

# The added worker effect: Evidence from a disability insurance reform

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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. Using administrative data on all individuals who reported sick shortly before and after the reform, we study the impact of the reform on labor participation of individuals who fell sick and their spouses during the next ten years. Difference-in-differences estimates imply that the reform increased the labor participation of sick individuals and their spouses by 1.1 and 0.9 percentage points, respectively. Both effects are persistent in the years following the reform. We interpret the effect on the spouse as an “added worker effect” where additional earnings of the spouse compensate the sick individual’s income loss so that both partners share the burden of a more stringent disability scheme. This interpretation is confirmed by estimations that distinguish between individuals who had a permanent work contract, a temporary contract, or were unemployed when falling sick. It is also in line with differences between reform effects on sick individuals with and without a spouse.

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# 1 Introduction

In the beginning of this century the Netherlands became one of the countries with the highest share of disabled workers in the insured population. In 2002, this share was around 11 percent, with almost one million DI recipients (Koning and Lindeboom, 2015). To reduce the number of DI recipients and promote work resumption, successive governments implemented several reforms of the DI system. 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 the old system (WAO) was replaced entirely by WIA. The transitional scheme preserved the main features of WAO except that entry criteria were made stricter. WIA introduced major changes in both the DI scheme and the sickness insurance (SI) scheme that precedes it. The duration of SI was extended from one to two years. Since employers must compensate the employee for wage loss during the sickness period, this implies that employer incentives to facilitate work resumption increased. For the DI scheme, WIA introduced stricter entry criteria and stronger incentives for work resumption, both for employees and employers.

Kantarci et al. (2019) analyzed the effects of WIA reform on labor participation and benefit receipt among long-term sick individuals. Comparing individuals who reported sick and participated in the WAO and WIA schemes, they showed that the reform reduced the probability of DI receipt by 5.8 percentage points during the ten years after the reform. On the other hand, their labor participation rate rose by 1.8 percentage points and the percentage with unemployment insurance (UI) benefits rose by 1.4 percentage points.

The focus of the current paper is whether spousal labor participation also responds to the reform and the possible loss of DI benefits. We also investigate whether such a response depends on the work status of the sick individual at the time of reporting sick. To identify the reform effects, we compare spouses whose partners reported sick before and after the reform. We exploit administrative data on all individuals who reported sick in the last quarter of 2003 (potential participants in transitional WAO) or the first quarter of 2004 (falling under WIA).

We find that among sick individuals with a spouse, labor participation rose by 1.1 percentage points due to the DI reform, while their spouses' labor participation rose by 0.9 percentage points. Furthermore, the reform increased the probability of working by 1.5 percentage points among sick individuals without a spouse. This suggests that the negative income effect of the DI reform is shared by partners in a couple: spousal labor supply is a substitute for sick people's own labor supply when facing a stricter disability benefit regime. Considering heterogeneous effects of the reform, we find that spouses respond to the DI reform only when the sick individuals have an unstable labor market position. Sick individuals who had a permanent work contract at the time of reporting sick increased labor participation by 2.2 percentage points, while their spouses did not respond. On the other hand, those who had a temporary work contract or were unemployed did not increase labor participation, while their spouses' participation rose by 2.9 and 2.2 percentage points, respectively. Findings for other outcomes (earnings, unemployment benefits) and comparing with individuals without a spouse confirm that the spouse's response is larger when there is a larger need to compensate for the more stringent rules of the new DI system.

These findings contribute to two strands of the literature. The first studies the impact of DI reforms, offering important policy implications for many countries where the number of DI recipients has substantially grown in the last decade (OECD, 2018). This literature mainly focuses 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 increasing (Zaresani, 2018, 2020) or 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; Deuchert and Eugster, 2019; Ruh and Staubli, 2019). These studies do not consider spill-over effects on the spouse. Our findings suggest that for a complete evaluation of the DI reform, it is important to consider such spill-over effects on both labor participation and the adequacy of household income.

The second strand analyzes income complementarities in households as an insurance mechanism. The “added worker effect” hypothesis suggests that married women respond to a negative shock on their husbands’ earnings by increasing their hours of paid work (Lundberg, 1985). Most studies find no or a small added worker effect (Maloney, 1987, 1991; Spletzer, 1997; Bretdmann et al., 2018; Halla et al., 2020). One explanation is that the affected partner is already insured through social insurance so that the spouse does not need to respond (Cullen and Gruber, 2000; Bentolila and Ichino, 2008). Couples may also self-insure through savings and run down their wealth in case of a negative income shock (Blundell et al., 2016). Another explanation is that the wife’s earnings are low compared to the husband’s, so that the wife’s labor supply response does not fully compensate for the earnings loss of the husband (Cullen and Gruber). Moreover, the wife’s response will be small if the husband’s unemployment only leads to a transitory reduction in earnings (Cullen and Gruber; Bretdmann et al.) or if the husband’s unemployment is anticipated by the household and the expected income loss already led to adjustments in household consumption and labor supply. In addition, the wife’s response will depend on the magnitude of the expected loss in lifetime income (Cullen and Gruber; Stephens, 2002; Bretdmann et al.). A recent exception that does find a notable added worker effect is Blundell et al., who show that of the total amount of consumption insured against permanent shocks to the husband’s wage, about 63 percent comes from family labor supply. Schöne and Strøm (2021) find that the rise in wives’ labor supply annihilates around one third of the loss in husbands’ earnings. Fadlon and Nielsen (2021) provide evidence on the role of spousal labor supply in offsetting permanent household income losses due to fatal events. We add to the limited evidence for the added worker effect by showing that a substantial share of the total labor supply response of couples to the reform comes from the spouse.

At the intersection of these two strands of the literature are a few studies that analyze spousal labor supply responses when sick individuals receive DI benefits or eligibility rules for DI benefits change. Results of Duggan et al. (2010) suggest that an increase in enrollment in the US disability compensation program for veterans due to a legislative change somewhat reduced their wives’ labor supply. Borghans et al. (2014) studied the impact of reassessing existing DI recipients and new applicants younger than 45 years of age based on new DI eligibility criteria introduced in 1993 in the Netherlands. They found that affected individuals were able to fully offset the loss of DI benefits with higher earnings and social support income, but they found no significant effect on spousal earnings. Garcia Mandico et al. (2021) analyzed the impact of reassessing DI recipients younger than 45 years based on new DI eligibility criteria introduced in 2004 in the Netherlands. They found a significant positive effect on male spouses but no significant effect on female spouses. Autor et al. (2019) analyzed the consequences of DI receipt for labor supply and consumption decisions of households in Norway. They show that DI denial has little impact on income and consumption of married couples, since spousal earnings and benefit substitution counteract the effect of denial of DI benefits.

Our study differs from these studies in three ways. First, the DI reform in 2006 differs from the DI reform in 1993 studied by Borghans et al. (2014): WIA provides disabled people with strong and unprecedented incentives to utilize their remaining work capacity, and this could limit the need for an increase in spousal labor supply. Furthermore, WIA affected new applicants but not existing DI recipients. Existing recipients who are denied DI benefits later

on might behave differently from new applicants. Moreover, the DI reform in 1993 only affected people younger than 45 years, while WIA applies to all sick individuals.

Second, [Autor et al. \(2019\)](#) struggle to find evidence on heterogeneous effects in labor supply decisions of couples, possibly due to limited statistical power. We show that job security of the sick spouse is an important determinant of the labor supply decisions of both spouses. We also show that the added worker effect is evident for both wives and husbands whereas earlier studies focus on wives' responses to husbands' income shock. Moreover, unlike earlier studies, we analyze the labor supply decisions of singles to better understand the decisions of couples. This analysis validates our finding that partners share the burden of the more stringent disability scheme after the reform. Finally, it shows that, among those who are unemployed, the benefit substitution effect of stricter eligibility criteria for DI is much stronger when a spouse is present.

This paper proceeds as follows. Section 2 explains the 2006 reform. Section 3 describes the data and the study sample. Section 4 gives descriptive evidence on the impact of the reform on spousal labor supply. Section 5 presents the empirical approach used to identify the effect of the reform. Section 6 discusses the results for couples and 7 compares with the reform effects on singles. Section 8 concludes. Appendices present robustness checks.

## 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 work disability. After major amendments in 1993 it preserved its main features until 2006. Under WAO, individuals earning wages or receiving unemployment insurance benefits (UI) who were unable to perform their work because of occupational or non-occupational illness or injury, were first admitted to the sickness scheme, with a duration of one year. The employer had to pay 70 percent of the former wage during this period; most employers paid the full amount. If there is no employer, the "Sickness benefit" is paid by the Employee Insurance Agency (UWV).<sup>1</sup> When the sickness scheme expired, individuals were admitted to the disability scheme (DI) if their disability grade was at least 15 percent. They were first entitled to the "Wage-loss benefit" and after this expired to the less generous "Follow-up benefit". The Wage-loss benefit replaced 70 percent of the former wage multiplied by the disability grade. Its duration depended on the individual's age, with a maximum of 6 years. The Follow-up benefit paid the minimum wage and an additional amount depending on the former wage and the age at which the individual became entitled to the benefit. It was paid as long as the individual was disabled or until the state pension age (65 years).

Due to easy access, the annual inflow rate into WAO rose to 1.5 percent of the insured working population in 2001, leading to reforms. A transitional scheme was introduced on 1 October 2004 for people who reported sick between October 1 2003 and January 1 2004. In this scheme, the features of the sickness and disability schemes of WAO were preserved except that entry criteria were made stricter. In particular, it 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 to qualify for DI and to reach a higher disability grade (with a higher benefit).

WIA applies to individuals who reported sick from 1 January 2004 onwards. It introduced major changes in both sickness and disability schemes, stimulating 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 after its introduction ([Koning and Lindeboom, 2015](#)). The duration of

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<sup>1</sup>UWV is the public body that implements employee insurance policies, and provides labour market and data services on behalf of the Ministry of Social Affairs and Employment.

the sickness scheme was extended from one to two years. The employer is obliged to compensate the employee for 70 percent of the wage loss during the two-year period, creating a strong incentive for the employer to facilitate work resumption.<sup>2</sup>

WIA kept the stricter eligibility criteria of the transitional WAO scheme with the broader definition of what work can still be done. In addition, the minimum disability grade for entering the scheme rose from 15 to 35 percent – under WIA, workers with limited disability are expected to resume working (with adaptations of their work if necessary) or apply for UI. Moreover, the scheme introduced a distinction between full and partial disability and strong financial incentives for partially disabled people to utilize remaining work capacity. Finally, experience rating for employers was extended from 5 to 10 years and it applied to partially disabled workers instead of all disabled workers, creating more effective incentives to reintegrate workers on sickness or DI benefits as quickly as possible. Experience rating was limited to permanent work contracts until 2013, and was extended to temporary contracts afterwards. All in all, compared to WAO, WIA provides more targeted and often stronger incentives to exploit remaining work capacity during both the sickness and disability periods.

### 3 Data

We use unique administrative data from the UWV on all individuals who reported sick in the fourth quarter of 2003 or the first quarter of 2004, and therefore could become eligible to either the transitional WAO or the WIA scheme.<sup>3</sup> We observe the beginning and ending dates of their sickness, their gender, and date of birth. They either earn a wage or receive UI at the time they report sick – other groups cannot 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 these individuals to administrative data on themselves and their partners (married or cohabiting) from Statistics Netherlands (CBS), with monthly information on wages and benefits. The benefits include DI, UI and social assistance. These data extend from January 1999 to February 2014, allowing us to study the impact of the DI reform over a period of 15 years.

The initial sample has 171,281 sick individuals. To select the estimation sample, we drop individuals who participate in the disability schemes for the self-employed (WAZ) or for young people (WAJONG) since their institutional rules and incentives for work resumption are very different. We also drop individuals who already receive DI when they report sick. We drop individuals in same-sex partnerships and only keep couples if their cohabitation started before reporting sick. We drop individuals whose spouse also reported sick between October 2003 and March 2004. Finally, we restrict the sample with respect to the number of days spent in sickness since employers only have to report sickness cases if they last longer than 90 days (temporary work agencies have to report all sickness cases). We divide the individuals into a “control group” of individuals (and their spouses) who fell sick in the fourth quarter of 2003 and were insured under the transitional WAO scheme and a “treatment group” of individuals (and their spouses) who fell sick in the first quarter of 2004 and are insured under the WIA scheme.

Based on the available data on wages and social security benefits, we define the following outcome variables: dummies that indicate labor participation, UI receipt and social assistance receipt, and the monthly amounts of wages, UI benefits, and social assistance. We transform

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<sup>2</sup>Most employers pay the full wage amount during the first year of sickness, and some pay more than 70 percent during the second year.

<sup>3</sup>Individuals who fall sick in the transitional WAO scheme could recover and fall sick again in the WIA scheme. We allow this possibility but do not observe it since data on individuals who fall sick after the first quarter of 2004 is not available.



earnings and benefit amounts as the natural logarithm of the amount plus 1, accounting for the skewed distribution and the zero values. During participation in the sickness scheme, the observed wage combines two types of payments: earnings (for the part remaining work capacity is used) and compensation for lost earnings due to sickness paid by the employer. We do not observe the separate amounts. Since we measure labor participation as positive earnings, this implies that we cannot determine whether or not sick people are working when in the sickness scheme. We therefore discard the first two years after individuals fall sick in most of our analysis.

## 4 Time trends and other descriptive statistics

Figures 1a and 1b show the labor participation rates and fractions of DI, UI and social assistance recipients in control and treatment groups over the observation period.<sup>4</sup> For the individuals who report sick, the inflow into the DI scheme increases sharply when the treatment and control groups become eligible to apply for DI benefits, and it continues to increase during the remaining years of the observation period. The treatment group is much less likely to become entitled to DI benefits, and the sizable difference between the two groups remains stable till the end of the observation period. This shows that the reform effectively limited the access to the DI scheme. For the spouses of sick people, DI receipt is stable during the reform period and is not affected by the reform.

For the individuals who report sick, the probability of working shows a strong time trend that is common to both groups. It increases until the date individuals report sick, reflecting that individuals can enter the sickness scheme only if they are working or receiving UI. 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 fall throughout the remaining years. The difference between control and treatment groups is small and insignificant before individuals fall sick, but notable and significant after they fall sick, suggesting that the reform increased labor participation of those who fell sick. For spouses, the probability of working shows a less pronounced decreasing pattern. The difference between control and treatment groups is not significant either before or after treatment, but it is larger post- than pre-treatment, suggesting that there might be a positive spill-over effect.

For sick individuals in both groups, the use of UI falls sharply right after reporting sick, since those who are unemployed replace UI with sickness benefits. UI use rebounds and increases during the remaining months of the sickness scheme because many individuals recover and replace their sickness benefit with UI. UI use peaks when individuals can apply for DI, because when the sickness period ends, rejected DI applicants turn to UI. UI use falls during the disability period because UI is temporary with a maximum of 38 months.<sup>5</sup> The difference between the control and treatment groups is sizable and statistically significant during the disability period, suggesting that the DI reform increased UI use among those who reported sick. UI use among the spouses is fairly constant over time. The difference between control and treatment groups is insignificant, both pre- and post-treatment.

For both the sick individuals and their spouses, social assistance receipt increases steadily from the time sickness is reported. Again, this seems to be related to labor participation: As labor participation falls, earnings fall so much that households become entitled to social assistance. During the second year after falling sick, less social assistance is needed under the WIA regime since most individuals still receive sickness benefits. During the disability period, however, no significant differences are observed between the WAO and WIA groups.

<sup>4</sup>Similar figures for wages and benefit amounts (not shown) reveal very similar patterns.

<sup>5</sup>The maximum duration of UI changed from 60 to 38 months in October 2006.

Table 1a presents sample means of some background characteristics when reporting sick for both groups, as well as outcomes before and after reporting sick. It also presents tests for equality of the means (“balancing tests”) in control and treatment groups. Panel A shows that, in both groups, the average age is about 43 and there are more men than women. The majority hold a permanent work contract; the others hold a temporary contract, a contract through a temporary work agency, or are unemployed. Column 3 shows that there are small but significant differences between the two groups. These possibly reflect labour market trends. Our identification strategy (difference in differences) accounts for such differences.

Columns 3 and 6 in panel B present mean differences in outcomes during the pre- and post-treatment periods for treatment and control groups. The fraction of sick individuals receiving disability benefits falls due to the reform, as expected. In line with Figures 1a and 1b, the difference is larger post-treatment than pre-treatment for all outcomes except social assistance, again suggesting that the reform may have increased labor participation and earnings, as well as UI receipt and the amount of UI benefits received.

Table 1b reproduces Table 1a for the spouses. Spouses in the treatment group are slightly older than in the control group. Couples in the treatment group have cohabited somewhat longer pre-treatment but not post-treatment. Columns 3 and 6 in panel B show that the difference in labor participation between groups is larger post-treatment than pre-treatment, again suggesting that the reform increased labor participation for the spouses. The mean differences in other outcomes are small and insignificant, both pre- and post-treatment.



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)	Dif. WIA and Trans. WAO (5) (6)
A. Background characteristics					
Age	42.946	43.108	0.162*		
Female (%)	0.379	0.384	0.005		
Permanent contract (%)	0.705	0.717	0.012***		
Temporary contract (%)	0.123	0.106	-0.017***		
Unemployed (%)	0.171	0.176	0.005*		
B. Labor market outcomes					
DI (possibly UI) receipt (%)				0.135	0.106
Labor participation (%)	0.887	0.889	0.002	0.611	0.615
UI (no DI) receipt (%)	0.033	0.033	0.000	0.057	0.063
Social assistance receipt (%)	0.016	0.015	-0.001	0.023	0.024
DI (and possibly UI) received per month				187.298	162.690
Wage per month	1,999.228	2,012.334	13.106	1,713.940	1,738.323
UI (excl. DI) per month	39.938	40.128	0.190	89.747	92.873
Social assistance per month	10.222	9.894	-0.328	19.521	20.675
Observations	1,589,880	1,716,480		2,543,808	2,746,368
Individuals	26,498	28,608		26,498	28,608

Notes: 1. "Before": period before individuals fall sick (January 1999 - October 2003 for individuals who fell sick in November 2003; January 1999 - January 2004 for individuals who fell sick in February 2004). "After": period after individuals fell sick excluding the first two years (November 2005 - January 2014 for individuals who fell sick in November 2003; February 2006 - January 2014 for individuals who fell sick in February 2004). 2. Age is 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 in control (Trans.) and treatment (WIA) group 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)	Dif. WIA and Trans. WAO (5) (6)
A. Background characteristics					
Age	42.333	42.535	0.205**		
Years of cohabitation	7.031	7.173	0.142***	8.269	8.224 -0.045
B. Labor market outcomes					
DI (possibly UI) receipt (%)				0.096	0.094 -0.002
Labor participation (%)	0.670	0.669	-0.001	0.586	0.590 0.004
UI (no DI) receipt (%)	0.016	0.016	0.000	0.027	0.028 0.001
Social assistance receipt (%)	0.015	0.015	0.000	0.025	0.025 0.000
DI (and possibly UI) received per month				115.472	113.790 -1.682
Wage per month	1,299.714	1,312.522	12.808	1,552.520	1,561.632 9.112
UI (excl. DI) per month	18.356	19.220	0.864	38.291	38.761 0.470
Social assistance per month	9.691	9.867	0.176	21.747	21.581 -0.166
Observations	1,589,880	1,716,480		2,543,808	2,746,368
Individuals	26,498	28,608		26,498	28,608

Notes: 1. "Before": period before individuals fall sick (January 1999 - October 2003 for individuals who fell sick in November 2003; January 1999 - January 2004 for individuals who fell sick in February 2004). "After": period after individuals fell sick excluding the first two years (November 2005 - January 2014 for individuals who fell sick in November 2003; February 2006 - January 2014 for individuals who fell sick in February 2004). 2. Age is at the time individuals fall sick. Years of cohabitation for the "Before" period indicates mean years of cohabitation by the time individuals fall sick. That for the "After" period indicates mean years of cohabitation during the period after individuals fell sick including the first two years. 3. Columns 1, 2, 4 and 5 present means 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.

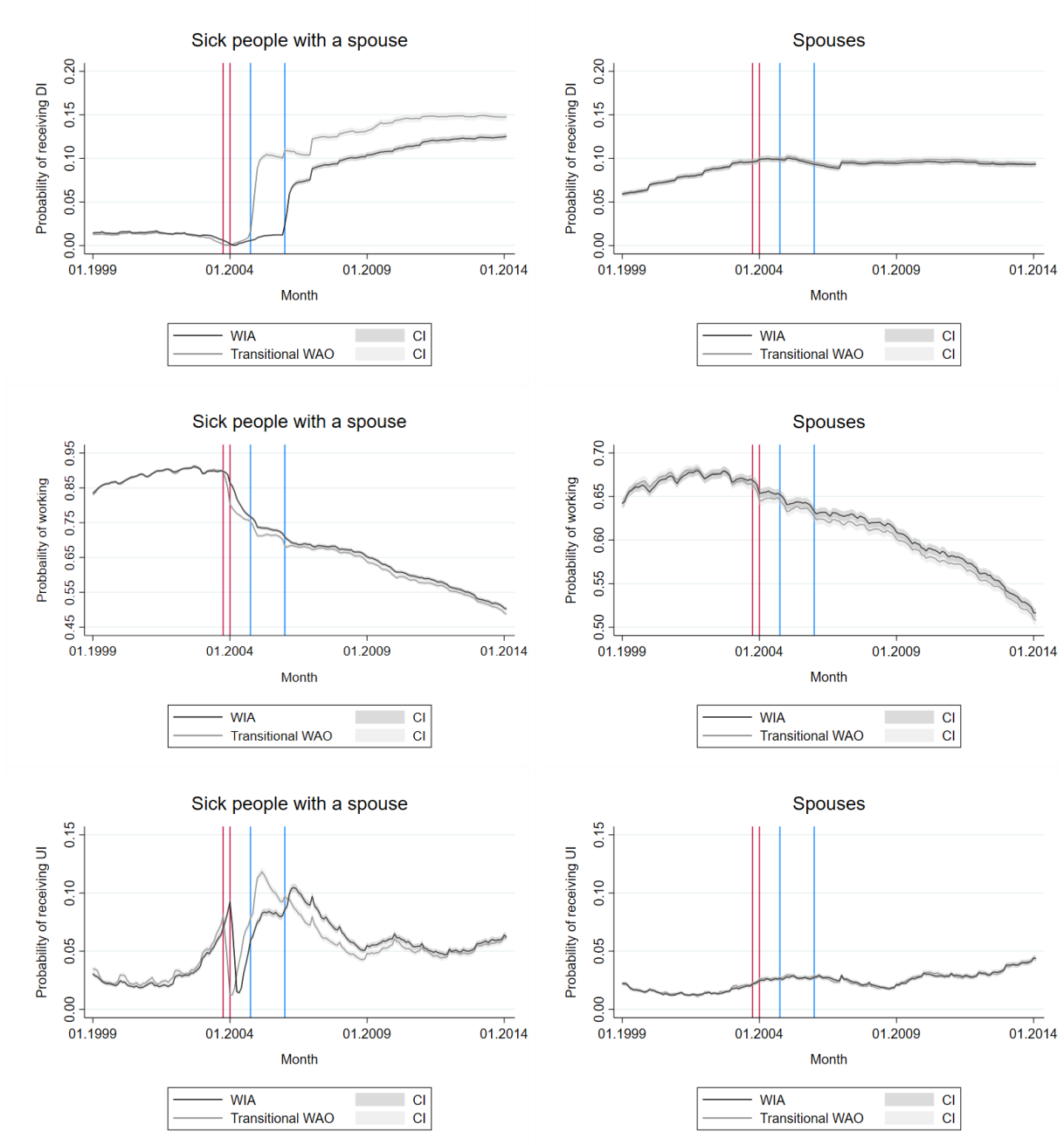


Figure 1a: Probability of DI receipt, working, and UI receipt for control and treatment groups by calendar month; sick individuals (left) and their spouses (right). Vertical lines mark the first instance sick partners can be entitled to the sickness (red) and disability (blue) benefits in the two schemes.

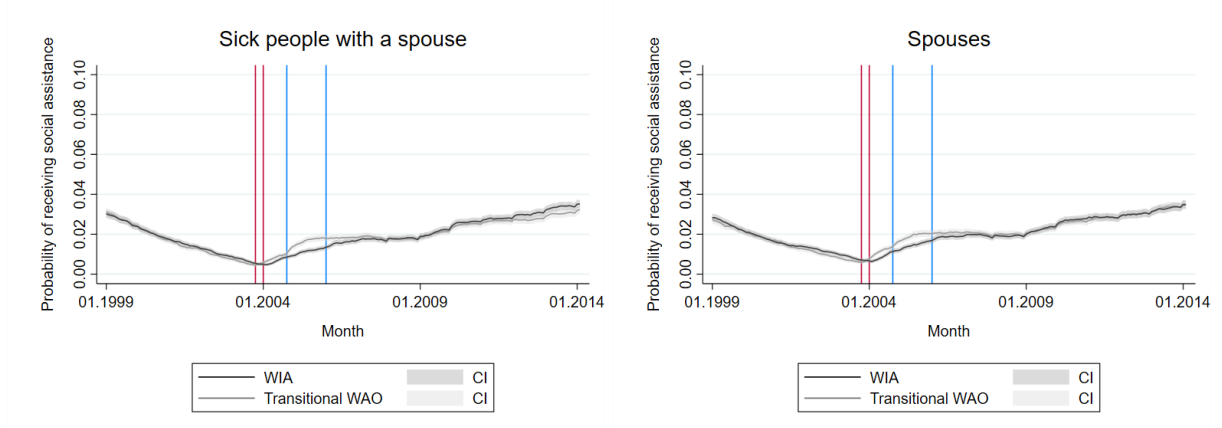


Figure 1b: Probability of social assistance receipt for control and treatment groups by calendar month; sick individuals (left) and their spouses (right). Vertical lines mark the first instance sick partners can be entitled to the sickness (red) and disability (blue) benefits in the two schemes.

## 5 Identification strategy

We use a difference-in-differences approach to identify the causal effect of the WIA reform on each outcome variable  $y_{it}$ , either concerning the sick individual or the spouse. The first difference is across groups. Those who reported sick in the first quarter of 2004 (treatment or WIA group) face different eligibility criteria and incentives to work or claim benefits than individuals who reported sick in the fourth quarter of 2003 (control or transitional WAO group). The second difference is across event time, that is before and after reporting sick.

We implement the DiD comparison using the following baseline regression model:

$$y_{it} = \gamma (Treated_i \times Post_t) + \delta Post_t + \lambda_{s(i,t)} + \alpha_i + \varepsilon_{it}. \quad (1)$$

Here  $i$  indexes the sick individual or their spouse.  $t$  indexes the month of event time: Values  $-57$  to  $-1$  indicate the months before reporting sick,  $0$  is the month when first reporting sick, and  $1$  to  $119$  are the months after reporting sick. (For some outcomes  $y_{it}$ , we do not use observations during the sickness period due to measurement issues; see Section 3).  $\lambda_{s(i,t)}$  is a monthly calendar time effect –  $s(i,t)$  indexes the calendar month (from January 1999 until February 2014; January 1999 is chosen as the base month) for individual  $i$  at a given month of event time  $t$ .  $\alpha_i$  is an individual-specific, time-invariant fixed effect that is potentially correlated with the control variables.  $\varepsilon_{it}$  represents an idiosyncratic (unobserved) shock, assumed to be uncorrelated with all the explanatory variables.

$Treated_i$  is a dummy variable for the treatment group.<sup>6</sup> The periods before and after individuals report sick are the pre- and post-treatment periods;  $Post_t$  is a dummy for the post-treatment event time. The individual effects capture differences between the two groups other than the reform effect. Under the identifying assumption that treatment and control group would have followed the same trend if there would not have been a reform, the coefficient  $\gamma$  on the interaction term  $Treated_i \times Post_t$  captures the mean effect of the reform, the main parameter of interest.<sup>7</sup>

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

<sup>6</sup>This group dummy has no time variation and is omitted in the fixed effects regression.

<sup>7</sup>We cannot 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.

following extended model:

$$y_{it} = \sum_{l=1}^{10} \gamma_l (Treated_i \times d_{lt}) + \sum_{l=1}^{10} \delta_l d_{lt} + \lambda_{s(i,t)} + \alpha_i + \varepsilon_{it}. \quad (2)$$

Instead of  $Post_t$  which refers to the entire post-treatment period, this model has separate dummies for each of the ten years in the post-treatment period:  $d_{lt}$  indicates the  $l$ -th year from the time the individual reports sick. The pre-treatment period is chosen as the base period. The coefficients on the interaction terms of treatment and year dummies are the estimated treatment effects.<sup>8</sup> In this setup, treatment and control groups are compared over event time  $t$ , i.e., months after the individual has reported sick. The calendar time dummies  $\lambda_s(i, t)$  on the other hand capture the (common) calendar time trend.

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

As explained in Section 2, reporting sick before and after January 1 2004 determines eligibility for either transitional WAO or WIA. This means that individuals with adverse health shocks in 2003 might select themselves into the transitional WAO or WIA scheme from the time the reform is announced. In particular, they might anticipate the much stricter WIA scheme and enter the more lenient transitional WAO scheme. In this case, the estimated impact of the reform can be biased. We argue that such self-selection is unlikely. The government presented a general policy program outlining 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 that a stricter DI law would be introduced for the individuals reporting sick as of 1 January 2004. The transitional WAO reform was announced only on 12 March 2004 and details of the WIA reform were announced on 18 August 2004. This means that, 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. If many individuals would do this, reporting sick should increase markedly in the last quarter of 2003.

Table 2 presents the number of individuals by the month they reported sick. The distribution is close to uniform and does not suggest any particular pattern. It 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, after the stricter WIA scheme was introduced.

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

Our main identifying assumption is that, conditional on observables, control and treatment groups share the same time trend in the potential outcome variables before and after individuals report sick and face the reform incentives or not. The assumption is testable during the pre-treatment period. Figures 1a and 1b show that control and treatment groups, both for sick individuals and their spouses, share very similar time trends until individuals fall sick, supporting the identifying assumption. To formally test the assumption, we use pre-treatment data (e.g. from January 1999 to September 2003 for people who fell sick in October 2003) to regress the outcome on biannual event time dummies, interactions of treatment and biannual dummies, calendar month dummies (seasonal effects), and fixed individual effects. The first six months of event time is the base for comparison. Figures 2a and 2b plot the estimates on the treatment and biannual dummy interactions for individuals before they report sick (left hand panel) and

<sup>8</sup>Here we also include observations for  $t = 0, \dots, 23$ .

for their spouses (right hand panel). For both groups and all outcomes, the estimates are insignificant throughout the pre-treatment period. They are also jointly insignificant, except for DI and UI receipt (F-test in the note below the figure). Similar test results (not shown) are obtained for wages and benefit amounts.

### Are the results robust to a regression discontinuity approach?

An alternative identification strategy is a regression discontinuity (RD) approach, using the date of falling sick as the running variable (exploiting that the reform applies to those who reported sick as of January 1, 2004). In Appendix A we provide some results. Both identification strategies lead to the same qualitative conclusions for all outcomes and to similar relative sizes of the effects across sick individuals, spouses, and sick individuals without a spouse. On the other hand, the RD estimates are typically much larger than the DiD estimates and appear to be sensitive to the choice of bandwidth. A possible explanation is the fact that individuals who report sick just before and just after January 1 are different, due to the Christmas holidays. For example, specific groups of workers in specific professions may continue working during the last weeks of the calendar year, whereas others do not. If the difference affects levels but not trends, this is accounted for in the DiD estimates but not in the RD estimates.

### Do couples dissolve their cohabitation due to the reform?

We study the labor supply responses of couples to the DI reform who started cohabiting before reporting sick. Couples can dissolve their cohabitation during post-treatment due to the reform or other reasons. In this case the estimated treatment effect may not only reflect the labor supply responses of individuals to the DI reform as partners but also the labor supply responses to a possible shock to their income due to the cessation of their cohabitation as former partners. In the sample, we find no statistical difference between the fractions of couples ending their cohabitation in the treatment and control groups during post-treatment, suggesting that the reform has no effect on cohabitation status. When we restrict the sample to couples who stay together during post-treatment, the estimated reform effects become slightly larger suggesting that baseline estimates are conservative - see Appendix B.

Table 2: Number of sick people according to the month they report sick

Month when individuals fall sick	Number of individuals	Percent
10.2003	14,925	17.71
11.2003	14,048	16.67
12.2003	11,680	13.86
01.2004	15,119	17.94
02.2004	13,106	15.55
03.2004	15,384	18.26



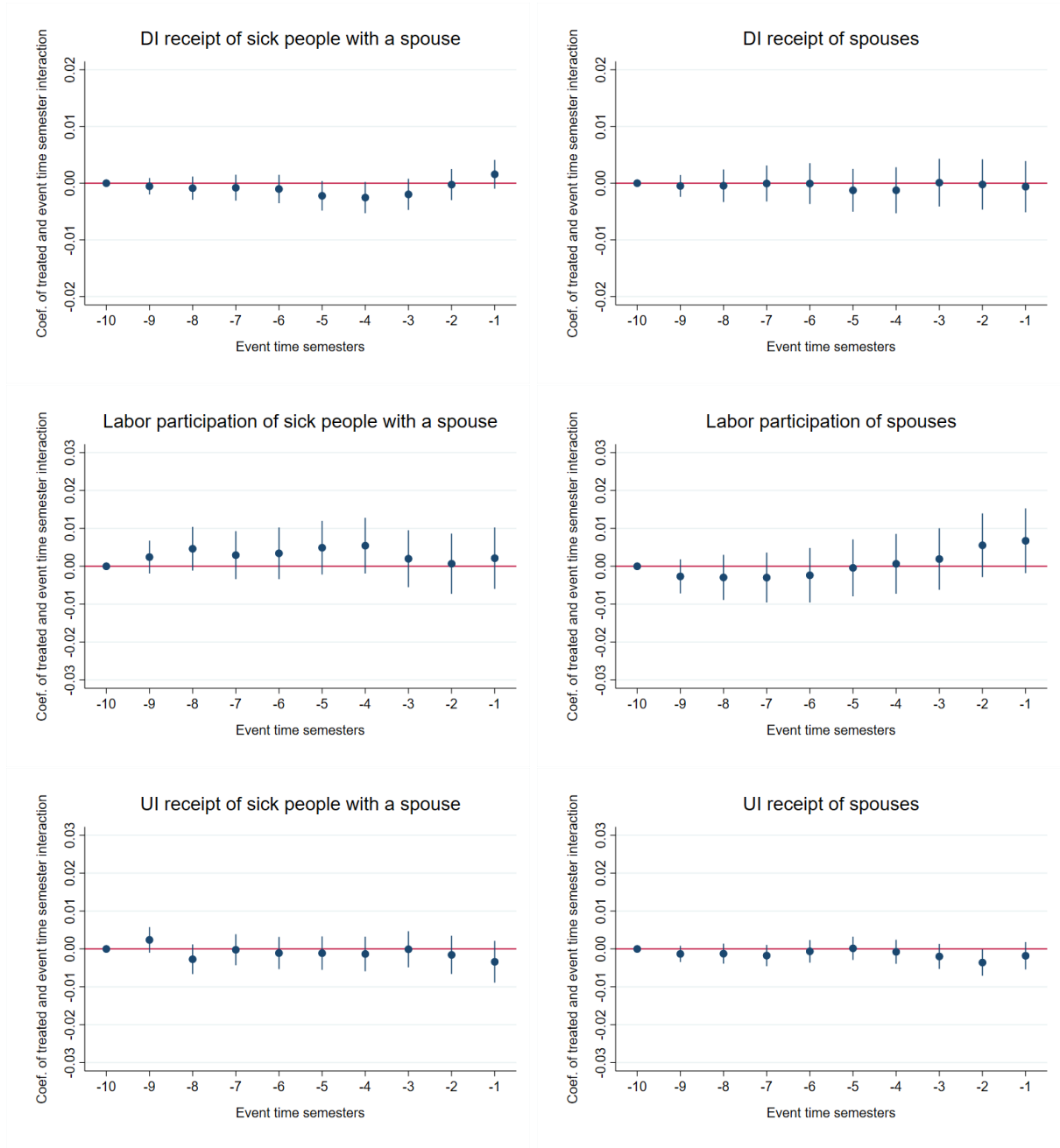


Figure 2a: Estimates of pre-treatment effects of the reform for sick people (left panel) and their spouses (right panel), with 95 percent confidence intervals. Notes: Standard errors are adjusted for heteroskedasticity and clustering at the individual level. The F-statistics (p-value in parentheses) for the assumption of common pre-treatment trends for sick individuals and spouses, respectively, are 2.753 (0.003) and 1.004 (0.434) for DI receipt, 1.134 (0.334) and 1.362 (0.199) for labor participation, and 3.194 (0.001) and 1.802 (0.062) for UI receipt.

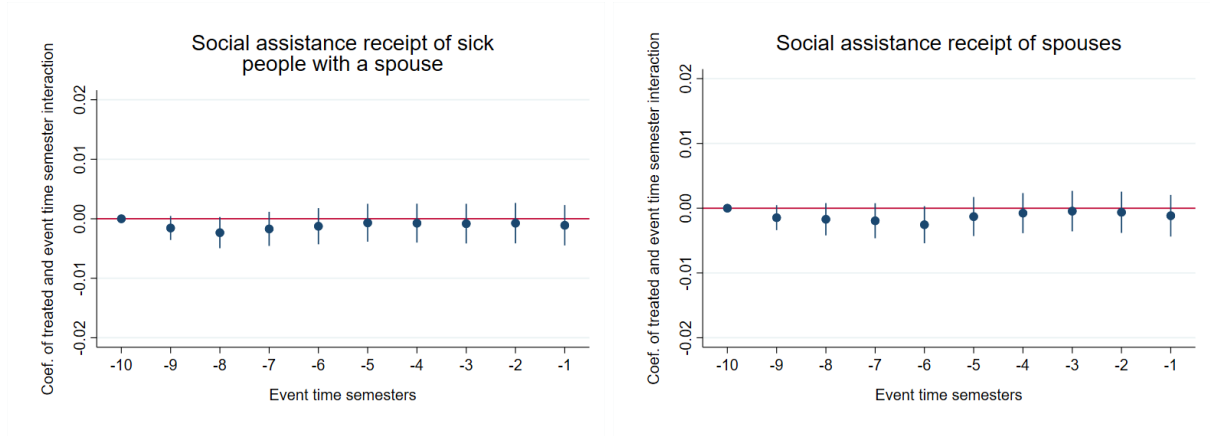


Figure 2b: Estimates of pre-treatment effects of the reform for sick people (left panel) and their spouses (right panel), with 95 percent confidence intervals. Notes: Standard errors are adjusted for heteroskedasticity and clustering at the individual level. The F-statistics (p-value in parentheses) for the assumption of common pre-treatment trends for sick individuals and spouses, respectively, are 0.625 (0.777) and 1.160 (0.316) for social assistance receipt.

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

We first present the effects for the whole post-treatment period (equation (1)) and then analyze the short- and long-run effects of the reform (equation (2)). In addition, we check the heterogeneous effects of the reform.

### Baseline effects

Table 3 presents the baseline DiD estimates of the reform effects on labor participation and benefit receipt. For the sick individuals, the reform decreased the probability of DI receipt by 3.2 percentage points on average during the post-treatment period (excluding the first two years). It increased the probability of working by 1.1 percentage points. It also increased UI receipt by the same amount. The reform induced the spouses of the sick individuals to raise their labor participation by 0.9 percentage points. These effects are significant and sizable and suggest that sick individuals with a spouse not only increase their labor market activity but also rely on income from their spouses.

The center panel of Table 3 shows the DiD estimates of the reform effects for monthly wages and benefits (in logs). The reform decreased monthly DI benefits of sick people by 21.1 percent. It increased monthly earnings by 8.8 percent and monthly income from UI by 7.6 percent. Moreover, it increased earnings of spouses by 6.5 percent.

The lower panel of Table 3 presents the DiD estimates of the reform effects for the monthly total income (in logs), pooling income sources from monthly wages and social security benefits including the DI, UI and social assistance. The table presents the total income of sick individuals and of spouses at the individual level, and of couples at the household level. On average, the reform does not affect the total income of sick individuals or spouses. It also does not affect the total income at the household level. This suggests that the sick individual is able to compensate lost disability benefits by increasing earnings and income from UI and keep his or her total income unchanged. Spouses contribute to the total household income but not to increase it by a statistically significant amount. Cullen and Gruber (2000) investigate wives'

earnings responses to their husbands’ unemployment spells and find a strong “crowd out” of family self-insurance: for each dollar of UI receipt, wives earn up to 73 cents less. Our results, however, show no evidence of a crowd out effect for the spouse.

## Dynamic effects

Figures 3a and 3b present the estimates of the reform effects for ten years of the post-treatment period. The effect of the reform on spousal labor participation is close to 1 percentage point and significant during the entire post-treatment period. We do not reject equality of the effects in all years (see the note below the figure), confirming that the effect of the reform on spousal labor supply is persistent. In line with the exploratory analysis in Section 4, the reform has no effect on spouses’ UI receipt. It has a significant negative effect on social assistance receipt during the sickness period but also during year 3, when the WIA group is first eligible to apply for DI.

For the sick individuals themselves, the reform decreases DI receipt by about 3 percentage points during post-treatment from the third year after reporting sick when both the treatment and control groups become eligible to apply for DI. The effect of the reform on labor participation seems particularly large during the first year of the sickness scheme, but interpreting the effects in the first two years is difficult, due to the measurement issue explained in Section 3. The effect on labor participation then falls to about 1 percentage point and remains fairly stable and statistically significant from the third post-treatment year onwards. For UI receipt, the large negative effect in the second year of the sickness scheme is due to the fact that individuals insured under the WIA are still entitled to sickness wage payment if there is an employer, or the sickness benefit if there is no employer. From the third post-treatment year onwards, however, the reform has a positive effect on UI receipt. It decreases over time but remains significant at the 1 percent level. In line with Section 4, the effect of the reform on social assistance receipt is negative and significant only during the first three years after reporting sick.

The effects for wages, DI, UI and social assistance benefits are in line with those shown in Figures 3a and 3b. For example, the reform has a persistent positive effect of about 6 percent on earnings of the spouses of individuals falling sick.

## Heterogeneous effects

We analyzed the heterogeneous effects of the DI reform with respect to gender and work status when reporting sick. The literature on the added worker effect that analyzes income complementarities in households as an insurance mechanism typically focuses on wife’s response to shocks in the husband’s income. We find hardly any differences between genders (details available upon request), in contrast to the findings of [Garcia Mandico et al. \(2021\)](#) for the Dutch reform implemented in 2004.

Incentivizing the employers to increase labor market participation has been a key element of Dutch labor market reforms throughout the years. Therefore it is important to distinguish between employees with and without employers. We analyze the reform effects for sick individuals who were wage earners with a permanent or temporary contract, and for those who were unemployed at the time they reported sick. For employees with a temporary contract, employer incentives last only as long as the contract lasts, and for employees of temporary work agencies there are no employer incentives during sickness. Unemployed individuals face no employer incentives. On the other hand, employers of employees with a permanent contract are fully incentivized due to experience rating (Section 2).

Table 4 presents the results by work status of the sick individuals at the time they report sick. Due to the reform, DI receipt fell substantially for all groups, and the largest fall is for

the unemployed. For the unemployed, there is no employer who could reintegrate them back into jobs. This increases their chances of remaining in the sickness scheme for a longer time and facing the stricter requirements of WIA to enter DI.

For the sick individuals, the reform increases labor participation among those with a permanent work contract but reduces it among the unemployed. It seems that the reform's work resumption incentives induce employers to reintegrate their permanent employees, but they prove ineffective if there is no employer. For the unemployed, the longer sickness period might 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 reduced by the additional year they can spend in the sickness scheme.

The spouses of sick individuals with a permanent work contract hardly respond to the reform, but spouses of sick individuals who work on a temporary contract or are unemployed increase their labor participation and earnings significantly. Since sick individuals without permanent contract struggle to resume working, these results suggest that their spouses increase labor participation and earnings to compensate for the lost disability benefits and lack of labor income. On the other hand, sick individuals with a permanent work contract more often resume work or increase earnings, implying a lesser need for their spouses to compensate.

The reform increases UI receipt for all sick individuals, irrespective of their work status. The increase is largest for the unemployed, since UI is usually their primary source of income. The effect for those on a temporary contract on receiving UI is larger than for those on a permanent contract – they have a less stable source of labor income, earn less, and seek additional income from UI if access to DI benefits is limited by the reform. The spouses of unemployed individuals reporting sick reduce their UI receipt by 0.4 percentage points, while spouses of sick individuals on a permanent contract increase their UI receipt by 0.2 percentage points in response to the reform. These responses are in line with the spouses' labor participation responses. When they increase their labor participation, they do not apply for UI.

The signs and the statistical significance of the estimated effects of the reform on earnings and amounts of benefits from DI, UI and social assistance in the center panel of Table 4 are in line with the estimated effects on labor participation and benefit receipt.

The lower panel of the table presents the effects of the reform on total income (in logs) of sick individuals and their spouses at the individual and the household level. Individual income of sick individuals who had a temporary contract or were unemployed fell due to the reform. However, due to the positive responses of their spouses, household income does not change significantly. For sick individuals with a permanent contract, total income increases both at the individual and household level. These results confirm that spouses' responses smooth household consumption over a period of 10 years after reporting sick when their sick partners are not able to respond to the negative income shock due to the reform. This result is in line with Blundell et al. (2016) who use a structural family labor supply model to predict consumption smoothing over the life-cycle due to self-insure through spousal labor supply when household income is affected by an income shock.

Table 3: Estimated effects of the WIA reform: Sick individuals and their spouses

	Sick individual	Spouse
Disability insurance receipt	−0.032*** (0.002)	−0.002 (0.002)
Labor participation	0.011*** (0.003)	0.009*** (0.003)
Unemployment insurance receipt	0.011*** (0.001)	0.001 (0.001)
Social assistance receipt	−0.000 (0.001)	−0.001 (0.001)
ln Disability insurance	−0.211*** (0.018)	−0.016 (0.012)
ln Wage	0.088*** (0.003)	0.065*** (0.023)
ln Unemployment insurance	0.076*** (0.009)	0.004 (0.006)
ln Social assistance	−0.001 (0.007)	−0.009 (0.007)
ln Total individual income	0.006 (0.023)	0.036 (0.022)
ln Total household income	0.026 (0.018)	
Observations	8,431,218	
Individuals	55,106	

Notes: \*\*\*, \*\*, \* denote statistical significance at 1, 5 and 10 percent, respectively. Standard errors (in parentheses) account for heteroskedasticity and clustering at the individual level. Linear probability models. In all specifications we control for individual and calendar month fixed effects. The regressions use data available for the whole pre-treatment period but exclude data for the first two years of the post-treatment period.

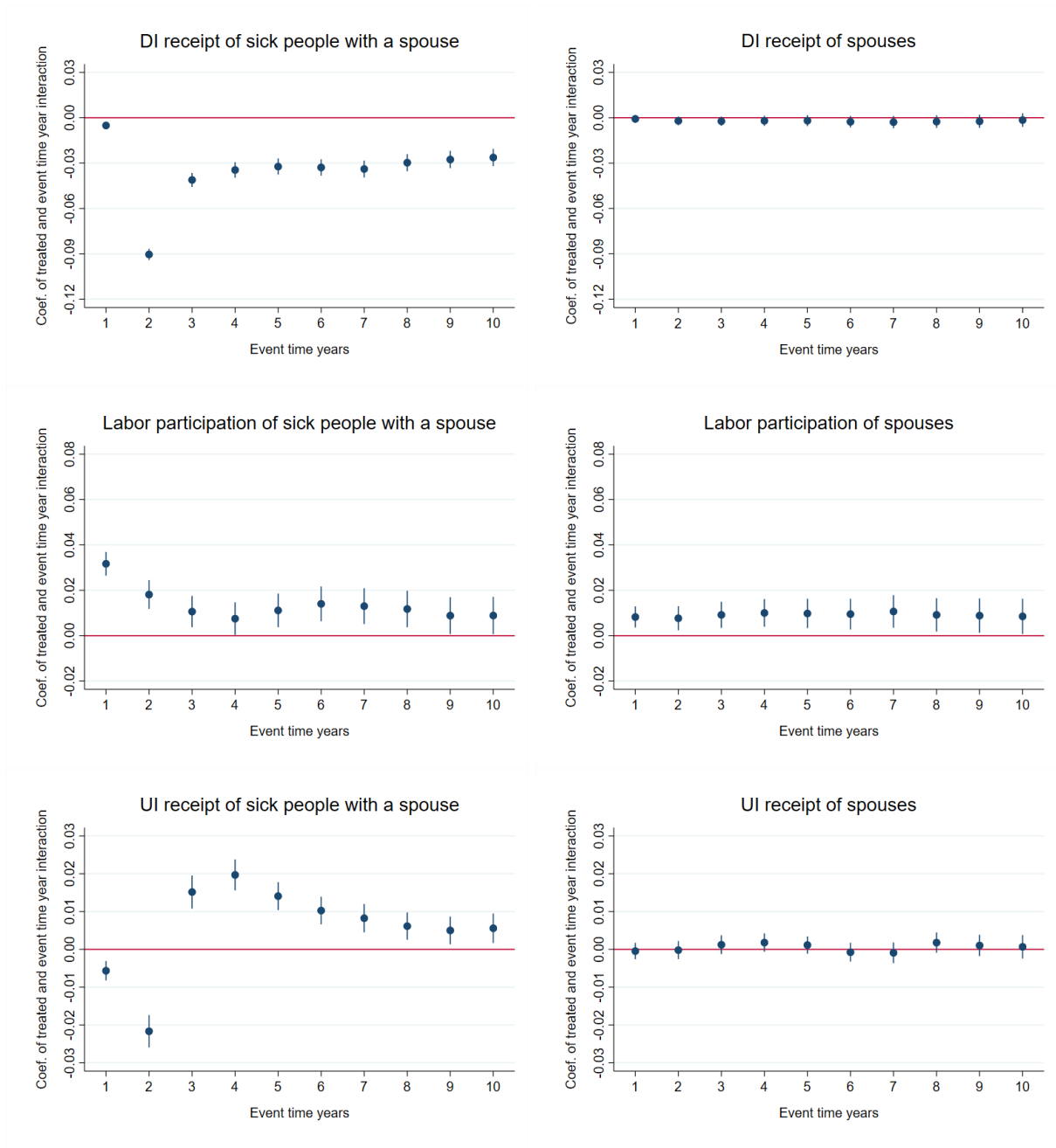


Figure 3a: Estimated treatment effects in each of the first ten years after falling sick, with 95 percent confidence intervals. Note: The F-statistics (p-value in parentheses) for the test on equality of treatment effects over the years for sick individuals and their spouses, respectively, are 331.8 (0.000) and 0.542 (0.844) for DI receipt, 5.961 (0.000) and 0.252 (0.986) for labor participation, and 44.04 (0.000) and 1.102 (0.357) for UI receipt.



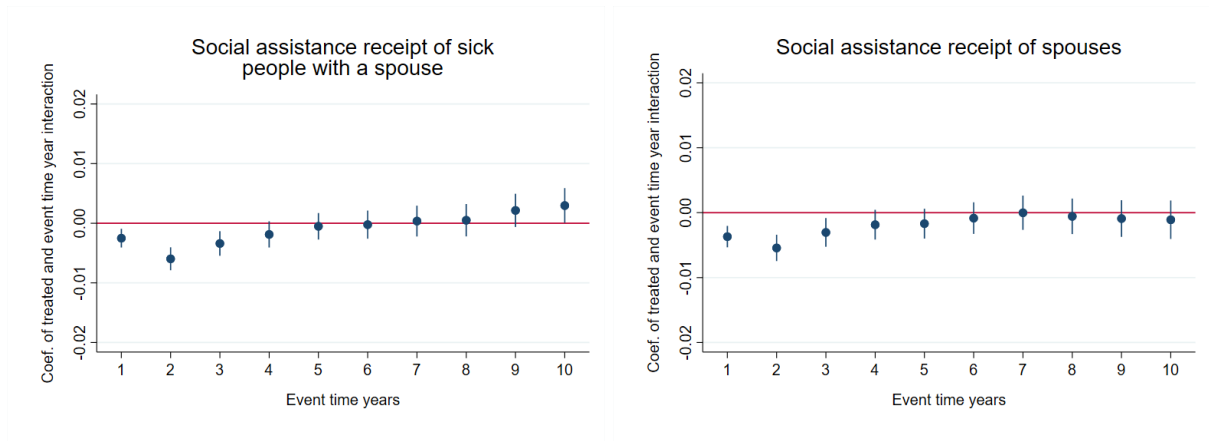


Figure 3b: Estimated treatment effects in each of the first ten years after falling sick, with 95 percent confidence intervals. Note: The F-statistics (p-value in parentheses) for the test on equality of treatment effects over the years for sick individuals and their spouses, respectively, are 5.829 (0.000) and 2.419 (0.01) for social assistance receipt.

Table 4: Estimated effects of the WIA reform by labor market status when reporting sick

		Sick individual on permanent contract	Sick individual on temporary contract	Sick individual unemployed
Disability ins. receipt	Sick individual	−0.028*** (0.002)	−0.030*** (0.009)	−0.044*** (0.008)
	Spouse	−0.001 (0.002)	−0.008 (0.006)	−0.002 (0.005)
Labor participation	Sick individual	0.022*** (0.004)	−0.011 (0.011)	−0.020*** (0.009)
	Spouse	0.003 (0.004)	0.029*** (0.009)	0.022*** (0.008)
Unemp. ins. receipt	Sick individual	0.003*** (0.001)	0.014*** (0.004)	0.043*** (0.005)
	Spouse	0.002** (0.001)	−0.001 (0.003)	−0.004* (0.002)
Social assistance receipt	Sick individual	−0.001 (0.001)	0.000 (0.005)	0.004 (0.004)
	Spouse	−0.001 (0.001)	−0.008* (0.005)	0.003 (0.003)
ln Disability insurance	Sick individual	−0.186*** (0.018)	−0.189*** (0.063)	−0.286*** (0.055)
	Spouse	−0.008 (0.014)	−0.060 (0.038)	−0.016 (0.034)
ln Wage	Sick individual	0.180*** (0.031)	−0.100 (0.084)	−0.174*** (0.067)
	Spouse	0.018 (0.027)	0.210*** (0.070)	0.168*** (0.058)
ln Unemp. ins.	Sick individual	0.022** (0.008)	0.102*** (0.028)	0.299*** (0.034)
	Spouse	0.016** (0.007)	−0.009 (0.019)	−0.031* (0.017)
ln Social assistance	Sick individual	−0.005 (0.005)	−0.000 (0.033)	0.024 (0.025)
	Spouse	−0.006 (0.007)	−0.058* (0.032)	0.017 (0.022)
ln Total individual income	Sick individual	0.075*** (0.027)	−0.153** (0.072)	−0.120** (0.060)
	Spouse	0.011 (0.026)	0.092 (0.068)	0.117** (0.056)
ln Total household income	Couple	0.050** (0.021)	−0.011 (0.051)	−0.010 (0.047)
Observations		5,996,682	966,042	1,468,494
Individuals		39,194	6,314	9,598

Notes: \*\*\*, \*\*, \* denote statistical significance at 1, 5 and 10 percent, respectively. Standard errors (in parentheses) account for heteroskedasticity and clustering at the individual level. Linear probability models. In all specifications we control for individual and calendar month fixed effects. The regressions use data available for the whole pre-treatment period but exclude data for the first two years of the post-treatment period.

## 7 Comparing with the reform effects on sick individuals without a spouse

The results in the preceding section suggest that spouses increased their labor participation to compensate for lost disability benefits of their sick partners, particularly if the sick individuals cannot increase their own earnings. Here we analyze how the reform affected labor participation of sick individuals who do not have a spouse and cannot compensate the loss of household income through spousal labor supply. This could induce them to increase their labor participation more than sick individuals with a spouse do. They could, however, also more often rely on social insurance.

In Figures 4a and 4b we compare the labor participation and benefit receipt of sick people with and without a spouse of control and treatment groups. Time trends of control and treatment groups of sick people without a spouse overlap pre-treatment but differ post-treatment. The difference in labor participation and social assistance receipt between the pre- and post-reform groups is much larger for sick people without than for those with a spouse, suggesting that the reform effects are stronger for sick individuals without a spouse. Figures for wages and benefit amounts lead to the same conclusions (not shown).

Table 6 presents the DiD estimates of the reform effects for sick people without a spouse, and reproduces the baseline estimates for sick people with a spouse from Table 3.<sup>9</sup> The DiD estimates confirm that, on average during the post-treatment period (excluding the first two years), the reform increases the probability of working by 0.4 percentage points more among sick people without a spouse than for sick individuals in couples. Together with the finding (in Table 3) that spouses increase their labor participation in response to the reform, this suggests that in couples, the response to the disability reform is shared by both partners: Spousal labor supply is a substitute for sick individuals' own labor supply when facing a stricter disability benefit regime. Similarly, the reform reduces social assistance receipt more for sick people without a spouse than for sick people with a spouse. This is due to the fact that increased total income from earnings and UI for sick individuals without a spouse reduces eligibility for the means tested social assistance benefit. The reform effects for wages earned and benefits received per month (in logarithm) for sick people with and without a spouse are in line with the reform effects on labor participation and benefit receipt. For example, while sick people without a spouse increase their earnings by 10.7 percent in response to the reform, sick people with a spouse increase their earnings by the lower amount of 8.8 percent.

As in Section 6, we also consider the possibility that the reform effect depends on the time since the individual fell sick, see Figures 5a and 5b. The time pattern of the effect on labor participation is similar to that for sick people with a spouse shown in Figures 3a and 3b (reproduced in the left panel of Figures 5a and 5b): It is large in the first year and then falls, but it remains significant at about 1.5 percentage points during the next nine years, showing that the effect of the reform on labor participation of sick people without a spouse is persistent in the long run.<sup>10</sup> The time patterns of the effects on DI and UI receipt are also similar to those for sick people with a spouse; they are persistent and remain significant throughout the entire post-treatment period. The pattern of the reform effect on social assistance receipt differs markedly between those with and without a spouse, with much larger negative effects over a period of seven years post-treatment for individuals without a spouse. Similar time patterns are found for the effects on wage and benefit amounts (not shown).

<sup>9</sup>We tested the common trend assumption for sick individuals without spouse in the same way as in Section 5. It is not rejected except for DI and UI in the last two semesters of pre-treatment; see Appendix C for details.

<sup>10</sup>As in Section 6, interpreting the effects in the first two years is difficult because of the measurement issue explained in Section 3.

As before, we find no notable differences between men and women (results not presented). Table 7 presents the results by work status before reporting sick. The table shows similar effects for sick people without and with a spouse who held a permanent contract. There is a substantial difference, however, if they held a temporary contract, with singles increasing their labor participation by a substantial amount of 2.9 percentage points. Table 4 showed that spouses of sick individuals with a temporary contract increased their labor participation by 2.9 percentage points due to the reform. Here we see that if there is no spouse who can compensate, sick people's own participation responds by the same amount. We observe a similar response when sick people are unemployed, with singles showing no response while spouses increase their labor participation, offsetting the decrease in labor participation by their sick partners. When sick people are on a permanent contract, those in a partnership and those who are single both increase their labor participation by similar amounts, and there is no response of the spouse. Overall, we find that the response of singles reflects the sum of the effects for the two partners for all three initial labor market states, suggesting that partners in a couple jointly decide how to respond, given the constraints faced by the sick individual.

Table 7 shows a substantial difference in UI receipt for sick people without and with a spouse if they were unemployed at the time of reporting sick. Those with a spouse increase UI receipt by 4.3 percentage points, while those who are single increase it by 2.3 percentage points. These responses in UI receipt seem to be related to the responses in labor participation, with those in a partnership increasing UI receipt more because their labor participation falls more than among singles. This suggests a spillover effect of the DI reform on participation in the alternative UI program due to the absence of a partner: If there is no spouse who can compensate, sick people's own labor participation compensates for reduced participation in DI, which in turn reduces participation in UI.

Similar results are obtained for monthly earnings and benefits in the center panel of Table 7. Pooling monthly earnings and benefits, the lower panel of the table shows the reform effects for the monthly total income of sick people without and with a spouse. The reform effects for singles confirm that spouses compensate to keep the household income unchanged when their sick partners lack job security. When with a permanent contract, singles increase earnings, as do sick people in a partnership, so that their total income does not decrease due to the reform. When with a temporary contract, however, total income of singles does not decrease due to the reform unlike the total income of sick people in a partnership. Singles increase their earnings at least as much as the spouses increase so that household income of singles is not affected as the household income of couples in Table 4. When singles are unemployed, their total income decreases. Unlike sick people in a partnership, this is not because their earnings decrease but because they lose DI benefits by a much larger amount and the smaller amounts of UI benefits they claim are not enough to compensate.

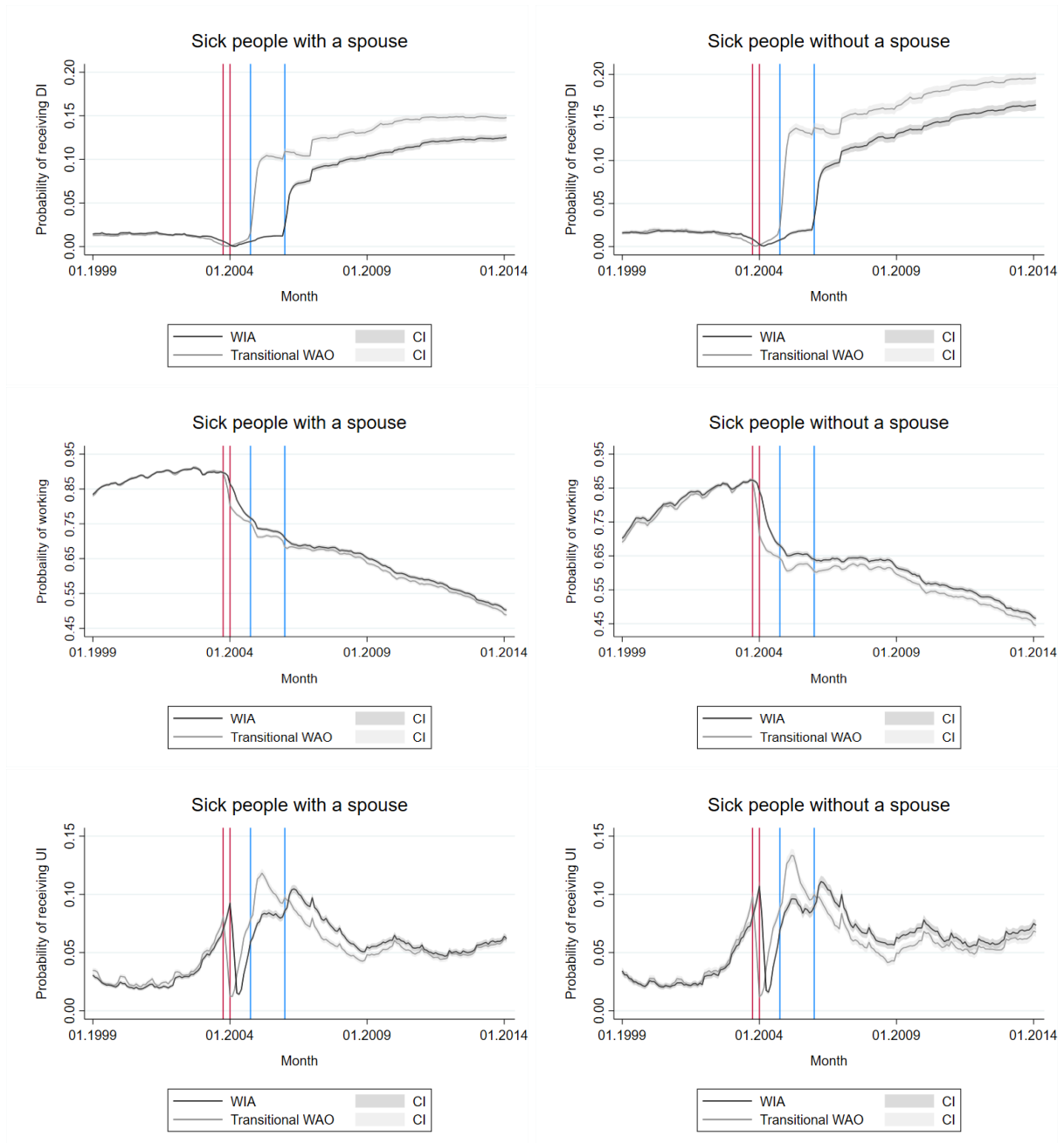


Figure 4a: Probability of DI receipt, working, and UI receipt for control and treatment groups over calendar months: for sick individuals (left panel reproducing the left panel of Figure 1a) and (right panel) spouses. Vertical lines mark the first instance sick partners could become entitled to the sickness 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.

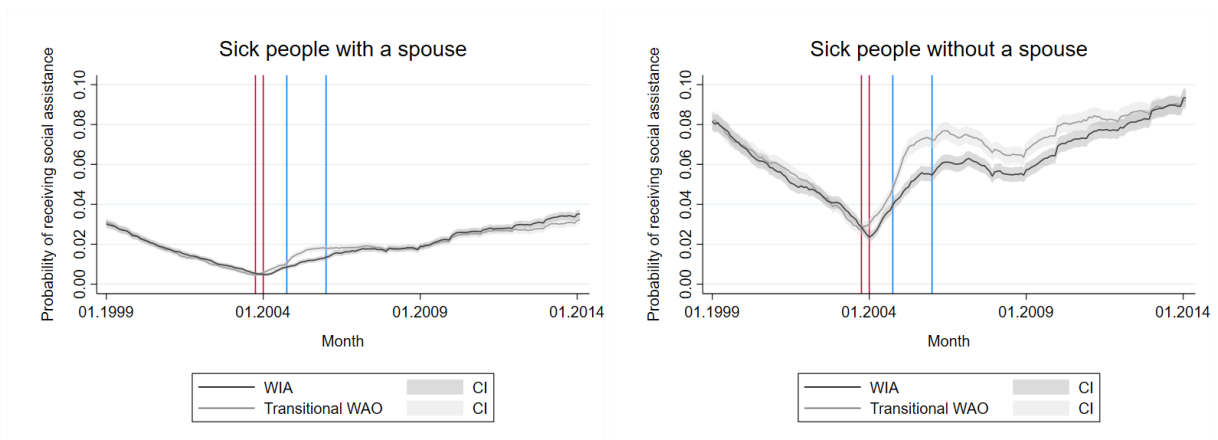


Figure 4b: Probability of social assistance receipt for control and treatment groups over calendar months: for sick individuals (left panel reproducing the left panel of Figure 1b) and (right panel) spouses. Vertical lines mark the first instance sick partners could become entitled to the sickness and 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.



Table 6: Estimated effects of the WIA reform: Individuals with and without spouse

	Sick people with a spouse	Sick people without a spouse
Diasbility insurance receipt	−0.032*** (0.002)	−0.037*** (0.0037)
Labor participation	0.011*** (0.003)	0.015*** (0.005)
Unemployment insurance receipt	0.011*** (0.001)	0.012*** (0.002)
Social assistance receipt	−0.000 (0.001)	−0.008*** (0.002)
ln Diasbility insurance	−0.211*** (0.018)	−0.247*** (0.026)
ln Wage	0.088*** (0.027)	0.107*** (0.038)
ln Unemployment insurance	0.076*** (0.010)	0.090*** (0.012)
ln Social assistance	−0.001 (0.007)	−0.052*** (0.017)
ln Total individual income	0.006 (0.023)	−0.044 (0.031)
Observations	8,431,218	4,460,868
Individuals	55,106	29,156

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. The regressions use data available for the whole pre-treatment period and exclude data for the first two years of the post-treatment period.

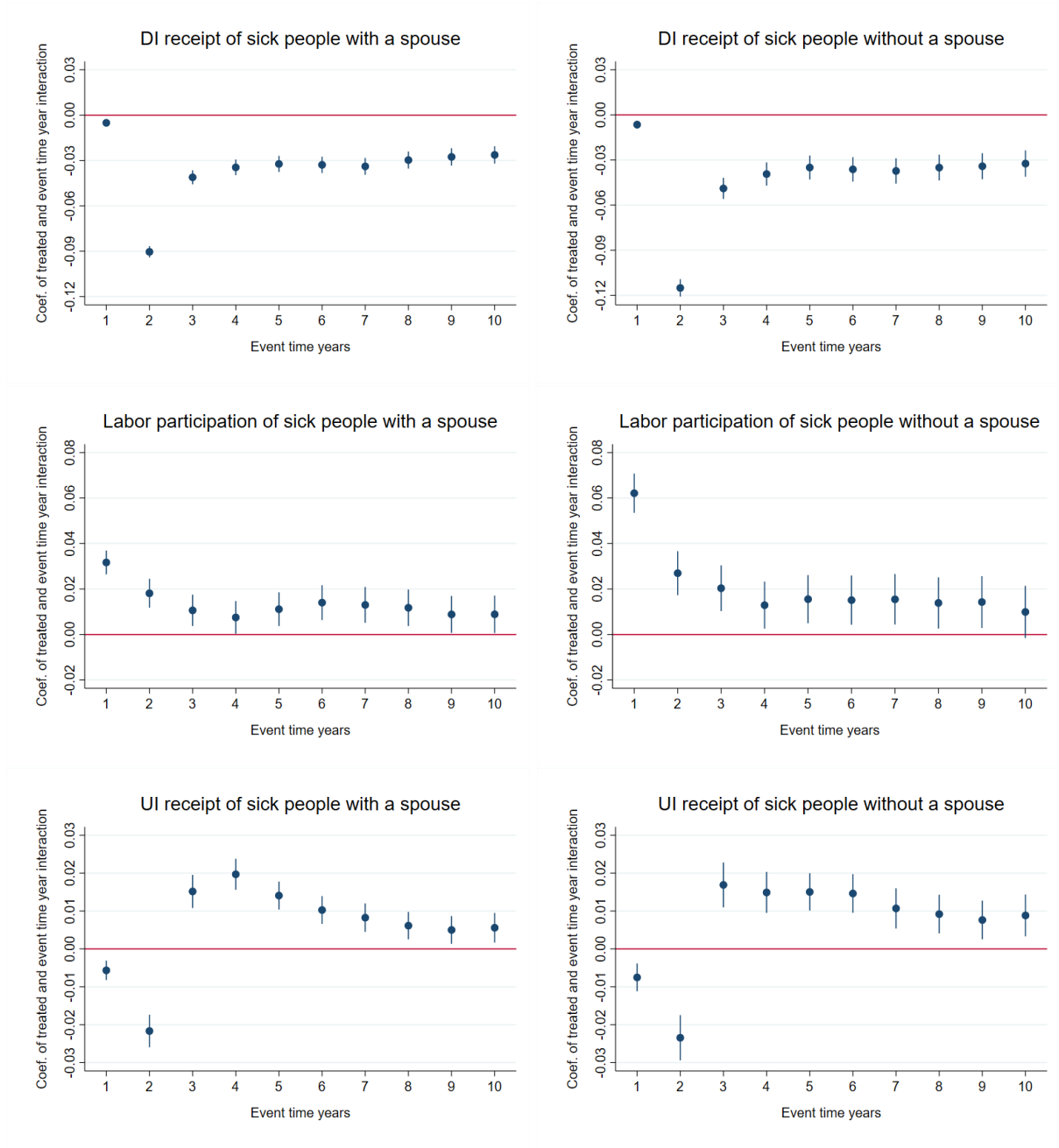


Figure 5a: Estimated treatment effects in each of the first ten years after falling sick, with 95 percent confidence intervals. Note: The F-statistics (p-value in parentheses) for the equality of the coefficients of treatment and event year interactions, respectively for sick individuals with a spouse and without a spouse are 334.0 (0.000) and 283.3 (0.000) for DI receipt, 5.961 (0.000) and 13.01 (0.000) for labor participation, and 44.04 (0.000) and 26.17 (0.000) for UI receipt.

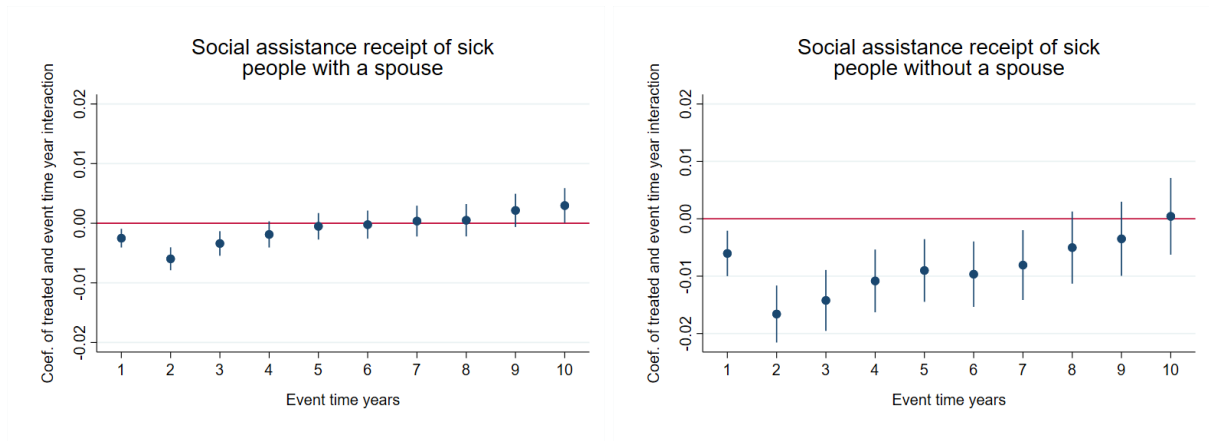


Figure 5b: Estimated treatment effects in each of the first ten years after falling sick, with 95 percent confidence intervals. Note: The F-statistics (p-value in parentheses) for the equality of the coefficients of treatment and event year interactions, respectively for sick individuals with a spouse and without a spouse are 5.829 (0.000) and 5.032 (0.000) for social assistance receipt.

Table 7: Estimated effects of the WIA reform by labor market status when reporting sick: Sick individuals with and without a spouse

		Permanent contract	Temporary contract	Unemployed
Disability ins. receipt	Sick people with a spouse	−0.028*** (0.002)	−0.030*** (0.009)	−0.044*** (0.008)
	Sick people without a spouse	−0.022*** (0.004)	−0.034*** (0.009)	−0.057*** (0.010)
Labor participation	Sick people with a spouse	0.022*** (0.004)	−0.011 (0.011)	−0.020*** (0.009)
	Sick people without a spouse	0.016*** (0.006)	0.029*** (0.011)	−0.006 (0.011)
Unemp. ins. receipt	Sick people with a spouse	0.003*** (0.001)	0.014*** (0.004)	0.043*** (0.005)
	Sick people without a spouse	0.006*** (0.002)	0.015*** (0.004)	0.023*** (0.005)
Social assistance receipt	Sick people with a spouse	−0.000 (0.001)	0.000 (0.005)	0.004 (0.004)
	Sick people without a spouse	−0.004 (0.002)	−0.012* (0.007)	−0.005 (0.007)
ln Disability insurance	Sick people with a spouse	−0.186*** (0.018)	−0.189*** (0.063)	−0.286*** (0.055)
	Sick people without a spouse	−0.140*** (0.029)	−0.241*** (0.064)	−0.389*** (0.069)
ln Wage	Sick people with a spouse	0.180*** (0.031)	−0.100 (0.084)	−0.174*** (0.067)
	Sick people without a spouse	0.117** (0.050)	0.242*** (0.087)	−0.074 (0.081)
ln Unemp. ins.	Sick people with a spouse	0.022 (0.008)	0.102*** (0.028)	0.299*** (0.034)
	Sick people without a spouse	0.042*** (0.013)	0.104*** (0.025)	0.166*** (0.036)
ln Social assistance	Sick people with a spouse	−0.005 (0.005)	−0.000 (0.033)	0.024 (0.024)
	Sick people without a spouse	−0.028 (0.017)	−0.081* (0.046)	−0.031 (0.048)
ln Total individual income	Sick people with a spouse	0.075*** (0.027)	−0.153*** (0.072)	−0.120** (0.060)
	Sick people without a spouse	0.064 (0.040)	0.026 (0.072)	−0.250*** (0.067)
Observations		5,996,682	966,042	1,468,494
Individuals		39,194	6,314	9,598

Notes: \*\*\*, \*\*, \* denote statistical significance at 1, 5, and 10 percent, respectively. Standard errors (in parentheses) account for heteroskedasticity and clustering at the individual level. All specifications have individual and calendar month fixed effects. The regressions use data available for the whole pre-treatment period and exclude data for the first two years of the post-treatment period. Presented number of observations and individuals are for sick people with a spouse. 2,552,652 observations for 16,684 sick people without a spouse are used in the regression for those on a permanent contract. 899,028 observations for 5,876 sick people with a spouse are used in the regression for those on a temporary contract. 1,009,188 observations for 6,596 sick people with a spouse are used in the regression for the unemployed.

## 8 Conclusion

We analyze the labor supply and earnings responses of spouses when income of their sick partners from social insurance change. The change is due to a major reform that introduced stricter eligibility criteria for disability insurance. 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 disability insurance. Based on a difference-in-differences identification strategy and using unique administrative data, we find clear evidence of an added worker effect, in contrast to many earlier empirical studies.

Due to the reform, the spouses of people who fell sick under the stricter benefit regime work, on average, 0.9 percentage points more often than the spouses of people who fell sick under the old regime. The effect of the reform on the sick people themselves is 1.1 percentage points. The sizes of the effects seem economically meaningful, considering that DI receipt decreased by 3.2 percentage points for the sick people due to the reform. The effect of the reform is persistent during the ten years following the start of sickness, not only for the sick individuals, but also for their spouses. This finding is remarkable given that an earlier major DI reform implemented in 1993 had no significant effect on spousal labor supply (Borghans et al., 2014). It implies that for a complete evaluation of the DI reform, it is important to consider spill-over effects on spouses, both for the effect on labor participation and for the effect on adequacy of household income.

The effect of the reform on spousal labor supply varies with the employment contract the sick individual had when falling sick. People who had a permanent contract at the time they fell sick increased labor market participation by 2.2 percentage points due to the reform, while their spouses did not respond. On the other hand, people who had a temporary contract at the time they fell sick did not increase labor participation because of the reform, while their spouses increased labor participation by 2.9 percentage points. Overall, this shows that the response at the couple level is more than 2 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. These findings are consistent with the hypothesis that partners substitute for each other's labor force participation.

As discussed in Section 1, various explanations for the lack of evidence of an added worker effect are given in the literature. In light of this discussion, there may be several reasons why the 2006 Dutch DI reform did lead to an added worker effect. First, the reform led to a permanent reduction of the income of the affected spouse. In line with this, we find persistent responses of both the sick individuals and their spouses in the ten years following sickness (Figures 3a and 3b). Second, the reform could not be anticipated so that couples could not adjust their consumption and labor supply before the reform took place. Pre-sickness trends of labor participation and earnings of the treatment and control groups support this: When comparing to the control group, the treatment group does not show any sign of adjusting labor participation or earnings at any time before reporting sick (Figure 2a). Third, as spouses have faced the sickness of their partner and the reform, they may have expected the lifetime income loss due to the reform to be large and responded by insuring against it. Labor market insecurity of the sick individuals in terms of working on a temporary work contract or being unemployed at the time of sick reporting, where spousal responses are observed exclusively, may have contributed to each of these three reasons.

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## Appendix A 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 the results confirm the results based on the DiD method.

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 – 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 WIA. We rely on a sharp regression discontinuity (RD) design to estimate the effect of the reform. In particular, the discontinuous jump at the cutoff identifies the treatment effect of interest which can be formalized as

$$\lim_{x \downarrow c} \mathbb{E}[Y_i | X_i = x] - \lim_{x \uparrow c} \mathbb{E}[Y_i | X_i = x] \quad (3)$$

where  $X_i$  is the date at which people fall sick and  $c$  is the cut-off point of 1 January 2004. The treatment effect is estimated using a triangular kernel and a MSE-optimal bandwidth selector (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. We consider the same time horizon as with the DiD estimates – the period after treatment but excluding the first two years. We pool all monthly observations of the post-treatment period excluding the first 24 months, implying that we have 96 observations for each individual. We do not account for individual fixed effects but this should not result in biased estimates since the distance from the cut-off date is assumed to be random for individuals who report sick close to 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. In other words, “all other factors” driving the outcome variables must be continuous at the cut-off point (see, e.g., [Hahn et al., 2001](#)). Although this assumption cannot be tested directly, relevant variables can be checked for whether they change significantly at the cutoff. We consider contract type at the time of reporting sick as a most relevant variable. We consider dummies for having a permanent contract, temporary contract and being unemployed as outcome variables, and check if they exhibit discontinuity at the cut-off. For sick people with a spouse, we find no significant change at the cut-off in any of the three outcomes. For sick people without a spouse, however, the RD estimate of the treatment effect on being unemployed is -0.088 with a standard error of 0.028. Therefore, we treat the RD estimates of the reform effects as suggestive rather than conclusive, at least for sick people without a spouse.

Figure 6 provides graphical evidence for labor participation and benefit receipt. 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 outcome with symmetric bandwidth thirty days around the cut-off date. The figure shows clear discontinuities at the cut-off point, in the expected direction. Furthermore, the relative size of the jumps are in line with the DiD estimates presented in Tables 3 and 6. For example, for labor participation, sick people without a spouse show the largest effect, followed by sick people with a spouse and by the spouses of sick individuals.

Table 8 presents estimated average treatment effects at the cut-off. Both the RD and DiD estimators provide evidence of a positive and significant effect of the reform on the employment probability of spouses and sick individuals with or without spouse. The RD estimates, however, are larger and less precise than the DiD estimates. A possible explanation is the fact that RD only identifies the average treatment effect at the cut-off point, that is, for a specific group of people who report sick around 1 January. These people might differ from those who report sick in other months of the year. 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.

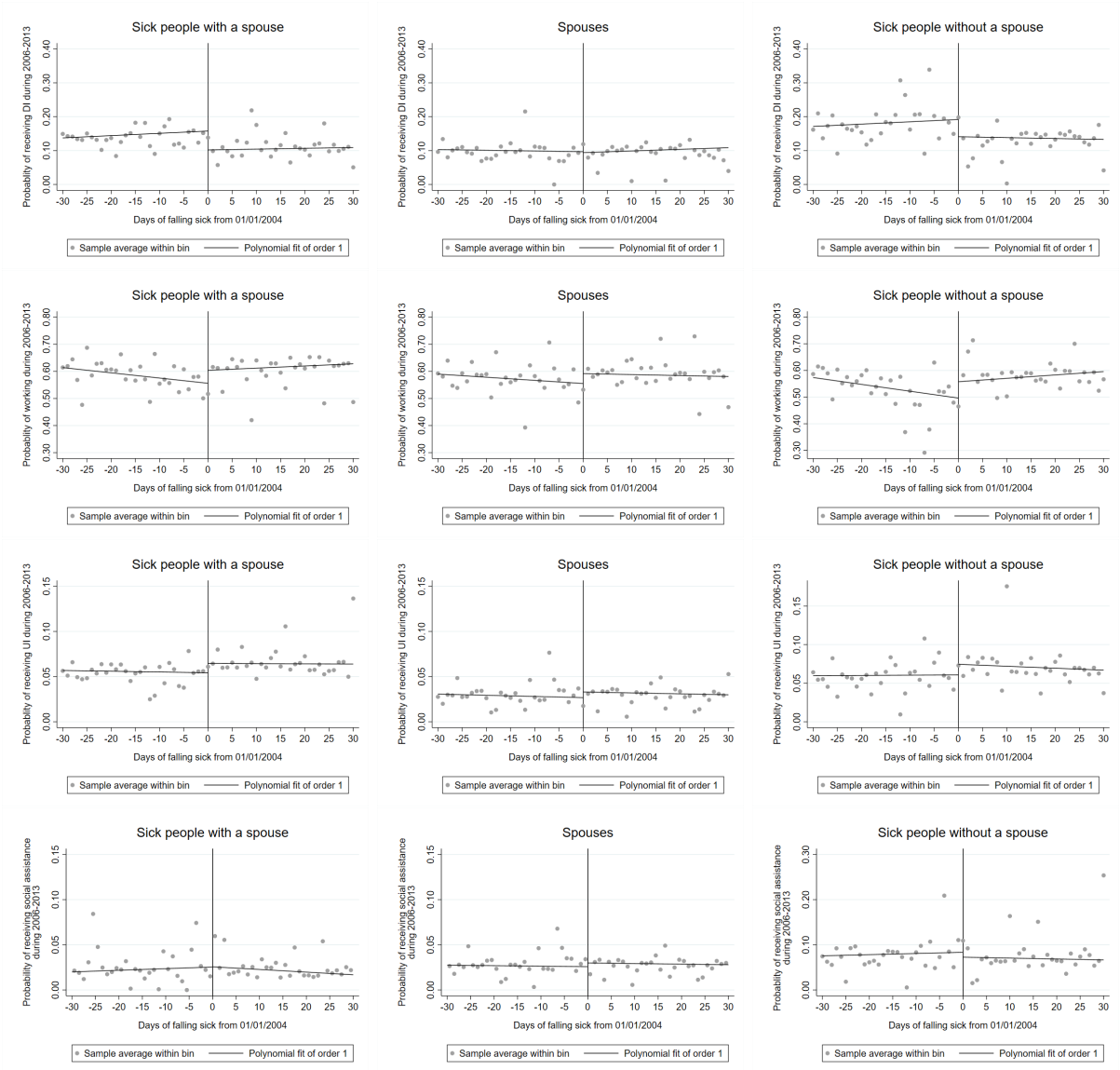


Figure 6: Local linear fit on the two sides of the cut-off. Standard errors are clustered at the individual level. All subfigures exclude data for the first two years after reporting sick.

Table 8: Sharp RD estimate of the effect of the reform on the labor participation of sick individuals and their spouses and of sick individuals without spouse

	Sick people with a spouse	Spouse	Sick people without a spouse
Disability ins. receipt	−0.054*** (0.012)	−0.001 (0.012)	−0.051*** (0.017)
Labor participation	0.048*** (0.017)	0.038** (0.019)	0.059*** (0.022)
Unemployment insurance receipt	0.010** (0.004)	0.004 (0.003)	0.014*** (0.006)
Social assistance receipt	−0.000 (0.005)	0.005 (0.005)	−0.010 (0.012)
ln Disability insurance	−0.382*** (0.089)	−0.013 (0.085)	−0.374*** (0.117)
ln Wage	0.375*** (0.131)	0.316** (0.156)	0.434** (0.176)
ln Unemployment ins.	0.076** (0.030)	0.025 (0.021)	0.103** (0.043)
ln Social assistance	−0.005 (0.035)	0.031 (0.034)	−0.063 (0.082)
ln Total individual income	0.123 (0.096)	0.327*** (0.139)	0.170 (0.135)
ln Total household income	0.110 (0.089)		

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. The regressions are based on post-treatment data excluding the first two years. Effective number of observations and individuals used in the estimations depend on the bandwidth. For example, 1,354,272, 1,274,784 and 835,392 observations for 14,107, 13,279 and 8,702 individuals are used when the bandwidths (days) are 25.9, 24.3 and 29.0 in the regressions of labor participation of sick people with a spouse, spouses and sick people without a spouse, respectively.

## Appendix B Do couples dissolve their cohabitation due to the reform?

We studied the labor supply responses of couples to the DI reform who started cohabiting before reporting sick. Couples can dissolve their cohabitation during post-treatment due to the reform or other reasons. This may confound the estimated reform effects. Here we first check to which extent the reform affected cohabitation status during post-treatment, and then analyze how the baseline estimates of the reform effects respond when we restrict the study sample to couples who stay together during post-treatment.

Figure 8 presents the probability that couples end their cohabitation during post-treatment. The probability is small and shows a decreasing time trend that is common to control and treatment groups. The confidence intervals for the two groups overlap which could suggest that the reform has no statistically significant effect on cohabitation status. These figures are in line with Table 1b which showed that couples in both the treatment and control groups cohabit for about 8 years on average during the 10-year period of post-treatment. To test whether the reform affected cohabitation status, we rely on a sharp RD design as in Appendix A. In particular, we exploit the date at which the reform came into effect as a source of exogenous variation in treatment status, and analyze whether sick-listed workers insured under the transitional WAO and WIA differ in their cohabitation status during the ten years after reporting sick. Figure 9 provides graphical evidence. It shows local linear fits for the probability that cohabitation ends with symmetric bandwidth thirty days around the cut-off date. The figure shows no discontinuity at the cut-off. The RD estimate (standard error in parenthesis) of the reform effect is 0.000 (0.000) and is statistically insignificant at the 10 percent level.<sup>11</sup> This shows that the reform did not cause couples to dissolve their cohabitation suggesting that the estimated treatment effect of the reform is not affected by a notable amount by cohabitation status changes post-treatment.

To analyze how much in fact the changes in cohabitation status affect the estimated reform effects, we check how much the baseline estimate of the reform effect changes if the study sample is restricted to couples who remain cohabiting during the entire post-treatment period. Table 9 shows the estimation results. The baseline estimates that were found significant in Table 3 become slightly larger. The effects for the total income of spouses and that for couples become significant confirming our main finding that spouses share the burden of a more stringent disability scheme.

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<sup>11</sup>The RD estimation uses the MSE-optimal bandwidth and data for all available years after reporting sick which includes 1,398,000 observations for 11,650 couples.

## Appendix C Common trend assumption for sick people without spouse

Figure 7 presents the estimates of pre-treatment effects of the reform for sick people without a spouse for three outcomes and provides evidence in favor of the common trend assumption.

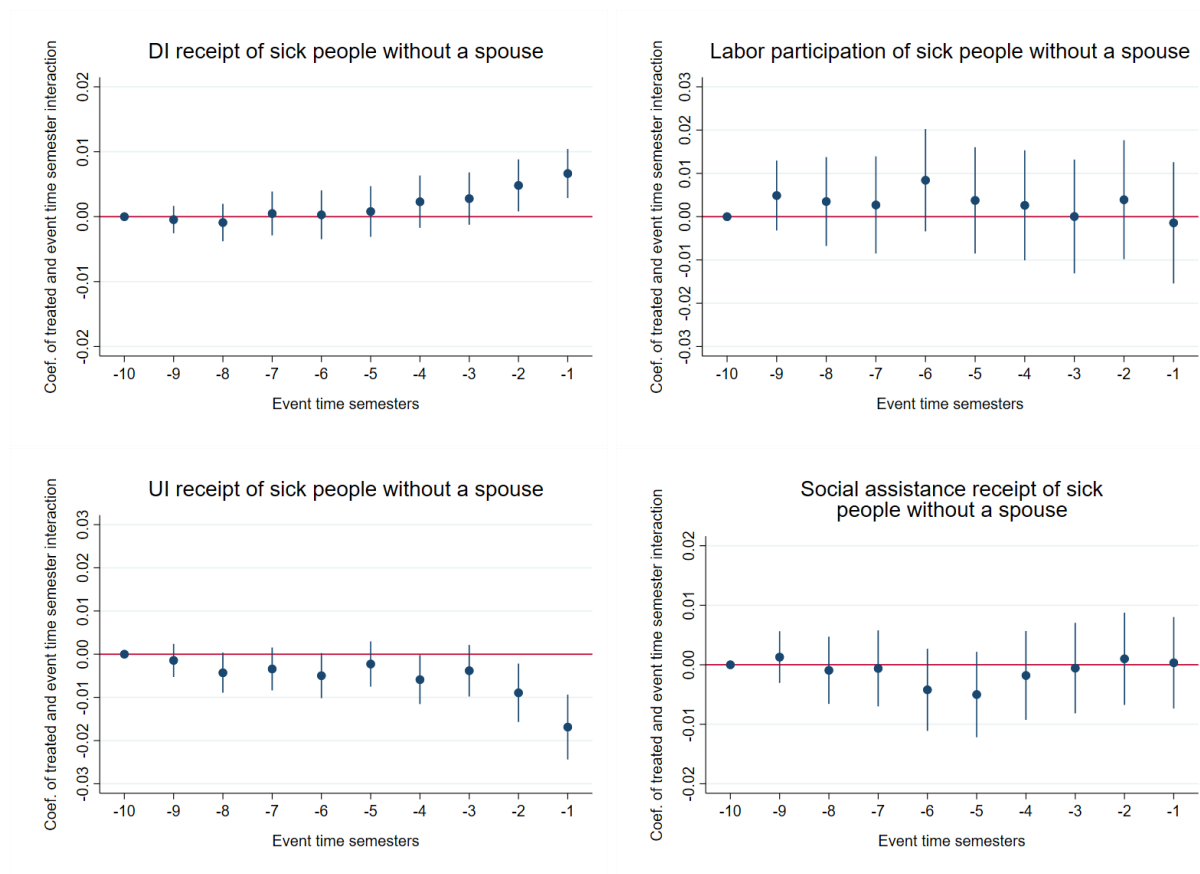


Figure 7: Estimates of pre-treatment effects of the reform for sick people without spouse, with 95 percent confidence intervals. Notes: Standard errors are adjusted for heteroskedasticity and clustering at the individual level. The F-statistic (p-value in parentheses) for the assumption of common pre-treatment trend is 3.421 (0.000) for DI receipt, 1.314 (0.223) for labor participation, 3.746 (0.000) for UI receipt, and 1.570 (0.118) for social assistance receipt.



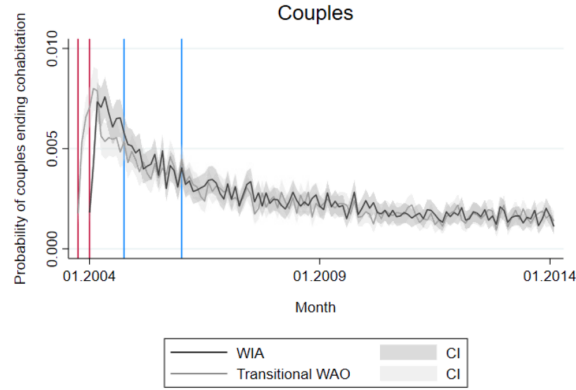


Figure 8: Probability of couples ending cohabitation after one spouse falls sick. Vertical lines mark the first instance sick partners could become entitled to the sickness and disability benefits in the transitional WAO and WIA schemes. Red lines correspond to 1 October 2003 and 1 January 2004 for the 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.

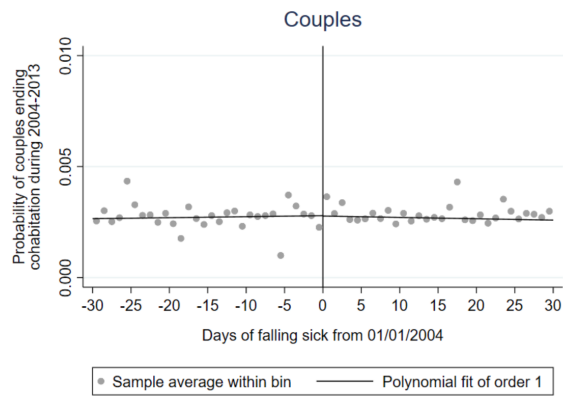


Figure 9: Local linear fit on the two sides of the cut-off. Standard errors are clustered at the individual level. The figure uses 1,996,200 observations for 16,635 couples. The figure uses data for all available years after reporting sick.

Table 9: Estimated effects of the WIA reform on labor participation, earnings, unemployment insurance receipt, and social assistance receipt of couples who stay together post-treatment

	Sick individual	Spouse
Disability insurance receipt	−0.034*** 0.003	−0.002 (0.002)
Labor participation	0.013*** (0.004)	0.012*** (0.004)
Unemployment insurance receipt	0.011*** (0.002)	0.001 (0.001)
Social assistance receipt	−0.000 (0.001)	0.000 (0.001)
ln Disability insurance	−0.224*** (0.021)	−0.011 (0.014)
ln Wage	0.106*** (0.032)	0.081*** (0.027)
ln Unemployment insurance benefit	0.077*** (0.011)	0.007 (0.007)
ln Social assistance benefit	−0.003 (0.006)	−0.001 (0.006)
ln Total individual income	0.022 (0.027)	0.066*** (0.025)
ln Total household income	0.036* (0.021)	
Observations	5,667,579	
Individuals	37,043	

Notes: \*\*\*, \*\*, \* denote statistical significance at 1, 5 and 10 percent, respectively. Standard errors (in parentheses) account for heteroskedasticity and clustering at the individual level. Linear probability models. In all specifications we control for individual and calendar month fixed effects. The regressions use data available for the whole pre-treatment period but exclude data for the first two years of the post-treatment period.