

## Do temporary jobs help the unemployed?

by

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### **Abstract**

Temporary employment contracts have become increasingly common in many OECD countries during the last decades. In Sweden, the fraction of employees that are on temporary contracts has increased from 10 to 15 % since the beginning of the 1990s. This paper studies whether temporary jobs in the form of fixed-term replacement contracts help unemployed workers in their transition to regular employment. The results show a significant positive average effect of having a replacement contract, whereas the length of the contract that can vary between 3 and 12 months does not play any role for the future labour market outcome.

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# 1 Introduction

The last decades have shown a sharp rise in the use of temporary contracts in many OECD countries. According to the overview in Booth et al (2002), the share of employees on temporary contracts rose in 10 out of 13 surveyed OECD countries during the 1990s. Sweden stands out as an extreme example in this respect: the share of employees on some kind of temporary contract rose from 10.1 to 15.5 % between 1990 and 1998.

Booth et al (2002) conclude their overview by stating that "... temporary jobs are – from worker's perspective – bad-jobs..." (p. F188). However, this statement is based on an analysis from the *employed* workers perspective. As a contrast, the focus of this paper is the potential role that temporary contracts can play in improving the labour market position of the *unemployed*.<sup>1</sup> Thus, we focus on the whether temporary contracts improves the opportunities for previously unemployed workers to move out of unemployment and into permanent employment, i.e. we study whether temporary contracts are "stepping-stones" for the unemployed (see also Zijl *et al*, 2004).

A temporary job may improve the labour market status of a previously unemployed worker through two channels: first, by providing a contact with a specific employer and second, by providing work experience. While the first of these effects should be independent of the length of the temporary employment contract, the second effect is likely to grow with the length of the contract. A cost of accepting a temporary job is that it reduces the amount of time that can be spent searching for permanent jobs, thus the net effect is unknown *a priori*.

Our empirical analysis uses data on workers receiving 3-12 months of temporary, fixed-term, jobs as replacements for participants in a Swedish subsidised career break program. The main advantage of using this particular form of temporary contracts is that we know the *a priori* planned duration of the fixed term contract. The length and structure of the contracts are in other aspects reasonably close to the contracts used for replacing workers on parental leave, a very common phenomenon in Sweden.

The analysis have two parts: first we use propensity score matching techniques to study the effects of receiving a temporary job relative to remaining in

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<sup>1</sup> Obviously, the use of temporary contracts may affect the labour markets also for workers holding permanent jobs as well since both average labour mobility and wage profiles are likely to be affected.

unemployment. The effect is currently measured during 18 months from the start of the temporary employment but will be extended by 6 months more in February 2005. The preliminary results from this register-data analysis show that temporary contracts reduces the probability of being registered at an unemployment office 16-18 months after the start of the 3-12 month long contract from 49 % to 42 %. The effect is thus significant but not huge.

The second part of the analysis studies the effect of receiving a longer rather than a shorter temporary contract. In doing this, we compare workers receiving temporary contracts with each other and we are able to use not only register data but also survey information about the number of hours worked, the probability of having an open-ended contract and on hourly wages. The preliminary results of this analysis show no effect whatsoever of the contract length. It seems as if it is a positive effect of an established employer contact rather than increased working experience that lies behind the positive average effect.

The paper is structured as follows: Section 2 describes the institutional background and the data. Section 3 describes the method of propensity score matching and shows the results from the matched analysis. Section 4 shows results concerning the effects of contract length. Section 5 concludes.

## **2 Background and data**

### **2.1 Temporary jobs in Sweden**

#### **2.1.1 Institutions<sup>2</sup>**

Booth et al (2002) describes the strictness of Swedish employment protection as being about average by international standards, both for temporary employment and regular employment. The Swedish Employment Protection Act stipulates that contracts are open ended by default unless otherwise stated. For permanent contracts, there is no redundancy pay, but the notice-periods are longer than in most countries. Mass layoffs are accompanied by negotiations and a seniority rule is the basic principle.

Swedish labour market institutions are characterised by high union membership rates and high coverage rates of union contracts. These contracts can, in

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<sup>2</sup> This section briefly describes the use of temporary contracts in Sweden in recent years. Unless stated otherwise, it is entirely based on Holmlund and Storrie (2002).

principle, contradict most labour laws in favour of either of the parties. However, in practice very few collective agreements mitigate the Employment Protection Act. The act was reformed in 1997, allowing for the use of temporary contracts without a specified reason for up to 12 months (under some conditions). At the same time the law instigated a right for local parties to sign agreements on fixed term contracts, an option that previously only was available at higher levels of bargaining. The reformed act also stipulated that a worker having more than 3 years of temporary employment within a 5 year period should be treated as having an open-ended contract.

### **2.1.2 The use of temporary employment**

Holmlund and Storrie (2002) discuss the use of temporary employment in Sweden in the 1990s in great detail: In 1990 10 % of all Swedish employment was in the form of temporary contracts. By 2000, this number had increased to 15 %. Most of the temporary contracts are held by female workers (18 % compared to 13 % for males). The three industries providing the most temporary jobs are “Personal and cultural services”, “Education”, and “Health and care”.

The most important form of temporary employment in Sweden is fixed term replacement contracts that constituted a stable fraction of around 4-5 % of total employment during the entire 1990s. (The increased use of temporary contracts was thus mainly due to other forms of temporary employment such as on-call contracts, project work and probationary employment.) The frequent use of replacement contracts are probably a function of the long statutory vacation periods as well as the generous Swedish parental leave schemes that allow for at least 16 months of subsidised leave from work after the child is born.

Holmlund and Storrie (2002) also show that the average duration of fixed term employment spells declined over the 1990s. They estimate the average length of a temporary contract to be three quarters (compared to 40 quarters for permanent employment) on average during the 1990s. They also conclude that the main reason for the increased use of temporary contracts is due to a changed macroeconomic environment (such as higher unemployment rates) rather than due to legislative changes.

## **2.2 Data**

### **2.2.1 The temporary jobs**

The data on temporary jobs that we use are generated by a Swedish subsidised career-break program that was run as a pilot program in 12 Swedish municipalities during the period February 2002-December 2004.<sup>3</sup> Participation in the career break program was conditional on that the employer hired a previously unemployed replacement worker for the duration of the subsidised absence.

This paper studies how the replacement workers were affected by the temporary jobs. The advantage of using these particular replacement contracts lies in the fact that we are able to observe the exact planned lengths of the fixed-term contracts. We use data on the replacement workers and other individuals registered with the Public Employment Service in the 12 participating municipalities from 2002 onwards.

### **2.2.2 The register data**

The register data are collected from the administrative register database Händel at the National Labour Market Board. The database contains detailed information on unemployed individuals' registration date, job training activities and participation in labour market programmes as well as individual characteristics such as gender, age and level of education. The data cover all individuals registered at the Public Employment Service (PES) since August 1991. According to the career-break program rules, the replacement workers should be recruited among job seekers registered with the PES. Thus, all of the replacement workers are included in the database. We will return to this issue in section 3.2.

The paper uses data from 1998 to August 2004 (the observation window will be extended until February 2005). The study uses individuals registered at the PES in the twelve Swedish municipalities included in the career-break program. For each individual we can observe an event history including program participation and the number of spells and days of unemployment.

In the Händel database, the fixed-term contracts associated with the career-break program are considered as ordinary employment and *not* as a labour market program. The replacement workers are thus recorded as “searching on-the-job” (or in a similar category) or as deregistered from the PES register. The choice between these two alternatives is determined by the unemployed (substitute) in consultation with the unemployment officer. We obtain information on the exact starting date and (initial) length of the fixed term contract from the

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<sup>3</sup> The program is instituted on a permanent and national basis from January 2005.

National Labour Market Board. This information is connected to each replacement worker in our data. We use all 1,100 fixed-term contracts starting between August 2002 and March 2003.

### **2.2.3 The survey data**

*A survey in February 2005 directed to 2000 workers receiving a temporary contract before March 2003 will provide additional information. The survey can be linked to the register data and will be used in section 4 to investigate to what extent contract length affects hours worked, wages and the probability of getting a permanent contract.*

## **2.3 The structure of the analysis**

We are interested in whether the unemployed workers who receive temporary employment improve their future labour market status more than if they had remained as unemployed. Furthermore, we wish to investigate whether workers who get long temporary contracts do better than workers with shorter contracts.

We do the analysis in two steps. First, we look at the average effect of receiving a temporary contract compared to not doing so. Second, we look at the effect of contract length. The reason for this division of the analysis is twofold: The first reason is that the workers receiving temporary contracts differ substantially from the average unemployed worker (see Larsson and Nordström Skans, 2004). Thus, to alleviate the “common support” problem we wish to use propensity-score matching techniques (see below) when estimating the effect of getting a temporary job compared to not getting a temporary job, and in order to do so we need to make the treatment variable dichotomous. The second reason is that we have access to detailed survey information about the labour market status after the end of the contract for those that got a temporary contract. Since we do not have this information for a control group we can only use it when comparing replacement-workers on different contract lengths with each other.

In the empirical model we study the effects on discrete outcomes and do not use a duration analysis framework. The reason is that the individuals receiving temporary jobs have a history of frequent transitions between unemployment and shorter spells of employment. Thus, we will not look at the time to a specific event takes place as an outcome, rather we use time periods during which we measure the incidence of unemployment or registration at an unemployment office.

### 3 Is a temporary job better than no job at all?

#### 3.1 Identification

We are interested in the average effect of receiving a temporary employment contract compared to not receiving one. According to the evaluation terminology, we want to identify and estimate the average effect of treatment on the treated

$$(1) \quad \theta = E(Y^1 - Y^0 | T = 1) = E(Y^1 | T = 1) - E(Y^0 | T = 1),$$

where  $T = 1$  denotes temporary job (treatment) and  $T = 0$  no temporary job (no treatment).  $Y$  is the outcome of interest, for example subsequent employment. The evaluation problem is that we cannot observe the same person in two different states at the same time, and thus the counterfactual  $E(Y^0 | T = 1)$  - what would have happened to the individual had she not had the temporary employment contract - is unknown.

The true causal effect can never be identified. To be able to identify the *average* treatment-on-the-treated effect, we need a valid proxy for the counterfactual outcome. If the data available contains information on all the factors affecting both the selection into the treatment and the outcome variable, we identify the identification of the counterfactual outcome assuming conditional independence

$$(2) \quad \{Y^0, Y^1\} \perp\!\!\!\perp T | X = x, \forall x \in \mathcal{X},$$

where  $\perp\!\!\!\perp$  is the symbol of independence and  $\mathcal{X}$  denotes the set of covariates for which the average treatment effect is defined. In words, the conditional independence assumption (CIA) states that, given all the observable characteristics ( $X$ ), the selection into the treatment are not based on the actual outcomes of the various treatments. Moreover, in order for the average participation effect to be identified, the probability of participation must be strictly between zero and one:



$$(3) \quad 0 < P(x) < 1, \text{ where } P(x) = P(T = 1 | X = x).$$

When these two assumptions are fulfilled the counterfactual outcome,  $E(Y|T = 1, X = x)$ , can be obtained by simply matching the participants with *identical* (with respect to  $X$ ) non-participants, and then taking the average of the non-participants' outcomes:  $E(Y|T = 0, X = x)$ .

In their seminal paper on matching, Rosenbaum & Rubin (1983) show that if the CIA is valid for  $X$ , it is also valid for a function of  $X$  called the *balancing score*  $b(X)$ , such that  $X \perp\!\!\!\perp T | b(X)$ . The main advantage of the balancing score property is the decrease in dimensionality: instead of conditioning on all the observable covariates, it is sufficient to condition on some function of the covariates. In the case of only one treatment, the balancing score with the lowest dimension is the propensity score  $P(x) = E[P(T = 1 | X = x)]$ .

### 3.2 Results

The stock of replacement workers consists of 1,153 individuals that started their temporary employment between April 2002 and March 2003. The matched comparison group is collected among all other individuals registered with the PES during the same period and in the same twelve career-break program municipalities. There are 125,484 potential comparison persons altogether. However, the number of comparison units is much higher, 829,468, since we define a unit by individual *and* month in order to control for seasonal variation. For example, a comparison person who is registered with PES on July 1, 2002, and August 1, 2002, implies two comparison units.

The first of every month constitutes the hypothetical start date for the comparison units, similar to the start date of the replacement contract. All time-variant variables, such as previous unemployment, are defined according to that date.

The propensities of having a replacement contract are estimated by probit. Table 1 shows descriptive statistics on the variables included in the probit estimations. The results from the probit estimations are reported in the Appendix. Having removed individuals with *e.g.* inconsistent PES histories, the final sample sizes are 978 replacement workers and 639,480 potential comparison units (118,567 comparison persons). We also remove replacement workers who were not registered immediately before they received the replacement contract, be-

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cause they cannot be matched with any of the potential comparison persons who all are registered with the PES.

Besides the propensity, we match on registration with PES prior to the temporary employment as it is considered as a crucial variable explaining both the probability of receiving a replacement contract *and* the future labour market status. The distance between the estimated propensity and the length of the registration spell is measured by a Mahalanobis distance metric.<sup>4</sup> Nearest neighbour matching is applied without replacement. Column (4) in Table 1 illustrates the matching quality by reporting the absolute standardized biases for each covariate and pair-wise comparison. In general, the match quality is satisfactory, and thus we consider the covariates to be balanced.

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<sup>4</sup> The Mahalanobis distance metric is frequently used in the matching literature, see e.g. Lechner (2001) and Larsson (2003).

**Table 1** Descriptive sample statistics and balance of the covariates

Variable	Replacements	All potential comparisons	Matched comparisons	ASB
Age (years)	35.6	39.9	35.5	0.90
Male	0.314	0.496	0.331	3.72
Citizenship				
Swedish	0.917	0.851	0.910	
Nordic	0.022	0.021	0.018	2.89
Non-nordic	0.061	0.128	0.072	4.10
Education				
Compulsory or less	0.135	0.267	0.151	
High school	0.516	0.469	0.488	5.73
University	0.349	0.264	0.361	2.56
<b>Registration data (days)</b>				
Duration of registration before contract start	288.7	775.1	288.7	0.00
Days of unemployment during the last year	79.3	134.3	80.6	1.28
Days of registration during the last year	99.1	121.3	89.8	7.91
Days of unemployment during last 4 years	259.6	411.6	255.6	1.41
Days of registration during last 4 years	289.8	369.6	257.3	9.80
Contract start				
May -02	0.001	0.106	0.002	2.61
July -02	0.001	0.118	0.000	4.52
August -02	0.048	0.121	0.049	
September -02	0.387	0.115	0.393	0.48
October -02	0.176	0.107	0.174	0.54
November -02	0.082	0.107	0.073	3.45
December -02	0.022	0.106	0.024	0.68
January -03	0.242	0.110	0.245	0.71
February -03	0.041	0.110	0.041	0.00
Status in the PES register 1 day before contract start				

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Openly unemployed	0.339	0.350	0.316	
Labour market training	0.015	0.065	0.008	6.64
Youth program	0.003	0.010	0.003	0.00
Subsidised employment	0.031	0.148	0.039	4.46
Non-subs. employment	0.545	0.303	0.542	0.62
Searching interlocally	0.067	0.124	0.092	9.06
No of observations	978	639,480	978	
Mean of ASB				2.85

Note: The absolute standardised bias (ASB) is defined as  $100 * \frac{abs(\bar{x}_m - \bar{x}_l)}{\sqrt{\{s_m^2(x_{mi}) + s_l^2(x_{li})\} / 2}}$ ,

where  $\bar{x}_j$  and  $s_j^2(x_{ji})$  ( $j = m, l$ ) are the sample mean and variance of each covariate  $x_{ji}$ .

We use two general outcome measures: registration with PES and unemployment. Recall that being registered with the PES may imply that the person is openly unemployed, participating in a labour market program, or even employed (subsidised or non-subsidised) but searching for a new job. Here, the outcome variable ‘unemployed’ contains open unemployment and participation in labour market training. The outcome variable ‘registration’ includes also all spells when the person is registered as employed job seeker.

Thus far, we have a observation window of minimum 18 months after the replacement contract started. (The observation window will be prolonged by 6 months in the revised version.) Table 2 reports the results for the treatment effect on the treated 12-18 months after the program start, i.e. when even the longest replacement contracts have ended. Two definitions of both outcome measures are used: The average number of days as unemployed/registered, and a dummy for having any days as unemployed/registered. Moreover, to look at the dynamics of the effect, we count the days within two periods: 12-15 months and 16-18 months after the contract start.

**Table 2** The average treatment effect on the treated (T-E-T)

	Unemployment			
	12-15 months after start		16-18 months after start	
	Average number of days	Share that has any days	Average number of days	Share that has any days
Replacements	15,9	0,247	13,4	0,218
Comparisons	18,8	0,505	17,9	0,269
T-E-T	-2,9	-0,258	-4,5	-0,051
Standard error	1,45	0,021	1,38	0,019

  

	Registration			
	12-15 months after start		16-18 months after start	
	Average number of days	Share that has any days	Average number of days	Share that has any days
Replacements	32,9	0,426	31,3	0,416
Comparisons	40,6	0,537	38,3	0,496
T-E-T	-7,7	-0,110	-7,0	-0,080
Standard error	1,88	0,022	1,84	0,022

Recall that all negative figures in Table 2 should be taken as positive results for the replacement contract. The results show that a replacement contract leads to significantly less unemployment after the contract has ended. Both average number of days in unemployment and the share of the sample that has any unemployment days are lower among the replacement workers than in the matched comparison group. Furthermore, the effect on average number of unemployment days seems to strengthen over the time period of six months that we are able to observe.

Putting together these results, they indicate that having a replacement contract first and foremost decreases the risk of unemployment after the contract is ended. Even if some of the replacement workers return to unemployment for a while, they seem to escape it within quite a short period.

When we count all type of registration with PES into the outcome measure, the treatment effect remains qualitatively the same: having a replacement improves the worker's future labour market situation. However, during the first follow-up period, the risk of having any registration days is much higher than

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the risk of being unemployed among the replacement workers. In the comparison group, these risks do not differ much. Thus, in the short run, the treatment effect on registration is not as large as the effect on unemployment. In the longer run, the magnitude of the registration effect is approximately the same as of the unemployment effect.

In sum, Table 2 suggests that, one year after the contract ended, the replacement workers have higher chances of not being registered with the PES than the matched comparison group. Their average number of registration days is smaller, as well. However, if they do register, they do to a larger extent it as employed job seekers than as unemployed. After additional three months, this difference between the groups disappears, and the effects on unemployment are approximately as large as the effects on registration. One explanation might be that the replacement workers receive a prolonged contract – on a general fixed-term basis – at the replacement contract employer for some months.

## 4 Is more better than less?

In this section we study how the length of a temporary employment contract affects labour market attachment after the end of the contract. The analysis is based on comparisons between workers receiving temporary contracts of various lengths. Since we compare workers who all received a temporary contract with each other we do not need an external control group. Since the length affects the month of contract expirations, we will control for this by including a dummy variable for each expiration month ( $M$ ). Thus, denoting the outcome by  $Y$  and the contract-length by  $L$  we have an empirical model describing the effect of the contract-length at  $t$  months after the end of the program, conditional on  $X$ , as

$$Y_{it} = \beta_t X_i + \lambda_t L_i + \delta_t M_i + \varepsilon_{it}$$

In the analysis we use two different datasets, one register based measuring the outcome  $t$  months after the end of the contract as a function of  $L$  and one survey based dataset measuring how the workers fared in February 2005 (i.e. approximately 1-2,5 years after the end of the temporary contract).

In this section we will not use propensity score matching since we are measuring the effects of a continuous variable,  $L$  (the contract-length measured in months). Instead we use OLS and correct for the same covariates as was used in the PSM-analysis in the previous section. However, as is shown in Table 3, the length appears to be uncorrelated with the explanatory variables we have access to (apart from the dummy for non-Nordic citizenship which is significant at the 10 %-level). When estimating the effect on length (see column 1, table 3) the (relatively high)  $R^2$  is almost completely due to the inclusion of the dummies for expiration month. When estimating the equation with these dummies replaced with dummies for starting month the  $R^2$  decreases to 0,03 (adjusted  $R^2=0,01$ ).

The result that contract length is uncorrelated with observed covariates is perhaps not surprising since the length of the contract is determined by the employer and the person whom the worker is replacing. This seems to suggest that

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we could treat the length as exogenous.<sup>5</sup> However, in the analyses below we present estimates both with and without covariates.

**Table 3** Covariates

Variable	Effect on length	Effect on unemployment (days)	Effect on registration (days)
Age	-0.0126 (0.0318)	-0.8028 (0.6107)	-0.6333 (0.7980)
Age <sup>2</sup>	0.0001 (0.0004)	0.0139 (0.0077)	0.0137 (0.0100)
Male	0.1374 (0.1032)	3.4856 (1.9840)	-3.0738 (2.5924)
U1	0.0001 (0.0007)	0.0183 (0.0135)	0.0634** (0.0176)
E1	-0.000 (0.001)	-0.0105 (0.0127)	0.0588** (0.0166)
U4	0.0000 (0.0002)	0.0146** (0.0046)	0.00947 (0.00601)
E4	-0.0002 (0.0002)	0.0050 (0.0043)	0.00276 (0.0056)
Pre program registration days	0.0001 (0.0001)	-0.0010 (0.0023)	0.0009 (0.0030)
Non Nordic	0.3666* (0.2037)	-4.0730 (3.9206)	-2.3112 (5.1228)
R <sup>2</sup>	0.77	0.07	0.12
Adjusted R <sup>2</sup>	0.76	0.04	0.09

Note: The models also control for education level, citizenship, month of expiration (dummies), geographical search area reported to the PES and registration category at the PES one day before the start of the temporary employment. The pre program registration period are defined as the total number of days of registration at the PES in direct connection to the temporary employment, with interruptions of maximum seven days. Registration includes all forms of registered job search at the PES. Standard errors in parenthesis. \*\*\*Significant at 1 % level. \*\*significant at 5 % level. \*significant at 10 % level.

<sup>5</sup> We have also used survey information on various family characteristics, occupation, sector etc and in none of the cases have we found any important differences. The reason for not including that information in this analysis is that it is available for only half of the sample analyzed here.



We consider four different outcomes: first we use an outcome corresponding to the previous section, registration as unemployed or employed at the public unemployment service. Second we use survey information on hours of employment in the survey week. Third, we study the effects on the probability of having an open ended contract at the time of the survey. Fourth, we study the wage effect for those working during the survey week.

#### **4.1 Do longer contracts reduce subsequent registration?**

Table 4 shows the effects of contract-lengths on unemployment within 3 months and 4-6 months after the end of the contract. In all cases we control for the calendar month that the contract ended. Table 5 shows the same exercise using registration (as either unemployed or in temporary employment) as the outcome. As can be seen from the estimates, the contract length does not seem to have effect on neither subsequent unemployment nor registration in temporary employment, a result that holds for any of the tested models. The estimates of the effect of contract length are not just insignificant; they are estimated to be zero with very small confidence intervals.

We also estimate the effect on unemployment and registration in the final week of our current sample, (2-6 of August 2004). The reason for this is that it mimics the structure on the analysis using survey data in the following subsections. Once again, however, we find no significant results of having a longer rather than a short fixed term contract.

The overall impression from this analysis is perhaps surprising since it suggests that the extra work experience received from the longer contract plays little or no role for the individuals. However, this can possibly be explained by the rich history of temporary employment that this specific group have. It is also possible that the outcome measures we are using are too crude. Thus, in the following sections we will study the effects of contract length on wages, working hours and the probability of having a regular contract in February 2005.

**Table 4** The effects of contract length on unemployment

Outcome period	Unemployment (Days)		Unemployment (dummy)	
0-3 months after end of contract	0.2057 (0.5928)	0.04045 ( 0.6000)	0.00008654 (0.0082)	-0.0027 (0.0084)
4-6 months after end of contract	0.4523 (0.5754)	0.2976 (0.5802)	-0.0055 (0.0080)	-0.0084 (0.0081)
Unemployment 2-6 August 2004	0.0512 (0.0340)	0.0386 (0.0339)		
Dummies for month	Yes	Yes	Yes	Yes
Other covariates	No	Yes	No	Yes

Note: The models with “other covariates” includes control for education level, citizenship, month of expiration (dummies), geographical search area reported to the PES and registration category at the PES one day before the start of the contract. Standard errors in parenthesis. \*\*\*Significant at 1 % level. \*\*significant at 5 % level. \*significant at 10 % level.

**Table 5** The effects of contract length on registration

Outcome period	Registration (Days)		Registration (dummy)	
0-3 months after end of contract	1.2932* (0.7708)	-1.1869 (0.7514)	-0.0142 (0.0093)	-0.0148 (0.0092)
4-6 months after end of contract	-0.0391 (0.7730)	0.0971 (0.7582)	-0.0106 (0.0093)	-0.0103 (0.0097)
2-6-August 2004	0.0642 (0.0443)	0.0649 (0.0439)		
Dummies for month	Yes	Yes	Yes	Yes
Other covariates	No	Yes	No	Yes

Note: The models with “other covariates” includes control for education level, citizenship, month of expiration (dummies), geographical search area reported to the PES and registration category at the PES one day before the start of the contract. Standard errors in parenthesis. \*\*\*Significant at 1 % level. \*\*significant at 5 % level. \*significant at 10 % level.

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**4.2 Do longer contracts increase subsequent working hours?**

*[To be written – awaiting survey data in February 2005]*

**4.3 Do longer contracts increase the subsequent probability of receiving a permanent job?**

*[To be written – awaiting survey data in February 2005]*

**4.4 Do longer contracts increase subsequent wages?**

*[To be written – awaiting survey data in February 2005]*

## 5 Concluding remarks

Temporary jobs are, perhaps not surprisingly, to a large extent received by workers with a history of cycling between unemployment and employment. Despite of this, and the fact that the recipients thus already from before have a stronger position at the labour market than the average unemployed, we find positive effects of 3-12 months long replacement contracts.

When the replacement workers are compared to a matched comparison group, the results show clearly that a replacement contract improves the worker's future labour market status. Both the probability of being registered with the Public Employment Service *and* the average number of registration days are significantly lower among the replacement workers. When only looking at days of unemployment, the effect is even larger: the risk of being unemployed among the replacement workers is only half of the corresponding risk among the comparison group. This effect, however, diminishes quickly. 15-18 months after the program start the effects on unemployment is about the same as the effects on registration: around -20 %. One potential story could be that the replacement workers get to prolong their employment for some months at the same employer, register with the PES as on-the-job seekers. 15 months after the replacement contract started many of the prolonged contracts have ended, and the positive effect of consist as much of less unemployment as of less on-the-job search registration.

The average effect of having a temporary contract seems to be clearly positive. The length of the replacement contract, on the contrary, does not seem to play any role. The effect is in fact estimated quite precisely to zero. This implies that the positive average effect reflects a positive effect of having established a contact with an employer, rather than an effect of increased working experience.

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## **Appendix**

To be written