

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

Mario Bernasconi[†] Tunga Kantarci[‡] Arthur van Soest[§] and Jan-Maarten van Sonsbeek[¶]

22 October 2022

Abstract

The Netherlands reformed its disability insurance (DI) scheme in 2006. Reintegration incentives became stronger, access to DI benefits became more difficult and the benefits often 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, and investigate if spousal labor supply is a substitute for sick people's own labor supply when facing a stricter DI regime. Difference-in-differences estimates show that spouses respond to the DI reform only when the labor market position of their sick partners is weak, i.e., when they entered sickness from a temporary job, worked in a sector with low vacancy rates, or earned low wages prior to sickness. The effects are persistent during the ten years after the reform. We interpret the effect on the spouse as an "added worker effect" where additional earnings of the spouse compensate for the sick individual's income loss so that both partners share the burden of a more stringent DI scheme. Comparing with singles provides further support for this interpretation.

1 Introduction

A large and growing strand of the literature analyzes income complementarities in the household as an insurance mechanism. The "added worker effect" hypothesis suggests that married women respond to a negative shock on their husbands' earnings due to unemployment by increasing their hours of paid work (Lundberg, 1985). Most studies find no or a small added worker effect

*This research is supported by Netspar under grant number LMVP 2019.01. Its contents are the sole responsibility of the authors. We thank UWV, and in particular Lucien Rondagh, Willy van den Berk, Carla van Deursen, and Roel Ydema, for providing the disability data. We thank the participants of the Netspar Pension Day 2020, the Netspar International Pension Workshop 2021, and the participants of the annual meetings of ESPE 2021, SEHO 2022 and EALE 2022 for their constructive comments and suggestions on an earlier version of the paper. Results are based on calculations by the authors using non-public microdata from Statistics Netherlands. Under certain conditions, these microdata are accessible for statistical and scientific research. For further information: microdata@cbs.nl.

[†]Department of Econometrics and Operations Research, Tilburg University, P.O. Box 90153, 5000 LE Tilburg, The Netherlands, and Netspar (e-mail: m.bernasconi@tilburguniversity.edu)

[‡]Department of Econometrics and Operations Research, Tilburg University, P.O. Box 90153, 5000 LE Tilburg, The Netherlands, and Netspar (e-mail: kantarci@tilburguniversity.edu)

[§]Department of Econometrics and Operations Research, Tilburg University, P.O. Box 90153, 5000 LE Tilburg, The Netherlands, and Netspar (e-mail: a.h.o.vansoest@tilburguniversity.edu)

[¶]Department of Labor and Knowledge, Netherlands Bureau for Economic Policy Analysis, P.O. Box 80510, 2508 GM The Hague, The Netherlands, and Netspar (e-mail: j.m.van.sonsbeek@cpb.nl)

(Maloney, 1987, 1991; Spletzer, 1997; García-Gómez et al., 2012; Bredtmann et al., 2018; Halla et al., 2020). One explanation is that the affected partner is 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 wife's employment prospects may be affected by the factors causing the husband's unemployment (Cullen and Gruber). Moreover, the wife's response may be small if the husband's unemployment only leads to a transitory reduction in earnings (Cullen and Gruber; Bredtmann 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; Bredtmann et al.). Two recent studies, however, do find a notable added worker effect. Blundell et al. 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. Schøne and Strøm (2021) find that the rise in wives' labor supply annihilates around one third of the loss in husbands' earnings.

The DI context is interesting to investigate the added worker effect because DI denial may imply a permanent income shock for applicants, and may trigger a larger spousal response than the one found in the literature on unemployment.

In the Netherlands, in 2002, the share of disabled workers in the insured population reached about 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, replacing the final version of the old system, "transitional WAO". WIA introduced major changes in both the DI scheme and the sickness insurance (SI) scheme preceding it, making it much more difficult to become eligible for but also stay on DI benefits. 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 the WIA reform on labor participation and benefit receipt among long-term sick individuals (both with and without partner). They found that the reform from transitional WAO to WIA substantially reduced the probability of DI receipt during the ten years after the reform. On the other hand, they found a rise in the labor participation rate and in the unemployment insurance benefits receipt that added up to almost half of the fall in DI receipt.

Since couples can pool income risk, spousal labor supply can be an important insurance mechanism to offset the loss of DI benefits. The current paper therefore focuses on whether spouses also responded to the reform. Taking a difference-in-differences approach, we compare the responses of not only the sick individuals who participated in the WIA and transitional WAO schemes, but also their spouses. We find that among sick individuals with a spouse, DI receipt fell by 3.1 percentage points (pp) and labor participation rose by 1 pp, while their spouses' labor participation rose by 0.5 pp, suggesting that both partners share the burden of a more stringent DI scheme.

To investigate the factors driving spousal responses to the DI reform, we analyze how spousal responses depend on the labor market position of the sick individuals. For individuals with a weaker initial labor market position, i.e., fewer opportunities to go back to work after recovery, there might be a larger need to compensate for the more stringent rules of the new DI system. In

this case the spouse's response may be larger. Exploring heterogeneity in employment conditions of sick individuals, we show that sick individuals who had a permanent work contract at the time of reporting sick increased labor participation by 1.6 pp, while their spouses did not respond. In contrast, sick individuals who had a temporary work contract did not increase labor participation, while their spouses' participation rose by 2.5 pp. These labor supply responses are sizable vis à vis the reduction in DI receipt for the sick individuals (2.7 pp and 3.1 pp for those on a permanent and temporary work contract, respectively). Furthermore, if sick individuals worked in a sector with a low vacancy rate or if they were low wage earners, spouses increased labor participation while the sick individuals themselves hardly responded. These effects are persistent over a period of ten years after reporting sick. Findings for other outcomes (earnings, UI benefits) confirm that the spouse's response is larger when the labor market position of the sick individual is weaker.

We also analyze how the reform affected labor supply responses 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. We show that for sick individuals without a spouse, the effect on labor participation is much larger than for those with a spouse (2.2 pp instead of 1 pp). This confirms our main finding 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.

These findings 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 DI reform comes from the spouse. Our findings on the heterogeneous effects of the reform identify conditions where an added worker effect may be evident.

Our findings also contribute to the strand of the literature analyzing the impact of DI reforms, offering important policy implications for the many countries where the number of DI recipients is large and growing (OECD, 2018). This literature analyses the effects of screening process and eligibility criteria (Karlström et al., 2008; De Jong et al., 2011; Staubli, 2011; Moore, 2015; Autor et al., 2016; Hullegie and Koning, 2018; Godard et al., 2022), benefit generosity (Gruber, 2000; Campolieti, 2004; Marie and Vall Castello, 2012; Mullen and Staubli, 2016; Deuchert and Eugster, 2019), and return-to-work incentives (Kostøl and Mogstad, 2014; Koning and van Sonsbeek, 2017; Ruh and Staubli, 2019; Zaresani, 2018, 2020). It also studies welfare effects (Low and Pistaferri, 2015; Deshpande, 2016; Fevang et al., 2017). None of these studies consider spillover 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.

At the intersection of the two strands of the literature on the added worker effect and the impact of DI reforms 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 offset the loss of DI benefits with higher earnings and social support income, but found no significant effect on spousal earnings. 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. In ongoing work, García-Mandicó et al. (2021) analyze the impact of an earlier

reform in the Netherlands in 2004, reassessing DI recipients younger than 45 years with new DI eligibility criteria introduced in 2004 in the Netherlands. They find significant positive effects on male but not on female spouses.

Our study differs from these studies in several respects. 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 of heterogenous effects in labor supply decisions of couples, possibly due to limited statistical power. We show that job security, employment opportunities, and earnings level of the sick spouse are important determinants 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.

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 Section 7 compares with the reform effects on singles. Section 8 conducts checks on the identifying assumptions. Section 9 concludes. Appendices include extended analyses.

2 Disability insurance in the Netherlands and the 2006 reform

The Disability Insurance Act (WAO) was introduced in 1967 to insure against loss of earnings due to long-term work disability. During the late 1970s and 1980s the number of DI beneficiaries rose rapidly to levels far beyond earlier expectations. Although major amendments were implemented in 1993, the WAO 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 at least 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).¹ When the sickness scheme expired, individuals were admitted to the disability scheme (DI) if their disability grade was at least 15 percent.

Due to easy access, the annual inflow rate into WAO rose to 1.5 percent of the insured working population in 2001, leading to further reforms. In April 2002 the “Gatekeeper protocol” was introduced, in which clear and concrete mutual obligations of employers and sick employees for reintegration during the sickness period were specified. A transitional WAO scheme was introduced on 1 October 2004 for people who reported sick between October 1 2003 and January 1 2004, making entry criteria 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 or to reach a high disability grade (with a high benefit).

¹UWV is the public body that implements employee insurance policies.

WIA was introduced in 2006 for people who reported sick from 1 January 2004 onwards. It introduced major changes in both sickness and disability schemes, stimulating work resumption. It reduced the annual inflow rate into DI to 0.5 percent of the insured working population during the first six years after its introduction ([Koning and Lindeboom, 2015](#)). WIA extended the duration of the sickness scheme from one to two years, implying an extension of two main incentives: First, the employer is obliged to compensate the employee for 70 percent of the wage loss during the additional sickness period, creating a strong incentive for the employer to facilitate work resumption.² Second, the Gatekeeper protocol was extended to a second year of sickness ([Hullegie and Koning, 2018](#)).

For the disability scheme, WIA kept the stricter DI eligibility criteria of the transitional WAO scheme with the broader definition of what work can still be done. It introduced three other changes. First, the minimum disability grade for entering the scheme rose from 15 to 35 percent – workers with limited disability are expected to resume working (with adaptations of their work if necessary) or apply for UI. Second, the scheme introduced a work resumption program providing strong financial incentives for partially disabled people to utilize remaining work capacity. Third, experience rating for employers was extended from 5 to 10 years, implying that employers are penalized with higher DI premiums if they incurred disability costs for up to five additional years. At the same time experience rating was restricted to partially disabled workers and abolished for permanently and fully disabled workers. Targeting the former group made experience rating more effective since the partially or temporary disabled have better prospects of reintegration. Experience rating was limited to permanent work contracts until 2013 and then extended to temporary contracts.³

For the income of the sick individuals during sickness and disability benefit periods, potential implications of the reform are as follows. In the first year of sickness, wages are not affected by the reform. However, employers may already do more for reintegration in the first year of sickness if they anticipate the cost of the additional year of wage payments. These stronger employer incentives may induce sick individuals to return to work, especially in combination with the requirements of the Gatekeeper protocol. On the other hand, reintegration incentives for employees might have become weaker in the first year since employees are no longer subject to a DI assessment after one year of sickness.

In the second year of sickness, WIA requires that the employer replaces (at least) 70 percent of the former wage. In WAO, DI and UI benefits together replaced 70 percent of the former wage. From the third year onwards, a potential fall in income is due to lower or a complete loss of DI benefits. As described above, this owes to the stricter eligibility criteria for DI, financial incentives for work resumption, and extended and more targeted reintegration incentives of experience rating. Note that these implications of the reform assume that the sick individual has a stable work contract with an employer. Employees with weak employer relationships or those who are unemployed will lack the reform incentives and may struggle to resume work and cope with the negative income shock of the reform. They may seek alternative welfare benefits, or rely on the income of their spouse.

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

³[Prinz and Ravesteijn \(2020\)](#) find that extension of experience rating reduced DI receipt by 12.7 pp and increased labor participation by 2.5 pp among workers with temporary contracts relative to those with permanent contracts.

3 Data

We use unique administrative data from UWV on all individuals who fell 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.⁴ We observe the beginning and ending dates of their sickness, their gender, date of birth, and sector of economic activity. 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 have 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 and UI. These data extend from January 1999 to February 2014.

The initial data set 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 only keep those who spent more than 90 days in sickness leave, 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 sample 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. We will not consider individuals who fell sick before October 1 2003 and fall under the old WAO scheme and will therefore refer to the transitional WAO group as WAO group from now on.

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

Figure 1 shows the labor participation rates and fractions of DI and UI recipients in control and treatment groups over the observation period.⁵ For the individuals in our data who all reported sick, DI benefit receipt increases sharply when they become eligible for DI benefits and continues to increase during the remaining years of the observation period. The WIA group is 3 pp less likely to receive DI benefits than the control group (13.5% versus 10.5%) and the difference between the two groups remains stable till the end of the observation period. This shows that

⁴Individuals 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.

⁵Similar figures for wages and benefit amounts (not shown) reveal very similar patterns.

the reform effectively limited access to DI benefits. For the spouses of sick people, DI receipt is stable and not affected by the reform (as expected).

For 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 WAO and WIA 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 groups is insignificant before and 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.⁶ 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.

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 Figure 1, the difference is larger post- than pre-treatment for all outcomes, again suggesting that the reform has increased labor participation, earnings, UI receipt, and the amount of UI benefits.

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.

⁶The maximum duration of UI changed from 60 to 38 months in October 2006.

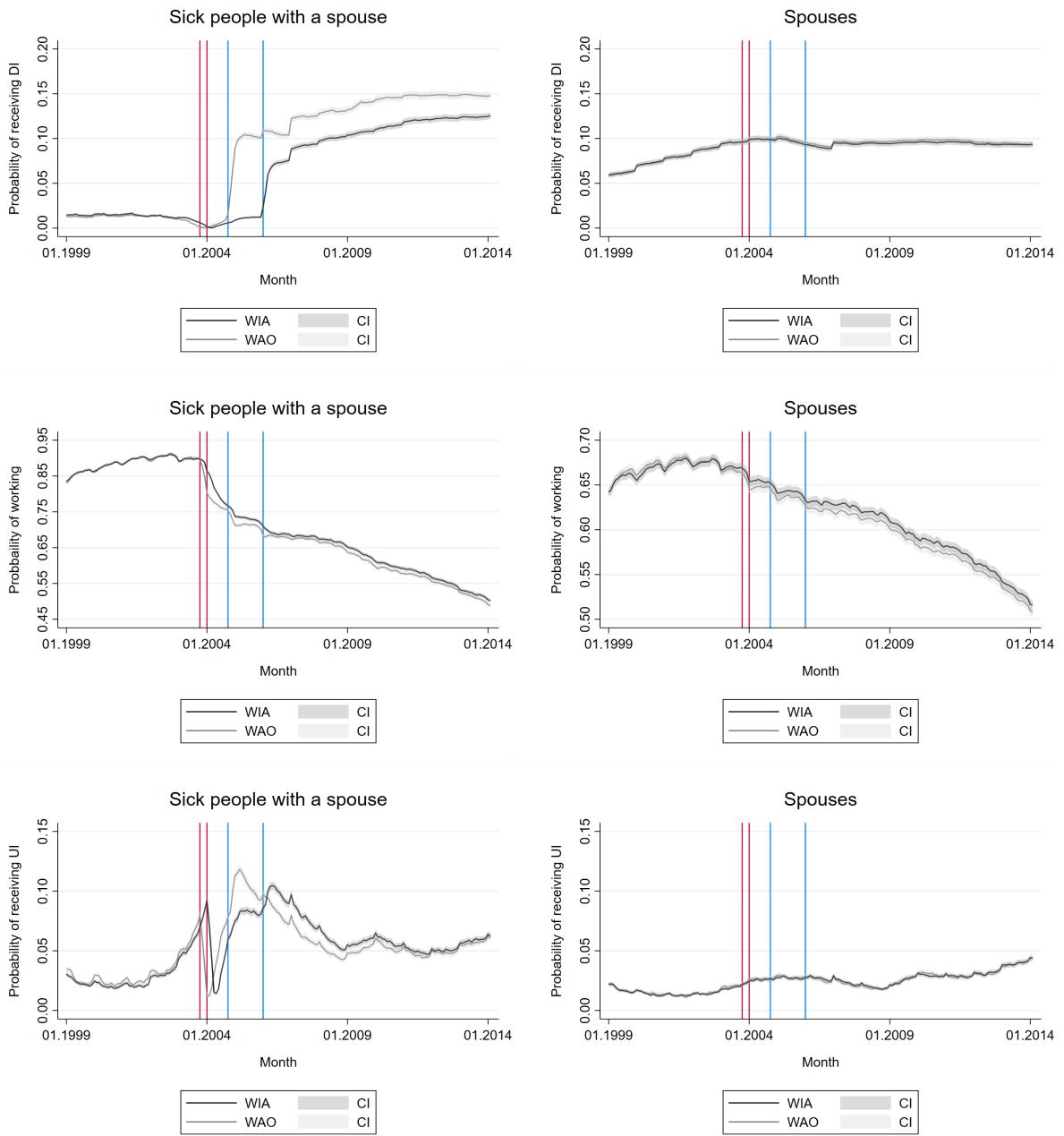


Figure 1: Probability of DI receipt, working, and UI receipt for control and treatment groups by calendar month; sick individuals (left) and their spouses (right). Vertical lines mark the first instance sick individuals can be entitled to sickness (red) and disability (blue) benefits.

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			
	WAO group	WIA group	Dif. WIA and WAO	WAO group	WIA group	Dif. WIA and WAO
	(1)	(2)	(3)	(4)	(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	-0.029***
Labor participation (%)	0.887	0.889	0.002	0.611	0.615	0.004
UI (no DI) receipt (%)	0.033	0.033	0.000	0.057	0.063	0.006***
DI (and possibly UI) received per month				187.298	162.690	-24.608***
Wage per month	1,999.228	2,012.334	13.106	1,713.940	1,738.323	24.383*
UI (excl. DI) per month	39.938	40.128	0.190	89.747	92.873	3.126*
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 (WAO) and treatment (WIA) group before and after start of sickness. Columns 3 and 6 present differences between individuals insured under WIA and WAO – the estimated coefficient from the regression of the characteristic or outcome as the dependent variable, and an indicator of participation in 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		Diff. WIA and WAO (6)
	WAO	WIA group	Dif. WIA and WAO	WIA group	
	(1)	(2)	(3)	(4)	
A. Background characteristics					
Age	42.333	42.535	0.205**	8.269	8.224
Years of cohabitation	7.031	7.173	0.142***		-0.045
B. Labor market outcomes					
DI (possibly UI) receipt (%)	0.670	0.669	-0.001	0.096	0.094
Labor participation (%)	0.016	0.016	0.000	0.586	0.590
UI (no DI) receipt (%)				0.027	0.028
DI (and possibly UI) received per month	1,299.714	1,312.522	12.808	1,552.520	113.790
Wage per month	18.356	19.220	0.864	38.291	1,561.632
UI (excl. DI) per month					9.112
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 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.

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 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:

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

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 . α_i is an individual-specific, time-invariant fixed effect that is potentially correlated with the control variables. ε_{it} represents an idiosyncratic (unobserved) shock, assumed to be uncorrelated with all the explanatory variables.

$Treated_i$ is a dummy variable for the treatment (WIA) group.⁷ $Post_t$ is an event time dummy with value 1 from the start of the sickness period. 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 γ on the interaction term $Treated_i \times Post_t$ captures the effect of the reform, the main parameter of interest.⁸

To disentangle the effect of the WIA reform in the short and long run, and test for the common trend assumption, we consider the following extended model:

$$y_{it} = \alpha_i + \sum_{l=-5}^9 \gamma_l (Treated_i \times d_{lt}) + \sum_{l=-5}^9 \delta_l d_{lt} + \lambda_{s(i,t)} + \varepsilon_{it}. \quad (2)$$

Instead of $Post_t$ which refers to the entire period after falling sick, this model has separate dummies for each year, after and before falling sick. d_{lt} indicates the l -th year from the time the individual reports sick. Year -1 is chosen as the base year. The coefficients on the interaction terms of treatment and these year dummies are the estimated treatment effects.⁹ For the years before reporting sick, they provide a test of the common trend assumption. For the period after reporting sick they reflect the dynamic effects of the reform. In this setup, treatment and control groups are compared over event time t , i.e., the months before and after the individual reported sick. The calendar time dummies $\lambda_{s(i,t)}$ on the other hand capture the (common) calendar time trend.

To control for observed differences between treatment and control individuals before reporting sick, we apply entropy balancing following Hainmueller (2012). In particular, individuals are weighted to adjust inequalities in representation with respect to the first moment of the covariate distributions. As covariates, we consider their gender and birth year, as well as all

⁷Since this is time invariant, it is omitted in the fixed effects regression.

⁸We 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.

⁹Here we also include observations for $t = 0, \dots, 23$.

outcomes of the sick individuals and their spouses before the first group reported sick. Regressions of equations (1) and (2) are estimated based on the constructed weights.¹⁰ The weights are regenerated in each subsample when analyzing heterogenous treatment effects. To check whether, after entropy balancing, the common trend assumption is satisfied pre-treatment and to analyze several other threats to our identification strategy, we perform additional analyses and robustness checks in Section 8.

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 for heterogeneous effects of the reform.

6.1 Baseline effects

Table 2 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 3.1 percentage points (pp) on average during the post-treatment period (excluding the first two years). It increased the probability of working by 1 pp. It also increased UI receipt by 0.8 pp. The reform induced the spouses of the sick individuals to raise their labor participation by 0.5 pp although the effect is statistically significant only at the 10 percent level. These effects 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 2 shows the DiD estimates of the reform effects for monthly wages and benefits (in log of the amount plus 1). The reform decreased monthly DI benefits of sick people by 20.3 percent. It increased monthly earnings by 7.6 percent and monthly income from UI by 5.6 percent. Moreover, it increased earnings of spouses by 3.6 percent but the increase is statistically not significant.

The lower panel of Table 2 presents the DiD estimates of the reform effects for monthly total income (in log of the amount plus 1), pooling income sources from monthly wages and social security benefits including DI, UI and social assistance. The table presents the total income of sick individuals and spouses at the individual level, and of couples at the household level. On average, the reform does not significantly affect total income of sick individuals, their spouses, or their household. This suggests that sick individuals are able to compensate lost disability benefits by increasing earnings and income from UI to keep the total income unchanged. This may explain why spouses contribute to total household income but do not increase it by a significant amount. That is, since there is no significant shock to the total income of the sick individual, there is no need for a spousal earnings response. In Section 6.3 we investigate a number of labor market conditions where sick individuals are less likely to compensate lost disability benefits to trigger spousal earnings responses as a plausible insurance mechanism.

6.2 Dynamic effects

Figure 2 presents the estimates of the reform effects for ten years of the post-treatment period. For the sick individuals, the reform decreases DI receipt by about 3 pp from the third year after reporting sick, when both the treatment and the control group can apply for DI. The effect of the reform on labor participation seems particularly large during the first year of the sickness

¹⁰Following Imbens (2004) and using propensity scores to construct weights leads to almost identical estimates.

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 pp and remains fairly stable. 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 falls over time and becomes insignificant from year 8 after reporting sick. In each year after reporting sick, the effect of the reform on spousal labor participation is about 0.5 pp but not significant. In line with the exploratory analysis in Section 4, the reform has no significant effect on spouses' UI receipt.

6.3 Heterogeneous effects

Baseline results show that sick individuals increase labor market participation to compensate for the lost DI benefits, while their spouses do so to a limited extent since, on average, the total income of sick individuals does not change. Existing studies on the added worker effect discuss a variety of factors that could induce wives to respond to a negative shock on their husbands' earnings. Some consider the nature of the income shock and argue that spouses would respond if the income shock is permanent, unanticipated, or its magnitude is large (Cullen and Gruber, 2000; Stephens, 2002; Blundell et al., 2016; Bredtmann et al., 2018; Fadlon and Nielsen, 2021). Others consider that lack of self-insurance through savings or formal insurance through social support programs, high earnings potential of the wife, and existence of job opportunities for wives may encourage wives to respond (Cullen and Gruber; Bentolila and Ichino, 2008; Blundell et al.; Halla et al., 2020). We hypothesize that when employment conditions of workers facing the income shock are weak, spousal responses will be stronger. We explore three conditions: employment status when reporting sick, earnings level before reporting sick, and the sectoral vacancy rate in the year of reporting sick.¹¹

Employment status when reporting sick

If sick individuals have temporary work contracts and therefore uncertain future earnings, spouses may respond more strongly when DI benefits are lost. Furthermore, stimulating employers to increase labor market participation has been a key element of Dutch labor market reforms throughout the years. The WIA reform in particular introduced strong reintegration incentives for employers. It extended wage compensation from the employer and mutual obligations of employers and sick employees for reintegration to an additional year in the sickness scheme. It also extended experience rating from 5 to 10 years in the disability scheme (Section 2). However, these incentives do not apply uniformly to all workers.

Employer incentives for temporary workers last only as long as the contract lasts. Temporary work agencies do not face incentives for their sick employees during sickness since the Employee Insurance Agency pays their sickness benefits. Unemployed individuals have no employer and therefore cannot benefit from positive effects of employer incentives either. On the other hand, employers of employees with a permanent contract are fully incentivized due to experience rating and continued wage payments. In other words, sick individuals who have a temporary contract or are unemployed have no employer who is incentivized to reintegrate them. As a consequence, they may struggle more to go back to paid work after recovery and more often have to cope

¹¹The literature on the added worker effect typically focuses on the wife's response to shocks in the husband's income. In contrast to García-Mandicó et al. (2021) who analyzed an earlier Dutch reform, we find hardly any differences between the spousal effects for men and women (details available upon request).

with a negative income shock due the reform. They may then also more often have to rely on their spouses' income.

We separately analyze the reform effects for sick individuals who were wage earners with a permanent contract, wage earners with a temporary contract, or unemployed at the time they reported sick. The upper panel of Table 3 presents the results. Due to the reform, DI receipt fell substantially for all groups, and the largest fall is for the unemployed, who have no employer that is stimulated to make them resume work. This increases their chances of remaining in the sickness scheme for a longer time and facing the stricter requirements of WIA to enter DI.

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 is larger than for those on a permanent contract – the former have lower and less stable earnings, and seek additional income from UI if access to DI benefits is limited by the reform. Only the spouses of sick individuals on a permanent contract increase their UI receipt by a statistically significant amount, possibly because they increase their labor participation the least.

For the sick individuals, the reform increases labor participation among those with a permanent work contract but not among those with a temporary contract or the unemployed, even though the fall in DI receipt is larger for the latter two groups. It seems that the reform's work resumption incentives induce employers to reintegrate their permanent employees, but are not effective if there is no employer or the work contract is temporary. 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 work quickly may be reduced by the additional year they can spend in the sickness scheme.

The spouses of sick individuals with a permanent work contract do not respond to the reform, while spouses of sick individuals who work on a temporary contract increase their labor participation and earnings significantly. Figure 6 shows that the effect for the latter group is persistent. Since sick individuals with a temporary 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 spouses of sick individuals who are unemployed also increase their labor participation by a notable amount of 1 pp but the effect is not significant.

The signs and significance of the estimated effects of the reform on earnings and benefit amounts in the upper panel of Table 4 are in line with the estimated effects on labor participation and benefit receipt.

The upper right panel of Table 4 presents the effects of the reform on total income (in log of the amount plus 1) 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, confirming that spouses' responses help to smooth household income. This result is in line with the findings of Blundell et al. (2016) based on a structural family labor supply model where households self-insure through spousal labor supply in case of a negative income shock. For sick individuals with a permanent contract, total income did not change significantly at the individual or household level.

Pre-sickness earnings

If sick individuals earn low wages (regardless of their sickness), spouses may respond more strongly for different reasons. Low wage earners may have smaller savings or wealth to draw on during sickness to smooth their consumption path. It might also be that they more often work in jobs where prospects of recovery from ill health are limited. If as a result the income shock becomes permanent, spouses may exhibit stronger responses. Pre-sickness earnings represent the average of the wages earned by sick individuals during five years before they report sick (where data is available). The center panel of Table 3 presents the estimation results by pre-sickness earnings quartile. Sick individuals in lower earnings quartiles increase their UI receipt somewhat more. It might be that these individuals struggle more to find suitable jobs where they can utilize their remaining work capacity and therefore have to rely on income from UI. For the lowest two quartiles, we find that spouses increase their labor participation. For the higher quartiles, however, no spousal response is observed. These results are in line with the results based on employment status in the preceding subsection – both suggest that spousal responses are stronger for sick individuals in a weak labor market position.

Vacancies in the sector

For sick individuals who have limited employment opportunities and hence a higher risk of unemployment, responding to the work incentives of the DI reform can be more difficult. In this case spousal labor supply responses can be stronger. We consider the sectoral vacancy rate as an indicator of employment opportunities – the number of open vacancies per one thousand jobs. The sector where sick individuals are or were employed is available in the sickness data, and we determine the vacancy rate in each sector prior to and in the year of reporting sick using data from Statistics Netherlands. We distinguish two groups: individuals who at the time of reporting sick worked in sectors with vacancy rates below (e.g., construction, manufacturing, transport, public sector) or above the average vacancy rate (e.g., agriculture, trade, financial services, catering). The lower panels of Tables 3 and 4 present the results. If sick individuals work in a sector with a vacancy rate below the average, their spouses increase labor participation by 0.9 pp. This can be due to the limited employment opportunities for the sick individual.

In contrast, if sick individuals work in a sector where the vacancy rate is above the average, the reform raises their labor participation by 1 pp and their spouses do not respond. Again, these results suggest that the added worker effect is a more powerful insurance mechanism when the labor market position of the sick individual affected by the reform is weak.

Table 2: Estimated effects of the WIA reform: Sick individuals and their spouses

	Sick individual	Spouse
DI receipt	-0.031*** (0.002)	-0.001 (0.002)
Labor participation	0.010*** (0.003)	0.005* (0.003)
UI receipt	0.008*** (0.001)	0.000 (0.001)
ln DI	-0.203*** (0.018)	-0.011 (0.012)
ln Wage	0.076*** (0.027)	0.036 (0.023)
ln UI	0.056*** (0.009)	0.003 (0.006)
ln Total individual income	-0.010 (0.023)	0.016 (0.022)
ln Total household income	0.009 (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. All specifications 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.

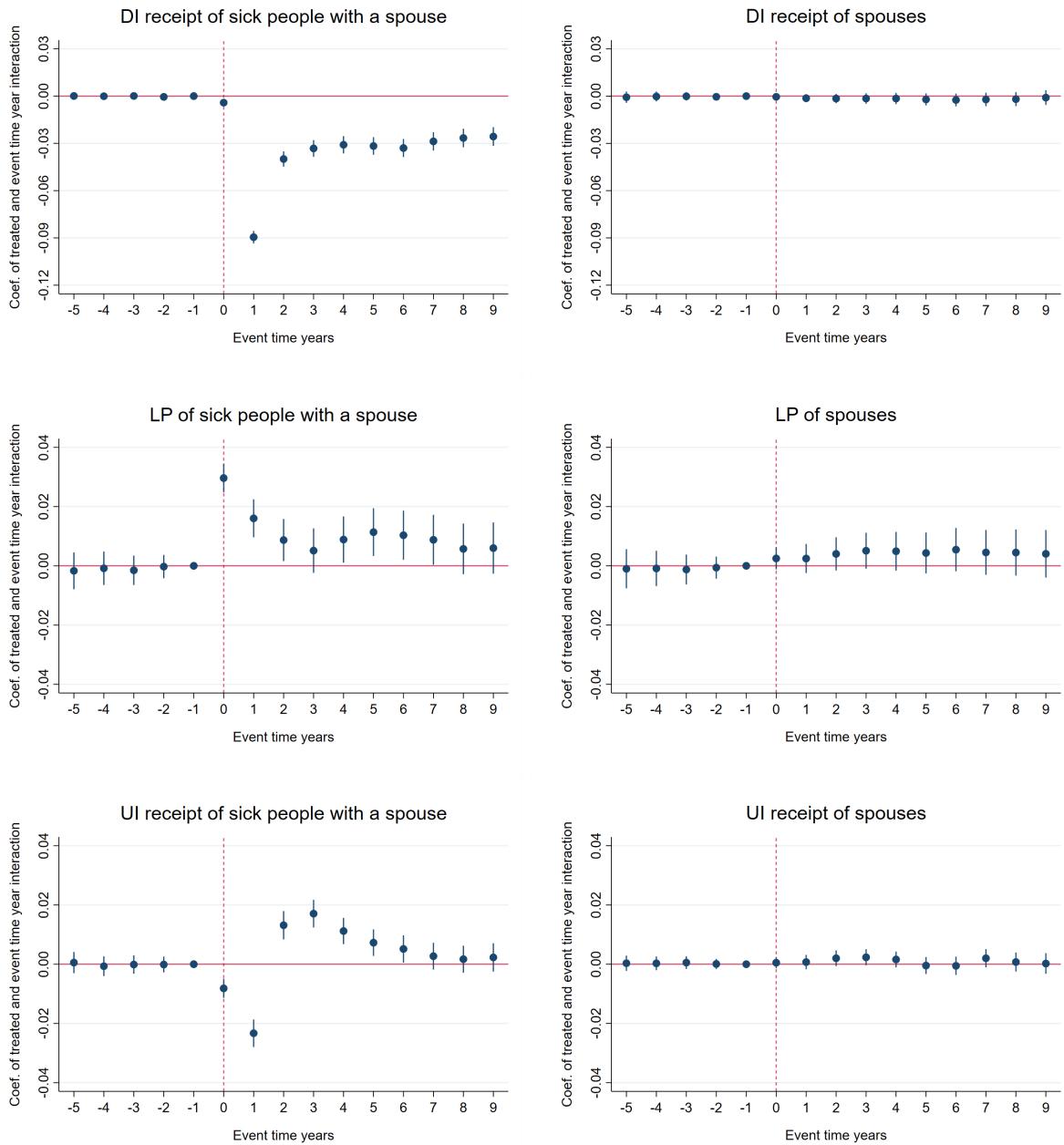


Figure 2: Estimated treatment effects in each of the five years before reporting sick and in each of the first ten years after reporting sick, with 95 percent confidence intervals. Observed differences between treatment and control individuals before reporting sick are controlled for using entropy balancing.

Table 3: Estimated effects of the WIA reform by labor market status, quartiles of average earnings before reporting sick, and sectoral vacancy rate

	DI receipt		Labor participation		UI receipt	
	Sick individual	Spouse	Sick individual	Spouse	Sick individual	Spouse
On permanent contract	-0.027*** (0.002)	-0.002 (0.002)	0.016*** (0.004)	0.001 (0.004)	0.004*** (0.001)	0.002** (0.001)
On temporary contract	-0.031*** (0.009)	-0.008 (0.006)	-0.015 (0.012)	0.025*** (0.009)	0.013*** (0.004)	-0.002 (0.003)
Unemployed	-0.041*** (0.008)	0.003 (0.005)	-0.014 (0.009)	0.010 (0.008)	0.021*** (0.005)	-0.004 (0.003)
Earnings in 1st quartile	-0.025*** (0.006)	-0.000 (0.004)	0.006 (0.007)	0.013** (0.006)	0.011*** (0.003)	-0.001 (0.002)
Earnings in 2nd quartile	-0.024*** (0.005)	-0.001 (0.004)	0.010 (0.007)	0.012* (0.006)	0.008*** (0.002)	0.002 (0.002)
Earnings in 3rd quartile	-0.036*** (0.005)	-0.001 (0.004)	0.004 (0.006)	-0.002 (0.006)	0.007*** (0.002)	0.002 (0.002)
Earnings in 4th quartile	-0.037*** (0.004)	-0.001 (0.003)	0.013* (0.007)	-0.004 (0.006)	0.007*** (0.002)	-0.000 (0.002)
Vacancy rate below the mean	-0.039*** (0.004)	-0.000 (0.003)	0.008* (0.005)	0.009** (0.004)	0.008*** (0.002)	0.000 (0.001)
Vacancy rate above the mean	-0.022*** (0.003)	-0.002 (0.002)	0.010** (0.005)	0.002 (0.004)	0.008*** (0.002)	0.001 (0.001)

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. All specifications 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.

Table 4: Estimated effects of the WIA reform by labor market status, by quartile of average earnings before reporting sick, and by sectoral vacancy rate

	In DI		In Wage		In UI		In Total individual income		In Total household income	
	Sick individual	Spouse individual	Sick individual	Spouse	Sick individual	Spouse	Sick individual	Spouse	Sick individual	Spouse
On permanent contract	-0.183*** (0.018)	-0.014 (0.014)	0.130*** (0.031)	0.004 (0.027)	0.027*** (0.009)	0.015** (0.007)	0.029 (0.027)	-0.006 (0.026)	0.012 (0.021)	
On temporary contract	-0.196*** (0.067)	-0.057 (0.041)	-0.126 (0.088)	0.182** (0.007)	0.091*** (0.030)	-0.016 (0.019)	-0.180** (0.075)	0.090 (0.071)	-0.017 (0.054)	
Unemployed	-0.265*** (0.059)	0.015 (0.035)	-0.126* (0.070)	0.076 (0.059)	0.148*** (0.033)	-0.030 (0.018)	-0.145** (0.062)	0.070 (0.059)	-0.040 (0.049)	
Earnings in 1st quartile	-0.164*** (0.041)	-0.004 (0.028)	0.039 (0.053)	0.098** (0.049)	0.073*** (0.023)	-0.009 (0.014)	-0.056 (0.051)	0.080* (0.048)	0.032 (0.039)	
Earnings in 2nd quartile	-0.152*** (0.036)	-0.013 (0.026)	0.076 (0.052)	0.071 (0.047)	0.057*** (0.017)	0.010 (0.013)	0.017 (0.041)	0.019 (0.046)	0.025 (0.032)	
Earnings in 3rd quartile	-0.232*** (0.035)	-0.004 (0.024)	0.030 (0.052)	-0.008 (0.044)	0.048*** (0.015)	0.014 (0.012)	-0.031 (0.043)	0.003 (0.042)	-0.010 (0.034)	
Earnings in 4th quartile	-0.260*** (0.033)	-0.006 (0.022)	0.121** (0.057)	-0.036 (0.047)	0.051*** (0.016)	-0.000 (0.012)	0.015 (0.050)	-0.029 (0.045)	-0.036 (0.041)	
Vacancy rate below the mean	-0.258*** (0.026)	-0.002 (0.018)	0.064* (0.039)	0.063* (0.033)	0.060*** (0.013)	0.000 (0.009)	-0.048 (0.034)	0.046 (0.031)	-0.001 (0.027)	
Vacancy rate above the mean	-0.145*** (0.025)	-0.018 (0.017)	0.084** (0.038)	0.010 (0.034)	0.058*** (0.013)	0.005 (0.009)	0.037 (0.033)	-0.009 (0.033)	0.025 (0.026)	

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. All specifications 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. If they do not manage to do that, they may more often rely on social assistance. This implies that reducing DI generosity may have adverse effects when there is no partner, both for the beneficiary (who is more at risk of losing income) and for the cost saving (due to social support substitution).

In Figure 3 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. During post-treatment, while the reform effect on DI receipt is negative and comparable for sick people with and without a spouse, the effect on labor participation is positive and notably larger for those without a spouse. Figures for wages and benefit amounts lead to the same conclusions (not shown).

Table 5 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 2.¹² The DiD estimates confirm that the reform increases the probability of working post-treatment by 1.2 pp more among sick people without a spouse than for sick individuals in couples. Together with the finding (in Table 2) 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.

The reform effects for earnings and benefits are in line with this. For example, sick people without a spouse increase their earnings by 15.8 percent in response to the reform, whereas for sick people with a spouse the increase in earnings is only 7.6 percent.

As in Section 6, we also consider the possibility that the reform effect depends on the time since the individual fell sick, see Figure 4. The time pattern of the effect on labor participation is similar to that for sick people with a spouse reproduced in the left panel of Figure 4: It is large in the first year (due to the measurement problem explained above) and then falls, but it remains significant at about 2 pp during the next nine years, showing that the effect of the reform on labor participation of sick people without a spouse is persistent, and larger than that for sick people with a spouse, in the long run.¹³ The time patterns of the effects on DI and UI receipt are also similar to those for sick people with a spouse; persistent and significant throughout the entire post-treatment period. Similar time patterns are found for the effects on wage and benefit amounts (not shown).

In Section 6.3 we analyzed heterogeneity in labor supply responses to the DI reform to better understand how couples make joint labor supply decisions. Here we conduct the same heterogeneity analysis for singles.¹⁴ The upper panel of Table 6 shows that due to the reform, DI

¹²Figure 4 presents the estimates of pre-treatment effects for sick individuals without a spouse for all outcomes and provide evidence in favor of the common trend assumption.

¹³As in Section 6, interpreting the effects in the first two years is difficult because of the measurement issue explained in Section 3.

¹⁴As for the partnered sick individuals, we find no notable differences between men and women for singles (results not presented).

receipt fell for sick people regardless of their contract type and partnership status. Sick people responded by increasing UI claims, again, regardless of their contract type or partnership status. The changes in DI and UI receipt are in line with each other: If DI receipt fell more, UI claims increased more. There is, however, a clear difference in how labor participation changed for sick people with and without spouses. When on a permanent contract, both groups increase their labor participation by about 1.5 pp. When on a temporary contract, however, sick individuals in a partnership do not increase labor participation while those who are single increase it by 2.2 pp. This is in line with our earlier finding in Table 3 that spouses share the burden of the negative income shock of the reform if sick individuals are on a temporary contract.

Similar results are obtained for monthly earnings and benefits in upper panel of Table 7. Pooling monthly earnings and benefits, the right hand side of the table shows the reform effects for the monthly total income of sick people with and without 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 change significantly due to the reform, unlike total income of sick people in a partnership. Singles increase their earnings by the same amount 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 larger amount and the smaller amounts of UI benefits they claim are not enough to compensate.

The center panel of Table 6 shows clear differences between sick people with and without a spouse when their labor supply responses are compared to each other in four earnings quartiles. Singles do not respond if their earnings are in the highest quartile, while they increase labor participation significantly if their earnings are in lower quartiles. Sick individuals with a spouse show much weaker responses in lower earnings quartiles. In this case their spouses respond to compensate as shown in Table 4. In line with the heterogenous reform effects with respect to employment status, these results show that if there is no spouse who can compensate, sick individuals act to compensate for the income loss with own labor participation when their labor market position is weak.

The lower panel of Table 6 presents the results by sectoral vacancy rate. Among the sick individuals working in sectors with vacancy rates below the mean, singles increase labor participation and earnings more than those who are in a partnership. Again, in this case those in a partnership rely on spousal labor supply to compensate for the income loss as shown in Table 3. These results, again, confirm our main finding that when there is no spouse to compensate, labor supply responses are stronger among singles when their labor market conditions are not favorable.

Figures 6 to 7 in Appendix A present the dynamic effects by employment status, vacancy rate, and earnings quartile for all groups: sick people with a spouse, their spouses, and sick people without a spouse. They are in line with the main findings. In groups where spouses respond, sick individuals without spouse also respond, confirming that in couples both partners share the burden of a more stringent DI scheme. The estimated effects at individual event years are not always significant at the 5 percent level, however.

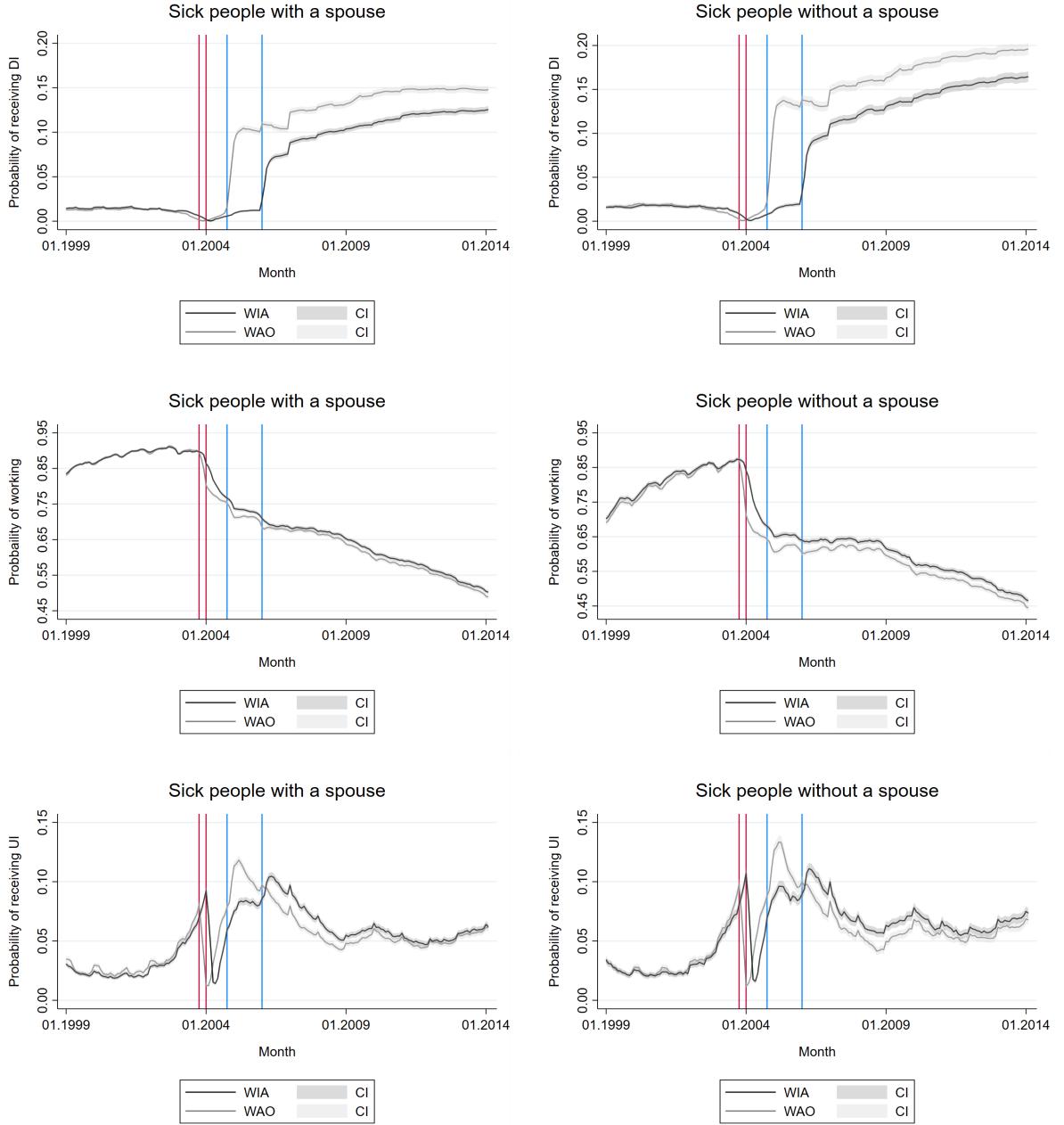


Figure 3: Probability of DI receipt, working, and UI receipt for control and treatment groups over calendar months: for sick individuals with a spouse (left panel reproducing the left panel of Figure 1) and without a spouse (right panel). Vertical lines mark the first instance sick partners could become entitled to the sickness and disability benefits in the WAO and WIA schemes. Red lines correspond to 1 October 2003 and 1 January 2004 for the WAO and WIA groups, respectively. Blue lines correspond to 1 October 2004 and 1 January 2006 for the WAO and WIA groups, respectively.

Table 5: Estimated effects of the WIA reform: Individuals with and without spouse

	Sick individual with a spouse	Sick individual without a spouse
DI receipt	-0.031*** (0.002)	-0.036*** (0.004)
Labor participation	0.010*** (0.003)	0.022*** (0.005)
UI receipt	0.008*** (0.001)	0.010*** (0.002)
ln DI	-0.203*** (0.018)	-0.246*** (0.027)
ln Wage	0.076*** (0.027)	0.158*** (0.038)
ln UI	0.056*** (0.009)	0.072*** (0.012)
ln Total individual income	-0.010 (0.023)	-0.008 (0.032)
Observations	8,431,218	4,425,372
Individuals	55,106	28,924

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 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.

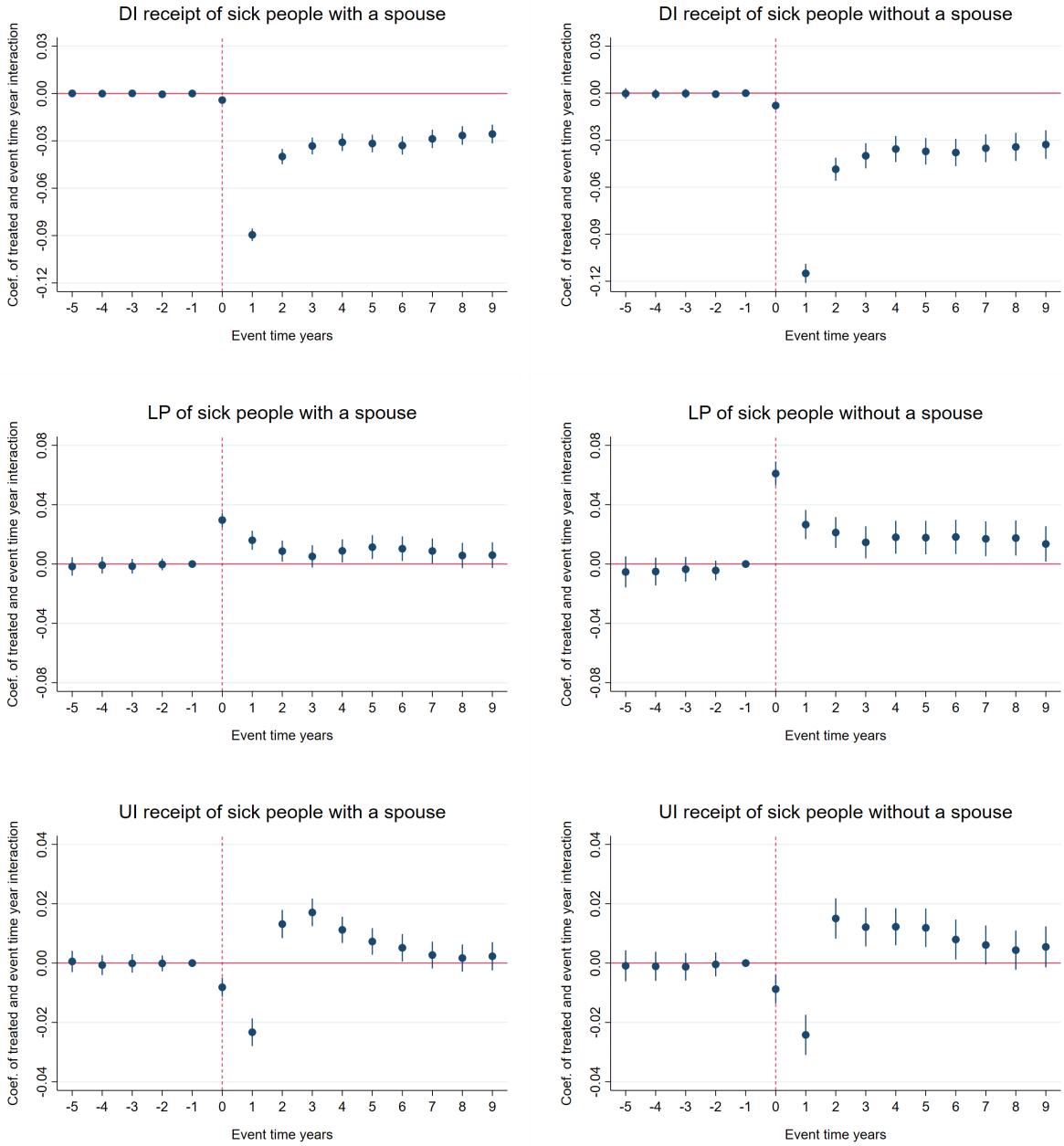


Figure 4: Estimated treatment effects in each of the first ten years after falling sick, with 95 percent confidence intervals. Observed differences between treatment and control individuals before reporting sick are controlled for using entropy balancing.

Table 6: Estimated effects of the WIA reform by labor market status, quartiles of average earnings before reporting sick, and sectoral vacancy rate

	DI receipt		Labor participation		UI receipt	
	Sick people with a spouse	Sick people without a spouse	Sick people with a spouse	Sick people without a spouse	Sick people with a spouse	Sick people without a spouse
On permanent contract	-0.027*** (0.002)	-0.022*** (0.004)	0.016*** (0.004)	0.015** (0.006)	0.004*** (0.001)	0.007*** (0.002)
On temporary contract	-0.031*** (0.009)	-0.036*** (0.009)	-0.015 (0.012)	0.022* (0.011)	0.013*** (0.004)	0.015*** (0.004)
Unemployed	-0.041*** (0.008)	-0.055*** (0.010)	-0.014 (0.009)	0.000 (0.011)	0.021*** (0.005)	0.016*** (0.005)
Earnings in 1st quartile	-0.025*** (0.006)	-0.033*** (0.009)	0.006 (0.007)	0.028*** (0.010)	0.011*** (0.003)	0.007* (0.004)
Earnings in 2nd quartile	-0.024*** (0.005)	-0.040*** (0.008)	0.010 (0.007)	0.031*** (0.009)	0.008*** (0.002)	0.015*** (0.004)
Earnings in 3rd quartile	-0.036*** (0.005)	-0.034*** (0.007)	0.004 (0.006)	0.022** (0.009)	0.007*** (0.002)	0.006* (0.003)
Earnings in 4th quartile	-0.037*** (0.004)	-0.037*** (0.007)	0.013* (0.007)	-0.000 (0.009)	0.007*** (0.002)	0.012*** (0.003)
Vacancy rate below the mean	-0.039*** (0.004)	-0.040*** (0.006)	0.008* (0.005)	0.023*** (0.007)	0.008*** (0.002)	0.010*** (0.002)
Vacancy rate above the mean	-0.022*** (0.003)	-0.031*** (0.005)	0.010** (0.005)	0.020*** (0.007)	0.008*** (0.002)	0.011*** (0.002)

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. All specifications 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.

Table 7: Estimated effects of the WIA reform by labor market status, quartiles of average earnings before reporting sick, and sectoral vacancy rate

	In DI		In Wage		In UI		In Total individual income	
	Sick people with a spouse	Sick people without a spouse						
On permanent contract	-0.183*** (0.018)	-0.142*** (0.030)	0.130*** (0.031)	0.112** (0.050)	0.027*** (0.009)	0.050*** (0.014)	0.029 (0.027)	0.060 (0.041)
On temporary contract	-0.196*** (0.067)	-0.248*** (0.065)	-0.126 (0.089)	0.182** (0.090)	0.092*** (0.030)	0.105*** (0.026)	-0.180** (0.075)	-0.046 (0.075)
Unemployed	-0.265*** (0.059)	-0.378*** (0.073)	-0.126* (0.070)	-0.015 (0.083)	0.148* (0.033)	0.118*** (0.037)	-0.145** (0.062)	-0.212*** (0.069)
Earnings in 1st quartile	-0.164*** (0.041)	-0.227*** (0.061)	0.039 (0.053)	0.220** (0.074)	0.073*** (0.024)	0.050* (0.023)	-0.056 (0.051)	-0.051 (0.069)
Earnings in 2nd quartile	-0.152*** (0.036)	-0.275*** (0.056)	0.076 (0.052)	0.241*** (0.072)	0.057*** (0.017)	0.109*** (0.026)	0.017 (0.041)	0.064 (0.056)
Earnings in 3rd quartile	-0.232*** (0.035)	-0.226*** (0.053)	0.030 (0.052)	0.176** (0.071)	0.048*** (0.015)	0.043* (0.024)	-0.031 (0.043)	0.040 (0.054)
Earnings in 4th quartile	-0.260*** (0.033)	-0.241*** (0.050)	0.121** (0.057)	0.011 (0.074)	0.051*** (0.016)	0.086*** (0.023)	0.015 (0.050)	-0.026 (0.061)
Vacancy rate below the mean	-0.258*** (0.026)	-0.269*** (0.040)	0.064* (0.039)	0.169*** (0.056)	0.060*** (0.013)	0.072*** (0.019)	-0.048 (0.034)	0.016 (0.046)
Vacancy rate above the mean	-0.145*** (0.025)	-0.209*** (0.040)	0.084** (0.040)	0.146*** (0.053)	0.058*** (0.013)	0.076*** (0.017)	0.037 (0.033)	-0.007 (0.044)

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. All specifications 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.

8 Checks on the identifying assumptions

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 WAO or WIA. This means that individuals with adverse health shocks in 2003 might select themselves into the WAO or WIA scheme from the time the reform is announced. In particular, 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 WAO reform was announced only on 12 March 2004, and details of the WIA reform were announced on 18 August 2004. 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 to enter the more lenient WAO scheme instead of the stricter WIA scheme. On the other hand, individuals could have an incentive to postpone their sickness claim until January 2004. They could know that DI rules would be stricter under the new scheme, but they could also know that they would get an additional year of sickness benefits which can be attractive if they think that their sickness is temporary. In these cases, the estimated impact of the reform can be biased.

We argue that such self-selection is unlikely. In Figure 5 we analyze the pattern of sick reporting in the last quarter of 2003 and first quarter of 2004 by individuals in the study sample who became eligible to participate, respectively, in the WAO or the WIA schemes. The distribution of sick reports is fairly 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, there are more sick reports in January 2004, after the stricter WIA scheme was introduced. It is unlikely that the increase in January is due to an aim to get an additional year of sickness benefits. The sickness benefit reduces from 100 percent of the former wage in the first year to 70 percent in the second year. Furthermore, it is unlikely that individuals are better off if they opt for sickness benefits for a second year in the WIA than for DI and UI benefits after receiving sickness benefits for one year in the WAO since in both cases income replacement is 70 percent (Section 2). The relatively low number of sick reports in December 2003 might owe to a seasonal employment pattern in that employees are absent from work, and do not report sickness during Christmas and New Year holidays.

Figure 5 also presents sick reporting one year after the reform among individuals who participated in the WIA only. The distribution of sick reporting across the six months is very similar to that the year before when the reform was introduced, providing additional evidence that there is no self-selection.

What is more, self-selection would be expected to happen only among people with mild sicknesses who would have time to seek medical attention to manipulate the timing of their sick reporting. However, both pre- and post-reform, the same Gatekeeper protocol was in place, according to which after 6 weeks of sickness a first reintegration plan has to be submitted to the Employee Insurance Agency by the employer. Due to this formal screening of sick reporting, mild sickness cases, including those with a manipulated timing of sick reporting, will be rejected to SI by then. Therefore, regardless of if it is the control or treatment group, the individuals on SI who constitute our study sample are not likely to have engaged in self-selection.

If, however, some individuals manage to select themselves to a DI scheme, they would most likely do this around 1 January 2004 when the WIA reform has come into effect. If we exclude individuals who report sickness within two weeks before or after this date, the baseline DiD results of our heterogeneity analysis, where we find statistically significant responses, remain largely unaffected (details available upon request).

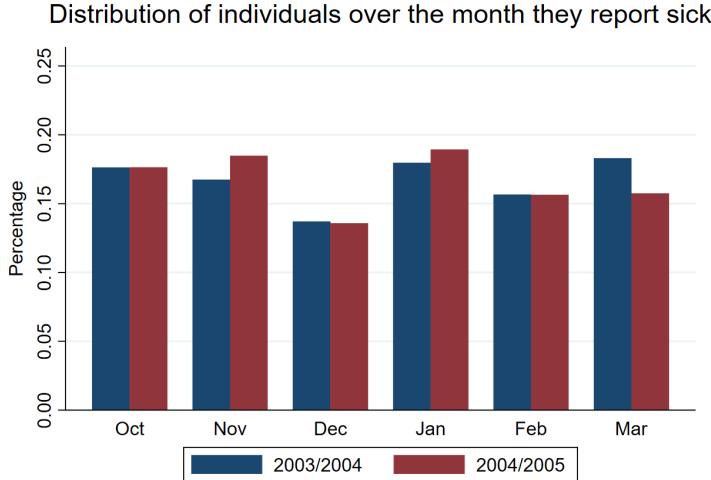


Figure 5: Distribution of the number of individuals reporting sick, among those who reported sick in the last quarter of 2003 and first quarter of 2004 and participated, respectively, in the WAO and WIA, and those who reported sick in the last quarter of 2004 and first quarter of 2005 and participated in the WIA.

Placebo test: Is a treatment effect absent in a non-reform year?

Our identification strategy assumes that the estimated effect is due to the reform and not some other factor. In our DiD design, treatment is falling sick in the first quarter of 2004 and participating in the reformed DI scheme, instead of falling sick in the fourth quarter of 2003 and participating in the old DI scheme. Other than the treatment itself, factors correlated with calendar time and the outcome could bias the estimated reform effect. To check this, we use data on individuals who reported sick one year after the reform and participated in the WIA only. We estimate the DiD regression given by equation (1), comparing individuals who reported sick in the fourth quarter of 2004 and the first quarter of 2005 as placebo control and treatment groups, respectively. For both the sick individuals and their spouses, estimated treatment effects are close to 0 for all outcomes, and insignificant at the 10 percent or higher levels (details available upon request).

Is the pre-treatment time trend 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 1 shows 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 equation (2). In particular, statistically insignificant estimates on the treatment and annual dummy interactions during the pre-treatment period provide evidence in favor of the assumption. Year -1 is chosen as the base for comparison. Figure 2 plots the estimates for sick individuals (left hand panel) and 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 with p-values of at least 70 percent. The estimates are also insignificant for wages and benefit amounts, and in all

sub-group analyses of heterogenous treatment effects (available upon request).

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 B we present the 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. A possible explanation is 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 – see Appendix C.

9 Conclusion

We analyze the labor supply and earnings responses of spouses to a change in the income of their sick partners. The change is due to a major reform that introduced stricter eligibility criteria for DI. Since couples can pool income risk, spousal labor supply can be an important self-insurance mechanism to counterbalance the loss of income. 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, 0.5 pp 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 pp. These effects are economically meaningful, considering that DI receipt decreased by 3.1 pp for the sick people due to the reform. This finding is particularly 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, for the effects on labor participation as well as adequacy of household income.

The effect of the reform on spousal labor supply depends on the type of the employment contract of the sick individual when falling sick. People who had a permanent contract at the time they fell sick increased labor market participation by 1.6 pp due to the reform, while their spouses did not respond. On the other hand, people who had a temporary contract when they fell sick did not increase labor participation because of the reform, while their spouses increased labor participation by 2.5 pp. The spousal response is persistent during the ten years

following the start of sickness. Overall, this shows that the response at the couple level is sizable 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. The effect of the reform also depends on the employment opportunities of the sick individuals: If they worked in a sector with a vacancy rate above the mean, they increased labor participation by 1 pp while their spouses did not respond, but if the sectoral vacancy rate was below the mean, spouses increased labor participation by a similar amount of 0.9 pp. Finally, spouses increased labor participation more often if their sick partners are low wage earners. These findings are consistent with the hypothesis that partners substitute for each other's labor force participation and spouses more often respond if the labor market position of the sick individual is weaker.

Various explanations for the lack of evidence of an added worker effect are given in the literature. There are 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 individual. In line with this, we find persistent responses of both the sick individuals and their spouses in the ten years following sickness (Figure 2). 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 1). 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, being unemployed, or having limited employment opportunities at the time of sick reporting, where spousal responses are observed exclusively, may have contributed to each of these three reasons.

References

- Arulampalam, W., 2001. Is unemployment really scarring? Effects of unemployment experiences on wages. *The Economic Journal* 111 (475), F585–606.
- Arulampalam, W., Gregg, P., Gregory, M., 2001. Introduction: unemployment scarring. *The Economic Journal* 111 (475), F577–584.
- Autor, D., Kostøl, A., Mogstad, M., Setzler, B., 2019. Disability benefits, consumption insurance, and household labor supply. *American Economic Review* 109 (7), 2613–54.
- Autor, D. H., Duggan, M., Greenberg, K., Lyle, D. S., 2016. The impact of disability benefits on labor supply: Evidence from the VA's disability compensation program. *American Economic Journal: Applied Economics* 8 (3), 31–68.
- Bentolila, S., Ichino, A., 2008. Unemployment and consumption near and far away from the Mediterranean. *Journal of Population Economics* 21 (2), 255–280.
- Blundell, R., Pistaferri, L., Saporta-Eksten, I., February 2016. Consumption inequality and family labor supply. *American Economic Review* 106 (2), 387–435.
- Borghans, L., Gielen, A. C., Luttmer, E. F. P., 2014. Social support substitution and the earnings rebound: evidence from a regression discontinuity in disability insurance reform. *American Economic Journal: Economic Policy* 6 (4), 34–70.
- Bredtmann, J., Otten, S., Rulff, C., 2018. Husbands unemployment and wife's labor supply: the added worker effect across Europe. *ILR Review* 71 (5), 1201–1231.
- Calonico, S., Cattaneo, M. D., Titiunik, R., 2014. Robust data-driven inference in the regression-discontinuity design. *The Stata Journal* 14 (4), 909–946.

- Campolieti, M., 2004. Disability insurance benefits and labor supply: some additional evidence. *Journal of Labor Economics* 22 (4), 863–889.
- Cullen, J. B., Gruber, J., 2000. Does unemployment insurance crowd out spousal labor supply? *Journal of Labor Economics* 18 (3), 546–572.
- De Jong, P., Lindeboom, M., van der Klaauw, B., 2011. Screening disability insurance applications. *Journal of the European Economic Association* 9 (1), 106–129.
- Deshpande, M., 2016. The effect of disability payments on household earnings and income: Evidence from the SSI children's program. *Review of Economics and Statistics* 98 (4), 638–654.
- Deuchert, E., Eugster, B., 2019. Income and substitution effects of a disability insurance reform. *Journal of Public Economics* (170), 1–14.
- Duggan, M., Rosenheck, R., Singleton, P., 2010. Federal policy and the rise in disability enrollment: Evidence for the veterans affairs disability compensation program. *The Journal of Law and Economics* 53 (2), 379–398.
- Fadlon, I., Nielsen, T. H., 2021. Family labor supply responses to severe health shocks: evidence from Danish administrative records. *American Economic Journal: Applied Economics* 13 (3), 1–30.
- Fevang, E., Hardoy, I., Red, K., 2017. Temporary disability and economic incentives. *The Economic Journal* 1127 (603), 1410–1432.
- García-Gómez, P., van Kippersluis, H., O'Donnell, O., van Doorslaer, E., 2012. Long-term and spillover effects of health shocks on employment and income. *The Journal of Human Resources* 48 (4), 873–909.
- García-Mandicó, S., García-Gómez, P., Gielen, A., O'Donnell, O., 2021. The impact of social insurance on spousal labor supply: Evidence from cuts to disability benefits in the Netherlands. Mimeo, Erasmus University Rotterdam.
- Godard, M., Koning, P., Lindeboom, M., 2022. Application and award responses to stricter screening in disability insurance. *The Journal of Human Resources* 57 (3).
- Gruber, J., 2000. Disability insurance benefits and labor supply. *Journal of Political Economy* 108 (6), 1162–1183.
- Hahn, J., Todd, P., Van der Klaauw, W., 2001. Identification and estimation of treatment effects with a regression-discontinuity design. *Econometrica* 69 (1), 201–209.
- Hainmueller, J., 2012. Entropy balancing for causal effects: a multivariate reweighting method to produce balanced samples in observational studies. *Political Analysis* 20 (1), 25–46.
- Halla, M., Schmieder, J., Weber, A., 2020. Job displacement, family dynamics, and spousal labor supply. *American Economic Journal: Applied Economics* 12 (4), 253–287.
- Hullegie, P., Koning, P., 2018. How disability insurance reforms change the consequences of health shocks on income and employment. *Journal of Health Economics* 62, 134–146.
- Imbens, G. W., 2004. Nonparametric estimation of average treatment effects under exogeneity: A review. *Review of Economics and Statistics* 86 (1), 4–29.
- Imbens, G. W., Lemieux, T., 2008. Regression discontinuity designs: A guide to practice. *Journal of Econometrics* 142 (2), 615 – 635, the regression discontinuity design: Theory and applications.
- Kantarcı, T., van Sonsbeek, J.-M., Zhang, Y., 2019. The impact of the disability insurance reform on work resumption and benefit substitution in the Netherlands. Netspar Discussion Paper 01/2019-013.
- Karlström, A., Palme, M., Svensson, I., 2008. The employment effect of stricter rules for eligibility for di: Evidence from a natural experiment in sweden. *Journal of Public Economics* 92 (10-11), 2071–2082.

- Koning, P., Lindeboom, M., 2015. The rise and fall of disability insurance enrollment in the Netherlands. *Journal of Economic Perspectives* 29 (2), 151–172.
- Koning, P., van Sonsbeek, J.-M., 2017. Making disability work? The effects of financial incentives on partially disabled workers. *Labour Economics* 47, 202–215.
- Kostøl, A. R., Mogstad, M., 2014. How financial incentives induce disability insurance recipients to return to work. *American Economic Review* 104 (2), 624–655.
- Low, H., Pistaferri, L., 2015. Disability insurance and the dynamics of the incentive insurance trade-off. *American Economic Review* 105 (10), 2986–3029.
- Lundberg, S., 1985. The added worker effect. *Journal of Labor Economics* 3 (1, Part 1), 11–37.
- Maloney, T., 1987. Employment constraints and the labor supply of married women: a reexamination of the added worker effect. *The Journal of Human Resources* 22 (1), 5161.
- Maloney, T., 1991. Unobserved variables and the elusive added worker effect. *Economica* 58 (230), 173–187.
- Marie, O., Vall Castello, J., 2012. Measuring the (income) effect of disability insurance generosity on labour market participation. *Journal of Public Economics* 96 (1-2), 198–210.
- Moore, T. J., 2015. The employment effects of terminating disability benefits. *Journal of Public Economics* 124, 30–43.
- Mullen, K. J., Staubli, S., 2016. Disability benefit generosity and labor force withdrawal. *Journal of Public Economics* 143, 49–63.
- OECD, 2018. Public spending on incapacity.
- Prinz, D., Ravesteyn, B., 2020. Employer responsibility in disability insurance: Evidence from the Netherlands. Mimeo, Harvard University.
- Ruh, P., Staubli, S., 2019. Financial incentives and earnings of disability insurance recipients: evidence from a notch design. *American Economic Journal: Economic Policy* 11 (2), 269–300.
- Schøne, P., Strøm, M., 2021. International labor market competition and wives labor supply responses. *Labour Economics* 70 (101983).
- Spletzer, J. R., 1997. Reexamining the added worker effect. *Economic Inquiry* 35 (2), 417–427.
- Staubli, S., 2011. The impact of stricter criteria for disability insurance on labor force participation. *Journal of Public Economics* 95 (9-10), 1223–1235.
- Stephens, M. J., 2002. Worker displacement and the added worker effect. *Journal of Labor Economics* 20 (3), 504–537.
- Zaresani, A., 2018. Return-to-work policies and labor supply in disability insurance programs. *AEA Papers and Proceedings* 108, 272–276.
- Zaresani, A., 2020. Adjustment cost and incentives to work: Evidence from a disability insurance program. *Journal of Public Economics* 188 (104223).

Appendix A Dynamic heterogenous effects

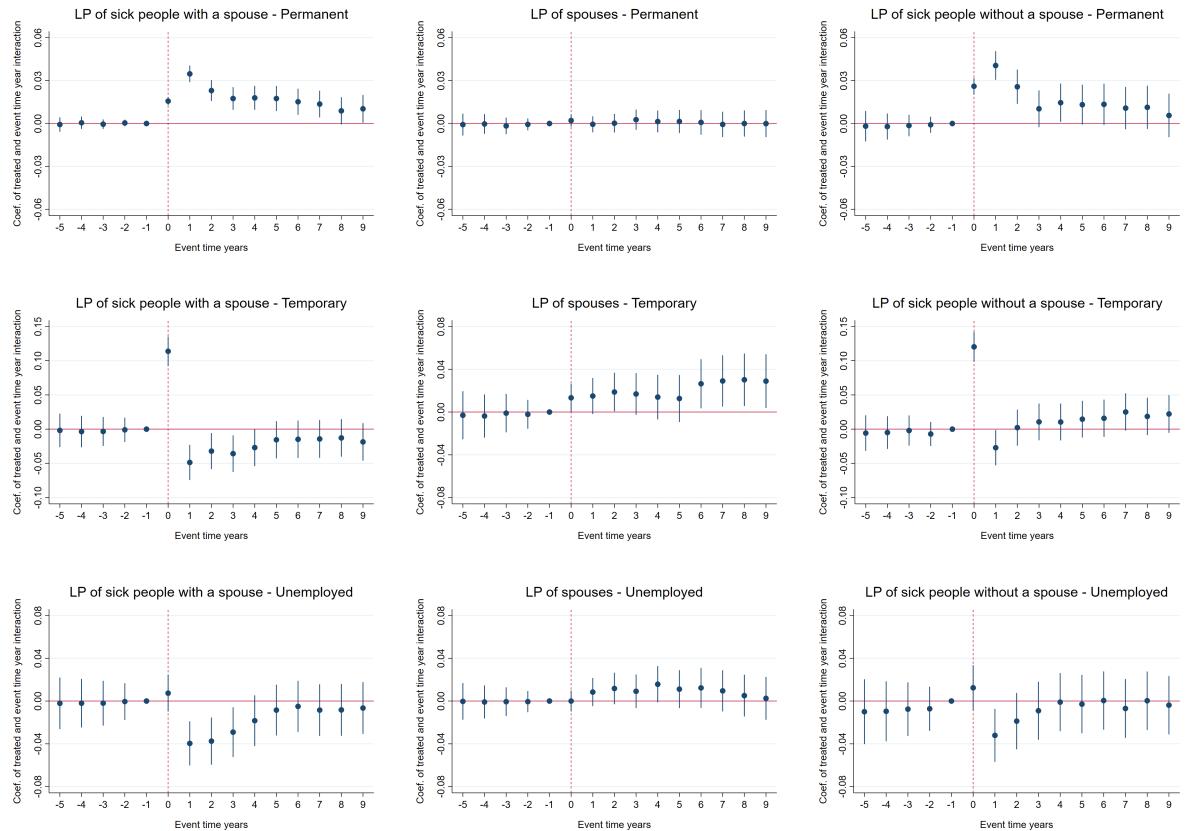


Figure 6: Estimated treatment effects in each of the five years before reporting sick and in each of the first ten years after reporting sick, with 95 percent confidence intervals for sick individuals, their spouses, and sick individuals without spouses, by labor market status when reporting sick. Observed differences between treatment and control individuals before reporting sick are controlled for using entropy balancing.

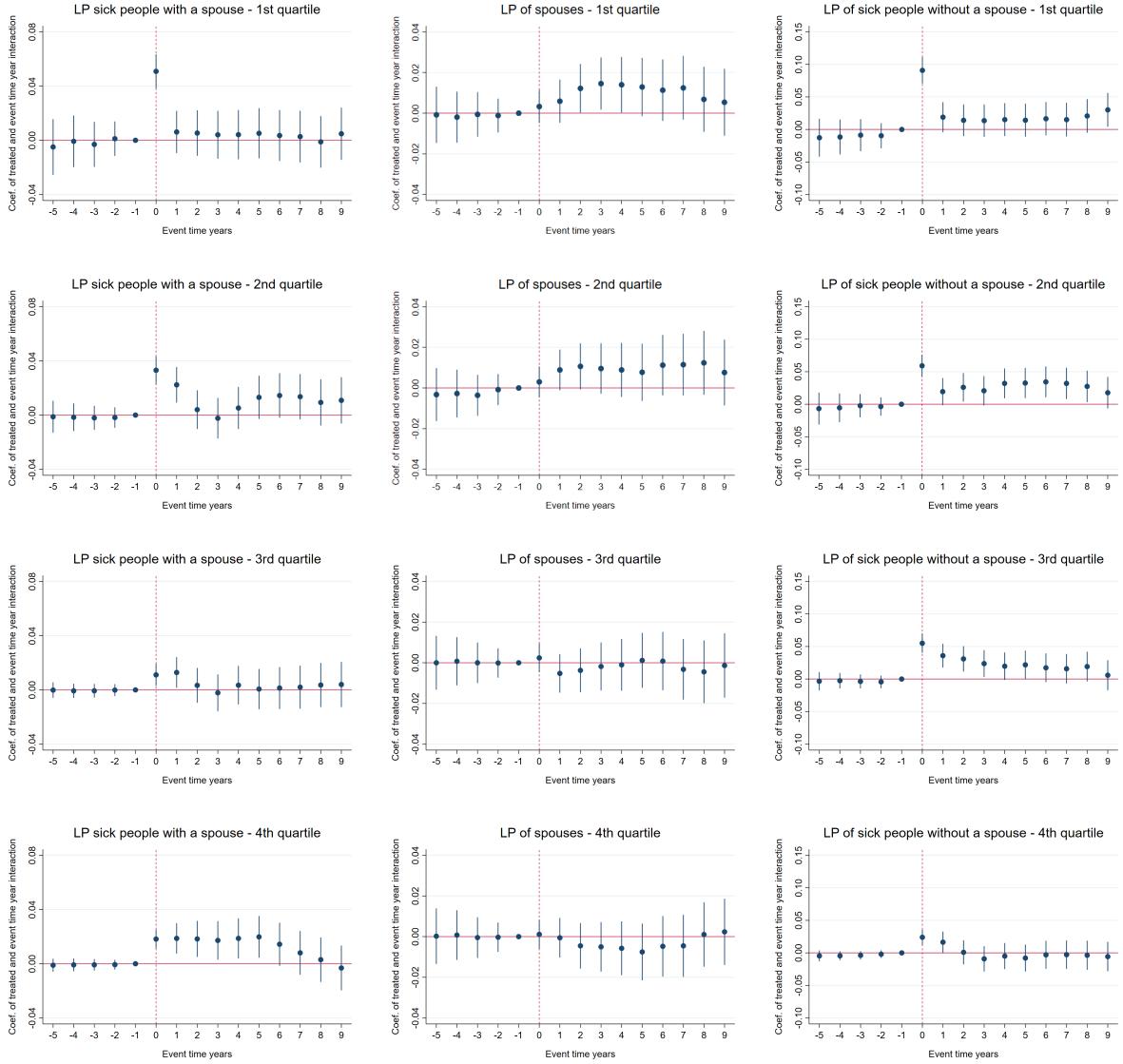


Figure 7: Estimated treatment effects in each of the five years before reporting sick and in each of the first ten years after reporting sick, with 95 percent confidence intervals for sick individuals, their spouses, and sick individuals without spouses, by earnings quartile. Observed differences between treatment and control individuals before reporting sick are controlled for using entropy balancing.

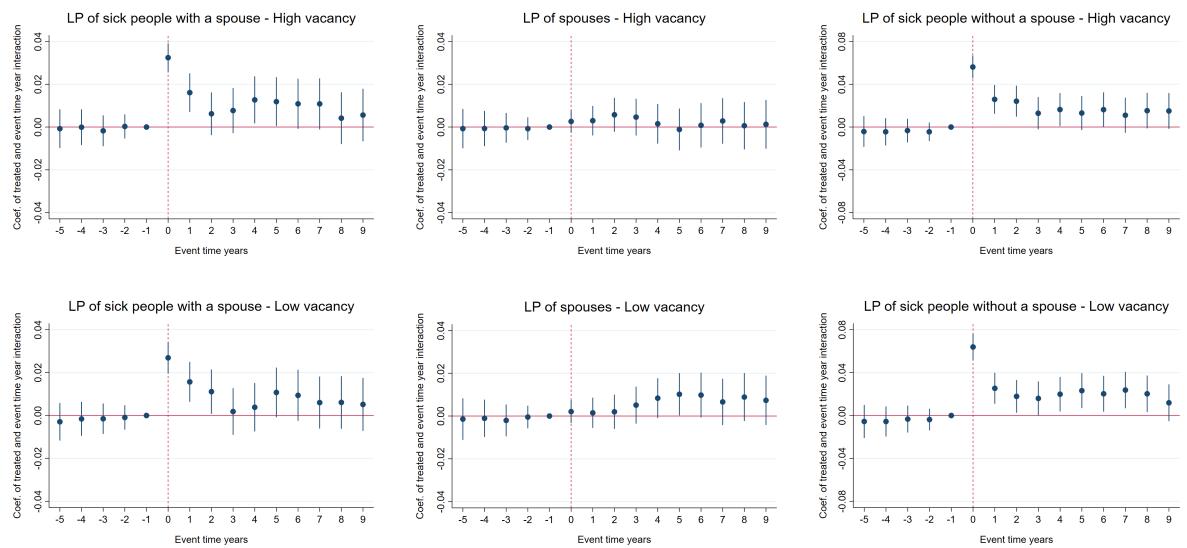


Figure 8: Estimated treatment effects in each of the five years before reporting sick and in each of the first ten years after reporting sick, with 95 percent confidence intervals for sick individuals, their spouses, and sick individuals without spouses, by sectoral vacancy rate above (high) or below (low) the average vacancy rate in all sectors. Observed differences between treatment and control individuals before reporting sick are controlled for using entropy balancing.

Appendix B 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 reported sick – people who reported sick right before 1 January 2004 are insured under the WAO scheme, while people who reported 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 cut-off 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 report 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 (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 cut-off. 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 9 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 2 and 5. 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 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.

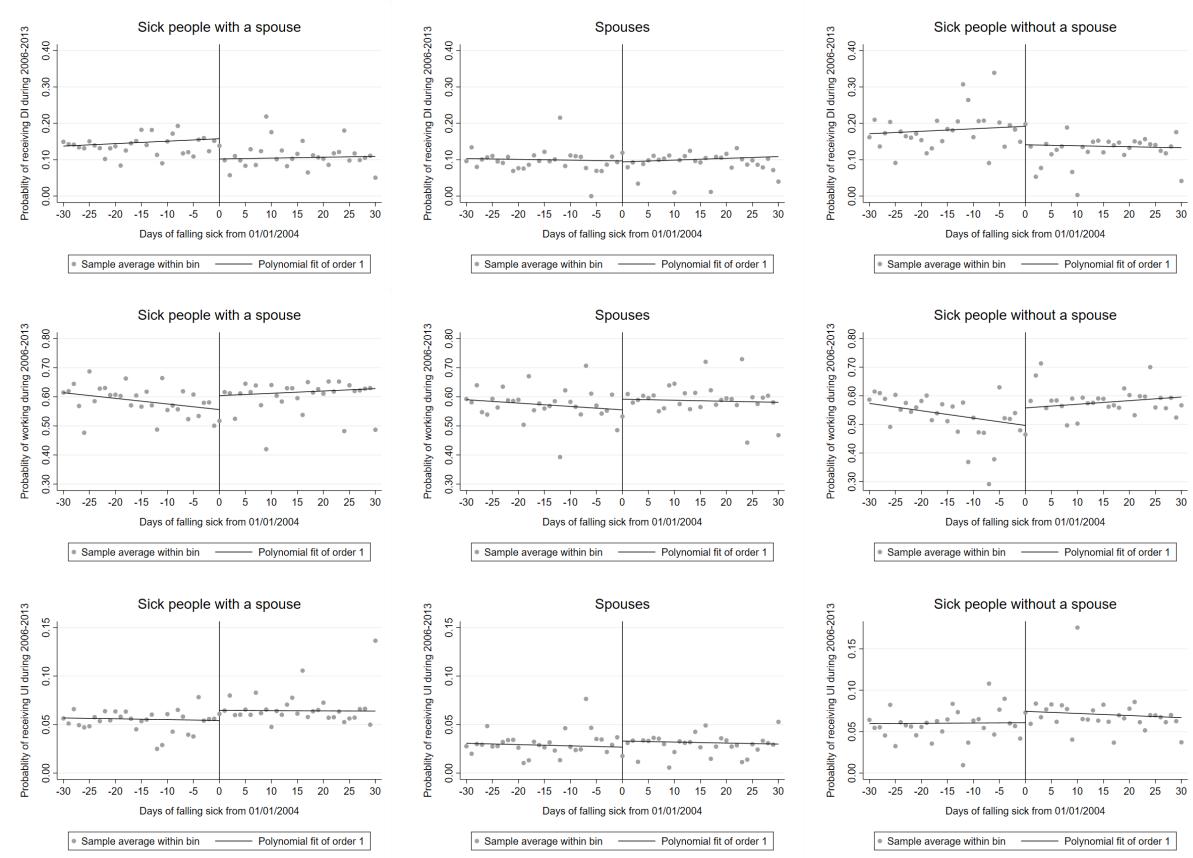


Figure 9: Local linear fit on the two sides of the cut-off. Standard errors are clustered at the individual level. All subfigures exclude data for the first two years after reporting sick.

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 individual with a spouse	Spouse	Sick individual without a spouse
DI 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)
UI receipt	0.010** (0.004)	0.004 (0.003)	0.014*** (0.006)
ln DI	-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 UI	0.076** (0.030)	0.025 (0.021)	0.103** (0.043)
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 C 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 check to which extent the reform affected cohabitation status during post-treatment.

The left panel of Figure 10 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 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 B. 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 WAO and WIA differ in their cohabitation status during the ten years after reporting sick. The right panel of Figure 10 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.¹⁵ This shows that the reform did not cause couples to dissolve their cohabitation.

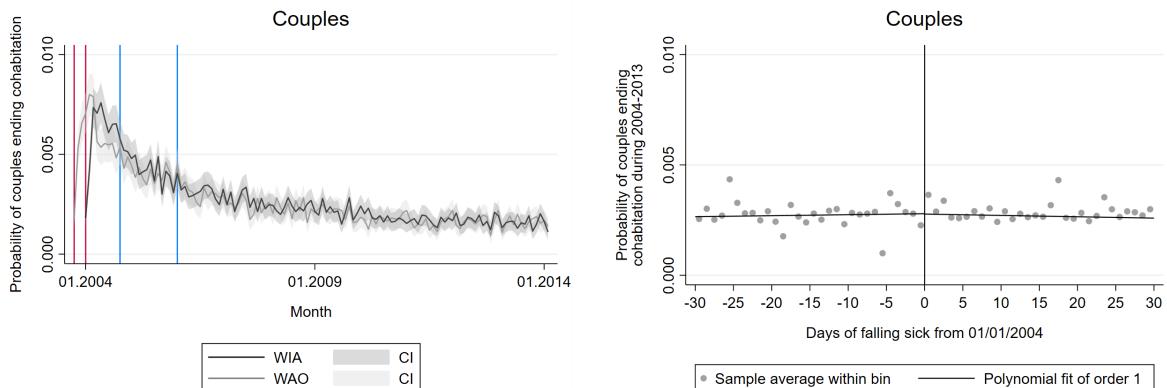


Figure 10: Left panel: Probability of couples ending cohabitation after one spouse falls sick. Vertical lines mark the first instance sick partners could become entitled to the sickness and disability benefits in the WAO and WIA schemes. Red lines correspond to 1 October 2003 and 1 January 2004 for the WAO and WIA groups, respectively. Blue lines correspond to 1 October 2004 and 1 January 2006 for the WAO and WIA groups, respectively. Right panel: 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.

¹⁵The 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.