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Do wage subsidies for disabled workers result in deadweight loss? – evidence from the Danish Flexjob scheme

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Abstract

We evaluate the effects of wage subsidy programs for the disabled, in particular, their potential for welfare-loss reduction vs. deadweight loss creation. We do this in the context of the Danish Flexjob scheme, a large, nation-wide scheme that was implemented in 1998 and targeted towards improving the employment prospects of the long-term disabled with partial working capacity. We analyse the hiring response to a shock in the wage reimbursement amount to certain firms using the program. Firms received a salary reimbursement for both current and new employees granted a Flexjob subsidy. In 2002, the reimbursement to government firms was lowered while the reimbursement to municipal and regional employers remained the same. We combine the reform with unique data on whether or not a new Flexjob hiree was previously employed in a regular (unsubsidized) job at the same firm. Thus, we can investigate whether the changes in the reimbursement amount to governmental units affected the share of Flexjobs within such firms that were allocated to retained employees versus to new hires. The findings show substantial substitution between “insiders” and “outsiders” after the reform. After the reform, governmental firms create fewer Flexjobs. At the same time, the composition of Flexjob hires within such firms changes substantially: the share of new Flexjobs allocated to retained employees is twice as large as it would have been in absence of the reform. The finding on deadweight loss seems to run counter to the theoretical prediction. A possible alternative mechanism for the finding could be that when subsidies are reduced and worker productivity is not known with certainty, employers have an economic incentive to fill Flexjob positions from inside the firm.

1. Introduction

Wage subsidies are regarded as a powerful tool in theory for reducing the welfare-loss of unemployment (Kaldor 1936, Kesselman,1969, Phelps, 1994, Calmfors,1994). These schemes can lower unemployment and, thereby, employer contributions to social insurance. Yet, the empirical evidence show at best modest effects of these programs in raising the employment prospects of the eligible unemployed (Hamermesh, 1978, Katz, 1996). Few employment-contingent programs exist that are specially targeted towards the disabled. One such program is the Ticket-to-Work in the U.S. giving SSDI beneficiaries a ticket that 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, less than 7% of the award cohort from 1998 had enrolled in the program by 2006 (Liu and Stapleton, 2010). In the U.S. context, the reluctance of the elderly disabled to come out of disability is intrinsically tied to a loss of health insurance (Medicare) (Autor and Duggan, 2007). In countries with universal health insurance systems targeted schemes should be more successful in raising employment of disabled persons.¹

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). To date, only few formal evaluations exist of these programs. Datta Gupta and Larsen (2010) use data from two independent cross-section surveys from 1994 and 2001 to assess whether a scheme for the disabled in Denmark, the Flexjob scheme, has been effective in raising the employability of the disabled and lowering their benefit receipt. They find an effect on (the probability of) employment but not on disability receipt. However, measurement issues can arise when comparing the two data sets both because disability is self-reported and because consistent definitions of the eligible are difficult to define.

¹Another program 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).

To date, the literature still lacks large-scale reliable evidence assessing whether a wage subsidy scheme can be effective in raising the employability of the unemployed or out-of-work disabled, or whether it is merely used to subsidize the wages of disabled workers within the firm who may have been employed even in the absence of the subsidy². The aim of this study is to fill this gap in the literature by evaluating the potential of wage subsidy schemes for welfare-loss reduction vs. deadweight loss creation. We provide descriptive evidence on the first question and econometric evidence on the second. We do this in the context of the Danish Flexjob scheme which is a large nation-wide scheme, basing the analysis on comprehensive Danish register data.

The scheme was introduced in 1998 and targeted towards improving the employment prospects of the long-term disabled with a permanent reduction in their capacity to work. Under the scheme, firms receive a salary reimbursement for both existing and new employees who are granted a Flexjob subsidy. Furthermore, eligible workers receive the wage subsidy for the duration of their employment contract. In 2002, the reimbursement to governmental firms was lowered while the reimbursement to private, municipal and regional employers remained the same. Exploiting the exogenous variation arising from the change to the reimbursement structure applying to government firms only, we analyze whether the differential shock in the wage reimbursement amount led to a different hiring response among governmental firms compared to the remaining part of the public sector, i.e. municipal/regional firms. We investigate how the reform affected both level of Flexjob hiring and the share of new Flexjobs that is made up of retained employees versus new hires in the group of government firms and in the comparison group. By retained employees

² We are only aware of one other study looking at the effects of disability policies on the labor market outcomes of disabled workers. A 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 modeled as time-invariant unobserved heterogeneity captured via fixed effects.

we mean those who were employed at the same firm prior to being found eligible for the wage subsidy.

Our main finding is that lowering reimbursement changes public (state) sector firms' preferences between hiring new Flexjob employees and retaining existing employees. After the reform, while state firms indeed reduce the total number of new Flexjobs created as theory would predict, they are also much more inclined to take in Flexjob hires from within the firm. On the face of it, this would imply that wage subsidies for the disabled do not lead to deadweight loss, rather the contrary. An alternative interpretation is, however, when subsidies are lowered and worker productivity not known with certainty as would be the case for disabled workers, increased internal hiring results.

The rest of the paper is organized as follows: Section 2 summarizes the literature on wage subsidies and deadweight loss. Section 3 describes the Flexjob scheme and the reimbursement reform. Section 4 outlines the empirical strategy and related issues and Section 5 describes the method. Section 6 presents the data and descriptives, Section 7 discusses the results of the estimations, and finally, Section 8 offers a brief conclusion.

2. Wage subsidy schemes and deadweight loss

In the recent empirical literature on the employment effects of wage subsidies, a number of papers find modest employment effects at best coupled with evidence of some degree of substitution or deadweight loss. 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 previously thought and conclude that to a great extent the success of these schemes depends on their incentives (pay-offs) to acquire experience and training. 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.

Blundell et al. (2004) use a difference-in-differences approach to evaluate the impact of a mandatory job search program for young people in the U.K. In a setting of differential timing of the introduction of this labor market program across areas as well as age-related eligibility rules, they are able to identify the treatment effects of the program. That is, the presence of separate pilot areas in which the scheme is implemented before roll-out in the entire U.K. as well as a fixed age threshold (24 years old) of eligibility for the scheme, allow the authors to develop an estimator type that compares before and after employment rates in the treatment and control areas for eligible and ineligible age groups, and in this way obtain a general equilibrium effect taking e.g. substitution effects into account. They find significant effects of the labor market program and conclude that they cannot reject the null hypothesis of no substitution and equilibrium wage effects. However, the authors note that substitution and equilibrium wage effects may cancel each other out and suggest, therefore, further research on this topic.

While the studies above mainly have been concerned with employment or earnings effects in the labor 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.

Rotger & Arendt (2011) also directly address the problem of potential substitution effects in a Danish wage subsidy scheme for the unemployed by looking at firm-level data. They analyse

whether subsidized employment leads to displacement of ordinary jobs which otherwise would have been created or maintained by the subsidized firms in absence of the subsidy. Closely related to this issue, they also analyse to what extent the wage subsidy contributed to net job creation at the subsidized firm. Rotger and Arendt use a matched employee-employer dataset to identify the stock of employees at firms that are divided into a treatment and control group dependent on whether they have hired a subsidized employee. Using monthly data, they apply a difference-in-differences matching estimator to determine if the firm's employee stock is affected by the hiring of subsidized employees. They find that the wage subsidy scheme for the unemployed has a positive net employment and only carry a small, yet existing, substitution of ordinary employees by subsidized employees.

Another paper that directly addresses the issue of deadweight loss of employment subsidies is Betcherman et al. (2010). Here, a difference-in-difference estimator is applied to covered and not-covered provinces to identify effects of two regional employment subsidy programs. The findings show that both programs lead to significant increases in registered jobs in eligible provinces. However, when a measure of deadweight loss is created by constructing the ratio of the total number of jobs subsidized to the total number of jobs created, deadweight losses amount to between 47%-78% in the first program, and between 27%-46% in the second program. Thus, deadweight losses are substantial for the first program, while in the case of the second program, which has a better design with less possibility for firms to manipulate employment, they are smaller.

In sum, the wage subsidy literature has found modest employment effects for disadvantaged groups as well as small substitution effects – unsubsidized workers being replaced by subsidized workers. In terms of the disabled workers, the issue of windfall gains going to employers who would have retained the disabled worker otherwise, i.e. deadweight loss, is relevant, particularly

since a worker's real degree of disability is difficult to measure so that eligibility can be more easily manipulated. However, the issue of deadweight loss has not been explored for the employment of (partially) disabled workers. The literature has been dominated by employment subsidies for unemployed workers in general, but not for specific groups of hard-to-employ individuals. In terms of the Flexjob program, deadweight loss is also a question of growing policy relevance. According to the National Labour Market Authority (2010), half of those directly referred to a Flexjob (i.e. without a period of wait unemployment³ in between), continue in a Flexjob at their previous workplace. Further, Holt et al. (2003) find that about half of the 2,495 surveyed firms in the private sector reported that one or more of their employees who were working in subsidized jobs would have been employed even without the subsidy. These concerns suggest some deadweight loss resulting from flexjobs being assigned to individuals who would have been employed anyway.

There is clearly a need within this sparse literature for more evidence from different settings on the welfare economic consequences of schemes designed to raise the employability of disabled individuals, as well as a need to evaluate the Danish Flexjob scheme. By exploiting variation arising from the reform of the reimbursement structure, we provide reliable evidence of its effect on the hiring response of affected firms compared to unaffected firms. Within our set-up, we are only able to address the issue of whether wage subsidy schemes for disabled workers lower the welfare losses of unemployed disabled individuals using descriptive evidence. We are able to bring causal evidence, however, on whether these schemes used by firms to subsidize the wages of their own disabled workforce who might have been retained nonetheless.

In the next section, we describe the design of the Flexjob scheme and the reimbursement reform.

³ "Wait unemployment" means unemployment among individuals found eligible for a flexjob.

3. The Flexjob Scheme

On January 1st 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 (National Labour Market Authority, 2010).

Under the Flexjob scheme, jobs are both subsidized and associated with special working conditions, e.g. reduced working hours or assistive devices. 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 1/3, 1/2 or 2/3 of the wage.^{4,5} Unlike many other wage subsidy programs, the subsidy is unlimited in duration existing as long as the worker retains the job⁶. Few Flexjob-grantees actually leave the scheme. The majority of those leaving the scheme retire permanently through e.g. disability or old age pension (National Labour Market Authority, 2010).

To be eligible for a subsidized job, the individual must have suffered a considerable and permanent reduction in working capacity and must have exhausted all other avenues of obtaining unsubsidized employment as determined by the competent local government authorities.⁷

In terms of the costs of the program, Denmark is among the few OECD countries spending more than 10% of their disability spending on active labor market programs (OECD, 2010). In 2010, government expenditures on the wage subsidies amounted to DKK 9.5 billion (0.54% of GDP). In total, the government spent DKK 33 billion on all active labor market programs (ALMPs) in 2012 (Ministry of Finance, 2012; Statistics Denmark, 2012b; Ministry of Employment, 2012). Since its introduction in 1998, 85,300 individuals have been found eligible for a Flexjob and have undergone formal visitation, far exceeding the initially estimated 23,000 visitations in 2004. The Flexjob

⁴ In 2011, the employer was on average entitled to a wage subsidy of DKK 184,500 (\$32,000) per man-year (Ministry of Finance, 2012).

⁵ For Flexjobs granted from 1st July 2006 and on, the minimum negotiated wage as stipulated in the relevant collective agreement constitutes a cap for the wage used for calculating the partial wage subsidy. In 2010, the cap was DKK 445,000 (\$79,400) (National Labour Market Authority, 2010).

⁶ This is true for the observation period in this study. From 1st January 2013, Flexjobs are granted for a 5-year period, after which the case is re-examined before it can be extended for another 5-year period.

⁷ In cases where participation in other labor market programs is not relevant, the latter criterion can be waived.

program has until recently been in its growth phase (see Figure A1 in the Appendix), and in 2011 covered around 61,300 disabled persons annually. Job creation, however, has not been able to keep up with this flow with only 389 Flexjobs created in net in 2011 compared to 2010, so currently wait unemployment is around 28%.

The reimbursement reform

One year after the introduction of the Flexjob scheme, on January 1st 1999, a circular letter came into force, granting government institutions reimbursement of all wage expenses paid to individuals granted a Flexjob. Other firms (private, municipal and regional sectors) were still subject to a subsidy of 1/3, 1/2 or 2/3 of the wage, depending on the assessed reduction in the employee's working capacity (limited to 1/2 and 2/3 after July 2002). In May 2002, this additional reimbursement to government entities was reduced to cover only half of the amount not reimbursed by the normal Flexjob scheme for those granted a Flexjob after April 1st 2002. After this change, the subsidy for government institutions is 3/4 and 5/6, while other firms are still reimbursed 1/2 and 2/3 respectively. For the purpose of estimating the effect of the reform, we use a difference-in-differences approach in which we employ municipal/regional firms as the control group, see also Sections 4 and 5.

Before the reform in 2002, the wage subsidy, w_s , paid to governmental units is equal to the market wage w_m (100% subsidy), $w_s = w_m$. To other firms w_s is a function of the percentage loss of working capacity, *WC Loss*,

$$\begin{aligned} w_s &= 0.33w_m, & \text{if } 0.32 < WC\ Loss < 0.50 \\ &= 0.50w_m, & \text{if } 0.49 < WC\ Loss < 0.67 \\ &= 0.67w_m, & \text{if } WC\ Loss > 0.66 \end{aligned}$$

After the reform in 2002, the subsidy to governmental firms is

$$\begin{aligned}
&= 0.75w_m, \quad \text{if } 0.49 < WC \text{ Loss} < 0.67 \\
&= 0.83w_m, \quad \text{if } WC \text{ Loss} > 0.66
\end{aligned}$$

while to other firms, it remains as before ($0.5w_m$ and $0.67w_m$ respectively). Thus, both groups of firms lose subsidization at the lowest level of working capacity loss after the reform, while governmental firms suffer an additional loss⁸.

Theory predicts that when the subsidy level is reduced, deadweight loss should decrease (see Figure A2). Even if the supply of disabled labor is not perfectly inelastic as is depicted in the figure, we would expect a high degree of inelasticity for this group because disabled workers probably cannot increase their labor supply freely when wage increases. In such a case, the subsidy incidence falls to a greater extent on the employee. Deadweight loss is given as the area of the triangle abc when the subsidy level is s , and shrinks to $a'b'c$ when the subsidy is lowered to s' . According to the Danish wage bargaining model, public sector wage determination is negotiated between employers and employee representatives in the collective bargaining rounds with little interference from the government. Thus, even though public sector wages may not adjust immediately, we can expect an adjustment to occur in subsequent collective bargaining rounds, implying the potential for deadweight loss induced by wage subsidization.

We access unique information on whether a new Flexjob hire was previously employed in a regular job at the same firm or originates from outside the firm. Controlling for health and other

⁸Our approach assumes that the share of Flexjobs at the 1/3 subsidy level was not substantially different across treated and control firms before the reform. We are unable to verify if this was the case. Information on the sectorial break-down of Flexjob-eligible at the various subsidy levels is not available, as confirmed by the Danish Agency for Labour Retention and International Recruitment. Even though the regional and municipal sectors most likely resemble the state sector in terms of working conditions and the composition of their Flexjob hires, there could be sectorial differences in this share. We try to control for any remaining differences by including the health of employees in the regressions measured as the number of days with sickness benefits, annual number of doctor visits and intake of various types of prescription medicine.

worker background characteristics, we can treat this as a proxy measure for the deadweight loss arising at the level of the firm. In the next section, we describe our empirical strategy.

4. Empirical Strategy

In order to properly evaluate the total labor market effect of the Flexjob scheme in terms of e.g. lower unemployment, it would require random implementation of Flexjobs to people in different areas, sectors or to people with given measurable characteristics.

Unfortunately (from a researcher's point of view), the Flexjob scheme was implemented universally with no measurable limitations to entry regarding age, education, geographical area, sector or the like. Instead, in order to be granted a Flexjob, an individual assessment is made based on medical examination and caseworker evaluation of the applicant's degree of reduced working capacity.

In the absence of relevant groups to compare with, an applicable econometric strategy would be to compare Flexjob employees to a matched group of (theoretically) identical non-Flexjob employees. This strategy would require some degree of equality between people granted a Flexjob and people not granted a Flexjob and for this strategy to be successful, we would have to assume some inefficiency in the authorities' assessment procedure and/or assume that some individuals eligible for Flexjob do not apply (which would also imply irrationality). Hence we reject this strategy.

Instead, we limit our scope to individuals who have been granted a Flexjob and who are employed either in the municipal/regional or the governmental (state) sector. The two sectors have been subject to different and changing reimbursement rules constituting natural experiments that can be used to evaluate the effect of the Flexjob wage subsidy scheme. Since only few Flexjobs were granted the first year, 1998, we are not able to exploit the initial 1999 reform giving state

firms 100% reimbursement. Instead, we exploit the exogenous variation from a reform of the scheme conducted in May 2002 of the reimbursement rules applicable to government institutions.

Since we analyze sectorial differences, the individuals of interest are necessarily employed, which obviously does not allow us to be concerned with employment probabilities. Instead, we supply descriptive information on the number of newly hired employees⁹ and the development of the share of Flexjob hires over time as indirectly providing evidence of the welfare-loss reducing effect of the scheme in terms of drawing unemployed/out-of-work disabled individuals into the labor market. Our main outcome variable, which we use to reflect any deadweight loss, is whether the Flexjob employee is a new hire in the firm or a retained employee.

Our focus on the extent to which firms retain current employees rather than hiring new ones allows us to evaluate a growing political concern about wage subsidy programs. That is, the extent to which the program is capable of increasing the employability of the targeted individuals who are out of work, i.e. whether the program reduces the welfare losses of unemployment. If the program on the other hand provides subsidies to individuals who would have been hired even in the absence of the subsidy, deadweight loss would occur. It seems reasonable to assume that retained workers – at least to some extent – perform the same tasks as before they were granted a Flexjob, but now with a subsidy.

In the particular natural experiment we exploit in this paper, we examine the behavior of government institutions. We assume that the behavior of all other public sector firms is similar to that of government institutions. Government institutions are fully subsidized before the reform, which is an unlikely scenario compared to the general design of employment subsidies. However, we assume that the firm behavior is linear in the ratio of subsidized wages.

⁹ Here, we also include individuals who were working in the same firm in year $t-1$ but were hired in a Flexjob in year t (i.e. a retained employee).

Based on this empirical setup, however, we are not able to completely quantify the general (labor market) equilibrium effect of the Flexjob scheme.

5. The Model

We exploit exogenous variation arising from the change in reimbursement rules applicable to government institutions from April 1st 2002. The focus is on the average treatment effect of the reform on the composition of Flexjob employees in the period between 1999 and 2005. The treatment group consists of employees in the governmental sector while the control group consists of employees in the regional and municipal sector. The regional and municipal sectors are jointly an appropriate control group for the governmental sector because employment in all these sectors is not – to the same extent as private sector employment – affected by the business cycle.

We apply a difference-in-differences framework in which we compare differences in the composition of individuals being granted a Flexjob before and after the reform. Specifically, we compare the share of retained Flexjob relative to all new Flexjob employees between the treatment and control group before and after the reimbursement reform. The firms that employ individuals in Flexjobs in the governmental sector before the reform receive a more generous subsidy than they do after.

The legitimacy of the difference-in-differences approach is grounded on three assumptions: 1) The change in the reimbursement rules provided exogenous variation; 2) there were no sector specific shocks coincident with the 2002 change in the reimbursement rules, and 3) secular trends in the labor market are common to individuals in both groups. As for the first assumption, the circular letter changing the reimbursement rules was published by the Danish Ministry of Finance on May 28th 2002 – after this date the rules took effect and applied to all Flexjobs even those that

had started earlier, from April 1st, 2002. Hence any bias arising from anticipation of the reform is unlikely.

The second assumption, that there were no sector specific shocks co-incident with the 2002 reimbursement reform might not be fulfilled. On December 14th, 2001 the government imposed a hiring freeze in the governmental sector which lasted about a month, until January 24th, 2002. The hiring freeze did not affect the employment of Flexjob employees. However, subsequent cuts in this sector might have affected Flexjob employment in 2002. Looking at the development in the total number of Flexjobs created in 2002 compared to 2001, we do not see a visible dip, see Figure 1. Still, to avoid any potential co-occurrence of the 2001/2002 hiring freeze with the reform effect we omit information for 2002 in our empirical analysis¹⁰. Another nearly co-incident reform of the Flexjob scheme is the before-mentioned abandonment of the 1/3 wage subsidy in July 2002. This reform did not affect reimbursement, but instead it strengthened the Flexjob eligibility criterion. A working capacity reduction of at least 50% (based on individual assessment made jointly by medical examiners and municipality caseworkers) was now required to be awarded a Flexjob. This reform was global and not sector-dependent. Hence we do not expect it to coincide with our findings in this article (see in addition footnote 8).

The third assumption is that secular trends in the labor market are common to individuals in the treatment and the control group respectively. By exploiting the longitudinal structure of the available data, we can justify the assumption by looking at comparisons of employees in the different sectors for each of our outcome measures in the pre-reform period from 1999 to 2001. Figure 3 shows the development over time for our outcome measure, the share of retained Flexjob employees relative to all new Flexjob employees, in the governmental and the regional/municipal

¹⁰Results are qualitatively similar when we include information for 2002, although the sample size is larger (available on request) and the treatment effect is, as expected, smaller, since any effect on hiring occurs only after the hiring freeze is lifted, i.e. after 2002.

sector respectively. This figure shows a plausibly similar trend among individuals employed in the two sectors before the reform.

DD Treatment equation – retained Flexjob employees

We estimate the following difference-in-differences equation on a sample of newly granted Flexjob holders where the outcome is the likelihood of being a retained Flexjob hire (as opposed to a new Flexjob hire) for the years 1999-2005 (excluding 2002):

$$(1) FJ_RET_{it} = \alpha + \beta_1 POST_t + \beta_2 GOVT_{it} + \tau POST_t * GOVT_{it} + \theta X_{it} + \varepsilon_{it}$$

where $FJ_RET=1$ if the employee i in year t is granted a Flexjob in the same firm where he or she worked in a regular (unsubsidized) job in $t-1$, while $FJ_RET=0$ if i is granted a new Flexjob but was not working in that firm in year $t-1$, $GOVT$ is an indicator for belonging to the treatment group for individual i in year t , government sector employee, $POST$ indicates 2003 and later, X is a set of characteristics controlling for compositional changes in treatment and control groups over time, and τ is the parameter of interest, i.e. the treatment effect, which is the relative difference in the effect of being a government sector employee between the pre- and post-reform periods. As FJ_RET is a 0/1 variable, we estimate both linear probability as well as probit models. In principle individuals could be observed multiple times in the data if they fulfill the condition of not being observed working in the Flexjob-granting firm the year more than once during the observation period. In our data, however, no individual is observed more than once, corresponding to the observation that very few Flexjob-grantees actually leave the scheme, see Section 3.

6. Data

We obtain our data from a Danish longitudinal register dataset created for administrative purposes. The dataset contains the entire Danish population of immigrants and their descendants as well as a representative 10% sample of native Danes. In the estimations, we use weights so that observations

for these groups correspond to their share in ten per cent of the Danish population. The dataset contains information for the period 1986 to 2006.

In the analyses, we restrict our dataset to the period 1999 to 2005, but omit information for 2002, see also Section 5. A major reform of the Flexjob scheme was introduced in 2006, and therefore the period of analysis is restricted to end at 2005. Furthermore, we do not include information for 1998 because the circular letter granting government institutions full reimbursement of all wage expenses paid to individuals being granted a Flexjob was put into force 1st January 1999. We split data into two groups: The treatment group consisting of individuals employed in the governmental sector, and the control group consisting of individuals employed in the regional/municipal sector. In Figure 1 we show descriptive evidence on the number of new Flexjob hires in the control and treatment group both before and after the reimbursement reform. In the pre-reform years, the two groups face a similar increase. After the reform, however, the number of new Flexjob hires is lower in the treatment group compared to the pre-reform period (2003-2005 vs. 1999-2001), while it is higher in the control group. Figure 2 shows the development of the share of new Flexjob hires in the firm relative to all new hires. In the pre-reform years, governmental firms show a stronger rate of increase in Flexjob hires, which reverses post-reform, to the tune of these firms reducing their Flexjob hiring by about a half percentage point in each of the years 2003-2005 compared to municipal and regional firms. Even though this evidence is quite striking, we do not analyze these data within a difference-in-difference framework because the pre-reform trends are not parallel¹¹. This descriptive evidence jointly suggests that lowering reimbursement to government firms reduced their incentives to hire Flexjob workers as intended. We turn next to the econometric analysis on the main outcome, which is the composition of the Flexjob hires.

¹¹Ignoring the parallel-trend assumption and estimating the DiD anyway produces the same result – a 0.05 percentage point reduction in new Flexjob hires compared to all new hires in governmental firms post-reform compared to control firms, both with and without controls, LPM or Probit (available on request).

(a) Sample and variables

Our sample consists of all new Flexjob hires in each of the years 1999-2001 and 2003-2005.

For outcome we employ a binary indicator of whether the newly hired Flexjob employee is a retained employee or not i.e. employed in the same firm before being granted a Flexjob.

As control variables in our difference-in-differences analysis, we include age, gender, an indicator variable showing whether the individual is a non-native Dane, the degree of unemployment the year before the Flexjob was granted, the annual number of days on sickness benefits, the annual number of doctor visits and purchases of different types of prescription drugs. We take all information from the administrative registers. Note that we measure the degree of unemployment on a scale of 0-1000 (scaled by a factor of 100), showing the fraction of the year the individual has been looking for work but unable to find one. We also scale the annual number of days on sickness benefits by a factor of 100. Drugs purchase is our proxy measure of health. This information is available by a link-up to the Danish Prescription Database (*Lægemiddeldata*).¹² All human medicinal products are classified according to the common WHO ACT (Anatomical Therapeutic Chemical) system consisting of a 5-level code. We employ the broadest categorization (1st level), and measure prescription drug purchase in terms of DDD (defined daily dose). In the sample restricted to newly hired Flexjob employees, 89% (94%) of all Flexjob holders in the treatment group and 88% (95%) of Flexjob holders in the control group are registered as having prescription purchase of at least one kind before (after) the reform. The disadvantage (from the researcher's point of view) is that prescription drug purchase is planned in advance and therefore cannot be considered as exogenous as a sudden health event requiring hospitalization. On the other hand, it is a less acute measure of health than the register-based health measures such as diagnosis during hospitalization.

¹²An alternative register-based measure of health is the diagnosis code (ICD) registered for every hospitalization episode. Given the low hospitalization rates under the age of 60, it may be difficult to detect an effect using this measure in a working-age sample.

(c) Descriptive statistics

In terms of the control variables, summary statistics are shown in Table 1. It appears that there are few differences between individuals in the treatment and control group. Thus, age develops similarly pre- and post-reform for both groups, as do degree of unemployment, sickness benefits, and annual doctor visits. Recall that degree of unemployment ranges from 0-100 and shows the fraction of the year the individual has been looking for work but unable to find one. The non-Danish share varies slightly across periods and across groups but within a fairly narrow range, 0.04-0.05. New Flexjob grantees in treated and control firms tend for the most part to have a similar development over time in prescription drug usage of all types between 'pre' and 'post' periods. The only notable difference is in the share male. Treated firms have a higher share of male Flexjob hires than control firms (regional and municipal sector), and there is also a differential development over time in this share. Therefore, it is necessary to control for these variables in the analysis.

7. Results

(a) Main results

The effect on the proportion of retained Flexjob employees relative to all new Flexjob employees before and after the reform appears in Table 1. In the control group, the proportion of retained Flexjob employees is at the same level before and after, namely 56-58 per cent. In the treatment group, the proportion before the reform is 38% and 74% after the reform. This implies a total difference in the before and after differences of 38 percentage points, which is highly significant (see Table 2), showing that lowering the reimbursement increases the proportion of retained employees among the newly granted Flexjob employees. Also after adding covariates, this effect of the reform remains significant, and the difference-in-differences coefficient even increases from 38

to 39 percentage points¹³. The impact is very large relative to the proportion of retained Flexjob employees before the reform (38%, see Table 1). That is, our results suggest that the proportion of retained Flexjob employees in the governmental sector is twice as large as it would have been in absence of the reform. As this result runs counter to the theoretical prediction that deadweight loss ought to fall when the subsidy is lowered, we subject the estimate to a series of robustness checks.

(b) Robustness checks

First, as a placebo test, we examine the impact of the reform year-by-year for each of these outcome measures, see Figure 4. Second, we present a number of other robustness checks in Table 3.

The year-by-year impact of the reform on the proportion of retained Flexjob employees is significantly positive in each of the years from 2003 and on (see Figure 4). The impact increases from 2003 to 2004 and after that, it seems to level out. As expected, we find no significant effect in the before period.

In Table 3, we first test for the presence of any anticipation effects by omitting Flexjob employees employed in the period 1st January 2001 to 31st December 2001 from the analysis and find that the omission does not influence the results. Next, we conduct a placebo test in which we compare the municipal/regional sector with the private sector and find no significant effects.

In the next row of Table 3, adding a number of employees as control variable does make the effect size a little smaller but not substantially so. One may expect that larger firms face lower costs of assessing applicants and would therefore have less of an incentive to intensify their search efforts to identify specific employees and, thus, a greater tendency to hire subsidized employees who they would have hired anyways. On the other hand, large firms also face higher monitoring costs which

¹³ In Table A.1 in the Appendix, we show the results for the full set of covariates.

would lead them to intensify their search efforts (see e.g. Welters and Muysken, 2006 who develop a sequential job search model of hiring costs which they relate to deadweight loss). However, our result remains when firm size is taken into account, meaning that economies of scale in assessments and monitoring effects roughly balance out. Because the data for duration on sickness absence is poorly measured before 2000, especially in the public sector, we check whether the removal of sickness benefits as control variable changes the estimated treatment effect. It does not.

We carry out yet another robustness test in which we change the definition of our outcome to only include those individuals who have not experienced any unemployment in the year of observation. Some retained Flexjob hires may have been employed in the firm, become disabled and subsequently experienced a spell of unemployment as a result of their sickness/disability and then re-joined the same firm. This should not count towards a case of the firm hiring an individual through the subsidy program they would have otherwise hired. Redefining these individuals as non-retained does not change the results.

In Table 4 we present the findings of tests of potential compositional changes between pre-reform and post-reform separately for our sample. The dependent variables are age and length of education in months, respectively. There is no sign of any compositional change in our sample before and after the reform.

(c) Heterogeneous treatment effects

In Table 4, we search for heterogeneity in the reform effect on our outcome with and without covariates. In general we find a similar pattern within almost all subgroups, although the results are stronger for men, for younger workers, for workplaces with > 49 employees, and in particular for the low-educated (less than vocational level). An interesting twist is seen in terms of firm size here. While small firms are *not* more likely to retain their own employees in subsidized positions after

the reform, the results for large firms resemble and, hence, drive those found earlier. The fact that internal hiring is strongest for the low-educated group matches earlier findings in the literature that firms intensify their assessment efforts to find productive workers when the job task to be performed is complex (Welters and Muysken, 2006, Barron et al., 1997, Barron et al., 1987).

8. Conclusions

This paper evaluates (observationally) the welfare loss effect vs. (causally) the deadweight loss effect of a change in the reimbursement structure of a major nation-wide wage subsidy scheme for the disabled in Denmark that applied only to governmental firms. We investigate empirically whether the changes in the reimbursement amount affected the share of new Flexjobs that are retained employees instead of new hires in governmental units compared to regional/municipal units. Descriptive evidence shows that after the reform, governmental firms seemed to be much less inclined to take in new hires. However, based on our empirical analysis we find a substantial substitution between “insiders” and “outsiders” showing that the reimbursement reform in 2002 aimed only at governmental institutions for the purpose of reducing their incentives to hire Flexjob workers changed such firms’ preferences between hiring new Flexjob employees and retaining existing employees.

After the reform, governmental firms created fewer Flexjobs while the control group increased this number. This suggests that after the reform, the treatment group created fewer Flexjobs than they would have done in absence of the reduction in the reimbursement amount. At the same time, the composition of Flexjob hires within such firms changed substantially so that the share of new Flexjobs allocated to retained employees is twice as large after the reform than it would have been in absence of the reform.

Thus, our descriptive results suggest that wage subsidy schemes for the disabled indeed do increase employment as theory would predict. However, the finding on deadweight loss seems to run counter to the theoretical prediction, i.e. showing higher deadweight loss when the subsidy is reduced. An alternative mechanism explaining this counterintuitive finding could be that when subsidies are reduced and worker productivity not known with certainty (as is the case for disabled workers), increased internal hiring is likely to result as firms now have an economic incentive to try to hire productive workers to fill these positions. In the absence of a way to determine productivity of disabled workers, firms resort to more internal hiring. The fact that the strongest evidence of internal hiring is obtained for low-educated workers corroborates this ‘assessment costs’ interpretation of the findings, as the higher the complexity of the job task performed, the greater is the incentive to intensify assessment efforts. In order to decrease internal hiring of disabled workers for wage subsidized positions, there must be a way for firms to screen new hires without incurring costs.

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Figures

Figure 1.
Number of new FJ hires by sector, 1999-2005.

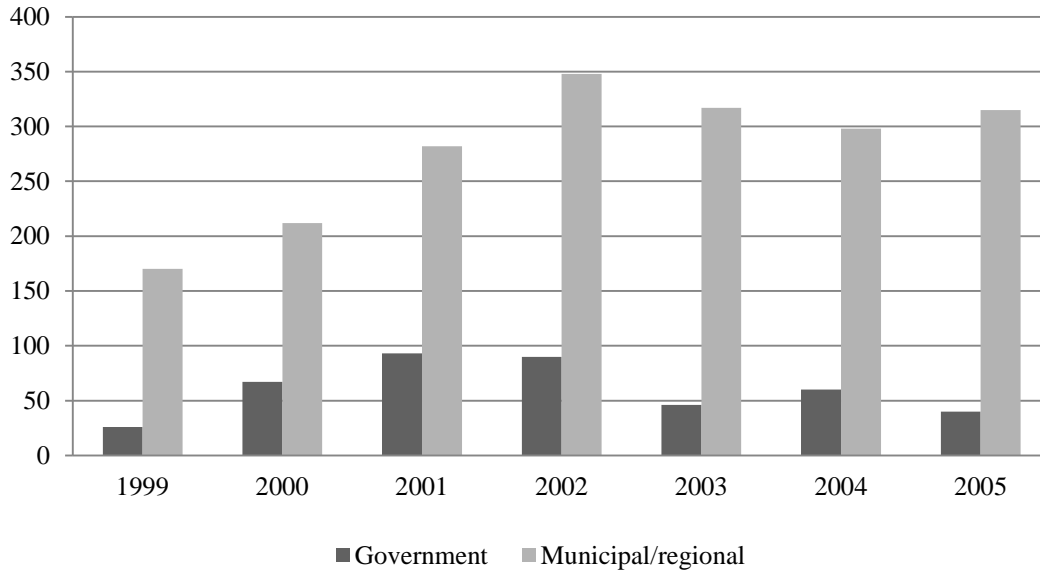


Figure 2. New FJ hires relative to all new employees, by sector, 1999-2001, 2003-2005.
Percent.

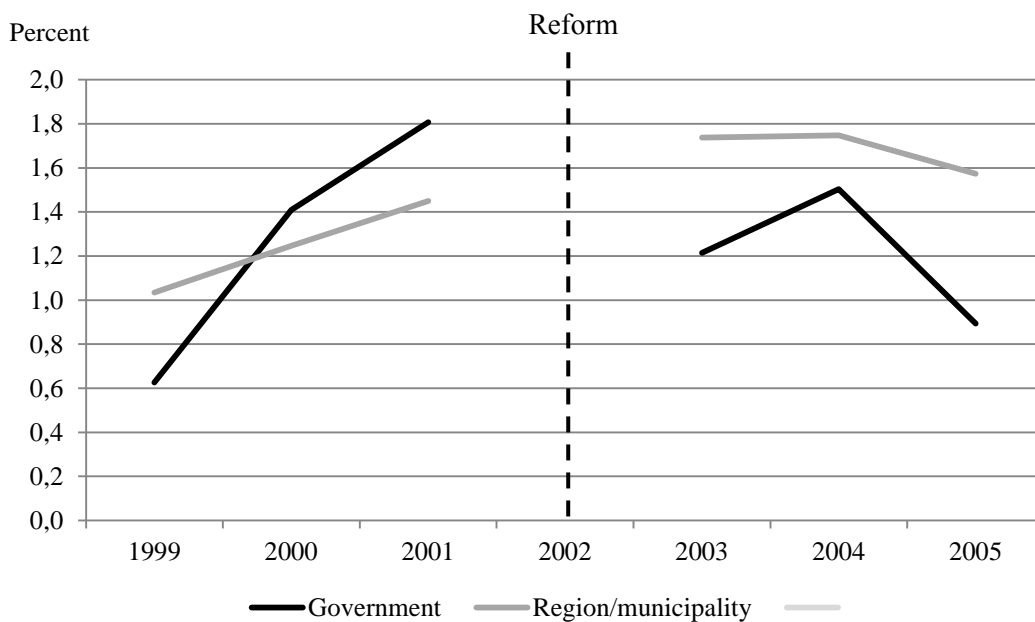


Figure 3.
Retained FJ relative to all new FJ employees by sector, 1999-2001 and 2003-2005. Percent.

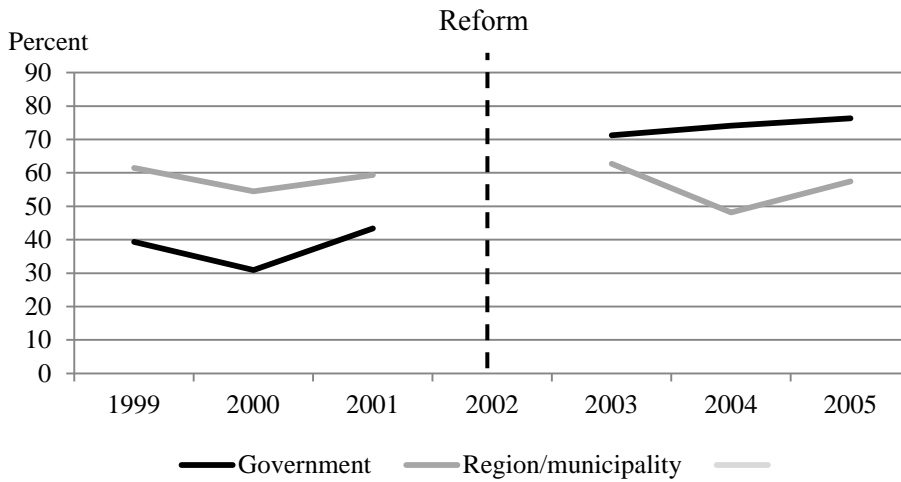
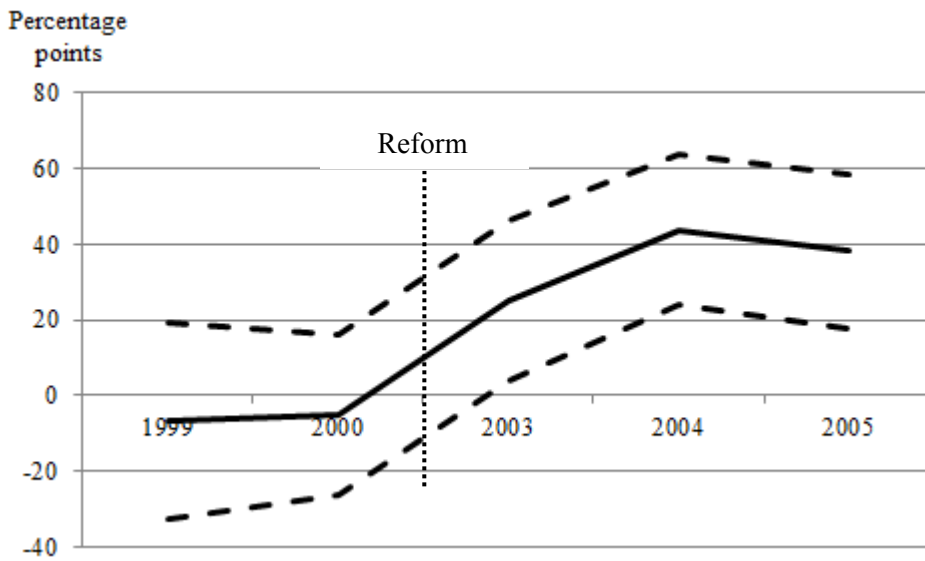


Figure 4. Reform effect on retained FJ year-by-year. Governmental sector compared to municipal/ regional sector. Difference-in-differences analyses. LPM estimates.



Difference-in-differences model. The solid line indicates the effect and the dashed lines represent a 95% confidence interval using robust standard errors. The model controls for age, gender, lagged degree of unemployment, annual number of sick days, annual number of visits to a doctor and use of different kinds of prescription medication. The period before the reform (2001) is set equal to zero.

Tables

Table 1.
Summary statistics for all new FJ hires, treatment and control group, before and after the reform.

	Treatment group		Control group	
	Pre-reform	Post-reform	Pre-reform	Post-reform
Share retained FJ (relative to all new FJ hires)	0.384 ^b	0.738 ^a	0.583	0.561
Non-native Dane	0.053	0.044	0.045	0.045
Age	45.19 (0.740)	47.76 (0.934)	46.15 (0.398)	48.65 (0.329)
Male	0.415	0.470 ^a	0.337	0.229
Degree of unemployment, year t-1 (*100)	6497 (1474)	2732 (1137)	5791 (728)	2694 (382)
Annual number of days with sick benefits (*100)	5667 (762)	7493 (929)	5485 (380)	8067 (359)
Annual number of doctor visits	9.989 (1.62)	12.79 (1.11)	10.32 (0.424)	12.09 (0.362)
<i>Prescription medication:</i>				
Alimentary tract and metabolism	0.234	0.245	0.275	0.260
Blood and blood forming organs	0.130	0.213	0.083	0.154
Cardiovascular system	0.243	0.333	0.268	0.338
Dermatologicals	0.168	0.211	0.217	0.251
Genito urinary system and sex hormones	0.216	0.228	0.212	0.238
Systemic hormonal preparations, excl. sex hormones and insulins	0.042 ^b	0.108	0.090	0.116
Antiinfectives for systemic use	0.321 ^b	0.401	0.404	0.412
Antineoplastic and immunomodulating agents	0.000 ^b	0.057	0.030	0.030
Musculo-skeletal system	0.316	0.533 ^a	0.362	0.445
Nervous system	0.577 ^b	0.641	0.464	0.594
Antiparasitic products, insecticides and repellents	0.035	0.099 ^a	0.048	0.042
Respiratory system	0.252	0.299	0.254	0.294
Sensory organs	0.127	0.170	0.156	0.151
Various other medicines	0.001	0.000	0.004	0.002
Number of observations	186	146	664	930

Notes: ^{a)} Significant difference between treatment and control group in the post-reform period, $p < 0.1$.

^{b)} Significant difference between treatment and control group in the pre-reform period, $p < 0.1$.

Table 2.**Reform effect on Retained FJ. Governmental sector compared to municipal/regional sector, Difference-in-differences, LPM and Probit analyses. Robust standard errors in parentheses.**

	LPM		Probit (marginal effects)	
	Without covariates	With covariates	Without covariates	With covariates
Post	-0.022 (0.029)	-0.064** (0.030)	-0.022 (0.029)	-0.062** (0.028)
Governmental sector	-0.199*** 0.048	-0.188*** (0.048)	-0.194*** (0.046)	-0.183*** (0.046)
Post * governmental sector	0.376*** (0.066)	0.386*** (0.066)	0.319*** (0.042)	0.324*** (0.040)
R ²	0.022	0.079		
Number of observations	1,926	1,926	1,926	1,926

Inference: *** p<0.01; ** p<0.05; * p<0.1

Table 3. Main results and robustness checks on reform effect on retained FJ. Difference-in-differences analyses. LPM/OLS estimates. Robust standard errors in parentheses. Number of observations in brackets.

	Retained FJ	
	Without covariates	With covariates
Main result	0.376*** (0.066) [1,926]	0.386*** (0.066) [1,926]
<i>Robustness checks</i>		
Anticipation effect: Year 2001 excluded	0.416*** (0.079) [1,551]	0.429*** (0.080) [1,551]
Placebo test: Municipal/regional vs. private sector	-0.030 (0.021) [5,223]	-0.051 (0.035) [5,223]
Number of employees added as control variable	-	0.300*** (0.068) [1,632]
Sickness benefits omitted	-	0.385*** (0.066) [1,926]
Retained FJ equal to 1 only if new FJ employees' unemployment rate in year t is zero	0.358*** (0.066) [1,926]	0.368*** (0.064) [1,926]

Inference: *** p<0.01; ** p<0.05; * p<0.1

Table 4.

Test for compositional changes between pre-reform and post-reform periods, all new FJ hires. Dependent variables are age and length of education in months respectively. Governmental sector compared to municipal/ regional sector. Difference-in-differences analyses. OLS estimates. Robust standard errors in parentheses. Number of observations in brackets.

	All new FJ hires	
	Without covariates	With covariates
Age	0.080 (1.296) [1,926]	0.127 (1.226) [1,926]
Education	-0.129 (4.369) [1,862]	0.580 (4.374) [1,862]

Inference: *** p<0.01; ** p<0.05; * p<0.1

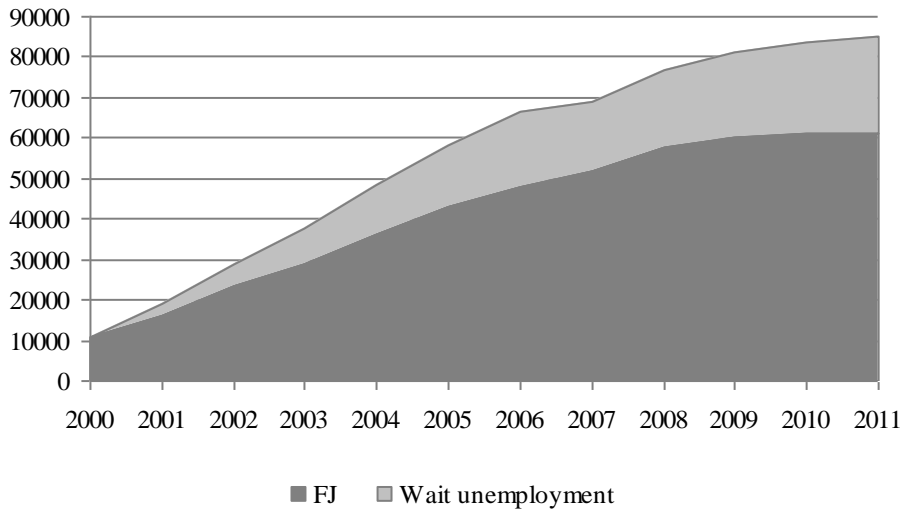
Table 5. Heterogeneity in the reform effect on retained FJ among subpopulations. Difference-in-differences analyses. LPM estimates. Robust standard errors in parentheses. Number of observations in brackets.

	Retained FJ	
	Without covariates	With covariates
Main results	0.376*** (0.066) [1,926]	0.386*** (0.066) [1,926]
Women	0.365*** (0.087) [1,304]	0.395*** (0.087) [1,304]
Men	0.452*** (0.105) [622]	0.434*** (0.104) [622]
Aged < 45	0.419*** (0.115) [666]	0.418*** (0.114) [666]
Aged > 44	0.375*** (0.081) [1,260]	0.348*** (0.037) [1,260]
Low educated	0.539*** (0.114) [729]	0.515*** (0.109) [729]
Highly educated	0.295*** (0.083) [1,197]	0.323*** (0.085) [1,197]
Workplace with < 50 employees	0.223 (0.173) [536]	0.159 (0.167) [536]
Workplace with > 49 employees	0.257*** (0.077) [1,096]	0.262*** (0.076) [1,096]

Inference: *** p<0.01; ** p<0.05; * p<0.1

Appendix

Figure A1.
Growth in the number of Flexjobs and wait unemployment, all sectors, 2000-2011.



Source: Statistics Denmark (2012a).

Figure A2.
The effect on deadweight loss of a reduction in the wage subsidy level

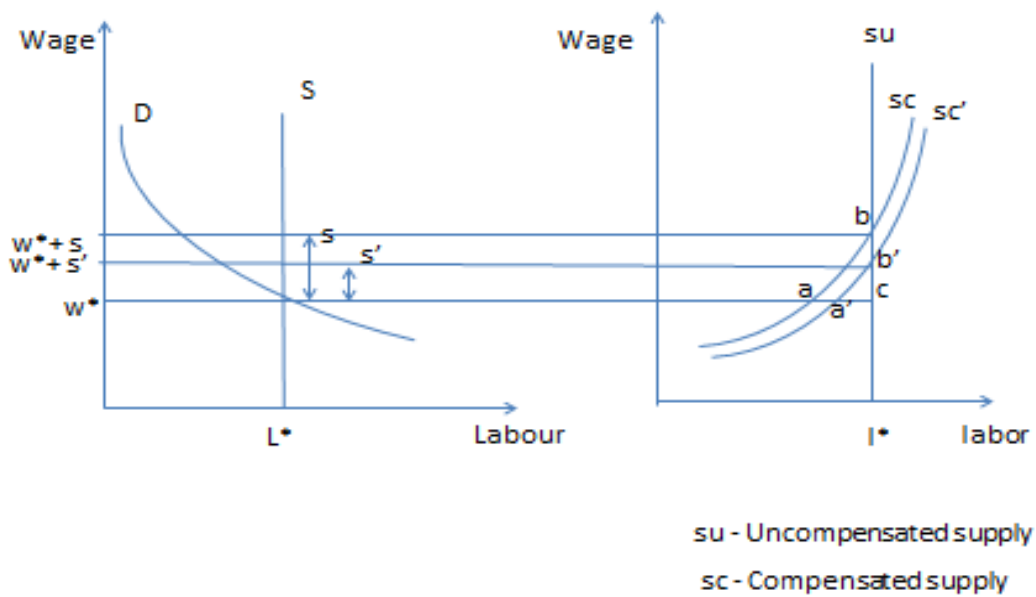


Table A1.

Reform effect on Retained FJ. Governmental sector compared to municipal /regional sector, Difference-in-differences, LPM and probit analyses. Robust standard errors in parentheses. Full set of covariates.

	LPM		Probit (marginal effects)	
	Without covariates	With covariates	Without covariates	With covariates
Post	-0.022 (0.029)	-0.064** (0.030)	-0.022 (0.029)	-0.062** (0.028)
Governmental sector	-0.199*** 0.048	-0.188*** (0.048)	-0.194*** (0.046)	-0.183*** (0.046)
Post * governmental sector	0.376*** (0.066)	0.386*** (0.066)	0.319*** (0.042)	0.324*** (0.040)
Non-native Dane		-0.052** (0.025)		-0.052** (0.024)
Age		0.008*** (0.001)		0.008*** (0.001)
Male		-0.043 (0.030)		-0.042 (0.030)
Degree of unemployment		-0.000*** (0.000)		-0.000*** (0.000)
Sick days		0.000 (0.000)		0.000 (0.000)
No. of visits to doctor		0.001 (0.001)		0.001 (0.001)
<i>Prescription medicine</i>				
Alimentary tract and metabolism		0.035 (0.031)		0.034 (0.031)
Blood and blood forming organs		0.055 (0.041)		0.058 (0.042)
Cardiovascular system		0.029 (0.030)		0.030 (0.030)
Dermatologicals		-0.018 (0.031)		-0.018 (0.031)
Genito urinary system and sex hormones		0.005 (0.032)		0.007 (0.032)
Systemic hormonal preparations, excl. sex hormones and insulins		-0.002 (0.042)		0.001 (0.043)
Antiinfectives for systemic use		-0.042 (0.027)		-0.041 (0.027)
Antineoplastic and immunomodulating agents		0.085 (0.079)		0.079 (0.079)
Musculo-skeletal system		-0.073*** (0.027)		-0.073*** (0.027)
Nervous system		-0.008 (0.028)		-0.009 (0.027)
Antiparasitic products, insecticides and repellents		-0.040 (0.059)		-0.046 (0.060)
Respiratory system		-0.032 (0.030)		-0.033 (0.030)
Sensory organs		0.041 (0.036)		0.042 (0.035)
Various other medicines		0.022 (0.208)		0.029 (0.224)
R ²	0.022	0.079		
Number of observations	1,926	1,926	1,926	1,926

Inference: *** p<0.01; ** p<0.05; * p<0.1

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