# The impact of a disability insurance reform on labor participation of couples

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**Abstract**

The Netherlands reformed its disability insurance (DI) scheme in 2006. The reform increased the period on sickness benefits from 12 to 24 months. Access to disability benefits after this period became more difficult and the benefits became less generous. We study the impact of the reform on the labor participation of individuals who fall sick and their spouses. Difference-in-differences estimates suggest that the reform increased the labor participation of sick individuals by 2.5 percentage points. The effect is persistent and constant in the ten years following the reform. The spouses of sick individuals increased labor participation by 0.8 percentage points, on average. The effects vary substantially with the individual's type of employment contract when falling sick. Comparing with singles suggests that in couples, both partners share the burden of the more stringent disability scheme after the reform.

## 1 Introduction

In the beginning of the century the Netherlands became one of the countries with the highest share of disabled workers in the insured population. The total number of DI recipients reached almost one million, corresponding to about 11 percent of the insured population in 2002 (Koning and Lindeboom, 2015). To reduce the number of DI recipients and promote work resumption, successive governments implemented a series of radical reforms in the existing Disability Insurance Act (WAO). In 2006, the Work and Income According to Labor Capacity Act (WIA) came into effect as the final element of these reforms. A transitional scheme was implemented before WAO was replaced entirely by WIA. The transitional scheme preserved the main features of WAO except that the criteria to enter the DI scheme were made stricter. WIA introduced major changes in both the DI scheme and the sickness insurance (SI) scheme that precedes

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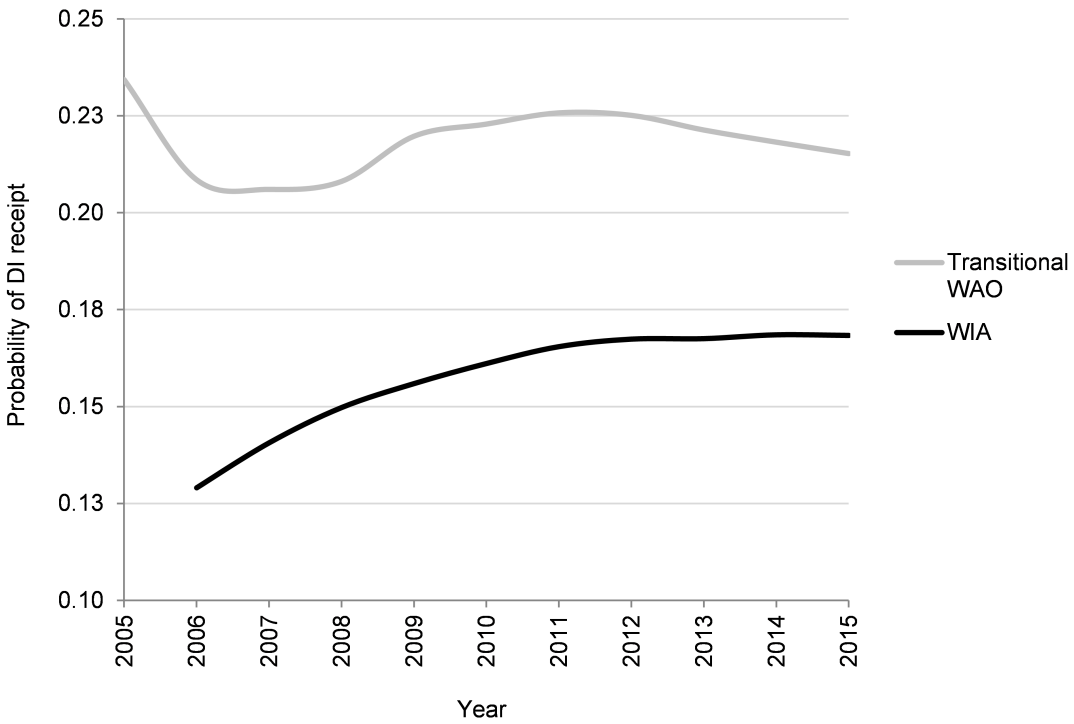


Figure 1: Probability of DI receipt among people who fell sick in the fourth quarter of 2003 (insured under transitional WAO) or in the first quarter of 2004 (insured under WIA). The transitional WAO group can apply for DI from the fourth quarter of 2004; the WIA group from the first quarter of 2006.

it. The duration of the SI scheme was extended from one to two years. The incentive for employers to facilitate work resumption was therefore increased: they are obliged to compensate the employee for wage loss during the two-year period of the scheme instead of for only one year. For the DI scheme, the WIA introduced stricter entrance criteria, but also introduced stronger financial incentives for work resumption for both employees and employers. Figure 1 presents the probability of DI receipt among people who fell sick and could apply for DI in the transitional WAO and WIA regimes. The figure suggests that the WIA reduced DI receipt by a substantial amount of 7.9 percentage points in 2006. The effect diminished somewhat but remains substantial over a period of nine years.

Kantarcı et al. (2019) analyzed the effects of WIA on labor participation and benefit receipt from DI and alternative social support programs. Using a difference-in-differences (DiD) approach, they compared people who fell sick before the reform and were insured under WAO with people who fell sick after the reform and were insured under WIA. They showed that, due to the reform, the probability of DI receipt decreased by 5.8 percentage points over the ten years after the reform. To compensate for lost DI benefits, individuals increased their labor participation by 1.8 percentage points, but also increased their claims from unemployment insurance (UI) by 1.4 percentage points. These effects imply that increases in labor participation and UI receipt did not fully compensate for the decrease in DI receipt.

Spousal labor participation could serve as an additional mechanism to compensate for lost DI benefits. In this study we analyze how sick people in couples and their spouses compensate for loss of DI benefits. To estimate the impact of the reform on spousal labor supply, we take a

DiD approach and compare the labor participation of two groups of spouses whose partners fell sick before and after the reform and who were therefore subjected to different eligibility criteria for DI. In particular, we exploit administrative data on the universe of individuals who fell sick in the last quarter of 2003 and in the first three quarters of 2004. The former cohort participated in the WAO scheme, the latter in the stricter WIA scheme. We link the sick workers in the data to spouses who are registered as cohabiting in municipal records to analyze whether the DI reform had spill-over effects on the spouse.

We show that among the sick individuals who have a healthy spouse (i.e., a spouse who was never reported sick), labor participation rose by 2.5 percentage points, while their spouses' labor participation rose by 0.8 percentage points in response to the DI reform. Furthermore, we compare how sick individuals who have a spouse and those who do not have a spouse respond to the reform, and show that the reform increased the probability of working by 0.7 percentage points more among sick individuals without a spouse than for sick individuals in couples. These results suggest that the response to the DI reform is shared by both partners in a couple. Spousal labor supply is a substitute for sick people's own labor supply when subjected to a stricter disability benefit regime.

These findings contribute to two strands of the literature. The first studies the impact of DI reforms offering important policy implications for many western countries where the number of DI recipients has substantially grown in the last decade (OECD, 2018). This literature mainly focused on the effects of two measures used to reduce the number of DI claimants: tightening eligibility criteria (Autor and Duggan, 2003; Karlström et al., 2008; De Jong et al., 2011; Staubli, 2011; Campolieti and Riddell, 2012; Moore, 2015; Autor et al., 2016; Hullegie and Koning, 2018) and reducing benefit levels (Gruber, 2000; Campolieti, 2004; Marie and Vall Castello, 2012; Kostøl and Mogstad, 2014; Low and Pistaferri, 2015; Deshpande, 2016; Mullen and Staubli, 2016; Fevang et al., 2017; Koning and van Sonsbeek, 2017; Zaresani, 2018; Deuchert and Eugster, 2019; Ruh and Staubli, 2019). These studies, however, do not consider spill-over effects on the spouse and might thus provide an incomplete view on the consequences of policy reforms.

The second strand of the literature analyzes income complementarities in households as an insurance mechanism. The added worker effect hypothesis suggests that married woman would work more in response to a negative shock on their husbands' earnings (Lundberg, 1985). Still, the empirical evidence for the added worker effect is limited (Maloney, 1987; Maloney, 1991; Spletzer, 1997). The leading explanation is that wives' labor supply responses to their husbands' unemployment shocks lead to earnings increases that are small compared to the household's lifetime income. Furthermore, many households are already sufficiently insured through formal social insurance (Cullen and Gruber, 2000). An exception to this literature is Blundell et al. (2016) who show that spousal labor supply insures against permanent shocks to partner's wage income.

A limited number of studies analyzed the impact of changes in DI rules on the labor supply decisions of couples. Duggan et al. (2010) found that an increase in enrollment in the disability compensation program for veterans reduced spousal labor supply in the United States. Autor et al. (2019) analyzed the consequences of DI receipt on household income, among other outcomes, in Norway. They find that DI receipt changes household income significantly for singles but not for those who are married, because spousal labor supply adjustments and benefit substitution offset the effect of a change in DI receipts. Two studies analyzed the impact of stricter DI rules on spousal labor supply in the Netherlands. Borghans et al. (2014) studied the impact of reassessment of existing recipients and new applicants younger than 45 years of age based on new eligibility criteria for DI in 1993. They found that affected people were able to fully offset the loss of DI benefits with higher earnings and income from alternative social support

programs. However, the reform did not have a statistically significant effect on spousal earnings. Garcia-Mandicó et al. (2020) analyzed the earnings responses to a reassessment of the earnings capacity under more stringent rules in 2004. They found that reassessment of female recipients significantly increased the earnings of their husbands. These studies suggest that changes in DI rules affect spousal labor supply to some extent, but the evidence is limited due to the small number of studies conducted and the estimates are not always precise.

The 2006 DI reform differs from earlier DI reforms studied by Borghans et al. (2014) and Garcia-Mandicó et al. (2020) in several important respects. This means that the potential of the 2006 reform to generate labor supply responses for sick individuals and spillover effects on the partner is very different. First, WIA provides disabled people with unprecedented and strong incentives to utilize their remaining earning capacity, which could limit the need for an increase in spousal labor supply. Second, WIA affected all new applicants, while the reform analyzed by Borghans et al. applied to new applicants as well as existing DI recipients and Garcia-Mandicó et al. applied to existing DI recipients. Existing recipients that later on are denied DI benefits might behave differently from new applicants denied DI benefits. Third, the reforms analyzed by Borghans et al. and Garcia-Mandicó et al. only affected people younger than 45 years, while WIA applied to all sick individuals.

The remainder of the paper proceeds as follows. Section 2 describes the 2006 Dutch DI reform. Section 3 describes the data and the study sample. Section 4 presents descriptive evidence on the impact of the reform on spousal labor supply. Section 5 describes the empirical approach used to identify the effect of the reform. Sections 6 and 7 present the results. Section 8 conducts specification checks. Section 9 concludes.

## 2 Disability insurance in the Netherlands and the 2006 reform

The Disability Insurance Act (WAO) came into effect in 1967 to insure against loss of earnings due to long-term disability. Since major amendments in 1993 it preserved its main features until it was replaced by the Work and Income Act (WIA) in 2006. WAO consists of two schemes. An individual who earns a wage or receives unemployment insurance benefits (UI) is first admitted to the sickness scheme if he is unable to perform his work because of occupational or non-occupational illness or injury. The duration of the scheme is one year.[1] When the sickness scheme expires, the individual is admitted to the disability scheme if his disability grade is at least 15 percent. He is first entitled to the "Wage-loss benefit" and after this has expired to the less generous "Follow-up benefit".[2]

Due to easy access, the annual inflow rate into WAO increased to 1.5 percent of the insured working population in 2001, leading to reforms. Before WAO was abolished entirely, however, a transitional scheme was introduced on 1 October 2004 for people who fell sick between October 1 2003 and January 1 2004. In the transitional scheme the features of the sickness and disability schemes of WAO were preserved except that entry criteria were made stricter. In particular, the transitional scheme adapted a broader definition of the work that the applicant could still do. As a result, the estimated wage loss due to disability was reduced, making it harder to reach the minimum disability grade for DI eligibility, or to reach a higher disability grade with a higher Wage-loss benefit.

[1] The employer is responsible to pay 70 percent of the former wage. Most employers pay the full amount.

[2] The Wage-loss benefit replaces 70 percent of the former wage multiplied by the disability grade. The duration of the benefit depends on the age of the individual and is limited to a maximum of 6 years. The Follow-up benefit pays the minimum wage and an additional amount that depends on the former wage and the age at which the individual has become entitled to the benefit. The benefit is paid as long as the individual is disabled but expires when he reaches the state pension age.

WIA came into effect on 1 January 2006 for individuals who fell sick from 1 January 2004 onwards. It introduced major changes in both the sickness and disability schemes to facilitate work resumption. It reduced the yearly inflow rate into the disability scheme to 0.5 percent of the insured working population during the first six years since its introduction (Koning and Lindeboom, 2015). The duration of the sickness scheme was extended from one to two years. The employer is obliged to compensate the employee for wage loss during the two-year period of the scheme, creating a strong incentive for the employer to facilitate work resumption.[3]

WIA kept the stricter eligibility criteria of the transitional WAO scheme with the broader definition of what work can still be done. In addition, it introduced several major changes. First, the minimum grade of disability required to enter the scheme was raised from 15 to 35 percent – workers with limited disability are expected to resume working with adaptations or apply for UI. Second, the scheme introduced a distinction between full and partial disability. If the wage loss is more than 80 percent and there is no potential for any degree of recovery, the worker is admitted to the Full Invalidity Benefit Regulation (IVA). If the wage loss is more than 35 percent and less than 80 percent, or if the wage loss is more than 80 percent but there is still potential for recovery, the worker is insured under the Return to Work Regulation (WGA). The eligible worker is first entitled to the "Wage-related benefit". The benefit has a UI component that compensates the individual if he is not able to utilize his remaining work capacity. When the Wage-related benefit expires, the individual is entitled to one of two types of benefits. If he utilizes at least 50 percent of his remaining earning capacity, he is entitled to the "Wage-supplement benefit". Otherwise he is entitled to the less generous Follow-up benefit.[4] At a given disability grade, the difference between the Wage-related benefit and the Follow-up benefit is 70 percent of the difference between the former wage and minimum wage, giving the partially disabled workers with higher former wages a strong incentive to utilize at least 50 percent of their remaining work capacity after the Wage-related benefit has expired. Since the UI component of the Wage-related benefit is also exhausted when the Wage-related benefit expires, the individual faces an additional incentive to exploit remaining work capacity in the continuation period of the DI scheme. All in all, compared to WAO, WIA provides stronger incentives to exploit remaining work capacity during the disability period.

Finally, WIA amended experience rating in DI: Firms with high disability costs are penalized with a higher premium. In WAO, experience rating applied to employer premiums to DI for a period of 5 years for all disabled workers. In WIA, the experience rating period is extended to 10 years and applies to employer contributions for partially disabled workers so that the employer is financially incentivized for a longer period to reintegrate beneficiaries with remaining work capacities. This no longer applies to fully disabled workers.

In WAO or WIA, if the individual has no employer during participation in the sickness scheme, he is eligible for the "Sickness benefit" (ZW) that replaces 70 percent of the former wage. During participation in the disability scheme, the individual is eligible for UI.[5] The

[3]The compensation must amount to 70 percent of the former wage. Most employers pay the full amount during the first year of sickness, and many pay more than 70 percent of the former wage during the second year.

[4]The IVA replaces 75 percent of the former wage. The Wage-related benefit replaces 70 percent of the former wage multiplied by the disability grade if the individual utilizes his remaining work capacity to its full potential. The duration of the benefit depends on the employment history, and is limited to a maximum of 38 months. The Wage-supplement benefit replaces 70 percent of the former wage multiplied by the disability grade. The Follow-up benefit replaces 70 percent of the minimum wage multiplied by the disability grade. Both benefits are paid as long as the individual is disabled but expire when he reaches the state pension age.

[5]If the benefit received from a benefit scheme (sickness, disability, or unemployment scheme), or the wage earned during the second year of sickness (in the WIA), is lower than the applicable social minimum, it is supplemented up to the social minimum according to the Supplementary Benefits Act (Toeslagenwet). The total of the benefit and the social minimum supplement cannot exceed the former wage. If the individual is living with a partner, the supplement is granted if the total income of the individual and the partner is below the social minimum. If

amount of the UI is a certain fraction of the remaining earning capacity. In WAO, the individual is required to file an application to claim UI. In WIA, however, UI is integrated into the DI benefit, and no separate application is required.

# 3 Data

We use unique administrative data from the Employee Insurance Agency (UWV) on sick people facing different criteria to enter the disability scheme and different incentives to resume working once they participate the disability scheme. In particular, the data contains information on the universe of individuals who fell sick in the fourth quarter of 2003 or the first three quarters of 2004, and therefore became eligible to participate in either the transitional WAO or the WIA scheme. We observe the beginning and ending dates of their sickness, their gender, date of birth, country of birth, and sector of employment. These individuals either earn a wage or receive UI at the time they fall sick – other groups are not eligible to enter the sickness scheme. For wage earners, we observe whether they hold a permanent contract, a temporary contract, or a contract through a temporary work agency at the time they fall sick.

We link the sick individuals in the UWV data to administrative data on their partners (married or cohabiting) from Statistics Netherlands (CBS), with information on labor participation, wages and benefits on a monthly basis. The benefits include DI, UI, general assistance for low-wage earners, and other benefits from a large number of smaller benefit programs. The data from the CBS extend from January 1999 to December 2015, and allow to study the impact of the DI reform on spousal labor supply over a period of almost ten years.

The initial sample of sick people has 88,218,240 observations for 367,576 individuals. To select our estimation sample, we first drop individuals if they are participants of the disability schemes for the self-employed (WAZ) or young people (WAJONG) since their institutional rules and incentives for work resumption are very different. This restriction leads to a sample of 87,366,720 observations for 364,028 individuals. Second, we drop individuals who already receive DI before they fall sick. This leads to a sample of 81,385,200 observations for 339,105 individuals. Third, we drop individuals if they start cohabiting with a partner after they fall sick or if their cohabitation dissolves before they fall sick, leading to 46,078,560 observations for 191,994 individuals. Fourth, we drop the sick individuals whose spouses are reported sick during the period October 2003 until September 2004. This reduces the data set to 43,475,520 observations for 181,148 individuals. Fifth, we drop individuals in same-sex registered partnerships, reducing the sample to 42,300,480 observations for 176,252 sick individuals. Finally, we restrict the sample with respect to the number of days spent in sickness. Employers are mandated to report to UWV sickness cases if they last longer than 90 days. Temporary work agencies are the main suppliers of short-term sickness cases. They complied with mandatory reporting to a large extent only from January 2004 onwards, after WIA came into effect. Therefore, in the data, short-term sickness cases are under-reported for participants of the (transitional) WAO who fell sick before January 2004. We drop the sickness cases that last shorter than 90 days.[6] This reduces the sample to 27,476,160 observations for 114,484 sick individuals – the study sample.

As described above, sick individuals become eligible to participate in one of two DI schemes depending on the date they fall sick. This allows constructing control and treatment groups of sick individuals and comparing their responses to the reform in a quasi-experimental research design. The control group consists of individuals who fell sick in the fourth quarter of 2003 and were insured under the transitional WAO scheme and their spouses. The treatment group

the individual is living alone, the amount of the supplement depends on whether the individual has children.

[6] In the section on sensitivity analysis, we present regression results from samples based on alternative numbers of days spent in sickness.

consists of individuals (and their spouses) who fell sick in the first three quarters of 2004 and are insured under the WIA scheme.

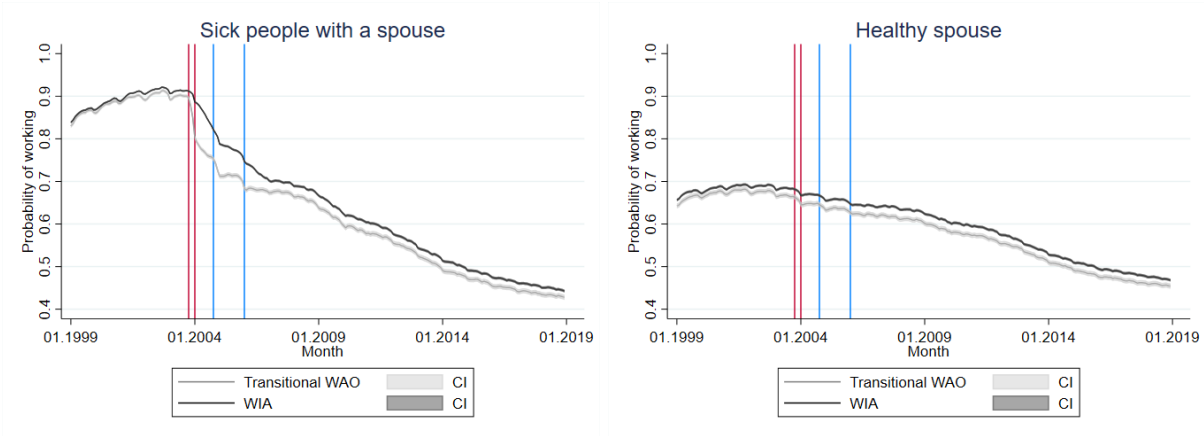


Figure 2: Probability of working for control and treatment groups over calendar months: for sick (left panel) and healthy (right panel) spouses. Vertical lines mark the first instance sick partners could become entitled to the sickness and disability benefits in the transitional WAO and WIA schemes. Red lines correspond to 1 October 2003 and 1 January 2004 for the transitional WAO and WIA groups, respectively. Blue lines correspond to 1 October 2004 and 1 January 2006 for the transitional WAO and WIA groups, respectively.

# 4 Time trend in outcome variable and the difference between control and treatment groups

Figure 2 shows the labor participation of control and treatment groups over the observation period from January 1999 to January 2019. The figure on the left shows the time profiles of labor participation for sick people insured under the transitional WAO scheme and under the WIA scheme. The figure on the right shows this for their (initially healthy) spouses. A time profile of a given group is generated as follows. First, within a group and in a given calendar month, the mean of the outcome variable (dummy variable that indicates labor participation) is calculated. The set of means calculated for each month of the observation period are then used to draw a time profile. Vertical lines are added at the first instance individuals could become entitled to the sickness and disability benefits in the transitional WAO and WIA schemes.

The figure for sick people shows a strong time trend that is common to control and treatment groups. The probability of working increases until the date individuals fall sick. This pattern does not reflect behavioral responses but the fact that individuals can enter the sickness scheme, and get reported as sick in the administrative data, only if they are working or receiving UI at the time they fall sick (Section 3). Before this, they can have another labor force status. The probability of working falls sharply during the first few years of sickness and continues to decrease further throughout the remaining years of the observation period. The difference between the control and treatment groups is small and statistically insignificant before individuals fall sick, while it is always substantial and statistically significant after they fall sick. This suggests that the WIA reform substantially increased labor participation among sick individuals.

The figure for spouses shows a decreasing time trend in labor participation that is similar to that of their sick partners, but the decrease is less pronounced. The difference between the control and treatment groups is statistically significant during both the pre-treatment and

post-treatment periods, but it is larger post- than pre-treatment. This suggests that the reform increased labor participation among healthy spouses of sick individuals. This provides prima facie evidence on the spill-over effect of the reform on the labor participation of spouses.

Table 1a presents sample means of background characteristics at the time of falling sick and labor participation rates before and after sickness for control and treatment groups. It also presents balancing tests between the two groups. Panel A of the table shows that, in both the control and treatment groups, the average age is about 43 and the fraction of women is lower than that of men. The majority of individuals hold a permanent work contract, and the remaining hold a temporary contract, a contract through a temporary work agency, or are unemployed. Column 3 presents the mean differences between treatment and control groups based on linear regression, and indicates whether the difference is statistically significant.[7] The differences are substantial and significant at the 1 percent level.

Columns 3 and 6 in panel B present mean differences in the outcome during the pre-treatment and post-treatment periods for treatment and control groups. In line with Figure 2, the difference for the post-treatment period is substantially larger than that for the pre-treatment period. This shows that the reform substantially increased labor participation among sick individuals.

Table 1b reproduces Table 1a for the spouses of sick individuals. As for the sick spouses, the treatment group is older than the control group. Couples cohabit for about 7 years during the observation period. Columns 3 and 6 in panel B show that the group mean difference in the outcome during the post-treatment period is larger than that during the pre-treatment period. This suggests that the reform increased labor participation for the spouses.

## 5 Identification strategy

We discuss the model for labor participation of the spouse of the sick individual. To estimate the reform effects on labor participation of the sick individuals themselves, we use essentially the same equations, only the outcome variable is different.

We use a difference-in-differences approach to identify the causal effect of the WIA reform on the labor participation of the spouses of sick individuals. The first difference is across groups. Those who fall sick in the first three quarters of 2004 are subject to different eligibility criteria and face different incentives to work or claim benefits, compared to individuals who fall sick in the fourth quarter of 2003 and are subject to the less restrictive "transitional WAO" regime. Spouses whose partners are subject to the stricter WIA regime have a stronger incentive to increase earnings to compensate for lost DI benefits, compared to spouses whose partners are subject to the old regime. The second difference is over (event) time: After their partners fall sick, the spouses' incentives to work are different from before their partners fall sick.

We implement the DiD comparison using the following regression:

$$y_{it} = \gamma \ (Treated_i \times Post_t) + \delta \ Post_t + \lambda_s + \alpha_i + \varepsilon_{it}. \tag{1}$$

Here $i$ indexes the spouse of a sick individual. $t$ indexes the month of event time. Values from –68 to –1 indicate the months before the partner falls sick, and values from 0 to 120 indicate the month in which and the months after the partners falls sick. $y_{it}$ is the outcome variable of interest – the spouse's labor participation dummy. $\lambda_s$ is a monthly calendar time effect. $s$ indexes the calendar month (from January 1999 until July 2015; January 1999 is chosen as the base month). $\alpha_i$ is an individual-specific, time-invariant fixed effect. $\varepsilon_{it}$ represents the

[7] In particular, it shows the estimated coefficient from the regression of the characteristic as the dependent variable, and the treatment indicator (whether member of the reform group) as the explanatory variable.

Table 1a: Sample means and balancing tests of background characteristics and outcome in control and treatment groups before and after sickness for sick individuals with a partner

| | Before | | | After | | |
|---|---|---|---|---|---|---|
| | Trans. WAO group (1) | WIA group (2) | Dif. WIA and Trans. WAO (3) | Trans. WAO group (4) | WIA group (5) | Dif. WIA and Trans. WAO (6) |
| A. Background characteristics | | | | | | |
| Age | 42.570 | 43.425 | 1.067*** | | | |
| Female (%) | 0.379 | 0.421 | 0.042*** | | | |
| Permanent contract (%) | 0.705 | 0.778 | 0.073*** | | | |
| Temporary contract (%) | 0.123 | 0.082 | −0.041*** | | | |
| Unemployed (%) | 0.171 | 0.139 | −0.032*** | | | |
| B. Labor market outcomes | | | | | | |
| Labor participation (%) | 0.886 | 0.895 | 0.009*** | 0.640 | 0.656 | 0.016*** |
| Observations | 1,589,880 | 5,279,160 | | 3,179,760 | 10,558,320 | |
| Individuals | 26,498 | 87,986 | | 26,498 | 87,986 | |

Notes: 1. "Before": period before individuals fall sick (January 1999 - November 2003 for individuals who fell sick in December 2003; January 1999 - August 2004 for individuals who fell sick in September 2004). "After": period after individuals fell sick (December 2003 - January 2019 for individuals who fell sick in December 200; September 2004 - January 2019 for individuals who fell sick in September 2004). 2. Age is computed at the time individuals fall sick. "Permanent contract", "temporary contract", and "unemployed" refer to labor market status of individuals when they fell sick. 3. Columns 1, 2, 4 and 5 present means of the outcome and background and labor market characteristics of individuals in control and treatment before and after start of sickness. Columns 3 and 6 present differences between individuals insured under WIA and transitional WAO – the estimated coefficient from the regression of the characteristic or outcome as the dependent variable, and an indicator of participation in the WIA as the explanatory variable. Standard errors clustered at the individual level.

Table 1b: Sample means and balancing tests of background characteristics and the outcome in control and treatment before and after sickness for spouses

| | Before | | | After | | |
|---|---|---|---|---|---|---|
| | Trans. WAO group (1) | WIA group (2) | Dif. WIA and Trans. WAO (3) | Trans. WAO group (4) | WIA group (5) | Dif. WIA and Trans. WAO (6) |
| A. Background characteristics | | | | | | |
| Age | 41.955 | 43.022 | 0.855*** | | | |
| Years of cohabitation | 7.031 | 7.396 | 0.365*** | | | |
| B. Labor market outcomes | | | | | | |
| Labor participation (%) | 0.670 | 0.638 | 0.013*** | 0.597 | 0.611 | 0.014*** |
| Observations | 1,589,880 | 5,279,160 | | 3,179,760 | 10,558,320 | |
| Individuals | 26,498 | 87,986 | | 26,498 | 87,986 | |

Notes: "Before": period before individuals fall sick (January 1999 - November 2003 for individuals who fell sick in December 2003; January 1999 - August 2004 for individuals who fell sick in September 2004). "After": period after individuals fell sick (December 2003 - January 2019 for individuals who fell sick in December 200; September 2004 - January 2019 for individuals who fell sick in September 2004). 2. Age is computed at the time individuals fall sick. "Permanent contract", "temporary contract", and "unemployed" refer to labor market status of individuals when they fall sick. 3. Columns 1, 2, 4 and 5 present means of the outcome and background and labor market characteristics of individuals in control and treatment before and after start of sickness. Columns 3 and 6 present differences between individuals insured under the WIA and transitional WAO – the estimated coefficient from the regression of the characteristic or outcome as the dependent variable, and an indicator of participation in the WIA as the explanatory variable. Standard errors clustered at the individual level.

individual-specific, time-varying shocks that are not observed. These error terms are assumed to be uncorrelated among each other and with all the explanatory variables in the model.

$Treated_i$ is a dummy variable that indicates the treatment group, spouses whose partners fall under WIA.[8] The periods of time before and after partners fall sick are the pre-treatment and post-treatment periods, respectively. $Post_t$ is a dummy variable that indicates the post-treatment period. We interact $Treated_i$ with $Post_t$ to capture the mean difference in the outcome variable between the treatment and control groups during the post-treatment period compared to the mean difference between the two groups during the pre-treatment period. In this comparison, the latter difference aims to account for differences between the groups due to factors other than the policy reform. $\gamma$ is the coefficient of main interest and reflects the (total) effect of the WIA reform.[9]

To disentangle the effect of the WIA reform in the short and long run, we consider the following regression:

$$y_{it} = \sum_{l=1}^{10} \gamma_{2l} \ (Treated_i \times d_{lt}) + \sum_{l=1}^{10} \delta_l d_{lt} + \lambda_s + \alpha_i + \varepsilon_{it}. \tag{2}$$

Compared to Equation (1), instead of the $Post_t$ dummy, which indicates the entire post-treatment period, we consider dummies for each of the ten years in the post-treatment period: $d_{lt}$ indicates the $l$-th year from the time the partner falls sick. The pre-reform period is chosen as the base period for comparison. The coefficients on the interaction terms of treatment and year dummies are the estimated treatment effects in a given year after falling sick.

In this DiD setup, treatment and control groups are compared over "event time", since our aim is to compare the behavior of treatment and control groups at a given number of months after the individual has fallen sick. Therefore, "event time" refers to the time periods before and after individuals are reported sick. "Calendar time" on the other hand captures the usual time trend.

### Are the pre-reform time trends common to control and treatment groups?

The main assumption of our identification strategy is that, conditional on observables, control and treatment groups share the same time trend in the potential outcome variable before and after individuals fall sick and face the reform incentives. The assumption is testable during the pre-reform period. Figure 2 shows that control and treatment groups, for both sick individuals and their healthy spouses, share very similar time trends until individuals fall sick, supporting this main identifying assumption. We use regression analysis to formally test this assumption. In particular, we use pre-reform data from January 1999 to August 2004 to estimate a regression where labor participation is the outcome, and calendar year dummies, interactions of treatment and calendar year dummies, and time-invariant individual fixed effects are controls. Year 1999 is chosen as the base year for comparison. Figure 3 plots the coefficient estimates of the treatment and year dummy interactions for individuals before they fall sick (left hand panel) and for their healthy spouses (right hand panel). For both groups, the coefficient estimates are insignificant throughout the pre-reform period. Furthermore, based on the F-test (see the note below the figure) we fail to reject the hypothesis that all the interaction terms are jointly equal to zero at the 5 percent level.

In the section on sensitivity analysis, we provide additional evidence of the causal effects of the DI reform using a regression discontinuity method.

[8] This group dummy has no time variation and is omitted in the fixed effects regression.

[9] We do not separately identify the effects of the different components of the reform, i.e. the extension of the sickness period, the change in financial incentives, and the stricter eligibility criteria.

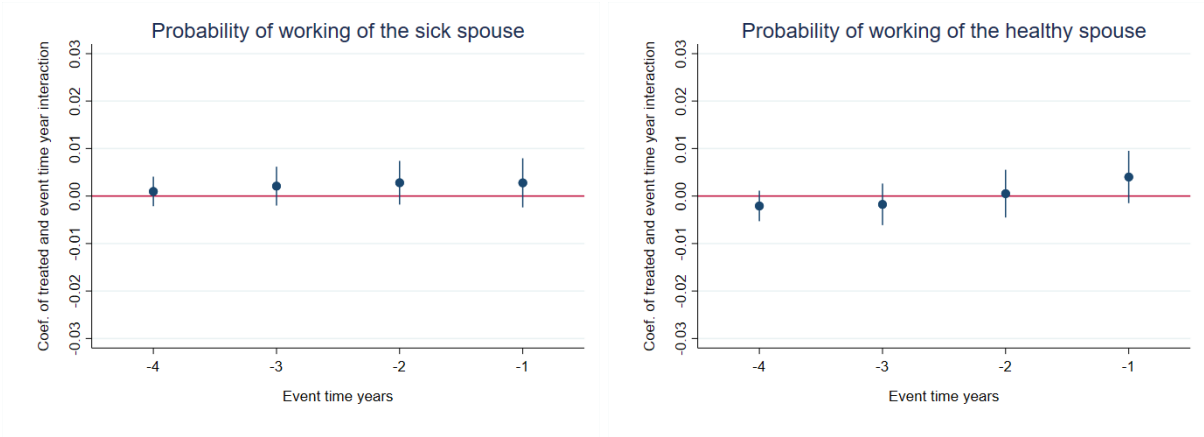


Figure 3: Coefficient estimates of treatment and event year interactions in the pre-reform period based on the regression equation (2) for sick people (left panel) and their spouses (right panel). Around each estimate is a 95 percent confidence interval. The regression is based on pre-reform data from January 1999 to September 2003. 1999 is the base year for comparison (Section 5). Labor participation is the outcome, event year dummies, treatment and event year dummy interactions, calendar month dummies, and time-invariant individual fixed effects are controls. Both regressions for the sick and healthy spouses use 6,813,881 observations for 114,484 individuals where the sick spouse fell ill during the period from October 2003 until September 2004. Standard errors are adjusted for heteroskedasticity and clustering at the individual level. The F-statistic (p-value in parenthesis) of whether the coefficients of all interactions are jointly zero is 0.371 (0.829) for sick people, and 2.162 (0.071) for healthy spouses.

### Do individuals self-select into the old or new disability scheme?

As described in Section 2, falling sick before and after January 2004 determines eligibility for the transitional WAO and WIA schemes, respectively. This means that individuals who have adverse health conditions in 2003 may react by selecting themselves into the transitional WAO or WIA schemes from the moment the reform is announced. In particular, they might anticipate of the much stricter WIA scheme and select themselves into the more lenient transitional WOA scheme. In this case, the estimated impact of the reform on the labor market behavior of the sick partner and his or her spouse might suffer from selection bias. We argue that such self-selection is unlikely. The government presented a general policy program outlining, among other targets, its plan to reform the disability scheme on 15 September 2003. They announced that the sickness period would be extended from one to two years, and a stricter DI law would be introduced for the individuals falling sick as of 1 January 2004. The transitional WAO reform was announced only on 12 March 2004. The details of the WIA reform were announced on 18 August 2004. This means that for people facing adverse health conditions in 2003, reporting sick in anticipation of the transitional WAO reform was impossible. However, following the first announcement in September 2003, individuals could report sickness during the last quarter of 2003 instead of after the implementation of the WIA reform on 1 January 2004. This suggests that reporting sick should increase markedly in the last quarter of 2003.

Figure 4 presents the number of individuals by the month they fall sick. The distribution is fairly uniform and does not suggest any particular pattern and certainly does not suggest that many individuals report sick in the last quarter of 2003 instead of early 2004. On the contrary, if anything, reporting sick seems to increase in January 2004 when the much stricter WIA scheme was introduced.

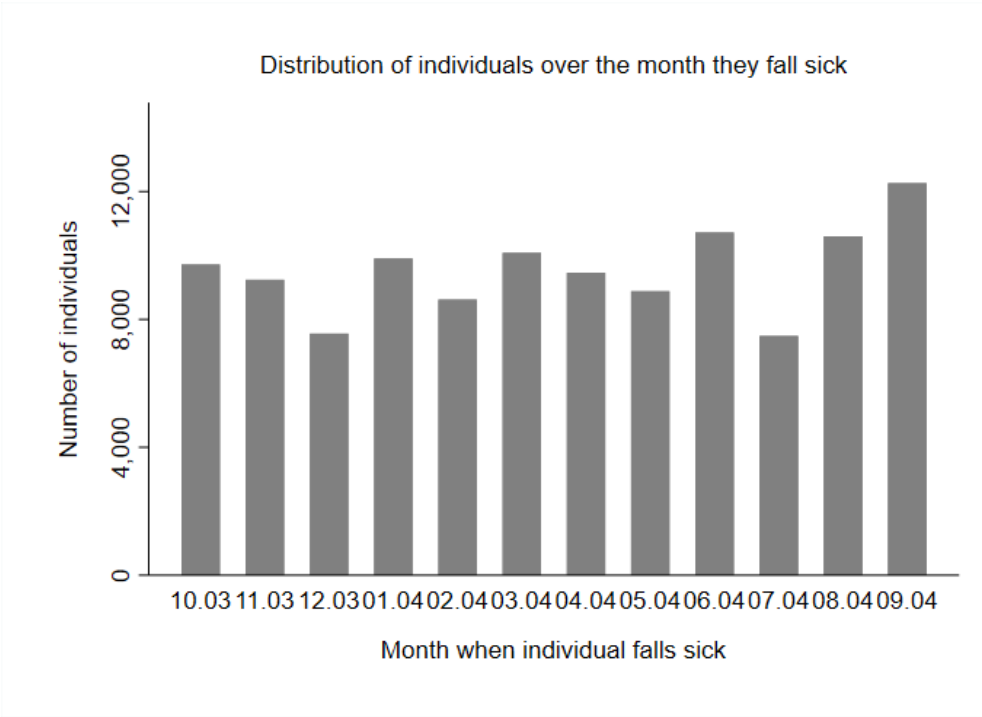


Figure 4: Number of sick people according to the month they fall sick.

# 6 The effect of the reform on labor participation of sick individuals and their spouses

Table 2 presents the baseline DiD estimates of the effect of the DI reform from transitional WAO to WIA, based on Equation (1). For sick individuals with a spouse, the reform increased the probability of working by 2.5 percentage points on average during the first ten years after the reform came into effect. The reform also induced the spouses of the sick individuals to increase their labor participation by 0.8 percentage points. Both effects are significant and sizable.

### Long-term effect

The exploratory analysis in Section 4 suggested that the effect of the reform on labor participation of spouses does not notably vary over time. Here we use regression analysis to test whether the impact of the reform is constant over time. Based on equation (2), Figure 5 presents the coefficient estimates of treatment and year dummy interactions over the post-reform period of ten years. The figure shows that the effect of the reform on spousal labor participation is slightly less than 1 percentage point and statistically significant during the entire post-treatment period. We also fail to reject equality of all coefficients using an F-test (see the note below the figure). This confirms that the impact of the reform on spousal labor supply is persistent and does not fade away in the long run.

### Heterogeneous effects

We analyze whether the DI reform affected individuals with different background and labor market characteristics differently. We consider differential effects by gender and three types of work status when falling sick: a permanent work contract, a temporary work contract or

Table 2: Linear model explaining the effect of the reform on labor participation of the sick and healthy spouses

| | Sick spouse | Healthy spouse |
|---|---|---|
| Treated × Post | 0.025*** | 0.008*** |
| | (0.002) | (0.002) |
| Observations | 20,915,280 | |
| Individuals | 114,484 | |

Notes: ***, **, * denote statistical significance at 1, 5 and 10 percent, respectively. Standard errors (in parentheses) account for heteroskedasticity and clustering at the individual level. In all specifications we control for individual and calendar month fixed effects.


Figure 5: Coefficient estimates of treatment and event year interactions in the post-reform period based on the regression equation (2) for healthy spouses. Around each estimate is a 95 percent confidence interval. The regression is based on post-reform data from October 2003 to July 2015. Pre-reform period is the base for comparison (Section 5). Labor participation is the outcome, event year dummies, treatment and event year dummy interactions, calendar month dummies, and time-invariant individual fixed effects are controls. The regression uses 20,551,961 observations for 114,484 healthy spouses whose partners fell sick during the period from October 2003 until September 2004. Standard errors are adjusted for heteroskedasticity and clustering at the individual level. The F-statistic (p-value in parentheses) for the equality of the coefficients of treatment and event year interactions is 0.327 (0.967).

Table 3: Linear model explaining the effect of the reform on labor participation of sick and healthy spouses by gender

| | | Sick spouse is male, healthy spouse is female | Sick spouse is female, healthy spouse is male |
|---|---|---|---|
| Sick spouse | Treated × Post | 0.019*** | 0.031*** |
| | | (0.003) | (0.004) |
| Healthy spouse | Treated × Post | 0.008*** | 0.016*** |
| | | (0.003) | (0.003) |
| Observations | | 12,304,022 | 8,611,258 |
| Individuals | | 67,414 | 47,074 |

Notes: ***, **, * denote statistical significance at 1, 5, and 10 percent, respectively. Standard errors (in parentheses) account for heteroskedasticity and clustering at the individual level. In all specifications we control for individual fixed effects and calendar month fixed effects.

Table 4: Linear model explaining the effect of the reform on labor participation of sick and healthy spouse by labor market status of the sick spouse

| | | Sick spouse on permanent contract | Sick spouse on temporary contract | Sick spouse unemployed |
|---|---|---|---|---|
| Sick spouse | Treated × Post | 0.030*** | 0.013 | −0.037*** |
| | | (0.003) | (0.008) | (0.007) |
| Healthy spouse | Treated × Post | 0.001 | 0.030*** | 0.016*** |
| | | (0.003) | (0.007) | (0.006) |
| Observations | | 15,946,971 | 1,913,069 | 3,055,240 |
| Individuals | | 87,183 | 10,523 | 16,778 |

Notes: ***, **, * denote statistical significance at 1, 5, and 10 percent, respectively. Standard errors (in parentheses) accounts for heteroskedasticity and clustering at the couple level. In all specifications we control for individual fixed effects and calendar month fixed effects.

a contract through a temporary work agency, and unemployment. Tables 3 and 4 present results based on the estimation of the regression equation (1) for these subgroups, for labor participation of the sick individuals as well as their spouses.

Table 3 presents the results by gender. Sick women respond to the reform more than men. It might be that it is easier for women to find jobs that fit their partial disability. For example, women may engage in part-time jobs and still utilize their remaining work capacity. Among the healthy spouses, however, men respond more strongly than women. The differences between the coefficients of men and women are significant at the 10 percent level for sick individuals and at the 5 percent level for their spouses.

If we estimate equation (2) by gender, the conclusion is the same as for the benchmark analysis (details are not presented): for both men and women, the effects are persistent over time and do not fade out in the long run.

Table 4 presents the results by work status of the sick individual before he or she fell sick. The top panel of the table shows the results for the sick individuals. The reform increases

labor participation among those with a permanent work contract while it decreases it among the unemployed. It seems that the work resumption incentives brought by the reform induce employers to reintegrate their permanent employees into their job, while they prove ineffective if there is no employer (Koning and Lindeboom, 2015). For the unemployed, the longer sickness period may lead to more human capital loss or a stronger scarring effect, reducing the prospects of finding a job (Arulampalam, 2001; Arulampalam et al., 2001). Moreover, their incentives to resume working may be smaller due to the reform since they can spend an additional year in the sickness scheme.

The bottom panel of Table 4 shows the labor supply responses of the healthy spouses of sick individuals. Spouses of sick individuals with a permanent work contract do not respond to the reform. However, spouses of sick individuals who hold a temporary work contract or are unemployed increase their labor participation significantly. These results suggest that because sick individuals who have a temporary work contract or do not have a work contract struggle to resume working, their spouses increase earnings to compensate for the lost disability benefits and lack of labor income. Sick individuals who hold a permanent work contract do increase their labor participation so that their spouses do not need to compensate.

# 7 The effect of the reform on labor participation of sick people with a spouse and on that of sick people without a spouse

In the preceding section we showed that healthy spouses increase their labor participation to compensate for lost disability benefits of their sick partners, but only if their sick partners have a temporary work contract or are unemployed. Here we analyze how the reform affected labor participation of sick individuals who do not have a spouse. These people lack the opportunity to self-insure against income loss through spousal labor supply. This could induce them to increase their labor participation more than sick individuals with a partner do when they face the reform incentives to work.

In Figure 6 we compare the labor participation of sick people with a spouse and that of sick people without a spouse over the observation period. Among the sick people with or without a spouse, we distinguish between control and treatment groups. The two groups of sick people show similar time profiles of labor participation. Time trends of control and treatment groups of sick people without a spouse overlap during the pre-treatment period but differ during the post-treatment period. A similar pattern is observed for sick people with a spouse. The notable difference during the post-treatment period is that the difference between the control and treatment groups for sick people without a spouse is larger than the same difference for sick people with a spouse. This suggests that sick individuals without a spouse respond to the reform more than sick individuals with a spouse.

As in Section 5, we use regression analysis to test for the common trends assumption in the sample for sick people without a spouse. Figure 7 presents the coefficient estimates of the treatment and year dummy interactions based on a regression using pre-reform data from January 1999 to August 2004 for sick people without a spouse. The coefficient estimates are not statistically different from zero except in the last year of the pre-treatment period. Not using data for this year in the DiD estimation has a negligible effect on the estimate of the reform effect. Furthermore, based on the F-test (in the note below the figure) we fail to reject the hypothesis that all the interaction terms are jointly equal to zero at the 5 percent level, providing evidence in favor of the common trends assumption for sick individuals without a spouse.

Table 5 presents the DiD estimate of the effect of the reform based on Equation (1) for sick

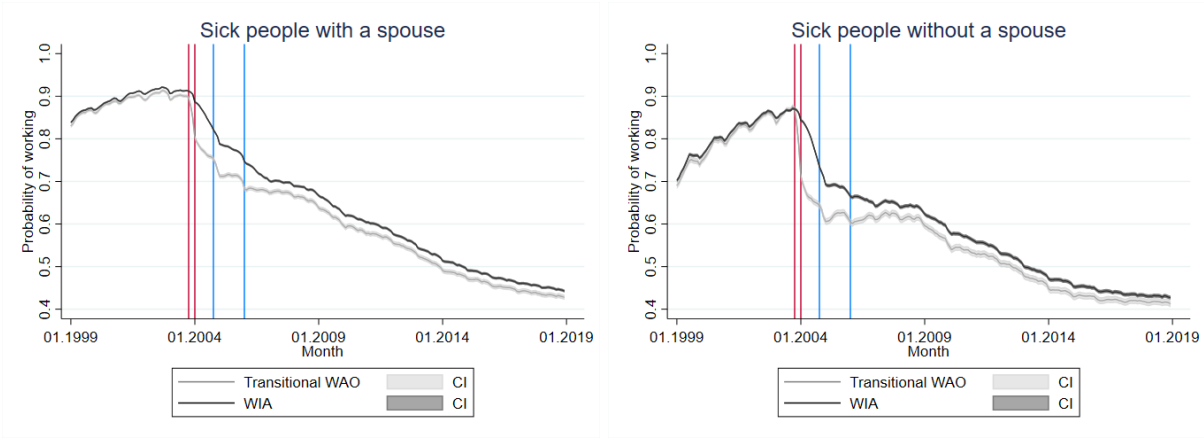


Figure 6: Probability of working for control and treatment groups over calendar months: for sick (left panel reproducing the left panel of Figure 2) and healthy (right panel) spouses. Vertical lines mark the first instance sick partners could become entitled to the sickness and disability benefits in the transitional WAO and WIA schemes. Red lines correspond to 1 October 2003 and 1 January 2004 for the transitional WAO and WIA groups, respectively. Blue lines correspond to 1 October 2004 and 1 January 2006 for the transitional WAO and WIA groups, respectively.

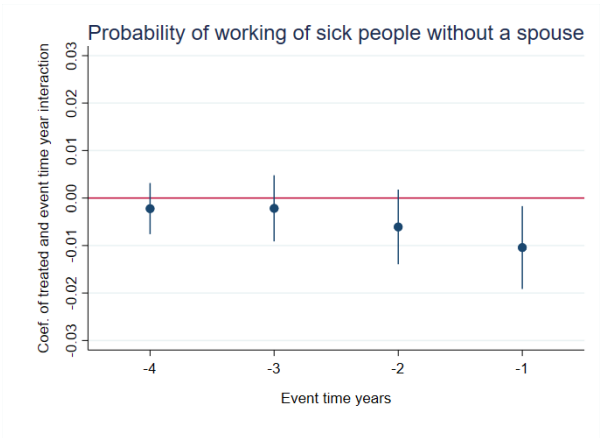


Figure 7: Coefficient estimates of treatment and event year interactions in the pre-reform period based on the regression equation (2) for sick people without a spouse. Around each estimate is a 95 percent confidence interval. The regression is based on pre-reform data from January 1999 to September 2003. 1999 is the base year for comparison (Section 5). Labor participation is the outcome, event year dummies, treatment and event year dummy interactions, calendar month dummies, and time-invariant individual fixed effects are controls. The regression uses 3,591,848 observations for 60,354 individuals who fell sick during the period from October 2003 until September 2004. Standard errors are adjusted for heteroskedasticity and clustering at the individual level. The F-statistic (p-value in parenthesis) of whether the coefficients of all interactions are jointly zero is 1.688 (0.150).

Table 5: Linear model explaining the effect of the reform on labor participation of sick people with and without a spouse

| | Sick people with a spouse | Sick people without a spouse |
|---|---|---|
| Treated × Post | 0.025*** | 0.032*** |
| | (0.002) | (0.004) |
| Observations | 20,915,280 | 11,024,209 |
| Individuals | 114,484 | 60,354 |

Notes: ***, **, * denote statistical significance at 1, 5 and 10 percent, respectively. Standard errors (in parentheses) account for heteroskedasticity and clustering at the individual level. In all specifications we control for individual and calendar month fixed effects.

Table 6: Linear model explaining the effect of the reform on labor participation of sick people with and without a spouse by labor market status of sick people

| | | Permanent contract | Temporary contract | Unemployed |
|---|---|---|---|---|
| Sick people with a spouse | Treated × Post | 0.030*** | 0.013 | −0.037*** |
| | | (0.003) | (0.008) | (0.007) |
| Sick people without a spouse | Treated × Post | 0.030*** | 0.042*** | −0.026*** |
| | | (0.004) | (0.011) | (0.008) |
| Observations | | 7,084,331 | 1,233,905 | 2,095,138 |
| Individuals | | 38,705 | 6,780 | 11,510 |

Notes: ***, **, * denote statistical significance at 1, 5, and 10 percent, respectively. Standard errors (in parentheses) accounts for heteroskedasticity and clustering at the couple level. In all specifications we control for individual fixed effects and calendar month fixed effects. Presented number of observations and individuals are for sick people without a spouse. Numbers for sick people with a spouse are presented in Table 4.

people without a spouse, and reproduces the baseline estimate of the effect of the reform for sick people with a spouse from Table 2. The DiD estimates show that the reform increases the probability of working by 0.7 percentage points more among sick people without a spouse than for sick individuals in couples. In the preceding section, this difference is close to the increase in the participation of healthy spouses found in Table 2 of 0.8 percentage points in response to the reform affecting their partners. It suggests that the response to the disability reform is shared by both partners in a couple. Spousal labor supply is a substitute for sick people's own labor supply when subjected to a stricter disability benefit regime.

Table 6 presents results by work status for individuals without a spouse (bottom panel), and reproduces results for sick people with a spouse (top panel) from Table 4. The table shows similar effects for sick people who are single and sick people in couples if they held a permanent contract or were unemployed before falling sick. There is a substantial difference however if they held a temporary contract, with singles increasing their labor participation by a substantial amount of 4.2 percentage points. In the preceding section, Table 4 showed that healthy spouses increase their labor participation by 3 percentage points in response to the

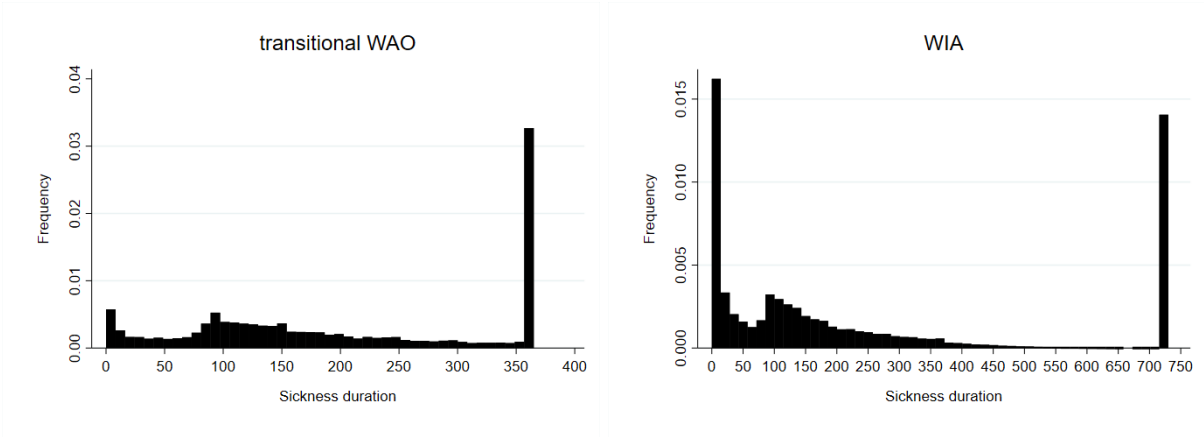


Figure 8: Distributions of the number of days spent in sickness among individuals who fall sick in October, November and December in 2003, and in January, April and July in 2004, and participate in the transitional WAO and WIA, respectively. Left panel shows the distributions when the number of days spent in sickness ranges from 0 to 360 where 360 is the the maximum number of days individuals are allowed to spend in the sickness scheme if they participate in the transitional WAO. Right panel shows the distributions when the number of days spent in sickness ranges from 0 to 720 where 720 is the maximum number of days individuals are allowed to spend in the sickness scheme if they participate in the WIA.

reform that affect their sick partners if their sick partners hold a temporary work contract. Here we see that if spousal labor supply cannot compensate, sick people's own labor supply responds much more. When sick people are on a permanent contract, those in a partnership and those who are single increase their labor participation by the same amount of 3 percentage points, and there is no response of the spouse. Overall, we find that the response of singles is close to the sum of the effects on the two partners for all three initial labor market states.

# 8 Sensitivity checks

### Number of days spent in sickness

As explained in Section 3, before the WIA reform, employers were mandated to report only the sickness cases that lasted longer than 90 days. This means that short-term sickness cases are under-reported for those insured under the transitional WAO. Figure 8 presents distributions of sickness duration for people who fell sick under the transitional WAO scheme (with a maximum number of 360 days in sickness), and under the WIA scheme (with a maximum number of 720 days in sickness). The distributions confirm that short-term sickness cases before the reform are under-reported. Otherwise, the distributions are comparable with many people spending about 100 days in sickness and with spikes at the maximum of one and two years of sickness duration for the transitional WAO and WIA groups, respectively

In the analysis, we restricted the initial sample of sick individuals to those who spent at least 90 days in the sickness scheme to ensure that groups of individuals who fell sick in the fourth quarter of 2003 and the first three quarters of 2004 are comparable in the number of days spent in sickness. Here we examine to which extent the estimated effects of the reform are sensitive to the lower bound of 90 days of sickness for selection into the sample.

Table 7 presents results for equation (1) estimated on subsamples of healthy spouses whose partners have spent at least 1, 120, 180 and 270 days in the sickness scheme. Comparing with

Table 7: Linear model explaining the effect of the reform on labor participation of healthy spouses by the number of days their partners spend in sickness

| | All cases | Cases longer than 120 days | Cases longer than 150 days | Cases longer than 180 days | Cases longer than 270 days |
|---|---|---|---|---|---|
| Treated × Post | 0.014*** | 0.008*** | 0.008*** | 0.007*** | 0.007** |
| | (0.002) | (0.002) | (0.003) | (0.003) | (0.003) |
| Observations | 32,262,617 | 17,656,702 | 15,248,838 | 13,406,296 | 9,931,596 |
| Individuals | 176,250 | 96,626 | 83,419 | 73,312 | 54,263 |

Notes: ***, **, * denote statistical significance at 1, 5, and 10 percent, respectively. Standard errors (in parentheses) account for heteroskedasticity and clustering at the individual level. In all specifications we control for individual fixed effects and calendar month fixed effects.

the baseline results in Table 2, we find a substantially larger effect of the reform if all sickness cases are included. A potential explanation is that the WIA group includes substantially more short-term sickness cases who on average have larger remaining work capacity and are better able to increase their labor participation. We find no notable differences with the baseline estimates if we use a more restrictive lower bound than 90 days. This shows that the baseline estimates of the effect of the reform is to the lower bound, as long as this is large enough to account for the change in reporting requirement.

### Regression Discontinuity instead of Difference-in-Differences

Our DiD estimates of the effect of the WIA reform rely on the assumption that trends of the outcome variable over event time would have been the same for the treatment and control groups had the reform not been implemented. Although it is not possible to directly test this assumption, we provided evidence that trends are parallel in the pre-treatment period. Here we argue that the fact that we find a significant effect of the reform does not depend on the specific identifying assumption we made. We consider an alternative identification strategy that relies on different identifying assumptions, and show that the reform still has significant effects on the outcome.

We exploit the date at which the WIA reform came into effect as a source of exogenous variation in treatment status. The assignment to the treatment or control group is a deterministic step-function of the date at which people fell sick. That is, people who fell sick right before 1 January 2004 are insured under the transitional WAO scheme, while people who fell sick right after this "cut-off" date are insured under the WIA scheme. We rely on a sharp regression discontinuity (RD) design to estimate the effect of the reform on labor participation of healthy spouses and sick people with and without spouses. The treatment effect can be estimated as the jump in the outcome variable at the cut-off which can be formulated as

$$\alpha_{RD} = \lim_{x \downarrow c} \mathbb{E}[Y_i \mid X_i = x] - \lim_{x \uparrow c} \mathbb{E}[Y_i \mid X_i = x] \tag{3}$$

where $X_i$ is the date at which people fall sick and $c$ is the cut-off point of 1 January 2004.

The sharp RD design relies on two main assumptions (Imbens and Lemieux, 2008). The first assumption requires a sharp discontinuity in treatment. This assumption holds in our setting

by design of the reform, since all individuals $i$ for which $X_i \geq c$ are in the treatment group (WIA regime) and all individuals $i$ for which $X_i < c$ are in the control group (transitional WAO regime). The second assumption requires continuity in potential outcomes as a function of the assignment variable around the cut-off point. This implies that had the reform not been implemented, the outcome variables should not discontinuously jump at the cut-off point. Or, in other words, "all other factors" driving the outcome variables must be continuous at the cut-off point (Hahn et al., 2001). We cannot test this assumption directly as we do not observe the counterfactual scenarios. Still, no other institutional reform or event occurred on 1 January 2004 that would suggest that potential outcomes are not continuous at the cut-off. In particular, this assumption could be violated if individuals could self-select themselves into the treatment or control group. The government only announced the transitional WAO reform on 12 March 2004, and the details of the WIA reform were presented on 18 August 2004. This implies that, at the time people fell sick, they did not know the details of the DI scheme that they would potentially be eligible for. Therefore, reporting sick in reaction or anticipation of the reform is very unlikely.

Assuming that the two assumptions are satisfied, the sharp RD identification strategy comes with two main concerns compared to the DiD method. First, RD only identifies the local average treatment effect (LATE) for a specific group of people. In fact, those who fell sick around 1 January might differ from the average sick person. In particular, the number of reported sickness cases show some seasonality. For example, fewer cases are reported in December than in November or January. This might indicate that people who fall sick in December are different from people who fall sick in other months of the year. It is plausible that people with mild symptoms are not reported sick during the Christmas holiday. Furthermore, during the holiday season many people are not working and therefore the probability of an (occupational) injury is lower than in other months. The second concern is that the effect of the reform might not be immediate so that it may not have affected people who fell sick close to the date the new scheme was introduced. For example, it might have taken time for organizations to fully adopt to the new scheme and implement its rules. Still, in spite of these concerns, we can expect that the LATE points in the same direction as the average treatment effect on the treated (ATET) estimated in the preceding section. We exploit the fact that DiD and sharp RD rely on different assumptions, and we show that regardless of which assumptions we are willing to make, we find evidence of statistically significant effects of the WIA reform.

The LATE parameter $\alpha_{RD}$ can be estimated parametrically as follows, where equation (4) is the counterpart of equation (3):

$$\hat{\alpha}_{RD} = \hat{\beta}_+ - \hat{\beta}_- \tag{4}$$

where $\hat{\beta}_+$ and $\hat{\beta}_-$ are estimated via weighted least squares:

$$\min_{\alpha,\beta_+,\beta_-} \sum_{i|X_i \in (c-h,c+h)} \sum_t \{Y_{it} - \alpha - r_-(X_i - c)\beta_- - r_+(X_i - c)\beta_+\}^2 K_h(X_i - c) \tag{5}$$

where $X_i$ is the date at which sick people fall sick (the "runing variable"), $r_- = r_-(X_i)$ ($r_+ = r_+(X_i)$) is an indicator for $X_i$ being below (above) the cut-off, $K_h(X_i - c) = K(X_i - c/h)/h$ with a triangular Kernel function $K(\cdot)$ and a positive bandwidth $h$. We use an MSE-optimal bandwidth selector for the RD treatment effect estimator (see Calonico et al., 2014). We use a robust variance estimator clustered at the individual level in order to account for the correlation of the error terms across calendar months for the same individual. Since the distance from the cut-off is assumed to be as good as random for individuals who fall sick close to 1 January 2004, not accounting for individual fixed effect should not result in biased estimates. We consider the

Table 8: Sharp RD estimate of the effect of the reform on the labor participation of sick and healthy spouses

| | Sick people with a spouse | Healthy spouse | Sick people without a spouse |
|---|---|---|---|
| $\hat{\alpha}_{RD}$ | 0.033** | 0.049*** | 0.072*** |
| | (0.017) | (0.014) | (0.018) |
| Bandwidth (days) | 24 | 38 | 36 |
| Effective obs. left of cut-off | 574,200 | 1,073,160 | 537,840 |
| Effective obs. right of cut-off | 930,840 | 1,456,080 | 750,120 |

Notes: ***, **, * denote statistical significance at 1, 5, and 10 percent, respectively. The estimates are obtained using a triangular Kernel and an MSE-optimal bandwidth selector. Standard errors are clustered at the individual level.

same time horizon as with the DiD estimates, that is a ten years period after treatment. We pool all monthly observations from 2004 to 2013 for all individuals in the sample, implying 120 observations for each individual, and consider a dummy for employment of the sick individual and the spouse as the outcome variables.

Figure 9 provides graphical evidence. In the figure we distinguish among sick people with a spouse, spouses of sick individuals, and sick individuals without a spouse. For each sample, the figure shows local linear fits for labor participation with symmetric bandwidths of thirty days around the cut-off date along with 95% confidence intervals. The figure shows obvious discontinuities at the cut-off. The jumps are in the expected direction as the groups affected by the reform show a larger response. Furthermore, the magnitudes of the jumps are in line with the magnitudes of the DiD estimates presented in Tables 2 and 5. That is, sick people without a spouse show the largest effect, followed by sick people with a spouse and healthy spouses. The confidence intervals for fitted lines on the left and right of the cut-off do not overlap, suggesting that the reform has a significant effect on the employment probability.

Table 8 presents estimated local treatment effects based on model (4). Both the RD and DiD estimators provide evidence of a positive and significant effect of the reform on the employment probability of healthy spouses and sick individuals with or without spouse. The RD estimates, however, are much larger and less precise than the DiD estimates. Overall, both identification strategies provide evidence that sick people in couples rely on the labor supply of their spouses to counterbalance the effect of the DI reform. This is confirmed by the finding that, due to the reform, sick individuals without a spouse increase their labor participation more as they are not able to compensate through spousal labor supply.

# 9 Conclusion

In the beginning of the century, the Netherlands registered one of the highest shares of individuals claiming disability benefits in the working population. In response, the Dutch governments implemented social security reforms to reduce the number of DI claims and the burden on the government budget and to promote work resumption. After several smaller reforms, the DI scheme introduced in 1967 (WAO) was replaced by a new scheme (WIA) in 2006. WIA introduced strong incentives for employees and employers to stimulate work resumption, both in the disability scheme and in the sickness scheme that precedes it, and stricter criteria to receive DI benefits.

While most studies on the impact of Dutch DI reforms focus on the labor market outcomes

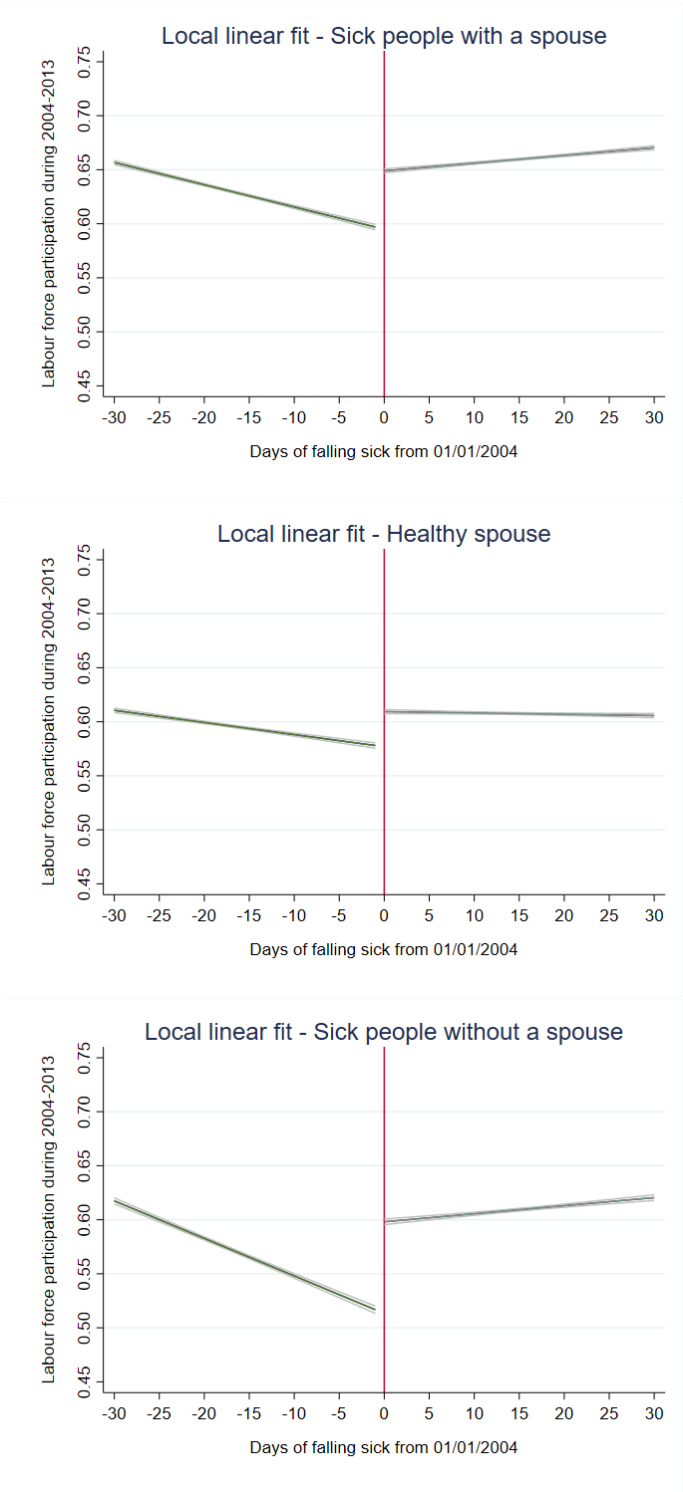


Figure 9: Local linear fit on the two sides of the cut-off with 95% confidence intervals.

of the sick people, we also evaluate the impact of the WIA reform on spousal labor supply. Since couples can pool income risk and jointly adjust their employment status, spousal labor supply can be an important self-insurance mechanism to counterbalance the loss of DI benefits due to the more stringent DI access criteria brought by the WIA reform. Using unique administrative data and a DiD identification strategy, we evaluate the impact of the WIA reform on spousal labor supply with respect to the existing DI insurance regime at the time WIA was introduced, that is the transitional WAO.

First, we show that in fact the reform had a positive effect on spousal labor market participation. In particular, the spouses of people who fell sick under WIA work, on average, 0.8 percentage points more often than the spouses of people insured under the transitional WAO. The effect of the reform on the sick people themselves is 2.5 percentage points. This implies that around one fourth of the couples' response in terms of labor supply comes from the spouses. The share of the response attributable to spouses is particularly relevant given that another major Dutch DI reform implemented in 1993 had no significant effect on spousal labor supply (Borghans et al., 2014).

Second, we find that the spousal labor supply response to the reform is stronger for male than for female spouses (around 1.6 and 0.8 percentage points, respectively). On the other hand, the response of sick females is larger than that of sick males (3.1 and 1.9 percentage points, respectively). This implies that, at the couple level, the overall response is larger for couples where the female partner fell sick. For these couples, the spousal labor response accounts for more than one third of the overall response at the couple level.

Third, we show that the impact of the reform is persistent during the ten years following the start of sickness. Moreover, the effect on spousal labor supply is constant over time and does not decrease in the long run. This is particularly important from a policy perspective, since earlier reforms failed to remain effective in the long run. This in particularly true for the effect of the other major WAO reform that took place in 1993, for which Borghans et al. (2014) found that the effect of the reform for new cohorts was short-lived.

Fourth, we find that substantial heterogeneity of the effect of the WIA reform on spousal labor supply exists with respect to the employment contract of the sick individual at the time he or she fell sick. In particular, our evidence is consistent with the hypothesis that partners substitute for each other's labor supply. We find that people who had a permanent contract at the time they fell sick increased labor market participation by 3.0 percentage points due to WIA, while their spouses did not adjust their labor supply. On the other hand, people who had a temporary contract at the time they fell sick did not increase labor market participation because of the reform, while their spouses increased labor participation by 3.0 percentage points. Overall, this shows that the response at the couple level is around 3 percentage points regardless of the type of contract the sick partners had at the time they fell sick. In the first case the response is only driven by the sick partners, while in the second it is only driven by the spouses. This suggests that there is scope to model the joint labor supply decision of both partners following the DI reform. This is left to future research.

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