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Evaluating Labour Market Effects of Wage Subsidies for the Disabled – the Danish Flexjob Scheme

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Abstract

We evaluate the employment and disability exit effects of a wage subsidy program for the disabled in a setting characterized by universal health insurance and little employment protection. We focus on the Danish Flexjob scheme that was introduced in 1998 and targeted towards improving the employment prospects of the long-term disabled with partial working capacity. We find a substantial, positive employment effect of the scheme in the 1994-2001 period within the target group compared to a control group of closely matched ineligibles, but no discernable effects on the probability of disability exit. For the target group employment probability is raised by 33 pct. points after the scheme is introduced relative to a mean employment rate at baseline of 44%. One explanation for a strong employment entry effect concomitant with a non-existent disability exit effect could be that subsidized jobs in this period were mainly granted to the inactive long-term disabled with partial working capacity, i.e., the required medical examination and visitation were able to efficiently separate the more able disabled from the less able disabled. While the former obtained employment through the Flexjob scheme, the disabled with more serious work limitations continued to exit via disability pension even after the introduction of the scheme.

1. Introduction

Welfare states facing future demographic challenges are seeking ways to replace passive support systems containing large work disincentives with active employment-contingent benefit schemes. Particularly in regimes where labour costs are high, such schemes can encourage under-represented groups to enter the labour market. Disabled individuals represent one such group that is only marginally integrated into the work-force at the current time. Yet, many disabled persons can and would be willing to do some work (OECD, 2003).

At the same time, few employment-contingent programs exist that are specially targeted towards the disabled. One such program is the Ticket-to-Work in the U.S. that gives SSDI beneficiaries a ticket, which can be exchanged for a job or support services from public and private providers, employers and other organizations jointly referred to as the employment networks (ENs). However, since the congressional authorization of the program in 1999, fewer than 1,400 of the 12.2 million tickets have been successfully converted to workforce participation. In the U.S. context, however, the reluctance of the elderly disabled to come out of disability is intrinsically tied to a loss of health insurance (Medicare) when leaving DI that makes employment unattractive (Autor and Duggan, 2007). Thus, in countries with universal health insurance systems targeted schemes should be more successful in raising employment of disabled persons. Yet, few such programs exist, and even if they do, no formal evaluation exists of the effectiveness of these schemes in raising the employment of disabled individuals.[1]

[1] Another is the New Deal for the Disabled (Britain), a voluntary program for the disabled that offers job-seeking assistance services through a national organization of Job Brokers. The take-up rate has been low, however, covering only 1.9% of the eligible population (Pires et al. 2006).

The Scandinavian countries, in particular Denmark and Sweden, have been cited for good examples of supported schemes for the disabled, which are adjustable according to a disabled person's ability to work (OECD, 2003). The Flexjob scheme in Denmark (hereafter FJ), which gives wage subsidies to employers for hiring the long-term disabled, has been cited as a scheme that both employers and employees seem to reap benefits from (European Commission, 2002). Since its introduction, the number of applications for disability pension has fallen from 22,000 to 15,000 yearly, and in particular the number applying for the lowest level of disability pension has decreased from 13,000 a year to around 4,000 (Ministry of Finance et al., 2005*)*. Still, this constitutes observational evidence only, and to date no formal evaluation has been made in assessing the effectiveness of FJ in enhancing the employability of disabled individuals. The aim of our study is to evaluate the employment effects of this scheme in the 18-49 population.[2] In doing so, we exploit exogenous variation arising from the introduction of the scheme in 1998. Our main finding is that the employment entry effects of this scheme are quite substantial but that exit to disability is not stemmed as a result.

The rest of the paper is organised as follows: Section 2 summarizes the wage subsidy literature. Section 3 describes the FJ scheme and relevant reforms. Section 4 outlines the empirical strategy and related issues and Section 5 the data and descriptive statistics. Section 6 discusses the results of the estimations, Section 7 undertakes a simple cost-benefit analysis of the scheme and Section 8 offers a brief conclusion.

[2]Early retirement (*efterløn*) is possible and widely used starting at age 60 in Denmark. An extension to the early retirement program was made in 1992 to include the 55-59-year-old long-term unemployed (*overgangsydelse*) and this program was further extended in 1994 to include the 50-54-year-old long-term unemployed. The extension program was annulled in 1996 but with grandfathering, so the 50-53-year-olds who had retired through this program in 1996 would be retired in 2002. Therefore, to avoid any coincidental effects of this program affecting our estimates, age 49 is the maximum age allowed in our sample.

2. The Wage Subsidy Literature

We draw on the insights arising out of a small but well-developed literature that goes back to Kaldor (1936) who, writing at the time of the Great Depression, demonstrated the conditions under which wage subsidies were an efficient tool for reducing unemployment. The optimal solution, according to Kaldor, was if wage subsidies replaced employer contributions to social insurance. Kessleman (1969) also compared wage subsidy schemes to other income-maintenance programs, showing that static disincentives were fewer than in other programs, but that dynamic incentives effects were also important to take into account. A strong case for wage subsidies as a means of reducing unemployment of the least productive workers in high wage regimes has been made by Phelps (1994, 2006). Yet, comprehensive surveys of the available evidence such as in Hamermesh (1978) and Katz (1996) caution that employment effects have been at best modest. Katz formally evaluates the TJTC (Targeted Jobs Tax Credit) program for disadvantaged youth, by exploiting a natural experiment arising out of a change of rules in the program that nullified the scheme for disadvantaged 23-24-year-olds. Comparing the outcomes of this group before and after the reform to the outcomes of non-disadvantaged 23-24-year-olds as well as to placebos sharing common labour market trends, Katz presents difference-in-difference-in-differences (DDD) estimates showing that the program appeared to have modestly raised the employment prospects of economically disadvantaged youth. Another conclusion from this study is that programs combining subsidies with job development, job training and job search assistance appear to be more successful in enhancing employment and earnings of marginal groups than stand-alone subsidies.

More recently, Bell et al. (1999) evaluate the New Deal for the Youth using a trend-adjusted DDD estimator. They find that the employment effects are far more modest than thought and conclude that to a great extent the success of these schemes depends on their incentives (pay-offs) to acquire experience and training. Sianesi (2008) uses Swedish administrative data to match recipients of various social programs to comparable non-recipients and finds that employment subsidies perform the best among a set of ALMP measures in putting the unemployed back to work. Thus, Sianesi takes into account that in welfare state economies a multitude of programs exist which even non-treated individuals have access to. Similarly, Gerfin et al. (2005) use large individual data from administrative sources to compare the effects of two different types of subsidized employment schemes – a pure non-profit employment program and a subsidy for firms operating in competitive markets – and find that the latter is superior in terms of getting the unemployed back to work, an additional employment of about 9 percentage points. However, they caution that there can be large indirect costs of such schemes.

The issue of employment subsidies creating dead-end jobs with little incentive for skill-formation is taken up by a series of recent articles spurred by the work of Heckman et al. (2002), Oskamp and Snower (2006) and Connolly and Gottschalk (2009). The latter also finds that earnings subsidies affect job choice and job duration. However, slightly different evidence is provided by Lydon and Walker (2005) who evaluate the impacts of the WFTC replacing the older FC on gross wages and find that there was faster wage growth even for low-skilled individuals under the WFTC, which may be due to training being general in nature for this group.

Finally, while the studies above mainly have been concerned with employment or earnings effects in the labour market as a whole as a result of subsidies, Kangasharju and Venetoklis (2007) use firm-level data to look directly at employment within firms. Using a large panel sample of Finnish firms, they find positive but not large employment effects and a substitution effect such that public subsidies replaced private employer expenditures, but no displacement effect in terms of crowding out of non-subsidized firms in the same industry or geographical area.

In sum, the wage subsidy literature has found modest employment effects for disadvantaged groups, but also some disincentives for specific skill formation. The consensus seems to be that a proper design of these programs is the key to ensuring both employment integration and also the preservation of incentives. In terms of the disabled, the lack of an incentive to invest in learning new skills to replace lost ones may be relevant, as may be windfall gains going to employers who would have retained the disabled worker otherwise.

We are only aware of a single study looking at the effects of disability policies on the labour market outcomes of disabled workers. A recent paper by Humer et al. (2007) examines the impact of the Austrian Employment Act for the Disabled that grants extended employment protection, requires a hiring quota for firms, and subsidizes the employment of severely disabled (SD) workers. Estimating fixed effects regressions on disabled workers, they show that workers holding a job when acquiring legal SD-status have substantially better subsequent employment prospects after an SD-award than before, while the opposite is the case for those who do not hold a job at the date of SD-entry. These findings suggest that employment protection legislation places substantial

firing costs on firms and has a major impact on the decisions of firms to hire disabled workers. The endogeneity of disability status is modelled as time-invariant unobserved heterogeneity captured via fixed effects.

There is clearly a need within this sparse literature for more evidence from different settings on the effectiveness of schemes to raise the employability of disabled individuals. Our paper adds to this literature by bringing evidence from a setting different from the Austrian one, characterized by universal health insurance and little employment protection. Furthermore, by exploiting variation arising from the introduction of a major reform, we provide reliable evidence on the employment prospects of the eligible long-term disabled compared to observationally equivalent ineligibles.

In the next section, we describe the design of the FJ scheme.

3. The Flexjob Scheme

On 1 January 1998, the Danish government put into force a law introduced by the Ministry of Social Affairs creating permanent wage-subsidized jobs for the long-term disabled known as the Flexjob scheme.[3] The law, which was designed to retain the long-term sick or disabled on the job, grew out of a more active line of social policy embraced by the Danish welfare state since the early 1990s. From this time on, two pillars would be simultaneously emphasized in public policy: *activation* and, starting from 1994, *corporate social responsibility* measures for promoting the inclusion of marginalized groups in the labour market (Rosdahl, 2000).

[3] In Danish, Fleksjob. See §61 and §62, *Lov om aktiv socialpolitik*..

Under the FJ scheme, jobs are both subsidized and associated with special working conditions, e.g. reduced working hours, adapted working conditions, and restricted job demands. Employers, who hire eligible workers, are entitled to a partial wage subsidy – graduated according to the degree of reduction of working capacity – corresponding to either ⅓, ½ or ⅔ of the wage up to a cap of the minimum negotiated wage as stipulated in the relevant collective agreement.[4] The wage paid is for full-time work even though a reduction in hours can be negotiated with the employer. Unlike many other wage subsidy programs, the subsidy is also unlimited in duration, existing as long as the worker retains the subsidized job. To be eligible for a subsidized job, the individual must have suffered a permanent reduction in working capacity and must have exhausted all other avenues of obtaining unsubsidized employment as determined by the competent local government authorities.

In terms of the costs of the program, in 2005 government expenditures on the wage subsidies amounted to DKK 5 billion (0.32% of GDP). Gross public social expenditures in Denmark were 27.1% of GDP (Adema and Ladaique, 2009). In comparison, the government spent DKK 5.2 billion in 2003 on all ALMPs (Ministry of Finance, 2005).

Previous descriptive studies have found that FJ-eligibles tend to be predominantly women (60%) as well as older persons who develop health problems later in life.[5] Not surprisingly, the most commonly stated reason is physical or mental illness, job wear-out and accidents. Most FJ-holders are at the 2/3 subsidy level. Almost 50% tend to be unskilled workers, 35% are skilled workers and 10% are college educated. About half are employed in service jobs in the public and private sectors or jobs requiring few formal

[4] In 2006, the employer was normally entitled to a wage subsidy of up to DKK 115 ($19) per hour.

[5] The age distribution is 5%:<30 years, 20%:30-39, 33%:40-49, 40%:50-59, 2%:>60.

competencies. Register data from DREAM and FLY which records FJ-status from mid-1999 and on show that 70% of FJ-holders come from some form of employment of which about 58% originate from sickness benefits receipt with the typical duration on sickness benefits exceeding 1 year for about half of this group. The rest tend to come from the ranks of welfare recipients or are enrolled in some form for activation before the visitation (DISCUS 2003, 2005).[6]

In 2002, about 18% of private-sector firms and nearly half of all public-sector organizations had one or more employees in some form of subsidized employment, but growth has been largest among private sector-firms (Holt et al., 2003). The same study conducts multivariate analyses and finds that the larger the firm, the higher the share of female employees, and the more contact with the municipality and job placement bureau, the more likely was the firm to have subsidized employees. Another study, which is based on qualitative interviews of 15 FJ-employees, mentions one drawback of the scheme – possible stigma effects of having employers and co-workers becoming aware of the individual's assisted-person status that make such jobs less attractive for disabled individuals. The disabled in that study report an ambivalent attitude to the scheme, feeling integrated on the labour market to some extent, but also partially stigmatized among their co-workers and employers (Hohnen, 2000). Still, the scheme should be attractive from an income-maximizing point of view, as the wage paid is close to the individual's former wage and higher than the replacement rate of government welfare programs.

Since its introduction in 1998, 40,000 individuals have been found eligible for FJ and have undergone formal visitation, far exceeding the initially estimated 23,000 visitations

[6] See also Ministry of Finance et al. (2005).

in 2004. The FJ-program is still in its growth phase, and at present it is expected to mature around 2015 where it is projected to cover 75,000-100,000 disabled persons annually. Job creation, however, has not been able to keep up with this flow with only 30,000 FJs created in 2004, so that currently wait unemployment is around 20%. A report released by the Ministry of Finance et al. (2005) expressed two concerns: a) leniency in the visitation procedure and b) the high unemployment among the FJ-eligible. These concerns suggest some deadweight loss of the program resulting from jobs being assigned to individuals who would have been employed otherwise.[7] We return to this issue in the cost-benefits analysis in Section 7.

(a) Relevant reforms

The FJ-scheme was not a new labour market program and replaced an older wage subsidy program for disabled individuals known as the "50/50" scheme which had been in place since 1995 and in which employers received a 50% wage subsidy for hiring disabled individuals.[8] In 1996, only 2,564 "50/50" jobs were established (Equal Opportunities Centre for Disabled Persons, 1997). FJ, in contrast, is much larger in volume and therefore we focus on this scheme.

The main changes that took place when FJ was introduced in 1998 compared to the existing "50/50" scheme were 1) the graduation of the subsidy according to the degree of disability, 2) from 2000 and on, persons found eligible under FJ but still waiting for an offer were entitled to reduced unemployment benefits during the waiting period and any

[7] Indeed, 50% of all such jobs go to individuals employed with the same firm. In the study by Holt (2003), about half of the 2,495 firms in the private sector that were surveyed reported that one or more of their employees who were working in subsidized jobs would have been employed even without the subsidy.
[8] The "50/50" scheme replaced an even older scheme existing since the mid-1980s which gave employers a 40% subsidy for hiring a disabled worker.

period in-between getting the job, as well as the creation of an early retirement scheme corresponding to the existing labour market early retirement scheme and 3) that the county's expenditures on subsidies were now 100% reimbursable by the state, as opposed to only 50% on the "50/50" scheme. Clearly (1) and (3) expand the employment chances of the disabled, while (2) increases employment chances by extending the duration of unemployment benefits, but reduces them by creating an early exit option. However, our sample does not extend up to the early retirement age so the net effect of the introduction of the scheme on disabled persons' employment chances should be positive.

Faced with concerns described earlier in this section, the Danish government implemented a reform of the FJ-scheme in 2002. The main features were: for all FJ employments that started 1 January 2002 and after, the state refunded 65% of the county's expenditures on the wage subsidy instead of the full amount. And from 1 July 2002 and on, there was no longer a subsidy given for the 1/3 level of reduction in working capacity. However, these changes do not influence our estimates since they take place after our outcome variables are measured in 2001, see the data section.[9]

4. Empirical Strategy

We exploit exogenous variation arising from the introduction of the FJ-scheme 1 January 1998. The treatment group consists of long-term disabled individuals with reduced working capacity, the group eligible for subsidized jobs as described in Section 3. That is, we uncover an intention-to-treat (ITT) effect, which is the average effect of the treatment

[9] In principle, it would be desirable to exploit this variation also, but unfortunately a major reform of the Danish disability system takes place coincidentally in 2003.

on the target group intended to receive the treatment, that is unaffected by dropouts or crossovers. The control group consists of the long-term disabled without reduction in working capacity that is chosen to resemble the treatment group as far as possible in terms of background characteristics, but is ineligible for subsidized jobs as loss in working capacity is the primary requirement for eligibility. Disabled applicants to the FJ-scheme are assigned a work capacity reduction between 0 and 100% where 33% is the (eligibility) threshold.[10]

(a) DD Treatment equation

As the control group is not eligible for a subsidized job, the relevant outcome measure would be employment in regular jobs (which include subsidized jobs for the treated). Following the introduction of FJ, we would expect that employment increases for the treated relative to the controls between 1994 and 2001. Thus, we estimate the following difference-in-difference equation:

$$(1)\ EMP_{it} = \alpha + \beta_1 WORK\_RED_{it} + \beta_2 AFTER_t + \tau WORK\_RED_{it} * AFTER_t + \theta' X_{it} + \varepsilon_{it}$$

where *EMP* is employment of individual *i* in year *t*, *WORK_RED* is an indicator for belonging to the treatment group, (long-term) disabled with working capacity reduction, *AFTER* indicates the time period after the introduction of FJ, *X* is a set of characteristics which control for compositional changes in treatment and control groups over time, and

[10] Unfortunately, the individual's working capacity reduction, which is based on a joint assessment made by medical examiners and the municipality's caseworkers, is not observed in existing registers; otherwise, we could have compared the outcomes of individuals just above and just below the eligibility threshold within a regression discontinuity (RD) design.

τ is the parameter of interest, i.e. the treatment effect, which is the relative difference in the effect of being long-term disabled with a working capacity reduction between the pre- and post-reform periods. As employment is a 0/1 variable, we estimate a linear probability model.

(b) DDD treatment equation

Note that in (1) we make the parallel-trend assumption, i.e. there are no group-specific cyclical trends. It would seem defensible to assume that labour market trends would not affect long-term disabled individuals differently depending on whether they had a reduction in working capacity or not. The common business cycle, which appears from Figure 1, shows an increasing trend. Thus, the unemployment rate is more than halved during our observation window from 12.3% in 1994 to 5.2% in 2001. Therefore, to relax the assumption of a common trend we use the short-term disabled with and without working capacity reduction as additional controls so we can net out changes in employment rates for an observationally equivalent group of ineligibles since individuals must be long-term disabled to qualify for a wage-subsidized job. Thus, the following difference-in-difference-in-differences regression is estimated:

$$(2)\ EMP_{it} = \alpha + \beta_1 WORK\_RED_{it} + \beta_2 AFTER_t + \beta_3 LONG\_TERM_{it} + \beta_4 WORK\_RED_{it} * AFTER_{it} + \beta_5 LONG\_TERM_{it} * AFTER_{it} + \beta_6 WORK\_RED_{it} * LONG\_TERM_{it} + \tau WORK\_RED_{it} * LONG\_TERM_{it} * AFTER_t + \theta' X_{it} + \varepsilon_{it}$$

(c) Announcement effects

The FJ-scheme was introduced by law on 10 June 1997, and the law came into force on 1 January 1998. Compared to the "50/50"-scheme, the FJ-scheme meant that employers received a higher wage subsidy for individuals whose working capacity was reduced by more than 50% and a lower subsidy for individuals with working capacity reduction less than 50%. It is unlikely, however, that the announcement of the FJ-scheme affected the disabled and employers' behaviour significantly, since determination of the degree of working capacity reduction would require an assessment by the county's medical examiners. More importantly, since our "before" observation is from 1994, there is no reason to believe that the announcement of the scheme affects our estimates.

5. Data

The primary data used in this study are obtained from two independent cross-section surveys. The first survey (*The Handicap Survey)* consists of two parts: In the third quarter of 1994, a random sample of 10,800 individuals is asked whether they are disabled or have a chronic disease. 9,188 respond to the survey corresponding to a response rate of 85%. If their answer to the question: "Does your health or disability mean that you find it difficult to do things that most people at your age can do?" is 'yes', or if they indicate that they have some kind of disability, then they receive a second questionnaire in March 1995. 1,633 respond to this questionnaire from which we get information about the working capacity of the disabled individuals.

The second survey is taken from the Labour Force Surveys for Denmark (LFS) that are available each quarter. We use the survey from the second quarter of 2002, which included a special segment on disability. Fortunately, this survey was collected right before the FJ-subsidy was nullified for the group with ⅓ level of reduction in work capacity. 10,900 individuals aged 15-66 years responded to the survey in the second quarter of 2002 corresponding to a response rate of 70%.

The two surveys are merged with register data from 1994 and 2001 respectively from which we obtain information about labour market status (including the outcome variables), labour market experience since 1980, education, family status and region of residence. Although the survey data is taken from the second quarter of 2002, it is merged to the register information from 2001. This is because all register information is recorded in the November of the year, and thus the 2002 register data would potentially be affected by the reform from July 2002.

Despite the wealth of register data available in Denmark, FJ-status has only been registered since the middle of 1999, and the duration on FJ is not known for those entering the state before this point in time. As actual FJ-assignment is only partially available in the registers, eligibility would have to be based on register-based measures of health care and health care usage. In the registers, the health measures available are based on diagnoses made during hospitalization. Defining disability-eligibility solely on the basis of acute episodes would limit the sample to only a small fraction of FJ-eligibles, since the majority most likely suffer from less serious ailments not requiring hospitalization. Our strategy, therefore, is to combine a more broad-based health measure such as survey self-reported disability with register-based measures of health-care usage

(doctor visits, hospitalizations) to enhance validity. The precise criteria used are at least one medical specialist visit or one spell of hospitalization due to illness within the last two years.

(a) Determining eligibility

The most important eligibility criterion for FJ is health (a reduction in work capacity). A second criterion is exhaustion of all other types of unsubsidized employment schemes. In both the Handicap survey and the LFS 2002 second quarter, we observe individuals' self-reported disability status and self-reported loss of working capacity. These two pieces of information will be combined to create eligibility to FJ.

Although the measures of disability are self-reported, individuals are queried about the precise nature of the disability, which should reduce the extent of misreporting. The precise wording of the questions relating to disability and reduction in working capacity is as follows:

Handicap survey (1995):

Do you suffer from any kind of illness, disability, or a functional limitation?

- *Is your working for pay changed in any way because of your illness/disability?*

LFS (2002):

Do you suffer from a permanent health problem or handicap? If yes,

- *Do you experience difficulty carrying out specific tasks on the job/difficulty or handling a normal work load/difficulty as a result of your health problem or handicap?*

In the DD analysis, the treatment group is the long-term disabled with a working capacity reduction, i.e. those who are disabled at least 3 years and who report some kind of working capacity reduction that has changed working for pay or affects either job tasks or work load. The control group will be the long-term disabled without any working capacity reduction. In the DDD, we use the short-term disabled with and without working capacity reduction as additional control groups.

We also omit the small number of disabled individuals who have not worked since 1980 to minimize chances of selecting individuals who have never worked or are unable to work. The resulting sample sizes are 911 observations in the DD analysis and 1,222 observations in the DDD. We retain the sampling weights in the LFS and run weighted regressions in all cases.

(b) Descriptive statistics

The outcome variables are whether or not the individual is employed (0/1) and whether the individual's main source of income is from social disability pension or SDP (0/1). These outcome variables are drawn from the linked register information and are therefore reliably measured.

Employment rates for both long-term and short-term disabled without reduction in working capacity increased as expected from year 1 to year 2 following the business cycle, but for short-term disabled individuals with working capacity reduction this rate decreased, while for long-term disabled with working capacity reduction it remained at the same level over the two years (Figure 2). While the economic boom resulting in increased work pace might explain the lowered employment rate for short-term disabled with reduction in working capacity, the introduction of the FJ-scheme could be one

reason why we do not find a similar trend for the long-term disabled with working capacity reduction. For the long-term disabled with working capacity reduction, the share of individuals on SDP benefits was relatively high and increasing from year 1 to year 2 (Figure 3). A similar, although less pronounced trend, is found for both long-term disabled without a work capacity reduction and short-term disabled with a work capacity reduction suggesting that introducing the FJ-scheme did not lower the exit of long-term disabled with working capacity reduction through the SDP scheme as intended.

Estimations are carried out on the 18-49 age group This age group covers nearly 60% of all FJ-holders (see footnote 5). Individuals 50 and over are omitted from the sample in order to avoid the contemporaneous introduction of an extension to the existing early retirement program for the long-term unemployed, which was opened in 1992 for the 55+, and extended to the 50+ in 1994. The program was terminated in 1996. The controls include year, age-group, regional unemployment rate, gender, education, experience, sector of work, lagged unemployment degree[11] and family status.

Table 1 shows summary statistics for the "before" period, 1994/95, for the dependent variable and the background variables for each of the four groups in the analysis. The employment rate is lowest among treated individuals – 44%, i.e. less than half are employed. The employment rate is highest for disabled individuals with no working capacity reduction (70% for long-term and 71% for short-term) suggesting that the categorization of individuals in this group as having full working capacity is correct. The treated also have the highest share of disability pension (one third). A greater share of the disabled is female corresponding to the earlier observation that FJ-eligibles tend to be predominantly women. In particular, the long-term disabled are disproportionately

[11] The fraction of the year (0-1000) the individual is unemployed.

female. Marital status rates are similar across treated and control groups. As expected, long-term disabled individuals with partial working capacity are on average older than the other groups. Thus, a full 75% of this group is 35-49 years compared to 55-59% of the control groups.

In the analyses, we control for the regional unemployment rate to take into account variations in the business cycle, and this is about 12% for all groups in 1994/95 as shown in Figure 1. Individuals with vocational education make up 40% of each of the groups and individuals with higher education about 10-16%, so education does not vary much between treatment and control groups. Work experience is lowest among the disabled with working capacity reduction, about 1 year less on average. Finally, there are no differences in sector of the most recent job (42-44% of individuals work in the public sector) although the disabled with partial working capacity are more likely to have missing sectoral information.

6. Results

Table 2 presents the DD and DDD estimates where the long-term disabled with work capacity reduction is the treatment group and the long-term disabled is the control group in the DD (columns 1 and 2) and the short-term disabled with and without reduction of working capacity are additional controls in the DDD (columns 3 and 4). Results are shown both without controls (columns 1 and 3) and with controls (columns 2 and 4). In the DD model, we find as expected that having a reduction of working capacity significantly reduces employment probability by 26 pct. points without other controls and

nearly 9 pct. points with controls. The employment rate is higher in 2002 than in 1994/95 due to the business cycle by 7.3 pct. points, but this is not significant. In the DD model, the treatment effect of FJ is a 3.5 pct. points increase in employment and insignificant. The weak effect of the treatment may be due to differential trends among the treated and the control samples, that is the long-term disabled without any reduction in working capacity may have faced a stronger employment rate over the period due to the improving economic conditions which biases the treatment effect downwards. To allow for such differential trends, the DDD model is estimated drawing on the short-term disabled as additional controls. The results show a strong positive treatment effect of FJ on the employment of the eligible long-term disabled of 27.5 pct. points without controls and 33 pct. points with controls. These two results suggest that the treated and control samples are very similar and therefore, it seems reasonable to believe that the two groups would face a parallel trend in absence of the FJ. The effects should be seen relative to a mean employment rate of the treated at a baseline of 44%. Thus, the results show that employment for the long-term disabled with partial working capacity is significantly improved after the introduction of the FJ-scheme in 1998, suggesting that this scheme does work as intended for the age group 18-49 years. Parameter estimates for the remaining background variables conform to expectations.

On the intensive margin, there appear to be no effects of the FJ scheme on hours worked, conditional on positive hours in both the before and after periods. This holds whether hours is measured as register-recorded contractual weekly working hour intervals (<10, 10-19, 20-29 or 30+) or as survey self-reported weekly hours (entered either as levels, logs or as a 0/1 dummy for more than 30 hours). If subsidized jobs at

reduced hours were mainly being awarded to employees already employed within the firm, we would expect to see a reduction in hours worked in the eligible group after the reform. The fact that we do not observe an effect along on the intensive margin may indicate that deadweight loss effects may not be as substantial as suggested by the earlier descriptive literature.[12]

Although the introduction of the FJ-scheme seems to improve employment for long-term disabled individuals with partial working capacity, does it reduce their use of disability pension? In Tables 3 we explore this effect using both DD and DDD approaches. We find no significant effect on reducing outflow to social disability pension among the treated. Again, we find quite similar results without and with controls supportive of our maintained hypothesis that the treated and control samples would face a similar trend in absence of the FJ.

In Appendix Tables A1-A2, robustness checks are presented for each outcome measure respectively. Four sensitivity checks are attempted based on the DDD model in each case: i) omitting lagged unemployment (past labour market history), which may be soaking up a lot of the underlying variation; ii) relaxing the definition of long-term disabled to having a disability for at least 2 years; iii) tightening the definition of long-term disabled to having a disability for at least 5 years; and finally, iv) using only the self-reported measure of disability without adding register-based criteria of doctor visits and hospitalizations. Treatment effects on employment are robust to these changes, although, as can be expected, only half as large when the definition of "long-term" is made stricter to be "at least 5 years" – 15.3 pct. points compared to 33 pct. points. Effects on social disability pension (SDP) are insignificant no matter the specification.

[12] Results are not shown here but are available on request.

7. Cost-Benefit Analysis

We present some simple cost-benefit calculations to be able to make an approximate comparison of the cost-effectiveness of the FJ scheme to other types of public investment programs for the unemployed (see e.g. Jespersen et al., 2008, and Danish Economic Council, 2002). We assume that a typical long-term disabled individual is 40 years of age in 2001, is full-time, full-year employed in a subsidized job[13] and will continue in subsidized employment for the next 19 years until she or he turns 60 which is the age of first eligibility for early retirement in Denmark. Following the approach outlined in the Danish Economic Council (2002), we assume a real interest rate of 6% to calculate the net present value of costs and benefits, taking into account the following components:

- *our estimated employment effect of the scheme*;
- *average transfers to those out of employment, treatment group*; Based on mean values for 2001, we assume that 75% of the non-employed in the treatment group are on social disability benefits (of these, 55% are singles), while the remaining 25% are on reduced unemployment insurance benefits
- *average transfers to those out of employment, control group*; We assume that non-employed in the control group are on full unemployment insurance benefits
- *earnings for those employed*; We use the 75th percentile annual earnings among those with positive earnings in 2001 to ensure a full-time, full-year earnings level

[13] Recall that the wage paid is for full-time work even though a reduction in hours can be negotiated with the employer.

- *the reduction in costs to society*; This arises as a consequence of private firms financing part of the wages for individuals in subsidized jobs in the private sector. We assume that, on average, working capacity is reduced by 50% for individuals in the treatment group implying that the wage subsidy is 50% of the wage. Further, we assume that 50% of individuals in subsidized jobs are employed in the private sector
- *the economic distortion*; This is related to levying of taxes to finance subsidies to employers; Direct resource costs of the program are assumed to be zero as the subsidies make up the greater part of the costs. Note that the subsidies to employers should be thought of as transfers going from one group in society to another, but these transfers still need to be financed out of marginal tax revenues and therefore will lead to deadweight loss. Following Jespersen et al. (2008), we assume a deadweight loss of 75% of the public expenditures, which is the midpoint of the range of estimates found for Denmark by Kleven and Kreiner (2006).

Table 4 shows the values used for the calculation of the various components. We find that there is a net social return of the FJ-scheme over a 19-year period of DKK 291,648 (EUR 38,886) for each individual in the scheme. In comparison, Jespersen et al. (2008) find that private job training for the unemployed is the most cost-effective training program and generates a surplus of about DKK 279,000 (EUR 37,200) per participant over an 11-year period. On an annual basis, therefore, the FJ-scheme appears not as cost-effective as private job training (EUR 2,046.7 vs. EUR 3,381.8). Furthermore, the

estimates reported above do not take into account deadweight losses. The qualitative evidence reported in Section 3 suggested that about half of all subsidized jobs went to individuals who would have been employed even in the absence of the program. Both subsidized jobs and job training programs suffer from potential substitution and displacement effects as well as foregone leisure effects. If all such costs taken together are assumed to be 80-90% of the estimated employment effect as is the approach adopted by Jespersen et al. (2008)[14], we end up with a positive annual net social return of the FJ-scheme of only DKK 131.0 (EUR 17.5) per person per year. In comparison, although not reported in their paper, Jespersen et al.'s estimates taking into account deadweight losses of substitution and displacement amount to 85% of the earnings effect end with a net social return of DKK 7,690 (EUR 1,025) per person per year. Private job training turns up a surplus even when accounting for displacement and lost leisure because the effect of income transfers is relatively high. An 85% rate of deadweight loss, however, is likely to be a conservative estimate given our findings of no decline in hours of work of those in employment following the introduction of the Flexjob scheme in Section 6, pg. 21.

8. Conclusions

This paper evaluates the employment and disability take-up effects of the introduction of the FJ-scheme in Denmark among prime aged long-term disabled individuals with some degree of working capacity reduction. Because our treatment group is defined on the basis of eligibility, we uncover an intention-to-treat (ITT) effect, which may be the

[14] In their case, however, applied to an estimated earnings effect.

relevant parameter when considering large-scale, universal government programs. We find a substantial positive employment effect of this scheme, but no discernable effect on disability exit. For the long-term disabled with a working capacity reduction in the 18-49 age-group, employment probability is raised by 33 pct. points after the scheme was introduced relative to a mean employment rate at a baseline of 44%. However, we find no effect of the scheme in terms of stemming their exit to disability pension.

One explanation for the co-existence of a strong employment entry effect concomitant with a non-existent disability exit effect among the long-term disabled with partial working capacity following the reform could be that subsidized jobs in this period were indeed being granted to the long-term disabled with remaining work capacity. That is, the medical examination and visitation required by the FJ-scheme was able to efficiently separate the more able disabled from the less able disabled. While the former were brought into employment through the Flexjob scheme, the latter were granted disability pension as before leaving the probability of disability exit unchanged. Subsequently, the Danish government tightened the criteria for disability pension in 2003 lending credence to the explanation that disability was being granted to long-term disabled individuals who were unable to be activated over this period. Finally, a simple cost-benefit calculation taking into account potential deadweight loss shows only a very small positive net social benefit of the FJ-scheme, but this is most likely a lower bound.

In terms of external validity our results are best applied to settings characterized by universal health insurance and little employment protection and show that wage subsidies in such settings can strongly increase employment among the relatively healthy disabled. Two qualifications should be kept in mind, however: First, in order for these schemes to

be socially efficient, potential deadweight losses, substitution and displacement effects need to be minimized, and second, the redesign of disability pension and other exit schemes must be instituted at the same time if the goal is to raise the employment prospects of disabled individuals with partial working capacity.

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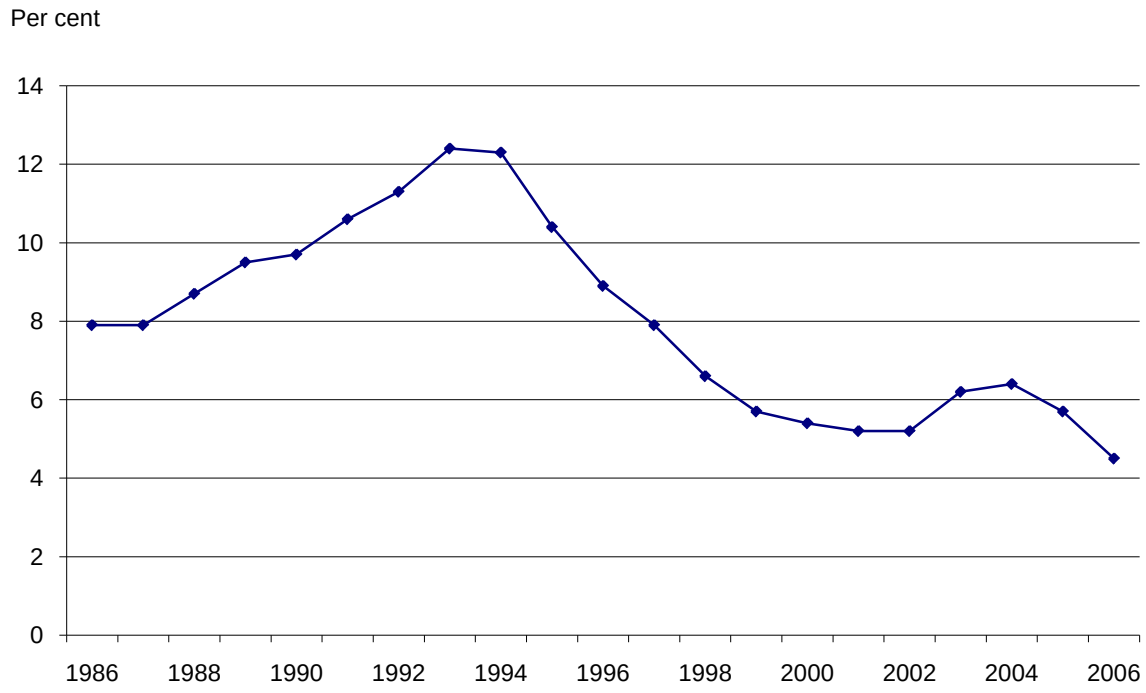
Figure 1.

Unemployment rate over time, 1986-2006, Denmark. Per cent.



Source: Statistics Denmark (2007): Statbank Denmark.

Figure 2.

Employment rates for long-term (LT) and short-term (ST) disabled with work capacity reduction (wr) and without work capacity reduction (w/o wr) respectively, year 1 and year 2.

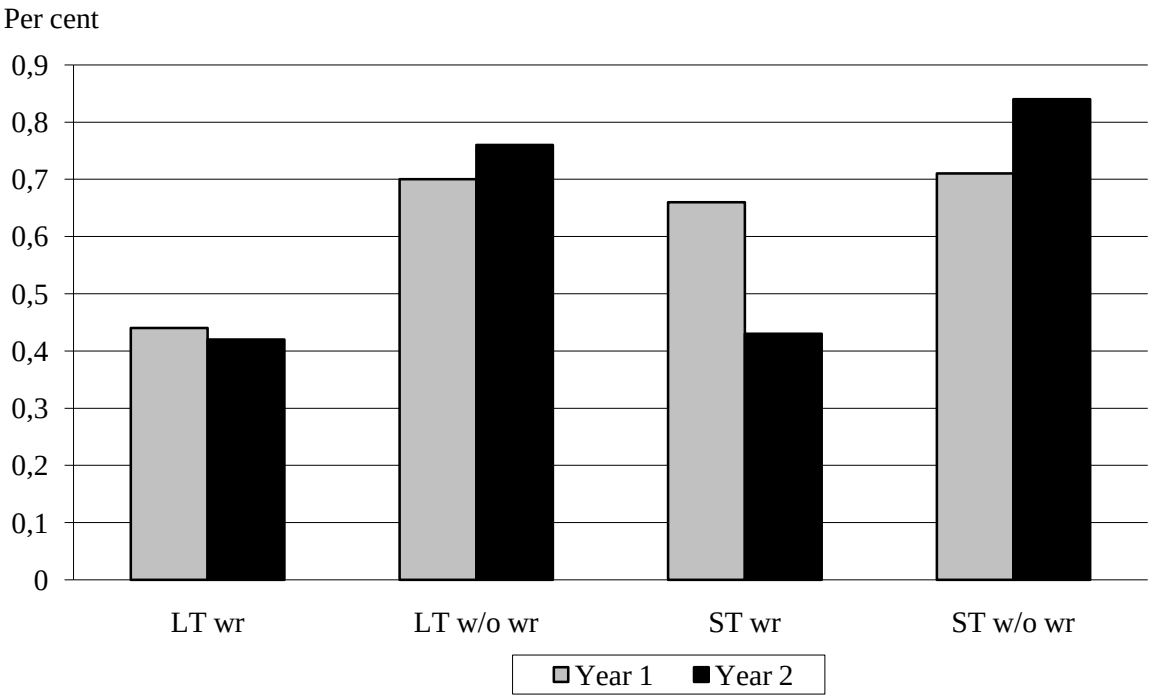


Figure 3.

SDP rates for long-term (LT) and short-term (ST) disabled with work capacity reduction (wr) and without work capacity reduction (w/o wr) respectively, year 1 and year 2.

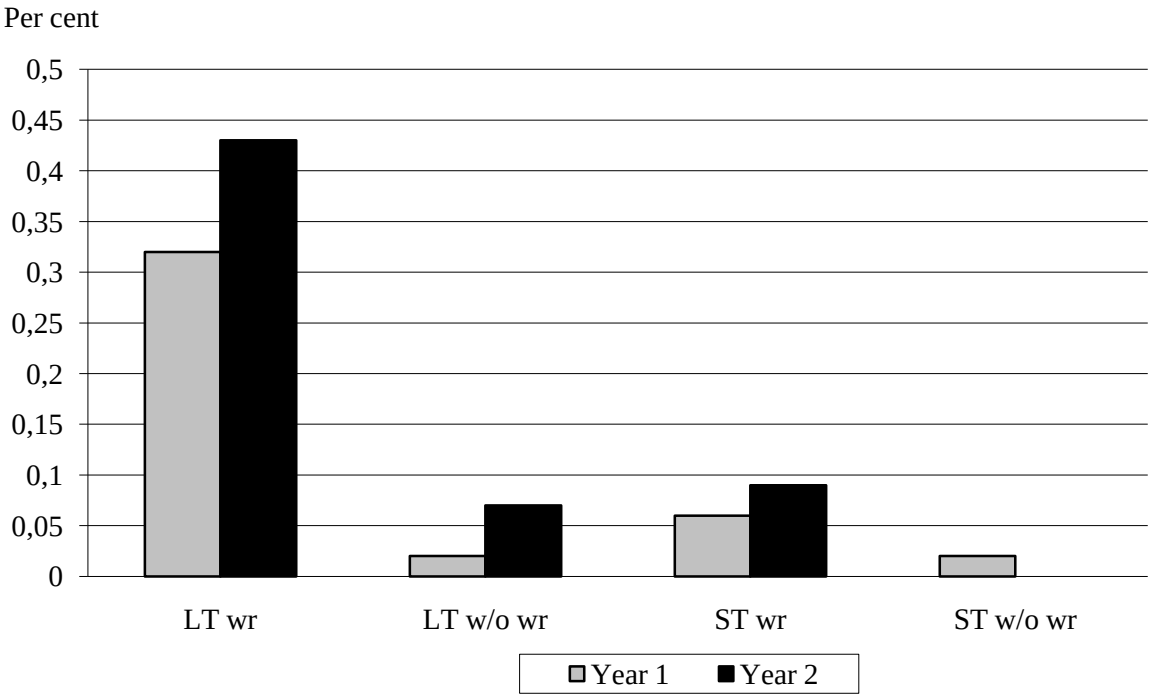


Table 1.

Summary statistics, means. Long-term[1] (LT) and short-term (ST) disabled separately, with and without work capacity reduction. Year 1994/1995 (before).

	Treatment group	Control group	Additional control groups for trend adjustment	
	LT disabled with work reduction	LT disabled w/o work reduction	ST disabled with work reduction	ST disabled w/o work reduction
Employed	0.44	0.70	0.66	0.71
On SDP	0.32	0.02	0.06	0.02
Female	0.65	0.61	0.56	0.58
Single	0.35	0.34	0.38	0.38
Age 35-49	0.75	0.59	0.55	0.59
Vocational education	0.38	0.38	0.39	0.32
Higher education	0.10	0.16	0.10	0.09
Experience	10.9	12.1	10.5	12.4
	(0.54)	(0.54)	(0.98)	(1.1)
Lagged unemployment	0.87	0.40	0.53	0.41
	(0.07)	(0.04)	(1.0)	(0.09)
Most recent sector: Public	0.43	0.44	0.44	0.42
Sector missing	0.48	0.20	0.35	0.32
Regional unemployment	12.0	12.3	12.1	11.7
rate	(0.19)	(0.16)	(0.30)	(0.28)
N=503	149	245	53	56

[1] Long-term is defined as being disabled for at least three years.

Table 2.

Employment effect of introducing FJs for 18-49-year-olds. Long-term (LT) disabled with work capacity reduction vs. long-term disabled without work capacity reduction.

	DD		DDD	
	No controls	Controls	No controls	Controls
Work reduction	-0.260	-0.087	-0.053	-0.015
	(0.050)***	(0.042)**	(0.089)	(0.068)
LT disabled			-0.012	-0.051
			(0.067)	(0.052)
After	0.054	0.073	0.123	0.070
	(0.041)	(0.064)	(0.071)*	(0.072)
Work reduction * LT disabled			-0.207	-0.068
			(0.102)**	(0.079)
Work reduction * After	-0.073	0.035	-0.349	-0.293
	(0.073)	(0.054)	(0.122)***	(0.093)***
LT disabled * After			-0.068	0.003
			(0.082)	(0.065)
Work reduction * LT disabled * After			0.275	0.330
			(0.142)*	(0.107)***
Female		-0.034		-0.034
		(0.028)		(0.024)
Single		-0.021		-0.019
		(0.028)		(0.025)
Age 35-49		0.086		0.061
		(0.036)**		(0.031)*
Vocational education		0.020		0.019
		(0.032)		(0.027)
Higher education		0.029		0.040
		(0.041)		(0.036)
Experience		0.027		0.027
		(0.007)***		(0.006)***
Experience squared		-0.075		-0.076
		(0.021)***		(0.018)***
Lagged unemployment		-0.239		-0.245
		(0.031)***		(0.028)***
Public sector		-0.018		-0.016
		(0.033)		(0.028)
Sector missing		-0.298		-0.286
		(0.070)***		(0.059)***
Regional unemployment rate		0.003		0.003
		(0.007)		(0.006)
Constant	0.702	0.625	0.714	0.689
	(0.029)***	(0.101)***	(0.061)***	(0.098)***
Observations	911	911	1222	1222
R-squared	0.09	0.49	0.10	0.49

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3.

SDP effect of introducing FJs for 18-49-year-olds. Long-term disabled (at least 3 years) with work capacity reduction vs. long-term disabled without work capacity reduction.

	DD		DDD	
	No controls	Controls	No controls	Controls
Work reduction	0.296	0.168	0.040	0.032
	(0.040)***	(0.037)***	(0.037)	(0.044)
LT disabled			0.007	0.041
			(0.020)	(0.028)
After	0.040	0.005	-0.018	-0.002
	(0.023)*	(0.045)	(0.018)	(0.042)
Work reduction * LT disabled			0.256	0.159
			(0.054)***	(0.056)***
Work reduction * After	0.073	0.011	0.048	0.003
	(0.064)	(0.056)	(0.060)	(0.066)
LT disabled * After			0.058	0.018
			(0.029)**	(0.036)
Work reduction * LT disabled * After			0.025	0.017
			(0.088)	(0.087)
Observations	911	911	1222	1222
R-squared	0.20	0.40	0.21	0.37

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Additional controls include dummies for gender, family status, age, educational category levels, public sector and missing information on sector and continuous variables for experience, experience squared, lagged unemployment and regional unemployment rate.

Table 4.

Values used in the cost-benefit calculation.

	Yearly amounts, 2001-values
Probability of employment for the treatment group	0.42
Employment effect of the FJ-scheme per year (=(0.33 percentage points/0.44 per cent)/7 years)	0.11
Wages for individuals in subsidized jobs (DKK)	205,050
Transfer benefits for non-employed, treatment group (DKK)	143,446
Transfer benefits for non-employed, control group (DKK)	152,880
Subsidy to private firms	51,263
Deadweight loss of taxation, per cent of public expense	0.75
Discount rate	0.06
Net benefits per individual, 2001, DKK/EUR	24,658 / 3,287
Net benefits per individual, over nineteen years, DKK/EUR	291,648 / 38,886

APPENDIX

Table A1.

Employment effect of introducing FJs for 18-49-year-olds. DDD estimates. Robustness checks.

	No control for lagged unemployment	Long-term defined as "at least 2 years"	Long-term defined as "at least 5 years"	Disabled defined only by survey data
Work reduction	-0.030	0.063	-0.091	-0.041
	(0.070)	(0.083)	(0.058)	(0.056)
LT disabled	-0.078	0.002	-0.103	-0.014
	(0.054)	(0.057)	(0.046)**	(0.039)
After	0.075	0.119	0.023	0.069
	(0.074)	(0.079)	(0.064)	(0.055)
Work reduction * LT	-0.089	-0.153	0.031	-0.041
Disabled	(0.081)	(0.091)*	(0.071)	(0.064)
Work reduction * After	-0.304	-0.268	-0.135	-0.233
	(0.098)***	(0.114)**	(0.076)*	(0.075)***
LT disabled * After	-0.001	-0.061	0.074	-0.005
	(0.067)	(0.070)	(0.058)	(0.047)
Work reduction * LT	0.308	0.267	0.153	0.221
disabled * After	(0.113)***	(0.124)**	(0.096)	(0.085)***
Observations	1222	1222	1222	1984
R-squared	0.42	0.48	0.49	0.51

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Additional controls include dummies for gender, family status, age, educational category levels, public sector and missing information on sector and continuous variables for experience, experience squared, lagged unemployment and regional unemployment rate.

Table A2.

SDP effect of introducing FJs for 18-49-year-olds. DDD estimates. Robustness checks.

	No control for lagged unemployment	Long-term defined as "at least 2 years"	Long-term defined as "at least 5 years"	Disabled defined only by survey data
Work reduction	0.037	0.005	0.075	0.003
	(0.043)	(0.035)	(0.041)*	(0.039)
LT disabled	0.051	0.051	0.043	0.024
	(0.027)*	(0.028)*	(0.023)*	(0.019)
After	-0.004	0.020	-0.010	-0.007
	(0.042)	(0.043)	(0.038)	(0.033)
Work reduction * LT	0.167	0.169	0.109	0.192
Disabled	(0.056)***	(0.049)***	(0.056)*	(0.048)***
Work reduction * After	0.007	0.022	0.017	0.069
	(0.065)	(0.065)	(0.062)	(0.060)
LT disabled * After	0.019	-0.004	0.023	0.014
	(0.035)	(0.037)	(0.032)	(0.025)
Work reduction * LT	0.026	-0.007	0.011	-0.036
disabled * After	(0.087)	(0.085)	(0.088)	(0.075)
Observations	1222	1222	1222	1984
R-squared	0.35	0.36	0.36	0.37

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Additional controls include dummies for gender, family status, age, educational category levels, public sector and missing information on sector and continuous variables for experience, experience squared, lagged unemployment and regional unemployment rate.