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ORIGINAL PAPER

Assessing the Effects of Reduced Access to Income Support for Disabled Young Adults

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Abstract

We examine the effects of the loss of income support for young adults with a work disability on employment, income, health outcomes, personal debts and criminal behaviour. Our identification strategy uses a 2015 policy reform that drastically restricted access to income support for disabled young adults in the Netherlands (DIYA). Using a fuzzy regression discontinuity (RD) approach that exploits the birth-date cutoff date for new applicant cohorts, we find that the policy change caused the gross income of young disabled adults to decrease by €4,524 per year. The withdrawal of income support payments, amounting to around €10,000 per year, was partly compensated by increased income from welfare benefits (30%) and a higher income from wage earnings (32%). Particularly women experienced a strong reduction, their annual total gross income decreased by €6,012. Based on subgroup analyses of singles and partners, we find suggestive evidence that within-household gender norms are an important driver of this difference. The loss of income support did not significantly affect medical expenditures, personal debts or criminal behaviour.

Keywords Work disability · Income support · Gender gap · Young individuals

JEL Classification I38 · J16 · J68

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1 Introduction

Many developed countries use income support programs specifically targeted at disabled young adults; examples include the United States (SSA), Australia, Denmark, Switzerland, and the Netherlands (Garcia-Mandico et al., 2022). These income support programs provide a safety net for vulnerable young adults who typically face significant barriers to participate in the labour market. As with most social insurance programs, however, income support may also induce disincentives to work. With potential path-dependence of early-age decisions, this may then lead to a long-term reliance on income support (Verbruggen et al., 2015b). Taking a broader welfare perspective, any adverse employment effects of income support for young disabled individuals should be balanced with potentially positive effects on other life domains, such as mental and physical health, debt formation and criminal behaviour – see e.g. Deshpande and Mueller-Smith (2022). This calls for empirical analyses that incorporate broader welfare effects that go beyond employment and earnings.

This paper provides insight into the various behavioural responses of young disabled individuals to reduced access to income support. We use a drastic reform of the Disability Insurance scheme for Young Adults (DIYA; in Dutch: "Wajong") in the Netherlands in 2015 to estimate the causal effects of relatively generous income support on employment, disposable income, medical consumption, the incidence of problematic debts and criminal outcomes. In light of the increasing inflow rates in the program at that time, as from January 2015 the Dutch government terminated the access to DIYA for disabled young adults with positive (expected) work capacity. In case of insufficient earnings from work, disabled young adults with capacity to work should apply for less generous welfare benefits instead. Monthly welfare benefits were between €300 (for 18 years old) to €650 (for 20 years old) lower than DIYA benefits. Moreover, since welfare benefits are means- and asset-tested, not all disabled young adults continued receiving any income support.

We exploit the discontinuous change in DIYA benefit conditions for cohorts who turned 18 in the year before and after the exact date of the reform (i.e. January 1, 2015). Using a fuzzy regression-discontinuity (RD) design, we acknowledge the fact that the reform in 2015 substantially reduced access to the DIYA, but not for all individuals. Since enrolment to the new DIYA scheme was based on date of birth of potential applicants, anticipation effects to the reform are ruled out. To increase the relevance of our instrument, we limit our sample to young adults who attended special education – targeted at children with special needs – in the cohort years 2014 or 2015. This way we capture around 85% of the annual DIYA inflow, consisting of 18-year-olds who have followed or still are following special secondary education (SSE) or practical education (PE).

In brief, our results can be summarized as follows. First, we find that young adults with a work disability compensated 32% of the income loss from DIYA benefits with increased earnings and 30% with replacing welfare benefits. The policy reform led to an average annual net income loss of €4,524, corresponding to around 40% of the lost DIYA benefits. Second, the annual income loss amounted to €6,012 for women and €3,518 for men. This difference mirrors the effect of the lower earnings responses to the reduced access to DIYA of women. The gender difference was most substantial

among young adults living with a partner. This suggests that within-household gender norms are an important driver of the differential impact on men and women. For women with partners, the replacement of income by (means-tested) welfare benefits was limited as well, reducing the potentially mitigating impact from this source of income. Third, the strongest employment responses were among young adults with limited intellectual disabilities and practical education. In contrast, individuals with mental or behavioural problems showed small employment responses and therefore experienced more substantial decreases in their total income. Finally, we estimate null effects on other domains of life than benefits, income and employment: including criminal activity, health outcomes, or problematic debts. We argue that the additional income from welfare benefits and increased participation in work played a mitigating effect.

We provide new evidence on the effects of reduced income support on income and employment for an understudied and vulnerable group on the labour market. So far, the literature on the work incentive effects of Disability Insurance (DI) benefit receipt has predominantly focused on the employment outcomes of workers, while the specific group of young disabled adults without work history has largely been neglected. For workers with disabilities, the employment effects of DI benefit receipt have typically been studied by comparing outcomes of approved and rejected disability benefit claimants (Bound, 1989; Chen & van der Klaauw, 2008; French & Song, 2014; Wachter et al., 2011; Gelber et al., 2017; Koning et al., 2022) or by instrumenting DI receipt with examiners' allowance rates (Maestas et al., 2013; French & Song, 2014). Overall, these studies indicate that a one-dollar increase in DI benefits leads to a decrease in earnings between $0.20 and $0.30. For the group of young adults, however, the evidence is limited to the US Supplemental Security Income (SSI) program. Deshpande (2016a, b) uses regression discontinuity (RD) models to estimate the effects of reduced access to SSI. She finds that young adults who were removed from the SSI at age 18 recover 38% of the lost SSI income with earnings, and without significant differences in responses between men and women. In addition, earnings responses to reduced access to SSI differ by disability type: those with intellectual disabilities compensated a larger fraction of the lost benefits with income from earnings.

Compared to the earlier work on the effects of SSI removal, the current study examines a more drastic policy reform: while the relative income effects of the loss of DIYA benefits was comparable to that of the SSI, the probability of access to disability benefits was much more curtailed than in the reform studied by Deshpande. In comparison, the probability of receiving DIYA for the 2015 cohort in the years 2016–2021 was on average around 40% points lower than for the 2014 cohort, while the study of Deshpande shows a removal from SSI of 10% points over 16 years after the reform. Similar to Deshpande (2016a) we find the strongest earnings responses among individuals with intellectual disabilities, but in contrast to her results we do find important gender differences in the earnings response to the loss of benefit eligibility.

As a second contribution to the literature, we broaden the scope of effects of income retraction for young disabled adults to life domains that go beyond employment and benefit receipt. This includes health outcomes, criminal behaviour and the

presence of problematic debts. For health outcomes, there is virtually no existing evidence on the effects of income support for the specific group of young disabled individuals. For older disabled individuals, existing studies point at mental health improvements due to work, either self-reported or derived from administrative data on mental health costs (Burns et al., 2007; Huber et al., 2011; Van Eijkel et al., 2021). Thus, if the reform would lead to higher employment and mechanisms are similar for younger individuals as for the larger population of disabled individuals, it might lead to better mental health among the targeted population. At the same time, however, lower and more unstable income from earnings due to the reform might also lead to opposite effects, rendering the overall expected effects ambiguous. Alongside this, a follow-up paper by Deshpande and Mueller-Smith (2022) finds that income support removal had adverse effects on criminal behaviour. Following the removal from SSI benefits, the number of criminal charges increased by a statistically significant 20% in the two following decades. Verbruggen et al. (2015a) investigate the impact of employment and income support (welfare, DIYA, and unemployment benefits) on the criminal behaviour of young men and women who were institutionalized in a Dutch juvenile justice institution in the 1990s. They find that both employment and income support reduced criminal behaviour among men, but not for women. So for both mental health and criminal behaviour, the simultaneous loss of income from benefits but increases in earnings may exert ambiguous effects.

Finally, our results add to a large literature on gender differences in labour market behaviours. In their survey article, Cortés and Pan (2023) connect the persistent gender disparities in the labour market to the different roles men and women fulfil within the household, particularly when raising children. Meekes and Hassink (2022) find that displaced women due to firm bankruptcies show stronger decreases in working hours, have longer unemployment durations, and higher hourly wage losses than men. They argue that women are less likely to increase their commuting times after job loss. Although firm closures are not relevant for young disabled adults, it may be that men in this group also show stronger flexibility in responding to lower income receipt from benefits. Our findings also align with the systematic literature review of Lindsay et al. (2017) on gender differences in employment among disabled young adults. Of their 48 included studies, 21 reported better results for men than women with regard to securing and maintaining employment, and only five reported better employment outcomes for women. Our findings also resonate with the observation that the gender employment gap is the highest for individuals with low educational attainment (Eurostat, 2021). As the individuals under study typically have lower levels of education, a reduction of income support for this group may exacerbate the gender income gap.

In what follows, Sect. 2 describes the institutional context of the DIYA programme and its 2015 reform. Section 3 outlines the data and provides descriptives. Section 4 presents the empirical strategy and main results of the analysis, and Sect. 5 ends with discussion.

2 Institutional Context

The two most important disability benefit programs in the Netherlands are disability insurance (DI) benefits for employed workers and DIYA benefits for young adults without employment history. The eligibility to and level of DI benefits depends on the actual loss of earning capacity, where the level essentially replaces part of the pre-disability wage earnings. DIYA benefits are linked to the statutory minimum wage instead. Both schemes are administered by the Employee Insurance Agency (EIA). DIYA benefits are available to those who were born disabled or became disabled before the age of 18, as well as students who became disabled before the age of 30. The vast majority of applicants applies when they turn 18.

Until 2015, disabled young individuals without work history were entitled to DIYA unless they could earn at least 75% of the statutory minimum wage. The EIA conducted medical assessments to establish this – and still does so. The DIYA benefit level amounted to 75% of the minimum wage for those without any expected (future) capacity to work, and 70% of the minimum wage for those with some expected (future) capacity to work. Benefit eligibility ended at the statutory retirement age (and still does so), or ended if recipients worked for five years in a row and earned at least 75% of the minimum wage.

The annual inflow into the DIYA program increased from around 10,000 individuals up to 2005 to around 18,000 individuals as from 2009 (see Fig. 1). Concurrent with this, total enrolment increased from 100,000 to 250,000 individuals between 2006 and 2014. In part, the increase in the inflow was driven by a reform in 2004 that implied a switch to lump-sum financing of welfare expenditures for most municipalities instead of the receipt of government reimbursements. The resulting incentive for municipalities led to a 14% decline of the welfare caseloads in the period 2004–2008 (Kok et al., 2017), partly driven by substitution into the DIYA scheme (Roelofs and Van Vuuren, 2017).

To curb the rapidly rising number of DIYA recipients, the government decided to terminate access to DIYA for young adults who were deemed to have a positive earn-

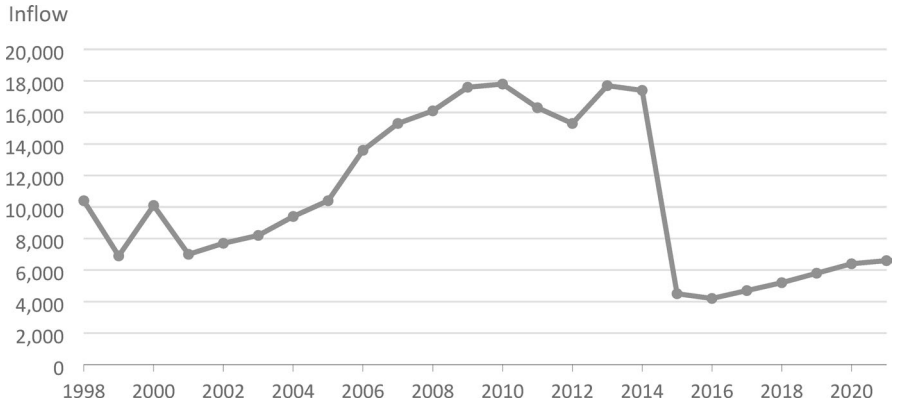

Fig. 1 Annual inflow into the DIYA program. Annual inflow in DIYA decreased sharply after the restriction of the inflow in DIYA in 2015. Source: Statistics Netherlands

ings capacity as of January 1st, 2015. This means that those who become 18 years of age after January 1st 2015 (i.e.: born after January 1st 1997) are only entitled to DIYA if they do not have any current or future work capacity.[1] Fig. 1 shows that this led to a sharp and instantaneous decrease in the inflow into DIYA, from around 16,000 individuals to around 4,200 persons per year. Unemployed disabled persons with work capacity are entitled to welfare benefits, amounting to 70% of the statutory minimum wage for single adults. The benefit level equals the level of DIYA benefits under the old regime, but the impact of means- and asset testing is considerably stronger. Welfare benefits are means-tested on one's own income from earnings, partner income, and assets, while the DIYA scheme is only means-tested on one's own income from earnings. Welfare benefits are not tested on parental income, regardless whether the recipient lives with his/her parents or not.

Table 1 shows the monthly gross income under the DIYA scheme and welfare benefits – both measured in 2020.[2] DIYA benefits for an individual with capacity to work would equal 70% of the statutory minimum wage under the old regime. For persons aged 18–20 years, this amounts to a gross monthly income of €579 to €926 in 2020 depending on their age, and €1,158 per month from the age of 21 and older. In 2020 the monthly welfare benefit for all individuals aged 18–20 amounted to €260 and €1,158 for singles at the age of 21 and older. Welfare recipients of age 21 and older living with a partner receive €827 per month, and those living with two other persons receive €717. As stated earlier, DIYA benefits are means-tested on one's own

Table 1 Monthly gross income under DIYA and welfare in 2020 for disabled young adults with capacity to work

	DIYA	Welfare
Age 18–20:		
Living with parents	€579–€926	€260
Living alone	€579–€926	€260
Living as a couple, both younger than 21	€579–€926	€260
Students	€207–€331	On average €110
Age 21 and older:		
Living with parents	€1,158	€717
Living alone	€1,158	€1,158
Living as a couple, both older than 21	€1,158	€827
Students	€414	On average €110

Data from https://zoek.officielebekendmakingen.nl/stcrt-2019-56680. Rijksoverheid: Normenbrief January 1st 2020 (in Dutch). Numbers shown are per individual. DIYA gross benefits are calculated as 70% of the gross statutory minimum wage. Older individuals have a higher statutory minimum wage. Gross welfare benefits are calculated as 70% of the gross minimum wage for 21 and older living alone and 43.3% of the gross minimum wage for 21 and older living with their parents. We assume there are no other siblings living in the household; with more siblings older than 21, the benefit level will be lower. Welfare recipients living as a couple receive 50% of the minimum wage each

[1] Students are not eligible for the new DIYA, but can enter after their study (if they do not have any current or future work capacity).

[2] When taking 2015 as a reference year for the table, individuals would still be enrolled in education and not have entered the labour market. This is why we chose 2020 as a reference year.

earnings from work only, and welfare benefits are means-tested for all sources of own income, partner income, and assets.

Both DIYA and welfare recipients can keep a part of their income from work. For welfare recipients with a disability this comprises 15% of earned income, with a maximum amount of around € 195 per month for a 21-years-old. DIYA recipients could keep part of their income if they earned more than 20% of the minimum wage. If they earned more than 20% of the minimum wage, the amount they could keep gradually increased to a maximum of € 447 up to the level of 80% of the minimum wage. At the level of 80% of the minimum wage, their benefit was set equal to 20%. If they earned more than 75% of the minimum wage for five consecutive years, they lost their DIYA entitlement all together. Overall, the DIYA thus provided incentives to work more hours – but only resulting in earnings up to 80% of the minimum wage for a maximum of five years.

To gauge the incentive effects of the DIYA reform, Fig. 2 graphically explores the consequences of the DIYA reform on income budget lines in 2020 for three groups of young disabled adults: 18 to 20-year-olds, 21-year-olds and older living at parent's home and 21-year-olds and older living on their own. The figure shows the incentives of the old DIYA scheme (relevant for the 2014 cohort) compared to the incentives of a welfare benefit for those who do not qualify for DIYA in 2015 (relevant for the 2015 cohort). Most notably, the figure shows that the new regime created strong income incentives to work fulltime. This particularly holds for individuals between 18 and 20 years (see part a of Fig. 2), who are not eligible for welfare benefits when earning €305 per month or more. For older individuals living with parents ore alone (see part b and c of Fig. 2) the difference in incentives are smaller, as welfare and DIYA benefits are more equal. Obviously, individuals who do not receive welfare benefits, for instance because they do not apply for welfare or have a working partner, have the strongest incentive as they can keep all their earnings.

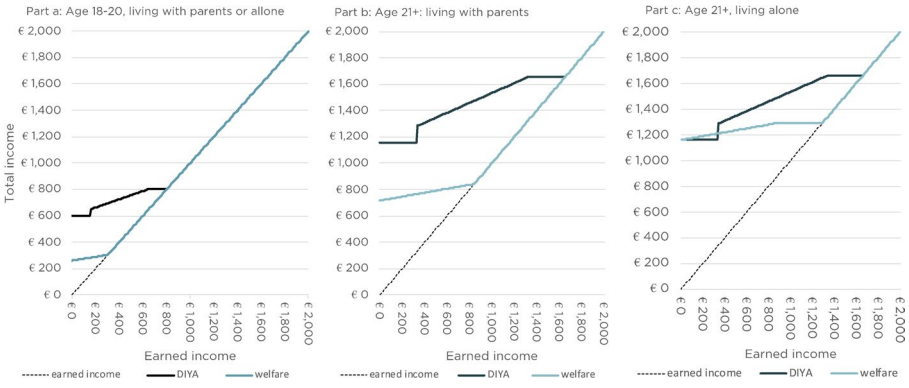

Fig. 2 Earned income and total income per month for those on welfare versus DIYA (2020)
Notes: Individuals on welfare have a smaller incentive to earn income than those with DIYA benefits if earned income is higher than 20% of the age-dependent minimum wage and lower than the maximum income with a welfare benefit, which is €305 for those aged 18–20 (€260 plus €45 of their earned income, left panel), €847 (€717 plus €130 of their earned income) for those aged 21 and older living with parents (middle panel), and €1288 (€1158 plus € 130 of their earned income) for those living alone (right panel)null

Although the picture for the other two groups aged 21 and older is more ambiguous than for the younger group, increased work incentives from a lower income (welfare or no benefit income at all as compared to DIYA-income) due to the reform are also likely to be most important. Same as for the youngest group, income effects are clearly stronger for welfare benefits than for the more generous DIYA benefits over the full support of wage earnings. In terms of earnings increases, however, we see that potentially discouraging substitution effects from benefit reductions are stronger for welfare benefits than for DIYA benefits. These substitution effects may offset (part of) the difference in the discouraging impact of income effects, but their impact is most likely smaller and confined only to situations where individuals opt for part time work. Overall, we therefore expect a positive impact of the policy reform on extensive margin employment.

3 Data

For our analysis we merge confidential individual administrative data from the EIA, municipalities, the tax office, the population registration, health insurers, and financial organizations (for information on debts), as well as law enforcement data from the ministry of justice. Statistics Netherlands provides these data within a secured environment. Throughout our analysis, we focus on two cohorts of 18-year-olds: one that completed Special Secondary Education (SSE) or Practical Education (PE) in 2014 and the other that did so in 2015. About 50% of the 2014 cohort entered the DIYA program in 2014, while 5% of the 2015 cohort did so in 2015. We follow these cohorts in the time frame between 2015 and 2021.[3]

The resulting data includes information on school type, demographics, employment status[4], financial situation, healthcare use, criminal activity, and household status. As to the school types, PE is targeted at children with an IQ level between 55 and 80. These students with limited intellectual abilities are trained for professions in sectors such as construction, metal working, gardening, retail, catering, logistics, and care. SSE students are registered to have one of the following conditions: (i) Visually impaired, hearing impaired, or language-speech development disorders; (ii) Physical and/or mental disabilities, and long-term illnesses; and (iii) Psychological disorders and behavioural problems. Demographics include age, gender, migration history, parental information and household status. Financial data in our sample include earnings from work, total income, benefits (all expressed in prices 2020) and problematic debts. Persons are defined as having problematic debts when they have been in arrears for several months (depending on the kind of debt) and have overdue payments for government agencies, health insurers or if they are participating in a debt relief program. Healthcare use includes public medical expenditure on mental care, medicines for mental disorders, home care, and youth care. Criminal behaviour

[3] In order to conduct placebo analyses, we also construct samples for SSE cohorts of 2012 and 2013.

[4] Employment status is defined as having a paid job at the end of a calendar year, including sheltered work.

is inferred from being registered as suspect of a criminal offence for crimes such as theft, burglary, fraud, vandalism, public violence, and sexual offences.

Table 2 provides an overview of the data at hand for the 2014 and 2015 cohorts. The cohorts are very comparable in size and in composition.[5] Two-thirds of both cohorts are male, and one-third of each is diagnosed to have special home care needs for more than 10 h a week in the year before turning 18. There is a significant difference between the two cohorts in the share of those with psychological disorders and behavioural problems. Furthermore, the 2014 cohort is more often suspected of theft, assault, and vandalism, but this difference is not significant, see Table 3.

Part a of Fig. 3 illustrates the much lower DIYA enrolment rate for the 2015 cohort. The steep decline in the second half of 2015 follows from a stipulation that applications for new benefits needed to be received by the EIA 16 weeks before the actual eligibility started. This means that disabled individuals should have applied no later than September 10, 2014, in order to receive benefits under the old regime. In effect, applicants who turned 18 before the cut-off date were eligible for the old DIYA program, unless they filed their application too late (after September 10, 2014). This led to a steep decline of DIYA-recipients for those turning 18 in the second half of 2014, see Fig. 3. However, since we use date of birth as a cut-off in our research design and date of birth cannot be manipulated, our design is not influenced by the fact that late applications were not considered by the EIA.

On average, 51% of the 2014 cohort was awarded DIYA benefits in 2014, against 5% in 2015. Note that the difference in DIYA receipt between the cohorts decreased to 31% points in 2021, reflecting the fact that the DIYA recipience of the 2015 cohort gradually increased between 2016 and 2021.[6] Panel (b) of Fig. 3 shows that the decrease in DIYA recipiency rates between the cohorts is partly compensated by an increase in welfare benefits: the share of welfare recipients increased in 2021 from 8% for the 2014 cohort to 14% for the 2015 cohort.[7]

Figure 4 makes apparent that the reduced access to DIYA benefits is mirrored by lower total incomes of the reform cohort, together with the potentially mitigating impact of increases in wage earnings. The figure also shows that in 2021 – when the cohorts reach the age of 24 and 25, respectively – the baseline level of earnings and income are substantially higher than in 2016. Also in 2021 we observe a much lower total income for the 2015 cohort as compared to the 2014 cohort; this effect is partly compensated by an increase in earnings from wages of the 2015 cohort.

Finally, Fig. 5 shows that the use of mental health care and criminal behaviour declines with age: both seem to be lower in 2021 compared to 2016 (except for one outlier in the use of mental health). However, it is difficult to draw conclusions about

[5] Note that Table 3 provides a formal covariate balance test to compare characteristics of the treatment and control groups in our sample.

[6] This increase is in line with changes in the subgroup of individuals without any capacity to work in of 2014 cohort. Of the 2014 cohort, 7% was diagnosed as having no capacity to work at the age of 18, and would therefore still be entitled to the new DIYA scheme of 2015. This fraction increased to 19% at the age of 24 as they finished their studies or when their impairments became diagnosed as permanent.

[7] In our empirical approach, we will estimate first-stage effects on the average DI receipt in the years after application. As such, we incorporate the lower effective cut-off effect of the reform in later years. We will also conduct a robustness test that only takes the first-year DI award as the first-stage outcome.

the effect of the reform on the use of mental health care and crime rates from this figure, as the patterns are very volatile.

Table 2 Descriptive statistics of the 2014 and 2015 18-year-old cohorts (fractions)

	2014 cohort	2015 cohort
Male	0.66	0.64
Household status (in the year before turning 18):		
Living with parents	0.83	0.84
Living alone	0.04	0.05
Living with partner	0.01	0.00
Living in institution	0.10	0.08
Other living situation	0.03	0.03
Migration status:		
No migration background	0.68	0.68
Migrant from non-western countries	0.25	0.25
Migrant from western countries	0.07	0.07
Health care use (in the two years before turning 18):		
Using medicines for ADHD	0.10	0.10
Special home care for more than 10 h per week	0.35	0.33
Using mental health care	0.24	0.24
Ambulatory youth care	0.12	0.12
Supervision order (child protection)	0.14	0.13
Youth probation	0.09	0.07
Criminal history (in the seven years before turning 18):		
Theft suspect	0.16	0.14
Assault suspect	0.15	0.12
Suspect of any crime (only year before turning 18)	0.11	0.10
Education:		
Enrolled in practical education (PE):		
Limited intellectual capabilities	0.44	0.44
Enrolled in secondary special education (SSE):	0.56	0.56
Visually impaired, hearing impaired disorder	0.03	0.03
Physical and/or mental disabilities	0.22	0.22
Psychological disorders and behavioural problems	0.31	0.28
Impairment unknown	0.00	0.04
Family characteristics:		
Father unknown	0.14	0.14
Mother receiving welfare in 2013/2014	0.17	0.17
Municipality size		
Small < 15,000	0.04	0.03
Medium > 15,000 and < 40,000	0.29	0.29
Large > 40,000	0.67	0.67
Observations	9,523	9,699

Source: Statistics Netherlands, own calculations

Table 3 Covariate balancing test – reduced form RD effects on explanatories

	Linear specification			Quadratic specification		
	Coefficient	SE	Standardized difference	Coefficient	SE	Standardized difference
Female	0.030	(0.0295)	0.05	0.028*	(0.0160)	0.05
Migration background	0.112	(0.1250)	0.05	0.105	(0.0678)	0.04
Mother receiving welfare	0.008	(0.0234)	0.02	0.007	(0.0127)	0.02
Father unknown	-0.007	(0.0211)	-0.02	-0.009	(0.0114)	-0.02
Enrolled in practical education: limited intellectual capabilities	0.015	(0.0304)	0.02	0.001	(0.0165)	0.00
Enrolled in secondary special education:						
-Visually impaired, hearing impaired disorder	-0.017*	(0.0101)	-0.10	-0.018***	(0.0055)	-0.10
-Physical and/or mental disabilities	0.038	(0.0262)	0.08	0.052***	(0.0141)	0.11
-Psychological disorders and behavioural problems	-0.071***	(0.0277)	-0.13	-0.072***	(0.0150)	-0.13
Using medicines for ADHD	-0.019	(0.0182)	-0.06	-0.040***	(0.0099)	-0.12
Ambulatory youth care	-0.036**	(0.0180)	-0.12	-0.024**	(0.0097)	-0.08
Special health care needs (AWBZ) for more than 10 h per week	0.058**	(0.0291)	0.10	0.038**	(0.0158)	0.07
Supervision order (child protection)	-0.022	(0.0198)	-0.06	-0.020*	(0.0107)	-0.06
Theft suspect	0.032	(0.0218)	0.08	0.012	(0.0118)	0.03
Assault suspect	0.013	(0.0209)	0.03	0.001	(0.0113)	0.00
Municipality size						
-small	-0.017	(0.116)	-0.01	-0.140**	(0.0062)	-0.01
-middle	-0.000	(0.0279)	0.00	-0.010	(0.0151)	-0.02
-large	0.017	(0.0288)	0.02	0.0239	(0.0156)	0.03
Observations	19,222			19,222		

Source: Statistics Netherlands, own calculations

No controls. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

4 Empirical Strategy

Similar to Deshpande (2016a), we employ a fuzzy regression discontinuity (RD) design that exploits the discrete regime change for cohorts turning 18 in 2015. As explained earlier, we focus specifically on two cohorts of 18-year-olds with high fractions of young adults with work limitations: one that completed SSE or PE in 2014 and the other in 2015. Since the new DIYA scheme has been restricted to applicants without any earnings capacity, we use a fuzzy RD design where the probability of DIYA receipt decreased substantially – but not to zero – in January 2015 (Angrist & Pischke, 2009). We use a (full) bandwidth of 12 birth-months cohorts before and 12

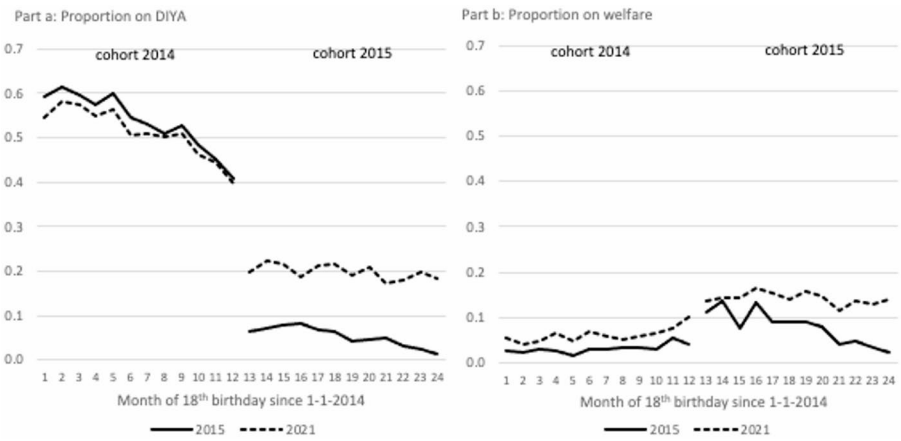

Fig. 3 Receipt of DIYA and welfare benefits by cohort (end of year). DIYA and welfare recipience are measured at the end of year 2015 and 2021. The proportion on DIYA (part a) decreases in 2015 from on average 51% for cohort 2014 to 5% for cohort 2015. In 2021 the proportion on DIYA increases from 5% in 2016 to around 20% in 2021.The proportion on welfare (part b) is higher for cohort 2015 compared to cohort 2014, as well in 2015 as in 2021. Source: Statistics Netherlands, own calculations

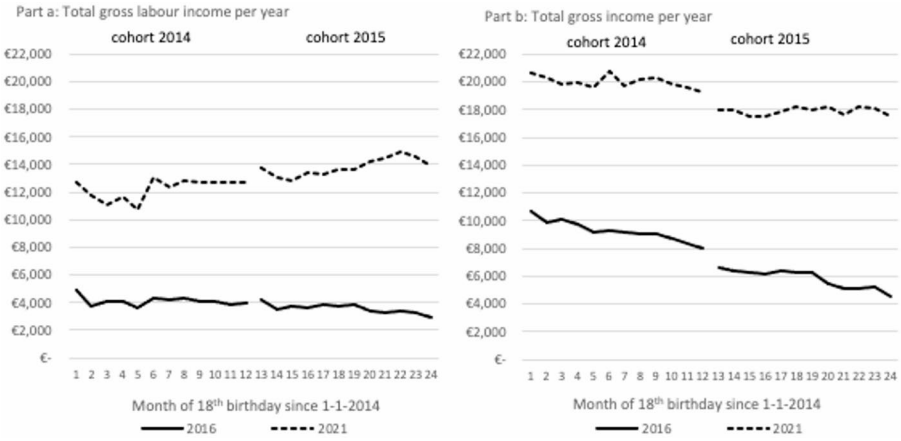

Fig. 4 Total gross income and income from work by cohort. Gross labour income (part a) increases from 2016 to 2021, partly due to aging. The increase is more prominent for cohort 2015. Total income (part b) is lower for the 2015 cohort, due to the policy change. Source: Statistics Netherlands, own calculations

birth-months after the cutoff date. As a robustness test, we will also conduct regressions based on smaller bandwidths.[8]

[8] Table 10 in the appendix to the paper presents estimation results with smaller bandwidths. The results show that smaller bandwidths lead to substantially higher standard errors, but not qualitatively different results. Note that we also have declining number of DIYA recipients at the end of 2014 (see also Fig. 3), rendering our estimates (disproportionally) less efficient with a small bandwidth.

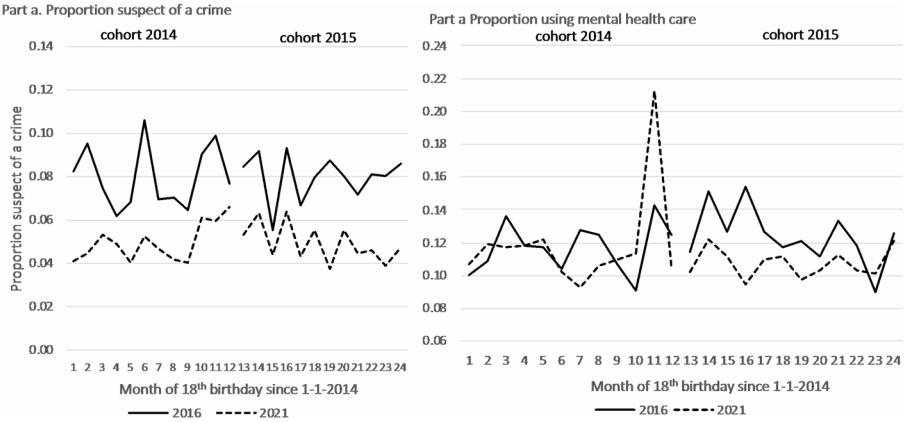

Fig. 5 Proportion suspect of a crime and using mental health care by cohort
Source: Statistics Netherlands, own calculations

In our empirical approach, we estimate pooled first-stage and second stage effects on the average DI receipt in the six years after application and on the average of various outcome variables during this same period. Following Deshpande (2016a), this provides a clearer overview of the results as compared to year-by-year estimates. A robustness test that only takes the first-year DI-receipt as the first-stage outcome gives results that are in line with the pooled estimates. We also calculate results for separate years, which are generally well in line with the pooled results.

Following from the fuzzy RD framework with the discrete cohort time effect as an instrumental variable (IV), the model specification we use consists of two stages for (1) the (non-) *DIYA* recipience rate and (2) the outcome variable of interest, *Y* for individual *i* with month-birth cohort τ in year *t*:

$$NDIYA_{it,\tau} = \alpha_1 + \beta_1\, cohort2015_i + f(\tau_i) + cohort2015_i * f^{+}(\tau_i) + X_i\gamma_1 + \Theta_1(t) + \epsilon_{it} \quad (1)$$

$$Y_{it,\tau} = \alpha_2 + \beta_2\, \widehat{NDIYA}_{it,\tau} + g(\tau_i) + cohort2015_{i,\tau} * g^{+}(\tau_i) + X_i\gamma_2 + \Theta_2(t) + \nu_{it} \quad (2)$$

In Eq. (1), $NDIYA_{it}$ is an indicator function equal to one when individual *i* from birth-month cohort τ is *not* enrolled in DIYA in year *t*, and zero otherwise. The birth-month indicator τ ranges from one if born in January 2014 to 24 if born in December 2015. $cohort2015_i$ is a dummy variable that equals one if a person became 18 in 2015 ($\tau > 12$), and equals zero if a person became 18 in 2014 ($\tau \leq 12$). The first-stage coefficient β_1 describes the reform effect on the non-receipt of *DIYA*. Inherent with the RD design, the polynomial functions *f* and f^{+} control for birth-month effects at the left- and right hand-side of the cutoff date of January 2015. X_i is a vector with time-constant individual characteristics in the year before turning 18, including gender, migration status, family characteristics, municipality size[9] and healthcare use, criminal outcomes, and school type. $\Theta_1(t)$ represents year-specific effects to incor-

[9] We add municipality size since lump-sum financing of welfare expenditures for large municipalities in the Netherlands creates a higher incentive for them to keep welfare caseload low and incentivize their

porate the (baseline) effect of increasing ages and potential other time effects on the (non-)DIYA status.

Equation (2) represents the second-stage regression. β_2 displays the LATE estimate of DIYA status on the outcome variable of interest Y_{it}. That is, the parameter β_2 represents the effect of reduced access to DIYA on various outcome variables over the subsequent six years. Similar to Eq. (1), the polynomial functions g and g^+ control for asymmetric and continuous birth-month cohort effects, and γ_2 gives the effects of X_i. Given that we pool individual-year observations on outcomes and controls, we again add yearly calendar-time specific effects $\Theta_2(t)$ to the model. We cluster standard errors at the level of the individuals.

Interpreting β_2 as the causal effect of reduced DIYA receipt requires four conditions to hold: (i) relevance; (ii) exclusiveness; (iii) independence and (iv) monotonicity of the reform effect. The relevance condition states that the birth-cohort cut-off should have a substantial effect on DIYA status. As we will show in Sect. 5, this condition is satisfied in our benchmark model and in all other specifications we consider. The exclusiveness condition states that any birth-month effects at the cutoff date on the outcome variables should only run through changes in DIYA status. We regard it unlikely that this condition is violated, since access to any of the related schemes is not linked to similar birth-cohort conditions. Still, we test for this with placebo analyses that use January 1, 2013 as an alternative cutoff date. We will report the results of this test at the end of Sect. 5.

In our context, the independence assumption states that turning 18 before or after the cut-off should be balanced with respect to individual characteristics. We can test for this by estimating Eq. (1) on variables in X_i as outcome variables and without further controls. Our interest lies in any discrete cohort effects at January 2015. The results of these balancing tests are presented in Table 3, both with a linear and a quadratic specification for continuous birth-month effects around the cut-off date. As the linear estimation shows a higher number of balanced coefficients, we use the linear specification in our preferred estimates. Based on the t-values for significance, the table shows that most of our covariates are balanced at the cut-off under the linear specification – and become imbalanced under the quadratic specification. The table also reports the standardized coefficients, which are all well below the threshold value of 0.25 in both the linear and the quadratic specification.[10] All together, we use the linear specification in our preferred estimates.

Finally, the monotonicity condition requires that our instrumental variable affects DIYA status of heterogenous groups in one and the same direction. From the subgroup analyses that we will conduct and present in the next section, we demonstrate that this condition is most likely satisfied: for all subgroups we obtain first-stage estimates ranging between 30 and 60% points. This confirms the fact that all subgroups we distinguish have substantial fractions of potential applicants with some capacity to work.

inhabitants to work more – see Kok et al. (2017) for more detailed information on the incentives for municipalities in the welfare system.

[10] The standardized difference normalizes the mean difference by the pooled standard deviation of the outcomes of the treatment and control groups (Imbens and Rubin, 2015).

5 Estimation Results

5.1 Main Findings

Table 4 presents the IV-estimation results of Eqs. (1) and (2) for all outcome variables. For a more extensive overview of the first-stage estimates, we refer to Table 8 in the appendix. The upper panel of Table 4 shows that the probability of receiving DIYA for the 2015 cohort in subsequent years was on average around 40% points lower than for the cohort 2014 in the subsequent years. Given that the baseline inflow rate amounted to around 60% per year, this is a substantial effect. As we show later in one of the robustness tests, the first-stage coefficient becomes larger when considering the effect on DIYA benefit receipt based on the effect at the age of 18 only.

Table 8 with the first-stage regression results shows that individuals with their birthday earlier in the year received DIYA benefits more often than those born later in the year (as shown earlier in Fig. 3). This mirrors the effect of application delays: in order to be entitled to the old DIYA scheme, an application had to be received by UWV no later than September 10, 2014 (see Sect. 2). We generally find the sign of most coefficients of the first-stage regression to be in line with expectations: DIYA inflow rates are higher for men, children with a mother on welfare, individuals with mental and behavioural problems, individuals with special healthcare needs, and individuals with a criminal history. The inflow into DIYA was also higher in small municipalities, and lower for individuals with a migration background. The latter finding suggests that migrant groups are less familiar with the DIYA scheme, or are more susceptive to negative stigma attached to using a scheme for disabled individuals (UWV, 2010).

Table 4 next turns to the pooled estimation results on our outcome variables for all young individuals in our sample and young individuals stratified by gender. For ease of interpretation, the treatment variable is defined as *non-receipt* of DIYA. The coefficients should therefore be interpreted as the effect of reduced access to the DIYA program. To start with, we see that the reform resulted in a substantially lower gross total income. From 2016 to 2021, the annual gross total income decreased on average by €4,524 for those who lost access to the DIYA. This effect is largely due to the reduced access to DIYA benefits, and it is partly compensated for by receiving more welfare benefits and earning income from wages. In total, the loss of annual DIYA benefits amounted to €10,751, of which €3,206 (30%) was compensated by welfare and €3,401 (32%) was compensated by increased income from labour. The latter finding is comparable to Deshpande (2016a), who finds that 38% of lost SSI benefits is mitigated by increased income from labour. Total DIYA income effects we find are smaller than for SSI, since welfare benefits that mitigate the decrease in (DIYA) income are substantially less generous in the US.

Table 4 also shows that the impact of not receiving DIYA benefits on wage earnings is clearly stronger for men than women. Both the effects on earnings and employment are small and/or insignificant for women, but large and significant for men. Specifically, men lose €10,317 of their DIYA benefits per year, of which they compensate €4,637 (45%) by a higher income from earnings and €2,694 (26%) by a higher income from welfare benefits. In total, they recover about 70% of the lost

Table 4 Effects of not receiving DIYA benefit on outcome measures; pooled IV Estimation results (2016–2021)

	All	Men	Women
First-stage estimate	-0.409***	-0.418***	-0.391***
	(0.0186)	(0.0225)	(0.0327)
Second stage-estimates for outcome variables			
Gross total income (€ per year)	-4,524***	-3,518***	-6,012***
	(880.0)	(1,180)	(1,124)
Gross labour income (€ per year)	3,401***	4,637***	1,519
	(1,242)	(1,662)	(1,625)
DIYA benefits (€ per year)	-10,751***	-10,317***	-11,633***
	(198.4)	(254.2)	(304.2)
Welfare benefits (€ per year)	3,206***	2,694***	4,270***
	(328.5)	(372.8)	(645.5)
Employment	0.104**	0.180***	-0.0279
	(0.0512)	(0.0617)	(0.0922)
Welfare receipt	0.334***	0.270***	0.462***
	(0.0365)	(0.0426)	(0.0693)
Crime suspect	0.0156	0.0136	0.00548
	(0.0197)	(0.0278)	(0.0175)
Use of mental health care	0.0483	0.0456	0.0715
	(0.0315)	(0.0371)	(0.0583)
Health care costs (log)	0.1894	0.1887	0.2817
	(0.2261)	(0.2765)	(0.3910)
Problematic debts (2018–2021)	0.0418	0.0199	0.0812
	(0.0385)	(0.0466)	(0.0682)
Living at home	-0.0192	-0.0611	0.0557
	(0.0558)	(0.0679)	(0.0976)
Observations	115,332	74,880	40,452
Year dummies	YES	YES	YES
Controls	YES	YES	YES
Specification	RDD-IV	RDD-IV	RDD-IV

Source: Statistics Netherlands, own calculations

IV analysis. Control variables include year dummies, birth-month, gender, migration background, mother's welfare receipt, unknown father, impairment types, municipality size, use of medicine for ADHD, ambulatory youth care, special health care, supervision needs, and suspected of theft or assault

We show coefficients for the estimated probability of *NOT* receiving DIYA.

Individual-clustered standard errors in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.1$

DIYA benefits. Women receive €11,633 less income from DIYA benefits per year, of which they compensate only €1,519 (10%) by a higher income form earnings and €4,270 (37%) by additional income from welfare benefits. In total, they recover about 47% of the lost DIYA benefits.

While we find substantial and significant income and employment effects of the DIYA reform, the effects on (mental) health, total (log) health care costs, problematic debts, criminal behaviour and household situation (living ate home) are statistically insignificant in all cases. At the same time, the effects of reduced access can be substantial on the non-employment effects and show a consistent pattern, with increases

in crime outcomes, more health care use and more problematic debts.[11] Fig. 5 showed the volatility in health and crime outcomes over time is large, which may cause the imprecise estimates. Still, our findings suggest that the income decreases for the most vulnerable young adults might have been sufficiently compensated for by welfare benefits, (unmeasured) increased financial support from parents, and/or increased earnings from work. This in turn may have mitigated potential increases in debt, health and criminal behaviours. Concurrently, the increase in labour force participation itself might lead to mental health improvements (Burns et al., 2007; Huber et al., 2011; Van Eijkel et al., 2021). In this respect, it should be noted that we estimate the short-term effects on a (still) young group of adults. In contrast, Deshpande and Mueller-Smith (2022) investigate the effects of SSI income loss over a time horizon of 20 years and find that income support removal had adverse effects on criminal behaviour.

5.2 Mechanisms: Gender, Household Status and Disability Types

Our results indicate that the effects of the DIYA reform have affected the incomes of young adults with work disabilities in the Netherlands, and not outcomes in other life domains. These income effects turn out to be markedly different for males and females. This raises questions on the differences in the context that males and females are facing, particularly their household status and also on the types of disabilities they have. Regarding housing status, Table 9 in the appendix shows that young women in our sample are more likely to have a partner and children than their male counterparts. Specifically, 14% of women has a partner and 9% has children, while 7% of men live with a partner and 2% has children. Still, these differences alone are not likely to explain the (full) gender differences in earnings responses.

To delve deeper into the potential role of household status, Table 5 presents response estimates to reduced access to DIYA benefits for samples stratified by household conditions and gender. The estimation results are less precise for those living independent, probably due to a low number of observations for these groups. Conditional on household status type, we see that the gender difference in net income responses is largest for young adults with a partner. For this group, we estimate an annual income loss for males equal to €2,630 and €8,883 for females. Similar to our earlier results, this difference largely mirrors the effect of the stronger work earnings response of men. In contrast to the results of Deshpande (2016a), these findings provide suggestive evidence that gender role models are prevalent within couples. It may be that differences in gender roles are stronger in the Netherlands than the USA. Women in the Netherlands more often work parttime to be able to combine work and family responsibilities, as compared to women in the US.[12]

In addition, we see that partners – especially women with a partner – have lower replacing income from welfare benefits. This mirrors the fact that their household

[11] Figure 5 showes the volatility in crime outcomes over time is large, which may cause the imprecise estimates.

[12] In the Netherlands, 52% of working women work less than 30 h a week, while in the US this is only 16% (figures for 2023, https://data-explorer.oecd.org).

Table 5 Effects of not receiving DIYA on income outcomes in Euros per year by household status and gender – pooled results for 2016–2021

	Living at parents' home	Living independent, without partner (singles and single parents)	Living independent with partner
Men			
Gross total income	-3,843***	-5,249**	-2,630
	(1,491)	(2,289)	(4,731)
Gross labour income	5,508***	-2,480	6,305
	(2,032)	(3,738)	(6,612)
DIYA benefits	-9,735***	-9,862***	-10,750***
	(329.3)	(563.6)	(727.6)
Welfare benefits	1,443***	5,921***	3,117***
	(303.7)	(1,366)	(1,161)
First-stage estimate	-0.398***	-0.452***	-0.460***
	(0.0287)	(0.0514)	(0.0617)
Observations	44,798	12,471	4,678
Women			
Gross total income	-5,860***	-4,615***	-8,883***
	(1,758)	(1,648)	(2,864)
Gross labour income	1,947	-92.38	1,708
	(2,528)	(2,635)	(3,560)
DIYA benefits	-9,890***	-11,882***	-11,424***
	(602.5)	(402.6)	(571.0)
Welfare benefits	3,135***	7,582***	1,782*
	(657.8)	(1,666)	(961.3)
First-stage estimate	-0.329***	-0.561***	-0.489***
	(0.0481)	(0.0657)	(0.0594)
Observations	19,661	7,620	5,369

Source: Statistics Netherlands, own calculations

IV analysis. Control variables include year dummies, birth-month, gender, migration background, mother's welfare receipt, unknown father, impairment types, municipality size, use of medicine for ADHD, ambulatory youth care, special health care, supervision needs, and suspected of theft or assault. All amounts are expressed in euros per year. We show the coefficient of the estimated probability of *NOT* receiving DIYA. Individual-clustered standard errors in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.1$

income rendered them ineligible for replacing welfare benefits. This conclusion is backed up by the observation that partners of women earn about €4,000 (or 10%) more on an annual basis than partners of men. Part of the difference between men and women might also be explained by the fact that male partners earn more. This reduces the need to increase income from earnings. In line with this, we do not observe a substantial difference in labour market responses between men and women who live with their parents.

We next turn to differences in responses across disability types – shown in Table 6. We distinguish between three disability types[13]: (i) limited intellectual capabilities

[13] Visually impaired individuals are not included because of a low number of observations.

(i.e. enrolled in practical education) (ii) physical or mental disabilities and (iii) psychological disorders and behavioural problems. The results suggest that the disability type matters more for males than females. Specifically, males with psychological and/or behavioural problems fare worse than males with other disability types. This is not the case for women. The earnings response of men with psychological and/or behavioural problems is small, causing a net income decrease of €5,880 per year. This smaller earnings response may be explained by the fact that employers find it more difficult to handle the needs – and unpredictability – of young individuals with behavioural problems (Kaye et al., 2011). In contrast to this, we find relatively strong

Table 6 Effects of not receiving DIYA benefits on income outcomes in Euros per year by gender and disability type – pooled estimation results 2016–2021

	All	Limited intellectual capabilities	Physical and/or mental disabilities	Psychological disorders and behavioural problems
Men				
Gross total income	-3,518***	-1,005	-1,766	-5,880***
	(1,180)	(2,046)	(2,031)	(1,918)
Gross labour income	4,637***	8,001***	6,512**	1,868
	(1,662)	(2,835)	(3,274)	(2,654)
DIYA benefits	-10,317***	-9,677***	-10,177***	-10,641***
	(254.2)	(321.6)	(1,053)	(337.9)
Welfare benefits	2,694***	1,104**	2,482***	4,099***
	(372.8)	(473.2)	(791.2)	(747.0)
First-stage estimate	-0.418***	-0.432***	-0.297***	-0.461***
	(0.0225)	(0.0269)	(0.0656)	(0.0362)
Observations	74,880	29,658	15,582	25,818
Women				
Gross total income	-4,590***	-5,277***	-4,020**	-6,648***
	(887.0)	(1,612)	(1,683)	(2,509)
Gross labour income	1,159	2,278	3,628	-33.0
	(1,261)	(2,399)	(2,502)	(3,194)
DIYA benefits	-8,879***	-11,145***	-12,044***	-12,112***
	(715.2)	(327.9)	(954.3)	(502.1)
Welfare benefits	3,259***	3,703***	4,245***	5,543***
	(493.7)	(834.8)	(1,087)	(1,768)
First-stage estimate	-0.391***	-0.423***	-0.298***	-0.463***
	(0.0327)	(0,0378)	(0,0789)	(0,0777)
Observations	40,452	20,478	10,134	8,070

Source: Statistics Netherlands, own calculations

IV analysis. Control variables include year dummies, birth-month, gender, migration background, mother's welfare receipt, unknown father, impairment types, municipality size, use of medicine for ADHD, ambulatory youth care, special health care, supervision needs, and suspected of theft or assault. All amounts are expressed in euros per year. We show the coefficient of the estimated probability of *NOT* receiving DIYA. Individual-clustered standard error (SE) in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.1$

earnings responses of men with limited intellectual capabilities and those with physical and/or mental disabilities. Young adults with limited intellectual capabilities show the lowest DIYA receipt before and after the reform: in 2021 38% of the 2014 cohort received a DIYA receipt, and 4% of the 2015 cohort. This is far below the average: 51% of the 2014 cohort received a DIYA benefit in 2021, and 20% of the 2015 cohort, as shown earlier in Fig. 3. Even though cognitive capabilities are limited for them, their more applied skills are probably more valuable to employers than what groups with other disabilities are able to offer.

Overall, the subgroup analyses on income effects of the reform suggest the presence of gender norms in explaining differences in earnings responses of males and females. Our results show that differences in the composition of disability types cannot explain the differences we obtained in earning response between males and females. In contrast, it is only for the young adults with psychological and behavioural problems that the earnings responses are substantially lower than for other disability types, with males being overrepresented in this group.

5.3 Robustness

As already discussed in Sect. 4, one of the key assumptions underlying our IV-strategy is that the birth-month cohort effect exclusively affects the outcome variables through decreased access to DIYA benefits. To test this assumption, we conduct placebo tests on the two cohorts that turned 18 in 2012 or 2013, respectively. Specifically, we run reduced form RD regressions on the income outcomes with a dummy for a discrete effect in January 2013 and test on the significance of the RD coefficients. The results are shown in Table 7. Only welfare benefits are significantly higher for the 2013-cohort than for the 2012-cohort, but the other outcomes show, as expected, no significant effects. The standardized difference are all well below the threshold value of 0.25, see footnote 11.

Alongside the placebo tests, we conducted sensitivity analyses wherein we changed the parameters of the RD/IV design. First, as discussed in Sect. 4, we changed the bandwidth for the support of birth-month cohorts – see Table 10 for the results. We find that smaller bandwidths generally lower the efficiency of our estimates, but not the size of effects. Second, we re-estimated the model on separate years instead of on

Table 7 Placebo tests: reduced form estimates of cohort effect of 2013 compared to 2012 on income outcomes in Euros per year – pooled Estimation results

	Total	SE	Standardized difference
Gross total income	145.7	(300.0)	0.01
Gross labour income	-425.5	(490.2)	-0.03
DIYA benefits	461.0	(294.8)	0.05
Welfare benefits	198.2**	(89.13)	0.08

Source: Statistics Netherlands, own calculations

Income expressed as euros per year. Control variables include year dummies, birth-month, gender, migration background, mother's welfare receipt, unknown father, impairment types, municipality size, use of medicine for ADHD, ambulatory youth care, special health care, supervision needs, and suspected of theft or assault. Income expressed as euros per year. Individual-clustered standard errors (SE) in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.1$

the pooled data. Table 11 in the appendix shows the corresponding results. Albeit that the (first-stage) effect of the reform on DIYA recipiency decreases over the years, our results on the second-stage estimates are in line with the pooled estimates. Income response estimates of more recent years do become less precise, which mirrors the effect of smaller first-stage estimates for later years. Finally, we re-estimated the model with first-stage estimates that are based on DIYA enrolment at the age of 18 instead of the year of the outcome measure; the results are shown in Table 12 in the appendix. This way, the first-stage effect becomes constant over time. Again, this yields outcomes that are similar as those obtained for the benchmark model.

6 Discussion and Conclusion

This paper uses a drastic reform of the Disability Insurance scheme for Young Adults (DIYA; in Dutch: "Wajong") in the Netherlands in 2015 to estimate the causal effects of relatively generous income support on employment, disposable income, expenditure on medical consumption, the incidence of problematic debts and criminal outcomes. The reform implied an effective reduction in monthly benefit income that ranged between €300 (for 18 years old) to €650 (for 20 years old). For those older than 20 years of age, the reduction was lower, but stricter means- and asset-testing of welfare benefits also led to lower benefit receipt. Using a fuzzy RD design, we exploit the discontinuous change in DIYA benefit conditions for cohorts who turned 18 in the year before and after the exact date of the reform. We limit samples to young adults who attended special education – targeted at children with special needs – in the cohort years 2014 or 2015.

From the IV analyses, we conclude that the reduced access to the DIYA scheme had strong income implications: the inflow into the scheme decreased by about 40% points, income from DIYA benefits decreased considerably (on average around 10,000 euros per year), and replacing income from earnings and welfare amounted to around 60% of the income loss (32% from earnings and 30 from welfare on average). We find much larger earnings responses for men than for women, and relatively strong effects on labour earnings also for – mostly male – young adults with limited intellectual capabilities. Alongside these more conventional outcome measures, we estimate insignificant effects on other domains of life than benefits and employment, including criminal activity, health outcomes, or problematic debts. In terms of earnings responses, the size of the income replacing effect of additional employment is comparable to the effects found for the SSI program by Deshpande (2016a, b). Using similar RD modelsDeshpande (2016a, b) finds that young adults removed from the SSI at age 18 were able to recover 38% of their lost benefit income in later years. At the same time, her results also point at stronger earnings responses among young adults with intellectual disabilities.

Our results for the DIYA reform, however, also deviate from the existing literature that is based on the removal of SSI benefits in two important aspects. The first is that the significant and large response effects on employment and benefit outcomes go together with economically substantial but statistically insignificant effects on health expenditure and treatments, personal debts and criminal behaviour. As for criminal

offenses, these findings are at odds with Deshpande and Mueller-Smith (2022), who find large adverse effects as a response to the removal of SSI benefits. The most likely explanation for the absence of DIYA effects that go beyond employment and benefits is that increases in both earnings, possible financial support from parents or partner and welfare benefits – for those most in need – were sufficient to prevent this to occur. In this respect, it also should be noted that many of the concerned young adults were still living at their parents' home, and that Dutch health insurance has low out-of-pocket expenditures in comparison to the US.

The second finding that deviates from earlier work in this field concerns the role of gender differences. Our analyses reveal suggestive evidence of gender norms within partner households, with male young adults responding much stronger to the income benefit loss. Such gender differences are much less pronounced for singles. Presumably, differences in work culture between the Netherlands and the USA can cause the differences between our results and those obtained by Deshpande (2016a). More women in the Netherlands work parttime to be able to combine work and family responsibilities, in comparison to women in the US. Knowing that earnings and productivity levels are generally low for young disabled workers, this also underscores the more general finding that gender employment gaps are most substantial in households with low-educated individuals (Eurostat, 2021).

Similar to Deshpande (2016a,2016b) who considers a time window of 16 years, an interesting direction for further research would to estimate the long-term effects of the reform. She finds diminishing income effects after the SSI removal over time. Similarly, insight is also needed in how the labour market attachment evolves among those individuals screened out by the reform, and whether jobs obtained are durable. Future exits from DIYA of cohort 2015 will be rare, as these individuals are deemed to have no current or future work capacity. At the same time, we do see individuals initially not entering DIYA to enter in later years. This might dampen the reform effect. Alongside this, two eligibility rules are expected to lead to a higher inflow in the new DIYA of older applicants. Eligibility rules stipulate that after 10 years (so as from 2025) those who were initially rejected for the new DIYA in 2015, but have not worked since then, become eligible for the new DIYA.

Appendix

Additional Analyses and Descriptives

See Tables 8, 9, 10, 11, 12

Table 8 First-stage and second-stage estimates of not receiving DIYA benefit on outcome measures; pooled results (2016–2021)

	First stage	Second stage			
Variables		Gross total income	Gross labour income	DIYA benefit	Welfare benefit
Cohort_2015 (=instrument)	-0.409***				
	(0.0186)				
DIYA receipt		4,524***	-3,401***	10,751***	-3,206***
		(880.0)	(1,242)	(198.4)	(328.5)
Years					
year=2016	ref	ref	ref	ref	ref
year=2017	0.0199***	2,885***	2,208***	769.0***	246.1***
	(0.00122)	(36.83)	(46.02)	(16.90)	(12.66)
year=2018	0.0243***	6,155***	4,720***	1,646***	531.0***
	(0.00142)	(49.65)	(67.01)	(23.41)	(20.15)
year=2019	0.0243***	8,630***	6,841***	2,173***	759.6***
	(0.00142)	(57.69)	(82.18)	(28.42)	(23.81)
year=2020	0.0247***	9,833***	7,948***	2,450***	866.1***
	(0.00168)	(64.65)	(94.45)	(30.68)	(25.72)
year=2021	0.0265***	11,116***	9,160***	2,607***	914.6***
	(0.00177)	(69.93)	(103.4)	(31.06)	(26.67)
Age in months as off 1-1-2014	-0.0123***	-106.5***	-54.20**	-42.35***	-24.29***
	(0.00126)	(17.91)	(25.88)	(6.031)	(6.275)
age_cohort_2015	0.0113***	-8.907	-21.05	27.45***	-13.74**
	(0.00150)	(17.08)	(24.78)	(6.041)	(5.849)
Gender					
Female	0.0231***	-2,720***	-4,711***	305.4***	492.7***
	(0.00555)	(85.13)	(122.5)	(28.54)	(35.44)
Municipality size (inhabitants)					
Small < 15,000	ref	ref	ref	ref	ref
Medium > 15,000 and < 40,000	-0.0343**	36.36	216.0	-109.8	-28.48
	(0.0138)	(236.5)	(349.8)	(77.14)	(75.18)
Large > 40,000	-0.0386***	-482.7**	-513.9	-97.30	29.46
	(0.0135)	(229.8)	(340.4)	(75.22)	(73.66)
Migration background					
No migration background	ref	ref	ref	ref	ref
Morocco	-0.0487***	-1,881***	-3,160***	361.8***	-184.2**
	(0.0115)	(201.6)	(271.8)	(54.27)	(72.15)
Turkey	-0.0601***	-339.1	-870.8***	295.6***	-339.3***
	(0.0109)	(216.5)	(292.5)	(49.59)	(64.90)
Surinam	-0.0397***	-1,848***	-2,456***	127.8*	-291.1***
	(0.0137)	(231.5)	(317.2)	(69.67)	(84.16)
Netherlands Antilles and Aruba	-0.0415**	-1,778***	-2,333***	-42.83	-329.1***
	(0.0167)	(255.5)	(369.5)	(73.69)	(103.9)
Other non-Western countries	-0.0678***	-1,155***	-1,745***	7.908	-366.5***
	(0.00983)	(172.1)	(243.1)	(49.28)	(69.36)
Other Western countries	-0.0260***	-700.8***	-1,139***	123.9**	6.203
	(0.0100)	(171.4)	(243.9)	(53.72)	(66.18)

Table 8 (continued)

	First stage	Second stage			
Impairment types					
IQ between 55 and 80	ref	ref	ref	ref	ref
Visually impaired, hearing impaired	0.0387**	-2,545***	-3,281***	-3.309	56.54
	(0.0169)	(264.1)	(380.2)	(88.38)	(74.75)
Physical and/or mental disabilities	0.427***	-3,431***	-7,077***	1,678***	1,023***
	(0.00779)	(391.3)	(558.0)	(96.54)	(145.6)
Psychological disorders and behav. problems	0.0510***	-3,687***	-5,788***	346.0***	685.4***
	(0.00693)	(132.6)	(187.7)	(35.85)	(48.19)
Impairment unknown	0.102***	-4,280***	-6,510***	336.5***	793.7***
	(0.0159)	(338.5)	(455.9)	(57.42)	(157.4)
Using medicines for ADHD	0.0119	131.6	252.8	-76.38	-5.394
	(0.00903)	(150.3)	(221.5)	(47.46)	(53.56)
Ambulatory youth care	-0.0435***	572.4***	145.9	17.21	205.0**
	(0.00955)	(170.4)	(242.8)	(42.74)	(79.85)
Special healthcare, more than 10 h per week	0.213***	-1,061***	-3,020***	628.4***	877.7***
	(0.00668)	(211.0)	(301.7)	(54.79)	(81.78)
Supervision order (child protection)	-0.0195**	110.8	-394.7*	-6.977	401.7***
	(0.00916)	(143.5)	(210.5)	(44.53)	(68.46)
Criminal history					
Theft suspect	-0.0515***	-252.1	-611.7***	50.70	217.4***
	(0.00819)	(158.1)	(224.4)	(40.23)	(62.75)
Assault suspect	-0.0353***	117.8	-149.0	91.72**	169.5***
	(0.00847)	(161.2)	(229.5)	(40.69)	(64.69)
Family background					
Mother receiving welfare in 2013/2014	0.00866	-945.2***	-2,038***	103.9***	441.7***
	(0.00720)	(113.1)	(164.6)	(34.69)	(51.66)
Father unknown	-0.0124	-241.3*	-1,108***	7.064	232.7***
	(0.00800)	(128.2)	(186.2)	(40.29)	(55.55)
Constant	0.457***	11,537***	13,408***	-1,584***	774.7***
	(0.0168)	(443.2)	(626.4)	(98.52)	(157.8)
Observations	115,332	112,058	112,006	112,164	112,164
R-squared	0.396	0.290	0.287	0.829	0.042

Source: Statistics Netherlands, own calculations

Observations are cumulated over time. Clustered standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 9 Household status by gender and type of disability, pooled data 2016–2021

	Limited intellectual capabilities	Physi-cal and/or mental disabilities	Psychological disorders and behavioural problems	All
Men				
Living with parents	69%	58%	54%	62%
Living independently:				
Of which:	22%	10%	35%	24%
Living without children	20%	10%	33%	22%
Living with children (single parents and couples)	2%	0%	2%	2%
Of which:				
Single	14%	8%	27%	17%
With a partner	8%	2%	8%	7%
Other (mainly living in institutions)	9%	32%	11%	14%
Total	100%	100%	100%	100%
Women				
Living with parents	53%	56%	33%	50%
Living independently:	36%	11%	56%	33%
Of which:				
Living without children	24%	9%	41%	24%
Living with children (singe parents and couples)	12%	2%	15%	9%
Of which:				
Single	18%	7%	38%	20%
With a partner	17%	4%	18%	14%
Other (mainly living in institutions)	11%	33%	11%	17%
Total	100%	100%	100%	100%

Source: Statistics Netherlands, own calculations

Table 10 Effects of not receiving DIYA on total income and labour income with different bandwidths for RD estimation, pooled results

Bandwidth in months at each side of the cutoff date	Gross income (€ per year)	SE	Gross labour income (€ per year)	SE	Observations
3	-3,180	(4,957)	-4,259	(6,472)	27,136
4	-3,804	(2,844)	-5,758	(4,165)	36,594
5	-5,299**	(2,549)	-6,836**	(3,164)	46,147
6	-4,729**	(2,123)	-5,733**	(2,485)	55,977
7	-3,820**	(1,817)	-4,582**	(2,120)	65,910
8	-3,732**	(1,523)	-4,449**	(1,766)	74,960
9	-3,716***	(1,308)	-4,701***	(1,608)	84,215
10	-3,627***	(1,204)	-4,577***	(1,476)	93,275
11	-3,031***	(1,095)	-3,686***	(1,300)	102,182
12	-2,759***	(1,030)	-3,401***	(1,242)	112,058

Source: Statistics Netherlands, own calculations

IV analysis. Control variables include year dummies, birth-month, gender, migration background, mother's welfare receipt, unknown father, impairment types, municipality size, use of medicine for ADHD, ambulatory youth care, special health care, supervision needs, and suspected of theft or assault. Amounts expressed in euros in prices of 2020 per year. Observations are cumulated over time. Individual-clustered standard errors (SE) in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 11 Effects of not receiving DIYA on outcome measures, yearly estimates

	2016	2017	2018	2019	2020	2021
Gross total income, 2016–2021	-3,400***	-5,712***	-4,294***	-3,585***	-4,781***	-6,194***
	(631.7)	(906.7)	(1,049)	(1,212)	(1,398)	(1,501)
Gross labour income, 2016–2021	2,457***	3,258***	2,853**	4,098**	2,934	3,444
	(733.0)	(1,123)	(1,425)	(1,707)	(2,047)	(2,175)
DIYA benefits, 2016–2021	-8,300***	-10,027***	-11,332***	-10,899***	-11,591***	-12,699***
	(180.7)	(221.1)	(235.5)	(325.2)	(367.8)	(304.7)
Welfare benefits per year, 2016–2021	2,105***	1,438***	4,619***	3,501***	4,096***	3,578***
	(200.3)	(267.9)	(407.4)	(485.5)	(548.3)	(553.3)
Employee ultimo year, 2016–2021	0.0268	0.138**	0.136**	0.0455	0.140**	0.149**
	(0.0620)	(0.0666)	(0.0651)	(0.0672)	(0.0696)	(0.0691)
Welfare receipt, 2016–2021	0.330***	0.295***	0.406***	0.340***	0.303***	0.328***
	(0.0427)	(0.0459)	(0.0485)	(0.0490)	(0.0527)	(0.0507)
Crime suspect, 2018–2021	-0.0147	0.0201	0.0253	-0.0143	0.0111	0.0720**
	(0.0341)	(0.0353)	(0.0335)	(0.0333)	(0.0343)	(0.0334)
Use of mental health care, 2016–2021[a]	0.134***	0.0372	0.0456	0.0111	0.00877	0.0054
	(0.0453)	(0.0456)	(0.0449)	(0.0448)	(0.0469)	(0.0482)

Table 11 (continued)

	2016	2017	2018	2019	2020	2021
Health care costs (log)	0.313	0.540**	0.143	0.031	0.129	-
	(0.240)	(0.263)	(0.281)	(0.307)	(0.332)	-
Problematic debts, 2018–2021	-	-	0.0263	0.0527	0.0536	0.0347
			(0.0399)	(0.0441)	(0.0478)	(0.0489)
Living at home, 2016–2021	0.0427	0.0154	-0.0674	-0.0544	-0.0378	0.0508
	(0.0556)	(0.0641)	(0.0663)	(0.0696)	(0.0737)	(0.0743)
First-stage estimate	-0.506***	-0.444***	-0.411***	-0.418***	-0.409***	-0.389***
	(0.0166)	(0.0192)	(0.0195)	(0.0196)	(0.0198)	(0.0200)
Observations	19,222	19,222	19,222	19,222	19,222	19,222

Source: Statistics Netherlands, own calculations

IV analysis. Control variables include year dummies, birth-month, gender, migration background, mother's welfare receipt, unknown father, impairment types, municipality size, use of medicine for ADHD, ambulatory youth care, special health care, supervision needs, and suspected of theft or assault. Income outcomes are expressed in euros per year. We show the coefficient of the estimated probability of *NOT* receiving DIYA. Robust standard errors (SE) in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

a) Note that he significant coefficient in 2016 for the incidence of mental health treatment is most likely due to a reform of youth care in 2015, which caused underregistration of youth care in 2014, and subsequent rising registration of youth care in 2015 and 2016

Table 12 Effects of not receiving DIYA on outcome variables by gender, average per year, pooled results (2016–2021), based on estimated probability of receiving DIYA at age 18

	All	Men	Women
Gross total income	-3,670***	-2,953***	-4,590***
	(717.6)	(990.1)	(887.0)
Gross labour income	2,759***	3,893***	1,159
	(1,030)	(1,432)	(1,261)
DIYA benefits	-8,718***	-8,656***	-8,879***
	(409.1)	(497.4)	(715.2)
Welfare benefits	2,599***	2,260***	3,259***
	(269.4)	(316.4)	(493.7)
Employment	0.0847**	0.151***	-0.0214
	(0.0427)	(0.0539)	(0.0701)
Welfare receipt	0.272***	0.227***	0.354***
	(0.0303)	(0.0365)	(0.0538)
Crime suspect (2018–2021)	0.0134	0.0113	0.00663
	(0.0151)	(0.0221)	(0.0123)
Use of mental health care	0.0396	0.0388	0.0552
	(0.0258)	(0.0314)	(0.0448)
Problematic debts (2018–2021)	0.0332	0.0165	0.0597
	(0.0306)	(0.0387)	(0.0502)
Living at home	-0.0158	-0.0518	0.0434
	(0.0458)	(0.0572)	(0.0763)

Source: Statistics Netherlands, own calculations

IV analysis. Control variables include year dummies, birth-month, gender, migration background, mother's welfare receipt, unknown father, impairment types, municipality size, use of medicine for ADHD, ambulatory youth care, special health care, supervision needs, and suspected of theft or assault (see Table 3). Amounts are expressed in euros in prices 2020 per year. We show the coefficient of the estimated probability of *NOT* receiving DIYA. Individual-clustered standard errors (SE) in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.1$

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Declarations

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