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Screening disability insurance applications

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Screening disability insurance applications

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Abstract

This paper investigates the effects of stricter screening of disability insurance applications. A large-scale experiment was setup where in two of the 26 Dutch regions case workers of the disability insurance administration were instructed to screen applications more stringently. The empirical results show that stricter screening reduces long-term sickness absenteeism and disability insurance applications. We find evidence for direct effects of stricter screening on work resumption during the period of sickness absence and for self-screening by potential disability insurance applicants. Stricter screening seems to improve targeting efficiency, without inducing negative spillover effects to the inflow into unemployment insurance. The costs of stricter screening are only a small fraction of the monetary benefits.

Keywords: disability insurance, experiment, policy evaluation, sickness absenteeism, self-screening
JEL-codes: J26, J65

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1 Introduction

In most OECD countries disability insurance (DI) programs are substantial in size and have experienced strong growths over the past two decades (OECD, 2003). Relative to other OECD countries the Dutch DI program is exceptionally large. In 2000, 9.1 percent of the working force collected DI benefits, with total benefits payments as high as 2.65 percent of GDP. In the US, the UK and Germany the number of beneficiaries are much lower (4.7, 6.6 and 4.3 percent, respectively) as well as total benefits payments (0.71, 1.3 and 1.01 percent of GDP, respectively). For a long time the Dutch DI system focused on providing income support, rather than maintaining or improving work prospects, which had consequences for DI rates (Aarts, Burkhauser and De Jong, 1996). Throughout the Nineties a series of policy measures were implemented, but Dutch DI rates remained high. Due to its size the Dutch DI program is a recurrent topic in the political debate.

Over the time period we consider, 2001 to 2003, sick employees had a one year waiting period before entering DI. During this period employers are responsible for financing sick pay. After 13 weeks of sickness absence the employer reports the sick employee to the National Social Insurance Institute (NSII), the public administrator of the unemployment insurance (UI) and DI schemes. Employees who have not fully resumed work after 39 weeks can file a DI benefit claim.

Until April 2002 the NSII had during the waiting period of sickness absenteeism a joint responsibility with the sick worker and the employer to get the sick worker back to work. As of April 2002, the NSII is no longer involved during the waiting period. Under this new gatekeeper regime DI benefit applications at week 39 should be accompanied by a reintegration report, containing the reintegration plan as drafted after 8 weeks of sickness absenteeism, and an assessment on why it has not (yet) resulted in work resumption. The case worker of the NSII checks this reintegration report. If the report is delayed, incomplete, or proves that the worker and/or employer have been negligent, the DI benefit application is not processed and the case worker of the NSII can decide to start a sanction procedure. A sanction usually implies that the employer is obliged to continue providing sick pay for some additional months.[1]

DI programs are often subject to moral hazard (e.g., Bound and Burkhauser, 1999).[2] Easily accessible and generous DI programs allow workers to use DI benefits as a channel to leave the labor force and employers to use DI to lay-off redundant workers. With no or little screening of DI applications may devote no or only minimal effort to get sick employees back to work. Strict screening of reintegration reports may overcome both moral hazard problems. First, it forces employers to devote serious effort to getting

[1] An employer has the right to fire a worker who refuses to collaborate to execute the reintegration plan.
[2] See Bound and Burkhauser (1999) also for a review of studies on the disincentive effects of DI programs. See Chen and Van der Klaauw (2006) for a recent study on the disincentive effects of the DI program in the US and see Johansson and Palme (2002), for a recent contribution on the effect of the level of sick pay on long-term absence in Sweden.

sick workers back to work before they enter the DI program. If reintegration activities are effective, increased efforts positively affect work resumption during sickness absenteeism. Second, stricter screening of reintegration reports reduces the attractiveness of the DI program to potential applicants. This may trigger a mechanism of self-selection or self-screening (see Halpern and Hausman, 1986; and Parsons, 1991).

In this paper we investigate the effects of stricter screening of reintegration reports on the prevalence of long-term sickness absence (spells lasting at least 13 weeks) and on DI benefit applications. We have set up an experiment, where we instructed the case workers at the local offices of the NSII in two out of 26 regions in the Netherlands to screen reintegration reports considerably stricter than elsewhere. We monitored the behavior of the case workers to check that screening in the treatment regions was indeed more strictly. In the treatment regions the time spent on screening reintegration reports by the case workers was 40 percent higher than in the control regions.

Previous research in this area mostly relied on state level variation in the implementation of DI rules (e.g. Autor and Duggan, 2003; Gruber, 2000; Gruber and Kubik, 1997; and Parsons, 1991). Often denial rates for DI applications are used as a proxy for the screening stringency. A disadvantage of using observed denial rates is that besides the screening policy, they also depend on the composition of the inflow. The composition of the inflow may in turn depend on earlier screening policies. Therefore a high denial rate might reflect a strict screening policy as well as a relatively large fraction of non-eligible workers claiming DI benefits. If the latter is the case the high denial rate is merely the result of a loose screening practice in the past, rather than a strict screening policy. As far as we know the data used in this paper are the first that come from a controlled experiment in which regional variation in screening intensity is determined exogenously.

Our data are from the administrative records of the NSII and cover the period from 2001 until 2003. The screening of reintegration reports became effective in January 2003 and was not announced beforehand. In the empirical analyses we use both difference-in-difference analyses at the level of the regions and estimate more parameterized Logit models at the individual level. We find that the stricter screening decreases long-term sickness absenteeism and DI applications equally. We argue that the reduction in long-term sickness absenteeism is due to self-screening by potential DI applicants and that for DI applications the decline is due to a direct effect on work resumption during sickness absence.

An important issue in this literature is targeting efficiency (Parsons, 1991, Gruber and Kubik, 1997). Parsons (1991) shows that under certain conditions increases in the DI benefit denial rate can lead to perverse effects, leading disabled workers to reduce applications more than able workers. From the institutional background in the Netherlands and some sensitivity analyses, we argue that targeting of the Dutch DI system is likely to be improved and that there are no perverse effects of stricter screening.

Finally, a back of the envelope calculation shows that the costs of the stricter screening are only a small fraction of the monetary benefits, measured by the decline in DI benefit payments.

The outline of this paper is as follows. In Section 2 we provide some institutional background concerning the Dutch DI system. Section 3 discusses the setup of the experiment. Section 4 gives some theory. In Section 5 we discuss the administrative database that will be used in the empirical analyses. In Section 6 we present our estimation results. Section 7 concludes.

2 Institutional background

In this section we describe the Dutch DI system as it was in 2003, the year of our experiment. Furthermore, we discuss the reintegration activities to which sick workers and their employers have to comply. We only discuss aspects that are relevant for this study.

2.1 *Sickness and DI benefits*

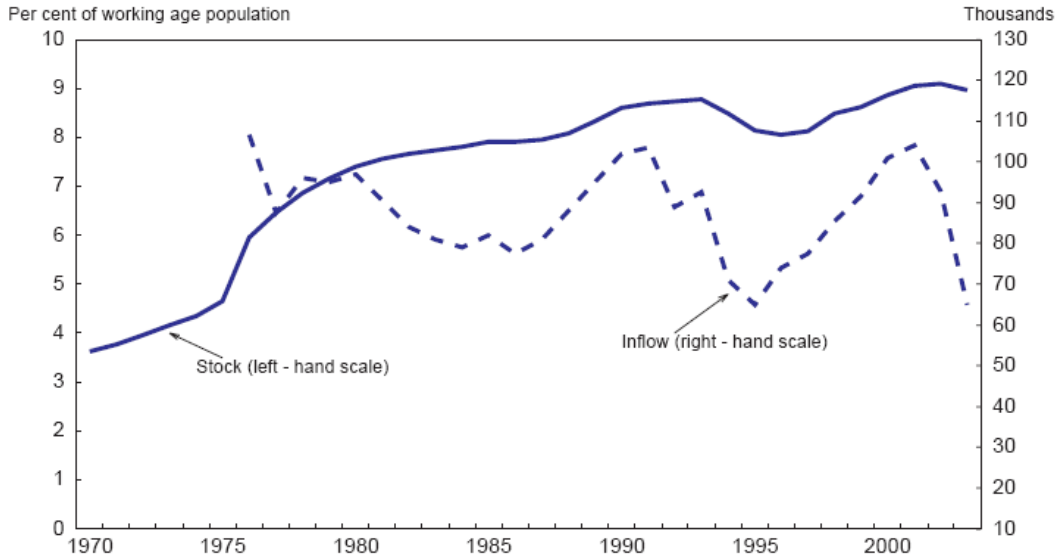
The Dutch DI program covers all employees. Any illness or injury entitles to entering DI after a mandatory waiting period of one year. The legitimacy of sickness absenteeism is checked by a doctor from an occupational health service contracted by the employer. During this period employers are responsible for financing sick pay.[3] The employers should furthermore contract an occupational health service for preventing and managing the sickness absenteeism.

While other OECD countries make a distinction by whether the impairment occurred on the job or elsewhere, only the consequence of impairment is relevant for the Dutch DI program. The degree of disablement is assessed by considering a worker's residual earning capacity, defined by potential earnings with his or her functional limitations as a fraction of pre-disability earnings. The degree of disablement is the complement of the residual earnings capacity, and is measured in seven classes, ranging from a 15 to a 100 percent loss. When considered fully disabled by the NSII, 70 percent of pre-disability earnings are replaced. If assessed as partially (15 – 80 percent) disabled the replacement rate is correspondingly lower. Benefits are capped at about 31,000 euro. The size of DI benefits and its unlimited duration (until pension age 65) make DI a more attractive option than UI. UI benefits have a limited entitlement period, after which the unemployed can apply for means-tested welfare benefits.[4]

[3] Collective bargaining agreements ensure that sick workers receive 90 to 100% of their net salary. Most employers insure themselves against financing sick pay.

[4] The DI benefit period is cut in two chronologically linked parts. The first is a short-term wage-related benefit replacing 70% of before-tax earnings, when assessed as fully disabled. The duration of this wage-related benefit depend on age at the onset of disablement. It varies from zero for those under age 33 to six

Figure 1: DI benefit recipients: stock and inflow 1970-2003.



Source: OECD (2004).

Figure 1 shows the number of individuals collecting DI benefits as a percentage of the working age population and the inflow rate into DI. The figure shows a sharp increase in the percentage of individuals collecting DI benefits in the Seventies and Eighties. The number of beneficiaries increased from 475,000 in 1976 to 921,000 in 1993. During the Nineties the disability definition was narrowed and benefit generosity was reduced. This lowered the inflow into DI. Furthermore, between 1994 and 1996 part of the stock of beneficiaries was reexamined using the new, more stringent, eligibility rules. As a result the number of benefit terminations grew sharply and, on balance, led to a 7 percent drop in the number of beneficiaries to 855,000 in 1996. From then on, the numbers started growing again and reached 979,000 in November 2002, coming close to the politically contentious level of one million disabled.

2.2 *The gatekeeper protocol and the role of reintegration reports*

As of April 2002, the role of the NSII changed due to the introduction of the so-called gatekeeper protocol. This protocol stipulated the responsibilities of the worker and the employer for sick spells lasting six weeks or longer. Figure 2 gives a schematic representation of the different steps taken in this protocol. After a maximum of six weeks

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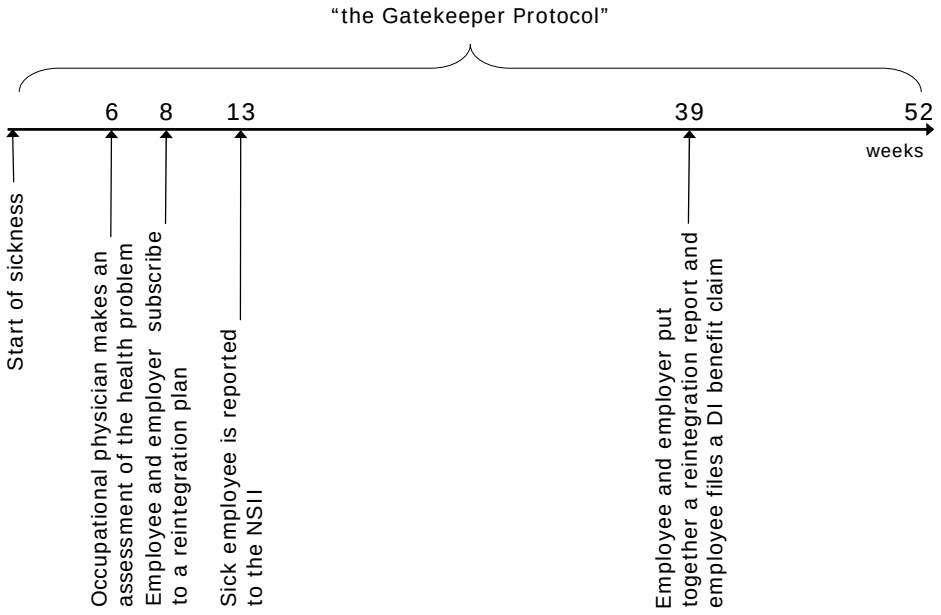
years for those whose disability started at age 58 or beyond. The second part is a so-called follow-up benefit with a lower income base. This earnings base is the minimum wage plus a supplement depending on age at onset according to the formula: 0.02 x [age at onset – 15]x [wage – minimum wage]. Most collective bargaining agreements covered the gap between the lower benefits in the follow-up period and the wage-related benefits in the initial period of disablement.

of absence the company doctor has to make a first assessment of medical cause, functional limitations and give a prognosis regarding work resumption. On the basis of these data the employer and employee together draft a vocational rehabilitation ("reintegration") plan in which they specify an aim (resumption of current/other job under current/accommodated conditions) and the steps needed to reach that aim. They appoint a case-manager, and fix dates at which the plan should be evaluated and modified if necessary. The reintegration plan should be ready in the eighth week of sickness. It is binding for both parties, and one party may summon the other when considered negligent. After 13 weeks of absence the employer should report the sick employee to the NSII, which is only a paper obligation.

Figure 2: Schematic representation of the process toward entering DI



DI claims have to be delivered before the 40th week of sickness. Benefit claims are only considered admissible if they are accompanied by a reintegration report, containing the original reintegration plan, and an assessment as to why the plan has not (yet) resulted in work resumption. If the report is delayed, incomplete, or proves that the reintegration efforts were insufficient the claim is not processed. Depending on the seriousness of the negligence the case worker can give the opportunity to complete the reintegration report or to start a sanction procedure against the employer. The employer and the sick employee are jointly responsible for reintegrating the sick worker back into his or her old job or into a new job commensurate with the worker's limitations. However, if the employee consistently rejects reasonable offers and accommodations, the

employer may stop paying sickness benefit and, eventually, may fire the employee. Part of the employer's responsibility, therefore, is to penalize an unwilling employee. The gatekeeper protocol became effective in April 2002 and thus affected DI claims as of January 2003.

3 The experiment

Our experiment period started January 1, 2003. At that date the first DI applications under the gatekeeper protocol arrived at the regional offices of the NSII. These were the first applications that were accompanied by reintegration reports. In two out of the 26 regions we instructed the case workers to implement a policy of screening reintegration reports in a way that was stricter than elsewhere. The standard (national) procedure was to screen the reintegration reports "on paper" and to only contact the employer and/or the sick worker directly if there was some suspicion of negligence.

In the two treatment regions we instructed the case workers to always contact or visit the employer and/or the sick employee, unless it was absolutely clear that, given the medical condition, sufficient efforts were undertaken.[5] Stricter screening was only applied to individuals who had not resumed work for more than 50 percent of their contractual working hours and were not severely and permanently disabled. The experiment period finished at the end of October 2003. We setup the experiment together with the NSII, which made additional resources available for the treatment regions to intensify the screening. The experiment was not announced beforehand to any of the parties involved (the worker, the employer and the occupational health services). We designed the experiment, but the treatment regions were assigned to us by the NSII. In Section 5, we use pre-experiment outcomes on sickness absenteeism, DI application and UI inflow to investigate if treatment assignment was actually independent of these outcomes.

The start of the experiment coincided with the introduction of the gatekeeper protocol. So none of the case workers at the NSII, independent of their location, had any experience in screening reintegration reports. For the success of the experiment it was crucial that in the treatment regions the new screening regime was conducted stricter than in the rest of the Netherlands. To monitor the screening regimes in all regions we sent out questionnaires to the 24 control regions. In the treatment regions we required case workers to fill out a questionnaire for each DI application. In the control regions we asked the local managers to regularly report summaries of the screening regime at their offices. Table 1 summarizes the difference in screening intensity in the treatment and the control regions. The table shows that in the treatment regions case workers visited sick

[5] The main reason to screen a reintegration report only "on paper" in the treatment regions is that the case worker already handled multiple applicants from an employer and that therefore the case worker is familiar with the reintegration protocol of this employer.

workers (Apeldoorn region) or their employers (Hengelo region) more often than elsewhere.[6] In the control regions reintegration reports are checked more often only on paper or by phone. Since face-to-face confrontations can be considered as the strictest form of screening, these data confirm that screening was indeed stricter in the treatment regions.

Table 1: Difference in screening stringency between treatment and control regions.

	Treatment regions		Control
	Apeldoorn	Hengelo	Regions
Only on paper	4%	14%	25%
Telephonic contact with employer	33%	34%	52%
Telephonic contact with worker	14%	14%	23%
Telephonic contact occupational health agency	3%	12%	32%
Visit to employer	9%	41%	7%
Face to face contact with worker	77%	41%	18%
Unknown	4%	2%	

4 Potential effects of screening

Employers are financially responsible for sick pay during the first 12 months of absence. The employer drafts together with the sick worker a reintegration plan, but usually it is the employer who decides on reintegration activities and workplace accommodations needed to stimulate work resumption. Recall from Subsection 2.2 that the NSII holds the employer responsible in case of negligence. We therefore take the employer as the relevant decision making agent.

The introduction of the gatekeeper protocol imposes minimum requirements on reintegration efforts that the employer should offer and the employer is at risk of getting a sanction if the actual reintegration efforts do not meet these minimum requirements. The probability of getting a sanction and the size of the sanction increase with the extent of noncompliance. Reintegration efforts are costly to employers, e.g. they are mandated to contract an occupational health agency to manage absenteeism. The occupational health agencies usually work for many different employers and thus collect and transmit information about the screening policy at the local offices of the NSII. We return to this issue below.

Employers choose their reintegration activities such that the marginal costs equal marginal returns. The returns to reintegration activities are not only reduced threats of getting a sanction, but if reintegration activities are effective, also a higher probability of earlier work resumption and hence reduced sick pay outlays. If optimal reintegration

[6] We return to the difference in screening methods between the two experiment regions in Subsection 6.3.

effort already exceeds the minimum requirements imposed by the NSII, introducing stricter screening and sanctions does not change the employer's behavior. However, if optimal effort is below the minimum requirements, stricter screening induces employers to increase reintegration activities. Stricter screening then leads to more reintegration activities and if these activities are effective, work resumption rates during sickness absenteeism should increase and DI application rates decrease. This is the direct effect of stricter screening.

Stricter screening also reduces the attractiveness of the DI program to potential applicants and may trigger a mechanism of self-selection or self-screening (Parsons, 1991). The decision to start a DI application process involves a comparison of expected utilities of alternatives, such as unemployment, early retirement and continuing work. Stricter screening increases the costs of applying for a DI benefit. Self-screening means that potential applicants who think that their DI application might not meet the eligibility requirements choose not to apply for the program. Indeed, Parsons (1991) finds that self-screening is important in explaining fluctuations in the inflow into DI in the US.

If self-screening is important, we should observe fewer 13 weeks sickness reports in regions where a stricter screening policy is implemented. If direct effects of screening are important, then we should observe more workers who report sick at week 13 to resume work before they file a DI claim (week 39). Recall that reintegration plans are drafted after eight weeks of sickness absence. Therefore, the impact of the reintegration activities on work resumption will mainly be concentrated after 13 weeks. A possible consequence of self-screening is that workers who without stricter screening would have applied for the DI program will apply instead for other programs, such as UI. Riphahn (1997) finds evidence for substitution between UI and DI schemes in Germany. If this is also relevant here, we expect to find higher UI inflow rates in the treatment regions.

The effect of stricter screening on the sanction rate is ambiguous. In the treatment regions noncompliance with the minimum requirements is more likely to be detected. This implies more sanctions due to stricter screening. However, self-screening and increased reintegration activities due to the stricter screening are expected to reduce noncompliance in the treatment regions. It remains unclear which of these effects dominates.

At the moment the experiment started in January 2003, employers, employees and occupational health agencies in both the treatment and the control regions did not have any experience with the way in which reintegration activities would be judged. Therefore, at the start of the experiment, employers in the treatment and in the control regions were expected to display the same behavior. However, sanction probabilities in the treatment regions are higher and therefore employers are notified faster about noncompliance. Employers in the treatment regions should thus learn faster about the minimum reintegration activities requirements. The occupational health agencies serve many different employers and collect and transmit information about changes in scrutiny at the

regional offices of the NSII. Therefore, employers not only learn through their own experience, but also through information disseminated by the occupational health agencies.

The experiment is not continued after 2003 and we do not have data later than 2003. As will be discussed in the next section we observe the year in which a worker applies for DI benefits, but not the exact date. This implies that many of the DI applications that are observed are from workers who first reported sick in 2002 or at the start of 2003 when employers and workers had not yet learned about the details of the screening policy. This implies that self-screening at the start of the DI application process (if present) is most likely not yet reflected in the DI application rates of 2003. Reductions in the 2003 DI application rates due to stricter screening should therefore be interpreted as direct effects of the increased reintegration activities on work resumption. Thirteen weeks absence reports are much less affected by the direct effects of increased reintegration activities and therefore the effect of the stricter screening on these reports should be interpreted as the consequence of increased self-screening. However also the 2003 reports on 13 weeks sickness absenteeism include reports from workers who became sick at the end of 2002 or the very beginning of 2003 and therefore the estimated effects will most likely be an underestimates of the long-run effect of stricter screening.

5 Data

Our data are from the administrative records of the NSII and describe the period 2001-2003. For each year, the data contain all insured individuals in 45 sectors of industry, which cover about 50 percent of the Dutch work force.[7] Not included are branches like construction, retail, health care and the civil service. Stratified by age, gender, sector of industry and region, we observe (1) the number of insured individuals on January 1 of each year, (2) the number of 13 weeks sickness reports during the year, (3) the number of DI applications during the year and (4) the number of individuals flowing into UI during the year. The setup of the data does not allow us to identify specific individuals and therefore we do not have a true panel data structure at the individual level.

Table 2 provides some descriptive statistics of the data. Our observation period is marked by low GDP growth, which is reflected in the data. The insured population, 13 weeks sickness absence and the number of DI applications decreased, while the inflow into UI increased. The sanction rate is only observed in 2003, since sanctions were only introduced when the gatekeeper protocol became effective. The data on sanctions describe the total number of sanctions given in each region. Therefore, we cannot link the

[7] Traditionally, the NSII had different organizations for different sectors of industry. In 2002 these organizations were officially integrated, but in 2003 case workers were still connected to particular sectors of industry and the databases of the different organizations are not yet merged. In the experiment, we only instructed the case workers handling these 45 industries.

sanctions to specific individuals. The sanction rate is the number of sanctions as a fraction of the number of DI applications. The fraction of females remained constant over time, but over the years the insured population aged. The latter may be a combination of an ageing work force, reduced early retirement options (e.g. OECD, 2004) and less job opportunities for school leavers and young workers.

Table 2: Some descriptive statistics of the data.

	2001	2002	2003
Number of insured individuals	3,352,516	3,346,515	3,277,887
Sickness absenteeism rate (13 weeks)	0.0473	0.0446	0.0399
DI application rate	0.0150	0.0137	0.0104
UI inflow rate	0.0304	0.0433	0.0587
Sanction rate	-	-	0.0524
Fraction female	0.317	0.320	0.320
Age -34 years	0.470	0.457	0.441
Age 35-44 years	0.266	0.271	0.276
Age 45-54 years	0.192	0.192	0.195
Age 55- years	0.072	0.080	0.089

As mentioned in Section 3 the treatment regions were not randomly selected, but assigned by the NSII. It is important to check if the choice of the treatment regions is unrelated to relevant outcome measures. Recall that the stricter screening of reintegration reports started in January 2003. Therefore, we can use the pre-experiment years 2001 and 2002 to investigate random assignment. In Table 3 we show for 2002 the sample means and standard errors of the outcome variables in the treatment and the control regions. The treatment regions do not differ substantially with respect to the outcome variables from the control regions, although 13 weeks sickness is somewhat higher and UI inflow is slightly lower. The main goal of stricter screening is to reduce the DI application rate. Since the table shows that DI application rates were virtually identical, we conclude that the treatment regions were not selected on their DI application rates (or any other relevant outcome) in 2002.

Table 3: Pre-experiment outcomes in treatment and control regions (year 2002).

	Treatment Regions	Control Regions
Sickness absenteeism	0.04823 (0.00079)	0.04483 (0.00433)
DI applications	0.01376 (0.00011)	0.01392 (0.00300)
UI inflow	0.04228 (0.00542)	0.04436 (0.09167)

Standard errors in parentheses.

Since our data describe multiple years, we only need random selection of the treatment regions conditional on regional fixed effects. A simple test for conditional random assignment is to check if in the treatment regions the outcome variables changed differently between the pre-experiment years 2001 and 2002 than in the control regions.

More specifically, we take the panel data model at the level of the region for the years *t* is 2001 and 2002:

$$Y_{r,t}=\alpha+\beta Year+\gamma Treatment_{r,t}+\delta_r+u_{r,t} \quad (1)$$

where β describes the nation wide time trend, $Treatment_{r,t}$ is an indicator that only equals 1 in the treatment regions in the year 2002 and δ_r is the regional specific effect. We estimated this equation in first-differences:

$$Y_{r,2002}- Y_{r,2001}= \beta+\gamma Treatment_r+e_r \quad (2)$$

The estimation results in Table 4 show that the outcomes in the treatment regions do not significantly differ from those in the control regions. We therefore conclude that the selection of the treatment regions is exogenous with respect to pre-experiment outcomes.

Table 4: Test for (conditional) random selection of treatment regions: difference-in-difference estimates (2002 compared to 2001).

	Sickness absenteeism	DI applications	UI inflow
Trend	-0.00291 (0.00018)	-0.00142 (0.00023)	0.01257 (0.00106)
Treatment region	0.00072 (0.00063)	0.00113 (0.00082)	0.00041 (0.00380)

Standard errors in parentheses.

6 Results

In this section we first discuss the effects of the stricter screening on 13 weeks sickness reports and DI applications. Next, we investigate how the sanction rate is affected by stricter screening. We then report on some additional analyses and discuss the issue of targeting efficiency. We finally report on the results of a simple cost-benefit analysis.

6.1 *Effects on sickness absence and DI applications*

Our main parameters of interest are the effects of stricter screening on 13 weeks sickness absenteeism reports and DI applications. Recall from Section 4 that our data only allow us to measure this effect during the first year after the stricter screening was introduced. Therefore, we are likely to underestimate the long-run effects of stricter screening.

In Table 5 we present the results from difference-in-difference analyses. For each region we use as outcomes the number of 13 weeks sickness absenteeism reports and the number of DI applications as fraction of the insured population in the pre-experiment year 2002 and the experiment year 2003. Effectively we estimate a regression equation similar to equation (2), but for the years 2002 and 2003. The trend effects show that both the probability that a worker reports sick and the probability that a worker applies for a

DI benefits decrease significantly between 2002 and 2003. This may be the result of the nationwide regime shift that occurred in April 2002 and became effective in January 2003. But it might as well be the result of adverse macroeconomic conditions. Absence rates are found to be pro-cyclical (e.g. Johansson and Palme, 1996) and the Dutch economy was in a recession during our observation period. From the table we see that the stricter screening causes a decrease in sickness absenteeism reports and in DI applications. However, only the effect on sickness absence is significant.

Table 5: The effect of stricter screening: regional difference-in-difference estimates (2003 compared to 2002).

	Sickness absenteeism	DI applications
Trend	-0.00455 (0.00028)	-0.00320 (0.00021)
Treatment (stricter screening)	-0.00213 (0.00103)	-0.00048 (0.00076)

Standard errors in parentheses.

Table 6: The effect of stricter screening: marginal effects from individual Logit estimates.

	Sickness absenteeism	DI applications
Trend	-0.00397 (0.00153)	-0.00091 (0.00084)
Treatment (stricter screening)	-0.00223 (0.00057)	-0.00059 (0.00032)
Male -34 years	-	-
Female -34 years	0.02286 (0.00030)	0.01028 (0.00017)
Male 35-44 years	0.02383 (0.00027)	0.00791 (0.00017)
Female 35-44 years	0.04072 (0.00032)	0.01593 (0.00018)
Male 45-54 years	0.03467 (0.00028)	0.01286 (0.00016)
Female 45-54 years	0.04680 (0.00035)	0.01926 (0.00019)
Male 55- years	0.04403 (0.00032)	0.01436 (0.00019)
Female 55- years	0.04835 (0.00051)	0.01861 (0.00027)

In this regression is also controlled for region, sector and interactions between the sector and the time trend (in total 114 additional parameters). Standard errors in parentheses.

In the empirical analyses above we only used regional variation and we have not exploited the individual characteristics observed in the data. We have also estimated Logit models for the individual probability of 13 week sickness and the probability to apply for DI benefits. Other than the indicator for stricter screening, we include as regressors a nationwide time trend, regional indicators, the gender of the individual interacted with age dummies, sector dummies and sector dummies interacted with time trends. The latter set of variables allow for different sector trends between 2002 and 2003. Table 6 shows the marginal effects from the estimated Logit models. We see little changes in the effect of stricter screening when we compare the difference-in-difference estimates with the estimates of the Logit model. This confirms that the selection of the treatment regions was not correlated to changes in the composition of the insured population over time. The standard errors in the Logit model are much smaller than in the difference-in-difference model. The effect of stricter screening on sickness absenteeism reports is significant and the *p*-value for significance of the effect on DI applications is

0.07.

Recall from Section 4 that we can distinguish two effects of stricter screening. As a direct effect, stricter screening may increase reintegration efforts and work resumption rates among sick listed workers. And, indirectly, reduced prospects may induce workers and employers not to start a DI application process. We argued in Section 4 that the effect of stricter screening on DI applications reflects direct effects, while the effect on 13 weeks sickness absence rates reflects self-screening effects. The empirical results show that stricter screening reduces both the incidence of 13 weeks sickness absenteeism and the number of DI benefit applications, which implies that both effects are important. To provide some more information on the relative importance of each of these effects, we provide in Table 7 the predicted number of sickness absenteeism reports and DI applications in 2003. Furthermore, Table 7 includes what the number of 13 weeks sickness absenteeism reports and DI applications would have been if stricter screening would have been implemented either nationwide, or nowhere in the Netherlands. From the table we can see that nationwide implementation of stricter screening would reduce the number of sickness absenteeism reports by 5.2 percent and the number of DI applications by 4.8 percent. To the extent that our interpretation of both effects is valid, this implies that indirect self-screening effects and direct effects of increased reintegration efforts are quantitatively equally important. Note however, that the long-run effects of stricter screening on DI applications are likely to be bigger. After all, the long-run effect of the stricter screening on DI applications is the cumulative of the effect of self-screening and the direct effect due to increased reintegration activities.

Table 7: Simulations for sickness absence and DI applications under different screening scenarios applying to 2003 using the estimates from the Logit model.

	Sickness absenteeism	DI applications
Predicted in current situation	130,423	34,066
Without nationwide stricter screening	130,997	34,196
With nationwide stricter screening	124,236	32,563

6.2 *Effects on sanctions*

The gatekeeper protocol provided the NSII the possibility to sanction employers. Since these sanctions did not exist prior to 2003, we cannot control for region specific unobserved effects. This reduces the reliability of the estimated effects of stricter screening on sanctions. The data on sanctions are also less informative than data on other outcome variables, we only observe the total number of sanctions applied in each region and we cannot connect these sanctions to specific DI applications. Therefore, we should interpret the empirical results with care and we should not draw too strong conclusions about how stricter screening influences the sanction rate.

Table 8 provides the results from an OLS regression on the sanction rate in each

region. We control for regional differences by including lagged (2002) 13 weeks sickness absenteeism rates, lagged DI benefit applications rate and lagged UI inflow rates. The estimation results show that the sanction rate is slightly lower in the treatment regions but this effect is not significant. However, sanction rates are lower in regions with a high DI application rate and a high UI inflow rate. A propensity score weighting estimator shows the same result. Using the estimator presented in Hirano, Imbens and Ridder (2003) we find an effect of stricter screening on the sanction rate of -0.0298 with a standard error of 0.4864.[8] Recall that stricter screening causes two mechanisms affecting the sanction rate in opposite directions. First, stricter screening has a direct effect that more cases of noncompliance will be detected. And second, if stricter screening is anticipated, then there will be an indirect effect that the composition of the DI applicants shifts towards applicants who experienced more reintegration activities.

Table 8: The effect of stricter screening on sanctions: results from an OLS regression on the sanction probability in 2003.

	Sanctions
Intercept	0.0648 (0.0483)
Treatment (stricter screening)	-0.0064 (0.0175)
Sickness absenteeism reports in 2002	-0.7398 (1.4198)
DI applications in 2002	-4.1712 (1.8475)
UI inflow in 2002	1.7875 (0.6013)
R^2	0.878

Standard errors in parentheses.

6.3 *Sensitivity analyses*

The Logit estimates of Subsection 6.1 show that sickness risks and DI application probabilities vary with individual characteristics. Recall from Subsection 2.1 that incentives to apply for DI vary with age. Therefore, also the effect of stricter screening may vary with age of the applicant. We estimate a Logit model that allows for age and also for gender specific effects of stricter screening. The marginal effects reported in Table 9 show differential age and gender effects for absence rates. More specifically, stricter screening does not affect absence rates of older (55 plus) females, but it does affect the absence rates of younger (below 55) females. For these younger females we find that the effect increases with age. For males we find the strongest effect on absence rates for prime aged (35-44) and older (55+) workers. For the DI applications, stricter screening only affects female DI application rates and the effects increase with age.

[8] When applying this method the propensity score was specified as a Logit model and included the same regressors as the OLS regression of Table 8.

Table 9: Effects of stricter screening by age and gender: marginal effects from individual Logit estimates.

	Sickness absenteeism	DI applications
Stricter screening interacted with:		
Male -34 years	-0.00075 (0.00110)	0.00105 (0.00066)
Female -34 years	-0.00253 (0.00123)	-0.00124 (0.00070)
Male 35-44 years	-0.00281 (0.00099)	-0.00008 (0.00059)
Female 35-44 years	-0.00301 (0.00137)	-0.00143 (0.00077)
Male 45-54 years	-0.00184 (0.00099)	-0.00019 (0.00057)
Female 45-54 years	-0.00381 (0.00156)	-0.00171 (0.00083)
Male 55- years	-0.00239 (0.00120)	-0.00060 (0.00073)
Female 55- years	-0.00022 (0.00245)	-0.00459 (0.00154)

In this regression is also controlled for a time trend, gender interacted with age, region, sector and interactions between the sector and the time trend (in total 122 additional parameters). Standard errors in parentheses.

To understand these results one should consider the composition of the DI inflow. Compared to other OECD countries, the inflow into the Dutch DI program is characterized by relatively high fractions of the women and young workers (OECD, 2003). The Netherlands is the only country where more than 50 percent of the inflow into DI is due to workers below 45 years. Until the mid-Eighties, women had lower DI inflow rates than men, but since 1985 female inflow rates exceed those for men. At the end of the Nineties, female inflow rates were almost twice the male rates. Between 1991 and 2001 the number of male DI recipients decreased by 13 percent, while its female counterpart increased by 43 percent. The sharp increase in female disability was accompanied by a strong growth of labor force participation of mothers. Traditionally, labor force participation was low among mothers. Whereas three out of four women stopped working after the birth of their first child in the 1970s, only one third stopped twenty years later. Part of this shift was accommodated by DI benefits which allow market production to be replaced by home production without a sharp drop in household income (de Jong, 2004).[9] Bratberg, Dahl and Risa (2002) examine whether career and family obligations increase sickness absence among mothers and conclude that it does. Among the OECD countries, only in the Scandinavian countries the fraction of female workers in the inflow in DI is as high as the Netherlands.[10] However, Dutch female labor force participation rates are much lower than in the Scandinavian countries. This suggests that in the Netherlands moral hazard problems might be more pronounced in female inflow in DI than in the male inflow. We return to this in the following subsection where we discuss targeting efficiency.

Recall from Table 1 that the case workers in both experiment regions used

[9] One could see if the pattern differs between married and unmarried women and between women with and without children. In our data we do not observe the marital status, nor do we have information on the presence of children.

[10] Like in the Netherlands, also the Scandinavian DI schemes are characterized by relatively lenient eligibility conditions and high benefit rates.

different channels for stricter screening. In Apeldoorn the case workers arranged more face-to-face contacts with workers, while in Hengelo the case workers visited employers more often. If indeed the employer is the most relevant decision making agent in the DI application process, one might expect the strategy in Hengelo to be more effective. To investigate this issue we estimated the previous Logit model but now allowing for different effects for Apeldoorn and Hengelo. These effects are reported in Table 10. We find strong and significant effects on 13 weeks sickness for Hengelo, but not for Apeldoorn. The effect is equivalent to a reduction of about 10 percent in 13-weeks sickness absenteeism rates in Hengelo. The effects on DI applications do not differ between both regions and are in size similar to the results in Table 6. From this we can conclude that the employer is indeed the relevant decision maker and that self-screening of potential DI applicants is triggered through the employer.

Table 10: The effect of stricter screening for both treatment regions: marginal effects from individual Logit estimates.

	Sickness absenteeism	DI applications
Apeldoorn	0.00005 (0.00081)	-0.00057 (0.00047)
Hengelo	-0.00413 (0.00076)	-0.00058 (0.00042)

In this regression is also controlled for a time trend, gender interacted with age, region, sector and interactions between the sector and the time trend (in total 122 additional parameters). Standard errors in parentheses.

6.4 *Targeting efficiency*

Stricter screening leads to a fall in DI applications due to direct effects and indirect (self-screening) effects. The success of stricter screening largely depends on the composition of those individuals who do not apply of DI benefits anymore. Parsons (1991) argues that increases in the screening policy parameters, such as the waiting time before applying for DI and the denial rate, can lead to greater reductions in DI applications among the disabled than among the able workers. This reduced targeting efficiency may be due to differences in credit market constraints, mortality expectations and risk aversion between the disabled and able workers. Parsons (1991) concludes for the US that the change in the denial rate due to the 1977 disability funding crises did not lead to perverse self-screening effects.

Testing targeting efficiency requires health information for workers in different regions in different years. Unfortunately, our data do not contain such information. Therefore, we use other information to assess targeting efficiency in the presence of stricter screening. First, in the Dutch context it is not directly clear that stricter screening may lead to perverse effects. Unlike the US where individuals have no income during the DI waiting period, Dutch workers get sick pay up to 90 to 100 percent of their net earnings during the entire waiting period. Stricter screening of reintegration reports as

such can only lead to an extension of the waiting period (if a sanction is applied), rather than a denial of the DI benefit claim. This implies that stricter screening does not have direct negative financial consequences for workers, unless employers penalize sick employees who are unwilling to collaborate with a reasonable reintegration plan. This should affect the less disabled more than those with severe disabilities. For workers there might be indirect financial consequences of reporting sick and trying to get into a DI program. For instance, workers with a history of long-term sickness absenteeism may have lower promotion rates and higher probabilities to be fired. These costs might in fact be higher for able workers than for truly disabled workers.

It is mainly the employer who suffers financially from stricter screening. To avoid a punitive extension of the waiting period, the employer must clearly argue why the reintegration activities did not lead to work resumptions during the waiting period. It should be clear that it is much more difficult to argue that reintegration activities were ineffective in the case of an able worker than for a disabled applicant. So the employer faces a higher sanction risk when using DI to lay-off able workers.

There are, however, clear benefits to lay-off able workers through DI and there is some evidence that this indeed occurs. Recall that DI is more generous than UI and DI does not require the workers to search for jobs. As a rule, severance payments are one month of salary for each year of tenure in the firm. So for older workers with many years of tenure, DI may be the cheaper channel for laying-off workers. Older labor force participation rates are much lower in the Netherlands than for example in the US. From comparing health, work and economic well-being of older men and women in the US and the Netherlands, Burkhauser et al (1999) conclude that differences between Dutch and US institutions, rather than differences in health explain the differences in labor force participation rates. If we combine these institutional facts with the results form the sensitivity analyses discussed in the previous subsection, where we saw that the impact of stricter screening increases with age, we conclude that stricter screening reduces moral hazard among older workers. Similarly, we argued that moral hazard problems might be more pronounced among women than among men. The sensitivity analyses indicated that stricter screening was more effective for women than for men.

The Dutch NSII publishes a top-5 of most frequently cited illnesses and conditions causing inflow into DI. During our observation period, the top-5 was dominated by hard-to-diagnose conditions like depression, burn-out and low-back problems. This could be taken as evidence that there is still a substantial fraction of able workers in the inflow in DI. Campolieti (2006) argues that hard-to-diagnose conditions are more subject to moral hazard as applicants can overstate their problems. He also finds for Canada that stricter screening leads to a decline in hard-to-diagnose conditions among DI applicants, while easy-to-diagnose conditions remained unaffected.

If stricter screening would scare disabled workers away from applying for DI benefits, we would expect these workers to leave the firm through some other channel.

Able workers who do not apply for DI benefits can continue working in the firm, while this is much more difficult for disabled workers. The population that is insured for DI coincides with those insured for UI. Therefore, if stricter screening would have perverse effects in the sense that truly disabled workers would not apply for DI, we should expect an increase in the UI inflow rate. It is not uncommon that institutions cause interactions between UI and sickness and DI programs (e.g. Riphahn 1997, Larsson, 2006).

Our data are informative on UI inflow. Table 11 presents estimates for the marginal effects of the Logit model for the effect of stricter screening on the UI inflow.

Table 11: The effect of stricter screening: marginal effects from individual Logit estimates.

	UI inflow
Stricter screening	-0.00043 (0.00065)
Trend	0.02217 (0.00122)
Male -34 years	-
Female -34 years	0.01547 (0.00027)
Male 35-44 years	0.00365 (0.00027)
Female 35-44 years	0.02418 (0.00032)
Male 45-54 years	0.00470 (0.00030)
Female 45-54 years	0.02240 (0.00038)
Male 55- years	0.01614 (0.00036)
Female 55- years	0.02802 (0.00059)

In this regression is also controlled for region, sector and interactions between the sector and the time trend (in total 114 additional parameters). Standard errors in parentheses.

Obviously, we do not find an increase in UI inflow in the treatment regions; the estimates of the stricter screening are even negative. This result remains if we allow for different effects for different age and gender groups (see Table 12). The lack of spillovers between DI and UI suggests that the workers who did not apply for DI benefits did not flow into UI and most likely remained at work. This again should be taken as evidence that the stricter screening did not harm the disabled workers, but rather reduced the attractiveness of the DI program for those able to work.

Table 12: The effects of stricter screening by age and gender on UI inflow: marginal effects from Logit estimates.

	UI inflow	
Stricter screening interacted with:		
Male -24 years	0.00001	(0.00095)
Female -24 years	-0.00015	(0.00115)
Male 35-44 years	-0.00063	(0.00111)
Female 35-44 years	-0.00065	(0.00151)
Male 45-54 years	-0.00053	(0.00126)
Female 45-54 years	-0.00044	(0.00184)
Male 55- years	-0.00141	(0.00166)
Female 55- years	-0.00116	(0.00305)

In this regression is also controlled for a time trend, gender interacted with age, region, sector and interactions between the sector and the time trend (in total 122 additional parameters). Standard errors in parentheses

6.5 *Back-of-the-envelope calculation of monetary costs-benefits*

We can use our estimation results to perform some back-of-the-envelope calculations of stricter screening. It should be stressed that we only look at the direct monetary costs and benefits and ignore wider social costs and benefits. We use the results of Table 7 to compare a situation where stricter screening is applied nationwide to a situation where the standard screening procedure is applied across the country. The results in Table 7 apply to the year 2003. We argued earlier that the full effects of increased strictness are likely to be larger than can be observed in the first year of its introduction. Therefore, it is likely that in the long-run equilibrium the costs will be lower and the benefits higher. Without stricter screening each case worker handles on average 110 DI applications per year. For all 34,196 applications about 311 case workers were needed. Stricter screening requires an additional time investment per case worker of 9.4 percent of the total time spent on a DI application. But given the reduction in the annual number of applications due to stricter screening only 13 additional case workers are required, which cost approximately 1.32 million euros per year.

Stricter screening induces a reduction of 1,633 DI applications per year. Recall that an application is made after 39 weeks of sickness absenteeism, while DI benefit entitlement starts after 52 weeks of sickness absenteeism. Due to both benefit denials and recoveries between 39 and 52 weeks, only 47 percent of the applications lead to actual enrolment into the DI program. The average DI benefit is 11,745 euro per year and average benefit duration is 12.9 years.[11] If we use a discount rate of 5 percent per year, the expected present value of future DI benefit payments is slightly less than 85,000 euros. Hence, the total amount of averted DI benefits payments of stricter screening are around 64.8 million euros.

[11] The gross annual exit probabilities from disability insurance are in the first year 0.16, in the second year 0.17, in the third year 0.10, in the fourth year 0.07 and around 0.06 afterwards.

This cost-benefit analysis assumes that a nationwide introduction of stricter screening would not affect the composition of the inflow into DI. In particular, annual exit probabilities are assumed to remain unchanged and the average benefit level is assumed not to change. It is, however, more likely that stricter screening shifts the population flowing into DI towards individuals who have worse health and labor market prospects and thus on average longer DI spells. This reduces the expected monetary benefits of stricter screening. On the other hand, the cost-benefit analysis does not take into account that stricter screening also significantly reduces the 13 weeks sickness absenteeism report rate, which reduces administrative costs. The main conclusion of these rough calculations is that the monetary costs of implementing the procedure of stricter screening are only a small fraction of the monetary benefits.

7 Conclusions

In the Netherlands, if a worker becomes sick, there is a one year waiting period of sickness absenteeism before the worker can enroll into DI. Applications for DI benefits should be made during the 39th week of sickness absence. These applications are only considered admissible if they are accompanied by a reintegration report, containing a detailed description of the reintegration activities of the employer and worker during the period of sickness absenteeism. If the report is delayed, incomplete, or proves that the reintegration efforts were insufficient the claim is not processed and the case worker of the NSII may start a sanction procedure.

In the paper we consider a policy of stricter screening of the reintegration reports by the case workers of the National Social Insurance Institute (NSII). With no or little screening, worker and employer can decide to devote none or only a minimal effort in trying to get the worker back to work. Stricter screening forces employers and workers to devote more effort in getting the sick worker back to work. This increased reintegration effort can have a direct effect on work resumption during sickness absence. Furthermore, stricter screening reduces the attractiveness of the DI program to potential applicants (and to employers), which can trigger a mechanism of self-selection by potential applicants (Parsons, 1991).

To investigate the consequences of stricter screening we have setup an experiment. We instructed case workers at two local DI offices to implement a regime of stricter screening of the reintegration activity reports than in the rest of the Netherlands. This provides truly exogenous variation in the screening intensity, which distinguishes this paper from previous contributions to this literature.

We use data from the administrative records of NSII and find that stricter screening leads to a decrease in both 13 weeks sickness absence reports and DI applications. We argue that in our data the decline in 13 weeks sickness absence reflects self-screening by potential DI applicants and the reduction in DI applications measures

work resumption during sickness absenteeism. The empirical results show that stricter screening causes both self-selection and work resumption during sickness absenteeism and that both effects are quantitatively equally important. The long-run effect on DI applications is the cumulative of the effect of self-screening and of the direct effect due to work resumption.

We argue that the stricter screening mainly reduces the DI application rate of able workers and thereby reduces moral hazard and improves targeting efficiency. A crude cost-benefit analysis shows that the monetary costs of the stricter screening are only a small fraction of the monetary benefits. If indeed stricter screening improves targeting efficiency of the DI program, the welfare benefits of the stricter screening may even be higher as more able workers remain productive.

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