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Do wage subsidies for disabled workers reduce their non-employment? - evidence from the Danish Flexjob scheme

Nabanita Datta Gupta[1*], Mona Larsen[2] and Lars Stage Thomsen[3]

* Correspondence: ndg@econ.au.dk
[1]Department of Economics and Business, Aarhus University, Aarhus, Denmark and IZA, Bonn, Germany
Full list of author information is available at the end of the article

Abstract

We evaluate the potential of wage subsidy programs for reducing non-employment of the disabled by exploiting a reform of the Danish Flexjob scheme targeted towards employing the long-term (partially) disabled. Firms received a salary reimbursement for all employees granted a Flexjob. We examine whether a change from full to partial reimbursement to governmental units affected the share of Flexjobs allocated to retained (insiders) versus non-employed hirees (outsiders). After the reform, the composition of hires changed substantially in favor of insiders, both in absolute and relative terms. A reduction in subsidies thus leads to a decrease in the hiring of the non-employed disabled.

JEL Codes: I38, J14, C21

Keywords: Disability; Wage subsidies; Non-employment; Difference-in-differences

1 Introduction

By bringing individuals out of unemployment or other benefit receipt schemes, wage subsidies are in theory a powerful tool for reducing the *welfare loss* of unemployment (Kaldor 1936, Kessleman 1969, Phelps 1994, Calmfors 1994), i.e., the utility loss from being out of work. By lowering unemployment, subsidies can lessen 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 1998, Card et al. 2010)[1].

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 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[2].

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The Scandinavian countries, in particular Denmark and Sweden, have been cited as 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 it is difficult to make consistent definitions of eligibility.

In contrast to wage subsidy schemes for other groups (such as the unemployed), a particular feature of subsidy schemes targeting the disabled is that both current and new employees can be granted a subsidy. Furthermore, especially relevant for such schemes is that the productivity of unemployed or out-of-the-labor force disabled individuals is to a larger extent unknown to employers relative to the productivity of their own disabled workforce. Thus, a wage subsidy scheme for raising the employability of the unemployed or out-of-work disabled may be used instead to subsidize the wages of disabled workers within the firm. This is one of the reasons why public subsidy schemes targeting the disabled might not in fact reduce welfare loss.

The aim of this study is to investigate to what extent this insider/outsider substitution arises in the hiring process due to wage subsidies for the disabled, a question which the literature so far has not addressed. There are other related issues with wage subsidies such as those employed under the scheme may have been employed even in the absence of the subsidy, i.e., deadweight loss, and the fact that subsidized employees may tend to displace non-subsidized employees. These questions are not addressed within the current study. We focus only on the potential of wage subsidies for the disabled for employment creation and the resulting implications for the optimal design of such schemes and welfare-loss reduction. We bring new evidence to the literature, which has up to now mainly evaluated employment quota schemes for the disabled.

Our study exploits a change in the provisions of a major nation-wide scheme for disabled workers, the Danish Flexjob scheme, and we base the analysis on comprehensive Danish register data. The scheme was introduced in 1998 and targeted towards improving the employment prospects of the long-term (partially) disabled who had suffered a permanent reduction in their capacity to work but still retained some remaining work capacity. Under the scheme, firms received a salary reimbursement for both existing and new employees who were granted a Flexjob. Furthermore, eligible workers received the wage subsidy for the duration of their employment contract. In 2002, the reimbursement to governmental units 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 governmental units only, we analyze whether the differential shock in the wage reimbursement amount led to a different hiring response among governmental units compared to the remaining part of the

public sector, i.e., municipal/regional firms. We investigate how the reform affected the number and the share of new Flexjobs made up of retained employees (insiders) versus non-employed hirees (outsiders) in the group of governmental units and in the comparison group. By retained employees, 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 changed public (state) sector firms' preferences from hiring new Flexjob employees from non-employment to retaining existing employees. After the reform, which lowered the subsidy level for state firms but left it unchanged for municipal/regional firms, state firms reduced the total number of new Flexjobs created as theory would predict. Furthermore, public sector firms were – both in absolute and relative terms – less inclined to hire disabled workers from non-employment and much more inclined to take in Flexjob hires from within. Thus, our results show that when subsidies are lowered in a regime where worker productivity is not known with certainty, as is the case for disabled workers, increased internal hiring can result. Put differently, when the salary reimbursement to firms is decreased, there is less scope for welfare-loss reduction.

The rest of the paper is organized as follows: Section 2 summarizes the literature on wage subsidies, section 3 describes the Flexjob scheme and the reimbursement reform, section 4 outlines the empirical strategy and related issues, section 5 describes the method, section 6 presents the data and descriptives, section 7 analyzes the results of the estimations, section 8 discusses implications of the findings and, finally, section 9 offers a brief conclusion.

2 The wage subsidy literature

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 (Bell et al. 1999, Blundell et al. 2004, Kangasharju and Venetoklis 2007. Girma et al. 2008, Rotger and Arent 2011, Betcherman et al. 2010). However, hardly any studies have investigated the potential of wage subsidies targeted towards the disabled as a means for increasing the employment of this group. One study estimating fixed effects difference-in-difference models exploiting the introduction of a wage subsidy scheme in Finland in 2006 for older workers (54+) found little effects of the scheme for the targeted group, arguing that demand for older workers may be quite inelastic and hence unresponsive to financial incentives (Huttunen et al. 2013).

We know of only a few studies directly addressing the issue of subsidies/employment quotas for 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. However, the SD award combines subsidies with other types of incentives to firms so it is difficult to conclude which part of the policy promotes employment of the disabled.

Focusing only on the employment quota aspect of the Austrian Employment Act for the Disabled whereby firms are required to provide at least 1 job to a disabled worker for every 25 jobs to non-disabled workers or otherwise face a non-compliance tax, Lalive et al. (2013) utilize the sharp discontinuity at 25 non-disabled workers and find that firms exactly at the threshold employ 20% more disabled workers than firms below the threshold. The main results are found by considering the first threshold only, firm size above or below 25, but this is the most relevant threshold capturing the majority of firms in the Austrian labor market. Furthermore, their results apply to private sector firms only. They disaggregate the treatment effect and find that 50% of it is due to firms retaining their own former employees, 42% arises due to firms employing disabled workers from other firms and 8% due to firms hiring disabled workers out of employment.

While many countries have adopted employment quotas of the above type for increasing the employment of the disabled, quotas are typically only filled in 50-75% of cases, fines are not always imposed, and furthermore, a quota may result in a cap on disabled employment if firms satisfy the required hiring and no more (Mont 2004). This should not be the case for wage subsidy schemes for the disabled since there is no fixed upper limit to hiring; however, whether subsidies lead to greater job creation for the disabled than quotas would depend, in part, on the size of the subsidy relative to the expected value of the fines.

Still, we have not found many studies that can analyze the potential for wage subsidies to increase employment of the disabled.

A recent paper by Baert (2014) reports on the result of a large-scale field experiment in Belgium which sent identical, fictitious applications originating from young male graduates with or without a disability to 768 vacancies in the Flemish labor market. The applications with a disability alternated between mentioning an entitlement to a wage subsidy program, which covered between 20-40% of the total wage cost, or not. The call-back rates showed that the applications from disabled individuals had a substantially lower call-back rate (47%) but no significant differences between those who mentioned the wage subsidy program and those who did not. Thus, the subsidy program did not have the intended effect at least where employment of young disabled individuals is concerned. This finding need not necessarily apply to all disabled workers since firms may need to be given larger incentives to hire young disabled workers who have a longer expected career horizon.

There is clearly a need within this sparse literature for more evidence from different settings on the welfare economic consequences of wage subsidy schemes designed to raise the employability of disabled individuals, as well as a need to evaluate the Danish Flexjob scheme, which is a large-scale wage subsidy scheme for disabled workers. By exploiting variation arising from the reform of the reimbursement structure, we provide quasi-experimental causal evidence of its effect on the hiring response of affected firms compared to unaffected firms. Within our set-up, we are able to address the issue of

whether wage subsidy schemes for disabled workers lower the welfare losses of non-employed disabled individuals or whether they are used by firms to hire their own former employees. The assumption being made here is that hiring from outside does more to reduce welfare loss of non-work than hiring from within, i.e., former employees. This assertion is likely to hold because the non-employed disabled have a lower probability of finding employment compared to the group of former employees of the firm; thus, as a group they suffer a greater utility loss of being out of work. While we control for the degree of unemployment in the year the individual has been looking for work, Kroft et al. (2013) show that individual labor market history also matters, i.e., the longer an individual has been out of work, the more difficult it is to find a new job.

If, on the other hand, tastes for work are lower for the non-employed, then the utility loss of former employees of being out of work could be higher. Since we control for various measures of health and health care usage in the analysis, differences in tastes for work cannot arise due to differences in, e.g., the severity of the disability across the two groups. We maintain therefore that welfare-loss reduction is greater if the disabled non-employed are hired via the wage subsidy scheme.

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

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 Labor Market Authority 2010).

Under the Flexjob scheme, jobs are both subsidized and associated with special working conditions, e.g., reduced working hours and/or the absence of physically demanding or stressful tasks. 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[3]. 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 job[6]. 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 Labor Market Authority 2010).

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 (1.77% of GDP) on all active labor market programs (ALMPs) in 2012 (Ministry of Finance 2012, Statistics Denmark 2012, Ministry of Employment 2012). Thus over a quarter of the ALM expenditures were on wage subsidies. In 2012, almost 85,000 individuals were found eligible for a Flexjob and underwent formal visitation, far exceeding the initially estimated 23,000 visitations in 2004. The Flexjob program has until recently been in its

growth phase (see Additional file 1: Figure S1), and in 2012, it covered around 60,000 disabled persons annually. Job creation, however, has not been able to keep up with this flow, with only 381 Flexjobs created in net in 2012 compared to 2011, so currently wait unemployment[7] is almost 30%.

3.1 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.

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

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

After the reform in May 2002, the subsidy to governmental units was

$$\begin{aligned} &= 0.67w_m, \quad if\ \ 0.32 < WC\ \ Loss < 0.50 \\ &= 0.75w_m, \quad if\ \ 0.49 < WC\ \ Loss < 0.67 \\ &= 0.83w_m, \quad if\ \ WC\ \ Loss > 0.66, \end{aligned}$$

while to other firms, it remained as before (0.33 w_m, 0.5 w_m and 0.67 w_m respectively). After July 2002, both groups of firms lost subsidization at the lowest level of working capacity loss, i.e., 0.32 < WC Loss < 0.50[8].

Theory predicts that when wages are subsidized, employment should increase. A simple illustration of the partial equilibrium effects of an employer-side subsidy for disabled workers can be shown as in Katz (1998) by an outward shift of the demand for labor curve, holding constant a perfectly elastic or upward sloping supply curve. According to the Danish wage bargaining model, public sector wage determination is negotiated between employer and employee representatives in the collective bargaining rounds. Thus, we expect that wages do not adjust much, and the greatest adjustment will take place with respect to hiring (firms are wage-takers). Furthermore, it is probably the case that the productivity of many of disabled is lower than the wage floor agreed at the sectorial level between social partners. In that case, also, employment is set by labor demand. Even if the supply of disabled labor is not perfectly elastic, an increase in the subsidy means that the demand curve for labor shifts out, and employment increases.

Assume that the available pool of hires for Flexjob positions is made up of former employees who have acquired a disability and of the disabled who are recruited from

outside the firm including the former employees from other firms and the non-employed, i.e., unemployed or individuals out of the labor force. Without being able to observe the productivity of the disabled accurately, the employer expects that former employees are more productive than individuals applying from outside the firm and fills up positions using the former group first. Now, if the subsidy is reduced, the firm will hire fewer workers in all but will still hire (those expected to be) the most productive first, meaning that the composition of hired disabled workers will change: Fewer will be hired externally, while more will be hired internally. In particular, the hiring of the non-employed will decrease because the productivity of this group is even more unknown than the productivity of employees from other firms[9].

We are able to test this prediction by combining a reform that exogenously changed the subsidy rate for disabled workers with unique information on whether a new Flexjob hire was previously employed in a regular job at the same firm or originated from outside the firm. Controlling for health and other worker background characteristics, we can treat the latter as a proxy measure for the potential of wage subsidy programs to reduce welfare loss, i.e., by promoting hiring from outside 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, for example, in terms of 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. 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 comparison groups, 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 publicly employed in either the municipal/regional or the federal governmental sector. These 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. We leave out private sector employees because this sector is affected by the business cycle to a much larger extent than the two public sectors. That is, we assume that the behavior of the municipal/regional sector is more similar to that of government institutions even though these sectors perform different public tasks. While the municipal/regional sector is responsible for decentralized tasks that vary

across the country, such as health and social services, employment, institutions for childcare and eldercare, and elementary school, federal governmental institutions are in charge of centralized tasks such as the judicial system, the police and the defence and institutions that take care of, e.g., tax collection, youth education and national environmental issues. Still, we expect and show that employment and other variables develop similarly over time in these two sectors.

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.

Because 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 employees[10] and empirical evidence on the development of the composition of Flexjob hires over time as indirectly providing evidence of the welfare loss reducing effect of the scheme in terms of drawing non-employed disabled individuals into the labor market. Our outcome variables, which we use to examine the reduction of the welfare loss of non-employment, are, relative to all new Flexjob hirees, 1) whether the new Flexjob employee is hired from non-employment (outsider) and 2) whether the new Flexjob employee is a retained employee (insider). In between these two groups is the (relatively small) group of new Flexjob employees hired from employment in other firms, which we do not examine in detail.

Based on this empirical setup, however, we are not able to completely quantify the general (labor market) equilibrium effects of the Flexjob scheme, such as any potential displacement of non-subsidized workers. Furthermore, if governmental units compete with private sector firms, and if they have a cost advantage with the subsidy, this could reduce employment among less-subsidized firms. If such general equilibrium effects exist, then a decrease in the subsidy rate for governmental units leaving unchanged the subsidy rate of other firms should reduce the number of employees within these units compared to in the other firms. Within the scope of this paper, we are not able to analyse local markets where the presence of governmental units varies to test for such effects.

5 The model

We exploit exogenous variation arising from the change in reimbursement rules applicable to government institutions from May 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 comparison group consists of employees in the municipal and regional sector.

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, first, the share of new Flexjobs employees hired from non-employment and, second, the share of new Flexjob employees who are retained employees between the treatment and comparison groups 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-incidence of the 2001/2002 hiring freeze with the reform effect, we omit information for 2002 in our empirical analysis[11]. 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 universal and not sector-dependent. Further, the share of Flexjob-grantees which were entitled to this subsidy before the reform seems to be negligible. Hence, we do not expect it to coincide with our findings (see in addition endnote 8).

The third assumption is that secular trends in the labor market are common to individuals in the treatment and the comparison group respectively. By exploiting the longitudinal structure of the available data, we can justify the assumption by first looking at pre-market trends for a number of labor market and health measures in 1997–2001 in the two sectors, see Figures 2, 3, 4, 5, and 6. It appears that these trends are very

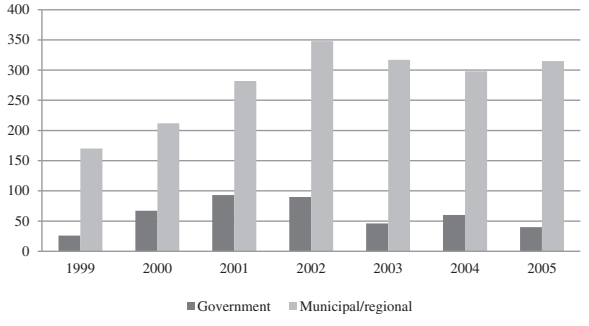


Figure 1 Number of new FJ hires by sector, 1999–2005.

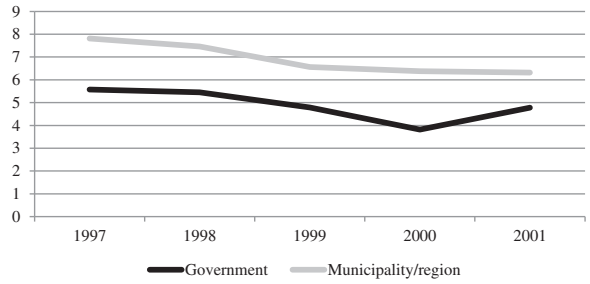


Figure 2 Employees recruited from non-employment relative to all employees by sector, percent, 1997–2001.

similar in the governmental and the municipal/regional sector before the reform. Second, we compare employees in the different sectors for each of our outcome measures in the pre-reform period from 1999 to 2001, that is, the share recruited from non-employment relative to all new Flexjob employees (Figure 7a) and the share of retained Flexjob employees relative to all new Flexjob employees (Figure 7b) in the governmental, the municipal/regional and the private sector, respectively, in the pre- and post-periods. These figures show plausibly similar trends among individuals employed in the two public sectors before the reform, which is supported by the placebo test conducted year-by-year in the analysis period, see below. The private sector, however, does not share the same pre-trend, which is why we do not include it in comparison group.

5.1 DD treatment equation – retained flexjob employees

We estimate the following difference-in-differences equations on a sample of newly granted Flexjob holders where the outcome is (1) the likelihood of being a Flexjob employee hired from non-employment and (2) the likelihood of being a retained Flexjob hire for the years 1999–2005 (excluding 2002):

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

$$FJ_RET_{it} = \alpha^R + \beta_1^R POST_t + \beta_2^R GOVT_{it} + \tau^R POST_t * GOVT_{it} + \theta^R X_{it} + \nu_{it} \quad (2)$$

where $FJ_NON = 1$ if the new Flexjob employee i in year t was either unemployed or out of the labor market in t-1, and $FJ_RET = 1$ if the new Flexjob employee i in year t was employed in the same firm as he or she worked in a regular (unsubsidized) job

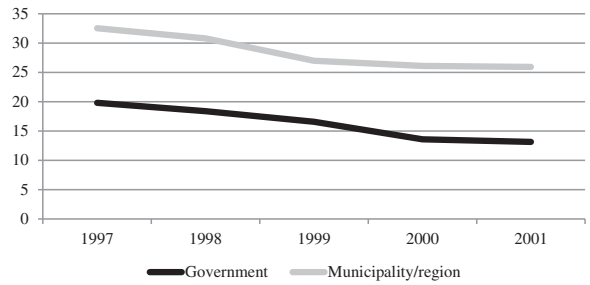


Figure 3 Degree of unemployment during the year among all employees by sector, 1997–2001.

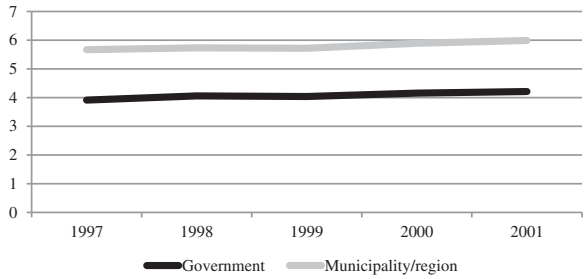


Figure 4 Number of doctor visits among all employees by sector, average, 1997–2001.

in t-1 (the balance being those who were hired into Flexjobs from within the labour market but from a different firm). FJ_NON (FJ_RET) = 0 if i is granted a new Flexjob but was not unemployed or out of the labor market (working in that firm) in year t-1, $GOVT$ is an indicator for belonging to the treatment group for individual i in year t, i.e., a government sector employee, $POST$ indicates 2003 and later, X is a set of characteristics controlling for compositional changes in treatment and comparison 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 outcomes are measured as 0/1 variables, both linear probability and probit models are estimated. In principle, individuals could be observed multiple times in the data if they fulfill the conditions of not being observed either in a Flexjob from out of non-employment or working in the Flexjob-granting firm the year before 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. In principle, we could estimate the ratio of (1) and (2) only since the balance group is relatively small, but that would not give information on whether the numerator or the denominator or both changed.

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 to correct for the overrepresentation of immigrants and their

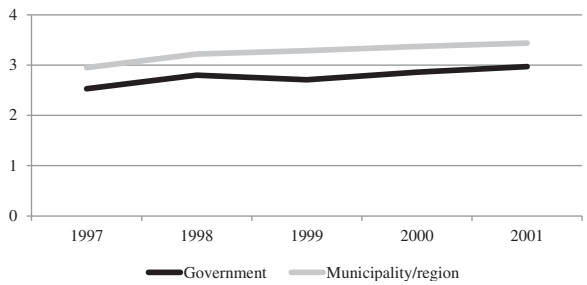


Figure 5 Employees diagnosed with diseases in bones, muscles and connective tissue, by sector, percent 1997–2001.

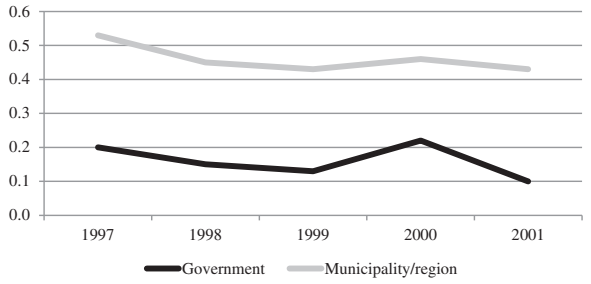


Figure 6 Employees that receive disability benefits the following year, by sector, percent 1997–2001.

descendants in the sample. The dataset used contains information for the period 1996 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

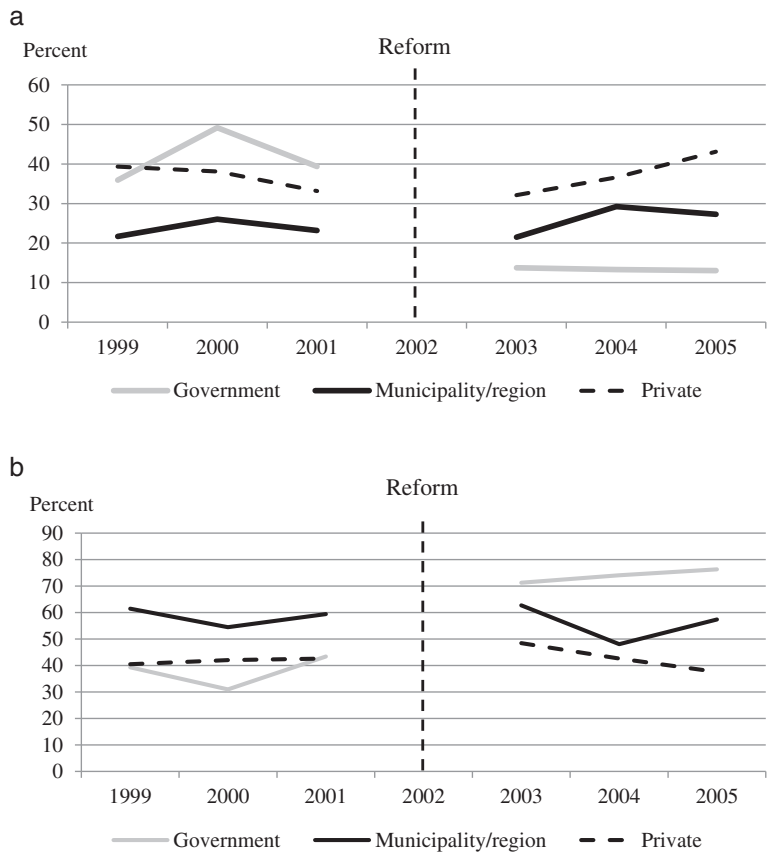


Figure 7 a. FJ recruited from non-employment relative to all new FJ employees by sector, 1999–2001 and 2003–2005. **b**. Retained FJ relative to all new FJ employees by sector, 1999–2001 and 2003–2005.

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 comparison group consisting of individuals employed in the municipal/regional sector.

In Figure 1, we show descriptive evidence on the number of new Flexjob hires in the comparison 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 comparison group[12]. This descriptive evidence suggests that lowering reimbursement to governmental units reduced their incentives to hire Flexjob workers as intended (and as theory would predict).

In terms of the effect of the reform on the composition of Flexjob hires, Figure 8 shows descriptive evidence that while the composition remains the same in the municipal/regional and private sectors, the share of non-employed employees (outsiders) falls and the share of retained employees (insiders) increases in the government sector after the reform. Note that Figure 8 shows that in the Danish context, a much higher share of subsidized workers come from non-employment and a much lower share from other firms compared to the Austrian private sector evidence in Lalive et al. (2013), presumably because of a greater pressure from the government for firms to hire out of non-employment.

Finally, Figure 9 shows descriptive evidence that even though the total number of Flexjob hires decreases from the before (1999–2001) to the after (2003–2005) period, the number of retained Flexjob hires actually increases, while the number of Flexjob employees hired from non-employment significantly decreases. By contrast, an increase in the total number of Flexjob hires in the municipal/regional and private sectors can be seen both among retained Flexjob hires and Flexjob employees hired from non-employment. In other words, according to this descriptive evidence, the compositional change in the treatment group from outsiders to insiders appears both in relative and absolute terms. We turn next to econometric analysis of the effect of the reform on the composition of Flexjob hires.

6.1 Sample and variables

Our sample consists of all new Flexjob hires in each of the years 1999–2001 and 2003–2005. For outcomes, we employ binary indicators of whether the newly hired Flexjob employee comes from a) non-employment or not, or b) is a retained employee or not, i.e., employed in the same firm before being granted a Flexjob, respectively.

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, educational level, 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 share with a hospital diagnosis of any type of musculoskeletal disease, which is the most prevalent diagnosis among Flexjob employees. We take all information from the administrative registers. Note that we measure



Figure 8 Composition in relative terms of new FJ employees separately by sector, 1999–2001 and 2003–2005. **a)** Government **b)** Municipality/region **c)** Private.

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.

6.2 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 comparison

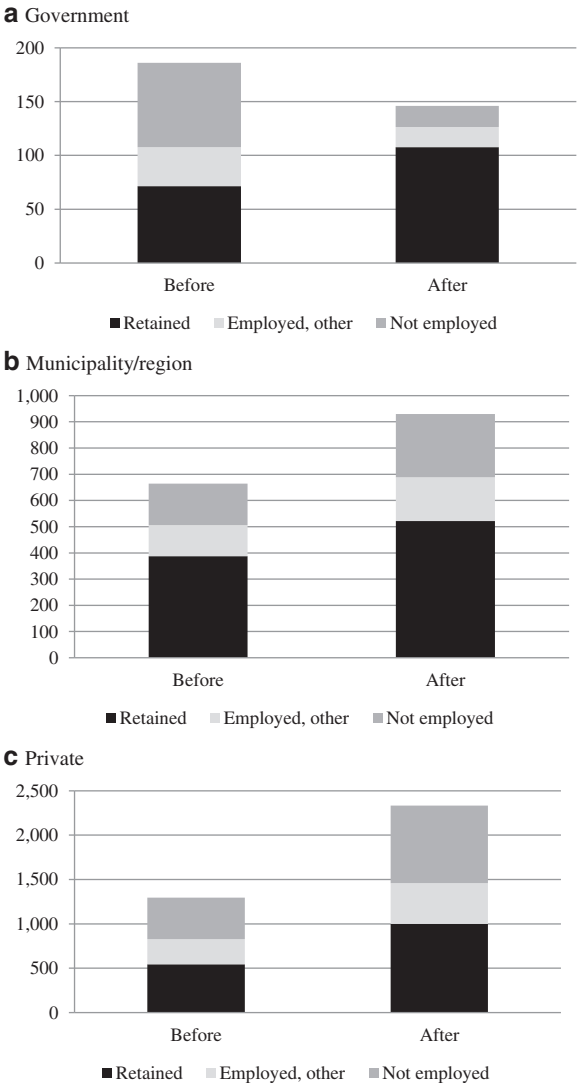


Figure 9 Composition in absolute terms of new FJ employees separately by sector before and after reform. **a)** Government **b)** Municipality/region **c)** Private.

groups. Thus, age develops similarly pre- and post-reform for both groups, as do educational level, degree of unemployment, sickness benefits, annual doctor visits, and share with a hospital diagnosis of any type of musculoskeletal disease. 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 comparison firms tend for the most part to have a similar development over time in most characteristics between 'pre' and 'post' periods. The only notable difference is in the share of males. Treated firms have a significantly higher share of male Flexjob hirees than comparison firms (municipal and regional sector) in the post period. Therefore, it is necessary to control for this variable in the analysis. Below, we test for composition changes between pre-reform and post-reform periods using each of the explanatory variables one by one as the dependent variable.

Table 1 Summary statistics for all new Flexjob hires, treatment and comparison group, pre- and post- reform

	Treatment group		Comparison group	
	Pre-reform	Post-reform	Pre-reform	Post-reform
Share Flexjob recruited from non-employment (relative to all new Flexjob hires)	0.422[b]	0.134[a]	0.238	0.260
Share retained Flexjob hires (relative to all new Flexjob 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	47.76	46.15	48.65
	(0.740)	(0.934)	(0.398)	(0.329)
Male	0.415	0.470[a]	0.337	0.229
Vocational training	0.334	0.367	0.336	0.361
Higher education	0.252	0.359	0.196	0.347
Degree of unemployment, year t-1 (*100)	6497	2732	5791	2694
	(1470)	(1133)	(728)	(382)
Annual number of days with sick benefits (*100)	5667	7493	5485	8067
	(761)	(926)	(380)	(359)
Annual number of doctor visits	9.989	12.79	10.32	12.09
	(1.62)	(1.11)	(0.424)	(0.362)
Diagnosed with diseases in bones, muscles and connective tissue	0.099	0.166	0.094	0.132
Number of observations (minimum)[c)]	185	146	664	927

Notes: [a)]Significant difference between treatment and comparison group in the post-reform period, $p < 0.1$.
[b)]Significant difference between treatment and comparison group in the pre-reform period, $p < 0.1$.
[c)]For the variable "Share retained Flexjob hires (relative to all new Flexjob hires)", we have a total of 1,922 observations. For the remaining variables, we have a total of 1,926 observations.

7 Results

7.1 Main results

The effect on the proportion of new Flexjob employees hired from a) non-employment and b) retained employees before and after the reform appears in Table 2 and 3. For the first outcome, in the comparison group, the proportion of previously non-employed Flexjob employees is at the same level before and after, namely 24–26 percent. In the treatment group, the proportion before the reform is 42% and 13% after the reform.

Table 2 Reform effect on Flexjob recruitment from non-employment

	LPM		Probit (marginal effects)	
	Without covariates	With covariates	Without covariates	With covariates
Post	0.022	0.102***	0.022	0.087***
	(0.025)	(0.026)	(0.025)	(0.028)
Governmental sector	0.184***	0.187***	0.179***	0.180***
	0.047	(0.046)	(0.046)	(0.044)
Post* governmental sector	−0.310***	−0.321***	−0.219***	−0.219***
	(0.059)	(0.057)	(0.027)	(0.026)
R^2	0.020	0.133		
Number of observations	1,922	1,922	1,922	1,922

Inference: ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.
LPM and Probit analyses. Robust standard errors in parentheses.
Governmental sector compared to municipal/regional sector, difference-in-differences.

Table 3 Reform effect on Retained Flexjob

	LPM		Probit (marginal effects)	
	Without covariates	With covariates	Without covariates	With covariates
Post	−0.022	−0.092***	−0.022	−0.099***
	(0.029)	(0.029)	(0.029)	(0.028)
Governmental sector	−0.199***	−0.198***	−0.194***	−0.192***
	0.048	(0.047)	(0.046)	(0.044)
Post* governmental sector	0.376***	0.389***	0.319***	0.324***
	(0.066)	(0.066)	(0.042)	(0.040)
R^2	0.022	0.097		
Number of observations	1,926	1,926	1,926	1,926

Inference: ***p < 0.01; **p < 0.05; *p < 0.
LPM and Probit analyses. Robust standard errors in parentheses.
Governmental sector compared to municipal/regional sector, Difference-in-differences.

This implies a total difference in the before and after differences of −31 percentage points, which is highly significant, see Table 2.

Along the same lines, in terms of the second outcome, the proportion of previously retained Flexjob employees is at the same level in the comparison group before and after, namely 56–58 percent, while 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 also is highly significant, see Table 3.

These results show that lowering the reimbursement decreases the proportion of employees hired from non-employment (outsiders) and increases the proportion of retained employees (insiders) among the newly granted Flexjob employees. Also after adding covariates, this effect of the reform remains significant, and the difference-in-differences coefficient remains at −32 and 39 percentage points respectively[13]. The impact is very large relative to the proportion of non-employed and, especially, retained Flexjob employees before the reform, namely, 42% and 38%, see Table 1. That is, our results show that in the governmental sector, the proportion of non-employed Flexjob employees is only about 3/4ths as large and the proportion of retained Flexjob employees, about twice as large as it would have been in absence of the reform. Similar results are obtained in the probit specifications. As the coefficients are rather substantial, we first subject the estimates to a series of robustness checks. Next, we test for compositional changes between pre-reform and post-reform periods and search for heterogeneity in the reform effect.

7.2 Robustness checks

First, as a placebo test, we examine the impact of the reform year-by-year for each of these outcome measures, see Figures 10a and b. Second, we present a number of other robustness checks in Tables 4 and 5.

The year-by-year impact of the reform on the proportion of non-employed (retained) Flexjob employees is significantly negative (positive) in each of the years from 2003 and on (see Figures 10a and b). The impact decreases (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 Tables 4 and 5, we first test for the presence of any anticipation effects by omitting Flexjob employees employed in the period January 1st, 2001 to December 31st, 2001 from the analysis and find that the omission makes the effect even slightly larger, but not significantly so.

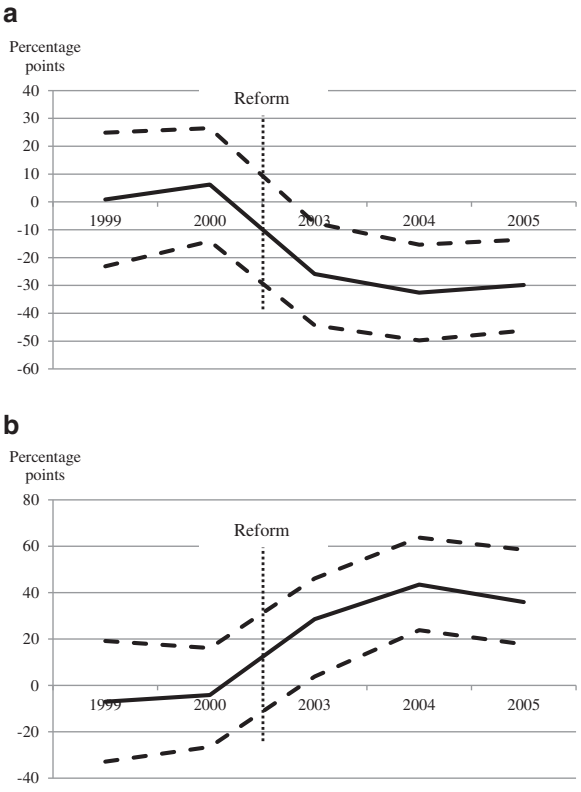


Figure 10 a. Reform effect on FJ recruitment from non-employment year-by-year. Governmental sector compared to municipal/regional sector. **b**. Reform effect on retained FJ year-by-year. Governmental sector compared to municipal/regional sector. Note: LPM estimates. Difference-in-differences model. 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, education, lagged degree of unemployment, annual number of sick days, annual number of visits to a doctor and diagnosed with disease in bones, muscles and connective tissue. The period before the reform (2001) is set equal to zero.

Table 4 Main results and robustness checks on reform effect on Flexjob recruitment from non-employment

	Flexjob recruitment from non-employment	
	Without covariates	**With covariates**
Main result	−0.310***	−0.321***
	(0.059)	(0.057)
	[1,922]	[1,922]
Robustness checks		
Anticipation effect: Year 2001 excluded	−0.334***	−0.348***
	(0.075)	(0.072)
	[1,547]	[1,547]
Sickness benefits omitted	-	−0.315***
		(0.057)
		[1,922]

Inference: ***p < 0.01; **p < 0.05; *p < 0.1.
LPM/OLS estimates. Robust standard errors in parentheses. Number of observations in brackets.
Difference-in-differences analyses.

Table 5 Main results and robustness checks on reform effect on retained Flexjob employees

	Retained Flexjob employees	
	Without covariates	**With covariates**
Main result	0.376***	0.389***
	(0.066)	(0.066)
	[1,926]	[1,926]
Robustness checks		
Anticipation effect: Year 2001 excluded	0.416***	0.426***
	(0.079)	(0.078)
	[1,551]	[1,551]
Sickness benefits omitted	-	0.387***
		(0.065)
		[1,926]
Retained Flexjob employees equal to 1 only if new Flexjob employees' unemployment rate in year t is zero	0.358***	0.369***
	(0.066)	(0.064)
	[1,926]	[1,926]

Inference: ***p < 0.01; **p < 0.05; *p < 0.1.
LPM/OLS estimates. Robust standard errors in parentheses. Number of observations in brackets.
Difference-in-differences analyses.

Because the data for duration on sickness absence is poorly measured before 2000, especially in the public sector, in the next rows of Tables 4 and 5, we check whether the removal of sickness benefits as a control variable changes a estimated treatment effect. It does not.

We carry out an extra robustness test in Table 5 where 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, became disabled and subsequently experienced a spell of unemployment as a result of their sickness/disability and then re-joined the same firm. Redefining these individuals as non-retained does not change the results.

7.3 Compositional changes and heterogeneous treatment effects

To examine whether our treatment group and comparison group are comparable, we present the findings of tests of potential compositional changes between pre-reform and post-reform separately for our sample in Table 6. The dependent variables are each of our explanatory variables, included one by one. For only one variable, gender, does the composition of the sample change from the pre- to the post-reform period. This reflects that the share of men among the Flexjob hires increases for the treatment group in this period, while it decreases for the comparison group. Since gender does not affect our main estimates significantly, see Additional file 1: Table S1 and Table S2, it seems reasonable to believe that this compositional change does not affect our main results. Below, we examine whether our results apply to men and women separately. For the remaining variables, there are no significant compositional changes in our sample before and after the reform.

In Tables 7 and 8, we search for heterogeneity in the reform effect on our outcomes (conditional on being a new Flexjob hire) with and without covariates. In general, we

Table 6 Test for compositional changes between pre-reform and post-reform periods, all new Flexjob hires

	All new Flexjob hires	
	Without covariates	**With covariates**
Dependent variable		
Non-native Dane	−0.010	−0.012
	(0.012)	(0.012)
	[1,926]	[1,926]
Age	0.080	−0.038
	(1.296)	(1.294)
	[1,926]	[1,926]
Male	0.162**	0.166**
	(0.070)	(0.070)
	[1,926]	[1,926]
Vocational training	0.008	−0.021
	(0.068)	(0.059)
	[1,926]	[1,926]
Higher education	−0.045	−0.042
	(0.065)	(0.057)
	[1,926]	[1,926]
Degree of unemployment	−668.8	−1153
	(2031)	(2024)
	[1,926]	[1,926]
Sick days	−755.0	−795.6
	(1308)	(1285)
	[1,926]	[1,926]
No. of visits to doctor	1.025	1.480
	(2.040)	(2.011)
	[1,926]	[1,926]
Diagnosed with diseases in bones, muscles and connective tissue	0.030	0.033
	(0.048)	(0.048)
	[1,926]	[1,926]

Inference: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.
Governmental sector compared to municipal/regional sector. Difference-in-differences analyses. OLS estimates. Robust standard errors in parentheses. Number of observations in brackets.

find a similar pattern within almost all subgroups, including across men and women. Nor is there a significant difference in terms of firm size. 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 employees internally. On the other hand, large firms also face higher monitoring costs, which should lead them to intensify their search efforts and search more widely (see, e.g., Welters and Muysken 2006). However, the results are the same when splitting by firm size, meaning that economies of scale in assessments and monitoring effects roughly balance out. The strongest result appears for the low-educated (less than vocational level) in each case, suggesting that the composition of Flexjob employees from this group shifts more towards internal hiring

Table 7 Heterogeneity in the reform effect on Flexjob recruitment from non-employment among subpopulations

	Flexjob recruitment from non-employment	
	Without covariates	**With covariates**
Main result	−0.310***	−0.321***
	(0.059)	(0.057)
	[1,922]	[1,922]
Women	−0.373***	−0.421***
	(0.079)	(0.074)
	[1,302]	[1,302]
Men	−0.269***	−0.249***
	(0.089)	(0.087)
	[620]	[620]
Aged < 45	−0.301***	−0.268***
	(0.107)	(0.099)
	[664]	[664]
Aged > 44	−0.335***	−0.335***
	(0.070)	(0.069)
	[1,258]	[1,258]
Low educated	−0.540***	−0.534***
	(0.104)	(0.098)
	[728]	[728]
Highly educated	−0.189***	−0.201***
	(0.069)	(0.069)
	[1,194]	[1,194]
Workplace with < 50 employees	−0.212	−0.204
	(0.160)	(0.157)
	[535]	[535]
Workplace with > 49 employees	−0.250***	−0.253***
	(0.070)	(0.076)
	[1,093]	[1,093]

Inference: *** p < 0.01; ** p < 0.05; * p < 0.1.
LPM estimates. Robust standard errors in parentheses. Number of observations in brackets.
Difference-in-differences analyses.

after the fall in the subsidization rate than the composition in other groups. This result matches earlier findings in the literature that suggest that firms intensify their assessment efforts to find productive workers, in particular when the job task to be performed is complex (Welters and Muysken 2006, Barron et al. 1997, Barron et al. 1987). That is, for the group of highly educated workers, the rise in internal hiring is lower because firms still have a financial incentive to search more widely to find productive workers.

8 Discussion

Could one interpretation of our findings be that when the subsidy is lowered, the greater internal hiring that results is a case of deadweight loss, i.e., hiring of employees who would have been retained otherwise? This could be particularly relevant

Table 8 Heterogeneity in the reform effect on retained Flexjob employees among subpopulations

	Retained Flexjob employees	
	Without covariates	**With covariates**
Main results	0.376***	0.389***
	(0.066)	(0.066)
	[1,926]	[1,926]
Women	0.365***	0.403***
	(0.087)	(0.085)
	[1,304]	[1,304]
Men	0.452***	0.441***
	(0.105)	(0.103)
	[622]	[622]
Aged < 45	0.419***	0.405***
	(0.115)	(0.114)
	[666]	[666]
Aged > 44	0.375***	0.381***
	(0.081)	(0.079)
	[1,260]	[1,260]
Low educated	0.539***	0.534***
	(0.114)	(0.112)
	[729]	[729]
Highly educated	0.295***	0.313***
	(0.083)	(0.083)
	[1,197]	[1,197]
Workplace with < 50 employees	0.223	0.261
	(0.173)	(0.163)
	[536]	[536]
Workplace with > 49 employees	0.257***	0.264***
	(0.077)	(0.075)
	[1,096]	[1,096]

Inference: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.
LPM estimates. Robust standard errors in parentheses. Number of observations in brackets.
Difference-in-differences analyses

for disabled workers since a worker's real degree of disability is difficult to measure so that eligibility can be more easily manipulated. According to the National Labor Market Authority (2010), half of those directly referred to a Flexjob (i.e., without a period of wait unemployment 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. 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. However, deadweight loss ought to decrease and not increase when the subsidy is lowered. Furthermore, some retained

workers might have never been retained absent the program, and some new hires might have occurred absent the program. If the ratio of the two increases following a decrease in the subsidy rate, this most likely indicates, as mentioned earlier, that employers just want to save on search costs and give priority to workers already employed by renewing their contracts.

An important caveat is that government institutions are fully subsidized before the reform, which is an unlikely scenario compared to the general design of employment subsidies. We must assume then that firm behavior is linear in the ratio of subsidized wages. Alternatively, our finding may be interpreted as saying that anything less than a full subsidy will result in more internal hiring. This can only be confirmed by analyzing future reforms that lower subsidy levels even further. To return to the question we started with, it does appear that, conversely, increasing wage subsidies for the disabled may increase employment, reduce internal hiring and increase hiring out of non-employment and thus reduce the welfare losses of non-employment, but this may require full subsidization of wages of disabled workers.

9 Conclusions

This paper evaluates the welfare 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 units. We investigate empirically whether the changes in the reimbursement amount affected the number and the share of all new Flexjob hires that are, respectively, hired out of non-employment vs. the number and share that are retained employees in governmental units compared to municipal/regional units. Descriptive evidence suggests that after the reform lowering the subsidy rate, governmental units were much less inclined to take in new hires as theory would suggest. Based on descriptive evidence and our empirical analysis, we find a substantial substitution between "insiders" and "outsiders" in both absolute and relative terms, 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 from non-employment and retaining existing employees.

After the reform, governmental units created fewer Flexjobs, while the comparison 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 such that the share of new Flexjobs allocated to non-employed (retained) employees is three-quarters (twice) as large after the reform than it would have been in absence of the reform. At the same time, the absolute number of the non-employed (retained) decreased (increased). This compositional change favoring insiders arises even though firms in the Danish welfare state are presumably more committed to hiring non-employed workers, so we may expect an even larger impact in economies with less active welfare states.

A mechanism explaining our 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 economic incentive to intensify assessment efforts and search more widely. Furthermore, because of their credentials, the productivity of highly educated workers is arguably less likely to be unknown to firms. Thus, in the absence of full wage subsidization, in order to be able to reduce the welfare losses of non-employment and decrease internal hiring of disabled workers for wage subsidized positions – in particular those with low education – there must be a way for firms to screen new hires for such positions coming from unemployment or out of the labor market without incurring costs. Public job search assistance programs could aid employers by incorporating job auditions or job tryouts and by providing certification of job readiness in the case of disabled job seekers, particularly those with low education.

Our paper raises the important issue of how employment policies can change the composition of who is hired, e.g., insiders versus outsiders, an issue that has not been addressed much in existing literature. This substitution to insiders from outsiders following a decrease in the subsidy rate would appear to dampen welfare-loss reduction. We were not able to make causal statements on the effect of the subsidy change on total employment creation, although inspection of the data pointed to a decline in external hiring in absolute levels as well. A full assessment of the policy's effects would require modeling the effects on total employment in addition.

Endnotes

[1]A meta-analysis of microeconometric evaluations of active labor market policies showed that subsidized public sector employment programs are less likely to yield positive impacts compared to job search assistance, classroom and on-the-job training (Card et al. 2010).

[2]A similar program was the New Deal for the Disabled (Britain), a voluntary program for the disabled that offered job-seeking assistance services through a national organization of Job Brokers. The take-up rate, however, was low, covering only 1.9% of the eligible population (Pires et al. 2006). Since 2009, the New Deal was replaced by the Flexible New Deal and from 2011 by The Work Programme.

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

[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 July 1st, 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 Labor Market Authority 2010).

[6]This was true for the observation period in this study. From January 1st, 2013, a number of changes of the scheme were implemented. For example, 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. Further, the incentives for firms to employ disabled workers with a working capacity of less than 1/3 are increased.

[7]"Wait unemployment" means unemployment among individuals found eligible for a Flexjob.

[8]Findings in the report by DISCUS (2005) show that the use of the 1/3 subsidy was negligible; only 1.4% of all Flexjob employees were entitled to the 1/3 subsidy in 2003. Since only few Flexjob-grantees actually leave the scheme, we assume that the share of all Flexjob-grantees entitled to the 1/3 subsidy were also negligible in the pre-reform period analyzed here (1999 to 2001). Therefore, it seems reasonable to believe that the abandonment of the 1/3 subsidy does not affect the number and composition of Flexjob hires significantly.

[9]See, for instance, Eriksson and Lagerström (2006) for empirical evidence that firms prefer contacting and hiring employed candidates compared to observationally equivalent unemployed candidates.

[10]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).

[11]Results 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.

[12]We do not analyze these data within a difference-in-difference framework because the pre-reform trends are not parallel. Ignoring the parallel-trend assumption and estimating the DD anyway produces the same result – a 0.05 percentage point reduction in new Flexjob hires compared to all new hires in governmental units post-reform compared to comparison firms, both with and without controls, LPM or Probit (available on request).

[13]In Additional file 1: Table S1 and Table S2, we show the results for the full set of covariates.

Additional file

Additional file 1: Table S1. Reform effect on Flexjob recruitment from non-employment. Governmental sector compared to municipal/regional sector, Difference-in-differences. **Table S2**. Reform effect on Retained Flexjob employees. Governmental sector compared to municipal/regional sector, Difference-in-differences. **Figure S1.** Growth in the number of Flexjobs and wait unemployment, all sectors, 2000-2012. Source: Statistics Denmark (2015). Note: Yearly information about Flexjobs and wait unemployment is only available from year 2000 and on.

Competing interests
The IZA Journal of Labor Policy is committed to the IZA Guiding Principles of Research Integrity. The authors declare that they have observed these principles.

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Author details
[1]Department of Economics and Business, Aarhus University, Aarhus, Denmark and IZA, Bonn, Germany. [2]SFI-The Danish National Centre for Social Research, Copenhagen, Denmark. [3]SFI-The Danish National Centre for Social Research, Copenhagen, Denmark.

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