary and permanent workers

	DI application							
	(1)	(2)	(3)	(4)	(5)	(6)	$(7)^h$	$(8)^i$
Fixed-term contract coefficient	0.013	0.027	0.018	0.019	0.019	0.020	0.020	0.020
Estimated DI risk for workers with fixed-term contracts[a]	0.056	0.068	0.062	0.062	0.062	0.062	0.062	
Estimated DI risk for workers with permanent contracts[a]	0.043	0.040	0.043	0.043	0.043	0.043	0.043	
Relative risk[b]	1.30	1.68	1.43	1.45	1.45	1.45	1.45	
Robustness specifications:								
Logistic regression[c]	1.30	1.68	1.43	1.45	1.45	1.45	1.45	
DI award[d]	1.13	1.60	1.44	1.45	1.45	1.45	1.45	
Merge UI groups[e]	1.39	1.76	1.39	1.41	1.40	1.40	1.40	
Include switch groups	1.23	1.63	1.38	1.40	1.40	1.40	1.40	
Quarter-of-year dummies		X	X	X	X	X	X	
Demographic controls[f]		X	X	X	X	X	X	
Job controls[g]			X	X	X	X	X	
Lagged health controls				X	X	X	X	
Regional fixed-effects					X			
Sector x education controls						X		
All relevant cross-products							X	
Number of control dummies	0	50	133	143	546	703	672	
R^2 baseline regression	0.026	0.060	0.065	0.089	0.090	0.091	0.108	
Observations (ind.*month)	475 million							

The sample in all columns includes the full observation period (January 2010 until June 2015). All regressions include dummies for UI benefits after a fixed-term contract and UI benefits after a permanent contract (estimates not reported). All coefficients in this table are statistically significant with P-values < 0.0001. See Appendix Table A.1 for specification of control variables. (a) Average predicted DI risk if all individuals would have a fixed-term contract (and similar for permanent contracts); (b) Ratio of estimated DI risk fixed-term and estimated DI risk permanent; (c) Relative risk of logistic regression is calculated as e^{β} with β the fixed-term contract coefficient of a logistic regression; (d) Relative risk of DI award regression, (e) Relative risk of DI application regression with UI groups merged, (f) Age, gender, nationality and education level; (g) Wage, number of working hours and sector of employment, (h) Cross-products are included based on their predictive power of DI application and contract type using a double LASSO specification, see Appendix Section B.2 for details; (i) Upper bound of selection on unobservable characteristics using Oster (2019) analysis; $\beta^* = \tilde{\beta} - (\beta^0 - \tilde{\beta})\frac{1.3*\tilde{R}-\tilde{R}}{\tilde{R}-R^0}$

allows for interactions between all control variables. To balance the added value of these interactions and the risk of over-fitting, we estimate a double LASSO specification (Belloni et al. (2014)). The results, shown in column (7), yield again equal contract type effects.

To address the concern that there is selection based on unobserved characteristics, we follow the approach proposed by Oster (2019). The idea is to compute an upper bound for the contract type effect through extrapolating changes in coefficient estimates as additional covariates are added, weighted by the corresponding change in R-squared. We use the specification of column (3) as baseline and the specification of column (7) as the extended model. Given the stability of the coefficient estimates when moving from column (3) to column (7), and the strong (relative) increase in R^2, the effect of selection on unobservable characteristics is limited: the computed upper bound for the fixed-term contract coefficient is 0.020 (compared to our estimate of 0.019 in column (7).[17]

Table 2 also shows four robustness specifications that yield similar results. First, marginal contract type effects are almost identical with a Logit specification. Presumably, this reflects the fact that our large sample size allows for a sufficiently flexible specification of the linear model. As a second robustness test, we examine DI awards instead of DI applications. With similar proportional effects, we conclude that contract type has little impact on the application award probability. Third, we reclassify individuals who apply for DI benefits while receiving UI benefits.[18] This slightly increases the absolute DI risk (DI risk for UI beneficiaries is high), and slightly decreases the resulting relative risk. The decrease in relative risk is due to the fact that the DI risk is high for UI beneficiaries, irregardless of their contract type prior to entering UI. Lastly, we reclassify individuals of whom the contract type changed in the last 6 months.

[17]Note that we use the restricted estimator and R_{max} value proposed by Oster (2019); $R_{max} = 1.3\tilde{R}$. Given our large sample size, it is not computationally feasible to estimate the unrestricted estimator. Using larger R_{max} values, f.e. $R_{max} = 2\tilde{R}$ does not alter the conclusion.

[18]Individuals who receive UI benefits while their last contract was temporary, are included in the temporary contract category. Individuals who receive UI benefits while there last contract was a permanent contract are included in the permanent contract category.

More precisely, we add 2 types of employment status; (1) switched from fixed-term to permanent, and (2) switched from permanent to fixed term. The DI risk of the switch groups is very similar to the DI risk of the non-switch groups.[19] As a results, the inclusion of switch groups does not alter the relative DI risk. The gap in DI risk is thus not caused, or masked, by contract type effects which carry on after contract type switches.

The parametric estimates strengthen our findings of the reweighting exercise shown in Figure 2. Despite stark compositional differences between workers with fixed-term and permanent contracts, the DI risk premium is hardly explained by these differences. Demographic differences conceal that the risk premium is even slightly larger than observed numbers suggest, while differences in prior health have a negligible additional effect. Extrapolation based on Oster (2019) suggests that additional unobserved characteristics are unlikely to change the results substantially. We conclude that selection of relatively unhealthy workers into fixed-term contracts does not explain the high DI application risk of temporary workers.

4.2 Impact on probability of falling ill

The previous sections uncovered selection in worker and job types between contract types and found that the DI risk gap remains after correcting for compositional differences. We now decompose the DI risk gap into the impact of contract type on the relative risk of falling ill (λ_{shock}) and the subsequent relative DI risk conditional on illness ($\lambda_{DI|S}$). Contract type may impact the probability of falling ill through for example differences in occupational hazards, less prevention activities or increased stress resulting from lack of job security.

[19] Individuals who switch from fixed-term to permanent, have a similar DI risk as individuals with a permanent contract, and likewise individuals who switch from permanent to fixed-term have a similar DI risk as individuals with a fixed-term contract

Empirical specification

We regress an indicator for falling ill on contract type. For all model variants, we add the same set of controls as in the previous analyses and derive the implied relative risk of falling ill (λ_{shock}) using these regression results. Subsequently, we assess how the difference in probability of falling ill may contribute to the DI risk premium, by considering the probability of applying for DI for the subsample that has fallen ill, which yields $\lambda_{DI|S}$.

Administrative data in the Netherlands does not provide information on sick-leave of employees. We therefore proxy such an event by identifying increases in healthcare expenditure. We define a negative health shock using various thresholds. In the baseline model, we define a mental health shock as the start of a mental health treatment trajectory. A non-mental health shock is defined as an increase in annual healthcare expenditures from below the median of the population (approximately €150) to above the 90th percentile (approximately €2,400). Accordingly, the population at risk is defined as the working population without mental treatments or with healthcare expenditures below the median. This results in samples of workers that are sufficiently 'healthy' such that they can, according to our definition, experience a negative health shock. Our regression model for experiencing a negative health shock is:

$$H_{it} = \sum_{j}^{4} \beta_j^S E_{it}^j + \delta^S X_{it} + \varepsilon_{it}^S \,, \tag{8}$$

with H_{it} an indicator for individual i experiencing a negative health shock in period t. Again, our interest lies in the estimates of $\beta^{\mathbf{S}}$, which in this case captures the association between type of contract and the likelihood of experiencing a negative health shock. We sequentially add the same rich set of control variables as in Subsection 4.1, with the exception of healthcare expenditures. We thereby reduce the scope for omitted variable bias, strengthening the idea that $\beta^{\mathbf{S}}$ captures causal effects. Note that by defining our outcome as a *change* in healthiness, we essentially control for baseline health and thereby relax the required assumptions for a causal interpretation of $\beta^{\mathbf{S}}$.

We next estimate whether the DI risk *conditional* on a health shock differs by contract type. Together with the results of Equation (8), this allows us to decompose the DI risk differential into a part that is due to differences in probabilities of health shocks and a part that is due to differences in DI risks *conditional on falling ill.* Specifically, we re-estimate Equation (7) for the sub-sample of employees that experience a negative health shock. We define the month (for mental health shocks) or year (for non-mental health shocks) in which the shock occurs by t_i^* and only include observations in the 6 months before and 6 months after t_i^*.[20] Similar to our earlier analysis, the outcome variable is an indicator equal to one if a DI application is filed at time period $t+24$, and zero otherwise.

Results

Tables 3 and 4 show the regression estimates for the risk of experiencing a mental or non-mental health shock, respectively.[21] Additional controls are included in columns (2)-(6). The tables also present the implied conditional relative DI risks for both mental and non-mental health shocks. Any differences between the unconditional and conditional relative DI risk follow from differences in the probabilities of negative health shocks.

Without controlling for any characteristics, employees with fixed-term contracts have a 40% higher risk of a mental health shock (Table 3, column (1)). For a non-mental health shock their risk is 18% lower (Table 4, column 1). Both these observed differences decrease markedly when we control for demographics – as shown in column (2). When adding job controls, regional fixed effects and interacted sector and education level controls, there is little impact on the rela-

[20]The exact timing of the health shock is difficult to observe because (i) health expenditure data is annual and (ii) there may be some waiting time for certain types of health care, such that expenditures increase with some delay. To deal with these issues we also include the 6 months prior to the health shock.

[21]One might expect the 2013 reform to affect the probability of falling ill for temporary workers, but we find that results based on only the post-2013 period are very similar.

Table 3: Regression results for a negative mental health shock (> 0 minutes mental health treatment) and a subsequent DI application

	Mental health shock							DI application	
	(1)	(2)	(3)	(4)[e]	(5)	(6)[f]	(7)[g]	Unconditional	Conditional
Fixed-term contract coefficient	0.041	0.015	0.010	0.009	0.009	0.007	0.005	0.019	0.192
Estimated health shock risk temporary workers (%)[a]	0.142	0.125	0.122	0.121	0.121	0.121		0.062	0.639
Estimated health shock risk permanent workers (%)[a]	0.101	0.109	0.112	0.112	0.112	0.114		0.043	0.446
Relative risk[b]	1.40	1.14	1.09	1.08	1.08	1.06		1.45	1.43
Robustness specifications:									
Larger mental health shock (> 200 minutes)	1.56	1.19	1.10	1.08	1.08	1.06		1.45	1.41
Number of observations	286 million							475 million	4 million
Quarter-of-year dummies		X	X	X	X	X	X	X	X
Demographic controls[c]		X	X	X	X	X	X	X	X
Job controls[d]			X	X	X	X	X	X	X
Regional fixed-effects				X					
Sector x education controls					X				
All relevant cross-products						X	X		

All coefficients in this Table are statistically significant with P-values < 0.0001. See Appendix Table A.1 for specification of control variables. (a) Average predicted monthly risk of experiencing a health shock if all individuals would have a fixed-term contract (and similar for permanent contracts). Risk is reported as percentage, i.e. 0.1 means a 0.1% monthly risk of experiencing a shock; (b) Ratio of estimated probability fixed-term and estimated probability permanent; (c) Age, gender, nationality and education level; (d) Wage, number of working hours and sector of employment; (e) Based on a random subsample of 10 million observations due to computational load; (f) Cross-products are included based on their predictive power of DI application and contract type using a double LASSO specification, see Appendix Section B.2 for details; (g) Upper bound of selection on unobservable characteristics using Oster (2019) analysis; $\beta^* = \tilde{\beta} - (\beta^0 - \tilde{\beta})\frac{1.3*\tilde{R}-\tilde{R}}{\tilde{R}-R^0}$

Table 4: Regression results for a negative non-mental health shock ($> 90^{th}$ percentile) and a subsequent DI application

	Non-mental health shock							DI application	
	(1)	(2)	(3)	(4)[e]	(5)	(6)[f]	(7)[g]	Unconditional	Conditional
Fixed-term contract coefficient	-0.052	-0.004	-0.013	-0.012	-0.013	-0.012	-0.009	0.019	0.043
Estimated health shock risk temporary workers[a]	0.238	0.521	0.519	0.520	0.520	0.520		0.062	0.133
Estimated health shock risk permanent workers[a]	0.291	0.532	0.537	0.536	0.536	0.536		0.043	0.090
Relative risk[b]	0.82	0.98	0.97	0.97	0.97	0.97		1.45	1.48
Robustness specifications:									
Larger non-mental health shock ($> 99^{th}$ percentile)	0.56	1.02	0.96	0.96	0.97	0.97		1.45	1.58
Number of observations	192 million							475 million	11 million
Quarter-of-year dummies		X	X	X	X	X	X	X	X
Demographic controls[c]		X	X	X	X	X	X	X	X
Job controls[d]			X	X	X	X	X	X	X
Regional fixed-effects				X					
Sector x education controls					X				
All relevant cross-products						X	X		

All coefficients in this Table are statistically significant with P-values < 0.0001. See Appendix Table A.1 for specification of control variables. (a) Average predicted monthly risk of experiencing a health shock if all individuals would have a fixed-term contract (and similar for permanent contracts). Risk is reported as percentage, i.e. 0.1 means a 0.1% monthly risk of experiencing a shock; (b) Ratio of estimated probability fixed-term and estimated probability permanent; (c) Age, gender, nationality and education level; (d) Wage, number of working hours and sector of employment; (e) Based on a random subsample of 10 million observations due to computational load; (f) Cross-products are included based on their predictive power of DI application and contract type using a double LASSO specification, see Appendix Section B.2 for details; (g) Upper bound of selection on unobservable characteristics using Oster (2019) analysis; $\beta^* = \tilde{\beta} - (\beta^0 - \tilde{\beta})\frac{1.3*\tilde{R}-\tilde{R}}{\tilde{R}-R^0}$

tive risks. With the most extensive sets of controls, which includes all relevant cross products, we find that fixed-term contracts increase the risk of a mental health shock with 8%, while for non-mental health problems the risk is almost identical for the two contract types.[22] When computing an Oster (2019) upperbound of the contract type effect which takes into account selection based on unobservable characteristics (column (7)), the estimated contract type coefficients become even smaller. Similar results are obtained when considering larger mental or non-mental health shocks.

We next consider the probability of applying for DI conditional on a health shock ($\lambda_{DI|S}$). For comparison, Tables 3 and 4 show both the unconditional DI risk (as also reported in Table 2), and the DI risk conditional on experiencing a health shock. The estimated health shock risks after a mental health shock are, for both fixed-term and permanent contracts, approximately 10 times as large as the unconditional DI risks. This indicates that the mental health shocks that we identify are indeed strong predictors of later DI application. Comparing the two contract types, we find a risk premium for temporary workers of 43%, which is close to the unconditional risk premium (48%).

After a non-mental health shock, the DI application risk is approximately twice as high as the unconditional risks. For this group, we also find a large risk premium (50%) for temporary workers. This implies that the DI risk premium for fixed-term contracts for those who have fallen ill is, again, as high as for the unconditional risks. So any differences in the risk of falling ill between contract types is confined to mental shocks. However, the increased risk of mental health shocks does not explain the high DI risk for temporary workers. Instead, it seems that the divergence starts only *after* the onset of illness.

[22] Additionally, the evolution of health after a health shock is also similar for both contract types.

4.3 Employer incentives during the waiting period

After falling ill, employees face a two-year waiting period during which employer incentives to support reintegration differ based on contract type. Those with permanent contracts receive support from their employer, who is obliged to monitor progress and actively facilitate rehabilitation. Employers also face financial consequences if their employees enter DI through experience-rating: DI contributions depend on the DI inflow of their employees in the previous ten years. Until 2013, these employer responsibilities with respect to their *temporary* workers ended when the contract expired. As discussed in Section 2, the role of the employer during the waiting period prior to DI application became much more similar for temporary and permanent workers from 2013 onward. Using the notation we introduced in Section 3, the relative DI risk prior to 2013 corresponds to $\tilde{\lambda}_{DI|\cancel{R}}$. From 2013 onwards, the remaining relative DI risk corresponds to $\tilde{\lambda}_{DI|R}$.

Empirical specification

To identify the importance of employer incentives during the waiting period, we exploit the 2013 reform using a difference-in-difference (DiD) strategy. Since permanent workers were unaffected by the reform, we use these as the control group. To assess the validity of the parallel trends assumption, we first perform an event-study analysis – i.e. interacting the contract type dummies with the quarter of the year dummies in the regression. For the event-study analysis, we use the unconditional DI risk as this allows the inclusion of a longer time window.[23] The regression model is:

$$DI_{it} = \sum_{j}^{4} \beta_j E_{it}^{j} + \sum_{Q=2010Q1}^{2015Q2} \sum_{j}^{4} \delta_{jQ} E_{it}^{j} + \gamma X_{it} + \varepsilon_{it} \tag{9}$$

The time-constant difference in DI risk between permanent and temporary workers is captured by β. Differential time trends (by quarter) for contract type, denoted by δ_{jQ}, constitute our parameters of interest.

[23] Only a short period of time around health shocks is used in the conditional DI regressions.

After establishing common trends, we incorporate the 2013 reform in the conditional DI risk regression by interacting the contract type dummies with a dummy for post-2013 observations. As a result, the specification corresponds to a conventional DiD model with temporary workers the treatment group and permanent workers (for whom nothing changed after 2013) the control group. This DiD model allows estimating $\tilde{\lambda}_{DI|R}$ and $\tilde{\lambda}_{DI|\not{R}}$. At this point, it is important to stress that we estimate the DiD model only on samples of workers that have experienced a negative health shock. The idea behind this is that the 2013 reform primarily aimed to increase employer commitment to their temporary workers *during illness*.

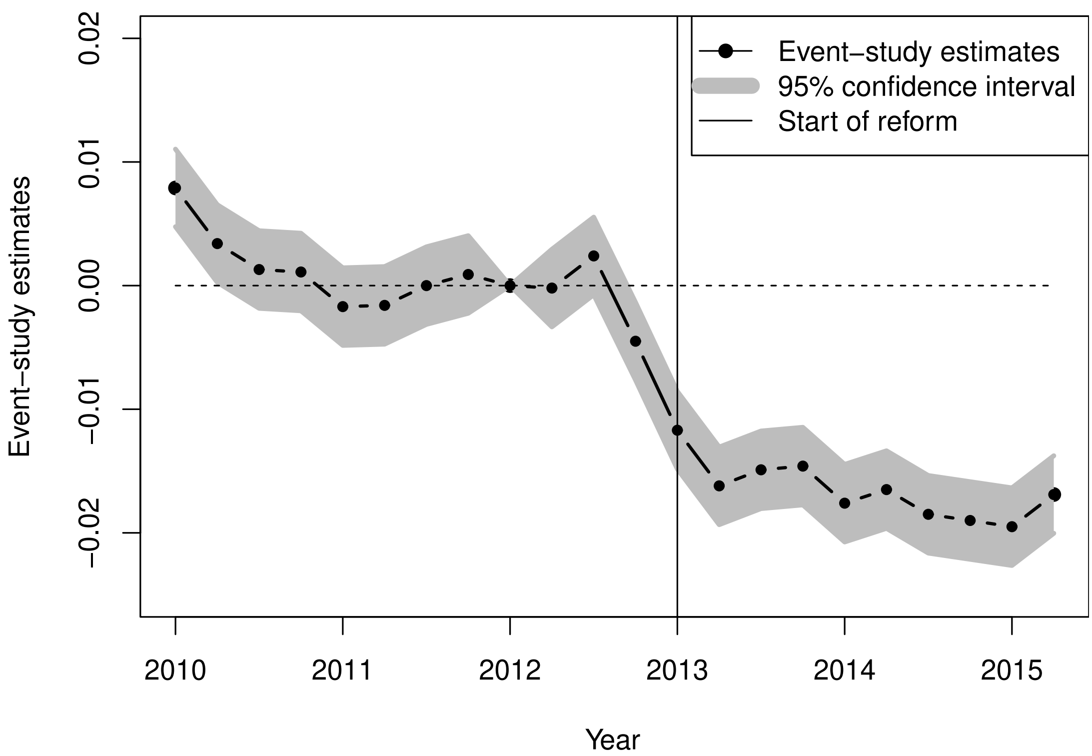
Results

Figure 3 shows the event-study estimates for δ_{jQ} (with j = fixed-term contracts), in which the first quarter of 2012 is used as baseline. Prior to the 2013 reform, the difference in DI risk between permanent and temporary workers is fairly constant over time, implying that the parallel trends assumption holds. Only in the last quarter of 2012 we see that the DI risk for temporary workers decreases substantially relative to permanent workers. This is likely due to anticipation of employers given that inflow into DI over the years prior to 2013 is used to determine DI insurance contributions in 2013. The gap in DI risk decreases further in the first two quarters of 2013 and remains constant afterwards. Further analysis, discussed in Appendix Section A.3, shows that the majority of the reform effect can be attributed to increased monitoring, while the effect of experience rating appears to be limited.[24]

Based on our DiD estimation results, Table 5 shows the estimated conditional DI risks before and after 2013. In contrast to our earlier decomposition stages,

[24]The 2013 reform simultaneously introduced extra financial incentives and monitoring obligations for employers. We can extend our DiD model and exploit the fact that incentive effects were proportional to firm size. When doing so, we can disentangle the importance of the various elements – as shown in Appendix Section A.3. Increased monitoring and the one-year assessment account for approximately 80% of the total effect, whereas the introduction of experience rating accounts for approximately 20%.

Figure 3: Event-study estimates for the 2013 reform for fixed-term contracts



we now explain a substantial part of the observed relative DI risk. For both mental and non-mental health shocks, the DI risk premium is almost halved: the relative risk drops from 1.59 to 1.34 for those with mental health shocks and from 1.63 to 1.27 for those with non-mental health shocks. Accordingly, differences in the employer incentives and obligations during the waiting period that pertained prior to 2013 explain a substantial part of the gap in DI risk.[25,26] As this point, it should be stressed once more that the 2013 reform did not fully offset the initial difference in employer incentives and obligations that existed until then. The estimated reform effect therefore provides a lower bound of the total potential importance of employer incentives and obligations.

[25]Note that the drop in the conditional DI risk for individuals with a permanent contract who experienced a non-mental health shock – as reported in Table 5 – reflects the effect of right censoring. Individuals who experience a health shock prior to 2013 are often also in the risk sample after 2013 due to the uncertainty about the timing of the shock, but the reverse does not hold.

[26]Prinz & Ravesteijn (2020) reach a similar conclusion regarding the impact of the reform on temporary agency workers, which concerns a (small) subset of all temporary workers.

Table 5: Estimation results difference-in-differences model for DI application conditional on a health shock

Panel A: DI application conditional on a mental health shock (> 0 minutes)		
	Pre 2013	Post 2013
Estimated DI risk fixed-term[a]	0.725	0.591
Estimated DI risk permanent[a]	0.455	0.442
Relative risk[b]	1.60	1.34
Robustness specifications:		
Larger mental health shock (> 200 minutes)	1.57	1.34
DI award	1.55	1.45
Number of observations	1.5 million	2.7 million

Panel B: DI application conditional on a non-mental health shock (> 90^{th} percentile)		
	Pre 2013	Post 2013
Estimated probability fixed-term[a]	0.161	0.100
Estimated probability permanent[a]	0.099	0.079
Relative risk[b]	1.63	1.27
Robustness specifications:		
Larger non-mental health shock (> 99^{th} percentile)	1.71	1.36
DI award	1.53	1.41
Number of observations	6.2 million	4.7 million

Regressions contain the same control variables as the regression of Table 2, column 4: quarter-of-year dummies, demographic and job controls. See Appendix Table A.1 for a specification of control variables. (a) Average estimated monthly risk of applying for DI if all individuals would have a fixed-term contract (and similar for permanent contracts); (b) Ratio of estimated DI risk fixed-term and estimated DI risk permanent.

4.4 Labor market prospects of ill employees

Having considered the risk of falling ill and the role of the employer during illness, we now turn to the final stage at which DI risk differential might open up: the decision whether or not to apply for DI after two years of illness. Temporary workers that have been ill for up to two years, during which their employment contract ended, face very different labor market prospects than ill workers with a permanent contract. As has been argued in the literature, such differences in outside options may well explain the higher propensity to apply for DI benefits for vulnerable groups in the labor market, such as those with fixed-term contracts (Autor & Duggan, 2003).

Empirical specification

To assess the importance of labor market prospects of ill employees, we consider sector-level labor market tightness as a proxy for labor market prospects of ill workers. Labor market prospects are by definition worse for ill workers whose contract has expired, but in tight labor markets the difference in prospects with workers with a permanent contract is likely to be smaller. We categorize 70 sectors as "tight" or "loose" based on the percentage of vacancies relative to the number of filled jobs (see Appendix Section B.3 for the categorization and the distribution of contract type over the sectors). Approximately 15% of all employment contracts are classified as being in a tight labor market.[27]

The categorization of tight and loose labor markets is incorporated in the DiD specification of the previous subsection. Accordingly, we allow for differential treatment effects of the 2013 reform in tight and loose labor markets. This requires that labor market tightness should evolve similarly over time in tight and loose labor markets. In Appendix Section B.3, we find similar overall trends indeed. The DiD analyses provide an estimate of how the risk of proceeding to a DI application after illness differs by labor market tightness before and after the 2013 reform.

Results

Table 6 shows the average estimated DI risks conditional on health shocks, which are now also stratified with respect to the type of labor market ("tight" or "loose"). As a result we obtain estimated risks for sets of workers with a health shock, the same contract type, the same degree of labor market tightness and measured before and after 2013.

To start with, the estimated conditional DI risks for permanent workers who experienced a mental or non-mental health shock are fairly similar before and after the reform. In both cases, labor market tightness seems more or less irrel-

[27]The distribution of contract type in tight sectors is comparable to the distribution in loose labor markets, see Appendix Section B.3

Table 6: Regression results for DI application conditional on a negative health shock and stratified by tight and loose labor market sectors

Panel A: Mental health shock (> 0 minutes)				
	Pre 2013		Post 2013	
Labor market tightness[a]	Loose	Tight	Loose	Tight
Estimated DI risk fixed-term[b]	0.763	0.646	0.611	0.494
Estimated DI risk permanent[b]	0.448	0.467	0.432	0.451
Relative risk[c]	1.70	1.38	1.42	1.10
Robustness specifications:				
Larger mental health shock (> 200 minutes)	1.67	1.33	1.41	1.08
DI award	1.67	1.28	1.54	1.16
Number of observations	1,326,962	186,101	2,347,014	319,721

Panel A: Non-Mental health shock (> 90^{th} percentile)				
	Pre 2013		Post 2013	
Labor market tightness[a]	Loose	Tight	Loose	Tight
Estimated DI risk fixed-term[b]	0.177	0.138	0.110	0.070
Estimated DI risk permanent[b]	0.097	0.094	0.076	0.072
Relative risk[c]	1.82	1.47	1.45	0.98
Robustness specifications:				
Larger non-mental health shock (> 99^{th} percentile)	1.93	1.43	1.55	1.00
DI award	1.75	1.16	1.61	0.92
Number of observations	5,551,666	607,829	4,241,347	456,050

Regressions contain the same control variables as the regression of Table 2, column 4: quarter-of-year dummies, demographic and job controls. See Appendix Table A.1 for specification of control variables. (a) Average predicted probability of applying for DI if all individuals would work in a sector with tight/loose labor market tightness before/after 2013; (b) Average estimated DI risk if all individuals would have a fixed-term contract (and similar for permanent contracts); (c) Ratio of estimated probability fixed-term and estimated probability permanent.

evant for the DI application decision. This lends credence to the idea that the existence of an employment contract – and corresponding employer commitment – renders alternative labor market opportunities unimportant for the decision to apply for DI.

Also in line with expectations, we find that the DI application probability is generally higher for temporary workers than for permanent workers, and this gap shrinks after 2013 regardless of labor market tightness. More strikingly, the gap between permanent and temporary workers is substantially smaller in tight sectors than in loose sectors. This is the case both before and after 2013. For example, before 2013 for those with mental health shocks, the relative DI risk is 1.70 in loose sectors and only 1.38 in tight sectors. Once we consider tight sectors in the post 2013 period, the relative DI risk of temporary workers decreases to only 1.10 in cases of mental health shocks and to 0.98 in cases with non-mental health shocks.[28] These findings are in line with the hypothesis that poor labor market prospects contribute substantially to the increased propensity to apply for DI of temporary workers.

5 Findings in perspective

We have investigated four potential mechanisms that may explain the increased risk of applying for DI for individuals with fixed-term contracts. In what follows, we will summarize these findings and decompose the raw relative DI risk into the relative risk in various scenario's, using the framework as explained in Section 3. We relate these findings to earlier findings in the literature. The implied contributions to the observed DI risk premium are visualized in Figure 4. In this figure, factors below the 0–% line decrease the relative DI risk of temporary workers, while those above increase the relative risk.

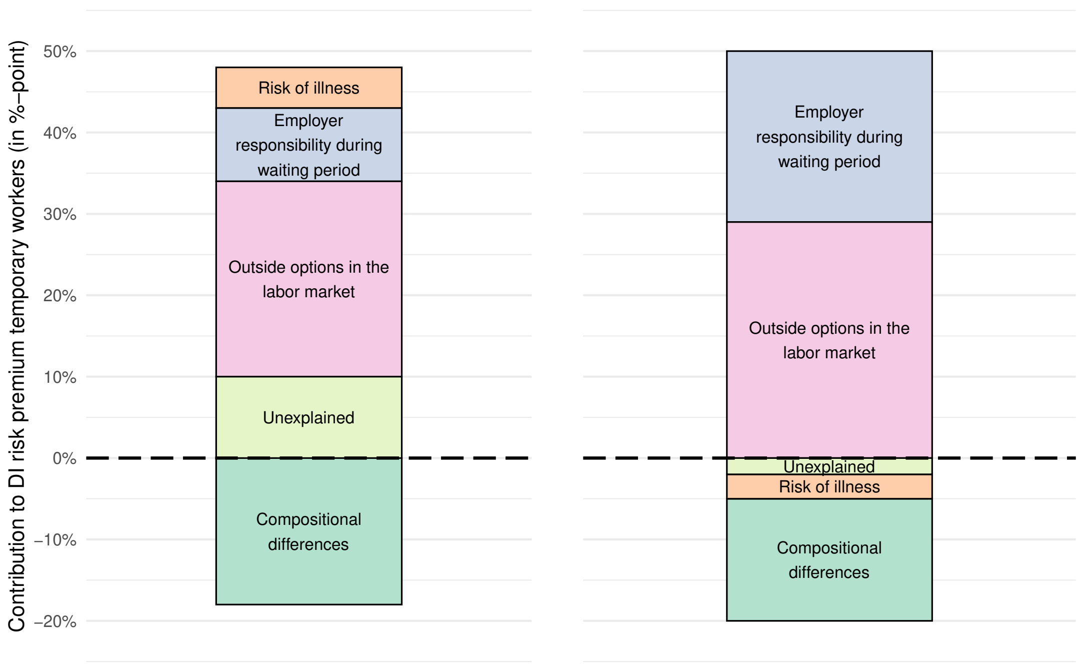
As a reference point, the observed 'raw' relative DI risk (λ_{raw}) equals 1.30, which implies that over the time period we consider, individuals with a fixed-term contract are 30% more likely to apply for DI benefits compared to permanent workers. In the figure, this percentage corresponds to the sum of the positive and negative factors. Once we correct for compositional differences the relative DI risk increases to 1.48 (λ_{cond}). In the absence of any compositional differences, the gap in the relative DI risk would thus be substantially higher than the observed relative risk. This is visualized in Figure 4 by the negative dark green blocks.

Considering the probability of falling ill, we distinguish between mental and non-mental health shocks. The risk of experiencing a mental health shock is 10% higher for temporary workers, while DI risk conditional on experiencing

[28]It should be noted that this exercise yields a lower bound for the impact of labor market prospects: even in sectors with tight labor markets, prospects are by definition a little better for individuals with a permanent contract than for individuals whose fixed-term contract has expired.

Figure 4: Decomposition of relative DI risk

(a) Mental health (b) Non-mental health



such a health shock, is still 43% higher for temporary workers. Contract type has a negligible effect on the risk of experiencing a non-mental health shock and consequentially the DI risk premium for temporary workers remains fairly constant when conditioning on experiencing a non-mental health shock.

The role of the employer *after* the onset of illness is substantial: especially for non-mental health conditions the gap in DI risk decreases strongly after the reform in 2013 which equalized monitoring and employer financial incentives during the two-year waiting period. While somewhat smaller for mental health, employer incentives still explain a substantial share of the DI risk premium for temporary workers.

Lastly, workers with fixed-term contracts appear more susceptive to labor market conditions than permanent workers when considering a DI application. The gap in DI risk decreases strongly when we consider only workers in tight labor markets. Once we shut down all four mechanisms by comparing workers with similar characteristics with a health shock after 2013 in a tight labor market, the gap in DI risk almost disappears. As is visualized by Figure 4 by the blue and purple blocks, it is mainly employer responsibilities during illness and differences in labor market prospects that increase the relative DI risk, respectively. The remaining unexplained difference in DI risk (light green) is small.

One important takeaway from our analysis is that employers do not offer fixed-term contracts specifically to workers with health conditions. This contrasts to previous research on selection into contract type, which finds that individuals in ill health are less likely to obtain a permanent contract (Wagenaar et al., 2012). In a broader perspective, there is also evidence from the US that the extra employer responsibilities inherent with the Americans with Disabilities Act (ADA) lowered the chances of disabled workers to be contracted anyway (Acemoglu & Angrist, 2001). In our setting, however, the a priori health conditions of workers are probably largely unobserved by employers. Given that our health proxies are strongly predictive of DI applications, one could therefore argue that employers have limited ability to select on more severe health issues which could lead to a DI application. Alongside this argument, it is important to stress that fixed-term contracts are often used for younger workers as a potential stepping-stone into permanent employment.

In line with other research, we do find that the use of fixed-term employment contracts increases the prevalence of mental health problems (Kim et al., 2012; Virtanen et al., 2005; Benach et al., 2014). Still, the causal effect is relatively small compared to the large *associations* found both in this paper and previous papers. We find that the raw difference in the likelihood of a mental health shock between temporary and permanent workers is 40%, but the gap decreases to only 10% when controlling for a wide range of pre-illness job and worker characteristics. The difference in prevalence of non-mental health problems even completely disappears when controlling for these observables. What this suggests is a role for selection – particularly on age – in explaining differences in the likelihood of health shocks as well as the use of contract types. Arguably, this type of selection may also play a role in the positive associations found in other papers. Our results are probably most comparable to those of Caroli & Godard (2016), who find a strong association between job insecurity and a wide range of health outcomes. When controlling for endogeneity with instrumental variables, however, only the prevalence of mental health problems appears to be affected.

Turning to the waiting period that precedes potential DI applications, we find that differences in employer incentives explain almost half of the gap in DI risk. This confirms earlier work on the effects of employer experience rating, which suggests reductions of DI inflow by 7 to 24% (Prinz & Ravesteijn, 2020; Koning, 2009, 2016; Hawkins & Simola, 2020). Our estimated effect of the 2013 reform is of a similar magnitude, but it should be stressed that in addition to experience rating, the formal monitoring obligations of employers were increased as well. Also note that experience rating was already in place for permanent contracts in 2013, whereas most other papers evaluate the introduction of experience rating in a context in which no experience rating exists.

Finally, our results shed new light on the concept of 'work disability' to explain changes in DI inflow risks. In the literature, work disability is commonly defined as the extra inflow into DI schemes induced by unfavorable business cycle conditions (Autor & Duggan, 2003; Autor, 2011; Benítez-Silva et al., 2010). This presumes that changes in DI inflow are driven by economic conditions and not by changes in health conditions, and that marginal applicants are predominantly low-productive workers that are also more susceptive to inflow into UI. Consistent with this, our analysis shows that DI application risks are higher for low-productive workers that are also more likely to have fixed-term contracts than permanent contracts. But other than that, health conditions are certainly not irrelevant. Specifically, the DI risk premium of fixed-term contracts originates from the interacted effect of health conditions and economic conditions. Health conditions are more likely to lead to DI applications for temporary workers since there is less employer commitment during illness and there are less favorable outside options.

6 Conclusion

The aim of this paper is to explain and decompose the large DI risk differential between workers with fixed-term and permanent contracts. Using rich Dutch administrative data from various sources, we show that compositional differences between temporary and permanent workers cannot explain the DI risk differential. Also the risk of falling ill is not substantially higher for temporary workers. We observe that most of the gap in DI risk arises after the onset of illness, and we show that the role of the employer during the waiting period is crucial. Increased employer responsibility for ill temporary workers in 2013 reduced the DI risk premium substantially. Finally, we find that opportunities in the labor market also matter for the decision to apply for DI. Conditional on illness, the probability to apply for DI increases for temporary workers if labor market prospects worsen. For permanent workers we find no such link. Jointly, these factors explain more than 80% of the DI risk differential between temporary and permanent workers.

From the perspective of policy, a key takeaway from our analysis is that the DI risk premium of fixed-term workers widens in the waiting period that precedes possible DI applications. In this period, a crucial role is featured by employers that may or may not e.g. implement work accommodations or organize therapeutic work. Depending on contract type and corresponding employer incentives and obligations, workers with similar health conditions face DI application risks that vary substantially. This provides a novel perspective on the concept of 'work disability': the economic context and corresponding contract settings do matter, but this is only relevant at the onset of health conditions. While this calls for sufficient commitment from employers of workers with fixed-term contracts, we are aware that the options to do so are more limited than for permanent workers. In particular, increased obligations and incentives for employers may have a downward effect on overall employment and could trigger substitution towards the UI scheme and social assistance. In addition, public employment offices might therefore play a more active role in supporting temporary workers during their waiting period. Our findings suggest that this is especially relevant in slack labor markets where opportunities for temporary workers are limited.

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A Appendix

A.1 Full set of control variables

Table A.1: Full set of control variables for regressions

Control variable	Values
Quarter-of-the-year controls	22 quarter-of-year dummies
Demographic controls:	
Gender	Male or Female
Age	≤ 24, 5 year age groups from 25-59, ≥ 60
Education	Education level split into 10 categories
Nationality	Dutch or Non-dutch
Family composition	Single / non-single and and 0/1/2/3+ kids
Population density of municipality	<500, 500–1000, 1000–1500, 1500–2500, >2500
Job controls:	
Monthly wage	≤ 1000, 500 euro brackets up to 5000, ≥ 5000
Weekly number of working hours	10 hour brackets from 0–40, ≥ 40
Sector of employment	70 sector dummies
Health controls:	
Health cost last year	10 dummies based on cost deciles
Health cost current year	10 dummies based on cost deciles
Health cost next year	10 dummies based on cost deciles

A.2 Additional empirical results

Table A.2: Healthcare cost coefficients DI risk regression (Table 2, column (5))

Healthcare cost decile	Coefficient
10%–20%	0.0038
20%–30%	0.0075
30%–40%	0.0119
40%–50%	0.0165
50%–60%	0.0223
60%–70%	0.0285
70%–80%	0.0434
80%–90%	0.0686
90%–100%	0.1279
Missing	0.0153

All coefficients in this Table are statistically significant with P-values < 0.0001.
The number of individual-year observations equals 475 million.

A.3 The 2013 reform: monitoring and experience rating

The disability reform of 2013 encompassed two major changes to the DI system. First, the reform increased monitoring and introduced an assessment after one year of illness for all ill-listed workers with a temporary contract. Second, the reform introduced experience rating for the same group of workers, making employers financially responsible for all their previous employees that have entered DI in the last two years – so also those employees no longer employed at the claims assessment. The impact of experience rating varies by firm size. Small firms with less than 10 employees pay a sector-level premium, whereas firms with more than 100 employees pay an individual premium that is fully based on their lagged DI inflow. Firms with 10 to 100 employees pay a weighted average of the sector-level premium and an individual premium (the individual weight increases from 0 to 100%). By exploiting the differential experience rating effects across observed firm size, we intend to disentangle the total effect of the reform into the effect of increased monitoring – which we assume equal across firm size – and the effect of experience rating.

Specifically, the effect of increased monitoring is estimated by comparing employees with permanent contracts to employees with temporary contracts at small firms.[29] To assess the validity of this approach, we first conduct an event study that assesses the parallel trend prior to the reform and also to evaluate the dynamic reform effects. Figure A.1 shows the quarterly estimates in which the first quarter of 2012 is used as baseline. The general picture is comparable to the event study performed in Section 4.3. That is, prior to the 2013 reform the gap in DI risk is constant over time, lending credence to the parallel trends assumption. The gap in DI risk decreases slightly in the last quarter of 2012, and continues to decrease in 2013 and 2014. Note also that the magnitude of the estimated DiD effects are very similar to the DiD effects estimated in Section 4.3. The effect of increased monitoring is approximately –0.014%-points, almost

[29]To make the treatment and control group more comparable, we could also compare employees with permanent contracts at small firms to employees with temporary contracts at small firms. This yields very similar results.

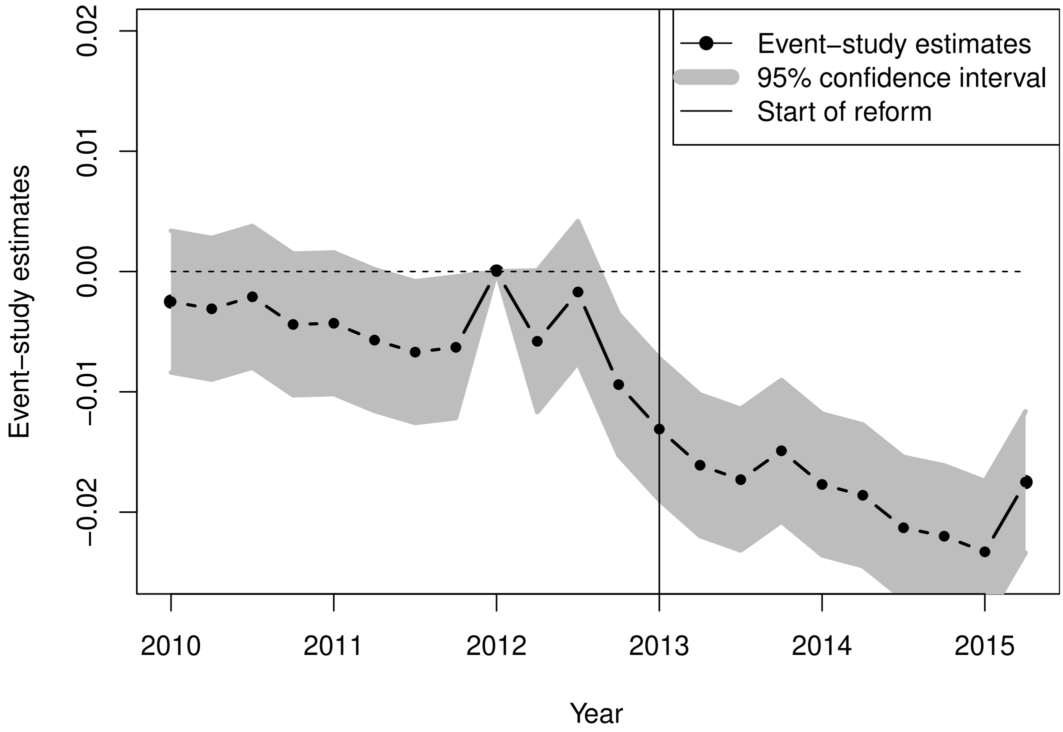


Figure A.1: Event study analysis on the effect of increased monitoring

equal to the full effect of the reform (the full effect of the reform equals –0.016%-points).[30] This suggests limited effects of the introduction of experience rating for temporary workers.

To estimate the effect of experience rating, we compare employees with a temporary contracts at small firms to employees with a temporary contract at large firms. For all of these employees, the reform increased monitoring and introduced the one-year assessment. However, experience rating was only introduced for employees at large firms and not for employees at small firms.[31] Figure

[30]The total effect is obtained by estimating a standard difference-ind-difference regression; we interact employment status with a post-2013 dummy.

[31]Note that we estimate the effect of introducing experience rating conditional on increased monitoring.

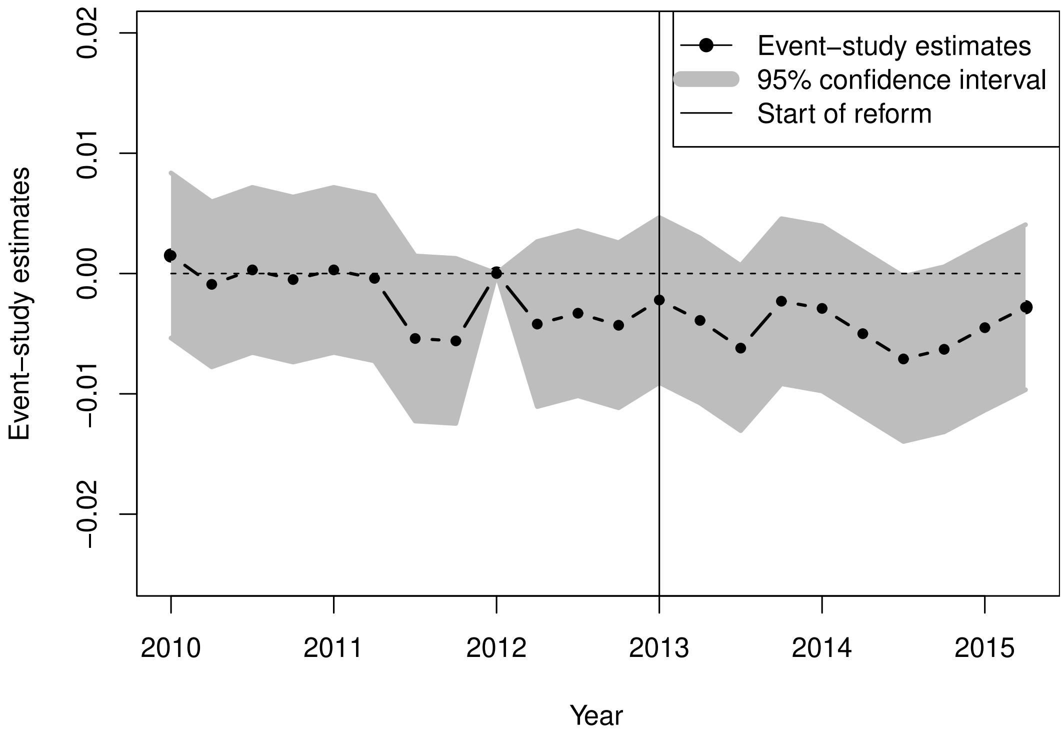


Figure A.2: Event study analysis on the effect of experience rating

A.2 shows the quarterly event-study estimates. Once again, prior to the reform the parallel trends assumption seems to hold. However, also after the reform, the event-study estimates are small and insignificantly different from zero. This indicates that introducing experience rating, on top of increased monitoring, has a limited effect. When using a single post 2013 dummy, we find a borderline significant effect of –0.002%. This implies that experience rating explains at most 10% of the total effect of the reform.

Contrasting to most of the literature, our results point at small effects of experience rating. One potential explanation for this is that the reform introduced experience rating for employees with temporary contracts at large firms only, while experience rating was already in place for employees with permanent contracts at these firms. These large firms might already have implemented return-to-work activities without discriminating between employees with permanent and temporary contracts. In addition, disentangling the impacts of the elements of the reform requires the additional assumption to hold that the effect of increased monitoring and increased financial incentives are independent. This assumption is necessary to draw the conclusion that monitoring *without* experience rating would have been almost equally effective. Given these limitations, we focus on the aggregate impact of the reform in the analysis in the main paper.

B Online Appendix

B.1 Decomposition of the relative DI application risk

The "raw" or observed relative risk of applying for DI equals:

$$\lambda_{raw} = \frac{Pr(DI|FT)}{Pr(DI|P)}$$

Using Bayes' theorem for conditional probabilities, this expression can be decomposed into selection effects multiplied by the conditional relative risk:

$$\begin{aligned}
\lambda_{raw} &= \frac{Pr(DI|FT)}{Pr(DI|P)} \\
&= \frac{Pr(DI|FT,x) \cdot \frac{Pr(x)}{Pr(x|DI,FT)}}{Pr(DI|P,x) \cdot \frac{Pr(x)}{Pr(x|DI,P)}} \\
&= \frac{Pr(DI|FT,x) \cdot Pr(x|DI,P)}{Pr(DI|P,x) \cdot Pr(x|DI,FT)} \\
&= \frac{\pi_{DI}^{FT} \cdot Pr(x|DI,P)}{\pi_{DI}^{P} \cdot Pr(x|DI,FT)} \\
&= \frac{\pi_{DI}^{FT}}{\pi_{DI}^{P}} \cdot \alpha_x = \lambda_{cond} \cdot \alpha_x,
\end{aligned}$$

with π_{DI}^{FT} and π_{DI}^{P} the probabilities of applying for DI conditional on all observables for fixed-term and permanent contracts and λ_{cond} the corresponding relative risk. α_x denotes the impact of selection due to compositional differences.

We next decompose the relative DI risk into the risk of a health shock (S) and the risk of applying for DI conditional on this specific health shock. Note that our measure of shocks is imperfect, and we have applications where we observe no prior shock($\not{S}$). As a result we obtain:

$$\pi_{DI}^{FT} = \pi_{S}^{FT} \cdot \pi_{DI|S}^{FT} + \pi_{\not{S}}^{FT} \cdot \pi_{DI|\not{S}}^{FT} \tag{B.1}$$

$$\pi_{DI}^{P} = \pi_{S}^{P} \cdot \pi_{DI|S}^{P} + \pi_{\not{S}}^{P} \cdot \pi_{DI|\not{S}}^{P} \tag{B.2}$$

$$\lambda_{cond} = \frac{\pi_{S}^{FT} \cdot \pi_{DI|S}^{FT} + \pi_{\not{S}}^{FT} \cdot \pi_{DI|\not{S}}^{FT}}{\pi_{S}^{P} \cdot \pi_{DI|S}^{P} + \pi_{\not{S}}^{P} \cdot \pi_{DI|\not{S}}^{P}} \tag{B.3}$$

The relative risk of a health shock for temporary workers is defined as:

$$\lambda_{shock} = \frac{\pi_S^{FT}}{\pi_S^P}, \tag{B.4}$$

and the relative DI risk conditional on a health shock for temporary workers is:

$$\lambda_{DI|S} = \frac{\pi_{DI|S}^{FT}}{\pi_{DI|S}^P}. \tag{B.5}$$

With our data, we obtain estimates for λ_{cond}, λ_{shock} and $\lambda_{DI|S}$. If there are *no applications without a prior health* shock, the second part of (B.3) drops out and we can write:

$$\lambda_{cond} = \lambda_{shock} \cdot \lambda_{DI|S} \tag{B.6}$$

If there are applications without a prior health shock, obtaining λ_{cond} requires the relative risk of applying after a health shock to equal the relative risk of applying after no health shock. We refer to this as the "equal relative risk assumption":

$$\begin{aligned} \lambda_{shock} \cdot \lambda_{DI|S} &= \lambda_{no\ shock} \cdot \lambda_{DI|\not S} \\ \frac{\pi_S^{FT} \cdot \pi_{DI|S}^{FT}}{\pi_S^P \cdot \pi_{DI|S}^P} &= \frac{\pi_{\not S}^{FT} \cdot \pi_{DI|\not S}^{FT}}{\pi_{\not S}^P \cdot \pi_{DI|\not S}^P} \end{aligned} \tag{B.7}$$

which can also be written as:

$$\frac{\pi_S^{FT} \cdot \pi_{DI|S}^{FT}}{\pi_{\not S}^{FT} \cdot \pi_{DI|\not S}^{FT}} = \frac{\pi_S^P \cdot \pi_{DI|S}^P}{\pi_{\not S}^P \cdot \pi_{DI|\not S}^P} = \beta, \tag{B.8}$$

with β as a constant. Rewriting the equal relative risk assumption and substituting it into (B.3) gives:

$$\pi_{\cancel{S}}^{FT} \cdot \pi_{DI|\cancel{S}}^{FT} = \beta \cdot \pi_{S}^{FT} \cdot \pi_{DI|S}^{FT}$$
$$\pi_{\cancel{S}}^{P} \cdot \pi_{DI|\cancel{S}}^{P} = \beta \cdot \pi_{S}^{P} \cdot \pi_{DI|S}^{P}$$

$$\begin{aligned}
\lambda_{cond} &= \frac{\pi_{S}^{FT} \cdot \pi_{DI|S}^{FT} + \pi_{\cancel{S}}^{FT} \cdot \pi_{DI|\cancel{S}}^{FT}}{\pi_{S}^{P} \cdot \pi_{DI|S}^{P} + \pi_{\cancel{S}}^{P} \cdot \pi_{DI|\cancel{S}}^{P}} \\
&= \frac{\pi_{S}^{FT} \cdot \pi_{DI|S}^{FT} + \alpha \cdot \pi_{S}^{FT} \cdot \pi_{DI|S}^{FT}}{\pi_{S}^{P} \cdot \pi_{DI|S}^{P} + \alpha \cdot \pi_{S}^{P} \cdot \pi_{DI|S}^{P}} \\
&= \frac{(1+\alpha) \cdot \pi_{S}^{FT} \cdot \pi_{DI|S}^{FT}}{(1+\alpha) \cdot \pi_{S}^{P} \cdot \pi_{DI|S}^{P}} \\
&= \frac{\pi_{S}^{FT} \cdot \pi_{DI|S}^{FT}}{\pi_{S}^{P} \cdot \pi_{DI|S}^{P}} \\
&= \lambda_{shock} \cdot \lambda_{DI|S}
\end{aligned}$$

Since we focus on probabilities that are conditional on health shocks in the remainder, we denote the above probabilities and relative risks as $\tilde{\pi}$ and $\tilde{\lambda}$. The next step is to further split the conditional probabilities by whether employers are responsible (R) or not ($\cancel{R}$). The probability that employers are responsible is denoted by π_R. We assume that employers are always responsible for permanent employees, and responsible for temporary employees only after the 2013 reform. We then derive the following expression:

$$\begin{aligned}
\tilde{\lambda}_{DI} &= \frac{\tilde{\pi}_{DI}^{FT}}{\tilde{\pi}_{DI}^{P}} \\
&= \frac{\tilde{\pi}_{R}^{FT} \cdot \tilde{\pi}_{DI|R}^{FT} + \tilde{\pi}_{\cancel{R}}^{FT} \cdot \tilde{\pi}_{DI|\cancel{R}}^{FT}}{\tilde{\pi}_{R|P} \cdot \tilde{\pi}_{DI|R}^{P} + \tilde{\pi}_{\cancel{R}}^{P} \cdot \tilde{\pi}_{DI|\cancel{R}}^{P}} \\
&= \frac{\tilde{\pi}_{R}^{FT} \cdot \tilde{\pi}_{DI|R}^{FT} + \tilde{\pi}_{\cancel{R}}^{FT} \cdot \tilde{\pi}_{DI|\cancel{R}}^{FT}}{1 \cdot \tilde{\pi}_{DI|R}^{P}} \\
&= \frac{\tilde{\pi}_{R}^{FT} \cdot \tilde{\pi}_{DI|R}^{FT}}{\tilde{\pi}_{DI|R}^{P}} + \frac{\tilde{\pi}_{\cancel{R}}^{FT} \cdot \tilde{\pi}_{DI|\cancel{R}}^{FT}}{\tilde{\pi}_{DI|R}^{P}} \\
&= \tilde{\pi}_{R}^{FT} \cdot \tilde{\lambda}_{DI|R} + \tilde{\pi}_{\cancel{R}}^{FT} \cdot \tilde{\lambda}_{DI|\cancel{R}},
\end{aligned} \tag{B.9}$$

which implies that we can decompose $\tilde{\lambda}_{DI}$ as a weighted average of the relative risk before and after the 2013 reform. The weights are the probabilities of being in the pre-reform or post-reform period.

The final step of our decomposition is to further split the conditional probabilities using an indicator whether labor market prospects are good (L=1) or bad ($L = 0$). Again, the probability that labor market prospects are good is denoted by $\tilde{\pi}_L$. We assume labor market prospects are always good for permanent employees, but can be good or bad for temporary employees:

$$\begin{aligned}
\tilde{\lambda}_{DI|R} &= \frac{\tilde{\pi}^{FT}_{DI|R}}{\tilde{\pi}^{P}_{DI|R}} \\
&= \frac{\tilde{\pi}^{FT}_{L|R} \cdot \tilde{\pi}^{FT}_{DI|L,R} + \tilde{\pi}^{FT}_{\not{L}|R} \cdot \tilde{\pi}^{FT}_{DI|\not{L},R}}{\tilde{\pi}^{P}_{L|R} \cdot \tilde{\pi}^{P}_{DI|L,R} + \tilde{\pi}^{P}_{\not{L}|R} \cdot \tilde{\pi}^{P}_{DI|\not{L},R}} \\
&= \frac{\tilde{\pi}^{FT}_{L|R} \cdot \tilde{\pi}^{FT}_{DI|L,R} + \tilde{\pi}^{FT}_{\not{L}|R} \cdot \tilde{\pi}^{FT}_{DI|\not{L},R}}{1 \cdot \tilde{\pi}^{P}_{DI|L,R}} \\
&= \frac{\tilde{\pi}^{FT}_{L|R} \cdot \tilde{\pi}^{FT}_{DI|L,R}}{\tilde{\pi}^{P}_{DI|L,R}} + \frac{\tilde{\pi}^{FT}_{\not{L}|R} \cdot \tilde{\pi}^{FT}_{DI|\not{L},R}}{\tilde{\pi}^{P}_{DI|L,R}} \\
&= \tilde{\pi}^{FT}_{L|R} \cdot \tilde{\lambda}_{DI|L,R} + \tilde{\pi}^{FT}_{\not{L}|R} \cdot \tilde{\lambda}_{DI|\not{L},R}.
\end{aligned} \tag{B.10}$$

Similarly, for $R = 0$ we obtain:

$$\begin{aligned}
\tilde{\lambda}_{DI|\not{R}} &= \frac{\tilde{\pi}^{FT}_{DI|\not{R}}}{\tilde{\pi}^{P}_{DI|\not{R}}} \\
&= \tilde{\pi}^{FT}_{L|\not{R}} \cdot \tilde{\lambda}_{DI|L,\not{R}} + \tilde{\pi}^{FT}_{\not{L}|\not{R}} \cdot \tilde{\lambda}_{DI|\not{L},\not{R}}.
\end{aligned} \tag{B.11}$$

Combining the two expressions then gives:

$$\begin{aligned}
\tilde{\lambda}_{DI} &= \tilde{\pi}^{FT}_{R} \cdot \tilde{\lambda}_{DI|R} + \tilde{\pi}^{FT}_{\not{R}} \cdot \tilde{\lambda}_{DI|\not{R}} \\
&= \tilde{\pi}^{FT}_{R} \cdot \tilde{\pi}^{FT}_{L|R} \cdot \tilde{\lambda}_{DI|L,R} \\
&+ \tilde{\pi}^{FT}_{R} \cdot \tilde{\pi}^{FT}_{\not{L}|R} \cdot \tilde{\lambda}_{DI|\not{L},R} \\
&+ \tilde{\pi}^{FT}_{\not{R}}) \cdot \tilde{\pi}^{FT}_{L|\not{R}} \cdot \tilde{\lambda}_{DI|L,\not{R}} \\
&+ \tilde{\pi}^{FT}_{\not{R}} \cdot \tilde{\pi}^{FT}_{\not{L}|\not{R}} \cdot \tilde{\lambda}_{DI|\not{L},\not{R}}
\end{aligned} \tag{B.12}$$

Finally, substituting all the results into the "raw" relative risk yields:

$$
\begin{aligned}
\lambda_{raw} = \alpha_x \cdot \lambda_{shock} \cdot & \quad \tilde{\lambda}_{DI} \\
= \alpha_x \cdot \lambda_{shock} \cdot & \Bigg(\quad \tilde{\pi}^{FT}_{R} \cdot \tilde{\pi}^{FT}_{L|R} \cdot \tilde{\lambda}_{DI|L,R} \\
& + \quad \tilde{\pi}^{FT}_{R} \cdot \tilde{\pi}^{FT}_{\not{L}|R} \cdot \tilde{\lambda}_{DI|\not{L},R} \\
& + \quad \tilde{\pi}^{FT}_{\not{R}}) \cdot \tilde{\pi}^{FT}_{L|\not{R}} \cdot \tilde{\lambda}_{DI|L,\not{R}} \\
& + \quad \tilde{\pi}^{FT}_{\not{R}} \cdot \tilde{\pi}^{FT}_{\not{L}|\not{R}} \cdot \tilde{\lambda}_{DI|\not{L},\not{R}} \Bigg) \\
= \alpha_x \cdot \lambda_{shock} \cdot & \Bigg(\quad \tilde{\pi}^{FT}_{R,L} \cdot \tilde{\lambda}_{DI|L,R} + \tilde{\pi}^{FT}_{\not{L},R} \cdot \tilde{\lambda}_{DI|\not{L},R} \\
& + \quad \tilde{\pi}^{FT}_{L,\not{R}} \cdot \tilde{\lambda}_{DI|L,\not{R}} + \tilde{\pi}^{FT}_{\not{L}|\not{R}} \cdot \tilde{\lambda}_{DI|\not{L},\not{R}} \Bigg) \qquad \text{(B.13)}
\end{aligned}
$$

Overall, this implies that we can decompose the raw relative risk into the effect of selection multiplied by the relative risk of a shock and by the weighted average of the relative risks of different combinations of employer incentives and labor market prospects conditional on a health shock.

B.2 Selection of cross-products in unconditional DI risk regressions

To determine the impact of selection on the DI risk premium, we estimate linear regression models in which we sequentially add control variables. In the first six specifications, no cross-products of explanatories are included.[32] The exclusion of cross-products could potentially cause omitted variable bias, for example when age has a positive effect on DI risk for the higher educated but a negative effect for the lower educated, while also affecting the probability to have a fixed-term contract. Inclusion of all cross-products could however lead to over-fitting. In order to minimize omitted variable bias while avoiding over-fitting, we select relevant cross-products using a LASSO specification.

[32] See Appendix Table A.1 for full set of controls

A standard LASSO regression uses an information criterion to select those variables which are predictive of the outcome variable. Given that the primary goal of our analysis is to estimate the contract type effect – and not to get the best prediction of DI risk – we implement a two-stage LASSO following Belloni et al. (2014). This method implies estimating a LASSO regression on the primary regression of interest, and estimating a LASSO regression with the dependent variable of interest as outcome variable. The intuition behind this method is that omitted variable bias is caused by variables which affect both the dependent variable, in our case DI, and the independent variable of interest, in our case contract type. Only control variables affecting both the outcome of interest and the dependent variable of interest should therefore be included in the final regression. We apply LASSO to the following two regressions:

$$\begin{aligned} DI_{it} &= \sum_{j}^{4} \beta_j E_{it}^j + \delta X_{it} + \varepsilon_{it} \\ E_{it} &= \delta X_{it} + \theta_{it}, \end{aligned}$$

with X_{it} containing all control variables and all cross-products between these control variables, resulting in approximately 1000 dummies.[33] Given the large number of controls, LASSO estimation is not feasible on the full sample. We therefore estimate LASSO regressions on 10 random subsamples, each containing 5 million observations. For both the DI regression and the contract type regression, the optimal trimming parameter is extremely small ($\lambda \leq 10^{-10}$), regardless of the information criteria used. As a result, almost all (more than 95%) of all cross-products are selected in both regressions, and hence in the seventh DI regression specification. This implies that over-fitting is not a problem due to the very large sample size of the regressions.

[33] We exclude the quarter-of-the-year dummies and the sector dummies from the cross-products as inclusion would drastically increase the computational burden. Furthermore, The cross product between all sectors and all education levels are included in the fourth specification

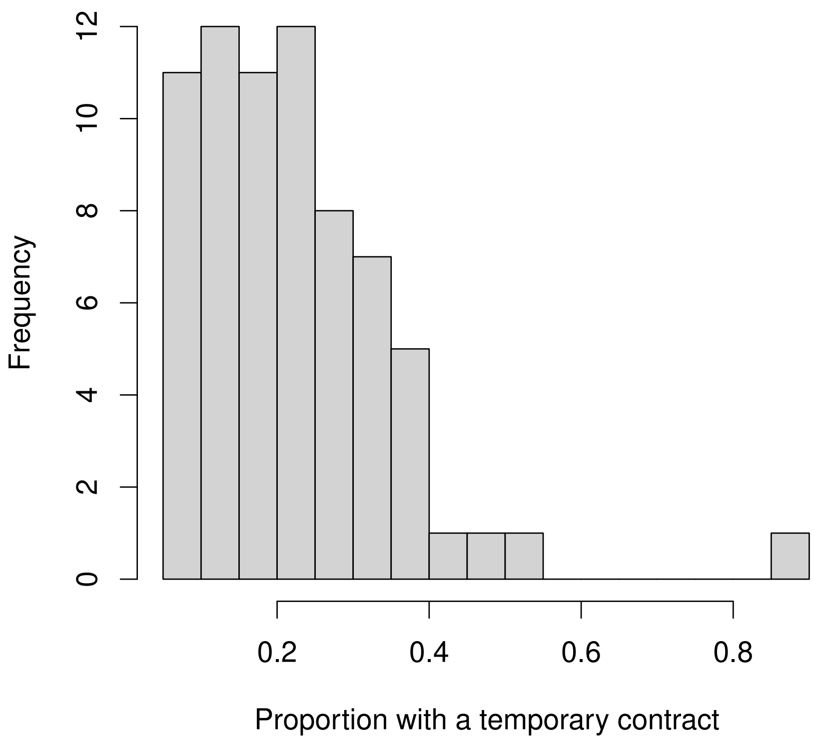
B.3 Classification of sectors

The classification of sectors into labor market tightness categories is done using the ratio of vacancies over jobs. For the years used in the analysis (2010–2015) we order all sectors based on this measure of labor market tightness. A sector is classified as being tight if, for all years considered, it ranks in the top five of most tight labor markets. Likewise, a sector is classified as being loose if, for all years considered, it ranks in the top 5 of most loose labor markets. Given the consistency of the vacancies over jobs ratio throughout the years, alternative specifications yield very similar classifications. The resulting classification is shown in Table B.1. Specifically, ICT, catering and retail are classified as tight and public administration, education and industry are classified as loose.

Table B.1: Construction of labor market tightness classification based on vacancies over job ratios used by Statistics Netherlands

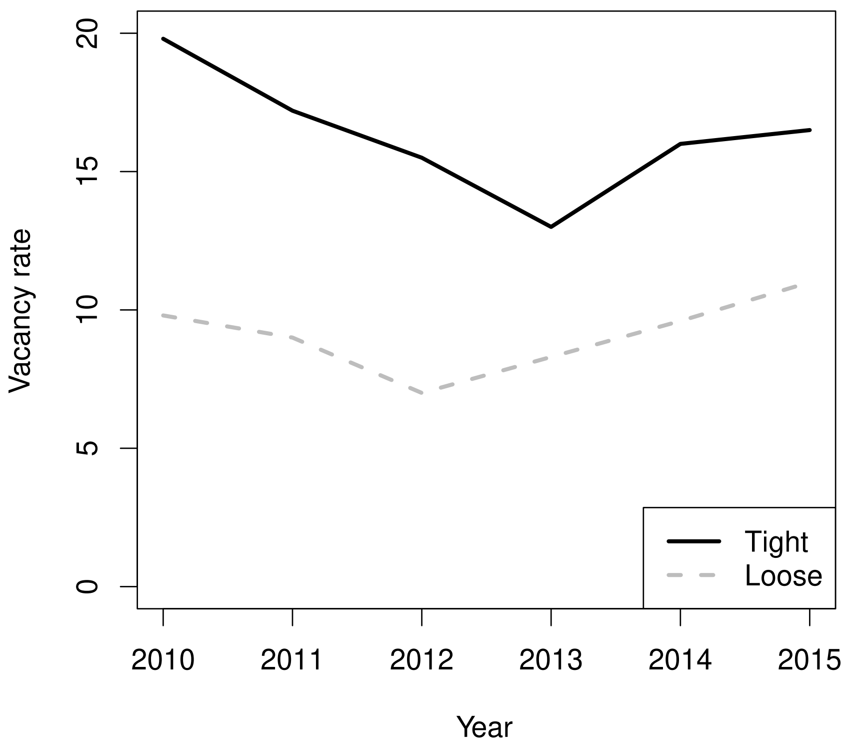
Sector	Tight	Loose	Sector	Tight	Loose
Unknown		X	Dairy industry		X
Agricultural		X	Textile industry		X
Tabaco industry		X	Stone, cement and glass		X
Construction		X	Chemical industry		X
Dredging		X	Food industry		X
Wood industry		X	General industry		X
Carpentry industry		X	Employment agencies		X
Furniture industry		X	Security		X
Wholesaler in wood		X	Cultural institutions		X
Graphics industry		X	Other		X
Metal industry		X	Painters		X
Electronics industry		X	Plasterers		X
Metal firms		X	Roofers		X
Bakeries		X	Mortar		X
Sugar industry		X	Stonemasons		X
Butchers businesses		X	Government: Education		X
Butchers other		X	Government: Police		X
Retail	X		Government: Defense		X
Cleaning		X	Government: Municipalities		X
Chain store		X	Government: Public utilities		X
Port firms		X	Government: Other		X
Port classifiers		X	Reintegration		X
Inland shipping		X	Rail construction		X
Fishing		X	Telecommunication	X	
Merchants		X			
Transportation KLM		X			
Transportation NS		X			
Postal services		X			
Taxi transportation		X			
Public transport		X			
Private bus transport		X			
Other passenger transport		X			
Other freight transport		X			
General catering	X				
Catering	X				
Healthcare		X			
Banking		X			
Insurance firms		X			
Publishers		X			
Wholesale 1		X			
Wholesale 2		X			
Business services 1		X			
Business services 2		X			
Business services 3		X			

Figure B.1: Distribution of the proportion of flexible contracts per sector



The distribution of contracts types over sectors is shown in Figure B.1. In most sectors, between 5 to 50% of all contracts are temporary. The only outlier is temp-work agencies, where approximately 80% of all contracts are temporary. The tight sectors are distributed evenly throughout the distribution. As we incorporate the labor market tightness classification into the difference-in-difference specification, we require that the absolute level of labor market tightness evolves parallel over time for tight and loose sectors. Figure B.2 shows that labor market tightness decreased towards 2012/2013, and increased afterwards. The increase starts earlier in loose labor markets, but the overall trends are similar.

Figure B.2: Absolute level of labor market tightness over time for loose and tight labor markets



B.4 Classification of healthcare cost

Table B.2: Construction of mental healthcare expenditures and physical healthcare expenditures based on expenditure categories used by Statistics Netherlands

Expenditure category[a]	Mental healthcare	Physical healthcare
General practitioner		X
Pharmacy		
Hospital healthcare		X
Paramedical healthcare		X
Apparatus		
Hospital transportation		
Birth care		
Health care expenditures incurred abroad		
Other cost		
First-line psychological healthcare	X	
Mental healthcare	X	
Basic-mental healthcare	X	
Specialist mental healthcare	X	
Geriatric rehabilitation healthcare	X	
Nursing without stay		X
Sensory disability healthcare		

Note: (a) Expenditure categories as used by Statistics Netherlands