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# Why does temporary work increase disability insurance inflow?

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#### 1. Introduction

The prevalence of fixed-term contracts has gradually increased in many OECD countries in recent decades, sparking debates about precarity and potential segmentation of the labour market.<sup>2</sup> These debates do not not only concern lower pay, but also the potentially negative impact of fixed-term contracts on workers' health. Evidence has indeed shown that in many countries, workers with fixed-term contracts suffer from worse health conditions than workers with permanent contracts (OECD, 2010).<sup>3</sup> This negative association is found both in studies based on cross-sectional data and in 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 physical health problems (Bardasi and Francesconi, 2004). In the Netherlands, permanent workers made up only 56% of the applications for disability insurance (DI) in 2010, even though they constitute 73% of the labour force.<sup>4</sup>

If workers with fixed-term contracts are more likely to enter DI, a crucial follow-up question is whether their contract type causally generates this higher probability of applying for DI or whether there is segmentation in the labour market such that relatively unhealthy workers with weaker labour market prospects end up in fixed-term contracts. The latter has been suggested for the US, where vulnerable workers with low productivity levels make up an increasing fraction of Social Security Disability Insurance (SSDI) benefit recipients (Maestas, 2019; Autor and Duggan, 2003; Deshpande and Lockwood, 2022). These workers have either worse health conditions (a 'health disability') or limited prospects in the labour market, which prevent them from working (a 'work disability') (Benítez-Silva et al., 2010).

In this paper, we show that workers with fixed-term contracts are indeed more likely to apply for and be awarded DI. We study potential mechanisms driving these differences in DI applications and awards.

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<sup>4</sup> Own calculations using administrative micro-data, see Section 2.2 for details.

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# ABSTRACT

We show that workers with fixed-term contracts are substantially more likely to apply for and be awarded disability insurance (DI) benefits than permanent workers. We study whether this differential can be explained by (i) selection of worker types into contracts, (ii) the relation between contract type and the risk of illness, (iii) differences in employer support during illness, and (iv) differences in labour market prospects of ill workers. We find that selection actually masks part of the differential, whereas the impact of contract type on health is limited. In contrast, the difference in employer support during illness is a significant cause of the heightened DI risk of temporary workers, especially in slack labour markets. We therefore conclude that, conditional on being ill, workers with fixed-term contracts face different support structures and incentives that make them more likely to ultimately apply for and be awarded DI.

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<sup>&</sup>lt;sup>2</sup> Within the EU, the average share of fixed-term contracts was 16% in 2016, with higher shares in countries such as Chile (28%), Poland (27%), Spain (25%), The Netherlands (20%) and France (16%) (OECD, 2015).

<sup>&</sup>lt;sup>3</sup> 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.

These include selection into contract type, (changes in) the health conditions of workers, differences in employer responsibilities, and differences in the labour market conditions that workers are facing. We use large-scale administrative data from the Netherlands on labour market histories, disability applications, and healthcare consumption. In the time period under investigation (2010–2015), the prevalence of fixed-term contracts increased and the probability of entering DI for this group was substantially higher than for permanent workers.

We make two key contributions to the existing literature. First, we provide a comprehensive picture of the drivers of higher disability risks among fixed-term workers by examining the various explanations simultaneously within the same setting. We examine the potential role of selection of (un)healthy workers in contract types, as well as the distinctive roles that the employer and the worker may have in preventing disability benefit applications. This is novel compared to previous work that typically analyses these issues in isolation, see e.g. studies on the impact of temporary work on health and well-being (Caroli and Godard, 2016; Bardasi and Francesconi, 2004; Benach et al., 2014; Kim et al., 2012) and studies on the impact of economic conditions on DI recipiency (Deshpande and Lockwood, 2022; Autor and Duggan, 2003).

A comprehensive analysis is essential for guiding policy: if contract type directly affects illness, policies to reduce DI applications could focus on discouraging the use of temporary work arrangements. If not, structural changes to employer incentives or improvements in the labour market prospects of vulnerable workers – e.g., through retraining – would be required to reduce inflow into DI. Assessing the relative importance of these competing explanations is only feasible when they are studied under comparable conditions. Our approach ensures such commonality: all analyses share a common institutional setting and economic environment, and our data (in particular the rich sets of control variables) are aligned across all specifications.

Second, we employ credible identification strategies to study the different explanations. Our main finding, that employer responsibilities are crucial for DI applications, is based on a nation-wide reform and a convincing difference-in-differences design. For explanations where no natural experiment is available, such as the effect of contract type on illness, we follow the literature in studying *changes* in health status. In contrast to most existing work, we can use administrative population-wide healthcare expenditure data to obtain detailed measures of illness over time. In addition, our rich set of individual and job characteristics allow us to credibly limit the role of remaining selection bias.

In brief, we find that the selection of workers into contract types cannot explain the higher probability of fixed-term contract workers applying for DI. Additionally, the risk of falling ill is not substantially greater for workers with fixed-term contracts (after controlling for observables). Rather, limited employer efforts during illness substantially increase the DI application probability of temporary workers. We find that this role of employers during illness is particularly important in slack labour markets where outside options for sick workers are limited. In other words, unhealthy workers do not sort into fixed-term contract and these fixed-term contracts themselves are not associated with more frequent illness. Instead, conditional on being ill, workers with fixed-term contracts face different support structures and incentives that make them more likely to ultimately apply for and be awarded DI. This highlights the importance of employer responsibilities and outside opportunities for workers, also in a context where fixed-term and permanent workers have similar health problems.

To obtain these results, our analysis follows the timeline in Fig. 1. Our initial focus is on selection into fixed-term contracts, stemming from specific worker types and specific jobs having a higher or lower a priori disability risk. To examine this, we first regress a dummy for applying for DI on contract type and sequentially add a wide range of controls for demographics, occupation and prior health. By controlling for demographic and occupation differences, we find that the DI risk premium for workers with a fixed-term contract *increases* by approximately 50%. Even with the inclusion of 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 nonparametrically weighting the DI application probabilities. While we cannot exclude the possibility that permanent and fixed-term workers differ on dimensions still unobservable to us, the robustness of the application risk premium estimate suggests that selection is unlikely to explain the difference but instead masks part of the DI premium.

Second, we consider differences in the likelihood of falling ill between fixed-term and permanent workers. We interpret these differences as the direct effects of contract type on a health deterioration due to a higher occupational hazard or increased stress as a result of the lack of job security.<sup>5</sup> To proxy for the occurrence of falling ill, we define health shocks as substantial increases in medical consumption for physical health (both hospitalizations and medication) and/or the start of mental health treatment. In line with other research in this field, we find a higher likelihood of a negative mental health shock for fixed-term contracts (Kim et al., 2012; Virtanen et al., 2005; Benach et al., 2014). We contribute to this literature by showing that most of these differences are driven by selection into contract type based on demographic and job characteristics, as the associations largely disappear once we control for these characteristics. This result is similar to Caroli and Godard (2016), who also find strong associations between job insecurity and health outcomes, but a limited causal impact.

We next focus on what happens after a worker falls ill. At this stage, the employer has weaker incentives to facilitate the rehabilitation of workers whose contracts will expire anyway (i.e., explanation 3). This divergence is strengthened by the financial incentives inherent in the targeting of continued wage payments and experience rating towards permanent workers only. In light of these considerations, we exploit a policy reform in 2013 that increased the monitoring obligations and financial consequences of the employer if their fixed-term workers entered DI. Using a difference-in-differences strategy on the subsample of workers who have experienced a health shock, we find that approximately half of the conditional difference in the DI application probabilities is explained by differences in employer incentives. As such, we also add to earlier findings in the literature that experience rating increases the incentives to provide work accommodations and affects fatality and injury rates (Aizawa et al., 2020; Kyyrä and Tuomala, 2023; Tompa et al., 2012; De Groot and Koning, 2016). Our findings suggest that employer support is perhaps even more important for fixed-term workers.

As a final step, we consider the role of job opportunities in the labour market for sick workers. Employees with fixed-term contracts may be more vulnerable to bad labour market prospects, as their contracts are likely to end during their sick pay period. As a result, employer support may be particularly important for temporary workers in slack labour market where alternative options are limited (Autor and Duggan, 2003; Benítez-Silva et al., 2010). We find strong support for this hypothesis by estimating heterogeneous effects of the 2013 reform for tight and slack labour markets. Conditional on illness, the reduction in the probability of applying for DI among temporary workers due to the reform is almost four times larger in slack labour markets than in tight labour markets.

The main outcome of interest in our analyses is applying for DI. Individuals applying for DI have been sick for at least two years and hence reaching the application phase has important economic consequences. As one could argue that DI *awards* have even larger economic consequences, we also reproduce the entire analysis using DI awards as the outcome of interest. The results for DI awards are similar to those

<sup>&</sup>lt;sup>5</sup> Note that these effects may be dampened if fixed-term contracts incorporate probationary periods and workers therefore have incentives not to be absent (Ichino and Riphahn, 2005; Riphahn and Thalmaier, 2001; Engellandt and Riphahn, 2005).



Fig. 1. Timeline from employment to DI application: Potential mechanisms explaining the gap in DI application probabilities between fixed-term and permanent workers.

of DI applications, reflecting the fact that conditional on application, award rates are comparable for both contract types.

The remainder of this paper is organized as follows. In Section 2, we describe the relevant institutions in the Netherlands and provide descriptive evidence regarding the DI application probabilities. Section 3 lays out our empirical strategies and presents the results. Finally, Section 4 summarizes our findings and compares them with those of other studies, and Section 5 concludes.

### 2. Institutional setting and descriptive statistics

## 2.1. Disability insurance in the Netherlands

The administration of the DI system in the Netherlands is carried out by the Employee Insurance Organization (UWV).<sup>6</sup> DI benefit applications can be filed after two years of sick pay; this two-year period leading up to a DI application is referred to as the 'waiting period' in the Netherlands (see also Fig. 1). DI applications consist of a medical assessment by a medical expert and an assessment of remaining earnings capacity by a labour market expert. If the loss in earnings capacity is less than 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 encourage making use of one's remaining earnings capacity. For further details, see Koning et al. (2022).

During the sick pay period, permanent and fixed-term workers face different levels of employer support and commitment. For permanent workers, employers are obliged to continue paying wages during the worker's illness over the entire sick pay period. Concurrent with this, they are obliged to actively monitor the health of the employee and provide support to facilitate rehabilitation, for example, by adjusting working conditions. Moreover, any DI benefits awarded are experience rated for permanent contract workers. For workers with fixed-term contracts, the employer continues paying their wages during their illness until the date when 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 cases in which the ill worker entered the DI system, the extra DI benefit costs were not experience-rated. Note that individuals on unemployment insurance (UI) can also apply for DI while facing a similar process as individuals with fixed-term contracts.

As the share of DI applications from fixed-term contract workers steadily increased after 2006, the government introduced a reform in 2013 that extended the monitoring and financial obligations of the employer to fixed-term workers on sick leave. Employers became financially responsible for temporary employees on sick leave after their contracts expired, because experience rating of employer premiums for sick-pay and the DI benefits now includes temporary workers.<sup>7</sup> Together with this change, a one-year medical assessment was introduced for individuals on sick leave whose contract had expired. A similar assessment was already in place for employees with a permanent contract who were on sick leave. Overall, the 2013 reform increased the similarities between employer obligations and incentives for fixed-term and permanent workers.

While the details of DI systems vary across countries, the Dutch system is fairly similar to other European countries with regard to fixedterm and permanent workers. Regulations set by the European Union ensure that fixed-term workers enjoy similar employment conditions as permanent workers. This also applies to eligibility for sick pay and DI. However, sick pay is often limited to at most one year in other countries, compared to two years in the Netherlands (Germany, Sweden and Greece have sick pay periods of approximately two years as well). Additionally, sick pay exceeding one month in duration is covered by public insurance in most EU countries, while in the Netherlands employers remain financially responsible for the full duration of sick pay (Koning, 2016).

The duration of fixed-term contracts is legally restricted 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 must be permanent.<sup>8</sup> While permanent contracts offer substantial job protection, neither permanent nor fixed-term contracts can be dissolved during illness. Fixed-term workers therefore remain employed during their illness for as long as their contract lasts. European Union regulations on fixed-term contracts imply that similar restrictions are in place in many other European countries.

## 2.2. Data sources

Our analysis is based on three administrative datasets that are merged at the individual level. The combination of these three datasets allows us to construct employment and health status trajectories for all employed Dutch individuals. First, we use tax records provided by Statistics Netherlands. These records contain detailed descriptions of all employment contracts in the Netherlands between 2010 and 2015. They include the commencement and end dates of the contracts, individual and firm identifiers, the type of contract (fixed-term or permanent), the industry code, weekly hours and the salary paid.<sup>9</sup>

<sup>&</sup>lt;sup>6</sup> Note that prior to 2006, a range of reforms was implemented to reduce the DI inflow after the number of DI recipients had grown substantially in the 1980s and 1990s. For further details and evaluations of these reforms, see Koning and Lindeboom (2015), Van Sonsbeek and Gradus (2012), Godard et al. (2024), Bernasconi et al. (2024) and Hullegie and Koning (2018).

 $<sup>^7</sup>$  To alleviate the financial risks that might arise for small employers, the premium is averaged within sectors for employers with fewer than 10 employees. For employers with more than 10 and fewer 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 firm size.

<sup>&</sup>lt;sup>8</sup> The count is reset after a break of at least three months, meaning that lengthy fixed-term employment spells with the same employer are possible only with short breaks.

<sup>&</sup>lt;sup>9</sup> The data additionally includes information on UI receipt.

Descriptive statistics for the full sample and selected subsamples

	Employed	Fixed-term	Permanent	DI
	population	contract in	contract in	applicants
		Jan 2010 <sup>5</sup>	Jan 2010 <sup>5</sup>	
Demographics <sup>c</sup> :				
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 <sup>d</sup> :				
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 <sup>e</sup> :				
Permanent contract <sup>f</sup>	47.8%	29.4%	81.1%	49.4%
Fixed-term contract <sup>f</sup>	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
Disability insurance measures <sup>g</sup> :				
DI application probability	0.06%	0.07%	0.05%	100%
DI applications award rateh	61.6%	55.1%	63.6%	61.6%
Number of individuals	10,583,956	1,785,327	5,184,711	253,628

<sup>a</sup> All unique individuals who are employed at some point in time between 2010 and 2015.

<sup>b</sup> The reference date for the contract type is January 2010.

<sup>c</sup> Demographics in January 2010.

<sup>d</sup> Health measures are averages computed over the time window 2010–2015.

<sup>e</sup> Employment measures are averages computed over the time window 2010-2015.

<sup>f</sup> Percentage of months with the corresponding contract type.

<sup>g</sup> DI measures computed over the full time window (2010–2015).

<sup>h</sup> Percentage of DI applicants who have been awarded DI benefits.

Second, we use healthcare expenditure data. These data capture 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 from 2011–2016 on mental health treatment trajectories for which the exact start and end dates are available, as well as the number of treatment minutes per month. These data are also provided by Statistics Netherlands.

Third, we use data describing all DI applications for which the sickness spell started between January 2010 and June 2015 (the corresponding DI applications were between January 2012 and June 2017), provided by the Employee Insurance Agency (UWV). These data contain detailed information on all applications in terms of the health impairments assessment and the subsequent labour market assessment by vocational experts, which 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 presents the descriptive statistics for our full sample of employed individuals. The sample contains over 10 million individuals who are employed for at least one month during our observation window (2010–2015). The top panel includes demographics and education as measured in January 2010. The middle panel describes the healthcare measures averaged over the period 2010–2015. Employed individuals have on average €948 in physical healthcare costs and €187 in mental healthcare costs per year. The lower panel includes the employment measures, again 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 separate statistics for workers with a fixed-term contract and those with a permanent contract, respectively. Due to the longitudinal nature of the data, a single individual can change his or her contract type over time. We therefore classify individuals by their contract types as measured in January 2010. Workers with fixed-term contracts are substantially younger, less likely to be Dutch natives, and lower educated than permanent workers.<sup>10</sup> 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 spend only 7.6% of the next five years in fixed-term contracts. Permanent workers also have higher hourly wages and work more hours per week. It is important to stress that fixed-term contracts are not merely stepping stones towards permanent contracts for labour market entrants. Fixedterm contracts are prevalent throughout the age distribution. Among the individuals who have a fixed-term contract at some point in time, over 25% work under a fixed-term contract for more than three years, and this share is constant across the age distribution (see Appendix Fig. A.1). The lower panel shows that individuals with a fixed-term contract are significantly more likely to apply for DI. Conditional on application, the probability of being awarded DI benefits is slightly larger for individuals with a permanent contract.

Finally, column (4) of Table 1 shows statistics for DI applicants (250,000 individuals in our observation window). The most striking

<sup>&</sup>lt;sup>10</sup> The relatively high share of missing educational data for permanent workers is driven by the fact that the educational data has only been collected from 1999 onward. For older workers, the probability of missing educational information is therefore larger. Conditional on age, the probability of educational information missing is equal for both contract types.

differences relative to the working population are their higher age, lower level of education, higher healthcare use (across all measures) and slightly lower wage level. Note that these descriptives conceal the substantially higher probability of applying for DI among fixed-term 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).<sup>11</sup>

## 2.4. DI application probabilities

To derive the DI application probabilities by contract type, we need to account for the two-year sick pay period that precedes DI applications. Specifically, the population at risk for applying for DI in month t + 24 consists of all individuals employed or unemployed in month t. The at-risk populations for permanent and fixed-term workers between 2010 and 2015 are shown in panel (a) of Fig. 2. While the at-risk population with a permanent contract decreases steadily, the prevalence of at-risk individuals with fixed-term contracts increases by a similar magnitude. For comparison, Fig. 2(a) also includes individuals increases sharply due to the recession, although this group is an order of magnitude smaller than the population of employed individuals.

The number of DI applicants per category between 2010 and 2015 is shown in panel (b) of Fig.  $2.^{12}$  The largest group of DI applicants are those with a permanent contract, but the difference relative to the number from the group of fixed-term workers is considerably smaller than the corresponding difference in the at-risk populations.

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

## 3. Empirical analysis

We perform regression analyses to study whether each of the different components explains the gap in the DI application probabilities between fixed-term and permanent workers. In line with the sequential nature of the DI procedure, we first consider the role of selection into contract type in Section 3.1. Second, in Section 3.2, we estimate the relation between contract type and the probability of falling ill, as well as the subsequent probability of applying for DI. Third, we investigate the role of the employer during the two-year sick pay period in Section 3.3 by exploiting the 2013 reform that introduced employer responsibility for temporary workers. Finally, we consider labour market opportunities, by studying whether employer responsibilities are particularly relevant when labour market prospects are poor (Section 3.4). The four analysis steps are summarized in Table A.1 in the appendix. (a) At-risk populations: number of (un)employed individuals











Fig. 2. Number of (un)employed individuals, DI applicants and DI application probabilities.

#### 3.1. Selection into contract type

#### Empirical specification

In the first step of our analysis, we attempt to split the difference in DI application probabilities between a part explained by observed (preillness) characteristics (selection) and a remaining unexplained part due to the contract type. To do so, we estimate how contract type explains the DI application probability with and without conditioning on a rich set of observables using linear regression models.<sup>13</sup> All individuals eligible for DI benefits are included in a monthly panel (this includes fixed-term workers, permanent workers and individuals receiving UI benefits). Since workers who apply for DI in a given month are no longer part of the at-risk population, we exclude observations of such workers for the 24 months preceding their application. We use the time period January 2010 until June 2015 (the full observation period for which have data available).

<sup>&</sup>lt;sup>11</sup> Note that in addition, the sampling in the table makes it infeasible to directly observe relative DI application probabilities because the shares of permanent and fixed-term contracts presented in column (1) represent the number of months, while in column (4), they represent the number of people. <sup>12</sup> We classify all DI applicants by their contract type 24 months prior to their application date. Most applicants can be classified into one of these three groups (permanent, fixed-term or unemployed), but a small share of approximately 1% cannot. This occurs because there are exceptions to the length of the sick pay period, meaning that *t* – 24 is not always the relevant month to consider. Furthermore, if a worker falls ill shortly after their contract ends, they are still eligible for DI benefits two years later. In this case, it is difficult to identify the relevant month. We assign all those without either an employment contract or UI benefits to the fixed-term contract category, as they do not have a responsible employer during the spell of their illness.

<sup>&</sup>lt;sup>13</sup> As a robustness check of our baseline model, we replace the linear specifications with logistic regressions.

The key explanatory variable is the dummy  $FT_{it}$ , which equals one if individual *i* has a fixed-term contract in month *t* and zero otherwise. The regression model is:

$$DI_{it} = \alpha + \beta FT_{it} + \delta X_{it} + \varepsilon_{it}, \tag{1}$$

where  $DI_{it}$  as an indicator dummy that is equal to one if individual *i* applies for DI in month *t* + 24, meaning that he or she fell ill in month *t*. Differences in the DI application probabilities between the contract types are captured by  $\beta$ . In the baseline specification,  $X_{it}$  includes a dummy variable for UI recipients with a prior fixed-term contract and a dummy for UI recipients with a prior temporary contract. As result  $\beta$  captures the difference in DI application probabilities only for individuals who are *employed*.<sup>14</sup>

We sequentially add control variables to  $X_{it}$  to assess the extent to which they explain the differences between contract types. We first add demographic controls, which we then extend with controls for job characteristics and two-year lagged healthcare expenditures. In a final step, we include all relevant cross-products by using a double LASSO specification following (Belloni et al., 2014).<sup>15</sup> Finally, we estimate an upper bound on any potential remaining bias from selection on unobservable characteristics using the method suggested by Oster (2019).

While our data (technically) allow for the use of individual fixedeffects to exploit individual variation in contract type, we choose not to include them. First, we would effectively lose all individuals who never apply for DI, which is the vast majority of our sample. Relatedly, the contract type effects ( $\beta$ ) would then be identified solely from individuals who switch between fixed-term and permanent contracts.<sup>16</sup> Furthermore, an individual typically only applies for DI once and then 'leaves' the sample afterwards, limiting the scope for a fixed-effects estimation. We therefore focus on cross-individual variation in contract type and DI risk.<sup>17</sup> Following (Abadie et al., 2023), we choose not to adjust our standard errors for clustering, as our sample contains the full Dutch population.

This analysis quantifies the role of selection in the observed disparity in DI applications across different types of contracts. Although it is impossible to account for all potentially relevant factors, the extensive set of demographic, health, and employment variables—together with the estimated upper bound on any remaining selection bias—assures us that the remaining estimate of  $\beta$  largely reflects the causal impact of holding a fixed-term contract on the likelihood of applying for DI.

## Results

The regression estimates for Eq. (1) are presented in Table 2. As a reference point, we find that a fixed-term contract increases the monthly probability of applying for DI by 0.013 percentage points, which corresponds to an approximately 30% increase relative to permanent contract workers. The inclusion of age, gender, nationality, education level, family composition and population density<sup>18</sup> as controls in column (2) *increases* the fixed-term coefficient to 0.027. This is mainly due to age differences: younger individuals are more likely to

have fixed-term contracts and are less likely to apply for DI. Controlling for job characteristics (wage, working hours and sector) reduces the coefficient to 0.018 (column (3)).

In column (4), we include 10 dummies for the level of lagged healthcare expenditures to control for differences in health status. The effect of lagged health on DI risk is substantial: individuals with lagged healthcare expenditures in the top 10% of the distribution are 40 times more likely to apply for DI than those in the bottom 10% (see Appendix Table A.3 for the healthcare coefficients from the regression in column (4)). The effect of large healthcare expenditures is ten times as large as the effect of contract type. Nevertheless, the difference in DI application probabilities due to contract type remains unchanged. Selection based on health appears limited and does not explain the difference in the DI application probabilities between contract types.<sup>19</sup>

Columns (5) and (6) in Table 2 show that our key regression results remain constant with the inclusion of even more extensive sets of controls. This holds for the inclusion of 408 municipality dummies, as well as the addition of interactions between sectors (70) and education levels (10) to control for more specific occupational characteristics that may drive the risk of disability. We also consider a specification that allows for interactions between all control variables. To balance the added value of these interactions and the risk of overfitting, we estimate a double LASSO specification (Belloni et al., 2014). The results, shown in column (7), again yield constant contract-type effects (with a coefficient of 0.020).

To address the concern that selection based on unobserved characteristics remains, we follow the approach proposed by Oster (2019). The idea is to compute an upper bound for the contract type effect by extrapolating changes in the coefficient estimates as additional covariates are added, weighted by the corresponding change in Rsquared. The changes in the estimated coefficient between columns (1), (2) and (3) indicate that there is selection based on demographic and job characteristics. We therefore use the specification in column (3) as our baseline and the specification in column (7) as our extended model. The Oster analysis thus tests whether conditional on the selection on demographic and job characteristics, there is any selection based on unobservable characteristics that correlate with the controls included in columns (4) through (7). 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 appears limited: the calculated upper bound on the fixed-term contract coefficient is 0.020 (almost equal to our estimate of 0.020 in column  $(7)).^{20}$ 

Table 2 also shows the results from four robustness specifications, and these results are very similar to the baseline results. First, we show that a logit model yields a marginal effect of contract type that is almost identical to the linear model effects. While we cannot directly compare coefficients between the baseline model and the logistic regression results, the relative impacts of having a fixed-term contract are similar.<sup>21</sup>

<sup>&</sup>lt;sup>14</sup> Individuals on UI are still eligible to apply for DI (and do so at a substantially higher rate). Excluding separate controls for UI recipients yields similar results.

<sup>&</sup>lt;sup>15</sup> See Appendix Section OA.1 for a discussion of the double LASSO specification.

<sup>&</sup>lt;sup>16</sup> To study the effect of switches in more detail, we estimate a robustness specification in which the subsample of individuals who recently switched contract types are included as separate employment contract groups.

<sup>&</sup>lt;sup>17</sup> Inclusion of individual fixed-effects in all of the analysis steps leads to similar general conclusion than in our baseline specifications. Full results with individual fixed-effects are available upon request.

 $<sup>^{18}</sup>$  Population density is measured at the municipality level and consists of 6 categories ranging from "nonurban" (less than 500 individuals per  $\rm km^2$ ) to "highly urban" (more than 2500 individuals per  $\rm km^2$ ).

<sup>&</sup>lt;sup>19</sup> In addition to the baseline model wherein we include lagged health outcomes, we have also conducted a robustness test wherein we control for current and future healthcare costs. Accordingly, we incorporate potential healthcare dynamics in our model. Although these variables could be directly affected by the contract type and therefore could be considered as bad controls, the resulting contract-type coefficients are similar to those obtained when only controlling for last year's healthcare cost. These results are available upon request.

<sup>&</sup>lt;sup>20</sup> Note that we use the restricted estimator and the  $R_{max}$  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, e.g.,  $R_{max} = 2\tilde{R}$ , does not alter our conclusion.

 $<sup>^{21}</sup>$  To obtain relative marginal effects of the baseline model, one can divide the fixed-term contract coefficient plus the mean DI application rate, by the mean DI application rate. For column (8), (0.053+0.020)/0.053=1.37, which is similar to the 1.46 of the logistic regression.

Regression results for monthly	DI	application ris	sk for	temporary	and	permanent	workers.
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	DI application							
	(1)	(2)	(3)	(4)	(5)	(6)	(7) <sup>f</sup>	(8) <sup>g</sup>
Fixed-term contract coefficient	0.013	0.027	0.018	0.019	0.019	0.020	0.020	0.020
Alternative specifications:								
Logistic regression <sup>a</sup>	1.30	1.68	1.43	1.45	1.45	1.45	1.45	1.46
Merge UI groups <sup>b</sup>	0.019	0.033	0.019	0.019	0.022	0.019	0.018	0.018
Include switcher groups	0.010	0.026	0.016	0.017	0.020	0.016	0.016	0.016
DI award <sup>c</sup>	0.004	0.015	0.011	0.012	0.012	0.012	0.012	0.012
Number of control dummies	2	52	135	145	548	705	674	•
R <sup>2</sup> baseline regression	0.026	0.060	0.065	0.089	0.090	0.091	0.108	
Observations (ind.*month in millions)	475	475	475	475	475	475	475	475
Mean DI application rate	0.053	0.053	0.053	0.053	0.053	0.053	0.053	0.053
Mean DI award rate	0.033	0.033	0.033	0.033	0.033	0.033	0.033	0.033
Quarter-of-year dummies		Х	Х	Х	Х	Х	Х	
Demographic controls <sup>d</sup>		Х	Х	Х	Х	Х	Х	
Job controls <sup>e</sup>			Х	Х	Х	Х	Х	
Lagged health controls				Х	Х	Х	Х	
Regional fixed-effects					Х			
Sector x education controls						Х		
All relevant cross-products							Х	

All estimates are reported as percentage points, i.e. a reported coefficient of 0.013 implies a 0.013 percentage point difference in the monthly probability of applying for DI. The sample in all columns includes the full observation period (January 2010 to June 2015). All regressions include dummies for receiving UI benefits after a fixed-term contract and for receiving UI benefits after a permanent contract (estimates not reported). All coefficients in this table are statistically significant with P values < 0.01 and therefore standard errors are omitted for brevity. See Appendix Table A.2 for the specific control variables included.

<sup>a</sup> Relative probabilities from the logistic regressions are calculated as  $e^{\beta}$ , where  $\beta$  is the fixed-term contract coefficient from the logistic regression.

 $^{\rm b}\,$  Regression of the relative probability of applying for DI with UI groups incorporated.

<sup>c</sup> Regression of the relative probability of receiving a DI award.

<sup>d</sup> Age, gender, nationality and education level.

<sup>e</sup> Wage, number of working hours and sector of employment.

<sup>f</sup> Cross-products are included based on their predictive power over DI applications and contract type using a double LASSO specification; see Appendix Section OA.1 for details.

<sup>g</sup> Upper bound on selection on unobservable characteristics using (Oster, 2019) analysis:  $\beta^* = \tilde{\beta} - (\beta^0 - \tilde{\beta}) \frac{1.3*\tilde{R} - \tilde{R}}{\tilde{B} - 0^0}$ .

Second, we omit separate controls for UI beneficiaries, thereby pooling UI recipients with employees.<sup>22</sup> The resulting decrease in the fixed-term contract coefficient is because the DI application probability is high for UI beneficiaries regardless of their contract type prior to entering UI. Third, we reclassify individuals whose contract type changed in the last six months. More precisely, we add two employment statuses: (1) switches from fixed-term to permanent contracts and (2) switches from permanent to fixed-term contracts. The DI application probabilities of the switcher groups are very similar to those with their prior contract resulting in a decrease in the fixed-term contract coefficient.<sup>23</sup> The gap in DI application probabilities is thus neither caused nor masked by contract type effects that carry on after contract type switches. Lastly, we examine DI awards instead of DI applications. While absolute levels of both the fixed-term contract coefficient and the mean DI award rate are lower, in relative terms, the results are similar to those when examining application. Given the high level of similarity between the results, we conclude that contract type has little impact on the probability that a DI application is approved.

As a further robustness test, we also perform a non-parametric analysis where we reweight the DI application risk based on a set of key characteristics. Results are presented and discussed in Appendix A.2 and strongly support our regression findings. Despite stark compositional differences between workers with fixed-term and permanent contracts, the higher DI application probability among fixed-term workers is barely explained by these differences. Demographic differences conceal the fact that the difference is actually slightly larger than raw numbers suggest, while differences in prior health have a negligible additional effect. The 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 probability among fixed-term workers.

### 3.2. Impact on the probability of falling ill

In the previous section, we showed that the gap in DI application probabilities is still present after correcting for differences in a large set of worker and job characteristics. We proceed by exploring whether the difference in DI application probabilities remains when we condition on individuals that have fallen ill. In other words, do we observe a disparity in the likelihood of falling ill, or does the DI application gap appear merely after the onset on illness? On the one hand, contract type may impact the probability of falling ill through, for example, differences in occupational hazards, fewer prevention activities or increased stress due to a lack of job security. On the other hand, *conditional on illness* workers may be treated differently depending on their contract type as we will investigate in Section 3.3.

#### Empirical specification

We proceed in two steps. First, we consider the relationship between contract type and falling ill, and second, we consider the DI application probability *conditional* on illness. In each step, we also consider the role of selection through adding the same set of control variables as in the previous analysis.

As the first step, we regress an indicator for falling ill on contract type, with and without the set of controls that were used in the previous analyses. This analysis has the purpose of quantifying the observed difference in illness between temporary and permanent workers and splitting that difference into a part explained by selection and a part that we argue has a causal interpretation. Causality is established

<sup>&</sup>lt;sup>22</sup> UI recipients whose most recent contract was temporary are included in the temporary contract category. UI recipients whose most recent contract was a permanent contract are included in the permanent contract category.

<sup>&</sup>lt;sup>23</sup> Individuals who switch from fixed-term to permanent contracts have DI application probabilities similar to those of individuals with permanent contracts, and likewise, individuals who switch from permanent to fixed-term contracts have DI application probabilities similar to those of individuals with a fixed-term contract.

under a conditional independence assumption: conditional on our set of worker and job characteristics, the contract type should be independent from all factors that affect illness. This is a strong assumption as we clearly do not observe all relevant characteristics. We nevertheless argue in favour of this assumption based on two arguments. First, our set of worker and job characteristics is very rich: it not only includes demographics (gender, age, education level, nationality, family composition, population density of place of residence), but also wage levels, working hours and sector. We also show that adding additional controls (such as all relevant cross-products) hardly changes the contract-coefficient. Second, we define our outcome as a *change* in health status (a 'health shock'), thereby essentially controlling for baseline health.

This approach advances the literature by leveraging our rich dataset that surpasses the scope of most existing studies. Although (Kim et al., 2012; Virtanen et al., 2005; Benach et al., 2014) synthesize findings from over 100 studies on the health effects of temporary employment contracts — consistently identifying a significant and sizeable negative correlation — these studies predominantly rely on associations or regression models with a limited set of control variables. We return to this literature below when discussing our results.

We now turn to the definition of health shocks. Administrative data in the Netherlands do not provide information on employee sick leave. We therefore proxy for this event by identifying increases in healthcare utilization. We define a negative health shock using various thresholds. In the baseline model, we define a mental health shock as the beginning of a mental health treatment trajectory. A physical health shock is defined as an increase in annual healthcare expenditures from below the 50th percentile for the population (approximately  $\in$ 150) to above the 90th percentile (approximately  $\in$ 2400). The at-risk population is defined as the population without mental health treatment or with healthcare expenditures below the 50th percentile. This results in samples of individuals who are sufficiently 'healthy' such that they can, according to our definition, experience a negative health shock.

The availability of healthcare data is limited to a shorter time window. For mental health, we observe health shocks starting from 2012 while for physical health shocks the time window starts in 2011. To ensure that we also capture differences in the severity of the health shocks, we additionally define more severe shocks by changing the lower threshold and/or the upper threshold of the shocks.<sup>24</sup> Our regression model for experiencing a negative health shock is as follows:

$$H_{it} = \alpha^S + \beta^S F T_{it} + \delta^S X_{it} + \varepsilon^S_{it}, \qquad (2)$$

where  $H_{ii}$  is an indicator for individual *i* experiencing a negative health shock in period *t*. Again, our interest lies in the estimate of  $\beta^{\rm S}$ , which in this case captures the association between the type of contract and the likelihood of experiencing a negative health shock. We sequentially add the same rich set of control variables as in Section 3.1, with the exception of healthcare expenditures. We thus reduce the scope for omitted variable bias, strengthening the claim that  $\beta^{\rm S}$  captures causal effects.

We next estimate whether the probability of applying for DI *conditional* on a health shock differs by contract type. Together with the results of Eq. (2), this allows us to assess whether the DI application differential persists *conditional on falling ill*. The goal is to provide insight into whether it is the likelihood of falling ill that differs by contract type, or that differences appear during the two-year sick pay period.

Specifically, we re-estimate Eq. (1) for the subsample of employees who experience a negative health shock. We denote the month (for mental health shocks) or year (for physical health shocks) in which the shock occurs as  $t_i^*$  and include only observations from the 6 months before and 6 months after  $t_i^*$ .<sup>25</sup> Similar to the outcome variable in our earlier analysis, the outcome variable is an indicator equal to one if a DI application is filed at time t + 24 and zero otherwise. All analyses that condition on health shocks thus use the same setup but focus on a specific subset of observations (those observations around the observed health shock).

#### Results

Table 3 shows the regression estimates for the risk of experiencing a mental (Panel A) or physical health shock (Panel B).<sup>26</sup> Column (1) shows the risk of experiencing a health shock obtained without including control variables while column (2) shows the risk conditional on a wide range of control variables. The tables also present the implied conditional relative DI application probabilities for both mental and physical health shocks. Any differences between the unconditional and conditional relative DI application probabilities follow from the differences in the probability of experiencing a negative health shock.

Without controlling for any characteristics, the monthly risk of a mental health shock is 0.041 percentage points higher for employees with fixed-term contracts. This corresponds to approximately a 40% increase relative to permanent workers. For a physical health shock, the coefficient is -0.052 (-18%). Both differences decrease markedly when we include control variables, as shown in column (2). With an extensive set of controls we find that fixed-term contracts increase the risk of a mental health shock by 0.010 (9%), while for physical health problems, the risk is almost identical between the two contract types (-0.013 or -3%).<sup>27</sup> This difference in coefficients without and with control variables is fully driven by an age effect: after controlling for age, additional control variables have little impact (see Table A.4 in Appendix A.).

To ensure that not only the likelihood of experiencing health shocks but also the severity of those health shocks are similar for individuals with fixed-term and permanent contracts, Table 3 includes the results with various alternative types of health shocks. For this, we increase or decrease the upper or lower bounds of our definitions of health shocks. The estimated fixed contract coefficients for these alternative shocks are different from our baseline estimates; this mirrors the fact that severe shocks occur less frequently. Relative to the average occurrence of the shock, all estimated fixed-term contract coefficients are small and of equal relative magnitude.<sup>28</sup> We therefore conclude that any differences in the likelihood of falling ill are due to selection: conditional on observable characteristics, the type and severity of health shocks are very similar for both contract types.

These results contribute to the earlier discussed surveys by Kim et al. (2012), Virtanen et al. (2005), Benach et al. (2014), which document negative associations between temporary employment and (mental)

<sup>&</sup>lt;sup>24</sup> For mental health, we define a larger shock as having less than 50 min of treatment in one month to having more than 200 min of treatment in the next month and we define the largest shock as receiving no treatment in one month to receiving more than 200 min of treatment the next month. For physical health, we define a smaller health shock as having expenditures below the 75th percentile one year followed by expenditures above the 90th percentile the next year. We define a larger shock as having below median expenditures one year followed by expenditures above the 99th percentile the next year.

<sup>&</sup>lt;sup>25</sup> The exact timing of the health shock is difficult to observe because (i) the health expenditure data are annual and (ii) there may be waiting time for certain types of healthcare such that expenditures increase with some delay. To address these issues, we also include the 6 months prior to the health shock. <sup>26</sup> One might expect the 2013 reform to affect the probability of falling ill

for temporary workers, but we find that results based on the post-2013 period only are very similar.

<sup>&</sup>lt;sup>27</sup> When including cross-products of controls and computing an (Oster, 2019) upper bound on the contract type effect, which takes into account selection based on unobservable characteristics, the estimated contract type coefficients become even slightly smaller (Online Appendix Table A.4).

 $<sup>^{\</sup>mbox{$28$}}$  Additionally, the evolution of health after a health shock is similar for both contract types.

Regression results for a negative mental or physical health shock and a subsequent DI application.

Panel A: Mental health shock (> 0 minutes mental healthcare treatment)

	Mental health she	ock	Conditional DI application	
	(1)	(2)	(3)	(4)
	No controls	Controls	No controls	Controls
Fixed-term contract coefficient	0.041	0.010	0.079	0.192
Mean dependent variable	0.116	0.116	0.571	0.571 4.2
Number of observations (in millions)	286	286	4.2	
Alternative specifications:				
< 50 minutes to > 200 minutes health shock	0.012	0.003	0.044	0.349
Mean dep. variable	0.027	0.027	1.082	1.082
0 min to $> 200$ minutes health shock	0.013	0.003	0.049	0.348
Mean dep. variable	0.028	0.028	1.087	1.087
Panel B: Physical health shock (< 50th to > 90th percent	centile healthcare exper	nditures)		
	Physical health s	hock	Conditional DI ar	oplication

	Physical health s	hock	Conditional DI ap	application	
	(1) (2)		(3)	(4)	
	No controls	Controls	No controls	Controls	
Fixed-term contract coefficient	-0.052	-0.013	0.045	0.044	
Mean dep. variable	0.278	0.278	0.107	0.107	
Number of observations (in millions)	192	192	10.9	10.9	
Alternative specifications:					
<75th to $>90$ th percentile health shock	-0.076	-0.023	0.047	0.047	
Mean dep. variable	0.464	0.464	0.121	0.121	
< 50th to $> 99$ th percentile health shock	-0.007	-0.001	0.075	0.066	
Mean dep. variable	0.014	0.014	0.209	0.209	

All estimates are reported as percentage points, i.e. a reported coefficient of 0.041 implies a 0.041 percentage point difference in the monthly probability of experiencing a health shock/applying for DI. All coefficients in this table are statistically significant with P values < 0.01 and therefore standard errors are omitted for brevity. Regressions contain the same control variables as in the regression reported in Table 2, column 4: quarter-of-year dummies, demographics and job controls. See Appendix Table A.2 for the specific control variables included. Appendix Table A.4 shows the results in which control variables are sequentially included. (a) Average predicted DI probability if all individuals had a fixed-term contract (or, correspondingly, a permanent contract). (b) Ratio of estimated DI probability among fixed-term workers and estimated DI probability among permanent workers.

health. While our findings align with these studies when estimating associations, our analysis reveals that much of this relationship is driven by selection effects based on demographic and job characteristics. This suggests that the true impact of temporary employment on health is likely smaller than previously reported correlations imply.

Next, we consider the probability of applying for DI conditional on experiencing a health shock. First, note that the mean dependent variable in columns (3) and (4) is approximately 10 times as large as the unconditional probability (as reported in Table 2). This indicates that the mental health shocks that we identify are indeed strong predictors of later DI application. Column (3) reports results without controls, showing that even conditional on sickness, a fixedterm contract increases the probability of a DI application significantly (0.079 percentage points). Once controls are included, we find that the probability of applying for DI is 0.192 percentage points (34%) higher for fixed-term workers, which is very close to the unconditional difference (0.019, or 37%). When turning to physical health shocks (Panel B), we find that the conditional difference in the DI application probabilities (with controls, column 4), is even larger (0.044/0.107 =41%) than the unconditional difference in Table 2 (37%). Results are similar when examining either larger or smaller health shocks.

We conclude that any differences in the risk of falling ill between contract types do not explain the DI application gap for both mental and physical illness. Instead, it seems that the divergence in DI applications starts to occur only *after* the onset of illness.

### 3.3. Employer incentives during the sick pay period

After falling ill, employees face a two-year sick pay period during which employer incentives and obligations to support reintegration differ by contract type. Those with permanent contracts receive support from their employer, who is obliged to monitor their progress and actively facilitate their rehabilitation. Employers also face financial consequences if their employees enter DI through experience rating: DI contributions depend on the inflow of their employees into DI during the previous ten years.

Before 2013, these employer responsibilities ended when the employment contract of temporary workers expired. As discussed in Section 2, for fixed-term workers, the role of the employer during the sick pay period prior to DI application became much more similar to that for permanent workers beginning in 2013.<sup>29</sup>

#### Empirical specification

To identify the importance of employer incentives during the sick pay period, we exploit the 2013 reform using a difference-in-differences (DiD) strategy. Since permanent workers were unaffected by the reform, we use these workers as the control group. Identification relies on the parallel trend assumption, which in this case states that in the absence of the reform the trend in DI applications would have been the same for temporary and permanent workers. We return to the plausibility of this assumption below. We derive our specification by adding interaction terms between the contract type dummies and a dummy for post-2013 observations to the specification in Eq. (1) (with superscripts R to distinguish from the earlier models):

$$DI_{it} = \alpha^R + \beta^R F T_{it} + \delta^R I_{t>2013} F T_{it} + \gamma^R X_{it} + \varepsilon_{it}, \qquad (3)$$

Event time *t* refers to the month in which an individual becomes ill (not the month in which the DI application takes place). The reform affects individuals for whom sickness starts in January 2013 or later.<sup>30</sup> Since  $X_{it}$  includes quarter-of-year dummies, this specification corresponds to a conventional DiD model with fixed-term workers as the treatment group and permanent workers (for whom nothing changed after 2013)

 $<sup>^{29}\,</sup>$  For UI recipients, the change due to the reform is similar. Their last employer prior to entering UI, became responsible for them after the reform.

 $<sup>^{30}</sup>$  As an example, someone falling ill in December 2012 and applying for DI in December 2014 would not be affected by the reform. See Fig. A.3 in the appendix for a detailed visualization of the timeline and empirical setup.



Fig. 3. Impact of the 2013 reform that introduced employer incentives for temporary workers on the difference in DI risk between fixed-term and permanent workers.

as the control group. Given that the contract type of individuals potentially changes from month to month, this also implies that their classification intro control group (permanent contract) and treatment group (fixed-term contract) could change from month to month. The model should thus be interpreted as a DiD model estimated on repeated cross-sections. We estimate the DiD model on the full sample and on samples of workers who have experienced a negative health shock. The analysis of the samples that have experienced a negative health shock allows us to assess the importance of employer responsibilities specifically for those workers that have fallen sick. The analysis of the full sample on the other hand shows that the results also hold for the individuals for whom we do not observe a health shock.

To assess the validity of the parallel trends assumption, we first perform an event-study analysis; i.e., we interact the contract type dummies with month-of-the-year dummies in the regression.<sup>31</sup> For the event-study analysis, we use the DI applications of all workers (thus, the probability of applying for DI is not conditional on experiencing a health shock) as this enables us to use a substantially larger sample and longer time window. Event-study plots for the samples conditional on a health shock can be found in Appendix Fig. A.4.<sup>32</sup> Fig. 3 shows the event-study estimates, in which June 2012 is used as the baseline. Prior to the 2013 reform, the difference in the DI application probabilities between permanent and temporary workers is fairly constant over time, providing support for the parallel trends assumption. Only in the last month of 2012 the DI risk for temporary workers decreases substantially relative to that for permanent workers. This may be due to some imprecision in the registration date around the months December/January. The gap in DI risk decreases further throughout 2013 and remains constant afterwards.

#### Results

To obtain a single estimate that can easily be interpreted, we focus on our simple DiD estimation (cf. Eq. (3)). Table 4 shows the estimated DiD estimates (conditional on observing a health shock). In contrast to our earlier results (on selection and the risk of illness), here we find that a substantial part of the observed difference in application

 $^{31}$  The regression model, in which June 2012 (M=2012M6) is taken as baseline, is:

$$DI_{it} = \alpha^R + \beta^R FT_{it} + \sum_{M=2010M1}^{2015M6} \delta^R_M FT_{it}I_{t=M} + \gamma^R X_{it} + \varepsilon_{it}.$$

probabilities can be explained by employer responsibilities. For both the sample with mental and the sample with physical health shocks, the DiD estimates are statistically significant and large in magnitude. Whether we include control variables (columns (1) and (3) vs columns (2) and (4)) makes little difference, signifying that the DiD design is unaffected by selection.

To get a sense of the magnitude of the estimates we compare them to the mean dependent variable (row 2), which is the average probability of applying for DI within the respective samples. For the sample that experienced a mental health shock (column 2), the reform reduced the DI application probability of temporary workers with 0.122/0.571 =21%. For the sample with physical illness (column 4), the reduction is 0.041/0.107 = 38%. Both estimates are statistically significant with p-values below 0.01. Results for the full sample (regardless of health shocks) are similar: in column (6) we find a 0.016/0.053 = 30% decrease.

Accordingly, before 2013 differences in employer incentives and obligations during the sick pay period contributed substantially to the gap in the DI application probabilities.<sup>33</sup> At this point, it should be emphasized once more that the 2013 reform did not fully offset the initial differences in employer incentives and obligations that existed up to that point. The estimated reform effect therefore provides a lower bound on the total effect of employer incentives and obligations. Further analyses, as discussed in Appendix A.4, suggest that the majority of the reform effect can be attributed to increased monitoring, while the effect of experience rating appears to be limited.<sup>34</sup>

The DiD estimates could be biased if the reform changed selection into contract type, for example through a reduction of fixed-term contracts being given to relatively unhealthy individuals. However, as can be seen in Fig. 2(a), the reform did not reduce the number of fixed-term or permanent contracts. To further rule out changes in selection into contract type, we interact the reform indicator with all control variables in the DiD specification. Allowing for a differential impact of control variables should absorb any part of the estimated reform effect driven by compositional changes. However, the resulting reform estimates remain similar, indicating a limited role of changes in selection.<sup>35</sup>

The lower panel of Table 4 presents alternative specifications to assess robustness of these results. Using alternative definitions for the health shocks yields DiD estimates of very similar relative magnitude. When considering DI award (instead of application) as the outcome, we find that the impact of the reform is substantially smaller (-0.023/0.377 = -6%). Therefore, conditional on applying for DI, the probability of being awarded DI benefits increased for fixed-term workers. One potential explanation is that increased monitoring affected mostly fixed-term workers who would have been denied DI benefits if they had applied.

#### 3.4. Labour market prospects of ill employees

The significance of the employer's responsibilities in explaining the probability of DI application after illness motivates the final analysis we perform. Employers can play a significant role in the rehabilitation of sick workers, through their ability to facilitate the worker's return

<sup>&</sup>lt;sup>32</sup> Health shocks are only observed from 2012 onward for mental health shocks and from 2011 onward for physical health shocks. As only a short period of time around health shocks is used in the conditional DI regressions, the pre-periods are shorter. Additionally, the smaller samples and uncertainty in the exact timing of the health shocks lead to more noisy estimates.

<sup>&</sup>lt;sup>33</sup> Prinz and Ravesteijn (2020) reach a similar conclusion regarding the impact of the reform on temporary agency workers, a group form a (small) subset of all fixed-term workers.

<sup>&</sup>lt;sup>34</sup> The 2013 reform simultaneously introduced extra financial incentives and placed monitoring obligations on employers. We can extend our DiD model and exploit the fact that the incentive effects were proportional to firm size. In doing so, we can disentangle the importance of the various elements as shown in Appendix A.4. 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%.

Estimation results: Difference-in-differences model for DI applications.

Estimation results, Sincrence in amerence	mouer for br upp	incutionsi				
	DI application conditional on experiencing a mental health shock (> 0 minutes mental healthcare treatment)		DI application conditional on experiencing a physical health shock		Unconditional DI application	
			(< 50th to > 90th healthcare expe	h percentile nditures)		
	(1) No controls	(2) Controls	(3) No control	(4) Controls	(5) No Controls	(6) Controls
Fixed-term*Post-2013 (DiD estimate) Mean dep. variable	-0.146 0.571	-0.122 0.571	-0.044 0.107	-0.041 0.107	-0.018 0.053	-0.016 0.053
Number of observations	4.2 million	4.2 million	10.9 million	10.9 million	475 million	475 million
Alternative specifications: DI award (DiD estimate) Mean dep. variable Alt. health shock 1 <sup>a</sup> (DiD estimate) Mean dep. variable Alt. health shock 2 <sup>a</sup> (DiD estimate) Mean dep. variable	-0.039 0.377 -0.247 1.082 -0.241 1.087	-0.023 0.377 -0.201 1.082 -0.195 1.087	-0.015 0.068 -0.051 0.121 -0.025 0.209	-0.012 0.068 -0.045 0.121 -0.025 0.209	-0.005 0.033	-0.004 0.033

Coefficients from estimating Eq. (3) ( $\delta^R$ ). All estimates are reported as percentage points, i.e. a reported coefficient of -0.146 implies a -0.146 percentage point difference in the monthly probability of applying for DI. All coefficients in this table are statistically significant with p-values < 0.01 and therefore standard errors are omitted for brevity. Columns (2) and (4) contain the same control variables as the regression in Table 2, column 4: quarter-of-year dummies, demographics and job controls. See Appendix Table A.2 for the specific control variables included.

<sup>a</sup> Alternative health shocks 1 and 2 refer to the alternative health shocks listed in Table 3. For mental health shocks, alternative 1 is < 50 minutes to > 200 minutes and alternative 2 is 0 min to > 200 minutes. For physical health shocks, alternative 1 is < 75th to > 90th percentile and alternative 2 is < 50th to > 99th percentile.

to their old position. This may involve necessary adaptations in tasks or working conditions. Temporary workers lack this opportunity and typically need to search for a new job if their health improves during the two-year waiting period. As a result, one might expect that labour market conditions will matter significantly for the importance of employer responsibility. As has been argued in the literature, differences in outside options may well explain the higher propensity to apply for DI benefits among vulnerable groups in the labour market, such as those with fixed-term contracts (Autor and Duggan, 2003).

Given that the sector in which an individual is employed is likely to be endogenous, we do not estimate the impact of labour market tightness on DI applications. Instead, we study this hypothesis by estimating whether the effect of the 2013 reform (as estimated in the previous section) is heterogeneous by labour market tightness of the sector in which the employee was employed.

#### Empirical specification

We consider sector-level labour market tightness as a proxy for the labour market prospects of ill workers. We categorize 70 sectors as "tight" or "slack" based on the percentage of vacancies relative to the number of filled jobs. Approximately 15% of all employment contracts are classified as belonging to a tight labour market.<sup>36</sup>

We incorporate labour market tightness into the DiD specification from the previous subsection Eq. (3) by adding full interactions with an indicator for slack labour markets. Accordingly, we allow the 2013 reform to have different treatment effects in tight and slack labour markets.

$$DI_{it} = \alpha_{0}^{L} + \alpha_{1}^{L}FT_{it} + \alpha_{2}^{L}I_{l>2013} + \alpha_{3}^{L}I_{l>2013}FT_{it} + \alpha_{4}^{L}Slack_{it} + \alpha_{5}^{L}Slack_{it}I_{t>2013} + \alpha_{6}^{L}Slack_{it}FT_{it} + \alpha_{7}^{T}Slack_{it}FT_{it}I_{l>2013} + \alpha^{L}X_{it} + \epsilon_{it},$$
(4)

Superscripts L are added to distinguish the parameters from Eq. (3). The resulting model is essentially a triple-differences model, where we are particularly interested in how the reform effect differs by labour

market tightness. For conciseness, we only report the coefficients  $\alpha_3^L$  (reform effect in tight labour markets) and  $\alpha_7^L$  (difference in reform effect between tight and slack labour markets). The estimates for the other parameters can be found in Table A.5 in the appendix.

### Results

Before turning to the heterogeneity of the reform effect, it is noteworthy that before 2013, temporary workers in slack labour markets faced a substantially larger risk of applying for DI after illness. This was not the case for permanent workers, where there was little difference between tight and slack labour markets in the probability of applying for DI after illness.<sup>37</sup> Even though we include the same set of controls as in previous analyses, there may still be selection (sorting) into different sectors, implying that a causal interpretation of the relation between labour market tightness and DI application risk is difficult. Nevertheless, the strong correlation between tightness and DI application risk for temporary workers points to the potential importance of labour market opportunities. It also supports the idea that the existence of an ongoing employment contract renders alternative labour market opportunities less important in the decision of whether to apply for DI.

Table 5 shows the estimates from Eq. (4) relating to the heterogeneity of the 2013 reform effect. Column (1) provides estimates for the sample with a mental health shock, column (2) for the sample with a physical health shock, and column (3) for all workers. In line with our hypothesis, we find that the introduction of employer responsibilities for temporary workers in 2013 had a much larger impact in slack labour markets than in tight labour markets. In column (1), the reduction in the DI application rate is -0.038 in tight sectors (not statistically significant), while it is -0.038 + (-0.112) = -0.150 in slack labour markets (strongly statistically significant). It appears that a lack of employer responsibilities is particularly detrimental when alternative opportunities in the labour market are scarce. The conclusion is the same when considering the sample with physical health shocks in column (2). Here the impact of the reform is a reduction in DI application probability of -0.016 (not statistically significant) in tight labour

 $<sup>^{36}</sup>$  The categorization of sectors is provided in Appendix Section OA.2, where we also show that the distribution of contract types in tight sectors is comparable to the distribution in slack labour markets.

 $<sup>^{37}\,</sup>$  See Table A.5 in the Appendix for the estimates from Eq. (4) that underlie these findings.

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#### Table 5

Heterogeneity of reform effect by labour market tightness.

	(1)	(2)	(3)
	DI application conditional on experiencing a mental health shock (> 0 minutes mental healthcare treatment)	DI application conditional on experiencing a physical health shock (< 50th to > 90th percentile healthcare expenditures)	Unconditional DI application
Fixed-term*Post-2013	-0.038 (0.045)	-0.016 (0.013)	-0.012 (0.001)
Slack*Fixed-term*Post-2013	-0.112 (0.048)	-0.036 (0.014)	-0.007 (0.001)
Mean dep. variable	0.571	0.107	0.053
Controls	✓	1	✓
Number of observations	4.2 million	10.9 million	475 million
Alternative specifications:			
DI award			
Fixed-term*Post-2013	-0.017 (0.037)	0.011 (0.011)	-0.004 (0.001)
Slack*Fixed-term*Post-2013	-0.014 (0.039)	-0.031 (0.011)	-0.001 (0.001)
Mean dep. variable	0.377	0.068	0.033
Alternative health shock 1 <sup>a</sup>			
Fixed-term*Post-2013	-0.113 (0.125)	-0.036 (0.009)	•
Slack*Fixed-term*Post-2013	-0.133 (0.135)	-0.018 (0.009)	
Mean dep. variable	1.082	0.121	
Alternative health shock 2 <sup>a</sup>			
Fixed-term*Post-2013	-0.109 (0.127)	0.128 (0.141)	•
Slack*Fixed-term*Post-2013	-0.129 (0.137)	-0.189 (0.149)	
Mean dep. variable	1.087	0.209	

Coefficients from estimating Eq. (4) ( $a_3^L$  and  $a_7^L$ ), reported in percentage-points. Standard errors in parentheses. All columns contain the same control variables as the regression in Table 2, column 4: quarter-of-year dummies, demographics and job controls. See Appendix Table A.2 for the specific control variables included.

<sup>a</sup> Alternative health shocks 1 and 2 refer to the alternative health shocks listed in Table 3. For mental health shocks, alternative 1 is < 50 minutes to

> 200 minutes and alternative 2 is 0 min to > 200 minutes. For physical health shocks, alternative 1 is < 75th to > 90th percentile and alternative 2 is < 50th to > 99th percentile.

markets, while it is three times as large (-0.016 + -0.036 = -0.052) in slack labour markets (strongly statistically significant).

Comparing these impacts to the mean of the dependent variable reveals that they are large: for the sample with mental health shocks the reduction in DI applications in slack labour markets is 0.150/0.571 = 26%. For the sample with physical health shocks the reduction in DI applications in slack labour markets is 0.052/0.107 = 49%. Finally, our results also extend to the full sample, where the reform impact is again stronger in slack labour markets (and statistically significant, see column 3, second row). As in the previous section, we report various alternative specifications in the lower panel of Table 5, which all reinforce our findings that the reform impact is stronger (more negative) in slack labour markets.

## 4. Findings in perspective

We have studied four mechanisms that may explain the increased likelihood of applying for and being awarded DI among workers with fixed-term contracts compared to workers with permanent contract. We now combine our findings regarding these four mechanisms and relate them to existing evidence in the literature.

We gather our empirical findings in Fig. 4. The first step of our analysis is concerned with the role of selection in the raw gap in DI applications between temporary and permanent workers. In panel (a), we visualize the key result: the raw gap in monthly DI application probability is substantial (0.013 percentage points, see Table 2), but once we correct for selection, the gap is even larger (0.020 percentage points, or 38% of the mean DI application rate). The part that is due to a selection is negative: mainly because temporary workers are younger on average with correspondingly lower DI application risks. In conclusion, selection into fixed-term contracts does not explain the high DI application rate among temporary workers but partly masks it.

The second factor is the risk of becoming ill for different contract types. In panel (b) of Fig. 4 we reproduce our estimates from Table

3. The estimates show that the risk of a mental health shock is larger for temporary workers, but most of this difference is due to selection. Once controlling for worker and job characteristics, only a small difference remains. The risk of a physical health shock is in fact smaller for temporary workers, but again, the difference is largely explained by observed differences in worker and job characteristics (selection). For both mental and physical health shocks, the remaining fixed-term contract effect is relatively small compared to the average probability of experiencing these shocks (less than 10% of the mean). Based on these findings, we conclude that the higher DI application rate of temporary workers is not explained by a higher risk of falling ill when on a fixed-term contract.

Consistent with a similar risk of falling ill across contract types, panel (c) shows that temporary workers also face a substantially larger DI application probability *conditional on illness*. A majority of the difference in the conditional DI application is not due to selection, both when conditioning on a sample of workers who experienced a mental health shock (left bar) and when conditioning on a sample of workers who experienced a physical health shock (right bar).

In our third step, we consider employer responsibilities during illness, which, prior to the 2013 reform were absent for temporary workers once their contract expired. Exploiting the reform, our differencein-differences estimates in panel (d) reveal that the introduction of such responsibilities significantly reduced the DI application probability for temporary workers (see Table 4 for the estimates). This holds both for workers with mental illnesses (left bar) and physical illnesses (right bar). In addition, the figure demonstrates that selection does not play a substantial role in the difference-in-differences estimation, reinforcing the identification strategy. The impact of the reform is substantial in magnitude, as increased employer incentives reduced the DI risk of temporary workers with approximately 20% and 40% of the mean DI application rate for workers experiencing mental and physical health shocks respectively. These results suggest that the DI application gap opens up after the onset of illness and that the employer role is crucial for facilitating return to employment during the sickness spell.



(d) Reform effect (employer responsibility)

# (e) Reform effect (by tightness)



#### Fig. 4. Summary of findings.

*Notes*: This figure is based on the following estimates. Panel (a): Table 2 coefficient column 1 (in blue), 6 (in green) and the difference between the two (in orange). Panel (b): Table 3 coefficients in column 2 (in green) and the difference between columns 1 and 2 (in orange). Panel (c): Table 3 coefficients in column 4 (in green) and difference between columns 4 and 3 (in orange). Panel (d) sample with mental health shock: Table 4 column 2 (in green) and the difference between column 4 and 3 (in orange). Panel (d) sample with mental health shock: Table 4 column 4 and 3 (in orange). Panel (e) sample with mental health shock: Table 5 column (1) first row coefficient (in yellow) and sum of first and second row coefficients (in purple). Panel (e) sample with physical health shock: Table 5 column (2) first row coefficient (in yellow) and sum of first and second row coefficients (in purple). Panel (e) sample with physical health shock: Table 5 column (2) first row coefficient (in yellow) and sum of first and second row coefficients (in purple). For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

Finally, we extend the DiD analysis to study whether employer responsibilities are particularly important when labour market opportunities are poor (see Table 5). Panel (d) in Fig. 4 visualizes that the DiD estimates are much larger in slack labour markets where job opportunities are scarce (pink bars) than in tight labour markets where alternative employment is easier to find (yellow bars). These findings support the idea that labour market conditions interact with health factors in shaping the decision to apply for DI after a lengthy sickness spell.<sup>38</sup>

The emerging picture from Fig. 4 is that it is mainly the lack of employer responsibilities during illness and differences in labour market prospects that increase the DI application probability of temporary workers. Selection into contract type as well as differences in the risk of falling ill appear less prominent. Additional analysis focusing on DI award rather than DI application supports these conclusions, as it leads to similar results (see Tables 2, 3, 4 and 5). Due to the limited availability of healthcare data for certain years, these conclusions are drawn on analyses using different time windows in each step. Results are similar when we use the consistent (restricted) 2012-2015 time window instead.<sup>39</sup> Additionally, our baseline specifications do not include individual fixed effects as the inclusion of such fixed effects would effectively leave out the vast majority of our sample. As a robustness exercise, we include individual fixed effects in all steps of the analysis. While these fixed effects absorb part (30%-40%) of the explanatory power, the general takeaways remain the same.<sup>40</sup>

One takeaway from our analysis is that employers do not offer fixed-term contracts specifically to workers with health conditions. This contrasts with previous research, which finds that individuals in ill health are less likely to obtain a permanent contract (Wagenaar et al., 2012). Taking a broader perspective, Acemoglu and Angrist (2001) state that the additional employer responsibilities stipulated in the Americans with Disabilities Act (ADA) in the US lowered the chances of disabled workers being hired, but this finding is challenged by Jolls and Prescott (2004). In our setting, 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 argue that employers have limited ability to select on the more severe health issues that could lead to a DI application.

In line with other research, we do find that fixed-term contracts are associated with an increase in the prevalence of mental health problems (Kim et al., 2012; Virtanen et al., 2005; Benach et al., 2014). Nevertheless, once we control for a wide range of observable characteristics, the remaining effect is relatively small compared with the large *associations* found both in this paper and in previous papers. The difference in the prevalence of physical health problems even completely disappears after controlling for these observables. Our results are most comparable to those of Caroli and Godard (2016), who find a strong association between job insecurity and a wide range of health outcomes, but a limited causal impact.

Turning to the sick pay period that precedes potential DI applications, the differences in employer incentives appear crucial for explaining the gap in the DI application probabilities. This confirms earlier work on the effects of employer experience rating, which suggests reductions in DI inflows of 7 to 24% (Prinz and Ravesteijn, 2020; Koning, 2009, 2016; Hawkins and Simola, 2020). Our estimated effect of the 2013 reform is similar in magnitude, but it should be emphasized that in addition to experience rating, the formal monitoring obligations of employers were increased as well. Further investigation of our results suggests that the additional monitoring indeed plays an important role in the impact of the reform.<sup>41</sup>

Finally, our results shed new light on the concept of a 'work disability' to explain changes in DI inflow risks. In the literature, a work disability is commonly defined as the extra inflow into DI schemes induced by unfavourable business cycle conditions (Autor and 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-productivity workers who are also more likely to enter UI. Consistent with this interpretation, our analysis shows that DI application probabilities are higher for low-productivity workers, who are also more likely to have fixed-term contracts, than for permanent contract workers. However, health conditions do matter. Specifically, health conditions are more likely to lead to DI applications by fixed-term workers since they experience less employer commitment during their illness and have less favourable outside options.

#### 5. Conclusion

The aim of this paper is to explain the large difference in DI application probabilities between workers with fixed-term contracts and those with permanent contracts. Using rich Dutch administrative data from various sources, we show that the compositional differences between fixed-term and permanent workers cannot explain the observed differential. Additionally, the risk of falling ill is not substantially higher for fixed-term workers. We observe that most of the gap in the DI application probabilities arises after the onset of illness, and we show that the role of the employer during the sick pay period is crucial. Increased employer responsibility for ill fixed-term workers in 2013 substantially reduced the difference in DI application probabilities. Finally, we find that opportunities in the labour market for sick workers also matter in the decision to apply for DI. Most notably, the probability of applying for DI is higher among fixed-term workers in slack labour markets than in tight labour markets. Among permanent workers, this relationship between labour market tightness and DI application rates is absent. Additionally, we find that the 2013 reform that increased employer incentives for temporary workers had a larger impact in slack labour markets as well.

From a policy perspective, a key takeaway from our analysis is that the higher DI application probability observed among fixed-term workers emerges during the sick pay period that precedes DI applications. In this period, employers play a crucial role in that they may or may not implement work accommodations. Depending on the contract type and the corresponding employer incentives and obligations, different workers with similar health conditions face DI application probabilities that vary substantially. This provides a novel perspective on the concept of 'work disabilities': the economic context and corresponding contract settings do matter, but this is relevant only at the onset of the health problem. While this calls for sufficient commitment from employers of workers with fixed-term contracts, we are aware that the options for how to do so are more limited than for permanent workers. Increased obligations and incentives for employers may have a negative effect on overall employment and could trigger substitution towards the UI scheme and social assistance. Public employment offices could therefore play a more active role in supporting fixed-term workers during their sick pay period. Our findings suggest that this is especially relevant in slack labour markets where job opportunities are limited.

 $<sup>^{38}</sup>$  In the appendix Fig. A.7 we replicate Fig. 4 but divide each coefficient by its corresponding sample mean. This facilitates a comparison of magnitudes. The conclusions are identical, with the only difference being that the effect sizes conditional on physical health shocks are larger in relative terms.

<sup>&</sup>lt;sup>39</sup> Appendix Table A.1 shows the time windows used for every step of the analysis. Results using a consistent time window in every step are available upon request.

<sup>&</sup>lt;sup>40</sup> Full results when individual fixed effects are included are available upon request.

<sup>&</sup>lt;sup>41</sup> Additionally, experience rating was already in place for workers with permanent contracts in 2013, whereas most other papers evaluate the introduction of experience rating in a context in which no experience rating exists.

#### Table A.1

Summary of the four main analysis steps for decomposing the Disability Insurance Application gap between temporary and permanent workers.

Summary of the four	main analysis steps for decom	posing the Disability insurance	Application gap between tempora	iry and permanent workers.	
	1. Compositional differences	2a. Risk of illness	2b. Risk of illness	3. Employer responsibilities during illness	4. Labour market conditions
Research question	Is the difference in DI risk between fixed-term and permanent employees explained by characteristics (worker, firm or job)?	Do fixed-term and permanent employees experience difference risks of (serious) illness?	Is the difference in DI risk between fixed-term and permanent employees explained by the difference in risk of (serious) illness?	Did the introduction of employer responsibilities during illness for temporary workers affect their probability of DI application?	Is the impact of employer responsibilities different depending on labour market opportunities?
Identification	Adding control variables, Oster analysis	Conditional independence (given baseline health, worker-, firm- and job-characteristics)	Difference in DI risk conditional on a negative health shock	Dif-in-dif using the 2013 reform ('BeZeVa')	Dif-in-dif using the 2013 reform ('BeZeVa') + sectoral variation in tightness
Equation	$DI_{ii} = \alpha + \beta FT_{ii} + \delta X_{ii} + \varepsilon_{ii}$	$H_{ii} = \alpha^{S} + \beta^{S} F T_{ii} + \delta^{S} X_{ii} + \varepsilon_{ii}^{S}$	$DI_{ii} = \alpha + \beta FT_{ii} + \delta X_{ii} + \epsilon_{ii}$	$DI_{it} = \alpha + \beta FT_{it} + \delta I_{t>2013}FT_{it} + \gamma X_{it} + \epsilon_{it}$	Same as 3, interacted with a dummy for tight/loose labour markets
Sample	All employed workers	All employed workers with sufficiently low healthcare expenditures/mental health treatment minutes to be able to experience a negative health shock.	All employed workers that experienced a health shock. For mental health shocks occurring in month x, only months x-6 until x+6 are included. For non-mental health shocks occurring in year x, all months in year x plus the six month before and six month after are included.	Same as 2b	Same as 2b
Time period	Jan-2010 to June 2015	Feb-2012 (mental health) or Jan-2011 (non-mental) to June 2015	August 2011 (mental health) or july 2010 (non-mental health) to June 2015	Same as 2b	Same as 2b
Sample size	475 million	286 (mental)/192 million (non-mental)	4 million (mental) /11 million (non-mental)	Same as 2b	Same as 2b
Results	Table 2	Table 3 (col's 1–2)	Table 3 (col's 3-4)	Table 4	Table 5

#### Table A.2

Control variables included in regressions.

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

## CRediT authorship contribution statement

**Pierre Koning:** Writing – review & editing, Writing – original draft, Project administration, Methodology, Funding acquisition, Conceptualization. **Paul Muller:** Writing – review & editing, Writing – original draft, Project administration, Methodology, Conceptualization. **Roger Prudon:** Writing – review & editing, Writing – original draft, Project administration, Methodology, Formal analysis, Conceptualization.

## Appendix A

A.1. Additional results

See Fig. A.1. See Tables A.1–A.4. A.2. Non-parametric analysis of selection into fixed-term contracts

In Section 3.1 and Table 2, we analyse compositional differences between workers with fixed-term and permanent workers. To support these regression results, we now provide results from a non-parametric approach. We do so by reweighting the DI application probabilities for fixed-term workers using the distribution of characteristics among the permanent contract workers.<sup>42</sup> We reweight using 48 cells defined by interacting age group, gender and education level, yielding results that

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

<sup>&</sup>lt;sup>42</sup> Reweighting is performed at the monthly level. For month t, we define the share of the population of permanent workers in group j as





(b) Share of individuals with a fixed-term contract for more than three years, conditional on having a fixed-term contract once



Fig. A.1. Distribution of fixed-term contracts over the age distribution.

Table A.3 Healthcare cost coefficients DI application probability regression (Table 2, column (4)).

-	
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 < 0.01 and therefore standard errors are omitted for brevity.

The number of individual-year observations equals 475 million.

are depicted by the yellow line in Fig. A.2. Most notably, conditioning on these demographic characteristics leads to an even larger difference in the DI application probabilities between fixed-term and permanent workers: it is now almost twice as large (before 2013). Additionally, after the reduction in DI application probabilities in 2013, the probability for fixed-term workers remains well above that for permanent workers. The most important explanation is that both the likelihood of securing a permanent contract and the probability of applying for DI increase with age.

Since the probability of applying for DI is clearly correlated with health status, differences in health status – prior to falling ill – between fixed-term and permanent workers might explain the risk premium. Our large number of observations allows for further reweighting using healthcare expenditures. We interact the previously defined cells with five levels of healthcare expenditure (measured in the calendar year prior to month t - 24) and show the reweighted DI application probabilities as the dark brown line of Fig. A.2. Surprisingly, the results are almost identical to those conditional only on basic demographics. Therefore, once we control for demographics, any remaining health differences are unable to explain the higher probability of applying for DI among fixed-term contract workers. These results confirm the regression results from Table 2.

#### A.3. Event-study analysis for samples with a health shock

The empirical approach for the event-study analysis is visualized in Fig. A.3. While the analysis entails a standard difference-in-differences approach, there are three features that warrant further explanation. First, the outcome of interest is a DI application, which occurs 24 months after the start of illness. However, the new regime introduced with the reform of 2013 applies based on the *date of illness*, not the date of the DI application. Second, our sample only includes individuals that experienced a negative health shock. Since this is an imperfect proxy for the start of sick leave, we include a 12 month period around the health shock for mental health shocks (24 months for physical health shocks). This means that the individual's 12 observations might partly fall before and partly after the reform. Third, individuals might change contract type (fixed-term to permanent) within the 12 months period, thereby switching from treatment to control group.

Fig. A.4 shows the event-study estimates conditional on a mental health shock (left panel) or a physical health shock (right panel). Information on healthcare utilization is only observed from 2012 onwards for mental healthcare and from 2010 onward for physical healthcare. For the construction of health shocks, we must observe healthcare utilization in the previous period as well. This implies that shocks are observed from February 2012 onward for mental healthcare (the first pre-period being January 2012), while for physical healthcare shocks are observed from 2011 onwards (the first pre-period being 2010).

For both samples, we see a reduction in the gap in DI risk between fixed-term workers and permanent workers after 2013. For mental health shocks, the estimates prior to 2013 indicate a parallel trend prior to the reform. Given that the data on mental healthcare utilization only starts in 2012, the pre-period is relatively short.

For physical health shocks, the event-study estimates are more noisy and display some cyclicality. This is most likely caused by the imprecise nature of the data on physical healthcare utilization. Utilization is only measured on an annual basis. To ensure that the actual health shock is included, the conditional analysis includes all months starting in July in the year prior to the shock, until June of the year after the shock.

month *t*. The reweighted DI application probability for fixed-term workers is the weighted sum of the probability for each group:

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

j

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

#### Table A.4

Regression results for a negative mental health shock (> 0 minutes of mental health treatment) or negative physical health shock (< 75th to > 90th percentile) and a subsequent DI application.

	Health shock					DI application			
	(1)	(2)	(3)	(4) <sup>c</sup>	(5)	(6) <sup>d</sup>	(7) <sup>e</sup>	Unconditional	Conditional
Mental health shock									
Fixed-term contract coefficient	0.041	0.015	0.010	0.009	0.009	0.007	0.005	0.019	0.192
Mean dependent variable	0.116	0.116	0.116	0.116	0.116	0.116	0.116	0.053	0.571
Number of observations (in millions)	286	286	286	286	286	286	286	475	4.2
Physical health shock									
Fixed-term contract coefficient	-0.076	-0.007	-0.022	-0.022	-0.023	-0.021	-0.020	0.019	0.047
Mean dependent variable	0.571	0.571	0.571	0.571	0.571	0.571	0.571	0.053	0.107
Number of observations (in millions)	192	192	192	192	192	192	192	475	10.9
Quarter-of-year dummies		Х	Х	Х	х	Х	Х	Х	Х
Demographic controls <sup>a</sup>		х	х	х	х	Х	Х	Х	Х
Job controls <sup>b</sup>			х	х	х	Х	Х	Х	Х
Regional fixed-effects				х					
Sector x education controls					х				
All relevant cross-products						Х	Х		

All coefficients in this table are statistically significant with P values < 0.01 and therefore standard errors are omitted for brevity. See Appendix Table A.2 for the specific control variables included.

<sup>a</sup> Age, gender, nationality and education level.

<sup>b</sup> Wage, number of working hours and sector of employment.

<sup>c</sup> Based on a random subsample of 10 million observations due to computational load.

<sup>d</sup> Cross-products are included based on their predictive power over DI applications and contract type using a double LASSO specification; see Appendix Section OA.1 for details.

<sup>e</sup> Upper bound for selection on unobservable characteristics using (Oster, 2019) analysis:  $\beta^* = \tilde{\beta} - (\beta^0 - \tilde{\beta}) \frac{1.3 * \tilde{R} - \tilde{R}}{\tilde{R} - R^0}$ .



Fig. A.2. DI application probabilities (in percentages).

The inclusion of these buffer months causes cyclical behaviour. If we only include the year of the shock (and not the buffer months), the cyclicality disappears but the absolute risk of applying for DI drops and the total number of observations drops by 50% increasing the noise in the estimates.

The event-study analysis on the conditional samples generally confirms the results of the analysis on the unconditional sample. Prior to the BeZaVa reform in 2013, the difference between fixed-term workers and permanent workers in the probability to apply for DI is fairly constant. This difference drops after 2013 and remains smaller throughout the post-period. As shown in Table 4, a difference-in-difference model with a single post-reform dummy shows that for both samples the difference in DI risk approximately halves and is statistically significant.

## A.4. 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 workers on sick leave 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.<sup>43</sup> 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. Fig. A.5 shows the quarterly estimates in which the first quarter

<sup>&</sup>lt;sup>43</sup> 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.



Fig. A.3. Visualization of difference-in-differences analysis employer responsibility: temporary worker cohorts.

Notes: Each series of dots in the figure visualizes an individual, with each dot representing one monthly observation. For readability the figure only shows individuals with a fixed-term contract. The reform did not affect permanent workers, so their observations always belong to the control group.



Fig. A.4. Impact of employer incentives (2013 BeZaVa reform) on the difference in DI risk between fixed-term and permanent workers.





Fig. A.5. Impact of monitoring (2013 BeZaVa reform) on the difference in DI risk between fixed-term workers at small firms and permanent workers.

of 2012 is used as baseline. The general picture is comparable to the event study performed in Section 3.3. That is, prior to the 2013 reform the gap in DI application probability is constant over time, lending

Fig. A.6. Impact of experience rating (2013 BeZaVa reform) on the difference in DI risk between fixed-term workers at small and large firms.

credence to the parallel trends assumption. The gap 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





Table A.5

Triple-difference model for heterogeneity of reform effect by labour market tightness: all coefficients

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	(1)	(2)	(3)
	DI application conditional	DI application conditional	Unconditional
	on experiencing a mental	on experiencing a physical	DI application
	health shock (> 0 minutes	health shock	
	mental healthcare treatment)	(< 50th to $> 90$ th percentile	
		healthcare expenditures)	
Intercept	1.109 (0.041)	0.259 (0.009)	0.056 (0.001)
Post-2013	-0.082 (0.032)	-0.023 (0.008)	0.000 (0.001)
Slack	-0.064 (0.027)	0.004 (0.006)	-0.001 (0.000)
Post-2013*Slack	0.073 (0.034)	-0.002 (0.009)	0.001 (0.001)
Fixed-term	0.118 (0.036)	0.031 (0.009)	0.008 (0.001)
Fixed-term*Post-2013	-0.038 (0.045)	-0.016 (0.013)	-0.004 (0.001)
Fixed-term*Slack	0.206 (0.038)	0.053 (0.010)	0.009 (0.001)
Fixed-term*Post-2013*Slack	-0.112 (0.048)	-0.036 (0.014)	-0.001 (0.001)
Controls	1	1	1
Number of observations	4.2 million	10.9 million	475 million

Coefficients from estimating Eq. (4). Standard errors in parentheses. All columns contain the same control variables as the regression in Table 2, column 4: quarter-of-year dummies, demographics and job controls. See Appendix Table A.2 for the specific control variables included.

very similar to the DiD effects estimated in Section 3.3. The effect of increased monitoring is approximately -0.014%-points, almost equal to the full effect of the reform (the full effect of the reform equals -0.016%-points).<sup>44</sup> This suggests limited effects of the introduction of experience rating for temporary workers.

To estimate the effect of experience rating, we compare employees with temporary contracts at small firms to employees with temporary contracts 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.<sup>45</sup> Fig. A.6 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.

#### Appendix B. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.labeco.2025.102719.

## Data availability

The data that has been used is confidential.

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<sup>&</sup>lt;sup>44</sup> The total effect is obtained by estimating a standard difference-inddifference regression; we interact employment status with a post-2013 dummy.

<sup>&</sup>lt;sup>45</sup> Note that we estimate the effect of introducing experience rating conditional on increased monitoring.

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