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How do spouses respond when disability benefits are lost?

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CONTENTS

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Summary

The Netherlands reformed its disability insurance system in 2006. The reform increased the sickness benefits period from 12 to 24 months, access to DI benefits became more difficult, and the benefits often became less generous in the long run. Using administrative data and comparing individuals who reported sick just before and just after the reform, we evaluate the impact of the disability insurance reform on the labor participation of sick individuals and their spouses and on their earnings, unemployment benefits, and social assistance benefits.

We find that spouses of people who fell sick just after the reform work, on average, 0.9 percentage points more often than spouses of people who fell sick just before the reform. The effect of the reform on the sick people themselves is 1.1 percentage points. These effects are persistent during the ten years following the start of sickness.

We find substantial heterogeneity with respect to the employment contract that the sick individual had when falling sick. Sick persons with a temporary contract and those who are unemployed often do not manage to get back to work, but their spouses compensate for the income loss by increasing their own labor participation. Those with a permanent contract, however, manage to respond to the reform by increasing their earnings and hardly rely on unemployment insurance, whereas their partners do not respond to the reform. These differences support the notion that partners respond to the reform to compensate for a loss in household income. Accordingly, sick people without a spouse increase their participation in the labor market more than sick people with a spouse since they cannot rely on the labor supply of a spouse. These results imply that spill-over effects on the spouse must be considered for a complete evaluation of the reform.

Samenvatting

Nederland heeft in 2006 de arbeidsongeschiktheidsverzekering hervormd: de ziektewet werd verlengd van 12 naar 24 maanden, voorwaarden voor een uitkering werden strikter en de uitkering op lange termijn werd vaak lager. Aan de hand van administratieve gegevens en door personen te vergelijken die kort voor en kort na de hervorming ziek werden, evalueren we de invloed van deze hervorming op arbeidsparticipatie, arbeidsinkomen en werkloosheids- en bijstandsuitkeringen van degenen die ziek werden en hun partners.

We stellen vast dat partners van degenen die kort na de hervorming ziek werden, gemiddeld 0,9 procentpunt vaker werken dan partners van degenen die net voor de hervorming ziek werden. Het effect van de hervorming op degenen die ziek worden is 1,1 procentpunt. Deze effecten houden aan gedurende de tien jaar nadat individuen ziek werden.

We stellen verder vast dat de effecten van de hervorming sterk afhangen van het type arbeidsovereenkomst wanneer iemand in de ziektewet komt. Individuen met een tijdelijk contract en individuen die werkloos zijn wanneer ze in de ziektewet komen, slagen er meestal niet in om weer aan het werk te komen, en hun partners compenseren het verlies aan arbeidsinkomen door te gaan werken. Degenen die vanuit een baan met een vast contract in de ziektewet komen, gaan vaker zelf weer aan het werk en er is geen effect op de participatie van hun partners. Deze verschillen ondersteunen daarom het idee dat partners reageren op de hervorming om verlies aan gezinsinkomen te compenseren. Zieke mensen zonder partner verhogen dan ook hun arbeidsparticipatie en -inkomen meer dan zieke mensen met partner; zij kunnen immers niet vertrouwen op de reactie van een partner. De resultaten impliceren dat de reactie van de partner van belang is voor een volledig beeld van de effecten van de hervorming.

1. Introduction

In the beginning of this century the Netherlands became one of the countries with the highest share of disabled workers in the insured population. In 2002, the total number of disability insurance (DI) recipients reached almost one million – about 11% of the insured population (Koning and Lindeboom, 2015). To reduce the number of DI recipients and stimulate work resumption, successive governments implemented several reforms of the DI system. In 2006, the Work and Income (Capacity for Work) Act (*Wet werk en inkomen naar arbeidsvermogen*, WIA) came into effect as the final element of these reforms.

A transitional scheme was implemented before the old system of the Invalidity Insurance Act (*Wet op de arbeidsongeschiktheidsverzekering*, WAO) was entirely replaced by the WIA. This transitional scheme preserved the main features of WAO, except that the criteria to enter the DI scheme were made stricter. WIA introduced major changes in both the DI scheme and the sickness insurance (SI) scheme that precedes it. The maximum duration of SI was extended from one to two years. This implies that the incentive for employers to facilitate work resumption was increased, as they must compensate the employee for wage loss during two years instead of one. For the DI scheme, the WIA introduced stricter entrance criteria, but also stronger financial incentives for work resumption, both for employees and employers.

Kantarcı et al. (2019) analyzed the effects of the WIA on labor participation and benefit receipt from DI and alternative benefit programs by comparing people who fell sick before (insured under WAO) with people who fell sick after the reform (insured under WIA). They showed that the reform reduced the probability of DI receipt by 5.8 percentage points during the ten years after the reform. To compensate for lost DI benefits, individuals increased their labor participation by 1.8 percentage points, but also increased their claims from unemployment insurance (UI) by 1.4 percentage points. Therefore, increases in labor participation and UI receipt compensated only in part for the decrease in DI receipt.

The focus of this paper is on whether labor participation by a spouse also compensates for lost DI benefits. To estimate the impact of the reform on spousal labor supply, we compare the labor participation of spouses whose partners fell sick before and after the reform. We use administrative data on individuals who fell sick in the last quarter of 2003 (potential participants in the transitional WAO scheme) or the first quarter of 2004 (falling under the stricter WIA scheme). We link the sick workers in the data to their spouses (married or registered as cohabiting) to analyze whether the DI reform had spill-over effects on the spouse.

We find that among sick individuals with a spouse, labor participation rose due to the DI reform by 1.1 percentage points, while labor participation of their spouses rose by 0.9 percentage points.[1] Furthermore, we show that the reform increased the probability of working by 0.4 percentage points more among sick individuals without a spouse than for sick individuals with a spouse. This suggests that the negative income effect of the DI reform is shared by partners in a couple: spousal labor supply is a substitute for the labor supply of sick persons when they face a stricter disability benefit regime.

Two earlier studies analyzed the impact of stricter DI rules on spousal labor supply in the Netherlands. Borghans et al. (2014) studied the impact of reassessment of current recipients and new applicants younger than 45 years of age, based on new eligibility criteria for DI that came into effect in 1993. They found that people who were affected by such reassessment were able to fully offset the loss of DI benefits with higher earnings and income from alternative social support programs, but they found no significant effect on spousal earnings. Garcia-Mandicó et al. (2020) analyzed the earnings responses to a reassessment of earnings capacity under more stringent rules in 2004, and found that reassessment of female recipients led their husbands to significantly increase their earnings. These studies suggest that changes in DI rules affect spousal labor supply to some extent, but the evidence is limited and the estimates are not precise.

The 2006 DI reform differs from earlier DI reforms in several important respects. First, the WIA provides disabled people with unprecedented strong incentives to utilize their remaining earning capacity, which could limit the need for an increase in spousal labor supply. Second, the WIA affected all new applicants but not existing DI recipients. Existing recipients who are denied DI benefits later on might behave differently from new applicants who are denied DI benefits. Third, the reforms analyzed by Borghans et al. and Garcia-Mandicó et al. only affected people younger than 45 years, while the WIA applies to all age groups.

Our findings imply that, for a complete evaluation of the disability insurance reform, it is important to consider spill-over effects on spouses, both for the effect on their labor participation and for the effect on income adequacy at household

1 Several reasons explain the differing labor participation effects found by Kantarcı et al. (2019) and this study. First, in our study we analyze labor supply responses, distinguishing between sick people with and without spouses. Second, as explained in Section 3, we discard data for the first two years after individuals fall sick in most of our analysis. Third, we compare sick individuals who participate in the WIA and transitional WAO schemes, whereas Kantarcı et al. (2019) compare sick individuals who participate in the WIA and WAO schemes.

level. Moreover, the oldest group of workers as well as their spouses are less likely to respond to the reform by resuming work. Changing disability access does not seem to be an effective way to stimulate paid work for this group, and this seems to become even more the case with the rising state pension age. This seems to be an additional argument for special state or occupational pension arrangements for professions that involve high disability risks at an older age (physically and perhaps also mentally demanding professions).

The remainder of 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 this with the effects on singles. Section 8 contains conclusions, including for public policy. Appendices present robustness checks.

2. Disability insurance in the Netherlands and the 2006 reform

The Disability Insurance Act (WAO) came into effect in 1967 to insure against loss of earnings due to long-term disability. Since major amendments to the Act in 1993, its main features were preserved until 2006. The WAO consisted of two schemes. Individuals earning wages or receiving UI benefits, 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 maximum duration of one year. The employer was obliged to pay 70% of the former wage during this period; most employers paid the full amount. In the absence of an employer, the "Sickness benefit" was paid by the Employee Insurance Agency (UWV). When the sickness scheme expired after one year, individuals were admitted to the disability scheme if the disability degree was at least 15%. They were first entitled to the "wage-loss benefit" and, upon its expiration, to the less generous "follow-up benefit". The wage-loss benefit replaced 70% of the former wage multiplied by the disability degree. The duration of the benefit depended on the individual's age, with a maximum of 6 years. The Follow-up benefit paid 70% of the minimum wage multiplied by the disability grade and an additional amount that depended on individual's former wage and the age at which the individual became entitled to the benefit. The benefit was paid as long as the individual remained disabled, but it expired upon reaching the state pension age.

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

The WIA came into effect on January 1, 2006 for individuals who fell sick from January 1, 2004 onwards. It introduced major changes in both the sickness and disability schemes, facilitating work resumption and reducing the yearly inflow rate into the disability scheme to 0.5% of the insured working population during the first six years after its introduction (Koning and Lindeboom, 2015). The maximum duration of the sickness scheme was extended from one to two years. The employer is now obliged to compensate the employee for 70% of the wage loss during the two-year period of the scheme, creating a strong incentive for the employer to facilitate work

Figure 1: Changes in the sickness and disability insurance schemes introduced by the transitional WAO and WIA reforms.

		WAO	Transitional WAO	WIA
Sickness insurance	Duration	1 year	1 year	2 years
Disability insurance	Minimum disability degree to become eligible	15%	15%	35%
	Definition of what work could still be done	Narrow definition	Broader definition	Broader definition
	Financial incentives for work resumption for employees	Limited	Limited	Strong incentives for partially disabled and workers with potential to recover
	Financial incentives for work resumption for employers	Experience rating for 5 years applied to all workers	Experience rating for 5 years applied to all workers	Experience rating for 10 years applied to workers with a permanent contract who are partially or fully disabled with potential to recover

resumption.[2] Figure 1 provides an overview of the main changes in the sickness and disability insurance schemes introduced by the transitional WAO and WIA reforms. The WIA maintained the stricter eligibility criteria of the transitional WAO scheme, with a broader definition of what work can still be performed. In addition, the minimum degree of disability required to enter the scheme was raised from 15 to 35 percent – workers with limited disability are expected to resume working with adaptations or to apply for UI. Moreover, the scheme introduced a distinction between full and partial disability. If the wage loss is more than 80% and there is no potential for any recovery, the worker is admitted to the Full Invalidity Benefit Regulation (IVA). If the wage loss is more than 35% and less than 80%, or if the wage loss is more than 80% but there is still potential for recovery, the worker is insured under the Return to Work Regulation (WGA). First, the worker is entitled to the "wage-related benefit" with a UI component. Later on, the worker is entitled to one of two types of benefits: either the "wage-supplement benefit", if the worker utilizes at least 50% of his or her remaining earning capacity, or the less generous "follow-up benefit". All in all, compared to the WAO, the WIA provides stronger incentives to use one's remaining work capacity during the disability period.

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

Finally, the experience rating for employers was extended from 5 to 10 years, and it applied to partially disabled workers instead of all disabled workers, creating more effective incentives to reintegrate workers on sickness or DI benefits as quickly as possible. The experience rating was limited to permanent work contracts until 2013; it was extended to temporary contracts afterwards.

3. Data

We use unique administrative data from the UWV on all individuals who reported sick in the fourth quarter of 2003 or the first quarter of 2004, who therefore became eligible to either the transitional WAO or the WIA scheme.[3] We observe the starting and ending dates of their sickness and their gender and date of birth. They either earn a wage or receive UI at the time they report sick – other groups cannot enter the sickness scheme. For wage earners, we observe whether they hold a permanent contract, a temporary contract, or a contract through a temporary work agency at the time they report sick. We link these individuals to administrative data on themselves and their partners (married or cohabiting) from Statistics Netherlands (CBS), with monthly information on wages and benefits. The benefits include DI, UI, and social assistance. These data extend from January 1999 to February 2014, allowing us to study the impact of the DI reform over a period of 15 years.

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

Based on the available data on wages and social security benefits, we define the following outcome variables: dummies that indicate labor participation, DI receipt, UI receipt, and social assistance receipt, and the monthly amounts of wages, DI benefits, UI benefits, and social assistance. We transform earnings and benefit amounts as the natural logarithm of the amount plus 1, accounting for the skewed distribution and the value zero. We analyze these outcomes for sick people and their spouses,

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

who constitute our main analysis. In Section 7 we also include individuals without a spouse who fell sick just before and just after the reform. We compare the results for this group to those for couples in order to back up the interpretation of our main findings.

While the administrative data have the important advantages of covering the complete population of those who reported sick and of avoiding errors in self-reported survey answers, they also have some limitations. For example, during participation in the sickness scheme, the observed wage is the sum of actual 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. Another issue is the fact that in cases where assessment of eligibility is time-consuming, advance payments are made, which in some cases must be repaid. These advance payments cannot be distinguished from final DI benefits; DI benefit payments may thus be overestimated. This is problematic since the outbreak of Covid-19 but not so much during our observation window (until 2014).[4]

4 See https://www.uwv.nl/overuwv/Images/ukv-2021-4-volumeontwikkelingen-voorjaar-2021.pdf.

4. Time trends of outcome variables

Figures 2a and 2b show the labor participation rates and fractions of DI, UI, and social assistance recipients in the control and treatment groups over the observation period.[5] For the individuals who report sick, the inflow into the DI scheme increases sharply when the treatment and control groups become eligible to apply for DI benefits, and it continues to increase during the remaining years of the observation period. The treatment group is much less likely to become entitled to DI benefits, and the sizable difference between the two groups remains stable throughout the observation period. This shows that the reform effectively limited the access to the DI scheme. For the spouses of sick people, DI receipt is stable during the reform period and is not affected by the reform.

For the individuals who report sick, the probability of working shows a strong time trend that is common to both groups. It increases until the date on which individuals report sick, reflecting the fact that individuals can enter the sickness scheme only if they work or receive UI. Prior to this, they can have another labor force status. The probability of working drops sharply during the first several years of sickness and continues to fall throughout the remaining years. The difference between control and treatment groups is small and insignificant before individuals fall sick, but it is notable and significant after they fall sick, suggesting that the reform led to increased labor participation of those who fell sick. For spouses, the probability of working shows a less pronounced decreasing pattern. The difference between control and treatment groups is not significant either before or after treatment, but it is larger post-treatment 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. The difference between the control and treatment groups is sizable and statistically significant during the disability period, suggesting that the DI reform led to increased UI use among those who reported sick. UI use

5 Similar figures for wages and benefit amounts (not shown) lead to quite similar patterns.

Figure 2a: Probability of DI receipt, employment, and UI receipt for control and treatment groups by calendar month, for sick individuals (left) and their spouses (right). Vertical lines mark the first instance that sick partners can be entitled to the sickness (red) and disability (blue) benefits in the two schemes.

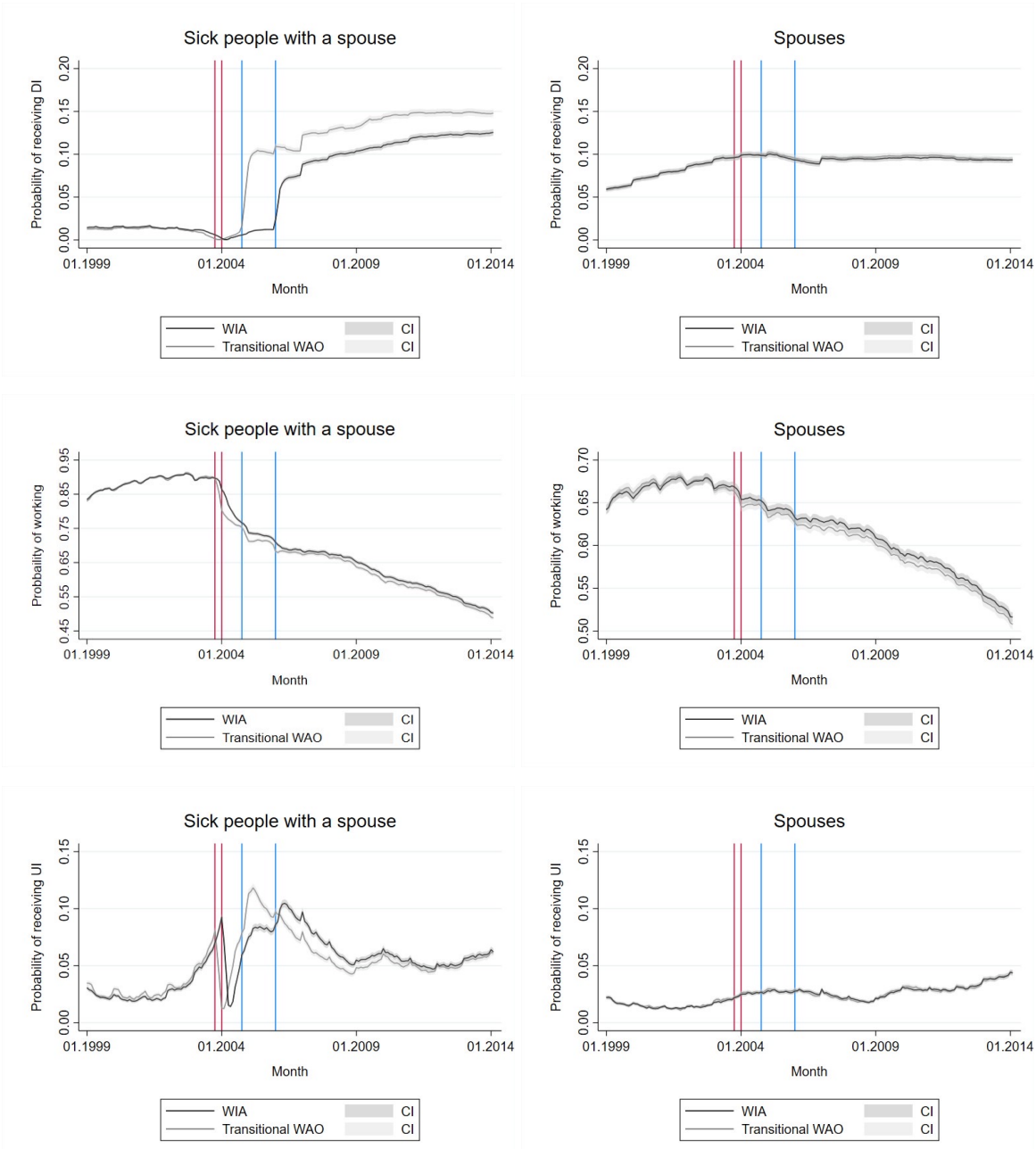
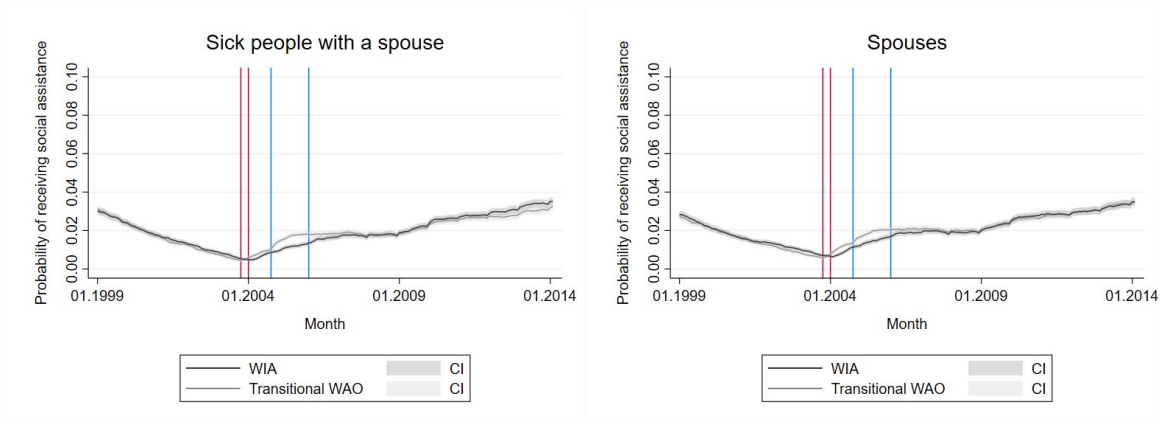


Figure 2b: Probability of social assistance receipt for control and treatment groups by calendar month; sick individuals (left) and their spouses (right). Vertical lines mark the first instance that sick partners can be entitled to the sickness (red) and disability (blue) benefits in the two schemes.



among the spouses is fairly constant over time. The difference between control and treatment groups is insignificant, both pre- and post-treatment.

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

5. Empirical model

To estimate the causal effect of the reform on the labor market outcomes of sick individuals as well as their spouses, we take a difference-in-differences (DiD) approach. The treatment group consists of persons who fell sick in the first quarter of 2004 and fall under the new WIA regime and their spouses; the control group consists of persons who fell sick in the fourth quarter of 2003 and are subject to the less restrictive transitional WAO regime with their spouses. The estimated effect of the reform is the difference between change in the outcome variable considered from before the individual fell sick until some point in time after falling sick.

The DiD method is implemented using linear regression. The regression includes an interaction term that is the product of an indicator of the group affected by the reform and a period indicator corresponding to the period after the individual fell sick. The coefficient on this interaction variable is the estimate of the reform effect. The set of controls includes a set of calendar month dummies for the observation period from January 1999 until January 2014, which capture the usual (common) time trend. We also allow for time-invariant individual fixed effects that are potentially correlated with the control variables. The error term of the regression is assumed to be uncorrelated with all explanatory variables.

The pre-treatment period is chosen as the base period. In the first model, we assume the reform effect is constant over time, and the entire period from 3 until 10 years after falling sick is used to estimate one common reform effect, namely the coefficient on the interaction of the treatment group with a dummy that indicates the entire post-treatment period.[6] In the second model, we disentangle the effects of the reform in the short and long run, and consider separate dummies indicating each of the ten years in the post-treatment period. The treatment effect after a given number of years since the start of sickness is the coefficient on the interaction term of the treatment dummy and the corresponding event time dummy.

The main identifying assumption is that the change over time would have been the same for the two groups had there been no reform (the common trend assumption). Furthermore, it is assumed that individuals do not select themselves into the transitional WAO or WIA schemes from the time that the reform is announced. Finally, it is assumed that couples do not dissolve their cohabitation during post-treatment due to the reform, as otherwise this might confound the estimated effect of the reform. A discussion of the validity of these assumptions is provided in Appendices A, B and C, respectively.

6 As explained in Section 3, we estimate the overall effect excluding the available data for the first 24 months of the sickness period.

6. The effects of the reform among sick individuals and their spouses

Table 1 presents the baseline DiD estimates of the effects of the reform on labor participation and benefit receipt. For sick individuals with a spouse, the reform decreased the probability of DI receipt by 3.2 percentage points on average during the post-treatment period (excluding the first two years). It increased the probability of working by 1.1 percentage points and of UI receipt by the same amount. These effects are all sizable and significant, whereas the effect on social assistance receipt is small and insignificant. The effects on the four participation outcomes for sick individuals are also jointly significant (p-value 0.000).

The reform induced the spouses of the sick individuals to increase their labor participation by 0.9 percentage points, even though it did not have a significant effect on the spouse's benefit receipt. Still, the four effects on the spouse's dummies on benefit receipt and employment are also jointly significant (p-value 0.022). The effect on the

Table 1: Estimated effects of the WIA reform: Sick individuals and their spouses

	Sick individual	Spouse
Disability insurance receipt	-0.032***	-0.002
	(0.002)	(0.002)
Labour participation	0.011***	0.009***
	(0.003)	(0.003)
Unemployment insurance receipt	0.011***	0.001
	(0.001)	(0.001)
Social assistance receipt	-0.000	-0.001
	(0.001)	(0.001)
In Disability insurance	-0.211***	-0.016
	(0.018)	(0.012)
In Wage	0.088***	0.065***
	(0.003)	(0.023)
In Unemployment insurance	0.076***	0.004
	(0.009)	(0.006)
In Social assistance	-0.001	-0.009
	(0.007)	(0.007)
Observations	8,431,218	
Individuals	55,106	

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

spouse's employment suggests that the spouse responds to the loss of DI by increasing his or her labor market activity in order to maintain household income.

The lower panel of Table 1 shows the DiD estimates of the reform effects on wages and benefits received per month, in response to a strong drop in disability income. As we measure these income amounts in logarithm, the estimates indicate that the reform led to increased monthly earnings by approximately 8.8% and unemployment benefits by approximately 7.6%. These effects are individually significant, with only the effect on social assistance benefits being small and insignificant; the four effects are also jointly significant (p-value 0.000). The effects on the spouse's income are also jointly significant (p-value 0.032). The main finding is a significant increase in earnings by approximately 6.5%; the effects on the spouse's benefit amounts are small and insignificant.

Dynamic effects

Figures 3a and 3b present estimates of the reform effects for ten years of the post-treatment period. It shows that the effect of the reform on labor participation by the spouse is close to 1 percentage point and significant during the entire post-treatment period. We do not reject equality of the effects in all years (see the note below the figure). This confirms that the effect of the reform on labor supply of the spouse is persistent. Also in line with the exploratory analysis in Section 4, the reform has no effect on the spouse's UI receipt. It has a significant negative effect on social assistance benefits receipt during the sickness period but also during year 3, when the WIA group becomes eligible to apply for DI.[7]

For the sick individuals themselves, the reform lowers DI receipt by about 3 percentage points during post-treatment from the third year after reporting sick, when both the treatment and control groups become eligible for DI. The effect of the reform on labor participation is particularly large during the first year of the sickness scheme. Interpreting the effects in the first two years is difficult, however, due to the measurement issue explained in Section 3. The effect on labor participation then falls to about 1 percentage point and remains fairly stable and statistically significant from the third year of post-treatment onwards. For UI receipt, the large negative effect in the second year of the sickness scheme is due to the fact that individuals insured under the WIA remain entitled to sickness wage if there is an employer, or to the sickness benefit if there is no employer. From the third year of the post-treatment period and onwards,

7 One of the changes in the WIA was that entitlement to the Supplementary Benefits Act (*Toeslagenwet*) was extended. As such, under the WIA some people did not have to turn to social assistance anymore. This may be caused by this institutional change.

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

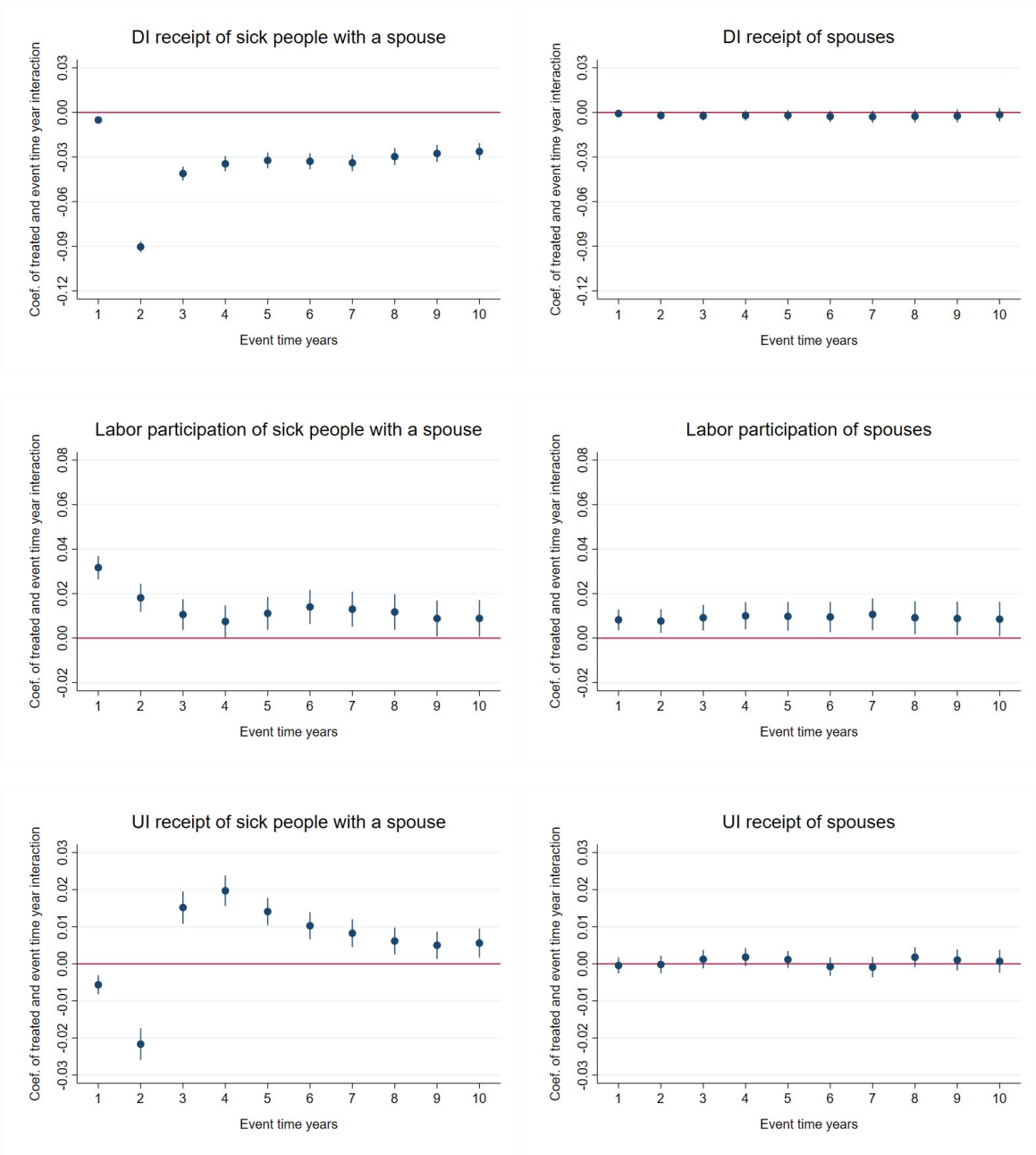
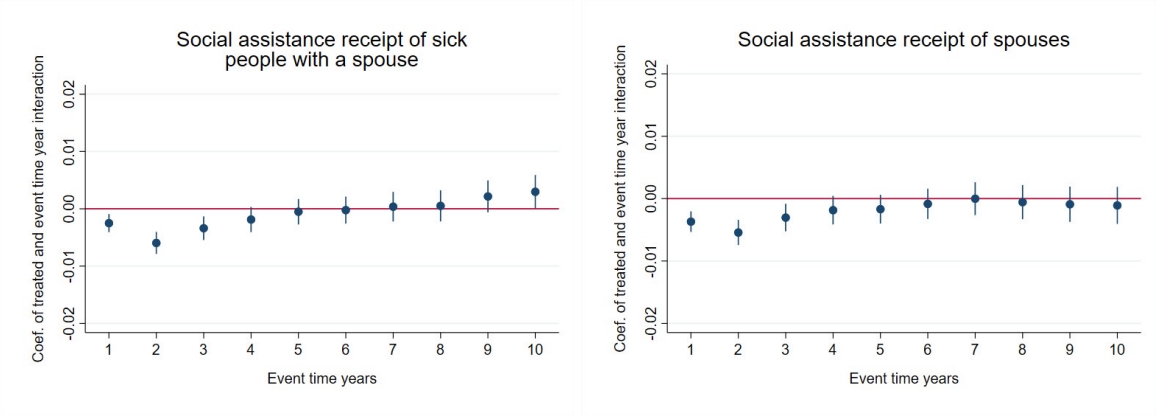


Figure 3b: Estimated treatment effects in each of the first ten years after reporting sick, with 95% confidence intervals. Note: The F-statistics (p-value in parentheses) for the test on equality of treatment effects over the years for sick individuals and their spouses, respectively, are 5.829 (0.000)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



however, the reform has a positive effect on UI receipt. It decreases over time but remains significant at the 1% level. In line with the exploratory analysis in Section 4, the effect of the reform on social assistance benefits is negative and significant only during the first three years after falling sick.

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

Heterogeneous effects

We analyze whether the DI reform affected individuals with different gender, age, or work statuses when falling sick, in different ways.[8] We find hardly any differences between men and women (details available upon request). This result differs substantially from Garcia-Mandicó et al. (2020), who, for the DI reform implemented in 2004, found an effect on husbands but not on wives.

Table 2 presents the reform effects by age of the sick individuals when they report sick. The effects on labor participation and earnings are much larger for the 45-54 age group than for both the younger and older age groups. Instead, the oldest group of sick individuals relies more often on unemployment insurance to compensate for lost disability benefits, suggesting that they find it hard to retain their job or to find a new job. Also for spouses, the labor participation and wage effect is largest for the middle

8 The data do not allow for an analysis by sickness type.

Table 2: Estimated effects of the WIA reform by age when reporting sick

		Fell sick before age 45	Fell sick between ages 45 and 54	Fell sick after age 54
Labor participation	Sick individual with a spouse	0.009**	0.018***	0.011
		(0.004)	(0.006)	(0.008)
	Spouse	0.007*	0.015***	0.010
		(0.004)	(0.006)	(0.008)
Unemp. ins. receipt	Sick individual with a spouse	0.009***	0.009***	0.019***
		(0.001)	(0.003)	(0.005)
	Spouse	0.001	−0.000	−0.000
		(0.001)	(0.001)	(0.003)
Social assistance receipt	Sick individual with a spouse	0.000	−0.001	−0.000
		(0.002)	(0.002)	(0.002)
	Spouse	−0.001	−0.003	0.002
		(0.002)	(0.002)	(0.002)
In Wage	Sick individual with a spouse	0.072**	0.135***	0.108*
		(0.032)	(0.047)	(0.061)
	Spouse	0.044	0.109***	0.079
		(0.030)	(0.043)	(0.062)
In Unemp. ins. benefit	Sick individual with a spouse	0.065***	0.062***	0.141***
		(0.010)	(0.018)	(0.035)
	Spouse	0.009	−0.001	−0.003
		(0.008)	(0.012)	(0.018)
In Social assistance benefit	Sick individual with a spouse	0.004	−0.008	−0.001
		(0.010)	(0.011)	(0.012)
	Spouse	−0.009	−0.020*	0.010
		(0.011)	(0.012)	(0.013)
Observations		4,744,071	2,442,645	1,244,502
Individuals		31,007	15,965	8,134

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

age group. There is hardly any effect on their unemployment or social assistance benefits.

Incentivizing employers to increase their employees' labor market participation has been the key element of Dutch labor market reforms over the years. Therefore, it is important to distinguish between employees with and without employers. We analyze the effects of the reform on sick individuals who are wage earners with a permanent or temporary contract, and on those who were unemployed at the time they reported sick. For employees with a temporary contract, employer incentives last

only during the term of the contract, while for employees of temporary work agencies there are no employer incentives during sickness. Unemployed individuals face no employer incentives, at least when they report sick. Employees with a permanent contract, however, have an employer and are fully incentivized to return to work. Moreover, this group is incentivized by their employers due to the experience rating that applied to permanent contracts until 2013 (Section 2), the second-last year of our observation period.

Table 3 presents the results by work status of sick individuals at the time when they report sick. DI receipt decreases for all groups, but it decreases more for the unemployed. These individuals have no employer who could reintegrate them into jobs. Furthermore, during their participation in the sickness scheme, job search requirements work less effectively for them. These factors increase their chances of remaining in the extended sickness scheme for a long time, even if they are not very sick. Therefore, this group may apply for DI but get rejected more often than other groups.

For sick individuals, the reform leads to higher labor participation among those with a permanent work contract but not among the unemployed. It seems that the work resumption incentives introduced by the reform induce employers to reintegrate their permanent employees, but that they prove ineffective if there is no employer. For the unemployed, the extended sickness period might lead to greater human capital loss or a stronger scarring effect, reducing the prospects of finding a job (Arulampalam, 2001; Arulampalam et al., 2001). Moreover, their incentive to resume working may be lower due to the reform since they can spend an additional year in the sickness scheme.

The spouses of sick individuals with a permanent work contract hardly respond to the reform, but spouses of sick individuals with a temporary work contract or without a job increase their labor participation and earnings significantly. These results suggest that, since sick individuals with a temporary or no work contract struggle to resume working, their spouses increase their labor participation and earnings to compensate for the lost disability benefits and lack of work income. On the other hand, sick individuals with a permanent work contract increase their own labor participation or earnings so there is less need for their spouses to compensate their loss of income.

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 upon receiving UI is larger than for those on a permanent contract – they have a less stable source of

Table 3: Estimated effects of the WIA reform by labor market status when reporting sick

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

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

labor income and earn lower wages and seek additional income from UI if access to DI benefits is limited by the reform. The spouses of sick individuals who are unemployed reduce their UI receipt by 0.4 percentage points, while spouses of sick individuals on a permanent contract increase their UI receipt by 0.2 percentage points in response to the reform. These responses are in line with the labor participation responses of spouses. When they increase their labor participation, they do not apply for UI.
The bottom panel of the table presents the results for earnings and benefit amounts from UI and social assistance. The signs and the statistical significance of the estimated effects of the reform on these outcomes are in line with the estimated effects on labor participation and benefit receipt in Table 3.

7. Comparison with the reform effects on sick people 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, if the sick partners find it difficult to respond themselves. In this section we analyze how the reform affected the labor participation of sick individuals who do not have a spouse. These people lack the opportunity to compensate their loss of household income through the labor supply of a spouse. This could induce them to increase their labor participation more than sick individuals with a partner when faced with the reform incentives to work. They could, however, also rely more often on unemployment insurance.

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

Table 4 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 1. The DiD estimates confirm that, on average during the post-treatment period (excluding the first two years), the reform increases the probability of working by 0.4 percentage points more among sick people without a spouse than for sick individuals who have a spouse.[9] Together with the finding (in Table 1) 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 the labor supply of sick individuals when they face a stricter disability benefit regime. Similarly, the reform reduces social assistance receipt more among sick people without a spouse than for those with a spouse. This is because an increased combined income from labor earnings and UI among sick individuals without a spouse reduces their eligibility for the means-tested social assistance benefit. The reform effects for wages earned and benefits received per month (in logarithm) for sick people with and without a spouse are in line with the reform effects on labor participation and benefit receipt. For example, while sick people without a spouse

9 The effects for individuals without a spouse in the top panel and the bottom panel are also jointly significant (p-values 0.000).

Figure 4a: Probability of DI receipt, work participation, and UI receipt for control and treatment groups over calendar months: for sick individuals with a spouse (left panel reproducing the left panel of Figures 2a and 2b) and without a spouse (right panel). Vertical lines mark the first instance that sick partners could become entitled to the sickness and disability benefits in the transitional WAO and WIA schemes. Red lines correspond to October 1, 2003 and January 1, 2004 for the transitional WAO and WIA groups, respectively. Blue lines correspond to October 1, 2004 and January 1, 2006 for the transitional WAO and WIA groups, respectively.

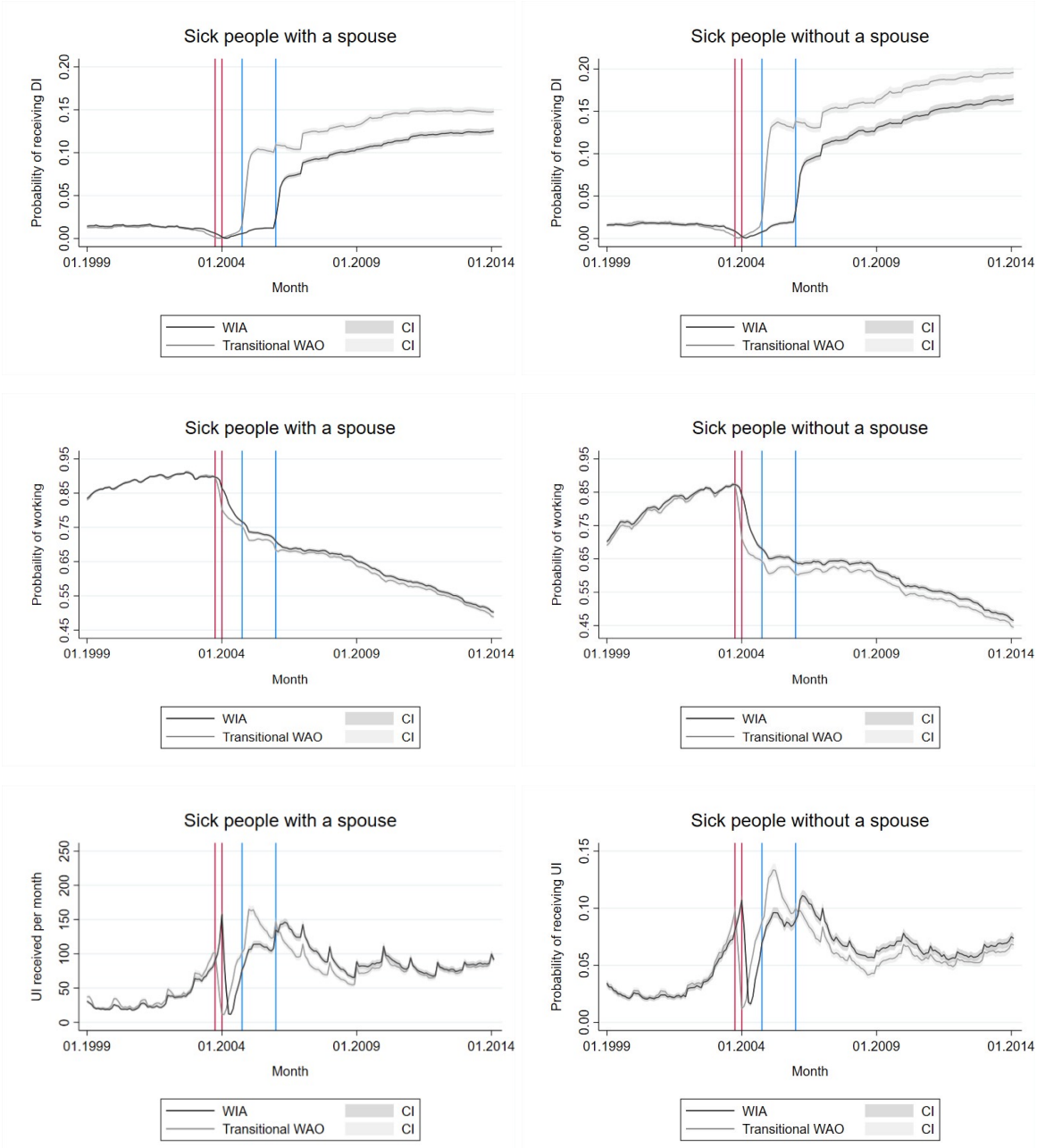
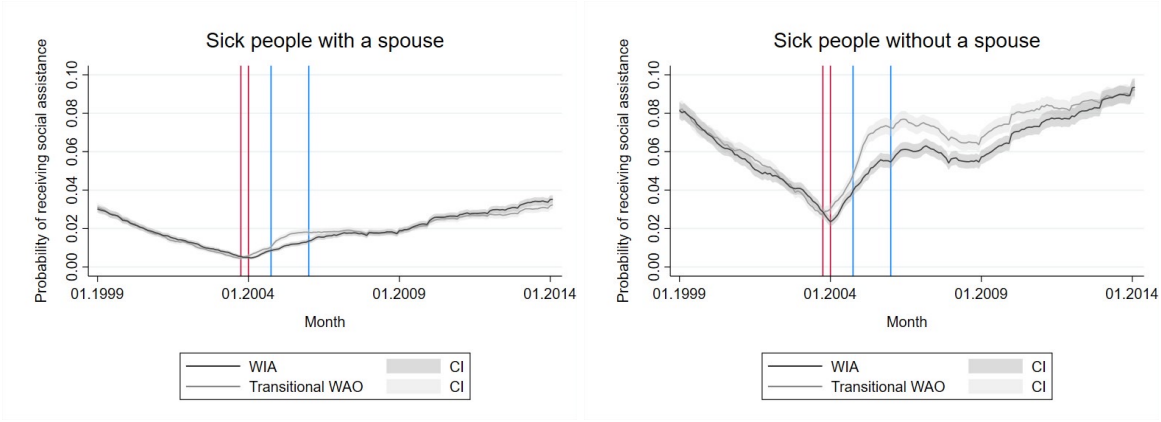


Figure 4b: Probability of social assistance receipt for control and treatment groups over calendar months: for sick individuals with a spouse (left panel reproducing the left panel of Figure 2a and 2b) and without a spouse (right panel). Vertical lines mark the first instance that sick partners could become entitled to the sickness and disability benefits in the transitional WAO and WIA schemes. Red lines correspond to October 1, 2003 and January 1, 2004 for the transitional WAO and WIA groups, respectively. Blue lines correspond to October 1, 2004 and January 1, 2006 for the transitional WAO and WIA groups, respectively.



increase their earnings by 10.7% in response to the reform, sick people with a spouse increase their earnings by the lower amount of 8.8%.

As in Section 6, we also consider reform effects that depend on the length of time since the individual fell sick. The time pattern of the effect on labor participation is similar to that for sick people with a spouse (not shown). The effect of the reform is large in the first year and then drops, but it remains statistically significant at about 1.5 percentage points during the next nine years, showing that the effect of the reform on participation of sick people without a spouse is persistent in the long run. The time pattern of the effect of the reform for UI receipt is also similar to that for sick people with a spouse and shows that the effect of the reform is persistent and remains statistically significant throughout the entire post-treatment period. The time pattern of the reform effect on social assistance receipt differs markedly between the two: larger and statistically more significant negative effects over a period of seven years post-treatment are found for those without a spouse. Similar time patterns of the effects of the reform over time are found for the wage and benefit amounts (not shown).

As before, we do not find notable differences between men and women (results not presented). With respect to the age of sick individuals at the time when they report sick, we find a notable difference between sick people with and without

Table 4: Estimated effects of the WIA reform: Individuals with and without spouse

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

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

spouses in how they respond to the reform (not shown). Compared to sick individuals with a spouse, those without a spouse increase their labor participation substantially more if they are younger than 45 years, while they do not respond if older. Instead, older sick individuals without a spouse increase unemployment insurance claims substantially more than older sick individuals with a spouse. The results on monthly earnings and unemployed benefits received are in line with this observation.

As in Section 6 for individuals with a spouse, we check the results by work status for individuals without a spouse. We find similar effects for sick people without and with a spouse if they held a permanent contract. There is a substantial difference, however, if they held a temporary contract, with singles increasing their labor participation by the substantial amount of 2.9 percentage points. In the preceding section, we showed that spouses of sick individuals with a temporary contract increased their labor participation by 2.9 percentage points in response to the reform. Here we find that, if there is no spouse who can compensate the loss of income, the sick person's own participation responds by the same amount. We observe a similar response

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

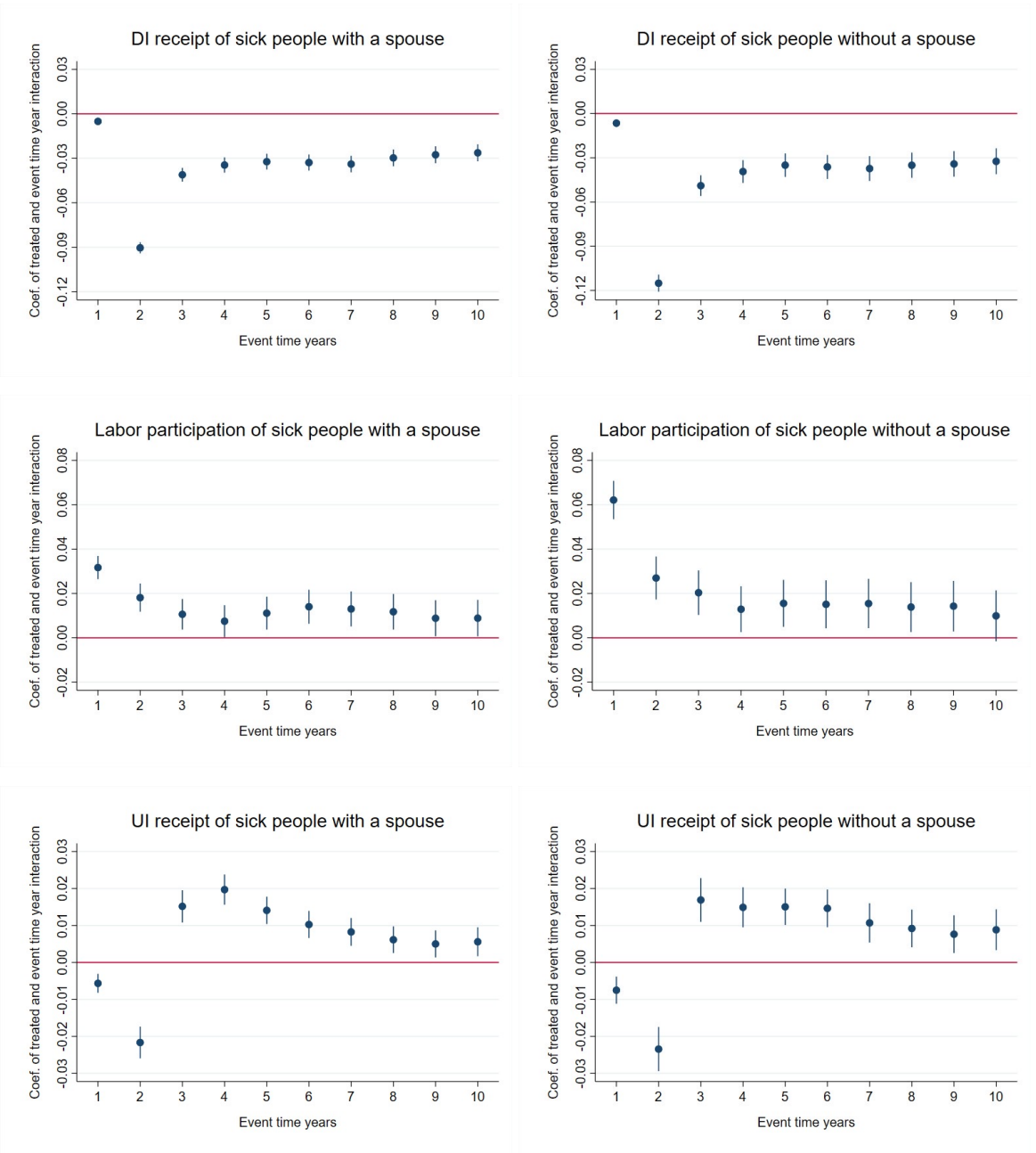
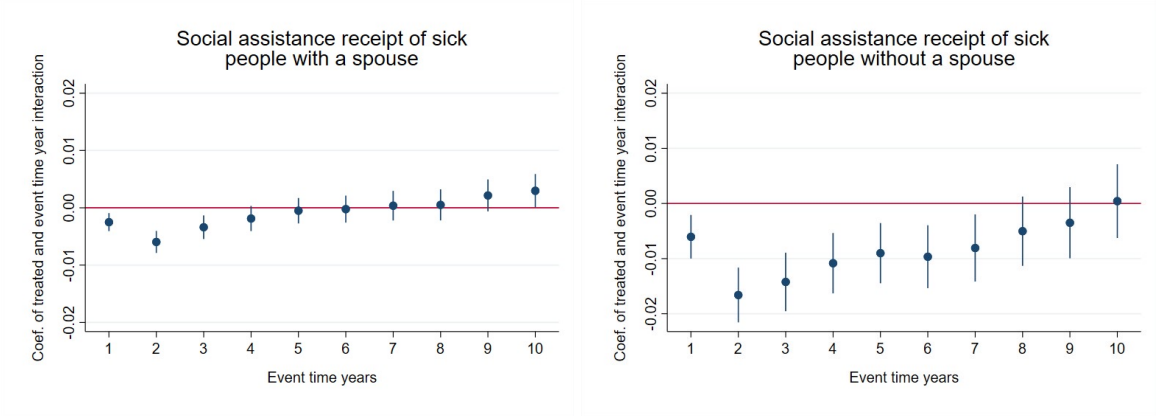


Figure 5b: Estimated treatment effects in each of the first ten years after falling sick, with 95% confidence intervals. Note: The F-statistics (p-value in parentheses) for the equality of the coefficients of treatment and event year interactions, for sick individuals with a spouse and without a spouse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



when sick people are unemployed, with singles showing no response while spouses increase their labor participation, offsetting the decrease in labor participation by their sick spouses. When sick people are on a permanent contract, those with a partner and those who are single both increase their labor participation by similar amounts, and there is no response by the spouse. Overall, we find that the response of singles is very close to the sum of the effects for the two partners for all three initial labor market states.

8. Conclusion

Early this century, the Netherlands registered one of the highest shares of individuals in the working population who claimed disability insurance (DI) benefits. In response, the Dutch government implemented social insurance reforms to reduce the number of DI claims and the burden on the national budget and to promote work resumption. After a series of smaller reforms, the DI scheme introduced in 1967 (*Wet op de arbeidsongeschiktheidsverzekering*, WAO) was replaced by a completely new scheme (*Wet werk en inkomen naar arbeidsvermogen*, WIA) in 2006. WIA increased the waiting period for disability benefits from one to two years, tightened the eligibility criteria, and introduced more targeted and often stronger incentives for employees and employers to stimulate work resumption.

In this study we focus on the impact of the 2006 reform on couples. Since couples can pool their income risks and jointly adjust their labor market status, the spouse's labor supply can be an important mechanism to counterbalance the loss of DI benefits caused by the reform. Using unique administrative data and a difference-in-differences identification strategy, we evaluate the impact of the WIA reform on the labor participation, receipt of unemployment benefits, receipt of social assistance benefits of sick individuals and their spouses, as well as on their earnings and benefit amounts. The policy relevance seems obvious: the effects on labor market position, earnings, and benefits are key to evaluation of the success of the reform. If the reform not only affects individuals reporting sick but also their spouses, a complete evaluation of the impact of the reform needs to take the effects on the spouses into account. Understanding the consequences of reforms in the design of DI not only helps in the development of tools to keep the DI system efficient and sustainable, but is also relevant for the system of occupational pensions, since both DI and early retirement with an early occupational pension are labor market exit routes for workers who cannot work until the (higher) state pension age.

We find that the reform had a positive effect on spousal labor market participation. In particular, the spouses of persons who fell sick under WIA work, on average, 0.9 percentage points more often than those of persons insured under the transitional WAO. The effect of the reform on the sick people themselves is 1.1 percentage points. These effects are persistent during the ten years following the start of sickness, not only for the individuals reporting sick but also for their spouses. These results confirm our finding that, for a complete policy evaluation of the WIA reform, the spillover effect on the spouse cannot be ignored.

We find substantial heterogeneity of the effect of the reform on both partners' labor supply with respect to the employment contracts that individuals had when they reported sick. Our evidence is consistent with the hypothesis that partners substitute for each other's labor force participation. Individuals who had a permanent contract at the time when they fell sick increased their labor market participation by 2.2 percentage points due to the reform, while their spouses did not respond. On the other hand, while the reform did not induce people who had a temporary contract at the time when they fell sick to increase their labor participation, their spouses did increase their labor participation substantially, by 2.9 percentage points. Overall, this shows that the response at the couple level is more than 2 percentage points regardless of the type of contract that individuals had at the time when they reported sick. Same as individuals who had a temporary contract, those who were unemployed at the time of falling sick did not manage to increase their labor participation in response to the reform, while their spouses did increase their labor participation by 2.2 percentage points.

These findings identify the labor market groups that are vulnerable to the income shock arising from the DI reform as studied here, but possibly also to other income shocks due to, for example, business cycles. Individuals who had a permanent contract at the time of falling sick manage to increase their labor participation and hardly rely on UI to compensate for lost DI benefits. On the other hand, those who had a temporary contract or were unemployed do not manage to increase their labor participation and rely on UI to compensate for lost DI benefits. Our findings on spousal labor supply responses make clear that persons with temporary contracts and the unemployed are indeed vulnerable groups as only for them a spousal response is observed. This also shows a distinction within these groups. Whereas the vulnerability of these groups of persons who face long-term sickness is demonstrated by the need for a spousal response in order to compensate for lost DI benefits, the most vulnerable groups of sick persons are the temporary contract workers and the unemployed who do not have a spouse who can make up for the loss of income.

Our results on the labor supply responses of sick people without a spouse confirm our main finding that partners substitute for each other's labor supply. In response to stricter criteria for disability insurance, they increase their labor participation more than sick people who have a partner, since they cannot rely on the labor supply of a spouse. They increase their labor participation even when on a temporary contract.

We also find differences between age groups. The oldest age group includes individuals who fall sick between age 55 and 65. In this group, neither these individuals nor their spouses increase their labor participation significantly due to the reform.

Instead, sick individuals in this age group rely more often on unemployment benefits after the reform. This suggests that the WIA reform has not been effective in stimulating work for older age groups. The increase of the state pension age and reduced opportunities for early retirement highlights the need for further research on alternative policies such as lifelong learning and improved working conditions in demanding occupations, as well as shifting age norms and age attitudes among employers that may help to reduce long-term sickness among older workers.

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

The main identifying assumption of the DiD approach is that – depending on observables – control and treatment groups share the same time trend in the potential outcome variable both before and after a person reports sick. The assumption can be tested during the pre-treatment period. Figures 2a and 2b show that control and treatment groups, both for sick individuals and their spouses, share very similar time trends until individuals report sick, thus supporting this assumption. We used some regressions to formally test this assumption. In particular, we used pre-treatment data (e.g. from January 1999 to September 2003 for people who reported sick in October 2003) to estimate a model where the outcome is explained by event biannual dummies, interactions of treatment and biannual dummies, and time-invariant individual effects. The first six months of event time form the basis for comparison. Figures 6a and 6b plot the estimates on the treatment and biannual dummy interactions for individuals before they reported sick (left hand panel) and for their spouses (right hand panel). For both groups and all outcomes, the estimates are insignificant throughout the pre-treatment period. Furthermore, they are also jointly insignificant, except for DI and UI receipt (F-test, see the note below the figure).

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

As described in Section 2, whether a person reports sick before or after January 2004 determines eligibility under either the transitional WAO or the WIA. This means that a person with a poor health in 2003 may respond by selecting into the transitional WAO or the WIA scheme from the moment the reform is announced. In particular, such person might anticipate the much stricter WIA scheme and enter the more lenient transitional WAO scheme. In such case, the estimated impact of the reform can be biased. We argue that such self-selection is unlikely. The Dutch government presented a general policy program outlining, among other goals, its plan to reform the disability scheme on September 15, 2003. It announced that the sickness period would be extended from one to two years, and a stricter DI law would be introduced for individuals reporting sick as from January 1, 2004. The transitional WAO reform was not announced until March 12, 2004. The details of the WIA reform were announced on August 18, 2004. This means that for people facing poor health conditions in 2003, reporting sick in anticipation of the transitional WAO reform was not possible. However, following the first announcement in September 2003, a person could report sick during the last quarter of 2003 instead of after the implementation of the WIA reform on January 1, 2004. If many individuals were to do this, reporting sick would increase markedly in the last quarter of 2003.

Table 5 presents the number of individuals by the month in which they reported sick. The distribution is fairly uniform and does not suggest any particular pattern. It certainly does not suggest that many individuals reported sick in the last quarter of 2003 instead of early 2004. On the contrary, reporting sick seemed to increase if anything in January 2004, when the much stricter WIA scheme was introduced.

Appendix C
Do couples dissolve their cohabitation in response to the reform?

We study the labor supply responses to the DI reform by couples who started cohabiting before reporting sick. A couple can dissolve its cohabitation during post-treatment in response to the reform or for other reasons. This may confound the estimated reform effects. Here we first check to what extent the reform affected cohabitation status during post-treatment, and then analyze how the baseline estimates of the reform effects respond when we restrict the study sample to couples that stay together during post-treatment.

Figure 7 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 both 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. We also find that couples in both the treatment and control groups cohabit for about 8 years on average during the 10-year period of post-treatment, with no statistically significant difference. To test whether the reform affected cohabitation status, we rely on a sharp regression discontinuity design. In particular, we use the date on which the reform came into effect as a source of exogenous variation in treatment status, and analyze whether sick-listed workers insured under the transitional WAO and the WIA differ in their cohabitation status on the reform date. Figure 8 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 parentheses) of the reform effect is 0.000 (0.000) and is statistically insignificant at the 10% level.[10] This shows that the reform did not cause couples to dissolve their cohabitation, suggesting that the estimated treatment effect of the reform is not affected notably by cohabitation status changes post-treatment.

To analyze how much the changes in cohabitation status actually affect the estimated reform effects, we check how much the baseline estimate of the reform effect changes if the study sample is restricted to couples that continue to cohabit during the entire post-treatment period. Table 6 shows the estimation results. The baseline estimates that were found significant in Table 1 become slightly larger. The effects on the total income of spouses and that on couples become significant, thus confirming our main finding that spouses share the burden of a more stringent disability scheme.

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

Figure 6a: Estimates of pre-treatment effects of the reform for sick people (left panel) and their spouses (right panel), with 95% confidence intervals. Notes: Standard errors are adjusted for heteroskedasticity and clustering at the individual level. The F-statistics (p-value in parentheses) for the assumption of common pre-treatment trends for sick individuals and spouses, respectively, are 2.753 (0.003) and 1.004 (0.434) for DI receipt, 1.134 (0.334) and 1.362 (0.199) for labor participation, and 3.194 (0.001) and 1.802 (0.062) for UI receipt.

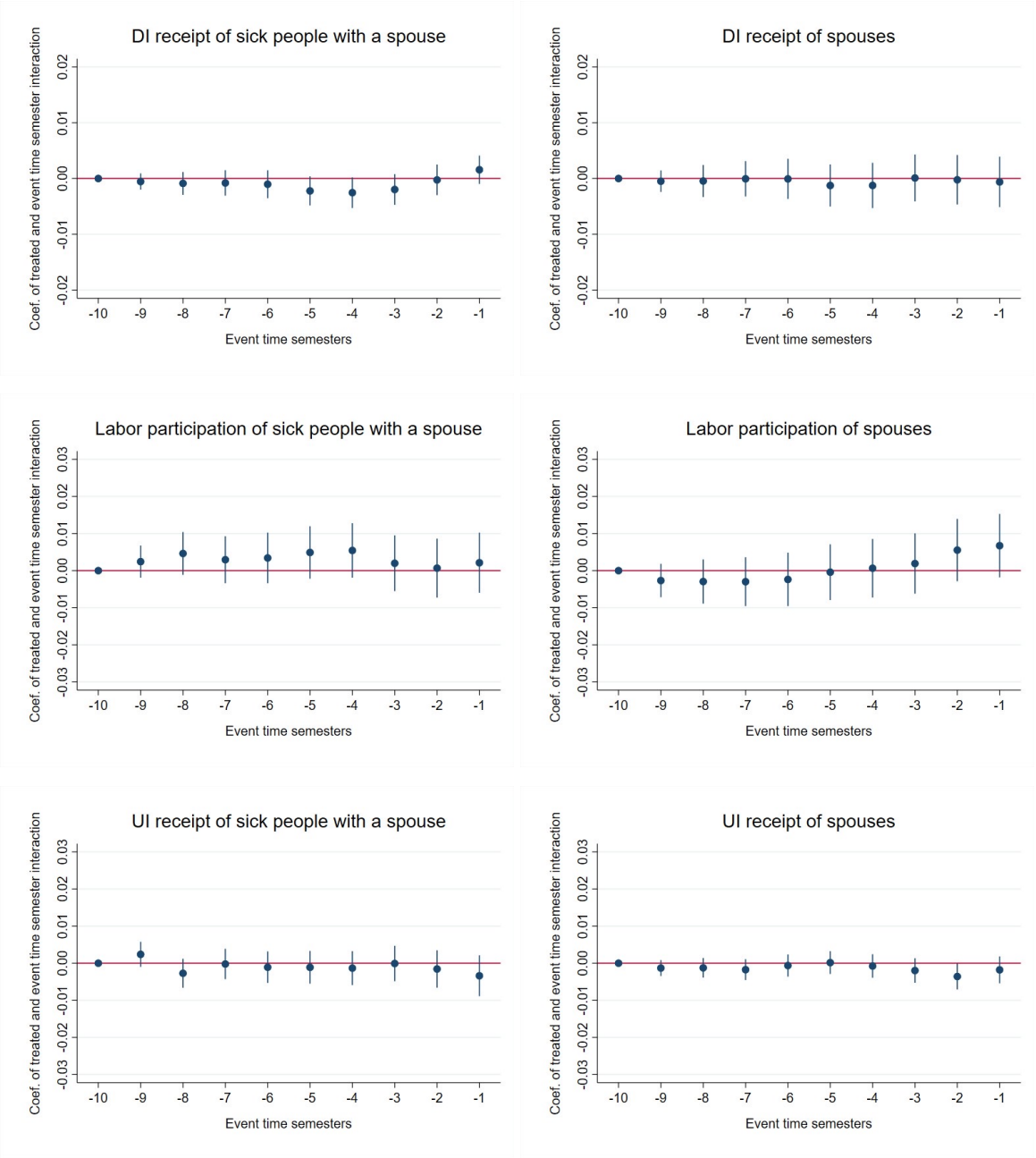

Figure 6b: Estimates of pre-treatment effects of the reform for sick people (left panel) and their spouses (right panel), with 95% confidence intervals. Notes: Standard errors are adjusted for heteroskedasticity and clustering at the individual level. The F-statistics (p-value in parentheses) for the assumption of common pre-treatment trends for sick individuals and spouses, respectively, are 0.625 (0.777) and 1.160 (0.316) for social assistance receipt.

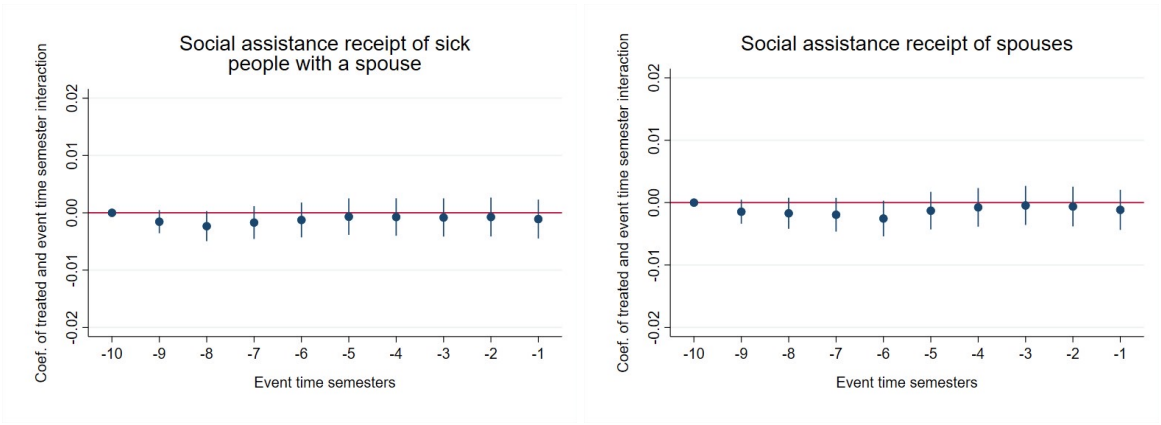

Table 5: Number of sick people according to the month of reporting sick

Month when reporting sick	Number of individuals	Percent
October 2003	14,925	17.71
November 2003	14,048	16.67
December 2003	11,680	13.86
January 2004	15,119	17.94
February 2004	13,106	15.55
March 2004	15,384	18.26

Figure 7: Probability of couples ending cohabitation after one spouse reports sick. Vertical lines mark the first instance that sick partners could become entitled to the sickness and disability benefits in the transitional WAO and the WIA schemes. Red lines correspond to October 1, 2003 and January 1, 2004 for the transitional WAO and WIA groups, respectively. Blue lines correspond to October 1, 2004 and January 1, 2006 for the transitional WAO and WIA groups, respectively.

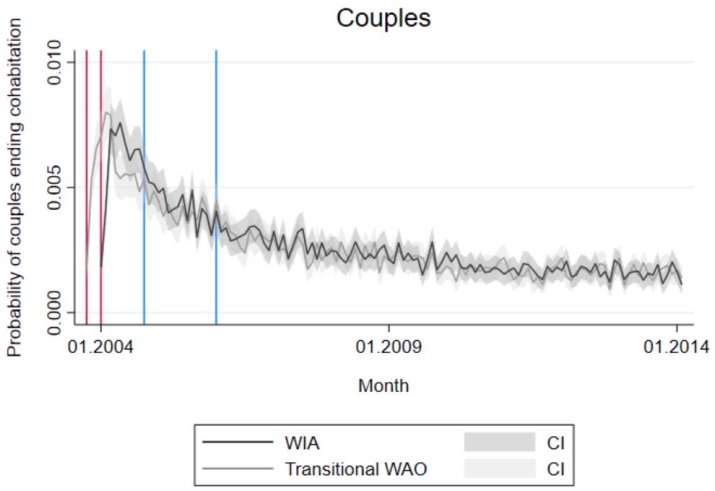

Figure 8: Local linear fit on the two sides of the cut-off. Standard errors are clustered at the individual level. The figure uses 1,996,200 observations for 16,635 couples and data for all available years after reporting sick.

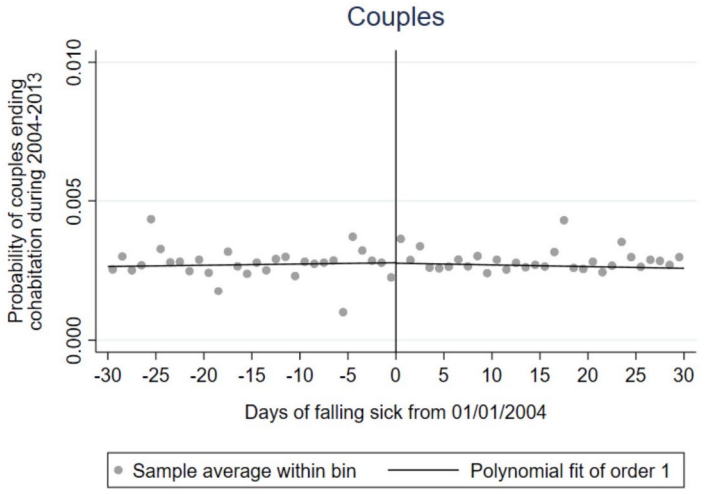

Table 6: Estimated effects of the WIA reform on labor participation, earnings, unemployment insurance receipt, and social assistance receipt of couples who stay together post-treatment

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

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

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