Journal of Health Economics 62 (2018) 134–146

Contents lists available at ScienceDirect

Journal of Health Economics

journal homepage: www.elsevier.com/locate/econbase

How disability insurance reforms change the consequences of health shocks on income and employment[☆]

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ARTICLE INFO

Article history:
Received 15 November 2017
Received in revised form
11 September 2018
Accepted 11 September 2018
Available online 26 September 2018

JEL classifications:
H53
J14
J18

Keywords:
Disability insurance
Health shocks
Difference-in-difference analysis
Work accommodations

ABSTRACT

This paper examines whether Dutch disability insurance reforms have helped or hindered employment opportunities of workers that are facing unanticipated shocks to their health. An important component of the reforms was to make employers responsible for paying sickness benefits and to strengthen their sickness monitoring obligations. This may stimulate preventive and reintegration activities by firms. Using administrative data on hospitalizations, we conclude that both financial incentives and monitoring obligations have substantially lowered DI receipt and increased the employment of workers after a health shock.

1. Introduction

In the past two decades the OECD has regularly voiced concern over the labor market position of people with disabilities and the cost of disability insurance (DI) programs (OECD, 1992, 2003, 2010). Improving the labor market position of people with disabilities is not only important for their own economic well-being, it is also considered essential in addressing the challenges that countries face regarding population aging (OECD, 2010). Recognizing the need for reform, many countries have implemented changes to their disability programs.

The objective of this paper is to examine whether the disability reforms that were implemented in the Netherlands have helped or hindered the continuation of work for individuals with health problems or disabilities. The Netherlands presents an interesting setting because the government fundamentally reformed its disability program. In the 1980s and 1990s the Dutch disability program was considered to be the most out-of-control disability program within the OECD, a status sometimes referred to as the "Dutch disease."[1] To illustrate, in 1990 the Netherlands spent4.7 percent of its GDP on disability insurance – which was 2.2 percentage points higher than

☆ We gratefully acknowledge two anonymous referees for their constructive and valuable comments to the paper. We also have benefitted from the opportunity to present this work at the 5th CAFE Workshop, the 11th World Congress of the Econometric Society, and in a seminar at the Erasmus School of Economics, Rotterdam. Statistics Netherlands has provided access to the data that was used in this project through a remote connection facility. As part of the data agreement, Statistics Netherlands has the right to review the results of our project prior to their dissemination to ensure that the confidentiality of the data is not unintentionally compromised and individual-specific information is not revealed. All errors are our own.

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[1] The phrase "Dutch disease" originally referred to the way in which the manufacturing sector in the Netherlands was adversely affected by discoveries of natural gas in the late 1950s. Meanwhile, it has become an umbrella term for the problems faced by economies with high levels of energy or other natural resources. For labor economists it also refers to the sharp increase in disability rolls in the Netherlands between the 1960s and 1980s.

https://doi.org/10.1016/j.jhealeco.2018.09.004

Norway, the second biggest spender on disability insurance in the OECD – and more than three times as large as the OECD average of 1.3 percent (OECD, 2010). Due to the reforms the disability program transformed from one that merely paid benefits to one in which employers play an important role in reintegrating disabled workers, and spending dropped to less than two percent of GDP in 2010.

An important component of the Dutch reforms was to enhance employer incentives, which was done by making them responsible for paying sickness benefits and by strengthening their sickness monitoring obligations. Especially the latter, as specified in the so-called "Gatekeeper protocol" enacted in April 2002, is widely considered to be the most effective DI reform in the Netherlands (see Section 2 for more details). Another reform entailed the prolongation of the sick pay period for which employers are responsible from one to two years in 2004. The rationale behind enhancing employer incentives was that they could stimulate preventive and reintegration activities, and workplace accommodations for sick and disabled workers, thereby improving their labor market opportunities. In this respect, the Dutch Survey of Working Conditions (NEA) of 2014 reports 17% of the working population receiving one or more workplace accommodations (NEA, 2014). Amongst these work accommodations, the most important ones include physical work adaptations, changes in working time or the working schedule and job changes within the firm.[2] However, as a consequence of the reforms employers are confronted with substantial costs when an employee gets sick. These costs are not only monetary, but also arise from increased monitoring obligations and the difficulty of terminating the contracts of workers with health problems or disability (OECD, 2010, p. 135).

While one would expect increased employer incentives and obligations in the sickness period that precedes DI claims to lower the probability of erroneous admissions into DI, this paper focuses on employed individuals that were not be able to continue working for some period of time and for which firms were supposed to exert preventative and reintegration activities. The idea is thus that these activities should strengthen the position of workers with disabilities – at least those that are employed. In this context, our analysis uses administrative data from hospital admissions to estimate changes in the effects of health shocks on employment and income of (formerly) employed individuals. Our data are from hospital admission records with universal population coverage to define a sudden deterioration of health (or "health shock") as an unscheduled hospital admission that requires immediate treatment. With this information, we utilize variation in health that is less prone to measurement error relative to self-reported health measures and arguably (more) exogenous to labor market status. We thus avoid identification from a self-reported health measure that is possibly endogenous to labor force status (Bound, 1991; Kreider, 1999) or affected by the reforms through changed (social) norms for reporting a disability. We combine the hospital admission records with administrative data from several other sources, which together provide a population-level panel data set with information for every person about their demographic characteristics, health status and labor market outcomes.

Our analysis is most related to the work of García-Gómez et al. (2013), who use similar data to identify the causal impact of acute hospitalizations on employment and income. We extend on this work by studying *changes* in health shock effects across worker cohorts that were hospitalized in 2000 and 2005 in order to evaluate the previously mentioned DI reforms. Our paper also contributes to a limited literature that evaluates policies that incentivize employers to improve labor market outcomes of people with disabilities – see e.g. Koning (2016) for a brief overview. As such, we focus on employer responsibilities that go considerably beyond the imposition of workplace accommodations – like e.g. the 1990 Americans with Disabilities Act (ADA) in the U.S.[3] – and largely apply to the sickness period that precedes DI claims.

We find that the labor market position of workers who experience a health shock has improved after the series of reforms: they are less likely to receive disability insurance benefits and they are more likely to remain employed. Also, we do not find strong evidence pointing at substitution effects into other social schemes as a result of the reform, like increases in UI or social assistance benefits. Based on more year-to-year comparisons of new worker cohorts that faced health shocks, we infer that both the Gatekeeper reform in 2002 and the extension of the sick pay period from one to two years in 2004 have contributed. Overall, the DI reforms implemented by the Dutch government have protected disabled individuals who already have a job. Our analysis thus shows that enhancing employer incentives might be a fruitful way to a more sustainable growth path of DI programs. This confirms reviews of e.g. Autor and Duggan (2006), Autor (2011) and Koning and Lindeboom (2015) that stress the role of employers in enhancing return-to-work of sick listed workers.

The remainder of this paper is organized as follows. Section 2 details the institutional context in the Netherlands. The empirical analysis based on the administrative data is presented in Section3. To put these findings in a broader perspective, Section 4 discusses the descriptive analysis based on the Dutch Labor Force Survey. Finally, in Section 5 we discuss how our findings may be useful for countries, such as the U.S., that face a rapid and unsustainable expansion of the DI beneficiary population.

2. Institutional context

The provision of disability insurance in the Netherlands is mandatory and covers all employees against all income losses resulting from impairments that occurred on the job or elsewhere. Since 2004 workers apply for DI claims after a "waiting period" of two years of sickness. During this period, employers are responsible for the provision of reintegration activities and the continued payment of wages. Next, disability claims are assessed by the Social Security Administration (SSA). Disability benefits depend on the "degree of disability," which is defined as the percentage difference between prior earnings and the remaining potential earnings capacity.

There are three key differences between the disability programs in the Netherlands and the United States. First, unlike in the U.S., workers in the Netherlands may receive partial disability benefits. Hence, Dutch DI beneficiaries may simultaneously work and receive disability benefits. Second, disability benefits only depend on the "degree of disability" and not the number of dependents and/or work history. For fully disabled individuals, disability benefits provide insurance for 70 percent of the loss of income due to impairments. Third, health insurance coverage is universal in the Netherlands and not tied to DI receipt (or a person's job).

[2] For the U.S., Zwerling et al. (2003) report findings on workplace accommodations that are based on the National Health Interview Survey Disability Supplement (NHIS-D). They find 12% of the respondents to receive workplace accommodations.

[3] Various empirical studies have examined the consequences of the ADA, which intends to ban discrimination and mandates "reasonable workplace accommodations." While DeLeire (2000) and Acemoglu and Angrist (2001) find support for adverse effects of the ADA on the employment of disabled workers, Beegle and Stock (2003) and Kruse and Schur (2003) challenge these findings. Bound and Waidmann (2002) provide suggestive evidence the rapid growth of the DI program during the 1990s played an important role in explaining the decline in the employment rate of people with disabilities.

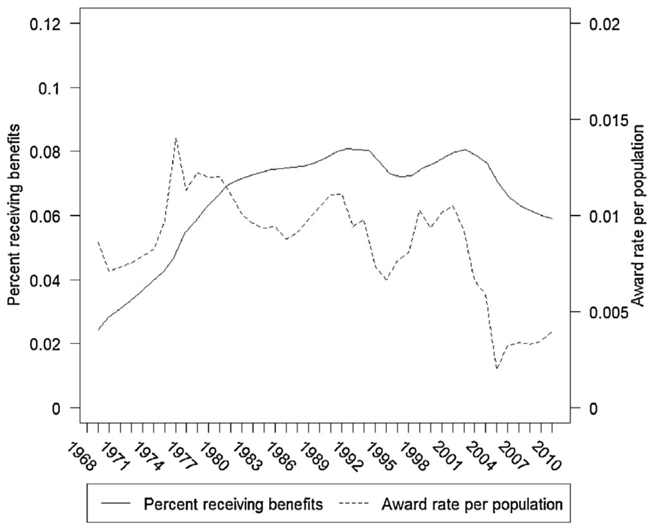

Fig. 1. Disability Insurance Recipiency and Award Rates per Adult Aged 20–65. Source: Author's calculations from data of Statistics Netherlands which are publicly available through statline.cbs.nl

There is good reason to believe the Dutch disability program has been plagued by moral hazard problems in the past. To illustrate, Fig. 1 plots the fraction of the working-age population (age 20–65) that receives DI benefits and the fraction that is newly awarded DI benefits for the period 1968 – 2010. The fraction of the working-age population receiving DI benefits quickly rose from 2 percent in 1968 to about 7–8 percent in the mid 1980s, remained roughly constant at this unprecedented level for the next two decades, and started to decline at the beginning of the 21st century.

One important institutional feature that gave leeway to the sharp rise in enrollment is that disability insurance includes all medical contingencies. Parsons (1991) argues that a broad definition of disability risks increases the likelihood of screening errors into disability determinations. As it seems, applicants successfully exploited this feature of the Dutch DI system, with the Social Security Administration prioritizing on minimizing erroneous denials (Burkhauser et al., 2008). In addition, moral hazard effects were aggravated by sickness benefits that fully replaced wages during the waiting period. As such, incentives to resume work quickly were limited.

Although a continuous need for reform in the DI system was felt since the eighties, it took until 2002 to attain substantial decreases in the inflow rate. Prior to 2002, policy changes aimed at increasing the financial incentives for employers to reduce DI enrollment. In particular, employers became responsible for wage payment of sick listed workers in 1996, and DI benefit costs are experience rated for permanent workers since 1998. Also, the wage payment period was extended to two years in 2004. While there is evidence that these incentives have gradually contributed to more preventative and reintegration activities of employers, the introduction of the so-called "Gatekeeper protocol" (in April 2002) is generally considered as the most effective policy change that has taken place. The Gatekeeper protocol specifies all the legal responsibilities of employers and their sick listed workers. This means the Social Security Administration is no longer involved in the process of checking and reintegrating sick workers, but merely acts as a gatekeeper of the DI program. The Gatekeeper protocol forces employers to focus their attention at the onset of a sickness period. In contrast with the gradual impact of increased employer incentives in the nineties, it seems that the protocol has had an almost immediate and persistent impact on the DI inflow rate (De Jong et al., 2011; Koning and Lindeboom, 2015).[4]

To understand why the Gatekeeper protocol has been successful in curbing DI inflow, it is important to realize that it prescribes a series of actions that should be taken in order to be eligible for DI benefits. After six weeks of absence, the employer and employee should make a first assessment on the medical cause and the functional limitations. On the basis of this assessment, they draft an accommodation and rehabilitation plan (or "reintegration plan") that specifies the steps that should be taken to resume work at the current or new job and the accommodated circumstances that are needed for this. At the same time, a case manager of the Social Security Administration is appointed and dates are determined at which the plan will be evaluated. The plan should be finalized in the eighth week of absence. If the worker has not returned to work after about three months prior to the end of the sickness waiting period, he files a DI benefit claim. The case manager decides whether the reintegration efforts of the employer and employee have been sufficient. If this condition is met, a doctor from the Social Security Administration determines the degree of disability of this worker at the end of the waiting period. In case of negligence, the employer is held responsible and has to continue providing sick pay for a maximum of twelve months.

In light of the dramatic decrease of new DI awards since 2002 (see also Fig. 1), Koning and Lindeboom (2015) argue that the protocol has accelerated the cost- and risk awareness of employers, as well as the specific ways that are needed for disability prevention. Short-term absenteeism could no longer be left unnoticed by managers, with the protocol providing guidance to employers in their new role in the reintegration process. As it seems, employers have become more aware of the costs of two years of continued wage payments, as well as the DI benefit costs that were passed through in their DI premiums. Not surprisingly, however, criticism against the employer incentives and obligations has grown as well. Similar to experiences with the ADA in the U.S. that mandates employers to provide reasonable accommodations to employees with disabilities, the additional responsibilities may have induced employers to hire new workers with a low risk of moving into poor health, thus reducing the costs associated with sickness or disability.

Persons who are not awarded disability insurance benefits may instead apply for unemployment insurance (UI) benefits if they are unemployed. During the period 2000–2010 the UI replacement rate was 70 percent. The maximum entitlement period was dependent on a person's employment history, and ranged from a minimum of six months to a maximum of five years. If a person is not eligible for either disability or unemployment insurance, he may apply for social assistance. This pays substantially lower benefits which are unrelated to previous earnings, and is means tested. With the exception of a reduction in the maximum duration from 60 to 38 months in October 2006, there have been no major reforms in the UI program during the period of analysis.

Finally, health insurance coverage was universal in the Netherlands during the period of analysis. Moreover, and more importantly, the Dutch health insurance system, by contrast to the DI system, has never been perceived as a source of labor market distortions.

[4] In particular, the DI inflow rate dropped from 0.71% to 0.52% of the insured population one year after the start of the Gatekeeper protocol, in 2003 (Statistics Netherlands, 2017). As the number of sickness absences did not decrease noticeably during those years, it seems that the protocol mainly shortened the length of sickness spells and thus lowered the probability of DI applications (absence incidence and duration data can be retrieved from cbs.overheidsdata.nl/70812ned).

3. Data and empirical implementation

3.1. Data and sample selection

In our empirical analysis, we use rich administrative data from hospital admission records, social security records, and the municipality registers, which can be linked through a unique identifier for each individual. Taken together, they provide a population-level panel data set with information for every person about their demographic characteristics, hospital admissions, and labor market outcomes.

The hospital register data contains information on both inpatient and daycare patients of almost all hospitals in the Netherlands from 1999 to 2005. For each hospital admission we observe (i) the admission and discharge data, (ii) whether or not it was an acute admission, and (iii) the main diagnosis. We follow García-Gómez et al. (2013) and identify a sudden decline in health (or "health shock") as an unscheduled hospitalization that requires immediate treatment and involves a stay of at least three nights. The admissions are required to involve a stay of at least three nights because unscheduled and acute hospitalizations may include less severe health problems such as a mild head injury. We also exclude hospitalizations due to pregnancy and childbirth. Due to the unscheduled and acute nature of the hospital admissions it is plausible that they are exogenous to labor market outcomes.[5]

We define workers who experienced an unscheduled and acute hospitalization of at least three nights, excluding those related to pregnancy and childbirth, as the "treatment group." Workers who did not experience a hospitalization at all form the "control group." This means that workers with types of hospitalizations other than those in the treatment group are excluded from the analysis.

Treatment and control cases are further restricted to persons who in the year prior to the potential health shock were: (i) aged 25–58, (ii) working – excluding those who are on disability benefits in the year of the shock, since they must have been on sickness benefits in the year before the shock, and (iii) not admitted to a hospital.[6] These sample selection criteria are similar to the ones imposed by García-Gómez et al. (2013). Following Borghans et al. (2014), we also exclude all individuals who appear on more than one disability scheme at the same time (within a year; about 3 percent of the sample), because it is not clear whether they result from administrative or coding errors, or whether these persons are truly entitled to multiple different DI schemes. By excluding these cases, we focus on workers for which we are sure that the disability reforms apply – that is, the Gatekeeper protocol and the extension of the sick pay period.

3.2. Empirical implementation

To investigate the effects of the DI reforms on the employment opportunities of people with health problems or disability, we compare the effect of a health shock in a year before the reforms to the effect of a health shock in a year after the reform. Our focus is on 2000 and 2005 as the two years in which a health shock can potentially occur. Given the sample selection criteria described before and data availability, 2000 is the earliest and 2005 the latest possible year. Moreover, and more importantly, people who get disabled in 2000 are not affected by introduction of the Gatekeeper protocol (in 2002) and prolongation of financing sick pay for employers (in 2004), whereas those who get disabled in 2005 are. Recall that especially the introduction of the Gatekeeper protocol is considered to be the most effective DI reform in the Netherlands, and that its incentives enhanced as a result of the extension of the sickness benefit period from one to two years in 2004. This selection results in a sample of 31,386 unscheduled and acute hospitalizations with a stay of at least three nights in 2000, and 27,911 hospital admissions in 2005. As we expect that the impact of health shocks varies by age and gender, we will conduct separate analyses for men aged 25–39, men aged 40–58, and for women in these two age groups.[7] We conjecture that older people are more likely to experience a more serious deterioration of health, and have fewer incentives to invest in work resumption due to lower remaining working-life expectancy (Charles, 2003).

Table 1 shows the relative frequency of the unscheduled admissions in 2000 by diagnoses on the basis of the International Classification of Diseases 9 (ICD-9) for four demographic groups, both for all observations and for the subset that entered into DI in 2001. Descriptive statistics for 2005 look very similar and are therefore omitted. Not surprisingly, there are many more unscheduled hospitalizations among men and women aged 40–58 than among their counterparts aged 25–39. Moreover, there are notable differences in the relative importance of certain diseases. For example, for men aged 40–58, diseases of the circulatory system are the most important cause of hospitalization and account for 36 percent of all admissions, whereas for younger men this is only 10 percent. Moreover, circulatory diseases account for 9 percent of the hospital admissions among women aged 25–39, whereas it accounts for 21 percent of the hospital admission among women aged 40–58. For the latter group, circulatory diseases are also the most important cause of hospitalizations, closely followed by diseases of the digestive system. By contrast, injuries are the most important cause of a hospital admission among men aged 25–39, accounting for 27 percent of the admissions. Among all other groups, injuries account for only 14 percent of the admissions. These examples illustrate that there are marked differences in the causes of hospital admission between men and women, and between age groups.

Of the sample of unscheduled hospital admissions in 2000, 7 percent entered into DI in 2001; this group constitutes about 4 percent of the total number of DI admissions in that same year. Although differences in the registration of the data render it difficult to make comparisons between the importance of medical conditions in the hospital records and the Social Security Administration (SSA), two broader observations stand out. First, both the share of mental diseases and the share of musculoskeletal diseases of workers with unscheduled hospitalizations that were awarded DI benefits are markedly lower than for the DI inflow at large. In particular, the share of awardees with mental impairments ranges between about 30 percent for older men to 45 percent for younger women, whereas these shares do not exceed 16 percent in the smaller sample who were hospitalized in the year prior to admission in the DI program. Likewise, less than 8 percent of those hospitalized in 2000 and receiving DI in 2001 were admitted to hospitals due to diseases of the musculoskeletal system, whereas it constitutes a much larger share among all DI admissions.[8] Sec-

[5] It should be noted that mental disorders are underreported in the health shocks we study. As such, we miss an impairment type, which is one of the major contributor to DI inflow nowadays (see also Table 1). With employment rates that are generally lower than for any other type of impairment – see e.g. OECD (2010) – there is a strong case for increased research effort for this particular group.

[6] In order to have a sample that has a strong labor force attachment, we restrict our sample to workers below the age of 59. In the time period under investigation, older workers first option to retire early was typically at the age of 60 – see e.g. Vermeer (2013). Bearing in mind that the waiting period for DI applications was extended to two years, the age cutoff of 58 thus prevents us from investigating shock effects for workers that may have opted for early retirement.

[7] Tables A.1 and A.2 in the appendix to this paper show summary statistics for these four groups, both for the cohort of 2000 and of 2005.

[8] It is likely that part of the workers being hospitalized with injuries (or poisoning) are diagnosed as having musculoskeletal conditions in the DI claims assessment.

Table 1
Treatment cases by diagnosis for four demographic groups in 2000.

	Men				Women			
	Age 25-39		Age 40-58		Age 25-39I		Age 40-58	
	All	DI	All	DI	All	DI	All	DI
Infectious diseases	5.95	2.87	2.52	1.70	4.60	1.38	2.64	0.38
Neoplasms	1.72	5.46	3.90	3.71	2.68	4.84	6.57	8.76
Endocrine disorders	1.98	1.44	1.42	0.95	1.94	2.08	1.40	2.48
Diseases of the blood and blood-forming organs	0.64	0.57	0.45	0.32	1.02	2.08	0.87	0.95
Mental disorders	2.75	8.62	1.58	3.50	4.70	15.22	2.77	5.71
Diseases of the nervous system	2.93	4.31	2.65	3.82	2.90	3.81	2.62	3.24
Diseases of the circulatory system	9.99	16.09	36.46	47.51	8.65	15.22	21.03	28.38
Diseases of the respiratory system	9.58	4.02	5.58	2.23	8.45	4.15	7.10	6.67
Diseases of the digestive system	19.58	6.61	14.56	4.88	22.37	9.69	19.06	8.19
Diseases of the genitourinary system	2.35	0.86	2.27	1.17	10.18	4.50	5.23	2.86
Diseases of the skin	2.89	0.29	1.29	0.42	2.04	1.04	1.30	1.90
Diseases of the musculoskeletal system	5.42	6.32	4.12	4.03	4.38	7.61	4.71	7.81
Congenital anomalies	0.19	0.29	0.17	0.00	0.10	0.35	0.12	0.19
Symptoms, signs, and ill-defined conditions	7.43	6.03	8.82	8.27	11.57	8.65	9.96	7.43
Injury and poisoning	26.60	36.21	14.22	17.50	14.40	19.38	14.62	15.05
Major health event	5.51	13.22	25.16	29.37	2.76	6.93	12.88	18.48
Chronic illnesses	26.26	31.61	28.04	35.10	27.48	45.67	29.62	41.33
Accidents	68.23	55.17	46.80	35.53	69.75	47.40	57.50	40.19
Observations	7,026	348	13,484	943	4,890	289	5,986	525

Notes: percentages are calculated from treatment cases used in the estimation, and are thus restricted to unscheduled and acute hospital admission that last at least three nights, to persons in the relevant age range, who were working and not hospitalized in the previous year.

ond, we find that the shares of hospitalized workers due to diseases of the circulatory system and the digestive system are overrepresented compared to the DI inflow at large. This suggests that the health shocks we study are less strongly related to work hazard than those ending in DI admission. At the same time, of course, the comparison indicates that medical conditions that require hospital admission often deviate from those that justify DI admission at a later stage.

When specifying the effect of health shocks on employment and other outcome measures, we first adopt a difference-in-difference structure that focuses on the difference in the “health shock – no health shock” outcomes between 2005 and 2000. We interpret this difference in the light of the major DI reforms that have taken place. This yields the following model:

$$Y_{it} = \alpha_t + \beta_t S_i + \gamma_t T_i + \delta_t (S_i \cdot T_i) + \theta'_{st} X_i + \varepsilon_{it}, \quad t = 1,\ 2,\ 3,\ 4, \tag{1}$$

where i refers to the person; t to the number of years passed since the year of the shock; S_i indicates whether or not person i had a health shock; T_i equals 1 if the year of the health shock is 2005, and zero otherwise. α_t represents the baseline level of the outcome measure at time t. X is a vector of covariates, including dummies for five year age groups, nationality (Native, Nonnative non-Western, Nonnative Western), household size, municipality size, province, and labor income from the year prior to the possible health shock (in quartiles). The effects of these covariates are allowed to differ both by the time passed since the possible health shock, and by whether or not a person actually had a health shock (as indicated by the subscripts s and t on θ). Furthermore, the subscript t on the other parameters indicates that we allow the effects to differ by the time passed since the health shock. Instead of estimating the model for each t, we pool all observations and cluster standard errors on the individual level. The parameters $(\beta_1, \beta_2, \beta_3, \beta_4)$ give the effect of a health shock in 2000. The parameters of main interest are $(\delta_1, \delta_2, \delta_3, \delta_4)$ which give the difference in “health shock – no health shock” outcomes between 2005 and 2000. If the Gatekeeper reform (in 2002) and the prolongation of the sick pay period (in 2004) have been effective in reducing the extent to which ill health reduces employment opportunities and a person's earnings capacity, we expect these parameters to have a positive sign for employment and labor income.

Essentially, there are two assumptions that underlie our empirical strategy. As we follow a difference-in-difference strategy, the first one is that individuals in the treatment group and control group have common time trends in the absence of a health shock. To address this issue, García-Gómez et al. (2013) analyze differences in pre-treatment trends of hospitalized individuals and their matched controls on similar data as ours. They find no differences in pretreatment income trends of hospitalized individuals and their matched controls. Moreover, they show that their baseline results do not change if they use propensity score matching for treatment and control groups. Our second assumption concerns the possibility of interaction effects between health shock effects and the business cycle. In particular, we assume that annual differences in the magnitude of shock effects cannot be attributed to business cycle effects. To relax this assumption, we will therefore also estimate a model that includes the unemployment rate at the province level (province dummies are no longer included). Specifically, we will estimate the following model to test for the robustness of our findings:

$$Y_{it} = \alpha_t + \beta_t S_i + \gamma_t T_i + \delta_t (S_i \cdot T_i) + \theta'_{st} X_i + \rho_t U_i + \varphi_t (S_i \cdot U_i) + \varepsilon_{it}, \quad t = 1, 2, 3, 4, \tag{2}$$

where U_i is the unemployment rate in the province in which individual i is living in the year of the health shock.

We stated earlier that the initial focus in our empirical analysis is on comparisons of worker cohorts that are observed in 2000 and in 2005 and are followed in subsequent years. As such, our aim is to identify the *overall* impact of policy changes in sick pay and DI between 2000 and 2005.

3.3. Results

Tables 2–5 report the estimation results of our model for younger men, older men, younger women and older women, respectively. For each demographic group, we assess the effect of

Table 2
Regression estimates for men 25–39.

	DI	UI	SA	Employed	Earnings (x €10,000)
Effect health shock in 2000					
1 year later	0.046***	0.014	0.004	−0.036**	−0.046
	(0.007)	(0.008)	(0.004)	(0.012)	(0.078)
2 years later	0.049***	0.024	0.004	−0.052***	−0.131
	(0.009)	(0.009)	(0.004)	(0.014)	(0.071)
3 years later	0.039***	0.017	0.005	−0.059***	−0.164
	(0.010)	(0.009)	(0.004)	(0.015)	(0.085)
4 years later	0.036***	0.025	−0.001	−0.050**	−0.160
	(0.010)	(0.010)	(0.005)	(0.016)	(0.082)
Effect health shock in 2005 - effect health shock in 2000					
1 year later	−0.046***	−0.005	0.001	0.010	0.016
	(0.003)	(0.004)	(0.002)	(0.005)	(0.029)
2 years later	−0.031***	0.001	−0.003	0.012	−0.017
	(0.004)	(0.004)	(0.002)	(0.006)	(0.032)
3 years later	−0.024***	−0.003	−0.002	0.021**	0.012
	(0.004)	(0.004)	(0.002)	(0.007)	(0.041)
4 years later	−0.023***	−0.004	0.001	0.017	−0.014
	(0.004)	(0.005)	(0.002)	(0.007)	(0.037)

Notes: Standard errors are reported in parentheses. *, **, and *** denote significance at the 10, 5, and 1% level, respectively, and are based on Holm adjusted p-values for multiple testing. The dependent variable is an indicator for DI receipt. All regressions include dummies for five year age groups, nationality, household size, municipality size, province, and labor income from the year prior to the possible health shock (in deciles), as well as interactions of these variable with an indicator for an unscheduled and acute hospitalization.

Table 3
Regression estimates for men 40–58.

	DI	UI	SA	Employed	Earnings (x €10,000)
Effect health shock in 2000					
1 year later	0.064***	0.015**	0.000	−0.039***	−0.170
	(0.005)	(0.005)	(0.002)	(0.008)	(0.073)
2 years later	0.079***	0.019**	0.004	−0.070***	−0.034
	(0.008)	(0.006)	(0.002)	(0.010)	(0.107)
3 years later	0.086***	0.022**	0.004	−0.089***	−0.121
	(0.008)	(0.006)	(0.002)	(0.011)	(0.082)
4 years later	0.083***	0.026	0.003	−0.101***	−0.201
	(0.008)	(0.007)	(0.002)	(0.012)	(0.084)
Effect health shock in 2005 - effect health shock in 2000					
1 year later	−0.064***	−0.008***	0.001	0.010***	−0.048
	(0.002)	(0.003)	(0.002)	(0.005)	(0.033)
2 years later	−0.036***	−0.005	−0.003	0.012***	−0.107
	(0.003)	(0.003)	(0.002)	(0.006)	(0.038)
3 years later	−0.042***	−0.003	−0.002	0.021***	−0.037
	(0.004)	(0.003)	(0.002)	(0.007)	(0.037)
4 years later	−0.044***	−0.002	0.001	0.017***	−0.022
	(0.004)	(0.003)	(0.002)	(0.007)	(0.038)

Notes: Standard errors are reported in parentheses. *, **, and *** denote significance at the 10, 5, and 1% level, respectively, and are based on Holm adjusted p-values for multiple testing. The dependent variable is an indicator for DI receipt. All regressions include dummies for five year age groups, nationality, household size, municipality size, province, and labor income from the year prior to the possible health shock (in deciles), as well as interactions of these variable with an indicator for an unscheduled and acute hospitalization.

health shocks on the probability of DI benefit receipt, Unemployment Insurance (UI) benefit receipt, Social Assistance (SA) benefit receipt, employment and employment earnings. At this point, it should be stressed that the P-values that are reported for the health shock dummies – denoted as (β_1, β_2, β_3, β_4) for the effect of a health shock in 2000 (δ_1, δ_2, δ_3, δ_4) for the difference in health shock outcomes between 2005 and 2000 — are adjusted for multiple testing. In particular, we use the Holm correction method on these eight dummies for all (five) outcome measures of interest.

Table 2 shows the estimation results for men aged 25–39. For this group the probability to receive DI benefits is 4 percentage points higher in the year after a health shock, it increases to 4.9 percentage points in the next year, and then declines to 3.5–4 percentage points in the following two years. This effect is mirrored by a reduction in the employment probability of a similar magnitude. Also, the reduction in earnings losses after a health shock is 3500 euros initially and 4400 euros after four years. These outcomes are comparable to those obtained by García-Gómez et al. (2013). Finally, there is no statistically significant evidence that younger men substitute between DI benefits and UI or SA benefits after a health shock (see e.g. Borghans et al., 2014).

Moving to the lower panel of the same table, the next set of estimates show the difference of the effect of a health shock between 2005 and 2000 (the δ coefficients of model (1)). The fact that the estimates for the probability of DI benefits in the first year after the shock are larger than for the remaining period is to be expected because since 2004 an ill person needs to wait two years before becoming eligible to receive DI benefits. Two years after a health shock, a man aged 25–39 is 3.1 percentage points less likely to receive DI benefits and this effect declines slightly in the next two

Table 4
Regression estimates for women 25–39.

	DI	UI	SA	Employed	Earnings (x €10,000)
Effect health shock in 2000					
1 year later	0.030***	0.020	−0.010	−0.024	−0.076
	(0.008)	(0.009)	(0.006)	(0.015)	(0.037)
2 years later	0.045***	0.024	−0.005	−0.014	−0.043
	(0.011)	(0.010)	(0.006)	(0.018)	(0.045)
3 years later	0.043***	0.014	−0.004	−0.028	−0.095
	(0.012)	(0.010)	(0.005)	(0.020)	(0.051)
4 years later	0.051***	0.019	−0.001	−0.031	−0.090
	(0.013)	(0.012)	(0.006)	(0.021)	(0.056)
Effect health shock in 2005 - effect health shock in 2000					
1 year later	−0.052***	−0.015**	0.001	0.009	0.009
	(0.003)	(0.005)	(0.003)	(0.007)	(0.016)
2 years later	−0.043***	−0.008	−0.001	0.024	−0.008
	(0.005)	(0.005)	(0.003)	(0.008)	(0.019)
3 years later	−0.036***	−0.000	0.003	0.016	−0.028
	(0.005)	(0.005)	(0.003)	(0.009)	(0.022)
4 years later	−0.031***	−0.006	−0.001	0.018	−0.040
	(0.006)	(0.005)	(0.003)	(0.009)	(0.024)

Notes: Standard errors are reported in parentheses. *, **, and *** denote significance at the 10, 5, and 1% level, respectively, and are based on Holm adjusted p-values for multiple testing. The dependent variable is an indicator for DI receipt. All regressions include dummies for five year age groups, nationality, household size, municipality size, province, and labor income from the year prior to the possible health shock (in deciles), as well as interactions of these variable with an indicator for an unscheduled and acute hospitalization.

Table 5
Regression estimates for women 40–58.

	DI	UI	SA	Employed	Earnings (x €10,000)
Effect health shock in 2000					
1 year later	0.060***	0.010	0.003	−0.032	−0.129**
	(0.008)	(0.007)	(0.004)	(0.012)	(0.037)
2 years later	0.085***	0.002	0.005	−0.060***	−0.145***
	(0.013)	(0.009)	(0.004)	(0.015)	(0.040)
3 years later	0.096***	−0.001	0.008	−0.076**	−0.129*
	(0.014)	(0.009)	(0.004)	(0.017)	(0.044)
4 years later	0.093***	−0.001	−0.002	−0.079***	−0.016
	(0.014)	(0.010)	(0.004)	(0.017)	(0.049)
Effect health shock in 2005 - effect health shock in 2000					
1 year later	−0.078***	−0.018***	−0.000	0.008	0.008
	(0.004)	(0.003)	(0.002)	(0.005)	(0.015)
2 years later	−0.071***	−0.007	−0.001	0.012	0.008
	(0.005)	(0.004)	(0.002)	(0.006)	(0.018)
3 years later	−0.074***	−0.002	−0.001	0.006	0.015
	(0.006)	(0.004)	(0.002)	(0.007)	(0.019)
4 years later	−0.066***	0.000	0.001	0.002	−0.040
	(0.006)	(0.004)	(0.002)	(0.007)	(0.022)

Notes: Standard errors are reported in parentheses. *, **, and *** denote significance at the 10, 5, and 1% level, respectively, based on Holm adjusted p-values for multiple testing. The dependent variable is an indicator for DI receipt. All regressions include dummies for five year age groups, nationality, household size, municipality size, province, and labor income from the year prior to the possible health shock (in deciles), as well as interactions of these variable with an indicator for an unscheduled and acute hospitalization.

years. This change goes together with an increase in the employment probability of younger men, with an effect estimate of about 2 percentage points. In part, these effects reflect the fact that after 2004 the employer is responsible for the first two years of sick pay, together with the fact that in our data workers who receive sickness benefits are classified as being employed. In light of this, it is important to note that our results show that the improvement in the relative employment probability persists and even increases after two years. This suggests that the DI reforms have not been at the costs of the well-being of workers with health problems, and thus have been successful in targeting the program to younger men having substantial health problems.

Table 3 shows the estimation results for men aged 40–58. If this group of workers faces a sudden deterioration of health, initially the probability of DI receipt increases by 6.4 percentage points, which increases to 7.9 percentage points in the second year, and increases further to around 8.5 percentage points in the third and fourth year. Older men are also about 10 percentage points less likely to work after a health shock in the fourth year after the shock. This finding is similar to Trevisan and Zantomio (2016) who, based on ELSA and SHARE data, find that a first acute health shock results in a 10-percentage point reduction in labor market participation among older European men. Our findings also suggest that health shocks decrease the amount of earnings and increase the likelihood of UI benefit receipt for older men.

Clearly, the effects on DI benefits and employment effects we find are markedly higher for older men than for their younger counterparts. One explanation may be that younger people have stronger incentives to return to the labor force because they have fewer options to replace lost income. In addition, younger workers are significantly less likely to suffer from a circulatory disease or to get cancer, illnesses that may lead to a longer (or permanent) withdrawal from the labor force. Thus, the nature of the health shock may also explain why the employment effects are smaller for younger men and women.

We next turn to the lower panel of Table 3, which shows higher reductions in the health shock effects on DI benefit receipt than for younger men. In particular, four years after the onset of a health shock the effect is 4.4 percentage points smaller. At the same time, the employment effect of health shocks is about 2 percentage points, which is comparable to the employment effect for younger men. This indicates that about half of the decrease in DI benefit receipt among older men consisted of increases in work resumption or work continuation.

Finally, Tables 4 and 5 show that the probability to receive DI benefits after a health shock is initially smaller for women in both age groups compared to their male counterparts. However, in the third or fourth year after the health shock, the probability to receive DI benefits is higher among women. The employment effects of health shocks tend to be smaller for young women than young men, while earnings responses are insignificant. As for the difference in health shocks between the cohorts of 2000 and 2005, the reduction in the effects on DI receipt are 3.1 and 6.6 percentage points for younger and older women, respectively. The results further indicate evidence of employment effects among younger women, with size effects between 1 and 2.5 percentage points. For older women, the decrease in DI receipt is not mirrored by any increases in employment.

The results so far indicate that after the DI reforms it has become less likely to enter the disability program for all demographic groups. Furthermore, individuals did not substitute to the UI or SA program because of the reforms. Even though coefficient estimates are not always statistically significant if we adjust for multiple testing, we also conclude that employment increases were proportional to the decreases in DI receipt, except for older women. As earnings did not increase, the employment increases are probably associated with increases at the extensive margin, rather than hours increases for those workers remaining employed after a health shock

3.3.1. *Do effects differ by type of health shock?*

Next, we further examine the role of the nature of the health shock. In his analysis of the effects of changes in health status on health insurance coverage and labor market outcomes, McClellan (1998) makes an interesting distinction between (i) major health events, (ii) chronic illnesses, and (iii) accidents. Major health events – such as cancer, heart attack, or stroke – have a substantial immediate effect and imply long-term functional limitations. Chronic illnesses – such as diabetes, lung disease, arthritis, or heart failure – generally only moderately limit current functioning, but may result in more severe impairments due to progression of the disease. Finally, accidents have substantial immediate effects on functioning, but are less likely to result in severe long-term impairments.

Table 1 shows that among the young men and women who experience a health shock, around 5 percent has a major health shock. 25 percent is due to a chronic illness, and the remaining 70 percent are due to accidents. Among men aged 40–58 who are hospitalized, 25 percent is due to a major health event, 28 percent due to a chronic illness, and 47 percent due to an accident. Among women aged 40–58 the distribution is significantly different, with only 13 percent of the hospitalizations due to a major health event, 30 percent due to a chronic illness, and 57 percent due to accidents. Major health events are thus considerably more important among men aged 25–39 compared to women in that age category. Major health events are also much more important among older men and women relative to their younger counterparts. The distribution is very similar for both years.

Tables 6 and 7 report the effect of an unscheduled and urgent hospitalization in 2000 as well as the differential effect of this in 2005 measured four years afterwards – both on the probability to receive DI benefits and the probability to be employed, respectively. To start with, the effect of a major health shock on the probability

Table 6
Regression estimates for the probability to receive Disability Insurance benefits four years after a health shock.

	Men		Women	
	Age 25–39	Age 40–58	Age 25–39	Age 40–58
Effect of major health shock in 2000				
	0.109***	0.154***	0.165***	0.182***
	(0.023)	(0.015)	(0.041)	(0.027)
Effect of major health shock in 2005 - effect of major health shock in 2000				
	−0.044*	−0.063***	−0.016	−0.098***
	(0.025)	(0.009)	(0.047)	(0.019)
Effect of onset chronic illness in 2000				
	0.070***	0.154***	0.154***	0.193***
	(0.018)	(0.015)	(0.025)	(0.024)
Effect of onset chronic illness in 2005 - effect of onset chronic illness in 2000				
	−0.035***	−0.049***	−0.066***	−0.086***
	(0.009)	(0.008)	(0.013)	(0.013)
Effect of accident in 2000				
	0.038**	0.083***	0.065***	0.077***
	(0.017)	(0.014)	(0.024)	(0.023)
Effect of accident in 2005 - effect of accident in 2000				
	−0.016***	−0.028***	−0.018***	−0.046***
	(0.005)	(0.005)	(0.006)	(0.007)

Notes: Standard errors are reported in parentheses. *, **, and *** denote significance at the 10, 5, and 1% level, respectively. The dependent variable is an indicator for employment. All regressions include dummies for five year age groups, nationality, household size, municipality size, province, and labor income from the year prior to the possible health shock (in deciles), as well as interactions of these variable with an indicator for an unscheduled and acute hospitalization.

Table 7
Regression estimates for the probability to be employed (= earnings > 20,000) four years after a health shock.

	Men		Women	
	Age 25–39	Age 40–58	Age 25–39	Age 40–58
Effect of major health shock in 2000				
	−0.131***	−0.167***	−0.207***	−0.212***
	(0.035)	(0.021)	(0.049)	(0.031)
Effect of major health shock in 2005 - effect of major health shock in 2000				
	0.033	0.024**	0.022	0.020
	(0.037)	(0.012)	(0.053)	(0.019)
Effect of onset chronic illness in 2000				
	−0.006	−0.127***	−0.079**	−0.075**
	(0.027)	(0.020)	(0.038)	(0.030)
Effect of onset chronic illness in 2005 - effect of onset chronic illness in 2000				
	0.043***	0.046***	0.015	−0.011
	(0.014)	(0.010)	(0.017)	(0.013)
Effect of accident in 2000				
	0.036	−0.050**	−0.028	−0.021
	(0.026)	(0.020)	(0.037)	(0.029)
Effect of accident in 2005 - effect of accident in 2000				
	0.003	0.015**	0.018*	−0.001
	(0.008)	(0.007)	(0.011)	(0.009)

Notes: Standard errors are reported in parentheses. *, **, and *** denote significance at the 10, 5, and 1% level, respectively. The dependent variable is an indicator for employment. All regressions include dummies for five year age groups, nationality, household size, municipality size, province, and labor income from the year prior to the possible health shock (in deciles), as well as interactions of these variable with an indicator for an unscheduled and acute hospitalization.

to receive DI benefits four years after the shock is as large as the effect of the onset of a chronic illness for all groups except men aged 25–39. An accident results in DI receipt significantly less frequently than a major health shock or chronic illness. Note also that for young men the effect is always substantially smaller then for older men. This may be due to the nature of the shock, but also to the fact that young men have fewer options to replace income. That the probability to receive DI benefits is lower for young women than young men may be due to differential preferences for leisure and financial constraints (Trevisan and Zantomio, 2016).

After the reforms, there is a substantial (and statistically significant) reduction in the probability to receive DI benefits after a major health shock, especially for men and women aged 40–58. Four years after a major health event, men aged 40–58 are 6 percentage points less likely to receive DI benefits and their female counterparts even 10 percentage points. In general, the improvements are larger among older men and women. Significant improvements have also been made after the onset of a chronic illness and accidents.

We next turn to the employment effects of health shocks after four years that are shown in Table 7. A major health shock results in the largest reduction of the probability to be employed, followed by the onset of a chronic illness and accident, respectively. Again, the effects for young men are smaller than for the other groups. Although after the DI reforms a major health shock is significantly less likely to result in DI receipt, it does not translate in substantially improved employment opportunities. For women, the DI reforms also did not result in a higher probability to be employed after the onset of a chronic illness. For men aged 40–58, however, the onset of a chronic illness is substantially less frequently a reason for labor market withdrawal after the DI reforms. In fact, the results suggest that (in absolute sense) most of the improvement in employment opportunities among older people is achieved among those who experience the onset of a chronic illness.

3.3.2. *Do effects differ by shock intensity?*

To shed light on the importance of the intensity of health shocks, we re-estimate variants of model (1) where the minimum number of days of hospitalization required for a unscheduled and urgent hospital admission to be classified as a health shock is set equal to more than 3 days. In particular, we subsequently increase the minimum number of days required to 5, 8, 10, and 12 days, respectively. In all cases, the control group remains the same as in the baseline specification. The corresponding effect estimates on the probability of DI benefit receipt four years after the onset of a health shock are portrayed in Fig. 2 for all age and gender groups; this applies both to the health shock estimates for the 2000 cohort and to the change in the shock effect between 2005 and 2000.

In line with expectations, Fig. 2 shows that the baseline effect of a health shock increases in the minimum required length of hospitalization. The health shock effect on DI benefit receipt four years after its onset increases from about 5 to 15 percent, while the effect increases to a somewhat lesser extent for older workers if we take the minimum of 12 days of stay instead of 3 days of stay. These findings confirm the idea that the length of hospital stay can be used as a proxy for the intensity if health shocks. For the change in the health shock effect between 2005 and 2000, however, we do not observe a clear relationship between the intensity of health shocks. It is only for older women that the decrease in health shock effects is more sizeable for longer hospital stays. Overall, there thus is no strong evidence that the improvements that were made in reducing the consequences of health shocks were confined to milder of more severe health shocks.

3.3.3. *Sensitivity analysis*

We argued earlier that failure of the common trends assumption to hold may affect our estimation results. To test for the robustness to this assumption, we extend our model with business cycle indicators at the province level – see Eq. (2). Table A.3 in the appendix reports the estimated δ coefficients for the probability to receive DI benefits and for the probability to be employed. It is reassuring that the effects on the probability to receive DI benefits remain similar across specifications (cf. Table 2). For the probability to be employed the estimates differ between both specifications, in particular for men and women aged 25–39. For men aged 25–39, the coeffi-

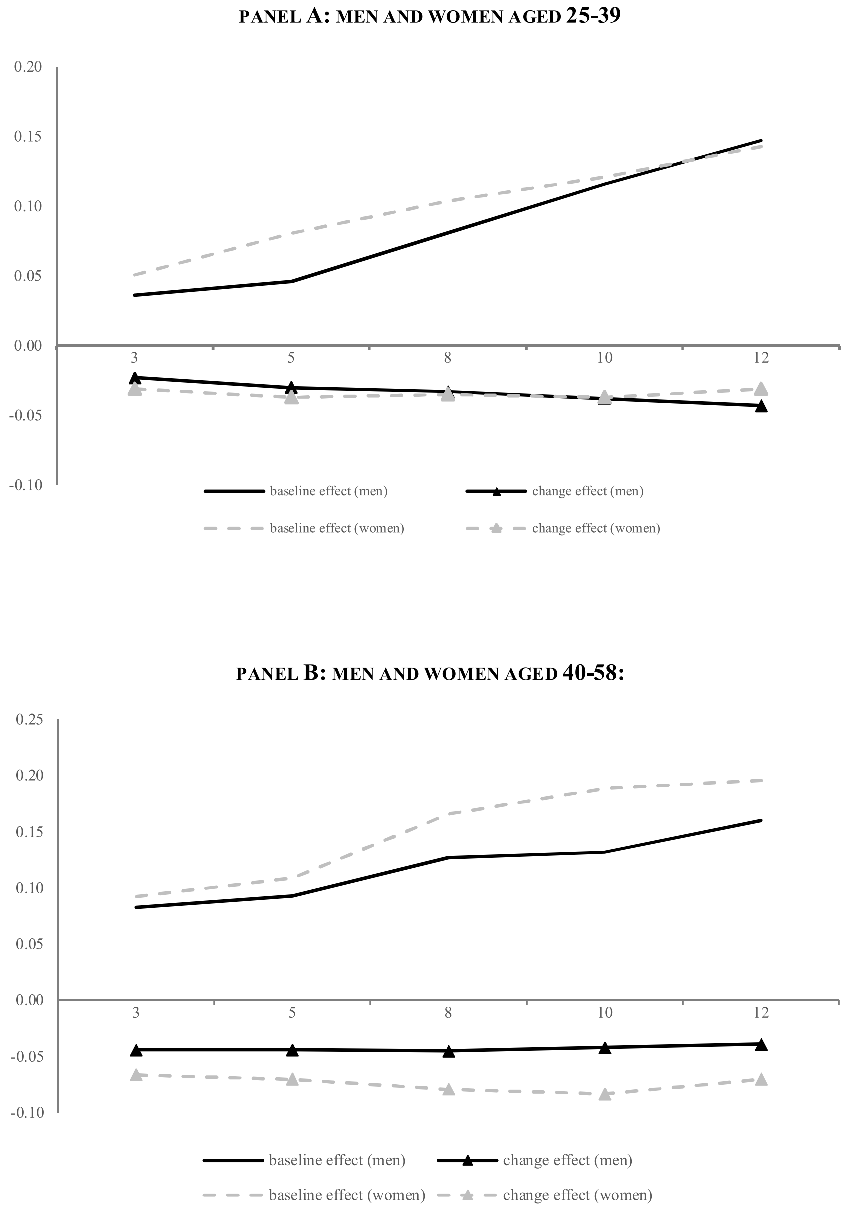

Fig. 2. Parameter estimates for the effects on di receipt four years after a health shock for demographic groups, using different threshold values of hospital length of stay.

cients become small and insignificant. For women aged 25–39, the coefficients remain substantial but also become insignificant. For men and women aged 40–58, the coefficients remain more similar and significant. This suggests that the disability reforms have been mainly beneficial for older workers.

Selective mortality could bias our results since individuals experiencing an acute hospitalization are more likely to die within the observation period than individuals who are not hospitalized. To examine whether this is a problem, we have repeated the analysis using only individuals who remain alive throughout the whole four-year follow-up period. The estimates obtained from this restricted sample are very similar to those generated by the full sample (results are available upon request).

Our estimates of the effects of a health shock on labor market outcomes may also be biased due to the omission of individual characteristics – such as health, job characteristics, and education – that are potentially correlated with both the propensity to be hospitalized and labor market outcomes. As stated earlier, we closely follow García-Gómez et al. (2013) to identify the effects of a health shock. These authors have also investigated whether their analysis has been compromised by the omission of individual characteristics. Using a Dutch household survey which provides detailed information on health, health behaviors, and socio-economic characteristics. García-Gómez et al. (2013) conclude that there is no reason to be concerned that the exclusion of certain characteristics compromises their estimation strategy. Moreover, even if the omission of certain characteristics does bias the estimates of the effect of a health shock, our parameters of main interest, the difference between the effects of a health shock in 2000 and 2005, would not be compromised as long as the bias remains constant over time.

4. Employment trends of disabled individuals – evidence from survey data

From our estimates with the administrative hospitalization and employment data, the picture that emerges is that it has become less likely to enter the DI scheme and more likely to remain employed for workers with unanticipated hospitalizations. These employment effects are most pronounced for older male workers. With this in mind, a pertaining question is whether these changes are reflected in trends for the larger and more general stock of individuals with disabilities in the Netherlands. We therefore have merged social security records with information from the Dutch Labor Force Survey (LFS) for 2000-2010. The LFS is a rotating panel, also administered by Statistics Netherlands, in which respondents are interviewed during five consecutive quarters.[9] Similar to the analysis on administrative data, we focus on individuals aged 25–58 which is likely to have strong labor force attachment. Sick and disabled workers are identified by two questions. First, respondents are asked: *"Do you suffer from one or more chronic diseases, disorders or handicaps?"* If a respondent gives an affirmative answer to this first question, he gets a second question of which the formulation depends on the employment status of the respondent. If the respondent is employed he is asked *"Are you limited in carrying out your work?"* and if unemployed he is asked *"Are you limited by your health in getting work?"*

The work disability questions have only been included in the LFS surveys between 2000 and 2009, and either during the first or third interview in 2010. Hence, we have one health measure per respondent. As some of the questions are dependent on the employment status of the individual, we simply group respondents in those who report to suffer from a (chronic) illness, and those who do not. Note that the formulation of the disability questions in 2007 and 2008 differed considerably compared to the other years, which is why data for 2007 and 2008 have been excluded from the analyses.

Fig. 3 documents the evolution of chronic disease rates and the rate of DI receipt for gender and age groups. To start with, we observe decreases in DI benefit receipt across in age and gender groups, except for the women aged 40–58. For chronic diseases, however, the picture is less pronounced. Chronic illness rates have been gradually decreasing for men throughout the sample period, while remaining more or less constant for women. Particularly for men, this decrease may be the result of improved population health. Alternatively (or additionally) the DI reforms may have affected the likelihood that individuals describe themselves as disabled, either through changed social norms or because more disabled people are working and for that reason do not report to be disabled any longer.

To provide graphical evidence on how the series of DI reforms have affected employment, Fig. 4 plots the average employment rate by age group for men and women with and without chronic disease, disorder or handicap. For men aged 25–39 and 40–58 without a chronic illness the development of the employment rate is remarkably similar and stable over time. For men with a chronic disease aged 25–39 the employment rate increased until 2002, but has been decreasing afterwards. By contrast, the employment rate for men with a chronic disease aged 40–58 has mostly been increasing over the entire sample period. Bearing in mind that reductions in the effect of health shocks on employment were most substantial for older male workers as well, this may well suggest that stronger employer commitment have reduced the 'employment-gap' for this group as well. For women without a chronic disease in both age groups, the employment rate is steadily increasing, with the employment rate for the younger age group initially at a higher level. The employment rate for women with a chronic disease also shows an upward trend, but to a somewhat lesser extent than healthy women in their respective age categories.

[9] Many labor market statistics published by Statistics Netherlands, including for example the unemployment rate statistic, are based on the LFS.

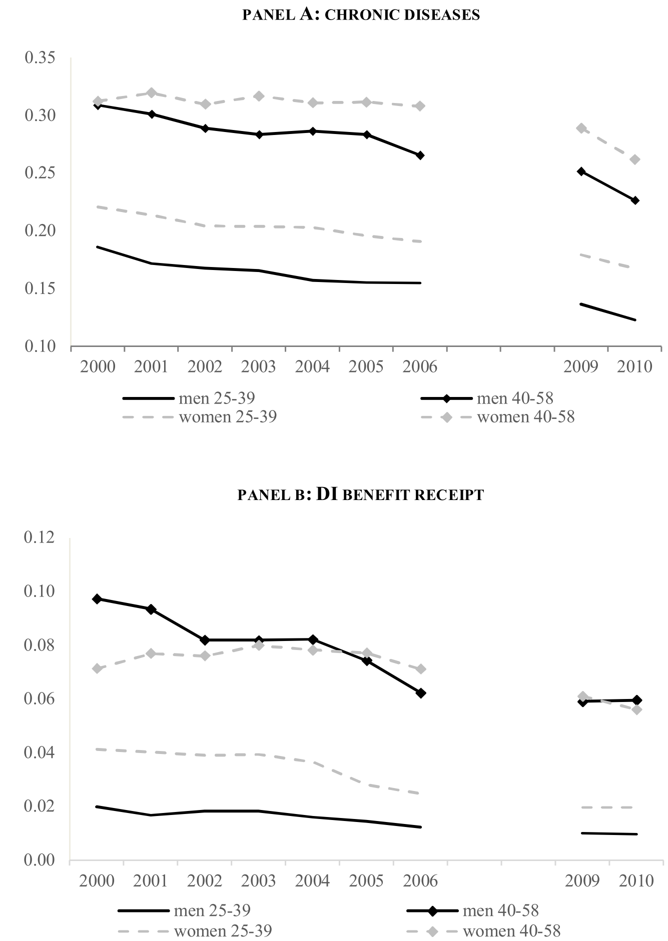

Fig. 3. Probability to have a chronic disease or to receive DI benefits for four demographic groups.
Notes: Figures are based on author's calculations of the Dutch Labor Force Survey and based on 94, 196 (men aged 25–39); 105,999 (men aged 40–58); 124,089 (women aged 25–39); 137,384 (women aged 40–58) observations.

In sum, the LFS does not suggest that the 'employment-gap' of workers with a chronic disease versus workers without has diminished over time. This may be perceived as a surprise, as DI benefit receipt has decreased considerably in the period wherein the DI reforms came into force. At the same time, one should bear in mind that the fractions of medical conditions in the sample of hospitalized workers deviate from those that justify DI admission. Moreover, and by construction, the administrative data only allow for inferences on the sample of employed workers that are faced with health shocks. The increased costs or financial risks may also have deterred employers from hiring disabled workers. If so, the relatively high share of temporary contracts for younger workers may explain why the employment-gap seems to have widened particularly for this group. In a similar vein, the relatively high share of permanent contracts for older male workers may explain why this group benefitted the most from the reforms.

5. Discussion

In this paper we examine the effect of disability reforms on the labor market position of sick and disabled workers in the Netherlands. An important component of the reforms was to make employers responsible for paying sickness benefits, and to strengthen their sickness monitoring obligations. While these employer incentives stimulate preventive and reintegration activities, employers are also confronted with substantial costs when

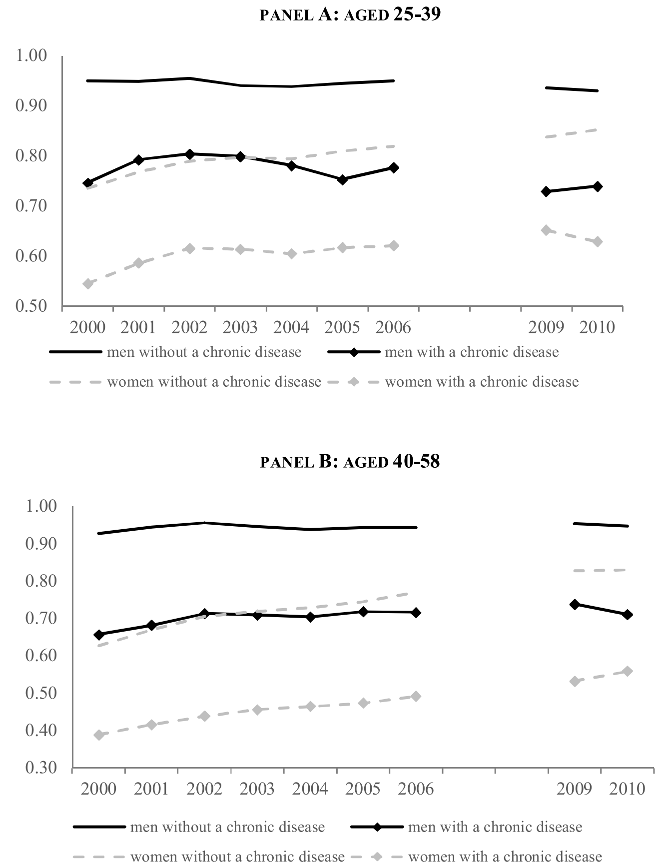

Fig. 4. Employment rates with and without chronic diseases for four demographic groups.
Notes: Figures are based on author's calculations of the Dutch Labor Force Survey and based on 94, 196 (men age 25–39); 105,999 (men age 40–58); 124,089 (women age 25–39); 137,384 (women age 40–58) observations.

an employee gets sick which may make them reluctant to hire workers whose medical history put them at risk for becoming disabled. To avoid endogeneity that subjective health measures may be subject to, we use rich administrative data that allow us to utilize unplanned shocks in health. Our results clearly show that the labor market position of workers who experience an exogenous shock to their health has improved: they are less likely to receive disability insurance benefits and they are more likely to remain employed.

On the basis of our analysis, we conclude that the DI reforms implemented by the Dutch government have substantially improved work resumption among employees. The effect of the health shocks under investigation has decreased considerably, particularly for the workers aged 40–58. Both the Gatekeeper reform that increased screening and the extension of the sickness period that precedes DI claims to two years seem to have contributed to this change. This suggests that employers may improve work resumption rates of long-term sick listed workers – even though the onset of a substantial share of the medical conditions in the treated sample is not (directly) related to work conditions. Our paper thus shows that enhancing employer incentives might indeed be a fruitful way to a more sustainable growth path of DI programs – at least for employed workers that were not able to continue working for some period of time. This is in line with earlier suggestions made by Autor and Duggan (2010) and Burkhauser and Daly (2011) which argue that employer incentives may counteract the rapid growth of the DI program in the U.S.

When taking a broader perspective on these results, researchers and policymakers should also address the issue of increasing work opportunities for all individuals with disabilities – not only those who are already employed. Based on the Labor Force Survey (LFS) statistics that are shown, there is no evidence that the employment position of *all* individuals with disabilities have improved in the Netherlands in the period under investigation. Together with the improved position of employed workers facing health shocks, it is plausible that employer incentives have inadvertently reduced the hiring opportunities of people with health problems or a disability. This is confirmed by other descriptive evidence presented in Koning and Lindeboom (2015), who argue that the most straightforward way for Dutch firms to circumvent continued wage payments is to hire workers on temporary contracts. In case a worker on a temporary contract becomes ill, costs are not assigned to individual employers but to a collective fund. Koning and Lindeboom (2015) do not offer causal evidence on the effect of enhanced employer incentives, but do show that the share of DI benefits awarded to workers with a temporary contract increased from 42 percent in 2007 to 55 percent in 2011. They argue that this increase cannot be fully explained by the (much smaller) increase in the share of workers with temporary contracts, suggesting that vulnerable groups with bad health conditions have sorted into flexible jobs.

Appendix A. Supplementary data

Supplementary material related to this article can be found, in the online version, at doi:https://doi.org/10.1016/j.jhealeco.2018.09.004.

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