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Abstract

The Netherlands reformed its disability insurance (DI) scheme in 2006. Eligibility for DI became stricter, reintegration incentives became stronger, and DI benefits often became less generous. Based on administrative data on all individuals who reported sick shortly before and after the reform, difference-in-differences regressions show that the reform reduced DI receipt by 5.2 percentage points and increased labor participation and unemployment insurance (UI) receipt by 1.2 and 1.1 percentage points, respectively. It increased average monthly earnings and UI claims to overcompensate lost DI benefits. However, older individuals, women, individuals with temporary contracts, the unemployed, and low-wage earners did not compensate or compensated to a much smaller extent for the lost DI benefits. The effects are persistent during the 10 years after the reform.

KEYWORDS

difference-in-differences, disability insurance, program evaluation

1 | INTRODUCTION

As the number of DI recipients grows in many Western countries, evidence on the impact of DI reforms on work resumption and benefit claims from DI and alternative social support programs are of utmost importance to policy makers. Figure 1 shows the number of people receiving DI benefits in 10 OECD countries during the period from 2007 to 2016. While many of the countries show an increasing trend over the observation period, in a small number of countries inflow into DI has decreased due to the major policy reforms implemented in these countries during the last 2 decades. Furthermore, during the period from 2007 to 2015, public spending on incapacity accounted, on average, for 2.06% of the gross domestic product in the OECD countries (OECD, 2018).

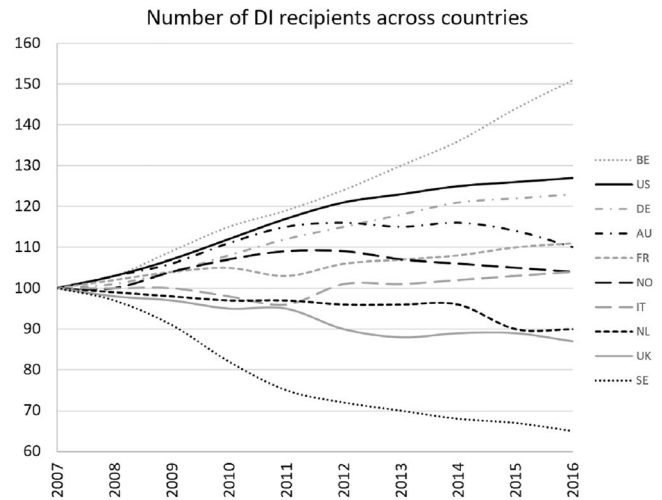
Through DI reforms, governments introduced different types of measures to reduce the size of DI programs and promote labor participation among sick individuals. Autor and Duggan (2006) and Dal Bianco (2019) distinguish among three types of measures: stricter screening for disability during the sickness period and stricter eligibility criteria for DI so that fewer insured workers qualify, reducing benefit generosity so that incentives of insured workers to seek benefits are reduced, and introducing return-to-work incentives for workers on benefits and for employers.¹

Some of these studies rely on DI reforms to provide causal evidence on the impact of DI policies. With respect to screening for disability during the sickness period, Hulleger and Koning (2018) and Godard et al. (2022) analyze the effects of the Gatekeeper protocol introduced in the Netherlands in 2002, which made reintegration obligations of both employers and employees claiming sickness benefits concrete, making the screening process stricter. With respect to eligibility criteria,

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FIGURE 1 Trends for the number of disability insurance (DI) recipients across countries based on the OECD Social Benefit Recipients Database (OECD, 2019a). Values normalized (100 in 2007).



Karlström et al. (2008) study the effect of abolishing generous eligibility rules for DI in the Swedish DI program in 1997. Staubli (2011) studies the effect of increasing the age at which conditions to be classified as disabled are relaxed in the Austrian DI program in 1996. Borghans et al. (2014) analyze the effect of medical reexamination of existing DI recipients based on stricter criteria as one of the measures introduced by the major DI reform implemented in the Netherlands in 1993. Autor et al. (2016) analyze the effect of a policy change that expanded the medical eligibility criteria in the disability compensation program for veterans in the US in 2001. Garcia-Mandicó et al. (2020) analyze the effect of medical reexamination of existing DI recipients based on stricter eligibility criteria implemented in the Netherlands in 2004. With respect to benefit generosity, several studies analyze the effects of reforms changing the generosity of the DI scheme in Canada and Europe (Campolieti, 2004; Deuchert & Eugster, 2019; Favre et al., 2021; Gruber, 2000; Marie & Vall Castello, 2012; Mullen & Staubli, 2016). Studies focusing on return-to-work incentives usually focus on changes in system parameters such as earnings allowances and earnings taper rates.

The main finding in this literature is that stricter screening, stricter eligibility, and reducing benefit generosity, which both aim at limiting inflow into DI, substantially reduce DI receipt and increase labor participation, although spillover effects into alternative benefit programs may be inevitable (Borghans et al., 2014; Garcia-Mandicó et al., 2020; Godard et al., 2022). Return-to-work incentives, which aim at encouraging beneficiaries to exit the benefit program, typically have limited effects. Based upon a series of DI reforms implemented in the Netherlands, covering all three types of the measures reviewed above, Van Sonsbeek and Gradus (2013) also conclude that inflow-related measures are far more effective than outflow-related measures. In addition, Haller et al. (2020) compare the effects of the measures of the first two types in Austria and find that in reducing DI program expenditures, tightening eligibility rules is more effective than lowering benefits, while at the same time it leads to smaller income losses for beneficiaries.

In the Netherlands, in 2002, the share of disabled workers in the insured population reached approximately 11%, with almost 1 million DI recipients (Koning & Lindeboom, 2015). Successive governments implemented a series of radical DI reforms, following the broader trend of shifting the policy focus from compensation to integration in OECD countries between 1985 and 2000 (OECD, 2003). In 2004, a transitional DI scheme (transitional WAO) came into effect, replacing the Disability Insurance Act (WAO) with a broader definition of what work can still be done by the DI applicant. In 2006, the Work and Income According to Labor Capacity Act (WIA) came into effect as the final element of these reforms. WIA implemented major changes in both the sickness insurance (SI) scheme and the DI scheme that succeeds it. It extended SI from one to 2 years, implying an extension of wage compensation by the employer and reintegration obligations of both the employers and employees by an additional year. DI eligibility was restricted to those with a disability grade above 35% instead of 15%. Benefit generosity was decreased for the partially disabled and increased for the fully disabled. Experience rating was extended for partially disabled workers and abolished for the fully disabled. Overall, the WIA is a comprehensive reform that introduced multiple measures of all three types of measures reviewed above. It is therefore studied by other countries facing unsustainable growth in their DI schemes, such as the US (Burkhauser et al., 2014; Fultz, 2015).

Research analyzing the impact of the WIA has been limited to Van Sonsbeek and Gradus (2013).² Based on aggregated quarterly data on DI use, they find that the whole of the WIA reform, including the extension of the sickness period in addition to the other incentives it introduced for work resumption, has led to a sharp fall in DI receipt, in addition to what has already been achieved through previous reforms, generating large budgetary savings for the government. In their ex-ante evaluation of

the reform, Van Sonsbeek and Gradus (2006) predicted that, on average, income of DI claimants remains stable because the gains from additional labor participation compensate for the loss of DI benefits.

Some of the main questions from the policy debate and the claims from the ex ante evaluation of the reform remain to be examined. First, although it is known that DI receipt strongly decreased after the reform, it is not analyzed to what extent the decrease in DI use among sick individuals led to an increase in labor participation or use of benefits from alternative benefit programs. Second, it is not clear how the amounts of earnings and benefits received by sick-listed workers are affected by the reform. Although it was claimed at the onset of the reform that the income position of sick-listed workers would remain stable, it was never confirmed that these workers are able to compensate for loss of DI benefits by other means. Third, it is not known whether the effects of the WIA reform are structural or fade in the long run, for example, because people who do not first enter the WIA later become sicker and still become incapacitated for work. It is known that DI receipt had risen since the onset of the reform (Berendsen et al., 2019), but this need not be caused by diminishing reform effects over time, but it can be caused, for example, by increasing labor participation of older workers who have a higher DI risk.

In addition to these questions, which regard the effect of the WIA reform for the average sick individual, it is not known how the reform affected subgroups of sick individuals. This is of particular interest because, as in many other continental European countries, the labor market in the Netherlands is characterized by a strong insider-outsider segmentation (Hausermann & Schwander, 2012) and low labor mobility of older workers (Visser et al., 2018). In fact, there is scarce evidence on how the same DI policy measures affect sick individuals of different socioeconomic backgrounds. The DI reforms analyzed by existing studies often affected subpopulations of sick individuals, for example, certain age groups (Borghans et al., 2014; Karlström et al., 2008; Staubli, 2011) or a sector (Autor et al., 2016; Mullen & Staubli, 2016). The WIA, on the other hand, induced a negative income shock and applied to all sick individuals, but it could affect subgroups of sick individuals to different extents. While some may be able to increase earnings to compensate for lost disability benefits, others may struggle to increase earnings and need to substitute lost disability benefits with unemployment benefits.

To answer these questions, we exploit unique administrative data and compare individuals who reported sick shortly before and after the reform. Individuals who reported being sick in the last quarter of 2003 were insured under the transitional WAO scheme, whereas those who reported being sick in the first quarter of 2004 were insured under the WIA scheme. Comparing the sick individuals insured under the transitional WAO and WIA schemes, we analyze the total effect of the comprehensive set of rules brought by the WIA reform that aimed at limiting inflow into DI and stimulating outflow. Individuals who reported illness in the third quarter of 2003 were insured under the WAO scheme, the most lenient of the three DI schemes. Comparing the sick individuals insured under the WAO and transitional WAO schemes, we analyze the effect of the one measure of the transitional WAO reform that made eligibility criteria for DI stricter. To estimate the reform effects, we use a difference-in-differences design. We test the main identifying assumptions and provide supporting evidence that the compared groups of sick individuals share the same time trend in the potential outcome variables before and after they report sick and face the reform incentives or not. Furthermore, individuals do not time their sick reporting in response to the reforms to select themselves to a DI scheme.

We add to the literature with three findings from the WIA reform. First, the reform tightened the eligibility criteria much more than earlier reforms did, such as the reform studied by Borghans et al. (2014). Under a much stricter DI regime, sick individuals might be expected to struggle to compensate for the lost DI benefits, as they might find it hard to increase earnings due to adverse health conditions. We show, however, that, on average, sick individuals overcompensate lost DI benefits by increasing their earnings and claims from UI. Mullen and Staubli (2016) provide similar evidence that when individuals lose DI benefits, they remain able to compensate lost benefits. This finding contributes to the growing recognition that even people with severe impairments can work up to some extent (Burkhauser et al., 2014). It, however, also confirms earlier studies that provide evidence of social support substitution when DI eligibility was made stricter (Borghans et al., 2014; Staubli, 2011).

Second, we find that the effects of the reform are persistent in the long term. This is an important finding from a policy perspective because earlier reforms were less successful in the long term. Koning and Lindeboom (2015) mention that while the DI reforms in the early 1990s at first seemed effective, they were politically not sustainable and therefore relaxed soon after introduction. We explain the long-term effectiveness of the WIA reform by its interventions early on during the sickness period when employees are still connected to their employers. For workers with permanent contracts, in particular, the reform facilitates work resumption with their own employer, where reintegration prospects are strongest. In other words, in the former DI scheme (WAO), for many DI beneficiaries, their work capacities were not fully utilized because the ties with the old employer were prematurely cut.

Third, we document substantial heterogeneity across subgroups of sick individuals in how they respond to the DI reform. Earlier studies provide evidence that DI reforms can induce both work resumption and participation in alternative benefit programs. The reform effects, however, appear to be age dependent (Borghans et al., 2014; Karlström et al., 2008; Staubli, 2011). While these studies carry out analyses on specific age groups in different countries, we analyze the impact of the reform over a

complete range of age groups and investigate the age gradient of the reform that affected sick people of all ages. Our findings over four age groups are in line with the predictions on different age groups from existing studies. Following the WIA reform, older individuals much less often increase labor participation and heavily depend on UI compared to younger individuals.

In the years preceding the WIA reform, growing DI receipt was mainly caused by the growing inflow of women into the DI scheme, causing a substantial overrepresentation of women claiming DI benefits. As both work- and nonwork-related reasons may explain this (Einerhand and van der Stelt, 2005), it is unclear if women are able to compensate if they lose DI benefits due to a reform, for example, by increasing their earnings. We show that women are not able to compensate for the DI benefits lost due to the reform by increasing either earnings or UI claims. Men, on the other hand, overcompensate for the lost benefits by increasing earnings by a large margin. In addition, they receive more from the UI.

The WIA reform increased employer incentives for reintegration, but these incentives do not apply uniformly to all workers. Employers of employees with a permanent contract are fully incentivized due to continued wage payments and experience rating. On the other hand, for temporary workers who have an employer, employer incentives last only as long as the contract lasts. Unemployed individuals have no employer and therefore cannot benefit from the positive effects of employer incentives. We analyze the reform effect separately for sick individuals who were wage earners with a permanent contract, wage earners with a temporary contract, or unemployed at the time they reported being sick. We show that while sick individuals who have a permanent contract increase their earnings substantially and much less often claim UI, those who have a temporary contract and those who are unemployed do not increase earnings by a statistically significant amount, but they increase UI claims substantially.

We also analyze the reform effects across income groups. Sick individuals of lower income groups might find it harder to increase earnings to compensate for lost DI benefits because job prospects may be limited for them or they may more often struggle to recover from sickness. In line with these expectations, we find that those in the highest presickness earnings tercile overcompensate lost DI benefits with earnings only, while those in the lower terciles just compensate or fail to compensate by increasing both earnings and UI claims. The fact that the effect of the reform is very different across labor market groups is of high relevance to policy makers who have to consider the risk of a widening gap between labor market segments as a consequence of social insurance reforms.

Evaluating the transitional WAO reform, we show that its effect on labor participation is as large as two-thirds of that of the WIA reform. The notable impact of one measure of the transitional WAO reform, relative to the total impact of the different measures of the WIA reform, provides supporting evidence for the earlier studies that find that measures that limit DI eligibility are more effective than the measures that stimulate reintegration among DI beneficiaries.

The remainder of this paper is organized as follows. Section 2 describes the Dutch DI scheme and the 2006 reform. Section 3 describes the data. Section 4 presents descriptive evidence on the impact of the reform. Section 5 describes the identification strategy. Section 6 presents the estimation results. Section 7 discusses the policy implications of the results.

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 disability. It was amended several times, but since the main amendments in 1993, it preserved its main features until it was replaced by the Work and Income Act (WIA) in 2006. Under WAO, individuals earning wages or receiving UI are first admitted to the SI scheme if they are unable to perform their work because of occupational or nonoccupational illness or injury. The duration of the scheme is 1 year. The employer is responsible for paying at least 70% of the presickness wage. When the SI scheme expires, individuals are admitted to the DI scheme if their disability grade is at least 15%.³

Due to easy access, the annual inflow rate into the WAO rose to 1.5% 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 October 1, 2004 for people who reported illness from October 1, 2003 until January 1, 2004, making DI eligibility stricter. In particular, it adapted a broader definition of what work can still be done by the DI applicant. As a result, the estimated wage loss due to disability was reduced, making it harder to reach the minimum disability grade to qualify for DI and to reach a higher disability grade (with a higher benefit).

WIA was introduced in 2006 for people who reported being sick from January 1, 2004 onward. It introduced major changes in both sickness and disability schemes to facilitate work resumption. It succeeded in limiting the yearly inflow rate into the disability scheme to 0.5% of the insured working population during the first 6 years since its introduction (Koning & Lindeboom, 2015). WIA extended the duration of the sickness scheme from one to 2 years, implying an extension of two main

incentives. First, the employer is responsible for paying at least 70% of the presickness wage during the additional sickness period, creating a strong incentive for the employer to facilitate work resumption. Second, reintegration obligations of the Gatekeeper protocol for employers and employees were extended to a second year of sickness.

For the disability scheme, WIA kept the stricter DI eligibility criteria of the transitional WAO. It introduced three other changes. First, the minimum disability grade to enter the scheme rose from 15% to 35%. Therefore, workers with limited disability are expected to resume working (possibly with adaptations) or apply for UI. Second, benefit generosity was reduced for the partially disabled if they did not utilize at least 50% of their remaining earning capacity, providing a financial incentive to increase labor participation. On the other hand, the benefit amount was increased from 70% to 75% of the presickness wage for the fully disabled. Third, experience rating for employers was extended from 5 to 10 years, meaning that employers are penalized with a higher DI premium 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 this group has better prospects of reintegration. Experience rating was limited to permanent work contracts until 2013 and extended to temporary contracts afterward.

In the WAO or WIA, if there is no employer, the Employee Insurance Agency pays the Sickness benefit during the sickness period.⁴ For the WIA reform, which extended the sickness period from one to 2 years, this implies that there is no extension of employer incentives to the second year of sickness since there is no employer. In the WAO, during the disability period, individuals can apply for UI in addition to DI if there is no employer. In the WIA, UI is integrated into DI, and therefore, no application is necessary.

3 | DATA

We use administrative data from the Employee Insurance Agency on all individuals who reported illness in the fourth quarter of 2003 and the first quarter of 2004 and therefore became eligible to participate in the transitional WAO and the WIA schemes, respectively. We use these data to analyze the impact of changing the transitional WAO to the WIA scheme. Sick individuals who participate in transitional WAO constitute our “control group” and those who participate in the WIA constitute our “treatment group”. We also observe all individuals who reported illness in the third quarter of 2003 and participated in the WAO scheme. We use these data to analyze the impact of changing the WAO to the transitional WAO scheme.

For these individuals, we observe the beginning and ending dates of their sickness, their gender and date of birth. They either earn wages or receive UI when reporting illness since other labor market groups are not eligible for SI. For wage earners, we observe whether they hold a permanent contract, temporary contract or a contract through a temporary work agency. We link these data to administrative data on wages and benefits available for these individuals on a monthly basis from Statistics Netherlands. The benefits include DI, UI, general assistance, and other benefits from smaller social support programs. The data extend from January 1999 to December 2015.

The initial data set has 251,567 sick individuals. To select the estimation sample, we impose three restrictions. Employers are mandated to report sickness cases if they last longer than 90 days. Temporary work agencies are the main suppliers of short-term sickness cases. They complied with mandatory reporting to a large extent only from January 2004 onward, after WIA came into effect. Therefore, in the data, short-term sickness cases are underreported for participants of the (transitional) WAO who reported illness before January 2004. The three cohorts of sick individuals, however, share similar distributions of sickness duration when sickness duration is restricted to be at least 180 days. Therefore, we drop individuals who spend less than 180 days in the sickness scheme.⁵ We also drop individuals who already receive DI when they report being sick. Finally, we drop individuals if they participate in DI schemes for the self-employed (WAZ) and young people (WAJONG) since their institutional rules for work resumption are very different.⁶

Based on the available data on wages and benefits, we define the following outcome variables: dummies that indicate labor participation and benefit receipt and monthly amounts of wages and benefits.⁷ In monthly amounts, we include zero values.

During participation in the SI 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 sick people are working when receiving SI. Therefore, in our analyses, we do not use observations during the sickness period—1 year for (transitional) WAO and 2 years for WIA. The transitional WAO reformed the DI scheme, and the WIA reformed both the SI scheme and the DI scheme that succeeds it. We analyze the effect of each reform during the DI period.

4 | TIME TRENDS AND DESCRIPTIVE EVIDENCE FOR REFORM EFFECTS

Figure 2 shows the labor participation rates and fractions of benefit recipients over the observation period among the sick individuals insured under the transitional WAO and WIA. Considering the time trends, for both groups, DI receipt increases sharply when they become eligible for DI. For the transitional WAO group, DI receipt decreases shortly after becoming eligible for DI. It might be that this group has better chances of recovery after entering the DI scheme because this group enters the DI scheme after 1 year of sickness instead of 2 years. The probability of working increases until individuals report illness, reflecting the

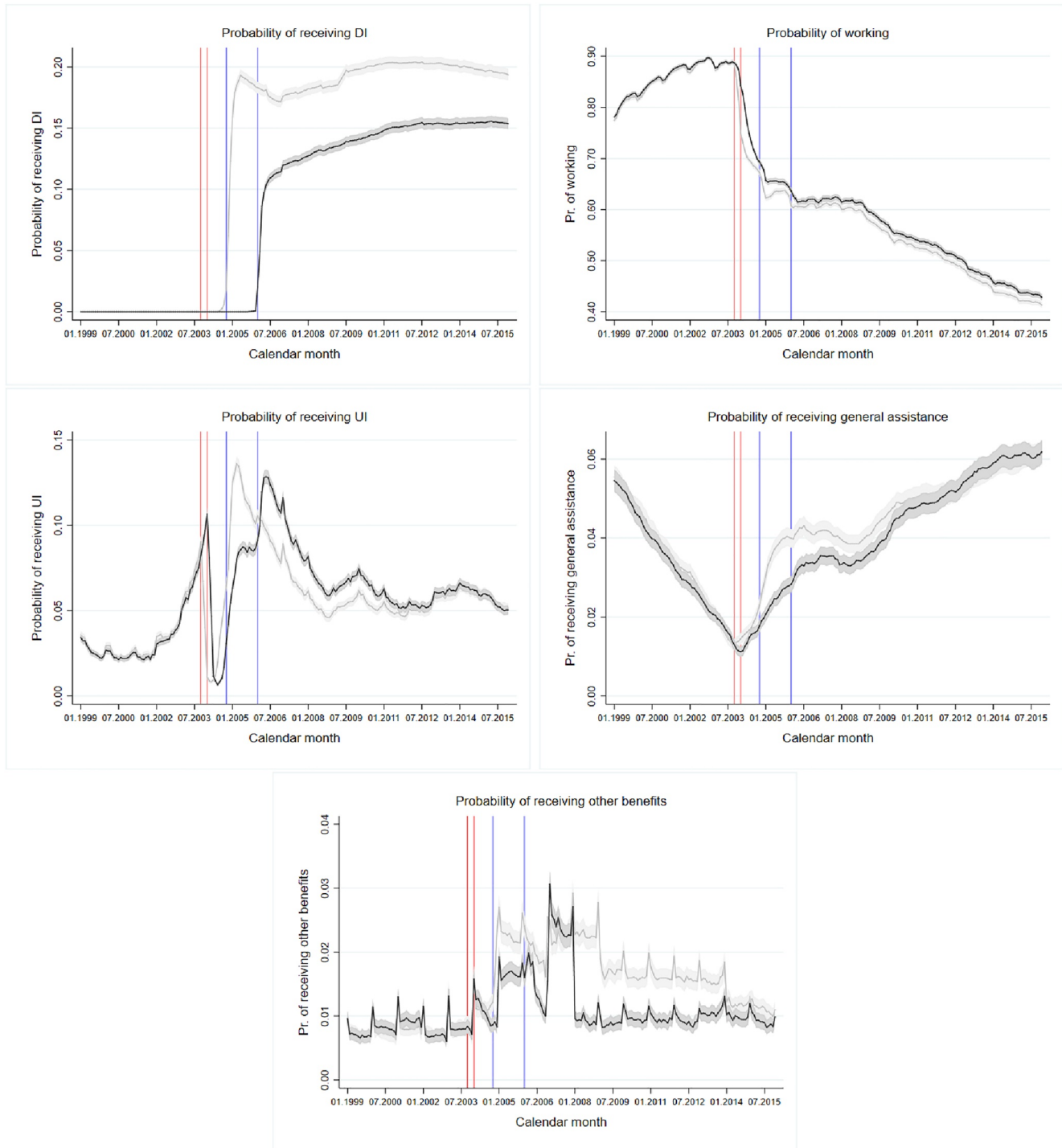


FIGURE 2 Probability of labor participation and benefit receipt for control (gray) and treatment (black) groups by calendar month, with 95% confidence intervals. Vertical lines mark the first instance sick individuals can be entitled to sickness (red) and disability (blue) benefits. [Colour figure can be viewed at wileyonlinelibrary.com]

fact that individuals can claim SI only if they are working or receiving UI at the time they report illness (Section 3). Before this time, they can have another labor market status. The probability of working falls sharply during the first few years of sickness and continues to fall throughout the remaining years.⁸ In both groups, UI receipt falls sharply after reporting sick, since those who are unemployed replace UI with SI benefits. UI use rebounds and increases during the remaining months of the sickness scheme because many individuals recover and replace their SI benefit with UI benefit. UI use peaks when individuals can apply for DI because when the sickness period ends, rejected DI applicants turn to UI. UI use decreases during the disability period. This is because UI is temporary with a maximum of 38 months.⁹ The time trends of general assistance and benefits from small social support programs seem to be related to that of working: As individuals work less due to sickness, they lose earnings and become entitled to general assistance and other benefits.

Comparing the transitional WAO and WIA groups shows that differences between the two groups are statistically insignificant before individuals report being sick for all outcomes, but they are notable and remain significant after they report being sick for most outcomes. This suggests that the reform effectively limited DI use and increased labor participation, but it also increased UI use among those who reported being sick. Due to the reform, sick individuals also rely less often on benefits from other social support programs.¹⁰

Table 1 presents sample means of some background characteristics when reporting sick for the transitional WAO and WIA groups, as well as outcomes before reporting sick and after leaving SI. It also presents tests for equality of the means in control and treatment groups.¹¹ In both groups, the average age is about 40 and there are more men than women. Most hold a permanent work contract, and others hold a temporary contract or are unemployed. The group mean differences are often small but statistically significant. Some of the differences may be due to labor market trends: more temporary jobs may be available in the fourth quarter of 2003 compared to the first quarter of 2004 due to a seasonal change in demand for labor. Our identification strategy accounts for such differences. In line with Figure 2, the group mean differences for labor market outcomes are often larger during the reform period than before the reform period, again suggesting that the reform induced behavioral responses.

5 | IDENTIFICATION STRATEGY

We take a difference-in-differences (DiD) approach to identify the causal effect of the WIA reform on labor participation, benefit receipt, monthly wages and benefits from social support programs. The first difference is across groups. Those who reported being 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 being sick in the fourth quarter of 2003 (control or transitional WAO group). The second difference is across event time, that is, before individuals report illness and after they leave SI and become eligible for DI.

We implement the DiD comparison using the following regression:

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

i indexes individuals. t indexes the months of event time: Values from -57 to -1 indicate the months before reporting sick, 0 is the first month individuals leave the sickness scheme, and $1-119$ are the months after they leave the sickness scheme.¹² y_{it} is an outcome variable. $\lambda_{s(it)}$ is a monthly calendar time effect where $s(i, t)$ indexes the calendar month (from January 1999 until December 2015; 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 time-varying shock that is unobserved and assumed to be uncorrelated with all the explanatory variables.

$Treated_i$ is a dummy variable that indicates the treatment group.¹³ $Post_t$ is an event time dummy with value 1 from the start of the possible disability period (after the end of the sickness period). The individual fixed effects capture differences between the control and treatment groups other than the reform effect. Under the identifying assumptions, the coefficient γ on the interaction term $Treated_i \times Post_t$ captures the effect of the reform.

To study the dynamic effects of the reform, we consider the following regression:

$$y_{it} = \alpha_i + \sum_{l=-4}^9 \gamma_l (Treated_i \times d_{lt}) + \sum_{l=-4}^9 \delta_l d_{lt} + \lambda_{s(it)} + \varepsilon_{it}. \quad (2)$$

Instead of the $Post_t$ dummy, indicating the entire period after SI expires, this model considers separate dummies for each year, before reporting sick and after leaving the sickness scheme. d_{lt} indicates the l th year of this period. Year -5 is chosen as the base year. The coefficients on the interaction terms of treatment and these year dummies are the estimated treatment effects. For the years before reporting sick, they provide a test of the common trend assumption. For the period after leaving the sickness

TABLE 1 Sample means and tests for equality of the means of background characteristics and outcomes in control and treatment before and after the sickness period.

	Before			After		
	Transitional WAO group	WIA group	Dif. between WIA and transitional WAO	Transitional WAO group	WIA group	Dif. between WIA and transitional WAO
	(1)	(2)	(3)	(4)	(5)	(6)
A. Background characteristics						
Age	40.137	40.628	0.490***			
Female	0.437	0.440	0.003			
Permanent contract	0.574	0.593	0.018***			
Temporary contract with employer	0.130	0.119	-0.011***			
Temporary contract via agency	0.049	0.034	-0.015***			
Unemployed	0.204	0.213	0.010***			
Other	0.043	0.041	-0.001			
B. Labor market outcomes						
DI receipt				0.191	0.141	-0.050***
Labor participation	0.856	0.860	0.004**	0.536	0.539	0.003
UI receipt	0.036	0.037	0.001	0.065	0.069	0.003***
General assistance receipt	0.035	0.032	-0.002*	0.048	0.047	-0.001
Other benefits receipt	0.008	0.008	0.000	0.018	0.012	-0.006***
DI per month				250.790	212.504	-38.286***
Wage per month	1792.750	1820.622	27.872**	1407.520	1453.732	46.212***
UI per month	43.528	45.717	2.189*	90.183	96.359	6.176***
General assistance per month	26.730	25.061	-1.669*	44.936	44.009	-0.926
Other benefits per month	6.486	5.922	-0.564	23.393	20.335	-3.058***
Observations	1,518,527	1,551,421		3,515,137	3,027,959	
Individuals	26,217	25,441		26,217	25,441	

Note: (1) "Before": period before individuals report sick corresponding to January 1999 - October 2003 for individuals who reported sick in November 2003 and January 1999 - January 2004 for individuals who reported sick in February 2004. "After": period after individuals leave SI and could become entitled to DI corresponding to November 2004 - October 2014 for individuals who reported sick in November 2003 and February 2006 - December 2015 for individuals who reported sick in February 2004. (2) Age is at the time individuals report being sick. (3) Columns 1, 2, 4 and 5 present means in control and treatment before and after the sickness period. Columns 3 and 6 present the differences between individuals insured under the WIA and WAO. A difference is 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 are clustered at the individual level.

***, **, * denote statistical significance at 1%, 5%, and 10%, respectively.

scheme, they reflect the dynamic effects of the reform. In this setup, control and treatment groups are compared over event time t , that is, the months before reporting sick and after leaving the sickness scheme. The calendar time dummies $\lambda_{s(it)}$ capture the (common) calendar time trend.

Our DiD design has two main identifying assumptions. First, individuals do not select themselves into the old or new DI 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 WAO scheme on September 15, 2003. They announced that the SI period would be extended from 1–2 years, and a stricter DI scheme would be introduced for individuals reporting illness from January 1, 2004. The transitional WAO reform was announced only on March 12, 2004. Details of the WIA reform were announced on August 18, 2004. Following the first announcement in September 2003, individuals could report sickness during the last quarter of 2003 instead of after the introduction of the WIA reform on January 1, 2004 to enter the more lenient (transitional) WAO scheme instead of the stricter WIA scheme. In this case, the estimated effect of the reform can be biased.

Self-selection, however, is unlikely. Figure 3 presents the probability distribution of individuals by the month they report sick during the last quarter of 2003 and the first quarter of 2004. Sick reporting shows a decreasing trend in the last quarter of

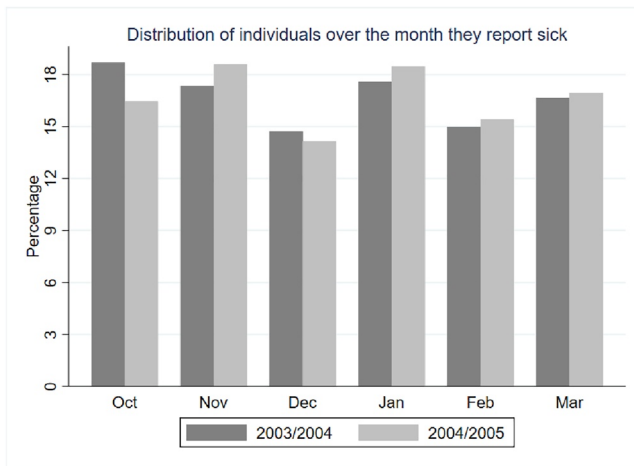


FIGURE 3 Distribution of the number of individuals reporting sick among those who reported sick in the last quarter of 2003 and first quarter of 2004 and participated, respectively, in the transitional Disability Insurance Act and Work and Income According to Labor Capacity Act (WIA), and those who reported sick in the last quarter of 2004 and first quarter of 2005 and participated in the WIA. [Colour figure can be viewed at wileyonlinelibrary.com]

2003 and a notable increase in January 2004. This does not suggest self-selection into the lenient transitional WAO scheme to avoid participation in the much stricter WIA scheme. Instead, the relatively low number of sick reports in December 2003 might be due to a seasonal employment pattern in that employees are absent from work and do not report sickness during Christmas and New Year holidays. Figure 3 also presents sick reporting 1 year after the reform among individuals who participated in the WIA only. We fail to reject the equality of the distributions of sick reporting in the year and 1 year after the reform was implemented.¹⁴ This provides additional evidence that there is no self-selection, and the observed trends reflect a seasonal pattern.

The second identifying assumption is that, conditional on observables, control and treatment groups share the same time trend in the potential outcome before and after individuals report sick and face the reform incentives or not. The assumption is testable during the pretreatment period. Figure 2 show that the control and treatment groups share very similar time trends until individuals report illness, supporting this 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 pretreatment period provide evidence in favor of the assumption. Year -5 is chosen as the base for comparison.¹⁵ To control for observed differences between control and treatment individuals in few months before reporting illness, 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, birth year and contract type, as well as all outcomes before reporting sick. Regression Equations (1) and (2) are estimated based on the constructed weights.¹⁶ The weights are regenerated in each subsample when analyzing heterogeneous treatment effects.

Figure 4 plots the estimates on the treatment and annual dummy interactions after entropy balancing. For all outcomes, the estimates are insignificant throughout the pretreatment period. They are also jointly insignificant with p values of at least 88.3%. The estimates are also insignificant in all subsample analyses of heterogeneous treatment effects (available upon request).

6 | THE IMPACT OF THE REFORM ON LABOR PARTICIPATION, BENEFIT RECEIPT, MONTHLY EARNINGS AND BENEFITS

In this section we first analyze the baseline effects of the WIA reform considering the whole post-treatment period. Next, we analyze the effects of the reform in the short- and long-run. In addition, we investigate the heterogeneous effects of the reform. Lastly, we analyze the effects of the transitional WAO reform.

6.1 | Baseline effects

Table 2 presents the baseline DiD estimates of the effects of the WIA reform based on Equation (1). The estimated effects of the reform are always interpreted as the effects of the new rules of the WIA regime compared to the old rules of the transitional WAO regime.

DI awards decreased by 5.2 pp on average due to the WIA reform during the 10 years after the reform was introduced (where data are available on DI). This corresponds to a 26.1% decrease in DI awards among the individuals who reported sick during the fourth quarter of 2003 and the first quarter of 2004.¹⁷ This large effect is in line with the findings of Van Sonsbeek and

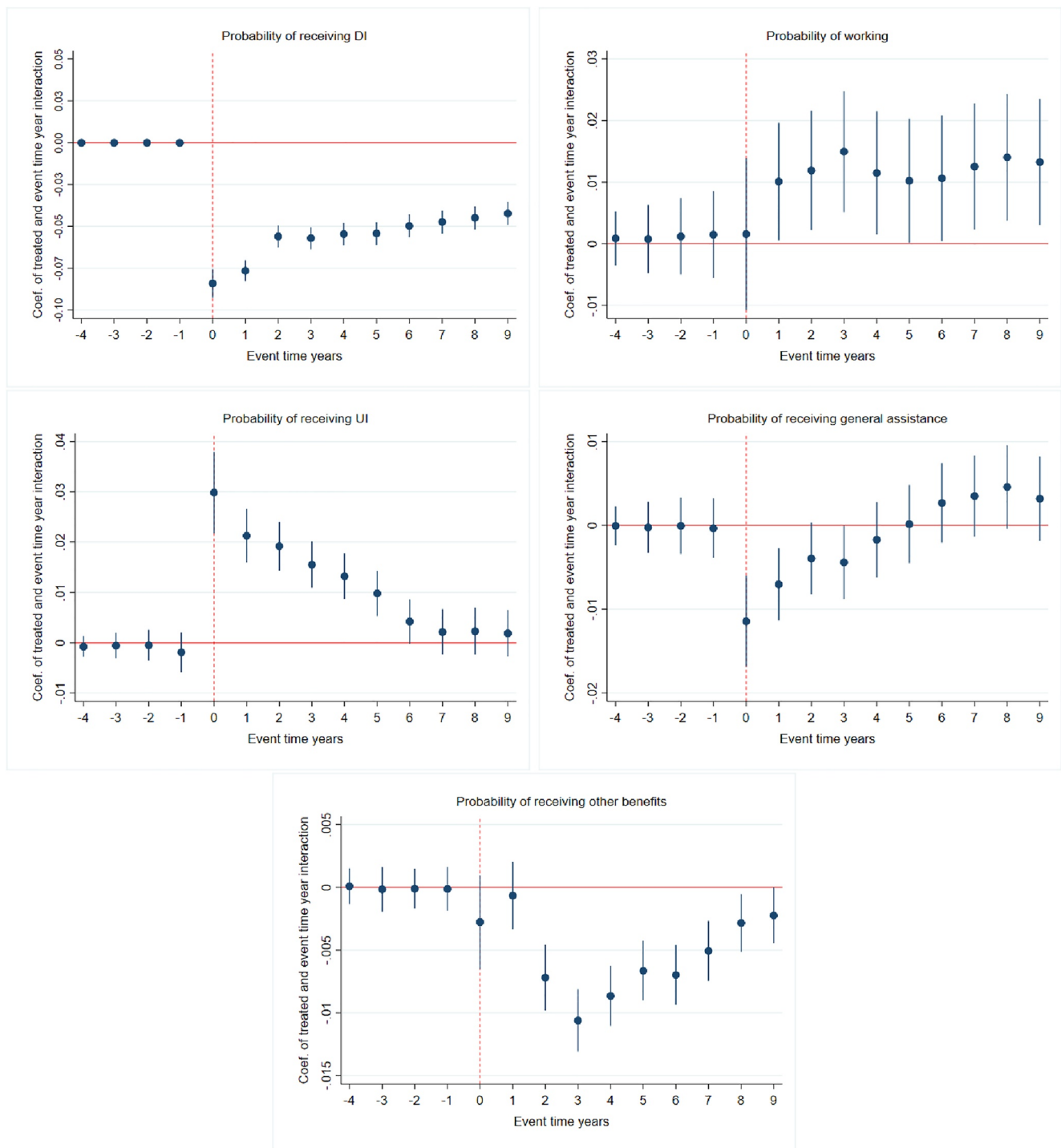


FIGURE 4 Estimated effects of the Work and Income According to Labor Capacity Act reform in each of the 4 years before reporting sickness and in each of the first 10 years after leaving the sickness scheme, with 95% confidence intervals. Observed differences between control and treatment individuals before reporting sick are controlled for using entropy balancing. [Colour figure can be viewed at wileyonlinelibrary.com]

Gradus (2013), who showed that the WIA reduced the number of DI awards in the insured population of workers and unemployed by 36% in the first year and had a slightly diminishing effect over time.

As DI awards decreased, sick individuals responded by increasing their labor participation by 1.2 pp compared to those in the transitional WAO group. This shows that the substantial decrease in DI awards is not matched by a comparable increase in labor participation among the individuals in this group.

Individuals who could not receive DI might have turned to benefits from other social support programs. We find evidence that the WIA reform induced sick individuals to turn to UI and increase their UI receipt by 1.1 pp. Sick individuals, however,

DI receipt	-0.052*** (0.003)
Labor participation	0.012*** (0.004)
UI receipt	0.011*** (0.001)
General assistance receipt	-0.001 (0.002)
Other benefits receipt	-0.006*** (0.001)
DI per month	-45.443*** (4.496)
Wage per month	39.923*** (13.850)
UI per month	15.601*** (1.981)
General assistance per month	-1.084 (1.531)
Other benefits per month	-2.799** (1.259)
Observations	9,565,392
Individuals	51,402

Note: Standard errors (in parentheses) account for heteroskedasticity and clustering at the individual level. All regressions control for individual and calendar month fixed effects and use data available for the whole pretreatment period but exclude data corresponding to SI years of the posttreatment period.

***, **, * denote statistical significance at 1%, 5%, and 10%, respectively.

became less likely to receive other benefits. A potential explanation is that there is less need for claiming these benefits or sick individuals did not qualify for them due to qualifying instead for UI. Earlier studies in other countries also find evidence that tightening the eligibility criteria leads to more take-up of other benefits (Karlström et al., 2008; Staubli, 2011). Studies on earlier DI reforms in the Netherlands, however, show mixed results. Borghans et al. (2014) find that the DI reform that introduced reexaminations using stricter entitlement criteria in 1993 led to more benefit claims from other benefit programs, while Koning and van Vuuren (2010) and De Jong et al. (2011) find that the experience rating reform in 1998 and the Gatekeeper protocol reform in 2002 did not increase the probability of UI receipt, although they substantially reduced DI receipt.

With respect to the monthly amounts of benefits and earnings, under the WIA regime, individuals on average received €45.4 less DI benefits, while they earned €39.9 more wages and received €15.6 more UI benefits. They also received €2.8 less of other benefits. This suggests that, due to the WIA reform, on average, individuals overcompensate lost DI benefits with higher income from increased earnings and more claims from UI. This result is remarkable in view of the finding that the increase in labor participation due to the reform is relatively small compared to the decrease in DI awards. These effects seem economically significant given the sizes of the average DI and UI received per month during the postreform period shown in Table 1.

6.2 | Dynamic effects

Figure 4 presents the estimates of the reform effects for 10 years of the posttreatment period. The estimates confirm the exploratory reform effects over time in Figure 2. The effect of the reform on DI receipt and labor participation is persistent during the posttreatment period, suggesting a structural change in these outcomes. The effect of the reform on UI is large when sick individuals leave SI and can apply for DI, but it diminishes over time and becomes insignificant from year eight after sick individuals can apply for DI. This is a consequence of the UI being temporary. The effects of the reform on general assistance and other benefits receipt appear to be related to the effect of the reform on UI. As UI expires over time, the need to claim these benefits increases.

TABLE 2 Estimated effects of the Work and Income According to Labor Capacity Act (WIA) reform.

6.3 | Heterogeneous effects

Baseline results show that sick individuals increase labor market participation but also claims from UI to compensate for the lost DI benefits. Sick individuals of certain socioeconomic backgrounds might struggle to cope with the negative income effect of the reform more than others and rely more on UI and less often increase labor participation or earnings. To investigate this, we explore the reform effects across background and labor market characteristics. We consider age when reporting sick, gender, employment status when reporting sick, and earnings before reporting sick.

6.3.1 | Age when reporting sick

Existing studies provide evidence that DI reforms can induce both work resumption and participation in alternative social support programs. The reform effects, however, appear to be age dependent. Borghans et al. (2014) find that due to the 1993 DI reform in the Netherlands, individuals approximately 45 years old on average offset € 1.00 of lost DI benefits by collecting € 0.62 from earnings and € 0.30 from other social support programs, but this benefit-substitution effect declines over time. Staubli (2011) shows that the DI reform that tightened eligibility criteria for men ages 55–56 years in Austria increased labor participation but also claims from SI and UI. Karlström et al. (2008) study the effect of abolishing generous DI eligibility rules for people 60–64 years old in Sweden. They find that the reform increased claims from SI and UI but not labor participation during the 2–3 years following the reform. These findings suggest that for older individuals, social support substitution is the most likely outcome, whereas for younger individuals, work resumption is more likely. Work resumption among older individuals may be less likely because their prospects of recovery from ill health may be limited or their incentives for work resumption may be weaker, as they can claim UI for longer periods (since the duration of UI depends on the number of contribution years to social insurance). To test this, we analyze the impact of the reform across four age groups and investigate the age gradient of the reform that affected sick people of all ages.

Table 3 shows notable age effects. Older age groups are much less likely to cope with the reform. They do not increase labor participation by a statistically significant amount. Instead, they heavily rely on UI to compensate for the lost DI benefits. The results on monthly income from UI in Table 4 confirm this. This finding is in line with the predictions of earlier studies on different age groups in different countries: For older individuals, social support substitution is the most likely outcome, whereas for younger individuals, work resumption is more likely when the disability benefit regime is made stricter.

6.3.2 | Gender

Compared to men, women may find it more difficult to compensate for the income lost due to the DI reform. Women more often occupy part-time jobs, have temporary work contracts, and earn lower wages and therefore possibly have weaker employee and employer relationships. Therefore, their opportunities to increase the number of work hours or earnings may be limited. Women may also find it more difficult to increase labor participation if they spend more time in household production.

Tables 3 and 4 show clear gender effects. First, the effects on DI receipt and income from DI are larger for men. This difference is expected. Men on average earn higher wages, mainly because women often work part-time in the Netherlands. In the Dutch DI scheme, a higher presickness wage means a higher probability of getting a partial DI benefit.¹⁸ Therefore, for individuals with higher presickness wages, stricter rules of the DI reform reduce DI awards more. Second, men increase their labor participation and earnings, while for women, these increases are not statistically significant. The earnings response of men is particularly strong. While men overcompensate for the loss in DI benefits with increased earnings only, women fail to compensate with increased earnings or UI benefits. These results suggest a clear gender gap in earnings responses to the negative income shock of the DI reform.

6.3.3 | Employment status when reporting sick

Stimulating employers to increase labor market participation has been a key element of Dutch DI reforms throughout the years. The WIA reform in particular made reintegration incentives stronger for employers. It extended wage compensation from the employer and mutual obligations of employers and sick employees for reintegration by an additional year in the sickness scheme. It also extended experience rating from 5 to 10 years in the disability scheme (Section 2). Employer incentives do not

TABLE 3 Estimated effects of the Work and Income According to Labor Capacity Act (WIA) reform by age, gender, labor market status, and tercile of average earnings.

	DI receipt	Labor participation	UI receipt	General assistance receipt	Other benefits receipt
Fell sick before age 35	−0.040*** (0.005)	0.017*** (0.006)	0.006*** (0.002)	−0.001 (0.003)	−0.003*** (0.001)
Fell sick between ages 35 and 44	−0.049*** (0.006)	0.014** (0.006)	0.009*** (0.002)	0.003 (0.003)	−0.006*** (0.001)
Fell sick between ages 45 and 54	−0.071*** (0.007)	0.010 (0.007)	0.017*** (0.003)	−0.002 (0.003)	−0.009*** (0.002)
Fell sick after age 54	−0.048*** (0.006)	0.009 (0.008)	0.015*** (0.006)	−0.002 (0.003)	0.005* (0.003)
Female	−0.043*** (0.004)	0.009 (0.006)	0.010*** (0.002)	−0.002 (0.003)	−0.002** (0.001)
Male	−0.059*** (0.004)	0.016*** (0.005)	0.011*** (0.002)	0.001 (0.002)	−0.009*** (0.001)
Permanent contract	−0.051*** (0.003)	0.021*** (0.005)	0.005*** (0.001)	−0.001 (0.001)	−0.007*** (0.001)
Temporary contract	−0.042*** (0.008)	0.004 (0.010)	0.015*** (0.003)	−0.004 (0.005)	0.003* (0.002)
Unemployed	−0.068*** (0.008)	−0.010 (0.009)	0.024*** (0.004)	0.007 (0.005)	−0.011*** (0.002)
Earnings in 1st tercile	−0.043*** (0.005)	0.013* (0.007)	0.012*** (0.003)	−0.003 (0.004)	−0.001 (0.001)
Earnings in 2nd tercile	−0.052*** (0.005)	0.010* (0.006)	0.009*** (0.002)	−0.001 (0.002)	−0.008*** (0.001)
Earnings in 3rd tercile	−0.059*** (0.005)	0.011* (0.006)	0.010*** (0.002)	0.002* (0.001)	−0.008*** (0.001)

Note: Standard errors (in parentheses) account for heteroskedasticity and clustering at the individual level. All regressions control for individual and calendar month fixed effects and use data available for the whole pretreatment period but exclude data corresponding to SI years of the posttreatment period.

***, **, * denote statistical significance at 1%, 5%, and 10%, respectively.

apply uniformly to all workers, however. For temporary workers who have an employer, they 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 benefit. Unemployed individuals have no employer and therefore cannot benefit from the positive effects of employer incentives either. On the other hand, employers of employees with a permanent contract are fully incentivized due to continued wage payments and experience rating. These mean that sick individuals who have a temporary contract or are unemployed have no employer who is incentivized to reintegrate them, and therefore, facing the stricter requirements of WIA to enter DI, these groups may more often struggle to resume work.

Employees who have a temporary contract and the unemployed may also have different reintegration incentives than the employees who have a permanent contract. Employees who have a temporary contract may find it harder to resume working since for them, adjustment costs may be higher (Zaresani, 2018). For the unemployed, a 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). These, again, imply that sick individuals who have a temporary contract or are unemployed may more often have to cope with a negative income shock due to the reform. They may then also more often have to rely on UI.

Tables 3 and 4 show substantial heterogeneity with respect to employment status. The reform decreases DI claims among the unemployed more than it does among the other groups. It might be that the unemployed are more often sick or their sickness is less strictly screened so that they are more likely to be affected by the stricter DI regime. Other results support our predictions. The reform has no statistically significant effect on the labor participation and earnings of the unemployed and sick individuals who have a temporary contract but a positive and large effect on those of sick individuals who have a permanent contract. The unemployed and those who have a temporary contract also more often rely on UI.

TABLE 4 Estimated effects of the Work and Income According to Labor Capacity Act (WIA) reform by age, gender, labor market status, and tercile of average earnings.

	DI per month	Wage per month	UI per month	General assistance per month	Other benefits per month
Fell sick before age 35	−30.154*** (6.254)	39.105* (20.160)	8.125*** (2.205)	−0.735 (3.139)	−0.872 (1.178)
Fell sick between ages 35 and 44	−38.546*** (8.230)	32.131 (23.882)	10.364*** (3.359)	2.652 (3.036)	−0.085 (1.802)
Fell sick between ages 45 and 54	−67.858*** (11.340)	48.608* (28.723)	24.231*** (4.618)	−3.018 (2.845)	−6.762* (3.574)
Fell sick after age 54	−57.220*** (11.440)	48.821 (37.289)	32.938*** (8.896)	−3.841 (2.655)	−3.951 (5.488)
Female	−31.855*** (5.742)	16.561 (15.188)	10.194*** (2.454)	−2.102 (2.756)	−2.159* (1.240)
Male	−57.854*** (6.685)	62.461*** (21.612)	19.859*** (2.983)	0.134 (1.696)	−3.439* (2.023)
Permanent contract	−51.260*** (5.501)	76.345*** (20.038)	7.122*** (2.244)	−1.363 (1.417)	−6.490** (1.738)
Temporary contract	−29.003** (12.098)	23.716 (31.060)	21.926*** (4.558)	−5.556 (5.111)	7.263** (2.827)
Unemployed	−52.302*** (11.636)	−40.279 (30.744)	34.318*** (5.827)	6.160 (4.569)	−0.338 (3.747)
Earnings in 1st tercile	−40.104*** (6.464)	25.172* (14.422)	13.768*** (3.436)	−3.791 (4.141)	1.038 (1.461)
Earnings in 2nd tercile	−37.854*** (7.362)	28.147* (16.906)	10.630*** (2.991)	−0.395 (2.057)	−2.529** (1.253)
Earnings in 3rd tercile	−58.881*** (9.501)	74.987** (34.610)	21.258*** (3.818)	1.256 (0.920)	−5.796* (3.343)

Note: Standard errors (in parentheses) account for heteroskedasticity and clustering at the individual level. All regressions control for individual and calendar month fixed effects and use data available for the whole pretreatment period but exclude data corresponding to SI years of the posttreatment period.

***, **, * denote statistical significance at 1%, 5%, and 10%, respectively.

6.3.4 | Pre-sickness earnings

Sick individuals who earn low wages (regardless of their sickness) may have smaller savings or wealth to draw on during sickness to smooth their consumption path. Therefore, they may exhibit stronger responses to compensate for lost DI benefits when they face reform incentives. On the other hand, sick individuals who earn low wages should suffer a smaller negative income shock due to the reform than those who earn higher wages since the amount of the disability benefit depends on the presickness wage. Therefore, the earnings responses of low wage earners may be weaker. Low wage earners may also more often work in jobs where prospects of recovery from ill health are limited. If, as a result, the income shock becomes permanent, they may exhibit weaker responses.

The lower panel of Table 3 presents the estimation results by presickness earnings tercile. Presickness earnings represent the average of the wages earned during 4 years and 9 months before individuals report illness (where data are available). The first and second terciles contain earnings up to €1200 and €2,200, respectively. All earnings groups increase labor participation and UI receipt by similar amounts, but the increases are statistically more significant for UI receipt. Table 4, however, shows that sick individuals in the lower earnings terciles increase monthly earnings and UI substantially less than those in the highest earnings tercile. While those in the highest earnings tercile overcompensate lost DI benefits with increased earnings only, those in the lower earnings terciles just compensate or fail to compensate with increased earnings and UI claims combined. A potential explanation is that high wage earners are able to increase earnings more for a given amount of increase in the number of hours worked due to earning higher hourly wages, and their UI benefits are higher due to higher former earn-

ings (since the amount of UI depends on former earnings). These results are in line with the results based on age, gender and employment status and suggest that labor supply responses are weaker for sick individuals with weaker initial labor market positions.

6.4 | The impact of the transitional WAO reform

As described in Section 2, in 2006, the WIA came into effect, replacing the transitional WAO, the final version of the old system. Compared to the transitional WAO, the WIA introduced major changes in both the SI scheme and the DI scheme that succeeds it. In the preceding sections, we analyzed the impact of changing the transitional WAO scheme to the stricter WIA scheme. Here, we analyze the impact of changing the WAO scheme to the transitional WAO scheme using the same identification strategy and verifying its assumptions (details available upon request) as described in Section 5. Compared to the WAO, the transitional WAO made the definition of what work can still be done by the DI applicant broader, implying tighter eligibility criteria for DI. Table 5 shows that the transitional WAO reform reduced DI awards by 0.9 pp, corresponding to a 4.5% reduction.¹⁹ It increased labor participation by 0.8 pp. This effect is two-thirds as large as the labor participation effect of the WIA reform (Table 2). Given that the WIA reform introduced multiple incentives to limit inflow and stimulate outflow, the one incentive of the transitional WAO reform to limit inflow appears to have been very effective. The decrease in DI awards is almost fully offset by the increase in labor participation, again suggesting that the transitional WAO reform was well targeted. Compared to the WIA reform, the impact of the transitional WAO reform on benefit substitution is smaller when the changes in benefit receipt from all social support programs are considered. Again, this may be taken as an indication that the reform has been effective.

DI receipt	−0.009*** (0.003)
Labor participation	0.008** (0.003)
UI receipt	0.003** (0.001)
General assistance receipt	−0.003** (0.002)
Other benefits receipt	0.001 (0.001)
DI per month	−7.328* (4.375)
Wage per month	32.650** (13.109)
UI per month	5.338*** (1.896)
General assistance per month	−3.531** (1.519)
Other benefits per month	2.416* (1.285)
Observations	9,993,024
Individuals	52,042

TABLE 5 Estimated effects of the transitional Disability Insurance Act (WAO) reform.

Note: Standard errors (in parentheses) account for heteroskedasticity and clustering at the individual level. All regressions control for individual and calendar month fixed effects and use data available for the whole pretreatment period but exclude data corresponding to SI years of the posttreatment period.

***, **, * denote statistical significance at 1%, 5%, and 10%, respectively.

7 | DISCUSSION

In recent decades, DI programs have grown in many Western countries. Governments implement social insurance reforms to reduce enrollment in DI programs and DI benefit costs and to boost labor participation at the same time. The WIA introduced strong incentives for work resumption in both the sickness scheme and the disability scheme that succeeds it. Using a DiD design and unique administrative data on all individuals who reported sick shortly before and after the reform, we analyzed the effect of the WIA on labor participation decisions and use of benefits from alternative benefit programs as well as on earnings and income received from these programs.

Compared to the transitional WAO, as the final version of the WAO, the WIA reduced DI receipt by 5.2 pp (26.1%) in the population of sick-listed workers and the unemployed. Earlier findings based on the same comparison but in the insured population of workers and the unemployed by Van Sonsbeek and Gradus (2013) confirm this. They estimated that the WIA reduced DI receipt by 36% in the first year after the reform compared to the prereform period and found a slightly diminishing effect of the reform over time. We extend the evidence on DI use in several respects and provide a broad picture of the many different effects of the DI reform.

First, we show that the reform increased labor participation by 1.2 pp but also UI receipt by 1.1 pp. These effects are sizable in comparison to the reduction in DI receipt. The 1.1 pp labor participation effect is smaller than the 2.9 pp effect found by Borghans et al. (2014) and the 6.7 pp effect found by Garcia-Mandicó et al. (2020), who analyzed the effects of reexamination of existing DI recipients. The results are not directly comparable, however, since the study populations differ across the studies. The populations in Borghans et al. and Garcia-Mandicó et al. consist of long-term DI beneficiaries, among which only a few were already working during the prereform period (before their reexamination), whereas our population consists of sick-listed workers, of whom the majority were working before sickness and most recovered before they became eligible for DI.

Second, we show substantial effects for monthly earnings and benefits. The reform cohort lost on average € 45.4 per month in DI benefits but increased monthly earnings by € 39.9 and income from UI by € 15.6. This means that sick individuals over-compensate lost DI benefits by increasing earnings (87.8%) and UI claims (34.4%). Borghans et al. (2014) and Garcia-Mandicó et al. (2020) show substantially lower earnings rebounds of 62% and 64%, respectively, whereas the rate of social support substitution is comparable to the 30% found by Borghans et al. Apparently, the fact that the new system intervenes early, that is, before the disability period (the most important changes take place during and at the end of the sickness period), allows for a stronger earnings rebound than a late intervention by means of reexamination of people who are already on DI benefits. These results are a confirmation of one of the most important policy lessons learned in the Netherlands after subsequent reforms of the DI schemes: The earlier an intervention is made after sickness, the better the chances of work resumption.

Third, we show that the impact of the reform has been persistent during the 10 years of the study period. This is an important conclusion from a policy perspective as well because earlier reforms failed to remain effective in the long run. This in particular holds for the major DI reform that took place in 1993, in which medical examinations of new DI claimants and reexaminations of existing DI recipients were based on stricter criteria. Whereas the effect of this reform was sustained (Borghans et al., 2014), the effect of the same reform among new cohorts was short-lived. After an initial sharp drop in annual inflow, the amount of new DI receipts returned to its prereform level within 5 years because of a gradual relaxation of the entrance examinations.

Fourth, the impact of the reform is substantially heterogeneous in age, gender, employment status and income groups. Older individuals appear to be a very vulnerable group. They are not able to compensate for lost DI benefits despite heavily relying on UI. This confirms the weak labor market position and low labor mobility of older workers in the Netherlands (Visser et al., 2018). Furthermore, as the statutory pension age is being raised, later retirement is likely to worsen the labor market position of disabled individuals approaching retirement who struggle the most to utilize their remaining work capacity.

Women appear as another group who struggle to cope with the negative income effect of the DI reform. Their earnings responses are much weaker than those of men, so they struggle to compensate for the lost income from DI. This reflects the relatively weak labor market position of women in the Netherlands, who in majority work part-time, earn lower wages, have smaller pension wealth, and progress slower into management roles. These gaps widen when women parent children, as mothers often reduce the number of hours they work to take on more unpaid care work at home (OECD, 2019b). Policy makers should therefore pay special attention to this group when developing social insurance policies to avoid a widening gender gap.

Individuals who are on a permanent contract are much more able to compensate for the loss of DI benefits with higher earnings than those who are on a temporary contract. This is evidence of labor market segmentation, which in fact has grown stronger over time in the Netherlands, where differences between a relatively well-off and low-risk group of insiders contrast with a group of less wealthy, less healthy and higher risk group of outsiders. Even more vulnerable than the workers with a temporary contract are the unemployed, who lack the employer incentives the group of individuals with permanent or temporary

contracts have to resume working. They are not able at all to compensate lost DI benefits by either wages or alternative benefits. This suggests that unemployed individuals face more limitations in their access to the labor market when they fall sick.

Finally, the earnings responses of individuals with presickness wages in the highest tercile are three times stronger than those of individuals in lower terciles. This result is in line with the results based on age, gender and employment status, all suggesting that responses are weaker for sick individuals with weaker initial labor market positions.

These heterogeneous effects raise inequality concerns and call for attention to vulnerable labor market groups when such a far-reaching reform is implemented, such as older workers with health conditions, women who are often low-income earners, workers who rely on temporary contracts for a long time, and the unemployed. As in the WIA reform, employers are supposed to play an important role in facilitating work resumption, but they are not eager to employ workers from vulnerable groups, especially when there are strong financial incentives for employers to reintegrate if their workers become sick or disabled. Therefore, comprehensive reforms should pay special attention to protecting vulnerable groups from welfare loss.

In fact, the share of vulnerable groups in the Dutch labor force is increasing. The number of workers with temporary contracts increased from approximately 1 to 2 million between 2003 and 2018. Two-thirds of this group preferred a permanent contract in 2016 (Salverda, 2020). Furthermore, the increase in the statutory retirement age since 2013 has increased the share of older people in the labor force. These compositional changes in the labor force suggest that future cohorts of DI applicants will include more people from vulnerable groups. Therefore, it becomes increasingly important to understand how DI reforms, such as the one studied here, affect earnings and benefit claiming and substitution across different segments of the labor force.

The WIA is a complex reform consisting of multiple measures that are implemented at the same time. Therefore, it is difficult to disentangle its overall effect into the effects of individual measures. However, findings from existing studies suggest that the measures implemented in the SI scheme and those regarding DI eligibility substantially reduced DI use and increased labor participation, while the impact of the measures implemented in the DI scheme has been limited. In particular, Hullege and Koning (2018) showed that workers who experience a health shock are less likely to receive disability benefits and more likely to remain employed after both the introduction of the Gatekeeper protocol in 2002 and the extension of the SI scheme in 2004. Strong effects of the introduction of the Gatekeeper protocol were previously found by Van Sonsbeek and Gradus (2013) and later confirmed by Godard et al. (2022), who showed that it reduced DI applications among sick listed individuals by 40%. Van Deursen et al. (2019) showed that increasing the minimum disability grade to become eligible for DI substantially reduced DI inflow but had a limited impact on labor participation.

With respect to the impact of the measures introduced in the DI scheme, Koning and van Sonsbeek (2017) found that reducing benefit generosity as a return-to-work incentive increased labor participation by 2.6 pp among partially disabled workers who, however, only constitute 20% of the WIA population. They found no significant effect on DI use through work resumption. The overall effect of the changes in benefit levels for partially and fully disabled workers can, however, be expected to be marginal since Van Sonsbeek and Gradus (2006) showed that these changes offset each other. Koning (2009) and Van Sonsbeek and Gradus (2013) both showed that introduction of experience rating in DI reduced inflow by approximately 15%. However, as the increased and targeted experience rating for partially disabled workers is offset by the abolishment of it for fully disabled workers, the overall effect of this change can also be expected to be marginal.

The notable impact of the one measure of the transitional WAO reform on DI receipt and labor participation relative to the total impact of the different measures of the WIA reform (Section 6.4) supports that an early intervention during the sickness period allows stronger responses than a later intervention during the disability period. Garcia-Mandicó et al. (2020) showed that reexamination of existing DI beneficiaries, using the same stricter eligibility criteria of the transitional WAO reform, does lead to strong responses in terms of reduced DI receipt and higher employment, but Van Sonsbeek and Gradus (2013) showed that these gross effects are offset by a sharply decreasing recovery rate of the population not affected by the reexamination, resulting in a positive but marginal net effect. Koning and Vethaak (2021) also conclude that implemented policy measures that changed the targeting of the DI scheme in the Netherlands led to stronger effects than those that aimed at changing the labor supply behavior of individuals on disability benefits.

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CONFLICT OF INTEREST STATEMENT

There is no conflict of interest for any of the three authors of the manuscript. Please also see enclosed for each author the conflict of interest statements.

DATA AVAILABILITY STATEMENT

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.

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ENDNOTES

- ¹ DeJong et al. (2011), Deshpande and Li (2019), Markussen et al. (2018), Hulleger and Koning (2018), Liebert (2019), and Godard et al. (2022) analyze the impact of stricter screening for disability during the sickness period. Karlström et al. (2008), Staubli (2011), Borghans et al. (2014), Moore (2015), Autor et al. (2016), and Garcia-Mandicó et al. (2020) analyze the impact of stricter eligibility criteria for DI. Gruber (2000), Campolieti (2004), Marie and Vall Castello (2012), Mullen and Staubli (2016), Deuchert and Eugster (2019), and Favre et al. (2021) analyze the impact of changes in benefit generosity. Weathers and Hemmeter (2011), Campolieti and Riddell (2012), Kostøl and Mogstad (2014), Büttler et al. (2015), Koning and van Sonsbeek (2017), Vall Castelló (2017), and Zaresani (2018); Ruh and Staubli (2019) analyze the impact of introducing return-to-work incentives for workers on benefits, and Koning (2009), Van Sonsbeek and Gradus (2013), and De Groot and Koning (2016) analyze this for employers.
- ² The lack of research on the impact of the WIA is due to the lack of data on individuals who claim SI and could subsequently file DI applications. The SI was reformed in 1994, 1996 and 2004 to mandate that the employer pay 70% of the earnings before sickness during the sickness period. Since no sickness benefit is paid by the government but wages are paid by the employer, there has been no registration of sickness absence by the government since these reforms. New reintegration regulations for employers were introduced in the SI scheme in 2002 (“Gatekeeper protocol”). Only after this year did the government start to register sickness cases to monitor whether employers complied with the new regulations.
- ³ Disability grade is determined by dividing the estimated wage loss due to disability by the presickness wage, where estimated wage loss is given by the difference between the presickness wage and the potential wage that the sick individual can still earn. An ergonomist determines the potential wage by taking the average of the highest wages the sick individual could still earn in three suitable occupations.
- ⁴ Employee Insurance Agency (UWV) is the public body that implements employee insurance policies and provides labor market and data services on behalf of the Ministry of Social Affairs and Employment.
- ⁵ Appendix A presents distributions of sickness duration for individuals who report sick in selected months and participate in the WAO, transitional WAO, and WIA. The left panel presents distributions for the three groups when the number of days spent in SI ranges from 0 to 360, where 360 is the maximum number of days individuals can spend in SI if they participate in the WAO or transitional WAO. The right panel presents distributions when the number of days spent in SI ranges from 180 to 360. Appendix A also presents regression results from samples based on alternative numbers of days spent in sickness.
- ⁶ A flow chart in Appendix B shows the number of observations and individuals excluded at each step of sample selection to construct the estimation sample.
- ⁷ As explained in Section 2, UI is integrated into DI in the WIA, while this is not the case in the (transitional) WAO. Therefore, monthly DI includes UI if the individual is insured under the WIA, and in our definition, we add the UI to DI if the individual is insured under the (transitional) WAO and receives UI in addition to DI. Monthly UI is exclusive of DI. DI and UI receipt are defined in the same manner.
- ⁸ As explained in Section 3, we cannot determine whether or not sick people are working when receiving SI. Therefore, the time trend of the probability of working during the sickness period does not necessarily reflect the true trend.
- ⁹ The maximum was 60 months until October 2006.
- ¹⁰ The time trends and differences between the WIA and WAO groups for monthly earnings and benefits are very similar to those for labor participation and benefit receipt.
- ¹¹ A difference represents the estimated coefficient from the regression of the characteristic as the dependent variable and the treatment indicator (whether member of the transitional WAO or the WIA group) as the explanatory variable.
- ¹² We do not use observations during the SI period due to the measurement issue explained in Section 3.
- ¹³ This group dummy has no time variation and is omitted in the fixed effects regression.
- ¹⁴ The Kolmogorov-Smirnov test of the equality of the distributions for 2003/2004 and 2004/2005 has a *p* value of 0.989.
- ¹⁵ Alternative years of the pretreatment period as the base lead to almost identical results.

- ¹⁶ Following Imbens (2004) and using propensity scores to construct weights leads to almost identical estimates. If we do not apply entropy balancing, estimates are hardly affected since the few observed differences during pretreatment are very small and confined to a limited number of months (see Figure 2).
- ¹⁷ Based on the coefficient estimate of the interaction of the treatment and post dummies, -0.052 (0.003), divided by the coefficient estimate of the post dummy, 0.199 (0.002) (not presented), in Equation (1).
- ¹⁸ As explained in Section 2, this is because the presickness wage is compared to a (lower) fictitious new wage that can be earned given one's disability. A higher presickness wage implies more alternative job opportunities with lower fictitious new wages and hence a higher disability grade and more DI claims.
- ¹⁹ Based on the coefficient estimate of the interaction of the treatment and post dummies, -0.009 (0.003), divided by the coefficient estimate of the post dummy, 0.200 (0.002) (not presented), in Equation (1).

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APPENDIX A: THE NUMBER OF DAYS SPENT IN THE SICKNESS SCHEME: DISTRIBUTIONS AND SENSITIVITY OF THE ESTIMATED REFORM EFFECTS

In Figure A1, we inspect the distributions of the number of days spent in the sickness scheme among three cohorts of sick individuals who participated in the WAO, transitional WAO and WIA schemes. We distinguish between those who spent from 0 to 360 days in the scheme and those who spent from 180 to 360 days. The comparison of the two types of distributions demonstrates that the three cohorts of sick individuals share similar distributions of sickness duration when sickness is restricted to last at least 180 days.

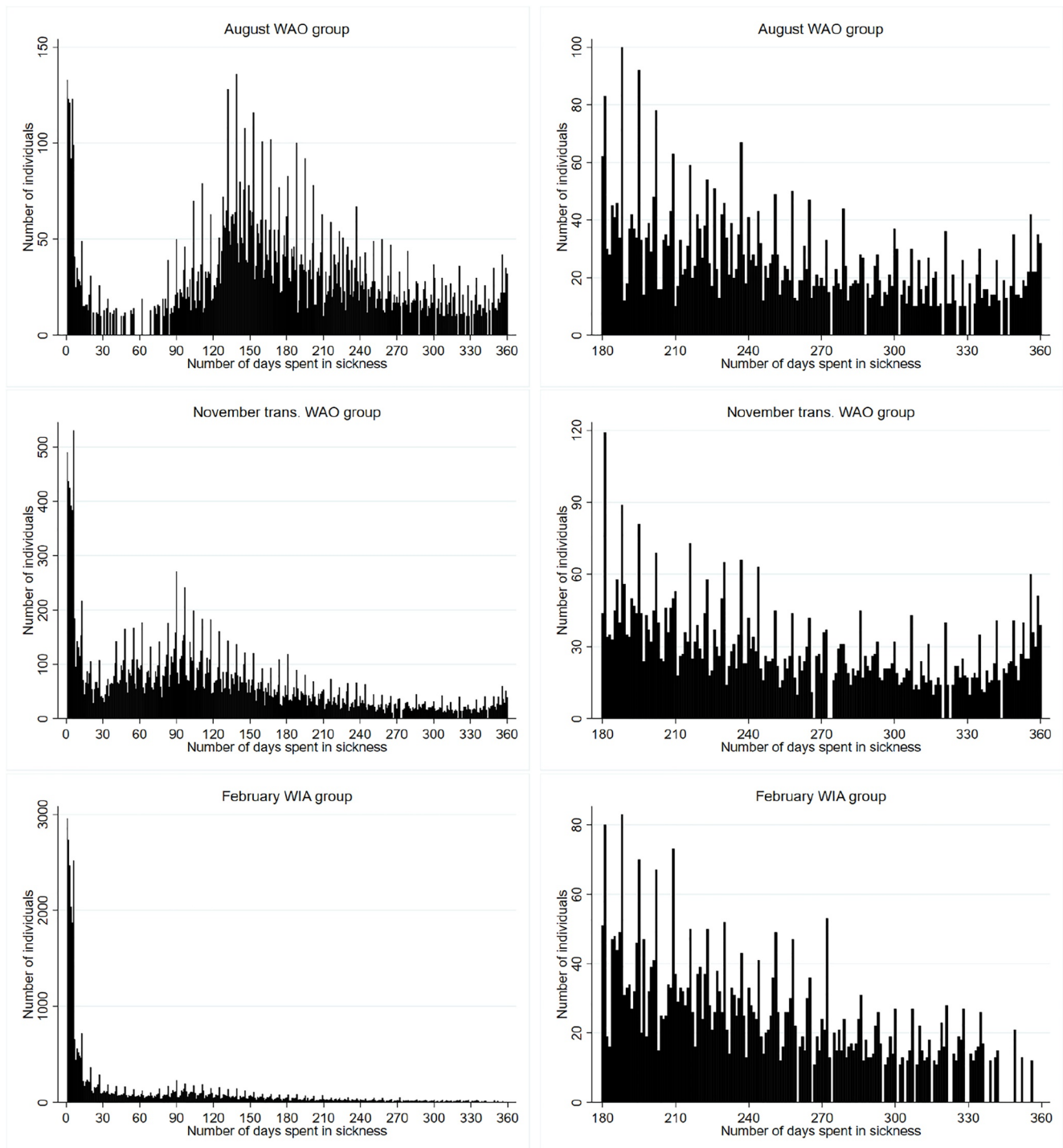


FIGURE A1 Distributions of the number of days spent in sickness insurance (SI) among individuals who reported illness in August 2003, November 2003 and February 2004 and participated in the Disability Insurance Act (WAO), transitional WAO, and Work and Income According to Labor Capacity Act (WIA), respectively. Left panel: distributions for the three groups when the number of days spent in SI ranges from 0 to 360, where 360 is the maximum number of days individuals can spend in SI if they participate in the WAO or the transitional WAO. Right panel: distributions when the number of days spent in SI ranges from 180 to 360. [Colour figure can be viewed at wileyonlinelibrary.com]

As explained in Section 3, since short-term sickness cases are underreported in the WAO group (control group) compared to the WIA group (treatment group), we restricted the initial sample of sick individuals to include only those who have spent at least 180 days in SI to ensure that the control and treatment groups are comparable in the number of days they spend in SI. The estimated effect of the WIA reform could be biased if we allowed the study sample to include individuals who spent less than 180 days in the sickness scheme and are proportionally more often underreported in the control group. In particular, if we allowed the study sample to include them, the control group would be composed of relatively healthier individuals with a higher chance of getting rejected to DI, leading to an underestimation of the reform effect. Here, we examine to what extent the estimated baseline effect of the WIA reform is sensitive to allowing individuals in the study sample who spent less than 180 days and are underreported in the control group. We estimate the baseline regression given by Equation (1) on three subsamples of individuals who have spent at least 90, 120, and 150 days in SI. Table A1 shows that, compared to the baseline results in Table 2, the estimated effect of the reform on labor participation is not affected. The estimated effects of the reform on DI and UI receipt are smaller, as expected. Since the control group is healthier, the estimated effects of the reform on DI and UI receipt are smaller.

TABLE A1 Estimated effects of the WIA reform by the number of days individuals spend in the SI scheme.

	Number of days spent in the sickness scheme is at least 90	Number of days spent in the sickness scheme is at least 120	Number of days spent in the sickness scheme is at least 150
DI receipt	-0.031*** (0.002)	-0.036*** (0.002)	-0.044*** (0.003)
Labor participation	0.012*** (0.003)	0.012*** (0.003)	0.012*** (0.003)
UI receipt	0.007*** (0.001)	0.008*** (0.001)	0.009*** (0.001)
General assistance receipt	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)
Other benefits receipt	-0.003*** (0.001)	-0.004*** (0.001)	-0.005*** (0.001)
DI per month	-26.041*** (2.914)	-31.204*** (3.417)	-37.830*** (3.947)
Wage per month	36.886*** (10.440)	42.838*** (11.785)	41.342*** (12.766)
UI per month	10.618*** (1.444)	11.728*** (1.622)	13.920*** (1.800)
General assistance per month	-1.729 (1.124)	-1.339 (1.248)	-1.181 (1.389)
Other benefits per month	-2.071** (0.867)	-2.391** (0.997)	-2.564** (1.118)

Note: Standard errors (in parentheses) account for heteroskedasticity and clustering at the individual level. All regressions control for individual and calendar month fixed effects and use data available for the whole pretreatment period but exclude data corresponding to SI years of the posttreatment period.

***, **, * denote statistical significance at 1%, 5%, and 10%, respectively.

APPENDIX B: STEPS OF SAMPLE SELECTION

Figure B1 shows the number of observations and individuals excluded at each step of sample selection to construct the estimation sample.

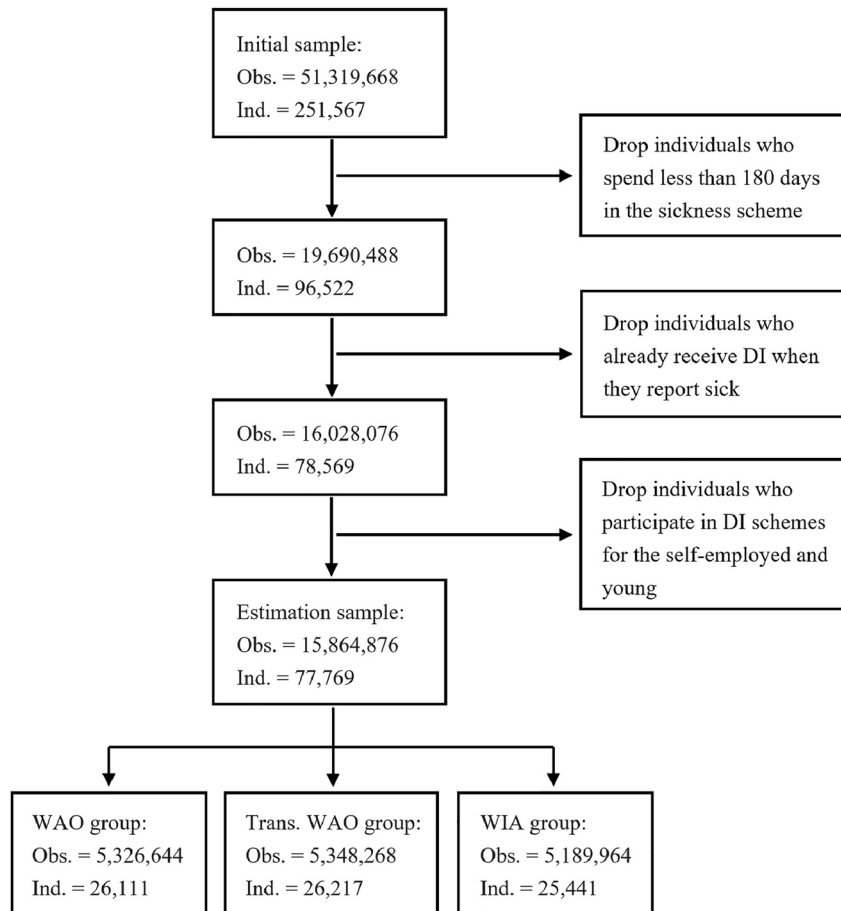


FIGURE B1 Excluded number of observations and individuals at each step of sample selection.