Job displacement effects of subsidized employment on municipal workplaces: Register-based evidence from Denmark

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Abstract

Aims: This study analyses the potential distorting effects that may occur at municipal workplaces that hire an unemployed individual with wage subsidy. The primary focus is job displacement effects on the regular employees at the workplaces. By distinguishing effects on the stock and flows of employees, different displacement effects can be identified. In addition, wage effects and effects on repeated use of subsidized employment are also analysed. **Method:** Difference-in-difference matching on a wide range of workplace characteristics and employment dynamics 13 months prior to the hire of a new subsidized employee. **Data:** Longitudinal monthly workplace data. **Conclusion:** It is found that the hiring of subsidized employment does not affect the number of regular employees nor firm expenditures on wages to regular employees. Yet, positive effects are found on both hiring and separations of regular workers and on increased renewal or repeated use of wage subsidies. The results indicate that subsidies employment appears to serve as a recruitment channel to try out new employees, and may cover up substitution that does not show in the number of ordinary employees.

1 Introduction

Among the OECD countries, Denmark is the one country that uses the most resources on active labour market policies, including activation, typically at workplaces or in the form of coaching and skill development (Rosholm & Svarer, 2011). The vast majority of the workplace-based activation takes place at public workplaces.

Whereas evidence that activation of unemployed individuals at private sector workplaces lead to regular employment has accumulated (e.g. Kluve 2010; Jespersen et al. 2008; Sianesi 2001; Rosholm & Svarer 2011), the existing evidence on activation at public sector workplaces is mixed (see e.g. Graversen (2012) and Rosholm & Svarer (2011)). A key concern surrounding the use of wage subsidies is that they displace ordinary employment (Calmfors 1994). Yet, very little is known about such unintended effects.

This study analyses the potential distorting effects that may occur at municipal workplaces that hire an unemployed individual with wage subsidy. The primary focus is job displacement effects on regular employees at the workplaces. According to Calmfors (1994), two major unintended negative employment effects at the workplace may appear as a consequence of hiring subsidised labor: If the workplace that hires an unemployed individual with wage subsidy would have hired an employee on regular terms without the subsidy[1], or it may be the case that the subsidized employee replaces other individuals employed on regular terms[2]. These effects are analysed separately by considering changes in stock as well as inflows and outflows of regular employees at the workplace level on a monthly basis 13 months following the hire of a subsidized employee. In addition, effects on repeated use of subsidized employment are analyzed. The job displacement effects are supplemented by estimates of wage effects for the regular

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[1] Within the literature on active labour market policies, this mechanism is sometimes referred to as the deadweight loss effect.

[2] This mechanism is often referred to as the substitution effect.

employees, which may capture internal dynamics at the workplaces in addition to those captured by the employment outcomes.

A priori there may be several reasons why it is difficult to identify distorting effects of hiring a subsidized employee. One reason is possible selection into the use of the wage subsidy scheme, if .e.g. workplaces which are cutting down are more likely to use the scheme. We seek to address this issue by using difference-in-difference matching (Heckman, Ichimura, Todd & Smith 1998) controlling for a wide range of workplace characteristics and employment dynamics 13 months prior to the hire of a new subsidized employee. The estimation of effects on annual differences is intended to eliminate unobserved differences between controls and participants which are constant over time.

In addition to the wide range of outcomes considered, providing different insights on potential displacement mechanisms, the study distinguishes itself from the existing literature by using longitudinal monthly workplace data. As adjustment of labour can be fairly short this is shown to be crucial, both because it allows us to estimate effects on workplaces 1-13 months after the hire of a new subsidised employee.

The paper proceeds as follows. Section 1.1 briefly discusses some existing empirical studies on job displacement effects of subsidised employment. Section 2 provides some background information on the legislative framework for employing unemployed workers with a wage subsidy at municipal workplaces. Section 3 describes the data used in the empirical analysis. Section 4 accounts for the econometric method. The results are reported in section 5 and discussed in section 6. Section 7 concludes.

1.1 Some previous studies on job displacement effects of subsidised employment

The empirical literature seeking to identify job displacement effects of subsidised employment is sparse and characterised by scarcity of data on the subsidised firms and corresponding methodological challenges.

One line of research is based on employer surveys, which turn out to give rise to quite heterogeneous findings. For example, Bishop & Montgomery (1993) find that in the absence of the US-based Targeted Job Tax Credit program, 70% of the surveyed employers state that they would have hired an employee on regular terms. A survey study

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of the Danish wage subsidy scheme at both private and public workplaces in 2004 (National Labour Market Authority, 2005) finds that 17% of the surveyed workplaces would have hired an employee on regular terms in the absence of the subsidy, and 7% state the they have replaced regular employees by subsidised employees. However, it may be questioned whether employer surveys provide reliable estimates of displacement effects, partly due to the risk of strategic response bias and partly because the number of regular employees realised if the firm had not hired a subsidised employee (the counterfactual outcome) can be difficult to assess for managers. Moreover, employers may be reluctant to reveal practices which are not permitted by law.

Another line of research have analysed the possible distorting effect of subsidised employment based on data from administrative registers. Dahlberg & Forslund (2005) analysed data aggregated on Swedish municipalities and found that two thirds of all (mainly public) subsidised employees crowd out regular employees. However, it may be argued to be associated with some uncertainty to make inference about causal relationships at the workplace level based on data aggregated at the municipal level.

To the best knowledge of the authors there are only three published studies which have analysed displacement effects of subsidised employment based on workplace level register data. For Finnish firms, Kangasharju (2007) finds that a subsidy increased the firm's payroll by more than the size of the subsidy during the period 1995-2002. Using the same methodology as the present study and data from 2006, Rotger & Arendt (2010) find that subsidised employment in small private firms in Denmark contributes to the employment of long-term unemployed individuals and others in existing jobs, but also in new jobs, which would not have been created in the absence of a wage subsidy. Hujer, Caliendo & Radic (2002) use West German data for firms subsidised in 1995. By using difference-in-difference matching, they find no effects on firm employment levels, and they attribute this finding to a potential substitution effect. However, to the best knowledge of the authors, no equivalent studies exist for public workplaces.

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2 Legislative framework for subsidised employment at municipal workplaces

The legislative framework for employing unemployed workers with a wage subsidy at municipal workplaces is described in the Act on an Active Employment Effort, chapter 12, §§ 51-68. As mentioned in the introduction, subsidised employment is offered to unemployed workers receiving unemployment benefits or social welfare. To be eligible, unemployed workers must either have been unemployed for at least 6 months, have no education beyond primary schooling, be beyond 50 years of age or be a sole provider for children. Subsidised employment is arranged by the job centre in collaboration with the unemployed worker and an employer and is intended to train and refresh the vocational, social or linguistic skills of the unemployed worker.

Working conditions are agreed between authorities and employers and are formalised in a contract. The maximum duration of a subsidised job is one year, and contracts of a shorter duration may be extended up to one year. Subsidised employees must continue to actively seek and accept offers of regular employment. Hence, the actual duration of a subsidised job can be shorter than initially planned if the subsidised worker finds a regular job during the activation period. Moreover, the employer may unilaterally terminate the agreement if the individual does not comply with the working conditions.

In 2010, public employers were given a subsidy of 130 DKK per hour when hiring unemployed workers receiving unemployment benefits or social welfare (National Labour Market Authority, 2010). The hourly pay for subsidised employment is determined according to the relevant collective agreement. However, the monthly salary for a subsidised job may not exceed the level of the transfer income that the unemployed individual would have received in case no subsidised job was available. Hence, the working hours in subsidised employment are usually less than full time.

Unemployed workers may not be employed at the workplace where they were most recently employed (either on ordinary terms or with a subsidy). Hereby it is prevented that unemployed workers are employed with a wage subsidy at the same workplace and perhaps in the same job that they were recently laid off from.

Moreover, the legislative framework includes two conditions which are intended to prevent subsidised employees from displacing ordinary employees and distort competition.[3]

The *employment contribution condition* stipulates that employment of subsidised worker must increase the level of employment at the workplace. The crucial point is whether employment increases given the current budget of the workplace. This means that employers which have recently laid off regular employees are allowed to hire new subsidised workers if their budget has been cut according to the lay-offs. It is up to the individual employer to document that a newly employed subsidised worker does in fact increase the level of employment.

While the *employment contribution condition* allows the substitution of an existing subsidised or otherwise non-regular employee by a new subsidised employee, employers cannot employ too many subsidised employees at the same time. This is regulated by the *reasonableness condition*, which stipulates that there should be a reasonable relationship between the number of subsidised and regular employees, respectively. More precisely, the number of subsidised employees may not exceed one per five regular employees for workplaces with up to 50 regular employees. Beyond this workplaces may employ one additional subsidized employee per 10 regular employees. The primary purpose of the *reasonableness condition* is to ensure that the subsidised workers are employed at regular workplaces and given the opportunity to interact with workers employed on regular terms.

It is a joint responsibility of the employers and their employees to ensure that the two conditions are met. Hence, the establishment of a new wage subsidy job must be discussed by the employer and a representative of the employees. Disagreement over whether the conditions are met is settled by the rules pertaining to labour legislation regarding mediation and may be settled by arbitration.

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[3] In Danish the *employment contribution condition* corresponds to 'Merbeskæftigelseskravet' and the *reasonableness condition* corresponds to 'Rimelighedskravet'.

Once a year, the ministry of employment determines minimum quotas for the number of subsidised employees in municipalities, regions and ministries based on the expected unemployment rate. Municipalities which do not meet their quotas may be subject to economic sanctions.

3 Data

The analyses are based on individual-level data from three administrative registers. Data on the monthly salary and terms of employment for all municipal employees are obtained from the Municipal and Regional Wage Data Office. Data on subsidised employment at municipal workplaces are available from the National Labour Market Authority. Finally, data from Statistics Denmark are used to describe the demographic and socioeconomic characteristics of the municipal employees, the sector of the workplace, and the local level of unemployment. Data from the three registers are merged on unique personal identification (CPR) numbers. Workplaces are identified using unique identifiers included in the Municipal and Regional Wage Data Office dataset.[4] Finally, the data are aggregated on workplaces and months, in order to create a panel data set where each observation pertains to a particular workplace in a given month.

3.1 Analysis sample

To ensure anonymity, municipal workplaces with less than three employees are dropped from the sample (required by Statistics Denmark). Moreover, we exclude some workplaces from the sample in order to get a more clean design. To be able to identify a the effect of hiring one subsidized worker the analysis sample is restricted to workplaces that hire none or one new subsidised employee in any given month. This is however not a severe restriction, as very few workplaces hire more than one new subsidised employee in a given month,

In order for it to make sense that one employee could potentially replace another one at the same workplace, workplaces categorised as administration and general public services are not included in the analysis, because they often include several different departments and individuals (such as janitors and gardeners) who are not employed at the same physical workplace, and who may arbitrarily be affiliated with the town hall (administration level) work place.

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[4] These identifiers (LOS code) are used for administrative purposes in the municipalities and are regarded as fairly reliable.

Sizewise, the analysis sample is restricted to workplaces with 5-40 regular employees measured at the beginning of the month. Preliminary analyses found that this delimitation maximizes the degree of comparability (balance) between workplaces that hire a new subsidized employee in a given month (participants) and those that do not (controls). We provide sensitivity analysis of how balance and effects changes when including larger work places.

Furthermore, we restrict the analysis to workplaces that have not used subsidised employment during the past 13 months. This is done in order to obtain a more clear research design. First of all, in not applied, firms acting as controls in one month may be treated in the month before. At the extreme, even if all subsidized workers displace ordinary workers, this would hard to detect because it occurs for both treated and controls at some point. Second, it is done to avoid sequential treatment effects, i.e. that a second hire of subsidized work is affected by the first hire. It implies that the analysis population is likely to be special in the sense that the included workplaces generally use wage subsidies to a lesser extent than the average municipal workplace. If substitution primarily occurs by extending contracts for subsidized employees, this delimitation thus entails a risk of underestimating the actual extent of displacement. We examine this indirectly by estimating whether use of subsidies begets more use.

Workplace identifiers have only been recorded recently[5]. Therefore, the analysis sample is restricted to workplaces that are observed during the entire period from January 2009 to February 2012. This allows us to measure effects 13 months ahead and control for employment dynamics 13 months back in time, and at the same time control for variation in employment over the course of a year (i.e. from March 2010 to February 2011). This is illustrated in Figure 3.1.

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[5] The recordings of the LOS code should have fair coverage rate and quality from 2009 (communication from the Municipal and Regional Wage Data Office..

Figure 3.1   Design of analysis
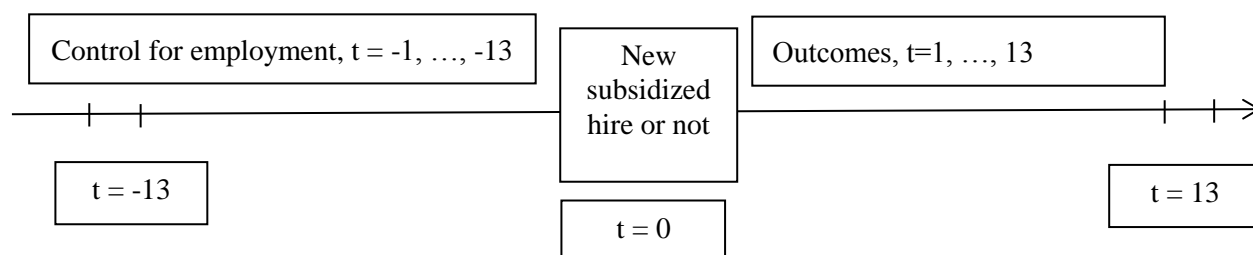
Workplaces may 'disappear' during the observed period for several reasons. In some cases there is a break in the workplace identifiers because the municipality changes data processing centre. In other cases workplaces are merged or closed. This may possibly cause some employment movements at the affected workplaces, which will interfere with our estimates, and hence avoided by the latter restriction.

3.2   Descriptive statistics

Table 3.1 presents descriptive statistics for the analysis sample of municipal workplaces observed from March 2010 to February 2011. Hence, an observation is a given workplace observed in a given month.[6] It is seen that the share of workplaces that hire a new subsidised employee is quite equally distributed across the 12 months examined except for July. The lower prevalence of subsidised hires in July is most likely explained by the fact that some municipal workplaces run with reduced staffing or close during the summer school holidays and thus do not hire.

Table 3.1   Distribution of workplaces on months

Month	Workplaces that <u>hire</u> a new subsidised employee (participants)		Workplaces that <u>do not hire</u> a new subsidised employee (controls)		Total
	Number	%	Number	%	Number
March 2010	60	1.52	3,876	98.48	3,936

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[6] The drop in the number of workplaces over time is due to the sample being restricted to workplaces that have not hired a subsidised employee during the past 13 months as mentioned in section 3.1.

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April 2010	58	1.49	3,843	98.51	3,901
May 2010	51	1.32	3,817	98.68	3,868
June 2010	54	1.40	3,806	98.60	3,860
July 2010	34	0.96	3,526	99.04	3,560
August 2010	55	1.45	3,743	98.55	3,798
September 2010	55	1.46	3,703	98.54	3,758
October 2010	70	1.88	3,657	98.12	3,727
November 2010	65	1.75	3,644	98.25	3,709
December 2010	48	1.31	3,619	98.69	3,667
January 2011	83	2.26	3,595	97.74	3,678
February 2011	61	1.68	3,568	98.32	3,629
Total	694	1.54	44,397	98.46	45,091

Likewise, it is seen from Table 3.2 that the use of subsidised employment is quite equally distributed across the municipal sectors in the analysis sample. However, workplaces delivering teaching or health services, nursing homes and residential institutions for mentally ill and/or addicts hire slightly more subsidised employees than the average, while the opposite applies to the remaining sectors.

Table 3.2   Distribution of workplaces on municipal sectors

Municipal sectors	Workplaces that hire a new subsidised employee (participants)		Workplaces that do not hire a new subsidised employee (controls)		Total
	Number	%	Number	%	Number
Teaching	103	1.64	6,160	98.36	6,263
Health services	37	1.67	2,175	98.33	2,212
Nursing homes	166	1.69	9,657	98.31	9,823
Residential inst., mentally ill/addicts	46	1.58	2,859	98.42	2,905
Residential inst., others	44	1.35	3,210	98.65	3,254
In-home help	97	1.52	6,280	98.48	6,377
Daycare for children	201	1.41	14,056	98.59	14,257
Total	694	1.54	44,397	98.46	45,091

Finally, Table 3.3 shows that larger workplaces are relatively more likely to hire a new subsidised employee. This does, however, not necessarily imply that the relative prevalence of subsidised employees is higher at larger workplaces.

Table 3.3   Distribution of workplaces on workplace size

Workplace size	Workplaces that <u>hire</u> a new subsidised employee (participants)		Workplaces that <u>do not hire</u> a new subsidised employee (controls)		Total
	Number	%	Number	%	Number
5-9 regular employees	174	1.17	14,678	98.83	14,852
10-14 regular employees	176	1.46	11,913	98.54	12,089
15-19 regular employees	140	1.89	7,253	98.11	7,393
20-24 regular employees	74	1.61	4,519	98.39	4,593
25-29 regular employees	59	2.00	2,885	98.00	2,944
30-34 regular employees	38	1.99	1,867	98.01	1,905
35-40 regular employees	33	2.51	1,282	97.49	1,315
Total	694	1.54	44,397	98.46	45,091

Descriptive statistics for the remaining covariates are not reported here for reasons of space, but can be found in the appendix.

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4   Method

4.1   Effect parameters

We estimate the average effect of hiring a new subsidized employee on the workplaces which have done this in a given month (average treatment effect on the treated, ATT) and frame the analysis within the potential outcome framework (see e.g. Rosenbaum & Rubin (1983)). As mentioned in section 3, we choose the month as the time unit, since lags in employment adjustment can be fairly short. Thus, we consider monthly time periods indexed by $t=...,-2,-1,0,1,2...$; where $t=0$ denotes the treatment month, i.e. the month where a new subsidized employee is hired. Observation units $i=1,...,N$ are workplaces which at the beginning of the treatment month ($t=0$) have not hired a subsidised employee during the past 13 months.

ATTs are estimated for various outcomes 1-13 months after the hire of a new subsidized employee. Because the wage subsidy scheme allows contracts of maximum 12 months length, the estimated effects cover the termination of the studied wage subsidy spells.

Participation is described by the random variable, D_{i0}, which takes the value 1 in case the workplace hires a subsidized employee and 0 otherwise. Considering first regular employment as the outcome, we assume the existence of two potential stocks of regular employees for workplace i, denoted (1) and $y(0)$ for $t>0$, where the first corresponds to the outcome which is realized if a new subsidized employee is hired at $t=0$, and the second denotes the control outcome (realized if no subsidized employee is hired). The observable stock of employees can be written as follows:

(1) $y_{it} \equiv D_{i0}(1)+(1-D_{i0})y_{it}(0)$

so that the workplace net employment effect of a new subsidized employee is defined by

(2) $\beta_t = (1)-y_{it}(0)$

The stock of employees may be decomposed into the components which are observable upon the start of $t=0$ and therefore not affected by participation, and the components which are potentially affected by participation. From the identity relating stock and flows

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of employees $y_{it} \equiv y_{it-1} - s_{it-1} + h_{it}$, where s_{it-1} denotes employees separated from workplace at month t-1, and h_{it} denotes the hires during month t, we have

$$(3) \qquad y_{it} = y_{i-1} - s_{i-1} + h_{i0} - S_{it-1} + H_{it}, \qquad\qquad t = 0,1,2\ldots,$$

where $S_{it-1} = \sum_{j=1}^{t} s_{it-j}$ denotes the cumulated separations from the treatment month and up to month t-1, and $H_{it} = \sum_{j=1}^{t-1} h_{it-j}$ is the cumulated hires one month after the treatment month and up to month t.[7]

Combining (1), (2) and (3) we can write the effect of a new subsidized employee on the stock of employees as follows $\beta_{it} = \beta_{it}^H - \beta_{it}^S$ where $\beta_{it}^H = H_{it}(1) - H_{it}(0)$, and $\beta_{it}^S = S_{it-1}(1) - S_{it-1}(0)$. Because we do not observe the counterfactual outcomes, we cannot identify the actual individual effects, $\beta_{it}, \beta_{it}^H, \beta_{it}^S$, but the ATT, $\beta_t \equiv (\beta_{it}|D_{i0}=1)$, can be identified under restrictions described in the next section.

Effects on the stock of regular employment can therefore be obtained as the difference in effects on hiring and separations of regular employment. In addition, as noted by Rotger & Arendt (2010), considering the flows of hires and separations directly addresses the two reasons that Calmfors (1994) described as key reasons for displacement: If a worker who is hired with a subsidy would have been hired without the subsidy (the deadweight loss effect), hiring of regular workers would go down when compared to the counterfactual. Similarly, if subsidized workers displace other workers already regularly employed, separations would go up, when compared to the counterfactual (the substitution effect). Therefore, total displacement, as measured by effects on the stock of regular employment is the sum of a potential deadweight loss effect and a substitution

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[7] Unfortunately, we lack information on the exact start date for an ordinary hire, and therefore we do not know whether ordinary hires occur before or after the start of the subsidy. This implies that we cannot measure the effect of the new subsidy on the hires in the treatment month. Instead we consider as outcome variable the cumulated ordinary hires occurring from the month after the subsidized hiring month, H_{it}, a limitation that implies that the estimated effects on cumulated hires can be seen as lower bounds. However, due to the fact that we are using high frequency data we can take hires effect one month after the treatment month as a relatively close measure of the unobservable hiring effect of the treatment month.

effect. For this reason we estimate ATT on hires and separations of regular employment. We also estimate ATT for the gross wages of regular employees, and on repeated use of subsidized employment. The former capture two aspects that employment measures do not: If subsidized workers earn more or less than the work place level average, and an indirect effect occurring if the use of subsidies put a pressure on wages for regular employees. Finally the ATT on subsequent use of subsidized work, addresses the question of whether use begets use, which could potentially cover up displacement effects, or simply reveal that there is a demand for the low paid work provided by subsidized workers.

4.2   Identification

The monthly effects are identified using difference-in-difference-matching proposed by Heckman, Ichimura, Smith & Todd (1998). This simply means, matching on differenced outcomes, hence eliminating time-invariant work place specific effects. The effects are estimated on annual differences in outcomes. This allows for some selection on unobservables in that the counterfactual outcome of control and participant workplaces may have unobservable fixed and annually varying characteristics (including a seasonality impact). For the annual differences, the ATT is identified under the following conditions:

1. $E(\Delta_{12}H_{it}(0)|x_{i-1}, D_{i0}) = E(\Delta_{12}H_{it}(0)|x_{i-1})$
2. $E(\Delta_{12}S_{it-1}(0)|x_{i-1}, D_{i0}) = E(\Delta_{12}S_{it-1}(0)|x_{i-1})$
3. $e(x_{i-1}) \equiv Pr(D_{i0} = 1|x_{i-1}) < 1$ for all x_{i-1}
4. Stable Unit Treatment Value Assumption

where e(x_{i-1}) is the conditional probability of hiring a subsidized employee in month t=0 given the set of covariates x_{i-1}, the propensity score (see Rosenbaum & Rubin 1983).

The conditional mean independence assumption (1)-(2) implies that given we control for x_{i-1}, workplaces in the participant and control groups are equally likely to hire a subsidized employee at t=0. Under this assumption, the mean difference in the outcome variable can be interpreted as the causal effect of hiring a subsidized employee.

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Assumption (3) implies that the support of x_{i-1} for the treated workplaces is a subset of the support of x_{i-1} for the controls.[8] In order to make this assumption realistic in our dataset, we have restricted the analysis to workplaces with 5-40 regular employees measured at the beginning of the month. As mentioned in section 3.1, preliminary analyses found that this delimitation maximizes the degree of comparability (balance) between workplaces that hire a new subsidized employee in a given month (participants) and those that do not (controls).[9]

In addition, the delimitation of the sample ensures that we do not mix the effect of one subsidy with that of subsequent subsidies. As mentioned in section 3.1, this is done by restricting the analysis to workplaces that have not used subsidised employment during the past 13 months.

Assumption (4) requires that the decision of any one workplace to hire a new subsidized employee does not affect the potential outcomes for all other workplaces. Given the relatively small scale of the subsidy scheme, it seems plausible to assume that it does not affect the potential outcomes of control workplaces through the relative wages offered (see Heckman, Lalonde & Smith, 1999).

The main identifying assumption, described by (1) and (2), states that we are able to control for selection by means of observable pretreatment variables. We argue that this seems plausible as we condition on a wide range of annual workplace characteristics, including the share of regular employees of a given gender, age, and origin as well as their average levels of education, experience, and previous unemployment. In addition, past monthly dynamics of hires and stocks of regular employees are controlled for, which are unarguably highly correlated with hard-to-measure costs of labour adjustment. We

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[8] Note that these assumptions allow for selection on unobservables through level (of *H* or *S*) differences as well as for selection on $\Delta_{12}H_{it}(1)$ or $\Delta_{12}H_{it}(1) - \Delta_{12}H_{it}0$ X (similarly for separations), but rules out selection on $\Delta_{12}H_{it}(0)$, after controlling for x_{i-1}.

[9] This is not surprising given the many covariates used in the matching algorithm and the fact that most municipal workplaces are within this size interval.

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also include dummy variables for small workplaces, indicating whether the stock of regular employees was below 10 in any of the months of the last quarter before the subsidized hire. Finally, since the costs of labour adjustment, the local labour market conditions and various other factors might vary across sectors, we also include the sector of the workplace in the set of covariates.[10]

4.3 Estimation

The propensity score is estimated using a logit model, controlling for the covariates discussed in section 4.2. Separate control groups are formed by matching for each of the months, March 2010 to February 2011, i.e. the participants in a given month are only matched with controls from the same month. However, we use the pooled sample to estimate effects, so that the effect a given number of months after the hire of a new subsidized employee is an average over the effects in the different months.

Different matching algorithms are explored: nearest neightbour matching with different numbers of nearest neighbours and different calipers as well as kernel matching. The results presented in section 5 are estimated with the algorithm that yields the best balance on the observable characteristics. Standard errors are estimated with the method proposed by Abadie & Imbens (2006; 2012). .

In order to test whether the effects differ for different types of workplaces (effect heterogeneity), we use a test for effect homogeneity proposed by Crump et al. (2009). The test is conducted by auxiliary regressions indicating whether the effects vary with covariates. The test is conducted for all covariates included in the propensity score. Hence, it explores the potential presence of effect heterogeneity across workplaces of different sizes, in different sectors, and with employees that differ with respect to levels of education and experience, gender and age composition etc.

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[10] Workplaces are grouped on the following sectors: Education, health services, nursing homes, residential institutions for mentally ill/addicts, residential institutions for others, in-home help, daycare for children.

5 Results

5.1 Propensity score and matching quality

The propensity score for the probability of hiring a subsidised employee in a given month from March 2010 to February 2011 was estimated using a logit model. The propensity score included sector and composition of employees at the workplace, local unemployment, and stock and inflow of regular employees 1-13 months back.[11] The results of the logit model used to estimate the propensity score are not reported for reasons of space, but are available from the corresponding author upon request.

Each participant is matched with a suitable control group using an epanechnikov kernel matching algorithm with bandwidth 0.001.[12] This reduces the median bias in covariate means from 5.87 to 0.62. The quality of the matching is further examined in Table 5.1 by comparing percentage standardised differences (%SDIF) in the means of selected covariates before and after matching. The standardised differences are reduced considerably by matching and that the remaining differences are small (and in all cases statistically insignificant, as may be seen in the appendix).

Table 5.1 Balance on selected covariates before and after matching

Covariates	Mean before matching		%SDIF before matching	%SDIF after matching
	Participants	Controls		
Gender: Female*	0.762	0.772	-4.2	-1.0
Age: 30-39 years*	0.232	0.229	2.4	-0.3
Age: 40-49 years*	0.278	0.273	3.2	0.3
Age: 50-59 years*	0.297	0.290	4.1	-0.7
Age: 60+ years*	0.080	0.091	-10.9	0.7

[11] Outflow is a linear combination of stock and inflow and is therefore not included.

[12] This algorithm was found to minimise the median bias, i.e. produce the best covariate balance, in comparison with alternative algorithms (matching with the 1, 5 and 20 nearest neighbours combined with calipers of 0.001 and 0.005).

Educ: Upper secondary school*	0.057	0.058	-1.1	-0.3
Educ: Vocational*	0.279	0.308	-10.8	0.1
Educ: Short further*	0.026	0.025	1.7	3.5
Educ: Medium further*	0.432	0.410	7.4	-1.7
Educ: Long further *	0.057	0.049	5.5	1.2
Orig. country: Western*	0.018	0.023	-12.0	-0.6
Orig. country: Non-western*	0.036	0.040	-4.4	-0.7
Experience: 5-9 years*	0.095	0.100	-5.2	-0.4
Experience: 10+ years*	0.858	0.853	4.1	0.4
Annual unemp: 1-49%*	0.065	0.059	7.4	1.0
Annual unemp: 50-100%*	0.005	0.004	6.4	4.4
Municipal unemployment rate	6.452	6.167	20.9	-0.2
#reg. emp. 1 mth. before	16.369	14.657	19.8	-0.4
#reg. emp. 6 mth. before	16.501	14.838	18.2	-0.3
#reg. emp. mean 1-12 mth. before	16.495	14.811	18.9	-0.3
inflow reg. emp. 1 mth. before	0.519	0.441	7.3	-2.0
inflow reg. emp. 6 mth. before	0.572	0.499	5.9	-1.3
inflow reg. emp. 12 mth. before	0.537	0.530	0.7	0.3
#reg. emp. 1 mth. before <10	0.251	0.330	-17.5	0.6
#reg. emp. 2 mth. before <10	0.244	0.329	-19.0	0.1
#reg. emp. 3 mth. before <10	0.245	0.329	-18.6	0.3
Sector: Health services	0.053	0.049	2.0	-0.9
Sector: Nursing homes	0.239	0.218	5.2	0.4
Sector: Residential inst., mentally ill/addicts	0.066	0.064	0.8	1.3
Sector: Residential inst., others	0.063	0.072	-3.5	-1.3
Sector: In-home help	0.140	0.141	-0.5	1.5
Sector: Daycare for children	0.290	0.317	-5.9	-0.6

Notes: * denotes that the variable is defined as share of regular employees with this characteristic. %SDIF denotes the percentage difference in means between participating and control workplaces standardised by dividing with pooled standard deviation for the full sample.

Figure 5.1 illustrates the balance after matching on stock and inflow of regular employees at the workplace 1-13 months before a new subsidised employee is hired.

Figure 5.1 Balance on regular employment dynamics after matching
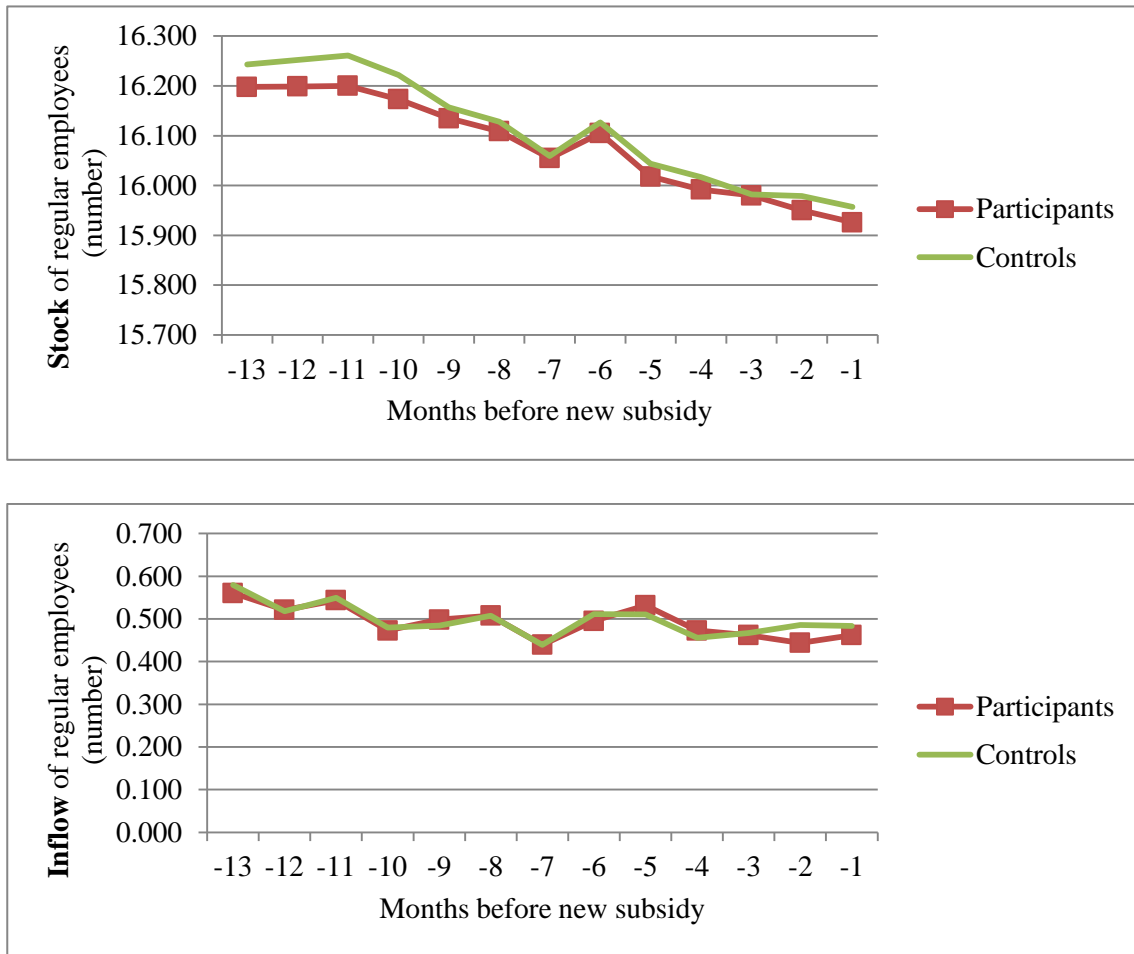
It is seen from Figure 5.1 that matching on the propensity score has succeeded in constructing a control group which is fairly comparable with the participating workplaces with respect to employment dynamics in the 13 months up until the hire of a subsidised worker.

Table 5.2   Common support

	Off	On	Total
Controls	0	44,397	44,397
Participants	34	660	694
Total	34	45,057	45,091

Finally, Table 5.2 shows that for around 5% (34 out of 694) of the participating workplaces it was not possible to find suitable controls, i.e. they were off common

support. It is insured that workplaces without suitable controls are not included in the analysis by dropping 5% of the participants for which the density of the propensity score for identified controls is lowest.

5.2 Effects

Table 5.1 shows the average effects of hiring a new subsidized employee on the workplaces which did this between March 2010 and February 2011. All effects are estimated from 1 to 13 months after a new subsidized employee has been hired, which implies that most of the contracts will run out within the time period considered. The effects on employment and income are estimated for annual differences defined as the difference between a given month (within the period March 2010 – February 2011) and the corresponding month one year earlier. This approach also controls for seasonal effects in employment dynamics.

Table 5.3   Average effects of hiring a subsidised employee for workplaces which did this from March 2010 to February 2011

Months after new subsidy	Inflow of reg. emp. (std.err.)	Outflow of reg. emp. (std.err.)	Stock of reg. emp. (std.err.)	Gross income (std.err.)	New subsidised hire (std.err.)
1	-0.047	0.049	0.049	7703*	0.036***
	(0.047)	(0.044)	(0.116)	(4411)	(0.009)
2	0.018	-0.015	0.018	6860*	0.026***
	(0.048)	(0.046)	(0.120)	(3539)	(0.008)
3	0.025	0.109 *	0.058	1095	0.039***
	(0.047)	(0.058)	(0.118)	(4137)	(0.009)
4	-0.046	0.043	-0.098	-3604	0.049***
	(0.046)	(0.047)	(0.124)	(3506)	(0.010)
5	0.027	0.082	-0.114	-5994	0.043***
	(0.046)	(0.070)	(0.126)	(3929)	(0.009)
6	0.058	-0.001	-0.138	-3009	0.127***
	(0.046)	(0.066)	(0.137)	(4643)	(0.014)
7	0.029	0.042	-0.109	-7455*	0.026***
	(0.047)	(0.047)	(0.145)	(4178)	(0.009)
8	0.056	0.089	-0.095	-9006**	0.034***
	(0.056)	(0.067)	(0.150)	(4329)	(0.009)
9	0.028	0.077	-0.156	-5992	0.028***
	(0.040)	(0.047)	(0.160)	(4919)	(0.009)
10	0.017	-0.001	-0.216	-6736	0.038***

|					
	(0.039)	(0.044)	(0.162)	(4840)	(0.010)
11	-0.004	0.069	-0.220	-6854	0.043***
	(0.041)	(0.044)	(0.166)	(4681)	(0.010)
12	-0.040	-0.100 *	-0.328**	-5912	0.038***
	(0.044)	(0.054)	(0.164)	(5410)	(0.010)
13	0.049	-0.011	-0.179	-5811	0.049***
	(0.045)	(0.051)	(0.169)	(4839)	(0.010)

Notes: The effects on employment and income are estimated for annual differences defined as the difference between a given month and the corresponding month one year earlier. Standard errors are estimated using the method of Abadie & Imbens (2006). * Significance at 10%-level; ** Significance at 5%-level; *** Significance at 1%-level.

5.2.1 *Employment effects on regular employees*

The employment effects in Table 5.3 include effects on the stock of regular employees as well as the flow in and out of the workplace. The effects on the inflow of regular employees 1-13 months after hiring a new subsidized employee are all statistically insignificant and of varying sign. Likewise, the outflow of regular employees is not significantly affected by the hire of a new subsidized employee in the majority of the following 13 months.

Figure 5.2 illustrates the cumulative effects on inflow and outflow over time. There appears to be a tendency for the use of subsidized employment to increase the inflow and the outflow of regular employees over time. While the cumulative inflow does not at any time reach statistical significance, the cumulative outflow of regular employees is significantly higher at workplaces which have hired a subsidized employee from 7 months after the hire and onwards. The magnitude of the significant increase in the cumulative outflow is between 0.30 and 0.55 employees.

Figure 5.2 Cumulative effects on flows of regular employees
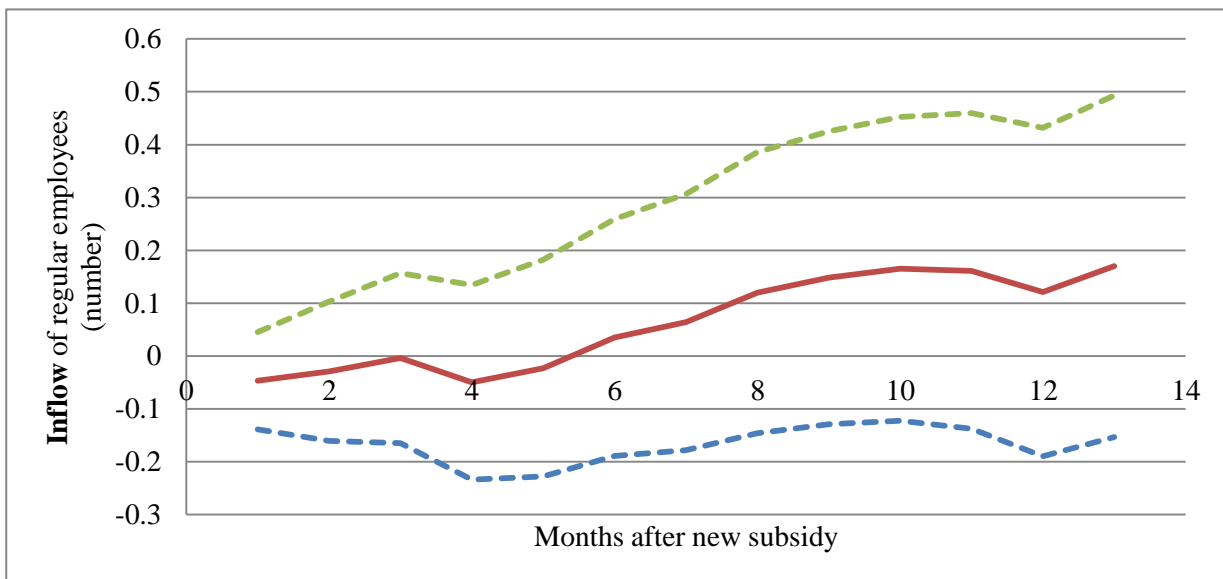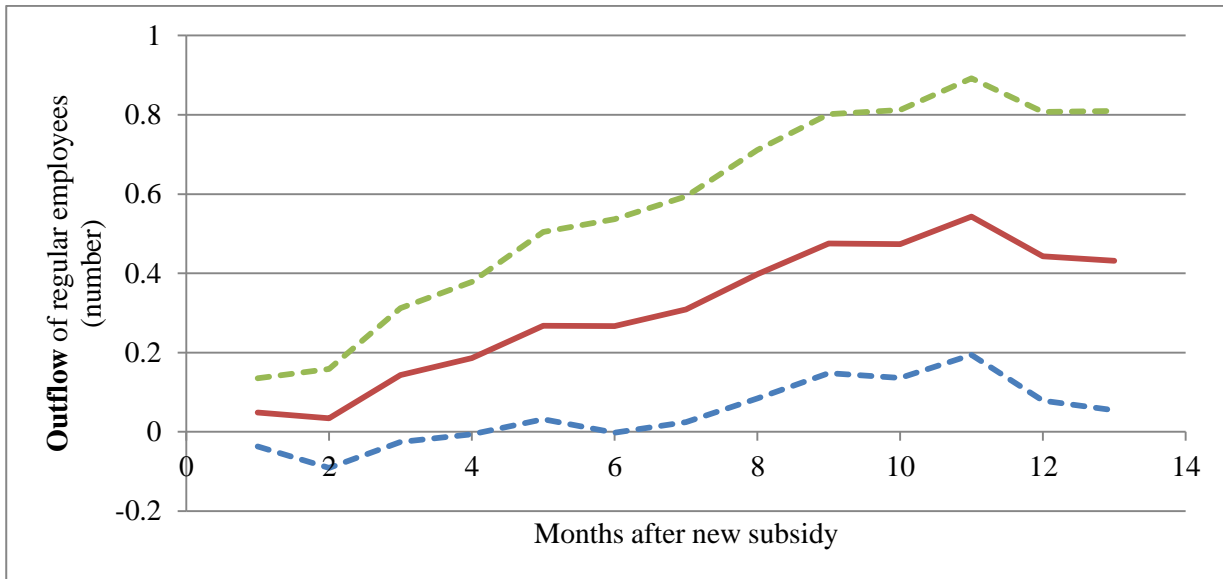
Note: The dotted lines are 95% confidence limits.

Finally, it is seen from Table 5.3 that the effects on the stock of regular employees are positive 1-3 months after the hire of a new subsidized employee and subsequently negative, i.e. added up over time the effects on the outflow are larger than the effects on the inflow. However, except for a significant decrease of 0.328 employees after 12 months, the estimated effects on the stock of regular employees are all statistically insignificant.

5.2.2   *Income effects on regular employees*

It is seen from Table 5.3 that the effects on the gross income for regular employees are positive the first two months after the hire of a new subsidized employee and thereafter negative, although largely insignificant. The significant estimate 8 months after the hire of a new subsidized employee implies that the gross expenditure on wages to regular employees drops on average by DKK 9,006 per workplace (not per employee). Besides an actual drop in the wage level, this decrease may be caused by fewer employees, a drop in working hours or substitution to less expensive employees.

5.2.3   *Repeated use of wage subsidies*

Finally, Table 5.3 shows that the effects of hiring an employee with a wage subsidy in a given month on future subsidized hires are positive and statistically significant in all months. The sharp rise in the effect 6 months after the subsidized hire is most likely due to biannual contracts being

extended. Although the estimated effects generally appear to be quite small, they should be interpreted keeping in mind that only 1-2% of the workplaces in the analysis sample hire a subsidized employee in any given month. Hence, the effects imply that the initial hire of a subsidized employee almost triples the probability of hiring a subsidized employee in a given future month. Cumulated over 13 months, the estimated effects correspond to an increase in the probability of repeated use of subsidized employment of over 50 percentage points.

5.3 Sensitivity analyses

The robustness of the results was assessed by several sensitivity analyses. As mentioned in section 5.1, participants and controls were matched using various matching algorithms, among which the epanechnikov kernel matching algorithm with bandwidth 0.001 was found to provide the best covariate balance in comparison with the alternatives of matching each participant with the 1, 5 and 20 nearest neighbours combined with calipers of 0.001 and 0.005. However, although the tested algorithms differ with respect to balance, they were found to result in almost identical effect estimates.

Estimating the propensity score using a probit model instead of a logit did not affect the results noticeably. Neither did excluding the 10% of the observations with the highest propensity scores. It was though that this might affect the quality of the matching and thus the effects, because there are relatively few workplaces with high propensity scores.

We also estimated the effects on the levels of employment and income, i.e. the absolute numbers of ordinary employees, inflows, and outflows, and the absolute level of wage expenditure, rather than the annual differences. This approach avoids the assumption of parallel trends invoked in the main analysis, but does not control for unobserved constant differences between controls and participants. The resulting effects are less precisely estimated, but quantitatively similar, to the effects for annual differences.

Finally, the effects were estimated for a sample including workplaces that have hired a subsidized employee within the previous 13 months. As expected, the resulting effects (which measure the effect of hiring a subsidized employee in a given month relative to one of the previous months) are smaller than the main effects. However, they point in the same direction: negative effects on the number of ordinary employees and positive effects on out- and inflows.

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5.4 Heterogeneity

Table 3.1 summarizes the results of the effect homogeneity tests. The hypothesis of effect homogeneity cannot be rejected for several outcomes and months, such as the effect on the stock of regular employees from six months after the hire of a new subsidized employee and onwards. On the contrary, the hypothesis of effect heterogeneity cannot be rejected in the previous months, just as it is the case various other months and outcomes. As mentioned in section 4.3, the tests summarized in Table 3.1 are based on several auxiliary regressions that may be used to indicate on which dimensions the heterogeneity occurs. The results of these regressions unambiguously indicate that the average employment level before the hire of a new subsidized employee is the dominant factor with respect to effect heterogeneity. The results of the auxiliary regressions are not reported for reasons of space, but are available from the corresponding author upon request.

Table 5.4   F-test (62, 533) of effect homogeneity, March 2010 – February 2011

Month	Inflow of reg. emp.	Outflow of reg. emp.	Stock of reg. emp.	Gross income	New subsidized hire
1	5.83***	1.23	1.73 ***	0.99	0.63
2	0.61	2.06***	1.31 *	0.96	0.97
3	2.10***	1.25	1.50 **	1.52***	0.78
4	1.34**	1.04	1.55 ***	1.21	1.36**
5	1.45**	0.54	1.30 *	1.64***	1.07
6	0.92	2.25***	1.15	1.46**	1.11
7	2.48***	1.94***	1.01	1.35**	1.41**
8	0.56	3.33***	0.86	0.83	0.97
9	1.23	0.80	1.14	0.91	1.04
10	1.38**	1.05	1.17	1.07	1.26*
11	0.96	4.73***	1.07	0.96	1.81***
12	1.04	0.68	0.88	0.84	0.87
13	3.92***	1.19	1.08	0.94	1.36**

Note: * Significance at 10%-level; ** Significance at 5%-level; *** Significance at 1%-level.

It was also tried to run the analyses for subsamples of small and larger workplaces, respectively, but the results of these analyses were judged to be invalid due to small sample sizes and poor balance. Moreover, we ran the analyses for subsamples based on specific sectors. However, it turned out that there were too few observations in these subsamples to estimate anything with reasonable certainty.

6 Discussion

The analyses show that the use of wage subsidies increases the outflow of ordinary employees. For workplaces with 5-40 employees, the total outflow of ordinary employees is increased by 0.44 employees one year after the hire of a subsidized employee. This effect is, however, offset by an average positive, but not significant effect on the inflow of ordinary employees at the same workplaces. The net effect on the total number of ordinary employees is negative, but does not differ significantly from zero. The effects on the gross expenditure on wages to ordinary employees are also predominantly negative, but likewise insignificant. Finally, the analyses show that present use of wage subsidy increases future use, either through renewal or because new subsidized employees replace the previous ones.

One possible interpretation of the results is that subsidized employment appears to serve as a recruitment channel to try out new employees, but overall does not reduce the number of ordinary employees or the expenditure on wages to ordinary employees significantly. However, the fact that hiring an unemployed worker with wage subsidy often leads to renewal or repeated use of wage subsidies may cover up substitution that does not show in the number of ordinary employees, which implies that the total substitution may be larger than what is estimated for the regular employees. This is, however, likely to be a consequence of the legislative framework surrounding the subsidy scheme.

The possible displacement effects found in this study are relatively large in comparison with those found by the National Labour Market Authority (2005), but small relative to the findings of Dahlberg & Forslund (2005) that two thirds of all (mainly public) subsidized employees in Sweden crowd out regular employment. However, methodological differences between the studies and the fact that they are conducted in different institutional settings and/or at different points of the business cycle imply that the effects are hardly comparable. Although we would expect to see larger displacement effects during an economic downturn, where many municipal workplaces are subject to cuts in public spending.

Even though several sensitivity analyses have been undertaken and the results appear to be rather robust, it should be kept in mind that the effects are determined with some uncertainty. Moreover,

the use of an observational design implies that the presence of selection effects, which it has not been possible to control for, cannot be ruled out.

As discussed in section 3.1, the estimated effects are based on a sample of workplaces that are observed during the entire period from January 2009 to February 2012. The analysis sample is restricted to workplaces that have not used subsidized employment during the past 13 months before the hire of a new subsidized employee. As mentioned in section 3.1, this is done mainly in order to avoid mixing controls and participants in the months before the hire of a new subsidized employee, but also to avoid that the studied wage subsidies are largely extensions or direct replacements for previous subsidized workers (dynamic effects). This delimitation entails a risk of underestimating the actual extent of displacement, if substitution primarily occurs by extending contracts for subsidized employees. As far as we can see, there is no obvious solution to this problem. Inclusion of workplaces that have used wage subsidy during the past 13 months before a new subsidized hire, will not solve it. As indicated by the sensitivity analyses discussed in section 5.3, it leads to an even larger downward bias in the estimates compared to the actual magnitude of displacement, because this approach compares use of wage subsidy in one month with use in the previous month. However, the presence of dynamic effects is to some extent explored for the analysis sample by estimating the effects of hiring an employee with a wage subsidy in a given month on future subsidized hires at the included municipal workplaces.

7   Conclusion and perspectives

Overall the results indicate that subsidies employment appears to serve as a recruitment channel to try out new employees, but in total does not reduce the number of ordinary employees or the expenditure on wages to ordinary employees. On the contrary, hiring an unemployed worker with wage subsidy often leads to renewal or repeated use of wage subsidies, which may cover up substitution that does not show in the number of ordinary employees. This type of substitution may, however, occur in consequence of the legislative framework surrounding the subsidy scheme, where the municipalities are given annual minimum quotas for the number of subsidised employees.

Seen in connection with individual-level analyses of wage subsidies at public workplaces, which find that subsidised employment does not unambiguously lead to ordinary employment, but

sometimes increases the time in unemployment (Graversen, 2012; Rosholm & Svarer, 2011), an overall picture of a scheme without positive employment effects appears.

For policy purposes, the results of this study imply that displacement of regular employment should be taken into account when designing subsidy schemes in the labour market, and care must of course be taken to ensure that a minimum of crowding out takes place. However, the immediate employment effects must be traded off against potential benefits. For one thing, the increase in employee turnover increases the opportunities of the unemployed to enter the labour market. Moreover, to the extent that the subsidised employees are weakly attached to the labour market, it may be the case that the alternative to subsidised employment is exit from the labour force. Hence, the present study does not say anything about how the current use of subsidised employment in Danish municipalities affects the overall welfare for society.

Finally, it is noted that subsidized employment may have other indirect effects than those investigated in this study, such as general equilibrium effects through wage pressure affecting other workplaces than those using the schemes. These effects are very difficult to quantify and therefore not considered in this paper. However, the immediate expectation is that in case such indirect wage effects occur, they are likely to be larger on the workplaces that use subsidized employment than on the other workplaces (Imbens & Wooldridge 2009), in which case the indirect wage effects identified in this study are the essential ones.

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Appendix

Table A.1   Descriptive statistics and balance on all covariates before and after matching, March 2010 – February 2011

Covariate	Mat-ching	Average		%SDIF	%reduc-tion |SDIF|	t-test	
		Participants	Controls			t	p>|t|
Gender: Female	Before	0.762	0.772	-4.2		-1.09	0.274
	After	0.763	0.766	-1.0	76.9	-0.18	0.861
Age: 30-39 years	Before	0.232	0.229	2.4		0.59	0.557
	After	0.233	0.233	-0.3	85.6	-0.06	0.951
Age: 40-49 years	Before	0.278	0.273	3.2		0.78	0.436
	After	0.278	0.278	0.3	90.0	0.06	0.954
Age: 50-59 years	Before	0.297	0.290	4.1		1.05	0.296
	After	0.296	0.297	-0.7	84.2	-0.12	0.904
Age: 60+ years	Before	0.080	0.091	-10.9		-2.58	0.010
	After	0.081	0.081	0.7	93.9	0.13	0.897
Educ: Upper sec. school	Before	0.057	0.058	-1.1		-0.27	0.786
	After	0.056	0.057	-0.3	77.0	-0.05	0.963
Educ: Vocational	Before	0.279	0.308	-10.8		-2.70	0.007
	After	0.279	0.279	0.1	99.5	0.01	0.992
Educ: Short further	Before	0.026	0.025	1.7		0.44	0.657
	After	0.027	0.025	3.5	-110.5	0.64	0.522
Educ: Medium further	Before	0.432	0.410	7.4		1.85	0.064
	After	0.434	0.440	-1.7	76.4	-0.32	0.750
Educ: Long further	Before	0.057	0.049	5.5		1.54	0.124
	After	0.056	0.055	1.2	79.1	0.20	0.838
Orig. country: Western	Before	0.018	0.023	-12.0		-2.87	0.004
	After	0.018	0.018	-0.6	95.1	-0.12	0.904
Orig. country: Non-western	Before	0.036	0.040	-4.4		-1.11	0.267
	After	0.034	0.034	-0.7	83.5	-0.15	0.879
Experience: 5-9 years	Before	0.095	0.100	-5.2		-1.34	0.179
	After	0.094	0.094	-0.4	92.9	-0.07	0.945
Experience: 10+ years	Before	0.858	0.853	4.1		1.03	0.304
	After	0.860	0.860	0.4	90.9	0.07	0.945
Annual unemp: 1-49%	Before	0.065	0.059	7.4		1.85	0.064
	After	0.065	0.064	1.0	86.7	0.18	0.860
Annual unemp: 50-100%	Before	0.005	0.004	6.4		1.76	0.078

Covariate	Mat-ching	Average		%SDIF	%reduc-tion |SDIF|	t-test	
		Participants	Controls			t	p>|t|
	After	0.004	0.004	4.4	31.1	0.87	0.387
Sociodemo: Missing	Before	0.003	0.007	-13.9		-2.79	0.005
	After	0.003	0.006	-8.9	36.3	-1.91	0.056
Municipal unemp. rate	Before	6.452	6.167	20.9		5.39	0.000
	After	6.426	6.429	-0.2	99.1	-0.04	0.972
#reg. emp. 1 mth. before	Before	16.369	14.657	19.8		5.25	0.000
	After	15.926	15.957	-0.4	98.2	-0.06	0.951
#reg. emp. 2 mth. before	Before	16.402	14.702	19.4		5.10	0.000
	After	15.950	15.979	-0.3	98.3	-0.06	0.954
#reg. emp. 3 mth. before	Before	16.389	14.742	18.7		4.86	0.000
	After	15.980	15.982	0.0	99.9	0.00	0.997
#reg. emp. 4 mth. before	Before	16.408	14.771	18.4		4.75	0.000
	After	15.992	16.017	-0.3	98.5	-0.05	0.961
#reg. emp. 5 mth. before	Before	16.432	14.795	18.2		4.68	0.000
	After	16.018	16.044	-0.3	98.5	-0.05	0.961
#reg. emp. 6 mth. before	Before	16.501	14.838	18.2		4.68	0.000
	After	16.105	16.127	-0.3	98.6	-0.04	0.966
#reg. emp. 7 mth. before	Before	16.392	14.854	16.7		4.27	0.000
	After	16.055	16.059	0.0	99.7	-0.01	0.993
#reg. emp. 8 mth. before	Before	16.478	14.870	17.3		4.39	0.000
	After	16.109	16.128	-0.2	98.8	-0.04	0.971
#reg. emp. 9 mth. before	Before	16.510	14.880	17.4		4.39	0.000
	After	16.135	16.157	-0.2	98.6	-0.04	0.967
#reg. emp. 10 mth. before	Before	16.682	14.880	18.6		4.82	0.000
	After	16.173	16.222	-0.5	97.3	-0.09	0.929
#reg. emp. 11 mth. before	Before	16.723	14.876	19.0		4.91	0.000
	After	16.200	16.261	-0.6	96.7	-0.11	0.911
#reg. emp. 13 mth. before	Before	16.690	14.850	18.9		4.85	0.000
	After	16.198	16.243	-0.5	97.6	-0.08	0.936
#reg. emp. mean 1-12 mth. before	Before	16.495	14.811	18.9		4.90	0.000
	After	16.067	16.095	-0.3	98.3	-0.05	0.957
outflow reg. 1 mth. before	Before	0.520	0.493	1.3		0.28	0.780
	After	0.473	0.578	-5.2	-288.7	-0.54	0.590
outflow reg. 2 mth. before	Before	0.552	0.486	4.3		0.91	0.364
	After	0.486	0.506	-1.3	69.8	-0.27	0.787
outflow reg. 3 mth. before	Before	0.523	0.485	2.7		0.60	0.551

Covariate	Matching	Average		%SDIF	%reduction |SDIF|	t-test	
		Participants	Controls			t	p>|t|
	After	0.474	0.488	-1.0	63.1	-0.17	0.862
outflow reg. 4 mth. before	Before	0.535	0.488	3.3		0.72	0.469
	After	0.474	0.503	-2.0	38.9	-0.44	0.657
outflow reg. 5 mth. before	Before	0.503	0.491	0.9		0.18	0.856
	After	0.498	0.482	1.2	-39.2	0.18	0.854
outflow reg. 6 mth. before	Before	0.661	0.514	9.3		2.24	0.025
	After	0.618	0.595	1.5	83.9	0.22	0.827
outflow reg. 7 mth. before	Before	0.463	0.516	-3.9		-0.81	0.419
	After	0.445	0.443	0.2	95.8	0.05	0.962
outflow reg. 8 mth. before	Before	0.533	0.530	0.2		0.04	0.969
	After	0.494	0.509	-1.0	-449.1	-0.19	0.848
outflow reg. 9 mth. before	Before	0.559	0.524	2.4		0.50	0.616
	After	0.533	0.536	-0.2	92.4	-0.03	0.975
outflow reg. 10 mth. before	Before	0.682	0.513	6.8		2.90	0.004
	After	0.536	0.549	-0.5	92.5	-0.14	0.887
outflow reg. 11 mth. before	Before	0.543	0.511	2.6		0.58	0.561
	After	0.500	0.520	-1.5	40.4	-0.25	0.806
outflow reg. 12 mth. before	Before	0.506	0.508	-0.2		-0.05	0.964
	After	0.505	0.492	1.2	-553.8	0.23	0.821
outflow reg. 13 mth. before	Before	0.578	0.516	5.5		1.27	0.203
	After	0.559	0.558	0.1	98.9	0.01	0.992
inflow reg. 1 mth. before	Before	0.519	0.441	7.3		2.02	0.043
	After	0.462	0.483	-2.0	72.4	-0.40	0.687
inflow reg. 2 mth. before	Before	0.536	0.446	8.4		2.37	0.018
	After	0.444	0.486	-3.9	53.9	-0.81	0.420
inflow reg. 3 mth. before	Before	0.516	0.459	5.0		1.47	0.143
	After	0.462	0.467	-0.5	90.8	-0.10	0.918
inflow reg. 4 mth. before	Before	0.478	0.466	1.3		0.31	0.757
	After	0.473	0.456	1.8	-38.3	0.33	0.744
inflow reg. 5 mth. before	Before	0.592	0.472	10.1		3.09	0.002
	After	0.532	0.511	1.8	82.4	0.36	0.716
inflow reg. 6 mth. before	Before	0.572	0.499	5.9		1.79	0.074
	After	0.495	0.512	-1.3	77.7	-0.29	0.770
inflow reg. 7 mth. before	Before	0.447	0.514	-6.8		-1.61	0.107
	After	0.439	0.439	0.0	99.8	0.00	0.998
inflow reg. 8 mth. before	Before	0.527	0.514	1.2		0.31	0.754

| Covariate | Mat-ching | Average | | %SDIF | %reduc-tion \|SDIF\| | t-test | |
		Participants	Controls			t	p>|t|
	After	0.508	0.507	0.0	98.7	0.00	0.998
inflow reg. 9 mth. before	Before	0.510	0.513	-0.3		-0.06	0.948
	After	0.498	0.485	1.4	-422.7	0.28	0.783
inflow reg. 10 mth. before	Before	0.501	0.515	-1.2		-0.33	0.742
	After	0.473	0.480	-0.6	49.8	-0.13	0.897
inflow reg. 11 mth. before	Before	0.579	0.519	5.4		1.44	0.149
	After	0.544	0.550	-0.5	90.3	-0.10	0.920
inflow reg. 12 mth. before	Before	0.537	0.530	0.7		0.16	0.869
	After	0.521	0.518	0.3	56.4	0.06	0.955
inflow reg. 13 mth. before	Before	0.641	0.544	7.3		2.12	0.034
	After	0.561	0.579	-1.4	80.7	-0.29	0.771
#reg. emp. 1 mth. before <10	Before	0.251	0.330	-17.5		-4.40	0.000
	After	0.261	0.258	0.6	96.5	0.12	0.908
#reg. emp. 2 mth. before <10	Before	0.244	0.329	-19.0		-4.76	0.000
	After	0.255	0.254	0.1	99.4	0.02	0.982
#reg. emp. 3 mth. before <10	Before	0.245	0.329	-18.6		-4.66	0.000
	After	0.256	0.255	0.3	98.5	0.05	0.958
Health services	Before	0.053	0.049	2.0		0.52	0.601
	After	0.052	0.054	-0.9	53.0	-0.17	0.869
Nursing homes	Before	0.239	0.218	5.2		1.37	0.170
	After	0.242	0.241	0.4	91.6	0.08	0.939
Residential inst., mentally ill/addicts	Before	0.066	0.064	0.8		0.20	0.841
	After	0.068	0.065	1.3	-64.3	0.23	0.822
Residential inst., others	Before	0.063	0.072	-3.5		-0.90	0.369
	After	0.061	0.064	-1.3	63.0	-0.25	0.804
In-home help	Before	0.140	0.141	-0.5		-0.13	0.900
	After	0.142	0.137	1.5	-217.7	0.28	0.780
Daycare for children	Before	0.290	0.317	-5.9		-1.52	0.129
	After	0.291	0.294	-0.6	89.2	-0.12	0.908

Notes: * denotes that the variable is defined as share of regular employees with this characteristic. %SDIF denotes the percentage difference in means between participating and control workplaces standardised by dividing with pooled standard deviation for the full sample.