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Policies for young adults with reduced work capacity. Labour market impact in Sweden and Norway

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Abstract

The rising numbers of young people with disability pension concerns many advanced economies. We present results from a comparative analysis of the neighbouring countries Sweden and Norway on the impacts of differing policy mixes aimed at enhancing the employability of the work disabled. Using rich longitudinal data, we follow unemployed young adults (ages 25-29) with work-impairment up to four years after they became unemployed to investigate the effect of different types of labour market policies. Our results indicate that, despite differences in programme composition and strategies, there are surprisingly small country differences in treatment effect patterns and signs of estimated impacts. In line with previous studies, we find strong lock-in effects of both workplace-related programmes and training/educational programmes. After participation, workplace-related programmes about double the likelihood of entering regular employment or education. Participating in training courses also increases this likelihood, but effect sizes are smaller.

Keywords Unemployment · Labour market programmes · Reduced work capacity · Young adults

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1 Introduction

In recent decades, many OECD countries have seen a rise in the number of young people with health impairments, hindering them from being fully integrated in the labour market (see, e.g., OECD 2009, 2015, 2020a, b). Worldwide, around 10-20 percent of youth suffer from mental health disorders (WHO 2018). Nearly one in five of 25–29-year-olds in the EU are NEETs, i.e., not in employment, education, or training (EUROSTAT 2020), and evidence shows a clear link between poor health and NEET status (e.g., Chandler Raeven et al. 2021). The daunting challenges faced by vulnerable groups, like the young, the unemployed, and the disabled, are often exacerbated in times of economic downturn. In this paper, we investigate how Sweden and Norway have approached the rising number of young unemployed adults with reduced work capacity and investigate if the combination of active policies in place has worked to enhance their employability. Specifically, we evaluate the impact of labour market policies targeted at young adults (aged 25–29) with work impairment in these two Scandinavian countries, which are often placed alongside when compared to the rest of the world. The period of analysis is the first decade of this millennium.

Young adults reaching their 30s differ in some crucial respects from youth in the school-to-work transition age. Young adults in their late 20s are likely to have completed some or most of the desired education and to have had the opportunity to gain some work experience. Failing to make a successful transition to the labour market may have serious consequences for their employment prospects and well-being.[1] It is well documented that scarring effects of youth unemployment are a huge burden to the individual and society (Bell and Blanchflower 2011).

The Scandinavian countries with their universalistic institutional regimes have a long tradition of implementing labour market programmes (LMP) to counteract potentially long-lasting scarring effects of unemployment. They also provide programmes targeted specifically at persons with disabilities. Importantly for our study, Norway and Sweden share some relevant common features: in both countries, individuals with reduced work capacity constitute a prioritised group in the active labour market policy and they receive publicly funded benefits upon programme participation. Interestingly, Norway and Sweden have pursued qualitatively different policy mixes when it comes to programmes targeted at young adults with reduced work capacity. Sweden has primarily emphasised workplace-related programmes. In contrast, the most popular measure for young adults with work disabilities in Norway has been to take part in training.

Our data cover work-impaired young adults aged 25–29 (born 1973–1984) in Norway and Sweden who have registered as unemployed at the public employment service (PES) in the period 2002–2009. We use the same sampling strategy and similarly defined variables in both countries to arrive at as comparable samples as possible. Access to rich longitudinal administrative data enables us

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[1] See, e.g., the special issue parts I and II of the International Journal of Manpower from 2019 on 'Contributions to School-to-Work Transitions' edited by Pastore and Zimmermann (Pastore and Zimmermann 2019a; b).

to follow these young adults and observe transitions in and out of active and passive labour market policies, and in and out of employment and education. We follow the individuals for a maximum of four years. Using a timing-of-events (ToE) framework developed by Abbring and van den Berg (2003), we estimate effects of programme participation in each country. A special feature of the model is that it controls for unobserved heterogeneity, in addition to the usual explanatory variables typically included in such analyses. The model exploits the randomness of the timing of entry into treatment and outcome to identify causal relationships, without having to rely on exclusion restrictions or conditional independence assumptions stating selection on observables only. Importantly, the method explicitly takes into account that there are unobservable time-invariant individual fixed characteristics that affect both transitions to LMPs and to employment and education. The method has been shown to perform well relative to other non-experimental methods (Muller et al. 2020).

There exists a vast number of studies on the impact of LMPs in advanced economies. An important lesson from the meta-analysis of Card et al. (2018) is that the effectiveness of programmes differs substantially by age group, where young people seem to gain less from participation compared to adults. Moreover, the closer the programmes are to the ordinary labour market, the more successful LMPs seem to be in terms of enhancing employment prospects. The meta-analyses of Kluve et al. (2019) and Hardoy et al. (2018) focusing exclusively on youth support these findings: programme effects for youth tend to be small in size and are often not statistically significant, in particular when it comes to classroom courses. There are also some recent studies evaluating Youth Guarantee programmes implemented across Europe during the last two decades. Hämäläinen et al. (2018) as well as Hall et al. (2022) find moderate positive effects on employment for the Youth Guarantee in Finland and Sweden, respectively. However, the positive impacts in Sweden are primarily attributed to a threat effect, rather than to programme participation. In both cases, the positive impacts seem to be concentrated among youth with stronger labour market prospects. Bratti et al. (2022) find no significant effects on labour market outcomes for the Youth Guarantee in Latvia, which was targeted at ex-ante low-employable jobseekers. Youth Guarantee programmes are usually targeted at individuals below age 25, while the focus of our study is on young adults aged 25–29.

There are fewer studies that focus particularly on jobseekers with disabilities, although individuals with reduced work capacity constitute a group with a strong need for support to enter the labour market. A couple of studies focusing specifically on Sweden and Norway deserve mention. Angelov and Eliason (2018b) find that wage subsidies tend to reduce unsubsidised employment among work disabled, but also the likelihood of leaving the labour market through receiving disability pension. As for Norway, Markussen and Røed (2014) find that participation in measures that most resemble regular work are more successful than alternative strategies in the sheltered sector. When it comes to young people with work disabilities, studies of their impact are limited. Von Simson and Hardoy (2020) study work-impaired youth, aged 18-25, and find that strategies that prioritise subsidising ordinary

education appear to be relatively successful. However, Salvanes et al. (2018) show that employment effects disappear after 5–9 years.[2]

Our study makes several contributions to this literature. Firstly, we compare the impact of two policy packages, namely workplace-related vs. training measures, in the neighbouring countries Sweden and Norway. Access to register data from the two countries allows us to apply the same model on comparable populations, making it possible to compare effect estimates more directly. Secondly, we contribute to the very scarce literature on active labour market programmes for work-impaired youth. The rising share of youth with disabilities, particularly young people with mental health problems, makes the need to study the impact of policies targeted at work-impaired youth of primary importance (e.g., OECD 2015). Finally, our study is the first to specifically focus on work-impaired young adults in their late 20s. These individuals are likely to have gained some work experience and may therefore have different labour market prospects than youth who come directly from school. Moreover, many of these young adults are at risk of long-term inactivity and exclusion. Getting them into a sustainable track is an important ambition of the labour market authorities.

Our results show strong lock-in effects during programme participation for both workplace-related programmes and training/educational programmes, and a positive impact on transitions to unsubsidised work or education after having participated in measures providing work practice and experience. In contrast, the success of training measures seems to be linked to whether they provide certified qualifications. We find that participation in programmes providing work experience almost doubles the chance of transitioning to either regular employment or education, relative to remaining openly unemployed. This is the case in both Sweden and Norway. The impact of participating in training is also positive but considerably smaller.

The paper continues as follows: In Section 2 we discuss the conditions young adults have faced on the labour market since the start of this millennium, and we highlight important differences and similarities between Sweden and Norway. Section 3 presents the estimation model, while Section 4 describes the data and variables. Section 5 presents results, and Section 6 concludes.

2 Young adults who are out of work: placing Norway and Sweden in the bigger picture

Norway and Sweden share culture, history, traditions, and common norms. The 'Scandinavian model' refers to the way Scandinavian countries organise society and is characterised by strong employer and employee unions committed to seek consensus, universal welfare arrangements, relatively small income differences, and

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[2] A recent Swedish study focusing on a related group of young adults with work-impairment, namely 19 to 29-year-olds receiving disability pension, finds positive employment effects of supported employment compared to regular vocational rehabilitation (Fogelgren et al. 2023). Young disability recipients are excluded from our study, as they are not considered job ready.

centrally coordinated collective negotiations. Both Norway and Sweden offer several predominantly publicly funded benefit schemes for individuals who are outside the labour market due to unemployment and/or health issues. However, as we will see below, the compensation schemes available for individuals who lack employment differ, with Norway relying more on health-related benefits than Sweden. This may affect the composition of young adults that are out of work.

Youth below age 30 who do not participate in a meaningful productive activity belong to the NEET population. Figure 1 shows the share of 'NEETs' aged 25–29 during 2005–2012 in selected OECD countries. Norway and Sweden have had very similar developments, quite distinguishable from other northern European countries like Denmark and Germany. Moreover, together with the Netherlands they have among the lowest share of NEETs among the OECD countries, although still amounting to a sizable fraction of the population (9–13 percent). However, the composition of NEETs differs somewhat. In particular, it is more common for young adults in Norway to not have completed upper secondary education. In 2009, 16 percent of individuals aged 25–34 in Norway lacked an upper secondary degree, compared to 9 percent in Sweden (OECD 2020a, b). Lacking secondary education is a strong predictor of future difficulties on the labour market in both countries (OECD 2020a, b).

Many young people who remain out of work and education have complex problems. They often have little work experience, little formal education, and many have mental health problems (OECD 2018, 2016). Sweden has seen a rising trend of mental health problems among young people during the past few decades, both in terms of self-reported problems, such as excessive worrying and anxiety, and hospitalisation due to mental illness (National Board of Health and Welfare 2013). A similar development has been documented in Norway (Bakken 2020). In both countries, mental health conditions are the predominant reason for inflows to disability pension among young adults (Swedish Social Insurance Inspectorate 2011; Brage and Thune 2015).

In this context, it is interesting to get an overview of what benefits are available to the inactive youth population. Figure 2 shows the share of youth in out-of-work benefits in selected countries. Norway and Sweden both had 9–10 percent of youth, aged 15–29, in out-of-work benefits in 2007 and nearly 12 percent in 2012 (NOU 2021). However, the distribution of benefits is different, reflecting differences in the entitlement rules and, potentially, underlying structural differences such as the unemployment level. Relatively more young individuals in Sweden claim unemployment insurance (UI) benefits compared to Norway, while youth in Norway are more likely to claim health-related benefits. The share of youth receiving social assistance is rather similar in both countries.[3]

Relative to the population size (9.2 million inhabitants in Sweden and 4.8 million in Norway), expenditure on LMP is rather similar in both countries, although Norway spends a lower fraction of GDP; see Table 1, which shows numbers for 2008. However, based on Eurostat's categorisation of LMPs, the distribution of measures

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[3] See Table B1 in the online appendix for more details on the available benefit schemes.

differs substantially: Norway spent a much larger share on training—50 percent compared to only 11 percent for Sweden. On the other hand, in Sweden 57 percent was spent on employment incentives, such as wage subsidies and work practice, compared to only 6 percent in Norway. Both countries spent similar shares on supported employment and rehabilitation. By and large, these patterns have been stable this millennium. Although it is possible that some of these differences are due to variations in data or definitions, the table indicates that Sweden and Norway differ in terms of the type of measures prioritised within their active labour market policies. In Section 4 we will have a closer look at the mix of programmes offered to unemployed young adults with work impairment.

All in all, this section has highlighted that Norway and Sweden are similar in many respects when it comes to the situation of young adults who lack employment. But, as we will see, the two countries offer different policy mixes to assist young adults with reduced work capacity. These observations form the starting point for our comparative analysis. By creating similar datasets for a similar population, and estimating the same type of model, we will analyse whether the combination of policies in place has worked to enhance the employability of this group of unemployed. However, as we have also seen in this section, there are some additional notable differences between the countries: available compensation schemes are somewhat different, and it is more common for young adults in Norway to not have completed an upper secondary degree. Such differences may also matter for our results and is something we will return to throughout the paper.

3 The estimation approach

A main challenge in all effect evaluations based on non-experimental data is to separate causal effects from spurious correlations. Those who receive treatment may have some observed or unobserved characteristics which may influence the probability of receiving treatment as well as the outcome of treatment. For instance, less healthy (motivated) individuals may be more likely to participate in LMPs as well as having a lower probability of finding work, leading to a negative correlation between programme participation and the job-finding rate. Failure to control for such heterogeneity in the form of self-selection into treatment would lead to biased estimates of the treatment effect.

We use the timing-of-events (ToE) approach formalised by Abbring and van den Berg (2003) to identify the causal effects of LMPs on subsequent transitions. Lombardi et al. (2021) and Gaure et al. (2007) show, using Monte Carlo simulations, that the ToE model is well suited for separating causal treatment effects from sorting effects. This framework utilises information about the timing of treatment and the timing of outcome to distinguish causal effects from selection effects. The outcome and the treatment assignment are modelled jointly as a competing risks hazard rate, allowing for unobserved heterogeneity in both processes. Selection effects are then explicitly controlled for by allowing the unobserved determinants associated with each hazard rate to be correlated.
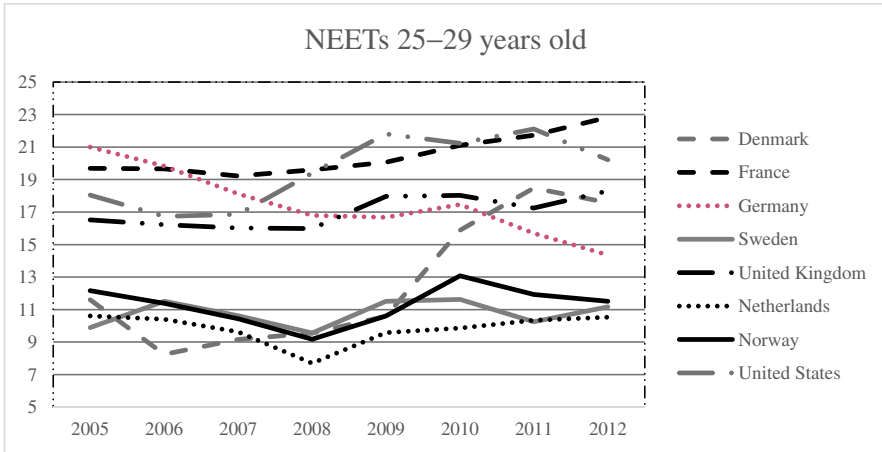
Fig. 1 NEETs 25–29 years old, in percent of the population in the same age group, selected countries. Period 2005–2012. Source: OECD statistics
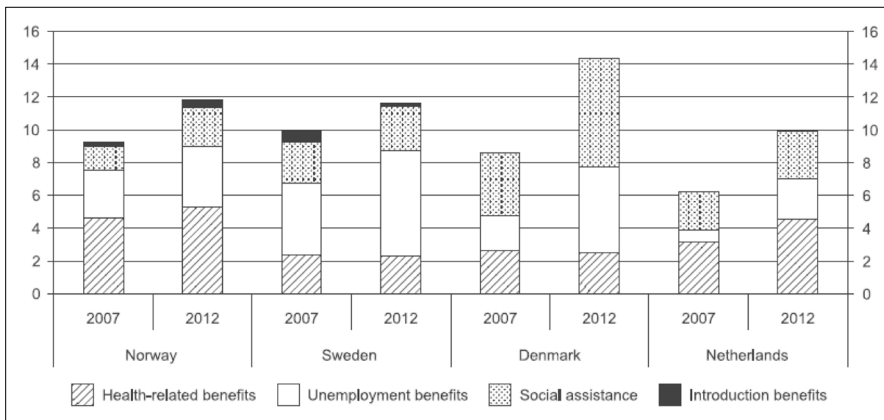

Fig. 2 Out-of-work benefits, youth 15–29 in selected countries. Source: NOU (2021)

A fundamental assumption of the ToE approach is the no-anticipation assumption: individuals should not know in advance the exact moment of treatment, or they should not react on such information. If individuals know for sure that they will participate in a programme at a certain date, they may choose to reduce (or intensify)[4] their search for jobs while waiting for the programme to start. If this is not controlled for, the estimated treatment effects will be biased. The magnitude of the

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[4] Several studies find a so-called threat effect of program participation, leading to increased search effort before the program starts (e.g., Black et al. 2003; Hägglund 2011; Maibom et al. 2014).

Table 1 Total expenditure on LMP measures (in euros and percent of GDP), and percent spent on different types of LMP measures 2008

	Expenditure on LMP		% spent on different types of LMP measures:				
	In euros	% of GDP	Training	Employment incentives	Supported employment and rehab	Direct job creation	Start-up incentives
Denmark	2,280,000	2.4	23.7	13.8	62.5		
Germany	13,193,900	1.9	54.8	14.3	6.1	12.1	12.7
Netherlands	4,253,800	2.0	14.0	20.5	65.5		
Sweden	2,150,200	1.4	10.6	57.3	30.0		
Norway	1,286,000	0.7	49.8	5.9	33.7	9.9	0.7

Source: Eurostat and OECD. LMP measures are grouped in broad categories accommodating significant country differences. See Eurostat for detailed definitions

bias, however, is likely to depend on the time span between the moment individuals are informed about the possibility of LMP participation and the actual start of the programme, since a longer time span provides more room for individuals to act on this information. Furthermore, the assumption does not rule out that individuals may have knowledge about the determinants of the process of programme assignment and act on this information.[5]

We do not have access to information about notification of LMP participation and can therefore not rule out anticipation effects. However, this is unlikely to be a major problem. In the case of Norway, the supply of programmes is constrained and there are indications that assignment to programmes is based on availability, often on short notice, and with local variations.[6] Although the same argument cannot be made for Sweden, Hall et al. (2022) find that anticipation effects among unemployed youth in Sweden tend to be concentrated among those with stronger labour market prospects; for individuals with a weak position on the labour market—which are the focus of our study—they find no significant anticipation effects before programme start. Moreover, the no-anticipation assumption does not rule out the possibility that some individuals know that they have a larger probability of participating in LMPs and act on this knowledge.

The model under consideration is a multivariate mixed proportional hazard rate model. We specify the following hazard rates:

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[5] The no-anticipation assumption is not specific to ToE models, but also central in other treatment evaluation methods based on spell-data, such as matching; see, e.g., discussion in Richardson and van den Berg (2013).

[6] Lande and Selnes (2017) report that for about a third of cases it took more than a year from the time the unemployed individual's ability to work was assessed until a programme was initiated. Reasons for the delay were many: a programme considered to be suitable was not available, the person was too sick, or negligence on the part of the PES. Furthermore, around half of registered work-impaired individuals lack activity plans, and follow-up is sporadic (Riksrevisjonen 2018).

$$\theta_P\big(t|x, c_t, v_P\big) = \lambda_P(t)\exp(x\beta_P + c_t\gamma_P + v_P) \tag{1}$$

$$\theta_O\big(t|x, \Delta_o(t), c_t, v_o\big) = \lambda_O(t)\exp(x\beta_O + \Delta_o(t) + c_t\gamma_o + v_o) \tag{2}$$

where x is a set of observed covariates,[7] c is the youth unemployment rate in the municipality of residence, and v is the error term. All variables are included as flexibly as possible, preferably using dummy variables for each value. Equation (1) is called the selection equation and represents the hazard rate from unemployment to LMP participation, for programme type p. t is the time elapsed since the start of the unemployment spell (normalised to zero). $\lambda_P(t)$ and $\lambda_O(t)$ measure the baseline risk of having a transition to either LMP or work/education, which is independent of other covariates. The baseline hazard will be modelled as piecewise constant, with two-month intervals for the first 12 months, four-month intervals for durations of 13–24 months, and six-month intervals for durations over 24 months. We censor durations over 36 months due to few observations and transitions after this month.[8] All durations are measured in months, and we thus use a discrete version of the underlying continuous hazard rate.

Equation (2), the outcome equation, measures the hazard rate from unemployment to outcome o (work/education). Here we include the effect of LMP participation, measured by the indicator function $\Delta_o(t)$. This effect is further divided into an on-programme effect and an after-programme effect.[9] While participating in an LMP, the unemployed is expected to have less time available for job-search activities and may thus reduce search effort, often referred to as a 'lock-in' effect. After programme completion, the likelihood of getting a job may increase again, e.g., due to higher job search activity, increased formal or job-specific human capital, better information, or larger networks.

Our models include a rich set of background characteristics, which we know from the vast literature evaluating LMPs may affect labour market outcomes. Recent employment history as well as local unemployment are particularly important to adjust for, as they are likely to affect current unemployment durations. To capture more general skills and abilities, we include the individual's education level as well as parental income and education. Nevertheless, as mentioned previously, individuals who participate in LMPs may have unobserved abilities that influence programme assignment as well as the probability of leaving unemployment for

[7] These include human capital and demographic variables, parental income, and education among others. The full set of observed variables is described in the next section. All background characteristics are measured at spell start.

[8] In Section 5.2 we show results when using different censoring times (27 months and 48 months, in addition to 36 months).

[9] In our data we can observe how long a given individual participates in a programme, but not how long the programme was supposed to last. Hence, for a transition to work/education to be categorised as an after-programme transition, the individual needs to first return to open unemployment after programme participation and thereafter make a transition to work/education. Transitions to work/education that occur in direct connection to programme participation will be categorised as taking place during the programme. See Table 4 for transition probabilities during as well as after programme participation.

work/education, which may bias the estimated programme effect if not controlled for. For instance, if only the most motivated unemployed individuals participate in LMPs, the treatment effect may be overestimated, as this group may be more likely to receive job offers as well. Another common problem in duration analyses is the 'weeding-out' effect or dynamic sorting. Some individuals may have certain unobserved characteristics which make them more likely to leave unemployment faster than others; hence, individuals with long durations may be a selected subsample of the original population. This may lead to an underestimation of the duration dependence parameters and the proportionate response of the hazard to variations in a characteristic (including the programme effect).

To try to solve these problems, a set of time-invariant individual unobserved characteristics v, which are allowed to be correlated across transitions, is included in the model. The unobserved characteristics enter the model as random effects and are thus assumed to be uncorrelated with the observed covariates. This is a rather strong condition to meet. However, Lombardi et al. (2021) show that the ToE-model is relatively robust to correlations between observed and unobserved covariates, as long as the distribution of unobserved heterogeneity is flexibly specified, the sample size is large, and there is some exogenous variation in the hazard rate. We follow the modelling framework suggested by Gaure et al. (2007) and let the unobserved heterogeneity follow a discrete distribution with an a priori unknown number of mass points. In addition, the inclusion of time-varying calendar variables induces exogeneity into the hazard rates, relaxing the proportionality assumption and further strengthening identification (Brinch 2007; Lombardi et al. 2021). The model is estimated by maximum likelihood. The estimation procedure starts with one mass point (no unobserved heterogeneity), and then more points are added sequentially until the log-likelihood can no longer be improved.[10] There is substantial evidence that under-correcting for unobserved heterogeneity is associated with substantial bias (Gaure et al. 2007; Lombardi et al. 2021). However, including too many (spurious) support points may also lead to biased treatment effects. To avoid over-correction for unobserved heterogeneity, we use the Akaike information criterion (AIC) to select the optimal number of mass points (Lombardi et al. 2021).[11]

4 Sampling, variables, and descriptive statistics

4.1 Data, sampling, and variables

Our databases consist of population-wide administrative registers with information on unemployment, participation in LMPs, employment, earnings, benefit uptake, education, demographics etc., provided by Statistics Norway, Statistics Sweden, and the PES in each country. The data has a panel structure, making it possible to

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[10] The package *durmod* in R, written by Simen Gaure, is used to estimate the model.

[11] As a robustness check, we also report results using the Bayesian information criterion (BIC), which is more restrictive; see Table B7 in the online appendix.

follow individuals over time with regard to transitions in and out of the labour market. Our starting point is the registers from the PES in Sweden and Norway. From these registers, we sample unemployment spells starting between January 1, 2002, and December 31, 2009, and include all individuals coded with reduced work capacity who have turned 25 but not yet 30 years old (at the start of the spell).[12] As mentioned above, this group differs from younger people transiting from education to work and, hence, it is interesting to focus on them separately.[13] The time frame of our study is chosen taking into account a break in the time series (2002) and a radical change of the follow-up and benefit system for work-impaired in Norway (2010) that are likely to contaminate and blur the interpretation of our results.

Common to the young adults in our sample is that they have some reduction in their work capacity that can be improved or overcome, and that they are registered as unemployed job seekers at the PES. In Sweden, if the caseworker suspects that the client has reduced work capacity, he/she can initiate an evaluation conducted by specialists employed at the PES. A medical report or a report from another specialist (e.g., a psychologist) is often required, but not always. The client also needs to consent to be labelled as 'occupationally disabled'.[14] In Norway, there are two ways to obtain the code 'work impaired': one is through a certificate of ill health issued by a general practitioner; the other is through an assessment done at the PES. Notably, in both countries obtaining this code is also likely to be associated with the individual's difficulties finding employment, as this code expands the number of tools available for the caseworker to help the client.[15] To get an idea of the type of work impairments that are relevant for our sample, Table B2 in the online appendix displays more detailed work impairment codes as registered by the Swedish caseworkers. We can see that both physical and mental work impairments are common among the individuals in our sample, with the most commonly used codes being 'impaired mobility', 'mental work impairment', and 'social-medical impairment'. Unfortunately, we do not have access to similar detailed information for Norway. However, Bragstad and Brage (2011) look at work-impaired youth, ages 18–29, in the period 1996–2008, and find that the dominant medical reasons for work impairment for this

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[12] We do not put any restriction on when the individual received the reduced-work-capacity code to be included in the sample as this condition may become apparent to the case worker some time after registering as unemployed.

[13] There is also a pragmatic reason that motivates our choice: in Sweden all unemployed individuals younger than 25 should be offered participation in guarantee programmes already after three months of unemployment, which implies that there is no control group of non-participants for younger unemployed individuals. The programme in place since 2007 is called the Youth Job Guarantee; see Hall et al. (2022) for a study of this programme. Between 1998 and 2006 a similar guarantee programme, the Youth Guarantee, existed in many municipalities; see Forslund and Skans (2006) for an impact evaluation.

[14] See Angelov and Eliason (2018a) for a thorough description of the process.

[15] Country differences in benefit entitlements mean that a larger proportion of work disabled unemployed are entitled to UI benefits in Sweden than in Norway. In Norway, on the contrary, receiving this code triggers vocational rehabilitation benefits not accessible as ordinary unemployed. We cannot disregard the possibility that differences in the process, or incentives, to obtain the code of work impaired would have implications for the sample selection, potentially giving rise to disparities in characteristics between the Norwegian and Swedish samples. We discuss this further and compare the characteristics of the two samples in the next section.

group were mental health problems (depression and anxiety) and musculoskeletal disorders.

The unit of analysis is spells of registered unemployment for individuals with reduced work capacity. We consider an unemployment spell to start when a person, who has not been registered at the PES for at least 60 days, registers as unemployed. If there is a break of less than 60 days between two consecutive unemployment periods, these periods are merged.[16] In our preferred models, we use a rather broad definition of open unemployment: counselling, coaching, and job search activities during unemployment are also categorised as open unemployment (see Table B3 in the online appendix for details).[17] Such measures, often referred to as follow-up measures or job search assistance, are mostly of short duration meant to assist the unemployed in finding a meaningful activity. Also, the Swedish program Vocational Rehabilitation is categorised as open unemployment, as its content often (but not only) includes counselling and coaching activities.[18] Noteworthy, as opposed to unemployed individuals in general who often enter unemployment directly from a job, many of the work-impaired individuals in our samples have relied on different welfare benefits for some time, e.g., sickness benefits or social assistance (see next section).

As openly unemployed, the young adults may be entitled to UI benefits, but not all are. UI benefits in Sweden depend on employment history while in Norway they require that earnings surpass a certain threshold.[19] Work-impaired young adults in Norway are entitled to vocational rehabilitation benefits, with the same benefit level as UI benefits. During the period registered at the PES, some young adults participate in LMPs. These measures encompass a large variety of interventions, with varying degrees of intensity, duration, and content. LMPs have changed over the period of analysis to accommodate volume and types of programmes to economic fluctuations and demand. Moreover, the supply of programmes reflects differences in policy objectives and means of achieving them in the two countries. While participating in LMPs the unemployed are entitled to benefits in both countries, and benefits may vary with type of programme and previous earnings/employment.[20] Following the international literature, we group LMPs into two broad categories. '*Train*' includes training courses of varying duration and certification (both preparatory

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[16] In Sweden, some spells start with a code indicating that the individual is not yet available for jobs, e.g., due to parental leave or sick leave. In such cases, this part of the spell is not included. Instead, the unemployment spell is considered to begin when the person changes status to open unemployment.

[17] We also run regressions with a more restrictive definition of open unemployment, which excludes such activities; see Section 5.2.

[18] In fact, a large share of the job seekers with work impairment are registered in this programme for a (usually short) period. This is probably related to the assessment needed to receive the work impairment code. Our results do not appear to be very sensitive to how these periods are coded; see Section 5.2.

[19] During the period of analysis, the individual had to have worked at least 6 out of the 12 months preceding unemployment to be entitled to UI benefits in Sweden. The threshold for benefit entitlement in Norway was either a minimum of about 14,000 euros the previous year or nearly 30,000 euros the last 3 years (today's value).

[20] Differences in benefit entitlements may give rise to various incentives. However, exploring this is beyond the scope of this study.

and vocational), and in Norway it also includes ordinary education. The reason for also including this programme category for Norway is that it is available to unemployed with work disabilities under the same conditions and with the same benefits as labour market training courses. '*Place*' consists of measures providing work experience in the workplace, such as work practice and wage subsidies, of varying duration as well as degree of subsidy. (In Appendix Table B3 we show how each individual programme in each country is categorised.) Two programmes within the same category that follow each other (or with a break of open unemployment lasting less than 60 days) are treated as the same programme. In the occurrence of transitions between the broad programme categories or a second transition to the same programme type (more than two months later), spells are censored. Spells where the unemployed enters programmes that are difficult to categorise as either *train* or *place* are also censored.[21]

An unemployment period ends if the person leaves the PES register for more than 60 days, referred to as a permanent transition. In accordance with the intention of LMPs, we model explicitly two transitions, to ordinary education[22] and to regular unsubsidised employment.[23] All other transitions are censored. Spells lasting longer than 36 months are also censored. The reason for this is that Sweden had different 'guarantee programmes' in place during the period studied which ensured that individuals did not remain in open unemployment for very long periods.[24] This means that we have few observations to compare LMP observations with after about 2−2.5 years of unemployment in the Swedish sample. (In Section 5.2 we show that our results are not very sensitive to the choice of censoring time.)

We use different data sources to code permanent transitions in Sweden and Norway. In the Swedish data, the caseworker always registers the reason why the client leaves unemployment (regular employment, education, other known or unknown reason), and we use this information to determine if the spell ends due to employment or education. In Norway, we use register data from several administrative registers, like the employer/employee register, income register, and educational register,

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[21] In the case of Sweden, these programmes include Projects with Employment Orientation and Work Life Introduction. Both have rather few participants in our sample. In Norway, programmes directed at permanently disabled individuals, in the sheltered sector, are censored.

[22] In Norway, ordinary education can be both an LMP and an outcome. The two states are distinguished by how they are registered in the data, i.e., by whether ordinary education is provided as a labour market programme or not.

[23] Evaluations of LMPs for individuals with work disabilities sometimes also consider subsidised employment a successful outcome (e.g., Angelov and Eliason 2018b). In our study, subsidised employment programmes constitute an important part of the treatment, and can therefore not be included as an outcome.

[24] Until June, 2007, the rules stated that all individuals should be offered full-time activity within 27 months of unemployment, the so called 'Activity Guarantee'. After July, 2007, the 'Job- and Development Guarantee' offered activation for individuals who had either used up all their UI benefits or had been registered at the PES for more than 18 months. The initial phase of this program (about 5 months) however consisted of follow-up type activities (e.g., counselling or job seeking activities) which we define as open unemployment.

to determine why the spell ends.[25] For both countries, we also consider the spell to end due to a transition to employment if the individual remains in the PES register, but is coded as temporarily employed, employed by the hour, or part-time employed for at least 60 days.

The registers from both countries contain daily information, but as a final step before estimating our model, we convert it to monthly data to facilitate estimation. The conversion to monthly data implies that spells with a transition to a programme during the first month of unemployment do not contribute to the identification of programme effects; we therefore exclude these spells. This excludes 4 percent of the spells for Sweden and close to 30 percent for Norway; see Table 2 below. Our estimation samples consist of 26,464 Swedish and 22,337 Norwegian unemployment spells, comprising 20,338 Norwegian and 22,194 Swedish young adults aged 25 to 29.[26] It is relevant to note that the estimation samples are similar to the full samples despite the fact that we exclude 30 percent of the spells for Norway, suggesting that this adjustment should not affect the representativeness of the samples.[27]

4.2 Descriptive statistics

Table 2 shows descriptive statistics for the full samples and the estimation samples. A first thing to notice is that the full sample for Norway is much larger than that of Sweden in relation to the population size. It is also larger as a share of the unemployed; see Table B4 in the online appendix. While unemployment spells with a work impairment code constitute around 4 percent of all new unemployment spells for the age group 25–29 in Sweden during the observation period, the corresponding figure for Norway is 10 percent. A possible reason is that in Norway unemployed young people with very limited work experience are not entitled to either UI or vocational rehabilitation benefits, and ordinary LMPs are limited, potentially creating de facto incentives for caseworkers to provide programmes earmarked for the work disabled, which are more abundant (OECD 2018). This may lead to the 'work impaired' in Norway being a relatively stronger group (their work capacity may be less reduced) compared to Sweden. But there could be other institutional differences

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[25] A more detailed description of the outcomes is found in online appendix A. Due to differences in register data between the countries, it was not possible to use completely consistent measures. Caseworker reports do not exist in Norway, while high-quality data on employment spells are not available for Sweden for this period. Instead, we have followed commonly used definitions of transitions to employment/education in each country. Surveys by the Swedish PES have shown that the caseworker reports tend to underestimate flows to employment somewhat, since not all individuals inform the caseworkers when they have found a job (see, e.g. Nilsson 2010). However, the share of unemployed that leaves the PES for unknown reasons is much smaller among job seekers with disabilities, compared to stronger groups of unemployed, making this less of an issue for our sample.

[26] The Norwegian data lacks information about the oldest cohorts born 1973–1975, meaning that we do not have a full sample of young adults aged 27–29.

[27] To check whether excluding these spells affects our results, we have re-estimated the model on the Norwegian sample redefining the duration clock such that spells starting with an LMP are interpreted as being exposed to an LMP some time during the first month. Estimates for *train* become somewhat stronger and larger in absolute value, while estimates for *place* remain unchanged.

working in the opposite direction. For instance, disability benefits may be easier to obtain in Sweden, since they are not permanent (below age 30) as they are in Norway.[28] This may lead to individuals with severe work impairment being less likely to appear in the Swedish compared to the Norwegian sample. However, as we will see below, none of the samples stand out as being in a stronger labour market position in terms of the characteristics we can observe in our data.[29]

A comparison of the Swedish and Norwegian samples shows that the estimation samples are similar in important ways known to affect labour market outcomes. Some differences need mention, nevertheless. The Swedish sample has a slightly higher fraction of women and a somewhat higher share born outside the Nordic region. Noticeable, a much higher fraction of the Norwegians has not completed upper secondary school. This could partly reflect differences between the education systems; Norway has overall a higher dropout rate than Sweden (see discussion in Section 2). On the other hand, the Norwegian sample has a stronger employment record during the three years preceding unemployment. Parental education is an important predictor of offspring completed education and labour market attachment and differences in this respect are small, with a somewhat higher education level among mothers in the Swedish sample while it is higher among fathers in Norway. Both the fact that parental education and the share with higher education is quite alike point in the direction of rather similar labour market prospects for the two samples.

Differences in social security benefits prior to unemployment shown in Table 2 reflect differences in benefit systems between the countries, with a greater share of the Swedish unemployed having claimed UI benefits, while relatively more having claimed rehabilitation benefits in Norway. Moreover, since disability benefits are not permanent in Sweden for young people under age 30, people enter and leave this status more frequently in Sweden than in Norway. Overall, the descriptive statistics suggest that there are no grounds to assert that the one target group is in a stronger labour market position than the other. Hence, although we cannot assert that the samples are comparable, we have reasons to believe that the institutional context plays an essential role in determining how young people adapt and that our samples, by and large, comprise similar people confined by different settings.

Table 3 shows descriptive statistics by programme category. Participants in *place* and *train* do not appear to be very different when it comes to age, family status, or foreign background, but *place* has a higher share of male participants than *train*, particularly in Sweden. A comparison across programme categories with respect to human capital variables shows that *place* participants stand out as having less education, less employment experience, and are more likely to have received social assistance benefits that do not depend on previous income compared to participants

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[28] From 2004 to 2010, disability pension in Norway was split into a time-limited and a permanent benefit. However, an evaluation of the time-limited disability benefit shows very low outflow rates to work, and for most recipients the time-limited benefit merely served as a postponement of the permanent disability benefit (Bragstad 2009).

[29] Unfortunately, our data do not contain comparable health indicators and we can thus not rule out differences in health or severity of work impairment between the samples.

Table 2 Descriptive statistics for the samples of unemployed young adults with reduced work capacity

	Sweden		Norway	
	Full sample	Estimation sample	Full sample	Estimation sample
Individual characteristics				
Age (at spell start)	27.50	27.50	27.18	26.74
Male	0.540	0.537	0.519	0.524
Married (year of spell start)	0.147	0.147	0.150	0.150
Child (year of spell start)	0.360	0.359	0.426	0.428
Born in Sweden/Norway	0.818	0.817	0.853	0.842
Born in other Nordic country	0.011	0.011	0.014	0.014
Born outside the Nordic countries	0.171	0.171	0.134	0.144
Compulsory education	0.319	0.318	0.513	0.537
Upper secondary education	0.559	0.560	0.394	0.370
Post-secondary education	0.113	0.114	0.076	0.074
Missing/unknown education	0.008	0.008	0.017	0.018
Years employed, previous 3 years	1.231	1.235	1.425	1.386
Sickness benefits, previous year	0.315	0.313	0.210	0.200
UI benefits, previous year	0.285	0.287	0.106	0.111
Rehabilitation benefits, previous year	0.065	0.064	0.262	0.251
Social assistance, previous year	0.302	0.302	0.337	0.349
Disability benefits, previous year	0.089	0.088	0.017	0.015
Parental characteristics				
Mother education: compulsory	0.276	0.277	0.393	0.399
Mother education: upper secondary	0.442	0.441	0.381	0.372
Mother education: post-secondary	0.139	0.138	0.116	0.109
Mother education: missing/unknown	0.143	0.144	0.110	0.118
Father education: compulsory	0.287	0.287	0.310	0.314
Father education: upper secondary	0.382	0.382	0.431	0.421
Father education: post-secondary	0.109	0.108	0.129	0.124
Father education: missing/unknown	0.222	0.222	0.130	0.140
Mother's rank in income distribution	33.34	33.30	36.14	35.48
Father's rank in income distribution	47.58	47.54	59.40	58.99
Number of observations	27,487	26,464	31,451	22,337

Past employment is defined as having wage income > 20 percent of the median for the relevant age group in each country. Data on disability benefits are missing for the first two years for the Swedish sample

Table 3 Descriptive statistics by programme type

	Sweden			Norway		
	Place	*Train*	*No LMP*	*Place*	*Train*	*No LMP*
Individual characteristics						
Age (at spell start)	27.50	27.52	27.50	26.77	26.76	26.74
Male	0.622	0.561	0.480	0.541	0.520	0.522
Married (year of spell start)	0.141	0.150	0.149	0.147	0.176	0.145
Child (year of spell start)	0.342	0.367	0.368	0.434	0.425	0.428
Born in Sweden/Norway	0.797	0.804	0.834	0.808	0.778	0.860
Born in another Nordic country	0.010	0.013	0.011	0.015	0.015	0.013
Born outside the Nordic countries	0.193	0.183	0.155	0.177	0.207	0.127
Compulsory education	0.346	0.308	0.304	0.581	0.512	0.536
Upper secondary education	0.529	0.591	0.570	0.311	0.394	0.373
Post-secondary education	0.116	0.097	0.118	0.087	0.079	0.071
Missing/unknown education	0.009	0.004	0.008	0.020	0.014	0.019
Years employed, previous 3 years	1.118	1.298	1.286	1.136	1.519	1.361
Sickness benefits, previous year	0.272	0.333	0.332	0.172	0.244	0.194
UI benefits, previous year	0.241	0.315	0.307	0.131	0.141	0.102
Rehabilitation benefits, previous year	0.060	0.052	0.070	0.188	0.141	0.282
Social assistance, previous year	0.344	0.293	0.280	0.395	0.299	0.354
Disability benefits, previous year	0.087	0.066	0.095	0.007	0.006	0.018
Parental characteristics						
Mother education: compulsory	0.276	0.265	0.281	0.395	0.355	0.410
Mother education: upper secondary	0.431	0.448	0.445	0.357	0.356	0.378
Mother education: post-secondary	0.137	0.126	0.143	0.095	0.118	0.109
Mother education: missing/unknown	0.157	0.161	0.131	0.153	0.171	0.102
Father education: compulsory	0.289	0.272	0.290	0.306	0.278	0.324
Father education: upper secondary	0.375	0.384	0.385	0.404	0.404	0.428
Father education: post-secondary	0.104	0.102	0.113	0.119	0.126	0.124
Father education: missing/unknown	0.232	0.242	0.211	0.171	0.191	0.125
Mother's rank in income distribution	32.99	32.31	33.76	34.98	36.38	35.37
Father's rank in income distribution	46.68	47.07	48.17	58.44	59.62	58.94
Number of observations	8323	4128	14,013	2288	3527	16,522

Past employment is defined as having wage income > 20 percent of the median for the relevant age group in each country. Data on disability benefits are missing for the first two years for the Swedish sample

Table 4 Duration of unemployment and programmes, shares in programme participation, and transition probabilities to outcomes states, by country

	Sweden		Norway	
	Mean	sd	Mean	sd
No programme	0.530	0.499	0.740	0.439
Place	0.315	0.464	0.102	0.303
Train	0.156	0.363	0.158	0.365
Duration of unemployment (months)	17.44	12.75	21.71	12.75
Duration before Place (months)	9.81	8.57	6.35	7.32
Duration before Train (months)	8.28	7.52	5.92	6.70
Duration in Place (months)	12.01	10.19	13.25	10.04
Duration in Train (months)	4.90	3.89	12.32	10.96
Transitions to regular employment	0.343	0.475	0.251	0.433
During Place	0.034	0.182	0.015	0.123
After Place	0.030	0.170	0.008	0.089
During Train	0.011	0.105	0.009	0.092
After Train	0.023	0.148	0.013	0.114
No programme	0.245	0.430	0.206	0.404
Transitions to ordinary education	0.069	0.239	0.062	0.242
During Place	0.003	0.057	0.002	0.047
After Place	0.005	0.072	0.001	0.036
During Train	0.001	0.033	0.007	0.081
After Train	0.006	0.076	0.004	0.061
No programme	0.046	0.208	0.049	0.215
Censored due to transition to second programme	0.153	0.360	0.039	0.195
Censored due to transition to programme other than place or train	0.010	0.100	0.015	0.122
Censored due to duration > 36 months	0.187	0.390	0.322	0.467
Censored for other reasons	0.247	0.431	0.325	0.468
Observations	26,464		22,337	

Censored for other reasons include the PES reporting 'unknown reason' for Sweden. For Norway censored for other reasons means that the individuals are not found in either the employment or the education register after the unemployment spell ends

in *train* and those who never participate in LMPs. This is the case in both Sweden and Norway. There are small differences and no clear pattern when it comes to parental human capital variables across programme categories in both countries.

Table 4 shows that nearly one in two of the Swedish young adults in our estimation sample participates in LMPs, compared to one in four in Norway.[30] *Place*

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[30] Note that since our estimation sample excludes a large fraction of Norwegian programme participants (all those who began a programme already during the first month of unemployment), we cannot conclude that activation is generally less common in Norway.

appears to be much more used in Sweden than in Norway for our target group: two-thirds of the participants participate in *place* in Sweden, compared to around 40 percent in Norway. On the contrary, about 15 percent of the sample in both countries participate in *train*.[31] Average spell duration is shorter in Sweden than in Norway, 17 months and 22 months, respectively.[32] Average duration of participation in *place* is slightly longer in Norway (13 compared to 12 months), while the duration of *train* is substantially longer in Norway than in Sweden (12 compared to 5 months). This difference has to do with the characteristics of the different programmes, which we will discuss in more detail in the next section. In online Appendix A we show and discuss survival curves by programme category.

The lower part of Table 4 shows transitions to regular employment and education by programme category, during programme participation as well as after programme completion. One out of three in Sweden and one out of four in Norway leave the register of unemployment to start regular employment, and 6−7 percent exit to start ordinary education. The share of spells that are censored due to participation in a second LMP is a lot larger in Sweden (15 percent) than in Norway (4 percent). The remaining observations, amounting to 44 percent in Sweden and 66 percent in Norway, are censored for other reasons. This is partly due to our censoring after 36 months, but also because some leave the registers for unknown reasons.

5 Results

5.1 Main results

We begin by evaluating the overall effect of the policy mix offered in each country, separating the programmes into *train* and *place*. Table 5 shows our main results. Model 1 presents the estimated impact of programme participation, while participating in a programme and after programme completion, on transitions to regular employment and education combined. Model 2 defines success only if programme participation increases the likelihood of a transition to regular employment. On the left we present results for Sweden and on the right for Norway. The models control for all background variables listed in Table 2 except previous benefits, as benefit eligibility rules differ between Sweden and Norway. The models also control for county, year, and quarter as well as the monthly municipal youth unemployment rate.

The sign of the estimates shows whether the estimated impact is positive or negative. Even though estimates cannot be interpreted directly, if the value of the interval for which we measure the effect is sufficiently small, and if the estimate of β is sufficiently small, then $\beta \approx (\exp(\beta) - \exp(0))*100$ approximates the percentage change

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[31] The difference in the use of *place* and *train* between Norway and Sweden holds also when looking at the full sample (rather than the estimation sample).

[32] Note that since we censor unemployment spells after 36 months, the numbers presented here do not reflect complete spell (or programme) durations.

of the effect of the covariate on the hazard rate. For instance, the value of 0.511, in the far right of the second row in Table 5, can be interpreted as approximately equivalent to a 67 percent increase in the likelihood of experiencing a transition to regular employment or ordinary education as a consequence of having participated in *place* in Norway.

The first (Sweden) and third (Norway) columns in Table 5 show the well-established finding of lock-in effects while participating in programmes. While *place* and *train* have similar lock-in effects in Sweden, the lock-in effects are a lot more prominent for *train* than for *place* in Norway. This can be understood in the context that ordinary education is available as an LMP in Norway, but not in Sweden, and can last up to three years. The patterns of lock-in-effects are rather similar both when we look at effects on regular employment and ordinary education (model 1) and when we focus on regular employment alone (model 2).

As regard the after programme-effects, column 2 (Sweden) shows that they are positive for both *train* and *place*, but the impact is substantially larger for *place*. Moreover, the Swedish estimates for *place* are almost identical irrespective of whether education is included in the criterion for success, suggesting that the impact is driven by transitions to regular employment and not to education. The opposite appears to be the case for training, indicating that any positive impact of *train* seems to be driven by transitions to education only. The last column shows positive and significant estimates for Norway, indicating that both *train* and *place* have a positive impact, irrespective of the success measure used. As in Sweden, after-effects in Norway are largest for *place*.[33] In contrast to Sweden, both estimates increase when education is removed as a measure of success, suggesting that it is employment which is driving the desirable results. Noteworthy, the after-effect on employment for *place* is nearly the same for both countries.

To facilitate the interpretation of the results, we have calculated monthly probabilities of experiencing a transition to work/education for a reference person; see Fig. 3.[34] The reference person is 27 years old, male, native born, has no children, is not married, has been unemployed for 8–10 months, has completed upper secondary education, has one year of work experience (during the last 3 years), and lives in the Stockholm/Oslo area. His parents have upper secondary school as their highest level of education, and both have incomes in the lowest category of the parental income rank.

The dashed line shows the monthly conditional probability of having a transition to work/education for this reference person. The baseline is higher for Sweden than for Norway; this may reflect country differences in labour demand or in incentives generated by various benefit schemes. Differences in sample composition and the definition of employment may also play a role. The bars show how programme

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[33] Since training in Norway partly comprises ordinary education, of considerable length in many cases, it can potentially lead to higher productivity, which could materialise in higher earnings. Investigating the impact on earnings is, however, beyond the scope of this paper.

[34] This is done by inserting the estimated parameters into the hazard rate equation. The observed covariates are set to their reference values, while the calendar variables and unemployment rate are set to their average values over the observation period. The unobserved heterogeneity is set to its average value.

Table 5 The effect of LMP during and after programme participation on the transition to employment and education together and on employment alone

	Sweden		Norway	
	During LMP	After LMP	During LMP	After LMP
Model 1: Transition to regular employment and ordinary education				
Train	−0.574***	0.229*	−0.808***	0.199*
	(0.125)	(0.130)	(0.102)	(0.106)
Place	−0.482***	0.795***	−0.256**	0.511***
	(0.091)	(0.103)	(0.117)	(0.132)
Model 2: Transition to regular employment				
Train	−0.552***	0.099	−1.047***	0.269***
	(0.135)	(0.142)	(0.093)	(0.083)
Place	−0.441***	0.794***	−0.046	0.739***
	(0.102)	(0.114)	(0.089)	(0.103)

Estimates and standard errors (in parentheses) for the mixed proportional hazard rate model presented in Section 3. All models include the same controls: gender, age, immigration background, being married, having children, education level, previous employment, each parents' earnings and education level, county, year and quarter, as well as the municipal youth unemployment rate (measured monthly). Observations are censored after 36 months. Model 1 has four mass points for both Sweden and Norway; model 2 has five mass points for Sweden, and three for Norway. The number of mass points is selected according to the AIC criterion. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

participation changes this probability, during and after participation in an LMP, ceteris paribus. The figures show typical lock-in effect of LMPs in both countries, but a stronger lock-in effect of *place* in Sweden than Norway. Regarding the impact after LMP participation, the results are remarkably similar in both countries: the likelihood of entering employment or education is about double as high for a person that has participated in *place* compared to the reference person, and around 25 percent higher if having participated in *train*.[35]

5.1.1 Looking closer at different sub-categories of programmes

So far, we have tried to reach a compromise between the policies of the two countries to highlight the impact of similar broad policy measures in the different settings. However, there are some differences between Norway and Sweden that should not be left unnoticed. These have mainly to do with the particularities of the spectrum of policies available to young adults with reduced work capacity in the two countries.

Model 3, in Table 6, shows separate estimates for different sub-categories of programmes belonging to the *place* category while leaving the *train* category unchanged. *Place* consists of a variety of programmes that differ in terms of length,

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[35] However, in absolute terms the effects are larger for Sweden; note the difference in scales on the *y*-axis.
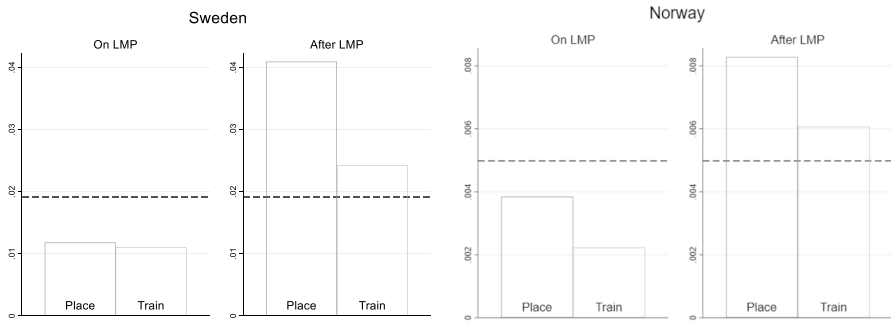
Fig. 3 The effect on employment/education of participation in place and train, during and after programme participation. Sweden (left) and Norway (right)

Table 6 The effect of LMP during and after programme participation on the transition to employment and education together. Programme types are divided into sub-categories

	Sweden		Norway	
	During LMP	After LMP	During LMP	After LMP
Model 3: sub-categories of place				
Train	−0.431***	0.392***	−0.871***	0.148***
	(0.122)	(0.126)	(0.058)	(0.057)
Supported employment	X	X	−0.939***	0.405*
			(0.170)	(0.243)
Work practice	−0.309**	−0.276**	−0.536***	0.245***
	(0.123)	(0.132)	(0.084)	(0.091)
Wage subsidies	−0.257*	1.541***	0.210***	1.091***
	(0.148)	(0.163)	(0.092)	(0.135)
Model 4: sub-categories of train				
Labour market training			0.084	0.123
			(0.130)	(0.115)
Vocational labour market training	−0.094	0.475**	X	X
	(0.234)	(0.233)		
Preparatory training	−0.933***	0.095	X	X
	(0.159)	(0.157)		
Ordinary education	X	X	−1.094***	0.423***
			(0.111)	(0.119)
Place	−0.461***	0.785***	−0.203*	0.574***
	(0.085)	(0.094)	(0.116)	(0.120)

Estimates and standard errors (in parentheses) for the mixed proportional hazard rate model presented in Section 3. All models include the same controls; see Table 5. Observations are censored after 36 months. Model 3 has six mass points for Sweden and 1 for Norway; model 4 has three mass points for both Sweden and Norway. The number of mass points is selected according to the AIC criterion. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

target group, and degree of subsidy. An important dividing line in Sweden is between *work practice* and different types of *wage subsidies.* Average duration is 3.8 months

for *work practice*, 9.6 months for *regular wage subsidies*,[36] and 16.3 months for *wage subsidies targeted at individuals with disabilities* (see Table A1 in the online appendix). Moreover, since participants in *wage subsidies* are employed during participation, the programmes resemble real jobs to a much larger extent compared to *work practice*. In Norway, *work practice* and *wage subsidies* resemble, in broad terms, the Swedish divide, but both last about a year on average. *Supported employment* is often targeted at people with relatively more serious disabilities than the other programmes in *place* and average duration is 18 months.

The results for model 3 show the typical lock-in effects for most sub-categories in both countries. For Norway, however, *wage subsidies* seem to function as a springboard to working life also during participation. The after-effect of *work practice* is negative for Sweden, and positive but modest for Norway. The estimated after-effect for *wage subsidies* is positive, highly significant, and large in magnitude for both countries. As regard *supported employment* in Norway, the after-effect is moderate and not stable, and we cannot exclude the possibility of no effect or a negative effect.[37] Overall, and in accordance with previous findings, our results indicate that programmes that more closely resemble real jobs seem to be most successful in both countries.

Model 4, in Table 6, instead shows separate estimates for different sub-categories of LMPs in the *train* category, while leaving the *place* category unchanged. *Train* for Sweden consists of two types of programmes: *vocational labour market training* and *preparatory training*. The former consists of training for specific professions where there is a shortage of trained workers, while the latter consists of courses that are more general and orientational in nature and which aim to prepare the jobseeker not only for jobs but also for future participation in other LMPs or regular education. Average duration for both categories is about 4 months (see Table A1). *Preparatory training* turns out to be the most common type of training for the individuals in the Swedish sample; 11 percent of the individuals participate in this type of training compared to 4 percent for *vocational labour market training*. In Norway *train* consists of *ordinary education* and *labour market training*. While *ordinary education* can last up to 3 years, labour market training consists of shorter courses (5 months on average). The Norwegian labour market training consists of both preparatory and vocational courses, and our data does not allow us to separate these from one another (hence, in model 4 they belong to the same sub-category). The results indicate that the two sub-categories of training programmes have very different impacts

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[36] Subsidised employment programmes that are not targeted specifically at individuals with disabilities.

[37] The estimates of model 3 for Norway are based on one mass point only, which means in practice that it hardly controls for unobserved heterogeneity. Had we used the ML rather than the AIC criterion to choose the preferred model, the estimate for the after-effect of *supported employment* would not be significant. Since model 3, which divides programmes into four categories for Norway, puts considerably strain on the data, we have also estimated an alternative specification for Norway where we add the two programme categories with the fewest participants (*work practice* and *wage subsidies*). For this specification the estimate for *supported employment* turns negative. Results can be obtained from the authors.

on transitions to jobs and regular education. In Sweden the lock-in effect is much larger for *preparatory programmes*,[38] and these programmes also do not seem to improve outcomes after they are completed. *Vocational labour market training*, on the other hand, has a clear positive after-effect.[39] Still, it is smaller in size compared to the *place* programmes. Estimates for Norway indicate that it is *ordinary education* that drives the positive after-effect found for the *train* category, while *labour market training* has no statistically significant after-effect.[40] Overall, the results for model 4 suggest that the success of training measures is likely to be linked to whether or not they provide certified qualifications.

5.1.2 Heterogeneity across subgroups of unemployed

As the descriptive section shows, compositional differences between the Norwegian and Swedish samples make cross-country comparisons of results not entirely straightforward. We approach this issue by looking closer into three concrete subgroups that share some key common characteristics known to be important for later labour market prospects. We investigate programme effects for young adults who were on sick leave prior to entering unemployment, and therefore are more likely to suffer from health problems. The second subgroup we consider are young adults who previously received social assistance, and as such can be regarded as relatively marginal to the labour market. The third group we shed light on is the more employable young adults with some employment experience during the previous three years. As Table 7 shows, our results are rather robust to changing the target group. The overall patterns remain: negative lock-in effects, clear positive after-effects for programmes providing work experience, while the after-effect for training programmes is less pronounced. Nevertheless, it is worth noticing that the impact of *train* appears to be somewhat weaker for these subgroups, even disappearing in some cases. In the case of Norway, training courses may even be counterproductive for young adults with a history of sick leave. Also interesting to notice is that *train* seems to have a positive impact for those with recent work experience in Norway, but the effect is not statistically significant in Sweden.

We have also investigated potential heterogeneity in effects by gender and education. Differences in impacts between these groups may occur if, e.g., men and women, or unemployed with varying levels of qualifications, are assigned to different types of programmes. Table B5 in the online appendix shows results from separate regressions for men and women, as well as a regression where we exclude individuals

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[38] That we do not find evidence of any lock-in effect for *vocational labour market training* may be due to that individuals who transition to jobs directly after the programme (during the same month as the programme ends) are coded as finding jobs *during* the programme.

[39] The finding that that *preparatory training* does not speed up transitions to employment to the same extent as *vocational labour market training* is in line with results presented in de Luna et al. (2008) for unemployed persons in general.

[40] Notice though that this may be due to that because of small sample issues, it is not possible to investigate the effect on transitions to education separately from the effect on employment. Von Simson and Hardoy (2020) study the impact of programmes for youth 18–23 with work impairment in Norway and find that *labour market training* has a positive effect on transitions to education but not to employment.

with post-secondary education. Overall, there is little evidence of heterogeneous effects in the Swedish sample. In Table B6 we can also see that these groups tend to participate in similar types of programmes in Sweden. The overall conclusions remain also for the Norwegian subsamples, but there are some interesting differences in effect sizes across sub-groups. In particular, young Norwegian men seem to have substantial benefits from participating in *place*-related measures, both during and after programme participation. This may partly be attributed to differences in type of programmes attended within the *train* and *place* categories: Norwegian men participating in *place* more often receive subsidised employment (see Table B6), driving the positive effects of *place* for men.[41] The effect of *place* is also larger for the less educated in Norway. This can, however, not be explained by differences in the shares participating in different sub-categories of programmes, and may thus reflect that less-educated individuals have particularly large gains from *place*-related measures.

5.2 Sensitivity analyses

In our main analyses, participation in *follow-up* measures is considered part of open unemployment. The reason being that *follow-up* measures provide mainly coaching, counselling, mapping, short courses to learn how to write a CV, etc. The duration in *follow-up* is also generally much shorter. Hence, one could argue that these activities do not provide concrete qualifications/work experience but are more of a preparatory and supportive nature. However, since such categorisation is not obvious, we investigate further whether treating these programmes differently in the analysis changes our results. In the top part of Table 8, we show results for our main model specification (model 1), but where we instead censor all observations with a transition to *follow-up* measures (we refer to this specification as model 5). The results show that the lock-in effects remain rather similar for both countries compared to the results obtained for model 1. The after-effect for Sweden remains larger for *place* than for *train*, but the difference between the two programme categories becomes smaller.[42] In Norway, both effects become slightly larger, indicating that participation in *follow-up* prior to *train* or *place* drives the estimates slightly downwards. We can thus conclude that the overall patterns for model 1 are rather stable when it comes to changes in the composition of the reference group.[43]

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[41] As shown in Table 6, wage subsidies in Norway are associated with large positive effects both during and after program participation, while the other *place*-related programmes show more moderate effects.

[42] The Swedish sample is altered quite a lot to meet this condition. Since quite many job seekers with work impairment are registered in the programme *Vocational rehabilitation* for a shorter period before participating in other LMPs, this change of coding implies that we now censor many participants in both *train* and *place* before programme participation. The remaining sample is likely to be a selected group of work disabled.

[43] In Table B9 in the online appendix we present estimates from two additional analyses for Norway that investigate the sensitivity of our results to how follow-up measures are treated; we either control for participation in these measures or include them as a separate programme category. The results indicate that the specification used in Table 5 captures the relevant patterns. These models were not possible to estimate on the Swedish data.

Table 7 The effect of LMP during and after programme participation on transitions to employment and education. Different subgroups

	Sweden		Norway	
	During LMP	After LMP	During LMP	After LMP
Main results (model 1): full sample				
Train	−0.574***	0.229*	−0.808***	0.199*
	(0.125)	(0.130)	(0.102)	(0.106)
Place	−0.482***	0.795***	−0.256**	0.511***
	(0.091)	(0.103)	(0.117)	(0.132)
Individuals previously on sick leave benefits				
Train	−0.589***	0.294*	−1.379***	−0.256*
	(0.162)	(0.173)	(0.241)	(0.182)
Place	−0.752***	0.786***	−0.152	0.808 ***
	(0.134)	(0.163)	(0.183)	(0.199)
Individuals previously on social assistance				
Train	−0.443*	0.241	−0.507***	0.072
	(0.265)	(0.272)	(0.116)	(0.115)
Place	−0.709***	0.713*	−0.336***	0.458 ***
	(0.15)	(0.110)	(0.102)	(0.127)
Individuals with employment experience during the last 3 years				
Train	−0.667***	0.059	−0.856***	0.171*
	(0.121)	(0.129)	(0.098)	(0.099)
Place	−0.761***	0.524***	−0.19*	0.503***
	(0.103)	(0.117)	(0.105)	(0.121)

Individuals previously on sick leave/social assistance are individuals who received these benefits at some point during the year prior to unemployment. Past employment experience is defined as having wage income > 20 percent of the median for the relevant age group in the relevant country. Number of masspoints included: regression 1) four for both Norway and Sweden; reg. 2) three for Norway and two for Sweden; reg. 3) one for Norway and three for Sweden; reg. 4) four for Norway and two for Sweden. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Another aspect we have investigated further is the choice of censoring time. The 'guarantee programmes' in place in Sweden means that there are very few untreated individuals to compare LMP participants with after about 27 months of unemployment (see Section 4.1). In Norway we have the opposite situation, where many young adults wait long before starting a programme. This means that transitions after around 27 months should not contribute much to the estimated effects in the Swedish case, independently of if we prolong the observation period or not, while for Norway we drop a lot of relevant information by censoring early. Choosing to follow observations for a maximum of 36 months is a compromise between the two regimes. Results from model 6 and model 7 in Table 8 show that no major changes to the estimates occur as we allow more or less time to elapse (censoring after 27 vs. 48 months), suggesting that we successfully capture the relevant patterns in model 1. Still, we can observe that censoring after 48 months gives stronger positive after-effects for Norway, indicating that also those that remain in the PES system longer may experience a successful transition.[44]

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[44] After 48 months the observed frequencies of transitions are too low for a precise estimation in the Norwegian data, making it difficult to consider longer follow-up horizons.

Table 8 The effect of LMP during and after programme participation on transitions to employment and education. Different sensitivity analyses

	Sweden		Norway	
	During LMP	After LMP	During LMP	After LMP
Main results (model 1)				
Train	−0.574***	0.229*	−0.808***	0.199*
	(0.125)	(0.130)	(0.102)	(0.106)
Place	−0.482***	0.795***	−0.256**	0.511***
	(0.091)	(0.103)	(0.117)	(0.132)
Model 5: follow-up is censored				
Train	−0.547***	0.453***	−0.757***	0.250 ***
	(0.144)	(0.154)	(0.103)	(0.104)
Place	−0.383***	0.664***	−0.198*	0.588 ***
	(0.118)	(0.130)	(0.109)	(0.121)
Model 6: observations are censored after 27 months				
Train	−0.509***	0.330**	−0.826***	0.191***
	(0.129)	(0.136)	(0.074)	(0.077)
Place	−0.490***	0.713***	−0.247***	0.529 ***
	(0.098)	(0.110)	(0.074)	(0.097)
Model 7: observations are censored after 48 months				
Train	−0.554***	0.249*	−0.764***	0.269**
	(0.124)	(0.129)	(0.105)	(0.112)
Place	−0.556***	0.742***	−0.069	0.749 ***
	(0.082)	(0.090)	(0.126)	(0.142)

Estimates and standard errors (in parentheses) for the mixed proportional hazard rate model presented in Section 3. All models include the same controls; see Table 5. In models 1 and 5 observations are censored after 36 months. Model 1 has four mass points for both Sweden and Norway; model 5 has two mass points for Sweden and four mass points for Norway; model 6 has three mass points for both Sweden and Norway; model 7 has five mass points for Sweden and four mass points Norway. The number of mass points is selected according to the AIC criterion. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

The Swedish data contain information on the type of work impairment that the unemployed are coded with (see Table B2 in the online appendix). To address concerns regarding selection into programmes based on type of work impairment, we have added this information as additional control variables to our baseline model in Table 9. This can be seen as a test of whether our model specification is successful in capturing health-related unobserved heterogeneity. It is reassuring to see that the overall pattern is similar in this analysis. The after-effect for *place* remains larger than for *train*, although the differences are again somewhat smaller.[45]

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[45] In Table B8 in the online appendix, we examine if effects of programme participation differ depending on type of work impairment. We do this by estimating separate models for individuals with physical vs. mental work impairment. The overall patterns are similar for these two groups, with the after-effect for *place* being larger than the after-effect for *train*. However, it is also interesting to note that the after-effect for *train* is larger for individuals with physical compared to mental work impairment. Moreover, Table B6 shows that these two groups of individuals participate in similar types of programmes within the *train* category; hence, differences in effects of *train* are not attributable to differences in the share participating in preparatory vs. vocational training.

Table 9 The effect of LMP in Sweden during and after programme participation on transitions to employment and education together. Controlling for type of work impairment

	During LMP		After LMP	
Main results (model 1)				
Train	− 0.574***	(0.125)	0.229*	(0.130)
Place	− 0.482***	(0.091)	0.795***	(0.103)
Controlling for type of work impairment				
Train	− 0.441***	(0.133)	0.370***	(0.136)
Place	− 0.572***	(0.091)	0.667***	(0.099)

Estimates and standard errors (in parentheses) for the mixed proportional hazard rate model presented in Section 3. Both models include the control variables listed in Table 5; the second model additionally includes dummy variables for categories of work impairment. Observations are censored after 36 months. The number of mass points is selected according to the AIC criterion. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table B7 in the online appendix displays results from several additional robustness checks, showing that the results stay similar if we use the Bayesian information criterion (BIC), rather than the Akaike information criterion (AIC), to select the number of mass points; if we only include the first unemployment spell for individuals who become unemployed more than once during our sampling period; and if we exclude all spells that started in 2009 (i.e., during the Great Recession) from the sample. We also show results for a model that includes an expanded set of control variables, including controls for benefit history as well as somewhat more detailed information on employment history. The results stay very similar also in this analysis.

6 Conclusions and remarks

Successful policies that can mitigate the rising numbers of work-impaired young adults who are outside the labour market are in great demand in many countries. This study compares the impact of labour market policies targeted at unemployed young adults (aged 25–29) with work impairment in Sweden and Norway, two rich advanced economies with highly developed welfare states and vast experience in implementing active labour market programmes. Striking differences exist between the two countries when it comes to the type of measures emphasised to increase the employability of young adults with reduced work capacity. In Sweden, LMPs targeted at this group predominantly consist of programmes providing work practice/experience, while qualifying/educational programmes dominate in Norway.

We use detailed longitudinal administrative data to construct similar data sets in both countries: We sample unemployed young adults with work impairment, who registered at the public employment service (PES) during 2002–2009, and we follow their unemployment spells up to four years. In both countries, the PES plays a

crucial role in defining the target group; being registered as work impaired is related to both the person's health and expected labour market prospects. Descriptive statistics show a lower education level but more work experience in the Norwegian compared to the Swedish sample, while other demographic and parental characteristics are rather similar. We estimate a proportional hazard rate model with competing risks using the framework proposed by Abbring and van den Berg (2003). A special feature of the model is that it controls for sorting effects by explicitly controlling for unobserved heterogeneity. Following a well-established distinction of programme types in the literature, we divide LMPs into programmes providing experience in the workplace (*place*), and training/educational programmes (*train*). We run separate country analyses and investigate how the programme portfolio targeted to young unemployed adults with work disabilities have worked to enhance their employability.

In line with previous studies, we find lock-in effects of programme participation on the probability of transitioning to regular employment or education in both countries. In Sweden, lock-in effects are similar for both programme categories. In Norway, we find stronger lock-in effects for *train*, mainly driven by participation in ordinary education as an LMP which can last up to three years. We find that *place* has a positive impact on transitions to regular employment/education after programme completion in both countries, almost doubling the likelihood of such a transition relative to non-participation. The after-effect for *train* is also positive in both countries, but considerably smaller in magnitude.

Programme-specific analyses shed light on policy particularities. In the case of Sweden, *train* programmes are intended to either meet shortages of staff in professions of high demand (vocational labour market training) or are more general or orientationally oriented (preparatory training). We find that only vocational training has a positive impact on transitions to employment/education after completion. In the case of Norway, the two major programmes in *train* are ordinary education and labour market training (which can be either general or vocational). Our results show that ordinary education is the main driver of the positive after-programme effect for *train*. Moreover, our results show that the positive effect of *train* increases as we allow for a longer follow-up time. As regard *place*, when we separate wage subsidies from the other workplace-related programmes, we find wage subsidies to be the major driver of the positive effects in both countries. The other programmes in the *place* category, such as work practice and supported employment, have more uncertain impacts.

Our data does not permit us to investigate the effects of programme types on transitions to education alone. However, by comparing results from our main specification with a regression that focuses on ordinary employment (only) as a measure of success, some interesting results emerge. For Sweden, the impact of *train* becomes insignificant while the impact of *place* is just as strong. This is indicative of a positive effect of *train* and no effect of *place* on transitions to ordinary education. For Norway, the estimates for both *place* and *train* become stronger, suggesting that both programme categories might have a negative effect on transitions to education, if anything.

To meet concerns regarding cross-country differences in sample composition, we run separate regressions for several more similarly defined subgroups in terms of key background characteristics known to be important for individuals' labour market prospects (history of sick leave, social assistance, and recent employment experience), finding overall rather similar patterns. We also carry out several additional sensitivity analyses, by, e.g., censoring unemployment spells differently and including health-related information as additional control variables, and conclude that the overall patterns found are rather robust to model specifications. For Norway, later censoring results in more positive effects, and as ordinary education offered as an LMP can last up to three years, it is possible that the after-program window does not capture the full effect on employment if censoring too early. It is also important to highlight that we study the duration of unemployment until the first transition to regular employment/education takes place. Previous studies have found that educational/training programs tend to have more positive effects on both employment and earnings with a longer follow-up horizon (e.g., Card et al. 2018; Markussen and Røed 2014; van den Berg and Vikström 2021; Alfonsi et al. 2020). Hence, it is possible that some programme effects would have turned out more positive if we had followed individuals even longer.

Overall, our results for young adults with work impairment align with the main findings in the literature on the effects of LMPs for the general unemployed population in advanced economies: the closer programmes are to the needs of the labour market, both in terms of qualifications and work experience, the more likely it is that programme participation leads to a successful transition (see, e.g., the meta-analysis of Hardoy et al. (2018)). Moreover, contrary to much of the international literature on youth, we find some positive effects of training courses for unemployed young adults with reduced work capacity. The content of the training programmes offered differs between Norway and Sweden, but in both countries, the success of training measures seems to be linked to whether or not they provide certified qualifications.

The adoption of a comparative case study approach that focuses on highly similar cases in crucial aspects makes it inherently challenging to know to what extent our results can be generalised to other contexts. However, the fact that treatment effect patterns and signs of lock-in effects and post-treatment effects are similar for both countries—despite some important differences in policy priorities and characteristics of the pool of unemployed—suggest that they may also extend to other similar countries.

Declarations

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