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# DO WAGE SUBSIDIES INCREASE EMPLOYMENT IN SUBSIDISED FIRMS?

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**Abstract:** The present paper examines whether subsidised jobs have contributed to the employment in subsidised firms or merely substituted for non-subsidised ones. The data set is an unbalanced panel of some 31,000 firms that are followed annually between 1995 and 2002. The analysis is based on difference-in-differences, which is adjusted by regression and matching methods. The results indicate that wage subsidies stimulate employment, and the magnitude of the effect is as aimed. It is also found that subsidies have no sizable effects on non-subsidised firms of the industry or geographical area in question.

**Key words: evaluation, employment, wage subsidies**

**JEL: J18, J38, J23**

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**Tiivistelmä:** Tutkimuksessa arvioidaan, ovatko työllistämistuet lisänneet työllisyyttä tukea saaneissa yrityksissä vai ovatko tukityölliset pelkästään syrjäyttäneet yritysten muita työllisiä. Määrällinen analyysi tehdään noin 31 000 yrityksessä ja vuodet 1995 – 2002 kattavalla aineistolla. Tulosten mukaan tukityö stimuloi tukea saavien yritysten työllisyyttä ja vaikutuksen suuruusluokka on politiikkatavoitteen mukainen. Lisäksi havaitaan, että tuella ei ole juurikaan vaikutusta sellaisten ilman tukityöllisiä toimivien yritysten työllisyyteen, jotka toimivat tukea saavien yritysten kanssa samalla maantieteellisellä alueella ja/tai toimialalla.

**Asiasanat: evaluaatio, työllisyys, työllistämistuki**

# Contents

# 1. Introduction

The purpose of active labour market programmes is to increase employment by improving the human capital of the unemployed work force through training and periods in subsidised jobs. Traditionally, the evaluation of these programmes has concentrated on the subsequent labour market performance of unemployed persons who have participated in the programmes (Fay, 1996; Heckman, et al., 1999; Heckman 2000). A consensus finding is that the effectiveness of active labour market measures has been rather weak and varies according to the sector where subsidies are applied.[1] In Finland, results suggest that labour subsidies applied in the private sector generate small positive effects, whereas those applied in the public sector do not (Aho et al., 1999; Hämäläinen and Ollikainen, 2004; Tuomala, 2000).

Very few studies have investigated the effects of active labour market programmes at the firm level. This paper looks at individual firms, focuses on subsidised jobs, and tests for the existence of a dead-weight loss. In other words, it questions whether subsidised jobs stimulate employment in firms, or whether employees in subsidised jobs simply substitute for non-subsidised employees.[2] In addition, it is checked whether wage subsidies have any adverse effects on non-subsidised firms in the same industry and/or region.

Rare points of reference include the following. Dahlberg and Forslund (2005) show that the participants of the wage subsidy programs reduce non-subsidised employment at the municipality level in Sweden. Hujer et al. (2002) report that wage subsidies targeted to special unemployment groups have not increased employment in German firms. There are more studies on more general measures, which reduce employers' social security contributions when recruiting to low-paid jobs. For example, according to a survey by van Polanen et al. (1999) as much as 90 per cent of new recruits would have been recruited even without these measures in the Netherlands. Bishop and Montgomery (1993) find that at least 70 per cent of the tax credits granted to employers are payments for workers who would have been hired even without the subsidy, suggesting that such payments represent mere free transfers to employers. There are similar findings in a survey by Martin (2000).

Among labour market programmes, many countries have favoured training over subsidised jobs. For instance, in the USA, Canada, Norway and UK the share of subsidised jobs was less than 10 per cent of the total spending on active labour

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[1] In terms of wage subsidies Robertson (1994) reviews wage subsidy policies in different countries. Burtless (1985) finds that wage subsidies have a stigmatizing effect on participants, whereas Richardson (1998) argues that subsidised jobs enhance the future employability of participants.

[2] This question is analogous to that asked in the studies on the effects of public R&D subsidies on private R&D (i.e. Klette and Moen, 1997; David et al., 2000; Busom, 2000; Wallsten, 2000; Lach 2002).

market programmes in 2001 (OECD, 2003). The Finnish experience is of interest, since subsidised jobs have been favoured over training programmes. The share of subsidised jobs of all measures was almost 30 per cent in 2001, a figure comparable to that in France and the Netherlands.[3] Moreover, Finland relies rather heavily on labour market programmes. In 2002 the share of all labour market programmes relative to GDP was 3.1 per cent, whereas the OECD average was 1.7 per cent (OECD, 2004).

Wage subsidies decrease the marginal cost of additional labour, enabling subsidised firms to employ workers who would not otherwise have been employed. Effects on labour demand can be direct and indirect. The direct effect emerges through the increase in the firms' total employment (which is firm-financed plus subsidised). The indirect effect occurs through the response of the firm-financed employment to the subsidy. If the wage subsidy substitutes for own (or private) employment, the effect on firm's total employment decreases compared to the full direct effect. On the other hand, if it stimulates firm-financed employment, then the effect of the subsidy is magnified relative to the direct effect. Thus, understanding the relationship between wage subsidies and firm-financed employment is necessary for a correct assessment of the role of wage subsidies in boosting employment.

Furthermore, addressing this question would provide new insights into the evaluation of active labour market programmes, as traditional analysis using individual data ignores the firm level reactions. Publicly supported jobs ought to be complementing private employment. It would therefore be contrary to the stated goal if wage subsidies were to substitute for private employment.

A dead-weight loss can occur if subsidised workers would have been hired even without subsidies. In this case, wage subsidies are superfluous, crowd out private employment, and the employment funds released by the subsidy are used elsewhere in the firm (instead of employment). If, however, the funds released by the superfluous subsidy are used to employ other workers, the subsidy may be accomplishing its purpose, though the effect would be backhanded.

The challenge in evaluating the effect of the wage subsidies is to find out how many workers the firm would have employed had it not received the subsidy. A variety of potential estimators for the present type of evaluation problem are summarised, among others, in Meyer (1995), Heckman and Smith (1996), Heckman et al. (1999), Angrist and Krueger (2000), Blundell and Costa Dias

[3] The number of people in subsidised jobs has traditionally been higher than in labour market training programmes (Ministry of Labour, 2000). For example, in 1999 the number of people in subsidised jobs was a little over 50,000, whereas that in labour market training was a little less than 40,000. Both figures are very substantial in a country with about 2.4 million people in active employment. Note, however, that a large number of subsidised jobs are in the public sector (municipalities), and the total amount of all direct subsidies to private firms is less than the EU average (Venetoklis, 2001).

(2000), Blundell and MaCurdy (2000), and Smith (2000). The present paper combines widely used the methods of difference-in-differences, regression and matching. Heckman et al. (1998) and Smith and Todd (2005) show that the combination of methods is a competitive alternative to pure matching estimators or index-sufficient methods, such as those using the inverse Mills ratio.

This paper uses empirical data that spans from 1995 to 2002. The analysed sample of firms is taken from the registers compiled by the Finnish Tax Authority. Data include firms from various industries, including manufacturing, wholesale and retail trade, and business services.

The rest of the paper is organised as follows. Section 2 provides a description of the subsidy scheme in Finland. Section 3 presents the evaluation methods and section 4 summarises the data at hand. Section 5 reports the results, while section 6 concludes.

# 2. Subsidy scheme in Finland

The purpose of wage subsidies is twofold. First, subsidies are aimed at creating employment that was not affordable without subsidies and, second, subsidies should raise human capital and skills of the (otherwise) unemployed workers so that after the subsidised period firms would be willing to pay them according to the standard wage level.

Wage subsidies are delivered on the basis of job specific applications submitted by firms. Finnish legislation provides very wide eligibility, basically stipulating that all firms are eligible unless non-profitable or otherwise facing a threat of bankruptcy.

Subsidies are distributed through Local Labour Offices (LLOs) that appoint unemployed workers to the subsidised jobs. Subsidised jobs are designed for the unemployed who cannot find themselves a job or labour market training through the LLO, who are long-term unemployed or are facing the threat of unemployment, or unemployed at the age of 25 or younger. Wage subsidies are used to fill the gap between wages that firms are willing to pay to these workers and the prevailing unionized wage level. The subsidies are grants, in that the recipient firm is not obliged to pay the money received back to the distributor.

The wage subsidy is between € 430 and € 770 per month. The exact amount is at the discretion of the LLOs. A prerequisite for the maximum amount is that the weekly working hours are at least 85 percent of the normal hours in the industry in question. The maximum length of a subsidised job is 10 months, the average being 6 months.

Typical subsidised jobs are for cleaners, clerks, secretaries, office workers, unskilled manufacturing workers and salesmen. According to rough estimates the average monthly wage in these jobs is about three times higher than the average subsidy. Thus, a wage subsidy decreases the wage cost of a subsidised worker by one third. Apart from their subsidised status, these jobs in private firms have exactly the same specifications as the non-subsidised ones. When receiving a wage subsidy the firm must be able to demonstrate that the job is new, the worker has a non-fixed term contract, and the firm has not laid-off workers from similar jobs just prior or during the subsidy period.

# 3. Framework of empirical analysis

## 3.1 Stimulation effect and dead-weight loss

As mentioned above, the subsidy covers the costs of subsidised jobs only partially. Therefore, other things staying equal, every subsidy euro, S, stimulates private employment outlays by ΔP euros and the total employment outlays by ΔY euros, yielding ΔY=S+ΔP. According to rough estimates, every subsidy euro should increase P by 2 €, since the average subsidy covers about one third of the wage cost of a subsidised worker.

If, however, the firm would have employed the worker even without the subsidy, the subsidy is superfluous from the point of view of the government and produces a dead-weight loss. Now, a wage subsidy decreases private employment outlays of the subsidised firm compared to the counterfactual case. The decrease in P equals the amount of the subsidy, as the subsidised firm directs the released funds to other-than-employment purposes, such as marketing, dividends and profits. Consequently, the effect on the total employment (private + subsidised) outlays remains unchanged compared to the counterfactual case. In other words, ΔY=S+ΔP=0, and thus S=-ΔP.

A superfluous subsidy can increase the total employment outlays of the firm, however, if the firm readjusts its employment expenditures released by a superfluous subsidy to employ other workers. This would leave the private employment unchanged compared to the counterfactual case and increase the total employment. In this case ΔY=S, and ΔP=0.

## 3.2 Difference-in-Differences

The effect of wage subsidies is illustrated utilising the event of receiving a subsidy. In the empirical analysis the size of subsidy is also taken into account. Let D=1 denote the event of receiving a subsidy and D=0 denote the event of not receiving a subsidy. Let y represent the logarithm of a firm's total payroll. Due to limitations in data availability, employment is measured by payroll and the average wage controls for differences in wages across firms.[4]

The difference between y1 and y0 is the change between the actual payroll and the payroll that would have occurred had the firm not received the subsidy. Since

[4] In the present data the personnel variable has a certain measurement error. Payroll figures are therefore preferred as the endogenous variable. The personnel variable is used to construct the average wage which in turn is used as a covariate.

it cannot be observed what the 'y0' would have been for firms that indeed received subsidies, the effect has to be estimated.

Following the tradition of the difference-in-differences approach, the effect of treatment on the treated is estimated by comparing the difference in outcomes before and after the intervention for groups affected by it to the respective difference for unaffected groups.[5] In practice, the effect of subsidies is estimated by following the change in payroll in firms that start receiving subsidies during the years compared with those that do not receive subsidies in any of the years. In other words, the firms that do not receive any subsidies approximate the counterfactual situation, y0.

Let $\Delta y1_{it} = y1_{it} - y1_{it-1}$ and $E(\Delta y1_{it} \mid D_{it}=1, D_{it-1}=0)$ be the expected change in payroll among firms that received a subsidy in period t but did not receive a subsidy in the previous period t-1. Similarly, let $E(\Delta y0_{it} \mid D_{it}=1, D_{t-1}=0)$ be the payroll that would have occurred in the same firms had they not received a subsidy at t. The difference-in-differences (DiD) parameter β measures the average percentage change in the firms' total payroll between what was actually observed among the subsidised firms and what the payroll change in these firms would have been had they not received the subsidy:

$$E(\Delta y1_{it} \mid D_{it}=1, D_{it-1}=0) = E(\Delta y0_{it} \mid D_{it}=1, D_{it-1}=0) + \beta \qquad (1)$$

$$\beta = E(\Delta y1_{it} - \Delta y0_{it} \mid D_{it}=1, D_{it-1}=0). \qquad (2)$$

In an extreme case, β=0, indicating that the subsidy is superfluous and all the released private payroll funds are used for non-employment purposes. At the other extreme, β=ms, where s=S/Y (s and β are comparable, since s is the amount of wage subsidies per total payroll and β measures relative change in total payroll), and m is the total wage of a subsidised worker relative to the size of subsidy. As discussed above, the average subsidy covers about one third of the total wage of a subsidised worker, yielding m≈3. If β=ms firms contribute to the cost of a subsidised worker and the subsidy stimulates employment. Finally, if 0<β<ms, a subsidy is superfluous and it replaces private payroll funds. It can nevertheless increase total payroll, as the subsidised firm redirects part of the released funds back to employment uses.

[5] Lalonde (1986), Meyer (1995), Heckman and Smith (1996), Heckman et al. (1999), Angrist and Krueger (2000), Blundell and Costa Dias (2000), Blundell and MaCurdy (2000), and Smith (2000). For restrictions and new developments in DiD estimation, see Athey and Imbens (2003), Bertrand et al. (2004), Donald and Lang (2001), and Hansen (2003).

### 3.3 The regression adjustment

Unfortunately, the method described above would not give a valid result, as firms are not randomly selected to receive subsidies. Therefore, the basic DiD method is elaborated by conditioning the expected mean payroll of firms on observable characteristics, which may correlate with both $y_{it}$ and $D_{it}$. The following fixed effect model of the log payroll is estimated:

$$y_{it} = \beta D_{it} + X'_{it-1}\alpha + \eta_i + v_t + \varepsilon_{it}, \qquad (3)$$

where the indicator variable D is a dummy for subsidised firms (i.e. it changes from 0 to 1 when a firm starts receiving a subsidy and remains zero throughout the period for the control group). $X_{it-1}$ is a vector of covariates measured around the time when subsidised firms are chosen. This model allows for firm-specific unobserved effects ($\eta_i$) and economy-wide shocks ($v_t$) to affect both the payroll and subsidy status of the firm.

The estimator is appropriate if, conditional on the covariates, the determinants of subsidy status (whether or not subsidised) stay constant during the three-year sample period (described below). The approach fails, however, if the receiving of a subsidy is associated with other changes in payroll that are not observable outside the firm prior the subsidy period. For example, firms may direct a superfluous subsidy to wages rather than employment. In the case of an effective subsidy, subsidised jobs may decrease the possibilities to raise the wages of non-subsidised workers of the firm. In another instance, firms may use wage subsidies to partially fund investments or direct superfluous subsidies to profits. These possibilities are controlled by estimating an additional specification that includes the average wage, fixed capital and profits both in the lagged and the current period form in the model (other covariates are described below). The estimated $\beta$ shows a change in payroll, keeping these variables constant before and during the year of subsidy.

Apart from cases above, the inclination to apply for wage subsidies should not be associated with abrupt increases in the payroll of firms, as the wage subsidies are directed to low-wage and low-skill jobs and to the unemployed work force. It is not likely that firms would rely on these workers in their growth plans. A downward bias is even less likely to occur, since eligibility criteria state that applicant firms must be profitable and subsidised firms may not fire workers from similar jobs during the subsidy period. Hence, wage subsidies cannot easily be used to support the operation of declining firms.

## 3.4 The matching approach

Matching methods utilise propensity scores and search for matches in the close neighbourhood of the treated units by using weighting functions (Rosenbaum and Rubin, 1983). It has been found to be crucial to estimate propensity scores with a model that is as detailed as possible (Heckman et al., 1998; Lechner, 2002). Usually, the cruder the model, the more biased the results come out. This finding is followed by building a detailed probit model to compute propensity scores.

The matching procedures assume that after controlling for a set of observable characteristics, the mean outcomes are conditionally independent of the subsidy status. In the probit model, programme participation is explained by pre-subsidy variables. Since the firms can be followed over time and the difference (including payroll) between subsidised and non-subsidised firms in the pre-subsidy period is minimised, the effect of the programme is the difference in log payroll in the year of subsidy.

However, there may be systematic differences between participant and non-participant outcomes even after conditioning on observables. Matching can be combined with the difference-in-differences approach, allowing for temporally invariant differences in outcomes between participant and non-participant firms (Heckman et al., 1997; Heckman et al., 1998). This estimator is analogous to the regression-adjusted DiD method, except that it does not impose the restriction of linear functional form in estimating the conditional expectation of the outcome variable, and it re-weights the observations according to weighting functions. The DiD matching estimator is also otherwise similar with the ordinary matching method, but it deducts the subsidy-year difference between the groups from the pre-subsidy difference (Smith and Todd, 2005).

## 3.5 Displacement effect

The identification of the treatment effect requires that subsidised firms do not harm the operations of non-subsidised ones. If they do, our estimate for the counterfactual employment decreases (as the employment of non-subsidised firms decreases) and our estimates are biased upwards. In this case, the estimate not only includes the true effect of treatment on the treated firms, but also the effect of subsidies on other firms. On the other hand, subsidised firms may benefit from other firms through supply-chain linkages, if subsidised firms win market share and therefore increase the demand for intermediate products.

If subsidies harm non-subsidised firms, the public subsidies displace private employment. This can happen via the product market. As the wage subsidies decrease the marginal cost of labour in the subsidised firms, they have a competitive edge over non-subsidised ones and thus may win market shares from them. In addition, the possible dead-weight loss may increase the odds for the displacement effect, since the subsidised firms are able to reallocate the subsidies to uses other than employment. For example, due to the subsidy, they may decrease their product prices or intensify marketing with the money released from employment expenditure.

It is hypothesised that if subsidies were to generate a displacement leakage, it would most probably happen among firms within the same industrial sector and/or geographical region. Three potential sources of displacement are estimated. They are based on the effects generated by other firms being in the same (i) industrial sector, (ii) region, or (iii) industrial sector and region as the subsidised firms during a specific year.

# 4. Data set

Our sample has been taken from the registers compiled by the Finnish Tax Authority. These registers cover the whole population of firms that pay taxes in Finland, including information on their industrial sector, size, financial statement accounts and possible business subsidy receipts. The data set under analysis spans from 1995 to 2002.

The analysis is based on fully functioning ordinary-sized firms for which there are at least three consecutive years of records in the data set (between 1995 and 2002).[6] This restriction facilitates the before-after estimation, the use of lagged variables in our impact estimations and makes the panel more balanced.

The total number of distinct firms is 30,908 and that of observations 165,211. The observations are distributed relatively evenly among years, although 2001 has the most. Most of the firms appear in the data for the full 8 years. The data set includes 28,637 subsidy records during the 8-year period (Table 1). Some 25,000 firm/year observations are subsidised, the remaining 141,000 observations concerning firms that did not receive any subsidies from any source during the period.[7] Wage subsidies are the most common type of subsidy (61%), whereas the average amount of wage subsidies (€ 4,300) is clearly smaller than that received from other sources. R&D subsidies are delivered the least often (14%), but their average size is the largest (€ 96,100). Investment subsidies comprise the largest total amount delivered during the period (€ 508 million).

[6] First, very small firms and those firms that did not have an active operation throughout the financial year were eliminated. In practice, firms whose yearly turnover and payroll in any year of the period examined was implausible low, or firms that employed less than one fulltime person were excluded. Second, to account for outlier effects, firms that had extremely low or high average wage costs per employee were dropped, as well as firms whose subsidy yearly receipts exceeded a certain very high threshold per subsidy type. To eliminate the effects of take-overs and mergers, firms whose logarithm of sales had changed more than (+/-) 2.0 times over a year were dropped.

[7] Note that some of the subsidised firms have received subsidies from several sources in one given year, yielding a difference between the subsidy records and the respective subsidised observations.

*Table 1. Summary statistics for subsidies, 1995-2002 (yearly observations)*

| Subsidy type (source) | N | % | Sum of subsidies, € million | Average subsidy, € |
|---|---|---|---|---|
| Wage | 17,367 | 61 | 83 | 4,272 |
| Investment | 7,838 | 27 | 508 | 39,165 |
| R & D | 3,432 | 12 | 316 | 96,092 |
| Totals | 28,637 | 100 | 809 | 36,793 |
| | | | | |
| Total with subsidies | 24,658* | 15 | | |
| Total without subsidies | 140,553 | 85 | | |
| Total observations | 165,211 | 100 | | |

* This total number of observations (22,932) does not correspond to breakdown per source (26,437), as there are firms that have received subsidies from more than one source during one year.

The subsidised firms are much larger than the non-subsidised ones (Table 2). The mean number of personnel in the subsidised firms is 71, whereas it is 21 in the non-subsidised ones. Subsidised firms are also more profitable than others. One novelty in the data set is that it includes firms outside the manufacturing sector. It appears that manufacturing firms receive wage subsidies most often. Among the subsidised firms, the average size of wage subsidy relative to payroll is 2.6 per cent.

*Table 2. Summary statistics*

| | Subsidised | | | | Non-subsidised firms | | | |
|---|---|---|---|---|---|---|---|---|
| | Mean | Std. Dev. | Min | Max | Mean | Std. Dev. | Min | Max |
| **Endogenous variables** | | | | | | | | |
| Number of personnel | 71 | 264 | 1 | 9,223 | 21 | 112 | 1 | 9,474 |
| Payroll, € 1000 | 1,847 | 8,313 | 12 | 319,074 | 596 | 4,114 | 12 | 392,965 |
| Wage subsidy / payroll | 0.026 | 0.055 | 0.00 | 0.98 | 0 | 0 | 0 | 0 |
| **Subsidy dummies** | | | | | | | | |
| Wage subsidies | 1 | 0 | 1 | 1 | 0 | 0 | 0 | 0 |
| Investment subsidies | 0.14 | 0.35 | 0 | 1 | 0.04 | 0.19 | 0 | 1 |
| R&D subsidies | 0.05 | 0.21 | 0 | 1 | 0.02 | 0.13 | 0 | 1 |
| **Control variables** | | | | | | | | |
| Net profit, € 1000 | 362 | 2,745 | -29,175 | 130,099 | 181 | 2,341 | -107,876 | 148,122 |
| Sales, € 1000 | 10,345 | 49,876 | 22 | 1,504,686 | 3,728 | 29,724 | 20 | 1,642,392 |
| Fixed capital, € 1000 | 2,855 | 24,854 | 0 | 1,108,363 | 1,131 | 15,215 | 0 | 979,054 |
| Payroll / personnel, € 1000 | 22 | 16 | 12 | 886 | 26 | 15 | 12 | 1,440 |
| **Sector** | | | | | | | | |
| Manufacturing | 0.45 | 0.50 | 0 | 1 | 0.33 | 0.47 | 0 | 1 |
| Wholesale and retail trade | 0.26 | 0.44 | 0 | 1 | 0.25 | 0.43 | 0 | 1 |
| Business services | 0.12 | 0.32 | 0 | 1 | 0.17 | 0.37 | 0 | 1 |
| Other private services | 0.16 | 0.36 | 0 | 1 | 0.25 | 0.43 | 0 | 1 |
| Other | 0.01 | 0.09 | 0 | 1 | 0.01 | 0.08 | 0 | 1 |

# 5. Results

## 5.1 Stimulation effect or dead-weight loss?

Starting the analysis, the panel data is re-organised as follows: First, all the firm/year observations are split into three period groups. For firms that appear in the data in more than three consecutive periods, more than one three-year groups can be formed (e.g. for firms that appear in 4 consecutive years, two three-year groups can be formed). Second, all groups are pooled next to each other in order to form a large three-period panel. Finally, we drop from the sample all those firms that have subsidies in periods 1 and 2. As a result, treated firms start receiving them in period 3, whereas the control firms do not have subsidies in any of the periods. Thus, firms that receive subsidies before they are observed in the data have a minimal effect on the results.

In a crude difference-in-differences estimation the effect is about 9 per cent ($e^{0.090}-1 = 0.094$) (Table 3). After the regression adjustment (adding lagged control variables to the fixed effect model) the effect stays rather similar. All coefficients of this specification are reported in the Appendix.

The following control variables are used in the estimations. Profitability is a major condition for eligibility and it eases liquidity constraints, creating room for future expansions. It thus correlates both with subsidy status and employment growth.[8] Sales, fixed capital and the average wage are the standard determinants of labour demand.[9] Time dummies and the average wages control for inflation, making the use of nominal rather than real figures sufficient. We also control for other possible subsidies that firms may receive during the period of analysis, such as investment and operation subsidies or R&D subsidies. The models also have firm-specific effects.[10] All variables used in the estimations are described in the level form in Table 2 above.

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[8] Profitability is added to the model in the form of the net euro-term profit/loss. We do not take the logarithm of this variable, as a fraction of the firms make gross losses every year (in our data, losses appear with a negative sign). Sales and fixed capital are in the log-form, the wage level is computed as payroll per personnel.

[9] The theoretical model behind the analysis can be considered as labour input (cost) minimising competitive firms with given demand (the level of sales) and fixed capital.

[10] There might be, first, regional differences in the economic environment or in industrial policies, or the legal form of firms could matter. Local Labour Offices may apply standards for applicant firms differently from each other, partially due to the fact that in some regions there may simply be more applicant firms (Venetoklis, 1999). Second, there are clear differences in growth rates between industries. Also, firms in one industry may be more inclined to apply for subsidies than those in another industry. Finally, there can be differences between firms (within industries), including variations in managerial skills and time-invariant efficiency levels.

In the model in Euro terms (i.e. the regression of payroll on subsidies in Euros and control variables), the effect is high but imprecisely estimated (the bottom row in Table 3). This is probably due to non-normal distribution of the payroll and subsidy variables, suggesting that it is preferable to use logged values. The point estimate is 6.7, showing that every subsidy euro would increase private payroll almost 6 euros. Due to the imprecise estimates of the level model the rest of the analysis focuses on log-form specifications with subsidy-dummy.

*Table 3. Fixed effect estimation*

| Endogenous variable | $D_t$ | Robust t-value | $R^2$-within | N | Controls |
|---|---|---|---|---|---|
| Ln(Payroll) | 0.090 | 11.6 | 0.05 | 145,074 | year dummies |
| Ln(Payroll) | 0.083 | 11.4 | 0.05 | 90,423 | + pre-subsidy period variables |
| Ln(Payroll) | 0.092 | 12.5 | 0.16 | 88,774 | + subsidy-period values of wage level, fixed capital and profits |
| | $S_t$, € | Robust t-value | $R^2$-within | | Controls |
| Payroll, € | 6.72 | 1.7 | 0.03 | 90,423 | year dummies and pre-subsidy period variables |

Note: We compute the Huber/White/Sandwich estimator of variance to remove heteroscedasticity. The column labelled $D_t$ gives the coefficient on the subsidy dummy, and that labelled $S_t$ gives the coefficient on the subsidy variable, measured in money terms. No firm has received subsidies in the period 1 or 2. Treated firms start receiving subsidies in the final year of the data. Control variables include firm fixed effects and year dummies, as well as one-period lags of the net profit, average wage, the log of sales and the log of fixed capital. All equations also include controls for firms that have received other types of subsidies, i.e. R&D and investment subsidies in the final period.

One argument against the result obtained with the lagged control variables is that subsidies may change the average wage during the subsidy period and thus affect the payroll without affecting employment itself. Similarly, firms may time their investments so that subsidised employment can replace the need for genuine recruitment. Further, subsidies may leak into profits. These possibilities are controlled by including the subsidy-period values of the average wage, fixed capital and profits in the model. The resulting change in the estimate is small, increasing the effect to 10 per cent.

Another possible argument against the estimated model is that labour demand equations usually include lagged dependent variable on the right hand side. The effect of the lagged dependent variable on the results is checked by using the GMM method (Arrelano and Bond, 1991). To estimate such a model more than three periods are needed. Therefore, the data are re-arranged so that all firms appear in the data from three to eight consecutive years, according to the availability of the data for these firms.

Moreover, for each year t we drop from the data firms that receive subsidies in period t-1. The treated firms start receiving them in period t, whereas the control firms continue not having subsidies. This means that depending on the total number of years that a subsidised firm appears in the sample (from three to eight), the subsidy dummy is zero from two to seven years prior to the subsidy period, and then turns to one in the final year. For all the non-subsidised firms the dummy is zero in every year.

Difference GMM and System GMM models are estimated. The difference GMM removes the fixed effects by first differences and estimates the model as a system of equations, one for each time period. The equations differ in only in their instrument condition sets. Since the lagged dependent variable is endogenous, it is instrumented by its own appropriate lags in levels. Other regressors enter the instrument matrix in the conventional instrumental variables fashion in first differences. The difference GMM estimation results in an 8 per cent effect (Table 4). The problem, however, is that the model does not pass the over-identification test. The test is passed when the lag of sales, profits, fixed capital and the average wage are excluded from the set of instrumental variables (only appropriate lags of log-payroll are used as instruments). This specification results in a 6 per cent effect. All coefficients of this specification are reported in the Appendix. The inclusion of current period values of fixed capital, average wage and profits raises the estimate to 9 per cent. However, this specification does not pass the over-identification test. The inclusion of second lags of the variables does not help passing the test either (not reported in Table 4).

The difference GMM estimation may suffer from the weak instrument problem if the lagged levels are poorly correlated with first differences. The system GMM alleviates the problem by adding to the system original period-wise equations in levels, which are instrumented with suitable lags of their own first differences. However, the system GMM does not help passing the over-identification test (Table 4).

*Table 4. One-step GMM estimations; endogenous variable is ln(Payroll)t*

| | $D_t$ | Robust t-value | Probabilities Ar1/ Ar2/over-id. |
|---|---|---|---|
| Difference-GMM | 0.073 | 7.4 | 0.00 / 0.14 / 0.00 |
| Difference-GMM (including only appropriate lags of log-payroll as instruments) | 0.054 | 3.1 | 0.00 / 0.28 / 0.76 |
| System-GMM | 0.063 | 7.1 | 0.00 / 0.04 / 0.00 |
| System-GMM (excluding sales, fixed capital, profits and wage level from the set of instruments) | 0.116 | 8.4 | 0.00 / 0.07 / 0.00 |

Note: We compute the Huber/White/Sandwich estimator of variance to remove heteroscedasticity. The column labelled Dt gives the coefficient on the subsidy dummy. No firm has received subsidies in t-1 or earlier. Treated firms start receiving subsidies in the final year of the data. Control variables include firm fixed effects and year dummies, as well as one-period lags of the net profit, average wage, the log of sales and the log of fixed capital. All equations also include controls for firms that have received other types of subsidies, i.e. R&D and investment subsidies in the final period. Ar1 and Ar2 are the tests for first and second order autocorrelation, respectively. Over-id. tests for over-identification of instruments.

Finally, the matching method seeks the most comparable control firms for the treated firms and compares payroll between the two groups. These estimations are conducted with two-period data, where no firm receives subsidies in the first period and a part of them start receiving subsidies in the second period.

In the probit model, programme participation in period two is explained by variables that are the same as in the regression approach. In other words, firms to be compared must have a very similar pre-subsidy payroll, average wage, fixed capital, sales, and profitability. In addition, 82 sub-regional and 19 industrial dummies are added to control for differences in the probability of receiving a subsidy, and for growth differences across regions and industries. Since the firms can be followed over time and the differences in the pre-subsidy period is minimised, the effect of the programme is the difference in log payroll in the year of subsidy.

Among the matching models, the neighbourhood matching produces the most plausible results (Table 5). When no matching is used the difference in log-payroll between subsidised and non-subsidised firms increases from 0.665 before the subsidy to 0.755 during the year of subsidy. The change (0.090) is the same as obtained above in the pure DiD model without control variables. The neighbourhood matching methods dramatically reduces the before-the-subsidy-difference between the groups. However, even the optimal number of neighbours (that turns out to be 11) cannot totally remove the difference.

*Table 5. Matching models*

| | Difference Before the subsidy | Difference After the subsidy | Difference After-before | Median bias | Bias reduction |
|---|---|---|---|---|---|
| No Matching | 0.665 | 0.755 | 0.090 | 3.2% | |
| Matching with | | | | | |
| Nearest neighbour | -0.062 | 0.039 | 0.101 | 2.1% | 33% |
| 5 nearest neighbours | -0.026 | 0.061 | 0.087 | 1.0% | 69% |
| 11 nearest neighbours | | | | | |
| with pre-subsidy matching variables only | -0.017 | 0.066 | 0.083 | 0.7% | 79% |
| also with subsidy-period variables | -0.029 | 0.084 | 0.113 | 0.8% | 76% |
| Kernel (bandwitdth 0.06) | 0.312 | 0.399 | 0.087 | 1.6% | 49% |

Note: "Difference" is the difference in the mean log-payroll between subsidised and non-subsidised firms. "Bias" refers to the difference of the sample means in the subsidised and non-subsidised firms as a percentage of the square root of the average of the sample variances in the two groups. "Median bias" is the median of biases over all 108 variables used in estimations. Propensity scores are obtained from probit models. The common support option is enforced. One-percent trimming. The subsidy-period variables include the average wage, fixed capital and profits.

Nevertheless, the method reduces the median bias[11] by almost 80 per cent. In this case the subsidy-year difference in log-payroll between the two groups is 7 per cent. If the remaining difference in the pre-subsidy period is taken literally, the DiD matching estimator raises the subsidy effect up to 9 per cent ($e^{0.066+0.017}-1=0.086$). Kernel and Radius matching methods produce much poorer test results than nearest neighbour methods, implying higher bias in the estimated effect. Finally, the inclusion of current period values of fixed capital, wage level and profits somewhat increases the estimate. In this case, the effect during the year of subsidy is 9 per cent.

## 5.2 Displacement effect

As wage subsidies have indeed increased employment in subsidised firms, a relevant follow-up question is whether the subsidies have affected non-subsidised firms. To create an industry-specific displacement variable, all wage subsidy amounts delivered within each industry in a year are first summed. Second, we deducted from this aggregate subsidy the amount of subsidies the firm in question received that year. Of course, in the case of non-subsidised firms the amount deducted was zero. The created variable shows for each firm the total amount of subsidies (in the log-form) given to the other firms in the industry in question.

[11] The mean difference in the subsidised and non-subsidised firms as a percentage of the square root of the mean variances in the two groups (Rosenbaum and Rubin, 1985).

The same approach was followed to create the other displacement variables, i.e. region-specific or region-industry-specific variables.

The displacement effect is estimated by adding each displacement variable in turn to regression equation (3) above with lagged covariates. If an effect is found, the previous estimates are biased due to the effects on the non-subsidised firms that act as counterfactuals. A displacement effect is tested with the three-period data, where no subsidies are delivered in periods 1 or 2. A proportion of the firms receive subsidies in the final period. The analysis is conducted utilising the amounts of all wage subsidies received, aggregating them at the level of twenty 5-digit SIC industries and 83 local labour markets.

Findings are listed in Table 6. First, in all specifications the inclusion of displacement variables has little effects on the coefficient of D (compare to row 2, Table 3). Thus, there is no indication of a displacement effect that would effectively bias the estimates on the subsidy dummy reported earlier.

Second, there is no indication of a displacement effect. The case for the effect should be the most pronounced when firms of the same industry and region are studied. Firms in one industry and geographical location are competing with each other more than firms in the same industry all over Finland. The point estimate of aggregate wage subsidies delivered within industry-region cell is negative but not statistically significant (row 1, Table 6).

*Table 6. Effects of industry- and region-wide subsidies: Fixed estimation*

| Effect on firms in… | $D_t$ | Robust t-value | ln(subsidies) | Robust t-value |
|---|---|---|---|---|
| the same industry and region* | 0.082 | 10.7 | -0.0001 | -0.4 |
| the same industry* | 0.082 | 10.7 | 0.0013 | 2.3 |
| the same region* | 0.082 | 10.7 | 0.0012 | 0.5 |

Note: All equations have lagged control variables. *as the subsidised firms. The column labelled Dt gives the coefficient on the subsidy dummy. The column ln(subsidies) gives the coefficient on a variable that refers to the log of industry-specific, region-specific, or industry and region specific amount of subsidies. N = 89,725.

The estimated effected of the aggregate wage subsidies delivered within industries (irrespective of region) is slightly positive (row 2). However, the magnitude of the effect is so small that the finding does not have much economic meaning. Doubling the wage subsidies in an industry increases the payroll of firms by one tenth of a per cent, suggesting that firms within an industry benefit from subsidies through supply-chain linkages.

Finally, there is no indication of a statistically significant effect of the regionally aggregated variable (row 3). All the findings of a possible displacement effect are virtually similar when we try other combinations of industrial aggregations or all subsidies (wage, investment R&D) instead of wage subsidies only (results not shown).

# 6. Discussion and Conclusion

This paper evaluated the effect of wage subsidies on the employment of subsidised and non-subsidised firms during 1995–2002. The main finding was that wage subsidies have stimulated employment in subsidised firms. The estimates range around 9 per cent ($\beta \approx 0.09$).

The full stimulation effect requires that wage subsidies should increase total (private plus subsidised) employment more than the size of subsidy, as the subsidy is only partial. According to our rough estimates the total effect on payroll should be three times higher that the average size of subsidy (i.e. $m \approx 3$). Since in the data at hand $s \approx 0.026$ (s is the average proportion of wage subsidies relative to payroll), the finding is that $\beta$ indeed equals $m*s$. Thus, the conclusion is that the wage subsidy programme has been stimulating employment, it has been reaching its objective, and the magnitude of the effect has been as aimed. Evidence was also found that subsidies have no sizable effects on non-subsidised firms of the industry or geographical area in question.

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# APPENDIX

*Table A1. Results of the fixed effect and difference GMM estimations*

| | Fixed effects | | One-step difference GMM | |
|---|---|---|---|---|
| Variable | Coefficient | Robust t-value | Coefficient | Robust z-value |
| $\text{Ln(payroll)}_{t-1}$ | | | 0.494 | 4.3 |
| $\text{D(wage subsidy)}_{t}$ | 0.083 | 11 | 0.054 | 6.6 |
| $\text{D(Investment subsidy)}_{t}$ | 0.045 | 3.3 | -0.068 | -0.1 |
| $\text{D(R\&D subsidy)}_{t}$ | 0.089 | 3.6 | 0.130 | 0.6 |
| Year dummies (Reference 2002) | | | | |
| 1996 | -0.12 | -12 | -0.018 | -0.7 |
| 1997 | -0.11 | -14 | -0.007 | -0.3 |
| 1998 | -0.09 | -13 | 0.039 | 1.8 |
| 1999 | -0.07 | -13 | 0.034 | 1.8 |
| 2000 | -0.05 | -11 | 0.022 | 1.9 |
| 2001 | -0.02 | -5.6 | 0.024 | 3.3 |
| $\text{Ln(sales)}_{t-1}$ | 0.075 | 12 | -0.430 | 1.2 |
| $\text{Ln(fixed capital)}_{t-1}$ | 0.041 | 13 | 0.349 | 1.7 |
| Average wage $_{t-1}$/FIM thousand* | -0.001 | -11 | -0.001 | 1.1 |
| Net profit $_{t-1}$/FIM million* | 0.000 | 0.7 | 0.036 | 1.9 |
| Constant | 11.85 | 129 | | |
| N; $R^2$-within | 90,423; 0.05 | | 60,645 | |
| Test for 1st order autocorrelation | | | -3.02 (.003) | |
| Test for 2nd order autocorrelation | | | 1.08 (.282) | |
| Hansen test of over-identified restrictions | | | 11.7 (0.76) | |

Note: We compute the Huber/White/Sandwich estimator of variance to remove heteroscedasticity. *) In our database the original figures are in Finnish marks, 1 € = 5.94573 FIM.

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