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Stepping-stones or dead-ends? An analysis of Swedish replacement contracts

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EVALUATION

Stepping-stones or dead-ends?

An analysis of Swedish replacement contracts

Laura Larsson
Linus Lindqvist
Oskar Nordström Skans

WORKING PAPER 2005:18

The Institute for Labour Market Policy Evaluation (IFAU) is a research institute under the Swedish Ministry of Industry, Employment and Communications, situated in Uppsala. IFAU's objective is to promote, support and carry out: evaluations of the effects of labour market policies, studies of the functioning of the labour market and evaluations of the labour market effects of measures within the educational system. Besides research, IFAU also works on: spreading knowledge about the activities of the institute through publications, seminars, courses, workshops and conferences; creating a library of Swedish evaluational studies; influencing the collection of data and making data easily available to researchers all over the country.

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Stepping-stones or dead-ends?

An analysis of Swedish replacement contracts*

by

Laura Larsson,^{*} Linus Lindqvist^{**} and Oskar Nordström Skans^{***}

June 28, 2005

Abstract

The paper studies if temporary jobs in the form of fixed-term replacement contracts reduce the risk of future unemployment among job-seekers. Using exact matching on labour market history and personal characteristics we find positive effects of the replacement contract on future labour market status. We also find that the longer the replacement contract the higher the probability of having an open ended contract at the same site 2–2.5 years after the start of the contract. No effect of the length is found on unemployment, employment or wages. Overall, the results suggest that receiving a fixed term contract reduces the risk of future unemployment, and that a longer contract increases the position within the workplace but not on the market in general.

JEL-Code: J64

Keywords: Unemployment, Temporary employment, Matching.

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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 experienced one of the sharpest rises as the share of all employees during a year that are on some kind of temporary contract rose from 10.1 to 15.5 % between 1990 and 1998. In 1998, only Spain (32.9 %), Finland (17.7 %), and Portugal (17.3 %) had higher shares than Sweden.

The presence of temporary jobs is likely to affect the labour market in a number of ways, both from a micro and macro perspective. Most previous micro oriented studies have focused on the jobs from the employed workers perspective, that is, by comparing temporary contract with permanent contract. For example, Booth et al (2002) conclude their overview of this literature by stating that "... temporary jobs are – from worker's perspective – bad-jobs..." (p F188).

To complement the picture, this paper focuses on the potential role that temporary contracts can play in improving the labour market position of workers lacking regular employment. We focus on whether temporary contracts help reduce the risk of future unemployment (i.e. act as "stepping-stones") or if they instead just postpone the unemployment experience ("dead-ends"). Our comparison state is thus further job seeking. Previous works in this strain of the literature include Autor & Houseman (2005), Heinrich et al (2005a, 2005b), and Zijl et al (2004), as well as several studies evaluating the impact of subsidized temporary employment (e.g. Gerfin & Lechner, 2002, and Forslund et al, 2004). Except for Autor & Houseman (2005) that finds negative effects of temporary help jobs in the US, the above studies suggest that temporary employment, and subsidized employment that mimics "real" jobs reasonably close, have positive effects on the outcomes of the participants. A general concern regarding subsidized employment, however, is crowding out of regular employment.

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 (potentially providing access to useful networks and producing positive 'signals') and second, by providing work experience. While the first of these effects should be more or less independent of the duration of the temporary employment, the second effect grows with the length of the contract.

A potential cost of being in a temporary job is that the effort spent searching for permanent jobs may be lower during the time spent at the temporary job. Thus, the effect of receiving a *longer* temporary contract may differ from the effect of receiving a *shorter* contract. This paper aims at not only identifying the total effect of receiving a temporary contract, but also to separately identify the net effect of contract length.

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. Apart from being an evaluation of the specific policy, we find it useful to study this particular form of temporary contracts since the data include information about the *a priori* planned duration of the fixed term contract.

The analysis has two parts: first we use exact matching techniques to study the effects on future labour market outcome of receiving a replacement contract. Since we have a huge pool of potential comparison individuals, we are able to use exact matching on a large number of variables capturing labour market history and personal characteristics. The effect is measured 13–24 months from the start of the contract. The results from this register-data based analysis show that replacement contracts do improve the labour market status: the probability of being unemployed as well as the probability of being registered with the Public Employment Service (PES) decrease due to the contract.¹ An analysis of the effect on different subgroups does not provide any conclusive answer to whether the contracts are more or less beneficial for workers with a relatively weak labour market position.

The second part of the analysis studies the effect of receiving a longer, rather than a shorter, replacement contract. The length of the contract is determined by the person on leave and is, we argue, likely to be exogenous to unobserved characteristics of the replacement worker. Thus, we believe it is reasonable to rely on a linear regression model. In the analysis, we compare workers receiving temporary contracts with each other. Besides register data we use survey information to study effects on the number of hours worked, self-reported unemployment, hourly wages and the probability of getting an open-ended contract. The results of this analysis show no effect whatsoever of the contract length on wages, unemployment or hours worked. The zero-effect is precisely estimated, suggesting that the insignificant estimate is due to a negli-

¹ Being registered with PES may imply both unemployment and having another temporary job.

gible actual effect and not due to lack of variation in the contract length. However, we do find positive effects on the probability of having an open-ended contract 2–2.5 years after the start of the contract. Also, we find some indications of a positive effect on the probability of remaining with the same employer.

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

2 Background and data

2.1 Temporary jobs in Sweden²

Booth et al (2002) describes the strictness of Swedish employment protection as being about average by international standards, both for temporary employment and for 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 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 This section briefly describes the use of temporary contracts in Sweden in recent years. Unless stated otherwise, it is entirely based on Holmlund & Storrie (2002).

Holmlund & 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 480 days of subsidised leave from work after the child is born.³

Holmlund & 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.

In this paper we study temporary jobs generated by a Swedish subsidised “career-break” program that ran as a pilot in 12 Swedish municipalities during the period February 2002–December 2004. The program is instituted on a permanent and national basis from January 2005.⁴ The program implies that an employee is offered a sabbatical leave for a period of 3–12 months, if an unemployed person (registered with the PES as unemployed) acts as a substitute. The purpose of the program is twofold: (i) to provide an opportunity for an employee to take a career-break for upgrading of skills, recreation, child care or whatever the absentee chooses; (ii) to improve an unemployed person’s labour market position. Participation in the career-break program is conditional on the employers consent; however the unemployed substitute must be hired in consultation with the Public employment office. This paper focuses on how the

³ Of these, 60 days are earmarked for each of the parents, thus the longest leave spell for one individual is 420 days.

⁴ The “career break” program is administered by the PES.

replacement workers were affected by the temporary jobs. Skans & Lindqvist (2005) is an evaluation of how the career breaks affect the workers on leave. In short, wages reduce some for all participants, and the probability of retirement increases for elderly workers. Sickness absence and working hours are not affected by the career break.

From Fröberg et al (2003) and Lindqvist (2004) we know that most replacement contracts were held by females in the public sector, a feature that is shared with temporary employment in general. Besides the unemployed persons recruited as substitutes had in general a better position on the labour market in comparison to the average unemployed registered at the PES: the substitutes were younger, less likely to have a work-related disability, they had a higher level of education and on average shorter spells of registration with the PES (before entering the career-break program). About 50 % of the substitutes where picked by the employer among persons who at some point of time had been working at the working place where the temporary job was offered (36 % did actually work at the working place just when offered the contract). We discuss how this may affect the results in section 3.3.

The mean length of the leave spell and consequently the spell of the temporary job for the previously unemployed worker was 10 months and the median 12 months. *Figure 1* shows the distribution of lengths. 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. Our data from the PES include information on the replacement workers and all job-seekers in the 12 participating municipalities during 2002 and 2003.

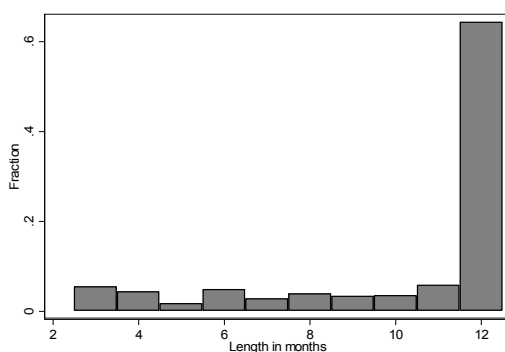


Figure 1 Lengths of the replacement contracts

Source: PES register data

2.2 The structure of the analysis

We are interested in whether a temporary employment contract improves the future labour market status for the person receiving it. Furthermore, we wish to investigate whether workers who get long temporary contracts do better than workers who get shorter contracts. We do the analysis in two steps. First, we look at the average effect of receiving a temporary contract compared to not receiving one, on those who receive one. This is the usual “average treatment effect on the treated” or ATT. We estimate it by matching the replacement workers with a comparison group. Second, we look at the effect of contract length by comparing the replacement workers with each other.

There are several reasons for dividing the analysis in two parts. To identify the ATT of the temporary contracts, we need an estimate of the counterfactual state of no contract. The register data include detailed background characteristics, which makes it possible to base the analysis on conditional independence assumption. Matching is thus an ideal method for estimating the ATT. It allows us to identify the effect fully non-parametrically without using comparison observations outside the common support. This is especially important in our case where the group of treated is substantially different from the average registered worker; they have a much stronger position (see Larsson & Skans, 2004).⁵

When analysing the effect of contract length, using a comparison group seems unnecessary. From the first part of the analysis we know whether the contracts *on average* were beneficial. Besides, we do not observe the contract length for the comparisons since they by definition not started as replacements.⁶ It would thus not be possible to match on contract length. Linear regression and probit models applied only on the replacement workers also allow us to test for various specifications with contract length as continuous and dichotomous variable. Moreover, we want to use the survey data that include interesting labour market outcome measures after the end of the contract. The survey data are available for workers who had a temporary contract only, and not for the comparison group.

⁵ Of course, it is possible to exclude observations outside the common support when using linear regression, as well.

⁶ Some of the comparisons may of course start on some kind of replacement contract later on. We do not want to exclude them from the analysis since that would imply conditioning on future events, potentially causing bias in the results.

2.3 The outcome of interest

Our empirical models study the effects on discrete outcomes. To be more specific, we will look at the time used in unemployment or as registered with PES during the second year after the contract start (months 13–24). We count the number of days in unemployment, or the number of days of registration, during the whole follow-up year, as well as during the quarters of that year.⁷ *Figure 2* illustrates. Consequently, the results will report the change of days due to the contract: the larger the decrease of days the better the effect of the contract.

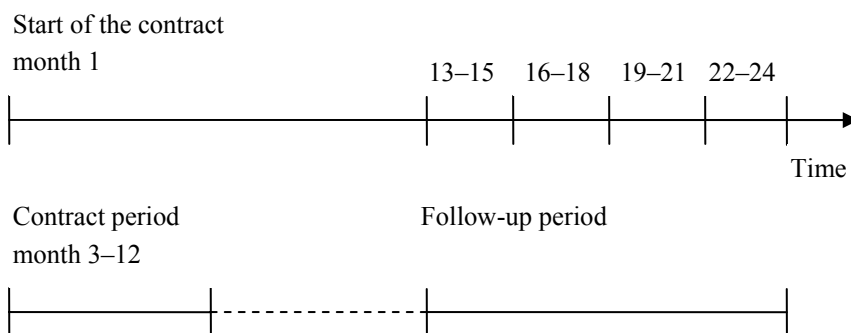


Figure 2 Illustration of how future labour market outcome is measured

In the first part of our analysis we are interested in whether time spent in a temporary contract was better for the person's future labour market outcome than time spent elsewhere, independent on how long the contract was. Thus, it seems reasonable to measure the outcomes relative to the start, rather than the end, of the contract. When analysing the contract length, other approaches could be considered. However, we still want to consider the alternative time-use of the replacement workers and thus we follow the workers from point of time they start their contract.

The data generating process and the institutional setting, however, imply some restrictions on the outcome of interest. The PES routines make it very difficult to judge the transitions *during* the contract since the data do not contain any specific category "Career-break replacement". The replacement workers

⁷ See e.g. Carling & Larsson (2005) for a similarly structured analysis.

were instead recorded into various categories, making it impossible to know whether transitions during the contract reflect actual changes or just changes in the recordings. Consequently, we do not consider what happens during the 12 first months after the contract start – the maximum duration of a contract.⁸

Second, we need a tool that is not sensitive to small changes in the contract length since the contracts were sometimes prolonged when the “regular” worker returned to work in order to have a short overlap with both workers. Counting the days in unemployment and registration as in *Figure 2* works well as it incorporates several months after the *ex ante* contract end.

Finally, workers on temporary contracts differ from the average unemployed in that they have frequent transitions between employment and unemployment. Thus, the time to the first exit from unemployment is not very informative in the long run; rather we are interested in the incidence of unemployment during a longer period.

In sum, the timing of recorded transitions is not sufficiently informative to use as an outcome measures. Therefore our empirical models study the effects on discrete outcomes, as illustrated in *Figure 2*.

2.4 Data

2.4.1 The register data

The register data are collected from the administrative register database at the Public Employment Service (PES). The data contain detailed information on unemployed individuals’ labour market history such as registration dates, job training activities, temporary jobs, and participation in active labour market programmes. Employed workers looking for a new job through the PES are registered in one of several possible categories; the exact category depends on the type of current employment.⁹ The database also contains individual characteristics such as gender, age and level of education. Thus, we can for each person observe the number of days in unemployment as well as the number of days in any other type of registration category at the PES since August 1991 until February 2005.

⁸ The data section will describe the procedure in more detail. Larsson & Skans (2004) analyzed what would have happened to the replacement workers during the participation time in the absence of a replacement contract. The results suggested substantial deadweight losses during the duration (in the order of 50 %).

⁹ Throughout this paper we disregard one on-the-job search category that mainly applies to workers in long-term contracts.

According to the regulations for the “career-break” replacement contracts, the replacement workers should be recruited among job seekers registered with the PES.¹⁰ Thus, all of the replacement workers should appear in the database prior to the contract start. In the PES 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 can thus either be recorded in one of several different categories for employed people or be deregistered from the PES. The choice between these alternatives is determined by the replacement worker in consultation with the PES officer and may change over time. This phenomenon creates a problem when identifying start- and stop-dates. However, the National Labour Market Board administrating the PES has documented these specific replacement contracts separately since August 2002, and we match this information to our data in order to get the start and end dates from then on.¹¹

Data coverage ends in February 2005 and since we wish to have a reasonably long follow-up period we restrict the analysis to replacement contracts starting up to February 2003. Thus, we study all the replacement contracts starting between August 2002 and February 2003, in total this amounts to 1,016 observations.¹² From this population we drop 115 individuals who were not registered immediately before they received the replacement contract because they cannot be matched with any of the potential comparison persons who all are registered with the PES. After imposing these restrictions the final sample consist of 901 replacement workers.

We use a comparison group selected from all individuals registered with the PES in the 12 Swedish municipalities covered by the career-break pilot.¹³

¹⁰ In theory, they should be registered in the category *unemployed*, however, in practice this has not been the case.

¹¹ In practice, it is the length and timing of the person on leave that is recorded together with a link to the replacement worker.

¹² This is the subset for which we know the start and stop dates: In 50 cases there were more than one replacement worker for a person on leave, in 8 cases the replacement worker replaced more than one person on leave and in 29 cases there were more than one leave-spell for a single person. Since this prevents the start and end dates from being inferred, the replacement workers connected to such cases are dropped. In addition, 11 individuals were dropped due to missing values in their start dates.

¹³ For the comparison group, we apply the same exclusions except for the exclusion of individuals who were not registered immediately before they received the replacement contract. Instead, we remove all individuals who were not registered at the first of each month. Furthermore, we

Every first date in a month (during August 2002–February 2003) we collect all individuals who are registered with the PES. That date is then a “fictional” start date of that comparison “unit”. All time-varying background and outcome variables are then determined relative to that date. Naturally, the same individual may be collected several times but with different values regarding background and outcomes. Thus, our stock of potential comparisons consists of 490,299 units based on 107,008 individuals.

2.4.2 The survey data

In February 2005 a telephone-administered questionnaire was directed to 942 of the 1,016 workers receiving a temporary contract during the period August 2002–March 2003;¹⁴ 538 responded the questionnaire, thus the response rate was 57 % of 942 (or 53 % of 1,016). The *Appendix* provides an analysis of response rates for different categories of workers. The main reason for non-responses was failure to contact the respondent, either because of incorrect telephone numbers or since the respondent was not accessible during the survey period.¹⁵ The non-response group is slightly overrepresented by younger individuals, males and non-Swedish citizens. This does not however seem to affect our results. Besides controlling for age, citizenship and level of education in our estimations we are able to use the register data outcome variables to check the sensitivity. The effect of the temporary contract based on the register data is not significantly different when estimated on the entire (register data) population and on the survey respondents.

The respondents were asked questions regarding their employment, working hours and wages. See the *Appendix* for descriptive statistics. The survey was linked to the register data from the Händel database. The responses, together with register information on the respondents’ labour market history is used as measures of outcome in section 4 to investigate to what extent contract length affects hours worked, wages and the probability of getting a permanent contract.

remove individuals who were registered in search categories that include zero replacement workers.

¹⁴ 74 individuals were excluded from the survey sample since they had refused to take part in an earlier survey of the career break program conducted by IFAU (see Lindqvist, 2004).

¹⁵ A more detailed description of reasons for non-responses is available upon request.

2.5 Further variable definitions and descriptive statistics

The replacement workers are far from a random sample of PES-registered individuals. They have less unemployment in their labour market histories and have spent more time as not registered. Furthermore, many of the workers have a short registration spell just prior to the replacement contract *but* considerable time spent registered in the past, suggesting frequent cycling between employment and unemployment.

Due to this phenomenon, we wish to characterize the unemployment histories of the individuals in a way that includes as rich information as possible on the mix of temporary employment, unemployment and time spent as not registered. We use the term “temporary employment” for individuals that are registered with the PES as temporary employed (any kind of contract), or are registered as being part-time unemployed, employed on an hourly basis or similar categories. As temporary employment we also include individuals in different forms of subsidised employment programs. By unemployed we mean “open unemployment” and participation in one of the many forms of training programs. “Not-registered” means either not registered at the PES, or registered as searching “on the job”.

We divide each individual’s labour market history into three parts: 0–3 months back, 4–12 months back and 13–48 months back. For each period we classify each individual in one of five classes:

- i) *Not registered* if the sum of unemployment and temporary employment is less than 10 % of the period.
- ii) *Unemployed* if more than 90 % of the period is spent as unemployed.
- iii) *Temporary employed* if more than 90 % of the period is spent in temporary employment.
- iv) *Mixed registration* if none of the above but the sum of unemployment and temporary employment is more than 90 % of the period.
- v) *Other* if none of the above, i e if the period is spent in a mix between unemployment, temporary employment and without registration.

The aim of this procedure is to generate variables that capture the employment-unemployment cycling of the group of replacement workers we are study-

ing. We also match on the category the individual is registered within one day before the contract start.

We use some additional information on the labour market history of the individuals. The first is benefits, where we use three categories; regular unemployment insurance (related to previous income), the Alfa fund (a lower fixed amount), and no compensation.¹⁶ We also use an indicator variable for whether the person is searching outside of the local labour market. Finally we group the registration categories of the individuals (as recorded one day before the fixed term contract started) into five groups: openly unemployed, labour market training, youth program, subsidized temporary employment, and non-subsidized temporary employment.

Table 1 below shows descriptive statistics for the replacement workers and the full set of potential comparisons. It should be obvious from the table that the sample of replacement workers is far from random. In general they appear to have a much stronger position at the labour market than the average unemployed.

¹⁶ To be covered by the income-related unemployment insurance, membership of an unemployment insurance fund is necessary. Those who are not members of an unemployment insurance fund, or has not been members long enough can receive a basic allowance from the Alfa unemployment insurance fund. However, the Alfa fund has also *members* who receive full compensation. Using information on *membership* in the different funds as an indicator for the type of compensation thus implies some measurement error.

Table 1 Descriptive statistics for labour market history

Variable	Replacements	All potential comparisons
Benefits		
Regular UI	0.73	0.72
Alfa fund	0.08	0.09
No benefits	0.19	0.21
LM history: 1–3 months		
Not registered	0.10	0.04
Unemployed	0.15	0.36
Temporary employed	0.23	0.29
Mixed registration	0.11	0.10
Other	0.41	0.21
LM history: 4–12 months		
Not registered	0.41	0.19
Unemployed	0.08	0.16
Temporary employed	0.08	0.18
Mixed registration	0.08	0.13
Other	0.35	0.34
LM history: 1–4 years		
Not registered	0.36	0.26
Unemployed	0.00	0.02
Temporary employed	0.01	0.07
Mixed registration	0.06	0.13
Other	0.57	0.52
Status in the PES register one day before contract start		
Openly unemployed	0.43	0.48
Labour market training	0.02	0.07
Youth program	0.00	0.01
Non-subsidised temporary employment	0.52	0.29
Subsidized temporary employment	0.03	0.15
Searching interlocally	0.08	0.12
No of observations (no of individuals)	901	482,705 (104,860)

Note: Due to the sampling procedure, described in the data section above, each comparison person appears in the comparison group on average 4.6 times.

In addition to the variables describing the unemployment history we look at basic individual characteristics (age, gender, citizenship, education) and use three seasonal dummies. These variables are described in *Table 2* below.

Table 2 Descriptive statistics – background variables

Variable	Replacements	All potential comparisons
Age at start date		
Max 24 years	0.25	0.17
25 – 31 years	0.25	0.21
32 – 40 years	0.25	0.24
Min 41 years	0.25	0.38
Male	0.32	0.50
Citizenship		
Swedish	0.91	0.85
Nordic	0.02	0.02
Non-Nordic	0.07	0.13
Education		
Compulsory or less	0.13	0.27
High school	0.52	0.47
University	0.35	0.26
Season		
August–September 2002	0.44	0.30
October–December 2002	0.27	0.41
January–February 2003	0.29	0.28
No of observations (no of individuals)	901	482,705 (104,860)

Note: Due to the sampling procedure, described in the data section above, each comparison person appears in the comparison group on average 4.6 times.

We create two outcome variables: days in unemployment and days in registration. Registration includes days in unemployment and days in any kind of temporary employment, both subsidized and non-subsidized. The reason for measuring outcome in two ways is that we are interested both in whether the fixed term contracts reduce unemployment and whether it facilitates a move away from registration altogether. Our underlying assumption is that days spent as not registered are better than registration in one of the temporary employ-

ment categories. Each outcome variable is measured during a 3 months period, the first period being 13–15 months after the start of the contract and the last being 22–24 months after.

3 Do temporary jobs reduce the risk of future unemployment?

3.1 Identification

We are interested in whether the temporary employment contract improves the individual's future labour market outcome. According to the evaluation terminology, we want to identify and estimate the average effect of treatment on the treated (ATT),

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

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.

To be able to identify the *average* treatment effect on the treated, we need a valid estimate 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 are able to identify the counterfactual outcome. Formally this assumption can be stated by conditional independence

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

where \amalg is the symbol of independence and \mathcal{X} denotes the set of covariates for which the average treatment effect on the treated 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 treatment. Moreover, in order for the treatment effect to be identified, the probability of treatment must be strictly less than one:

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

When these two assumptions¹⁷ are fulfilled the counterfactual outcome, $E(Y^0 | T = 1, X = x)$, 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: $E(Y | T = 0, X = x)$.

Since the potential comparison group consists of nearly 500,000 observations, we can implement the matching technique using exact matching. This allows us to interact all the different variables capturing the labour market history. Our matching procedure results in one-to-many comparisons, so that the outcome of each replacement worker is compared to the average of all comparison persons that have the same X -values. Formally, the average treatment effect on the treated is calculated according to:¹⁸

$$(4) \quad \hat{\theta}(T - E - T) = \sum_k \left[\frac{n_{1k}}{\sum_k n_{1k}} \right] \left[\sum_{i \in k \cap \{T_i=1\}} \left(\frac{Y_{1i}}{n_{1k}} \right) - \sum_{i \in k \cap \{T_j=1\}} \left(\frac{Y_{0j}}{n_{0k}} \right) \right],$$

where n refers to the number of individuals and the index k refers to the “cell”, and the index 1 (0) refer to the treated (comparisons). Indexes i and j are for individuals and Y denotes the outcome.

3.1.1 Does the conditional independence assumption hold?

The CIA assumption is an identifying assumption that cannot be tested. However, the standard result from the evaluation literature starting with Ashenfelter (1978) is that the main joint determinants of program participation and out-

¹⁷ Moreover, to make causal analysis possible, the stable-unit-treatment-value assumption (SUTVA) must be satisfied for all individuals in the population. The SUTVA has several consequences, the most important of which in our context is that the potential outcomes for an individual are independent of the treatment status of other individuals in the population. Thus, cross effects and general equilibrium effects are excluded.

¹⁸ Borland & Tseng (2004).

comes, and thus the factor that are most important to include in the matching procedure, is the labour market history of the participants.¹⁹

To the extent that we can generalise these results to our setting we should be able to trust the results. Not only do we have a lot of information on the labour market history (see the previous section for details), we are also using a completely interacted model: Where the more commonly applied propensity score matching method balances the distributions of observed characteristics, exact matching compares every treated worker to other workers with *exactly* the same observed labour market history. We divide the labour market history of each person into three periods, each of which we divide into five groups, resulting in 625 possible combinations. We also have three UI-compensation classes, five registration categories and a dummy for searching “interlocally”. Given that the model is completely interacted, this results in 3,750 possible labour market histories. In addition, we also have the human capital variables (72 combinations) and three seasonal dummies. In total this means that we characterise each replacement worker in one of 810,000 possible cells and compare their outcomes to other unemployed within the same cell. Using this strategy we find matches for 839 replacements, corresponding to 93 % of the sample.²⁰

3.2 Results

We use two general outcome measures: number of days as registered with PES and number of unemployment days. Recall that being registered with the PES may imply that the person is openly unemployed, participating in a labour market program, or even in a temporary employment (subsidised or non-subsidised) and searching for a new job. Here, the outcome variable ‘unemployed’ contains open unemployment and participation in labour market training. The outcome variable ‘registration’ includes all registration categories.²¹ *Table 3* shows the results.

¹⁹ Examples of more recent studies that all point to pre-training earnings as one of the most essential factors to be controlled for in a labor market program evaluation are Hotz et al (1999); Dehejia & Wahba (1999); Heckman et al (1998); Larsson (2003). See also Forslund et al (2004) who show that matching based on labour market history variables and instrumental variables (based on regional budget differences) provide similar results when studying the effects of subsidised employment for long-term unemployed in Sweden.

²⁰ This sub-sample seems to be quite randomly selected from the full sample of replacement workers. More detailed results can be obtained from the authors upon request.

²¹ The only exception being the “on-the-job search” category that mainly applies to those with long term contracts wishing to change jobs.

Table 3 The average treatment effect on the treated (ATT)

Time since start	Replacements	Comparisons	ATT	ATT (%)
Unemployment				
13–15	15.8	20.5	-4.65*** (1.09)	-23
16–18	13.1	18.9	-5.83*** (1.01)	-31
19–21	12.3	18.6	-6.25*** (1.01)	-34
22–24	13.0	19.2	-6.20*** (1.02)	-32
Sum (13–24)	54.3	77.2	-22.9*** (2.06)	-30
Registration				
13–15	31.0	40.7	-9.77*** (1.34)	-24
16–18	28.9	37.9	-9.02*** (1.30)	-24
19–21	27.6	37.7	-10.09*** (1.33)	-27
22–24	27.4	38.6	-11.22*** (1.30)	-29
Sum (13–24)	114.8	154.9	-40.1*** (2.64)	-26
	(1)	(2)	(3)	(4)

Note: Columns (1) and (2) measure the average outcomes for replacements and matched comparisons. Comparisons are selected according to exact matching based on the variables displayed in *Tables 1* and *2*. ATT is the difference in outcome, Column (4) is the difference in percent of the average comparison outcome. Standard errors (in parenthesis) are calculated by standard procedures assuming independent observations. ***Significant at 1 % level. **Significant at 5 % level. *Significant at 10 % level.

Recall that all negative figures in *Table 3* refer to reduction in unemployment (or registration) and thus should be taken as “positive” results for the replacement contract. The results show that the average number of days in unemployment is significantly²² lower among the replacement workers than in the matched comparison group throughout the time period. This suggests that a replacement contract leads to less unemployment after the contract has ended. Furthermore, the effect on average number of unemployed days seems constant over the time period of twelve months that we are able to observe.

When we add all types of registration within the PES into the outcome measure, the treatment effect remains qualitatively the same: having a replacement improves the worker’s future labour market situation. Once again the time pattern does not suggest that the effect wears off with time (during the follow up period). It can be noted that the comparison group does not appear to improve its labour market status during the follow-up window.

In sum, the overall effect of the temporary contracts as described by *Table 3* is a reduction of 22 (40) days of unemployment (registration) during the year following the replacement contract.²³

3.3 Who benefits from temporary contracts?

We also wish to investigate for whom the contracts work best. Especially we are interested in whether previous labour market attachment plays a role for the effect. In other words, is the effect stronger or weaker for replacement workers who had a relatively strong position on the labour market before they received the contract, compared to those with a relatively weak position? This is particularly interesting since Larsson & Skans (2004) showed that the deadweight losses during the duration of the replacement contract were substantial for those with a stronger labour market position.

²² The standard errors are calculated assuming independent observations in the comparison group.

²³ Most of these effects stem from the reduced probability of having any registration or unemployment days: Approximately 63% of the replacement workers compared to 51% of the comparison group are not unemployed at all during the follow-up year. For not being registered at all, the figures are 48% of the replacement workers and 33% of the comparison persons.

Table 4 Heterogeneous ATT effects

Group	Effects on unemployment 13–24		Effects on registration 13–24	
	ATT	ATT (%)	ATT	ATT (%)
<i>Gender: Men</i>	-18.61	-24.1	-40.11	-25.6
Women	-24.88	-26.9	-40.09	-26.0
Difference	6.27	2.8	-0.02	0.4
(se)	(4.87)		(5.74)	
<i>Age: Below 32 yrs</i>	-25.24	-39.4	-39.89	-32.0
32 yrs and over	-20.65	-22.9	-40.30	-21.8
Difference	-4.59	-16.5	0.41	-10.2
(se)	(4.12)		(5.27)	
<i>Citizenship: Swedish</i>	-22.06	-39.0	-40.85	-26.5
Non-Nordic	-22.18	-22.3	-15.47	-8.9
Difference	0.12	-16.7	-25.38***	-17.6
(se)	(5.25)		(6.06)	
<i>Education: Compulsory or less</i>	-32.38	-32.6	-62.50	-32.6
University	-20.46	-27.7	-41.51	-30.3
Difference	-11.92	-4.9	-20.99**	-2.3
(se)	(8.09)		(9.90)	
<i>Status before start:</i>				
Unemployed	-37.25	-36.3	-43.69	-28.3
Non-subsidized work	-9.20	-18.0	-33.88	-23.0
Difference	-28.05***	-18.3	-9.82**	-5.3
(se)	(4.08)		(4.83)	
<i>LM history: 0– 3 months:</i>				
Not registered	-33.98	-53.7	-34.82	-37.4
Full registration ^a	-15.19	-18.4	-28.84	-15.5
Difference	-18.79***	-35.3	-5.97	-21.9
(se)	(6.97)		(9.23)	
<i>4 months–4 years^{aa}:</i>				
Not registered	-28.45	-43.1	-46.30	-37.2
Full registration ^a	-16.80	-17.7	-28.02	-13.1
Difference	-11.65**	-25.4	-18.27***	-24.1
(se)	(5.50)		(6.87)	

Note: Standard errors (in parenthesis) are calculated by standard procedures assuming independent observations. ***Significant at 1 % level. **Significant at 5 % level. *Significant at 10 % level. ^a The category ‘full registration’ includes categories ‘unemployed’, ‘temporary employment’, and ‘mixed registration’, i.e. denotes individuals who have been registered with PES at least 90 % of the period. ^{aa} ‘LM history 3 months – 4 years’ combines information from the whole period so that the category ‘not registered’ (‘full registration’) includes individuals with no registration (full registration) during *either* 3–12 months *or* 1–4 years prior contract start.

Table 4 shows results on unemployment and registration when the sample is divided according to several characteristics. It should however be noted that the sample is split according to one dimension at a time and that these dimensions well may correlate with each other. We report the effects both in absolute terms (number of days) and in relative terms (percent).

We start by analysing heterogeneity according to demographic variables. There are no significant overall gender differences throughout the period. Somewhat surprisingly, however, we see that the effect is growing over time for women, and declining for men (although, the differences are not significant in any of the periods). The differences according to age and citizenship are not significant, but it should be noted that the sample of non-Swedish citizens is very small.

Turning to indicators of weakness or strength on the labour market, the results show a mixed picture. When measured in absolute terms, the effects seem to be larger for low-educated than for workers with university education, even if the difference is only significant when looking at registration. In relative terms, however, there is no difference between the educational groups. We also find a larger effects for those being unemployed before starting the temporary job than for those going from one temporary job to another. These results suggest that the effect is larger for workers with weak labour market status.

However, looking at the labour market history further back in time we find a different pattern: The effects are in this case largest for those without any registration. Thus, overall we find that the results are mixed and we are not able to draw any firm conclusion regarding whether temporary contracts works better for those with a strong or a weak labour market history.

In *Table 5* we use our survey data to separate people who have had previous contact with the workplace where they got their replacement contract. Replacement workers were asked whether they had previously worked for the same employer who offered them the replacement contract. Approximately half of the workers who responded to the survey answered ‘Yes’ (see the Appendix). The evidence in *Table 5* suggests that the effect was much larger for those that did not have a previous contact with the employer. Among those with a previous contact, the effects were similar and thus independent of whether he or she was working there at the time of receiving the replacement contract of-

fer.²⁴ Overall, this indicates that part of the overall effects may stem from the provision of a contact with a new employer.²⁵

Table 5 Previous contact with employer, effects 13–24 months after start of replacement contract

	Effects on unemployment	Effects on registration
Contact (N=260)	-19.31	-22.09
No contact (N=241)	-28.91	-49.02
Difference	-9.60*	-26.92***
(se)	(5.45)	(7.25)
Contact & job (N=163)	-17.64	-23.11
Contact & no job (N=97)	-22.10	-20.39
Difference	-4.45	2.72
(se)	(7.44)	(10.79)

Note: Contact means that the worker had been employed at the workplace of the replacement contract (self-reported) at some earlier point in time. Contact & job means that the person worked for that employer just before receiving the replacement contract.

4 What are the effects of having a longer contract?

Longer contracts are likely to provide more work experience. But they might also reduce the intensity at which the workers search for a permanent job. The net effect of the length on future labour market outcome is thus a priori unknown.

In this section we study how the length of a temporary employment contract affects labour market attachment using a number of alternative outcome measures: first we use outcomes corresponding to the previous section, unemployment-

²⁴ It is possible that some of the persons who worked for the employer just before receiving the career-break replacement contract would not have registered with the PES had they not been offered that contract. Recall that the rules for the career-break program required registration. To check whether this potential endogeneity of registration biases our results we have matched on the registration category *one week* before contract start, instead of the registration category *one day* before. The results are not significantly different.

²⁵ Support for this hypothesis is found in Johansson & Martinson (2000) that finds positive effects of a Swedish experiment that enhanced employer contacts among labour market training participants.

ment or registration with the PES. Second, we use survey information on hours of employment and self reported unemployment in the survey week. Third, we study the wage effect for those working during the survey week. Fourth, we look at the probability of working at the workplace of the replacement contract and of having an open-ended contract at the time of the survey.

The analysis is based on comparisons between workers receiving replacement contracts of various lengths. 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 individual characteristics X and the starting month of the contract M , as

$$(5) \quad Y_{it} = \beta_t X_i + \lambda_t L_i + \delta_t M_i + \varepsilon_{it},$$

We use both register and survey data to look at the effects of contract length. Data from the PES register – that are the same as in the previous section – measure the outcome t months after the start of the contract. The survey was conducted in February 2005, thus measuring the outcome approximately 2–2.5 years after the start of the temporary contract.²⁶ We include the month dummies in order to control for both the season and, when using the survey data, the time of the survey date.

We use regression models to correct for the same covariates as was used in the matched analysis in the previous section. We are unable to use matching since we no longer have access to the large potential comparison group when we compare workers on replacement contracts to each other. The regression model is less flexible than the matching model used previously as it is not completely interacted as the exact matching model. On the other hand, we are less concerned with selection in this part of the analysis since the persons on leave (with the consent of the employers) determine the length of the contract. Thus, we judge that managing selection into the length of contract requires a less elaborated model than selection into a contract at all. The identifying assumption underlying the causal interpretation in this section is that all (potential) factors affecting both contract length and the outcomes are captured by the linear functions of the observed characteristics.

²⁶ See the Appendix for a discussion of non-responses and descriptive statistics.

4.1 Do longer contracts reduce future unemployment more?

Table 6 shows the effects of contract-lengths on unemployment and registration. The same outcome measures as when estimating the average treatment effects on the treated of getting a temporary contract are used in these cases.²⁷ We present models estimated both with and without covariates as a support for our notion that contract length is largely uncorrelated with aspects of the replacement worker.²⁸ In all cases we control for the calendar month that the contract started. The table shows few significant effects of the length of the contract. The point estimates suggest that a one month longer contract affects unemployment and registration by less than one day during the entire one-year follow-up period. The insignificance of the estimates does thus not appear to be due to imprecision, but due to small actual effects.

Due to the small overall effects we find, studying the time pattern will be somewhat speculative. The few (marginally) significant estimates we find suggest that there are positive short run effects on *unemployment*, but negative short run effects on registration. This suggests that workers on shorter contracts found other forms of temporary contracts after terminating their replacement contracts, thus reducing unemployment but not registration.

²⁷ See section 2.5 for a further description of the outcome measures.

²⁸ We have also experimented with using the matching techniques provided in the previous section to study the length effect (grouping the length variable) and the results presented an identical picture.

Table 6 OLS-results on contract length

Follow-up period (months after contract start)	13–15	16–18	19–21	22–24	13–24
Effect on unemployment					
With covariates					
Estimate	0.609*	0.535*	0.005	-0.346	0.803
(se)	(0.351)	(0.323)	(0.315)	(0.324)	(1.089)
R ²	0.108	0.095	0.089	0.109	0.121
Adj R ²	0.072	0.058	0.052	0.072	0.086
Without covariates					
Estimate	0.550	0.545*	0.017	-0.389	0.723
(se)	(0.355)	(0.325)	(0.316)	(0.328)	(1.113)
R ²	0.019	0.016	0.010	0.017	0.014
Adj R ²	0.010	0.007	0.002	0.009	0.005
Effect on registration					
With covariates					
Estimate	-0.366	0.091	0.505	0.777	1.008
(se)	(0.438)	(0.429)	(0.514)	(0.517)	(1.737)
R ²	0.161	0.135	0.120	0.122	0.149
Adj R ²	0.127	0.100	0.084	0.086	0.114
Without covariates					
Estimate	-0.312	0.106	0.443	0.741	0.978
(se)	(0.456)	(0.442)	(0.524)	(0.528)	(1.802)
R ²	0.020	0.012	0.017	0.016	0.016
Adj R ²	0.011	0.004	0.008	0.007	0.008
Number of observations	901	901	901	901	901

Note: Dependent variable is days of unemployment/registration during the follow-up period. Estimates are for the effect of having one month longer contract. The models “with covariates” includes controls for all variables described in *Tables 1* and *2*. All models include a dummy for the start month. Standard errors are in parenthesis. ***Significant at 1 % level. **Significant at 5 % level. *Significant at 10 % level.

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. It is, however, possible that the outcome measures we are using are too crude. Thus, we proceed by studying the effects of contract length on wages, self-reported unemployment, and working hours in February 2005. To this end we use our survey data. In doing so we are restricted to a smaller sample of 485 individuals.²⁹ The respondents were asked to specify the number of hours they worked during the previous week.³⁰ If they worked less than 36 hours they were also asked whether they would have preferred to work more and if not so, why. Depending on the type of contract, people were asked to report their monthly or hourly wages. We convert the responses to monthly wages by multiplying hourly wages by 165 and by correcting monthly wages for part time work.

²⁹ We have estimated the register-effects (corresponding to *Table 6* above) for this sample, and all effects are insignificant in this sub-sample as well.

³⁰ If they could not give a specific number, they were asked to reply within specified intervals (0, 1-14, 15-24, 25-35, 36+). In the cases where respondents used the intervals we use the most frequent reply within the interval instead (1, 8, 20, 30 and 40).

Table 7 Effects on self-reported hours, unemployment and wages

	Hours of work	Unemployed	ln (Wages)
With covariates			
Estimate	0.398	-0.005	-0.000
(se)	(0.269)	(0.007)	(0.003)
R ²	0.141	–	0.384
Adj R ²	0.076	–	0.322
Without covariates			
Estimate	0.520*	-0.010	0.003
(se)	(0.267)	(0.007)	(0.004)
R ²	0.043	–	0.024
Adj R ²	0.027	–	0.002
Number of observations	485	485	363
Model	OLS	Probit	OLS

Note: Reported estimates from Probit models are marginal effects. Dependent variables are based on survey data from February 2005 (2–2.5 years after the start of the contract). Estimates are for the effect of having one month longer contract. The models “with covariates” includes controls for all variables described in *Tables 1* and *2*. All models include a dummy for the start month. Standard errors are in parenthesis. ***Significant at 1 % level. **Significant at 5 % level. *Significant at 10 % level.

Table 7 present the evidence. Point estimates suggest positive effects on hours worked during the previous week, but the estimate is insignificant with covariates and only significant at the 10 % level without covariates. The point estimates are relatively modest, a one month longer contracts providing half an hour longer average working hours about 2 years after the start of the contract. The estimated effect on self reported unemployment is also close to zero and insignificant. The estimated wage effect is not only insignificant, it is equal to zero to the third decimal after including the covariates. A 95 % confidence interval would at most allow for half a percent wage difference.

In *Table 8* we proceed by looking at the effects of remaining within the job and of having an open ended contract.³¹ We find a significant estimate on the probability of having an open-ended contract. Increasing the length of the replacement contract by one month is estimated to increase the probability of

³¹ These outcomes are self reported (i.e. the respondents has been asked whether they still work at the same workplace as during their temporary employment).

having an open ended contract by 2.5 % (about half of the sample does have an open-ended contract in February 2005). In order to understand the process generating these results we asked the respondents whether they still worked for the same employer as they did on the replacement contract; the fraction answering ‘Yes’ to this question was 57 % (see the Appendix). We estimated the effect of contract length on this outcome and the results are presented in the second column of *Table 8*. The point estimates suggest that those on longer contracts more frequently (1.4 % more) stayed on with the same employer. The estimate is however only significant at the 10 % level, and only with the covariates included, and should thus be interpreted with caution. When combining the two questions we find that the entire increase in probability of having an open-ended contract is within the same workplace as the replacement contract. There is no effect on the probability of having an open ended contract at another workplace.

Table 8 Contract and job effects

	Open ended contract	Same job	Open ended con- tract within same job	Open ended contract at other job
With covariates				
Estimate	0.024***	0.014*	0.027***	-0.001
(se)	(0.008)	(0.008)	(0.008)	(0.005)
Without covariates				
Estimate	0.024***	0.011	0.025***	-0.000
(se)	(0.008)	(0.007)	(0.008)	(0.004)
Number of observations	485	485	485	485
Model	Probit	Probit	Probit	Probit

Note: Reported estimates are marginal effects. Dependent variables are based on survey data from February 2005 (2–2.5 years after the start of the contract). Estimates are for the effect of having one month longer contract. The models “with covariates” includes controls for all variables described in *Tables 1* and *2*. All models include a dummy for the start month. Standard errors are in parenthesis. ***Significant at 1 % level. **Significant at 5 % level. *Significant at 10 % level.

5 Summary and discussion

This paper evaluates the effect of temporary, fixed-term, jobs as replacements for participants in a Swedish subsidised career-break program. These contracts are received mainly by workers with a history of cycling between unemployment and employment. This implies that, already prior to the contract, the replacement workers had a stronger position at the labour market than the average unemployed.

We compare the workers on replacement contracts with a matched comparison group. We use exact matching on a number of variables capturing labour market history and personal characteristics. The causal interpretation of the differences in the outcomes between replacement workers and their matched comparison group is primarily based on the notion that the labour market history is the main joint determinant of receiving the contract and future labour market performance.

Given our identifying assumption, the results clearly show that a replacement contract improves the worker’s future labour market status. Both unemployment and overall registration with the Public Employment Service are sig-

nificantly lower among the replacement workers. The effects could be considered large in relative terms (the reduction of unemployment days is in the order of 35 %).³² But counted in number of days the effect is in the order of 20 days of unemployment during a year, which perhaps is less impressive. The main issue here is that the comparison group also do fairly well, the reason being that workers receiving the replacement contracts are selected among workers with a relatively strong position at the labour market.

We also estimate heterogeneous treatment effects. We find no conclusive evidence on whether the effects are larger or smaller for workers with a relatively strong position on the labour market. The effects do however appear smaller for those with a previous contact with the employer of the replacement contract.

When separately studying whether a longer contract is better than a shorter contract we do not find any significant effects on unemployment or registration (at the PES). In this analysis we rely on regression models and the assumption that the length of the contract is uncorrelated with unobserved characteristics, a notion that gets some indirect support by the fact that the observed characteristics seems to play only a tiny role.

We also use survey data on self-reported hours of work and unemployment verifying that the length of the contract is unimportant for the future employment prospects. Furthermore, we study the effect on future wages and find no significant effect there either. The main reason for the insignificant estimates seems to be small actual effects rather than imprecision since all point estimates are tiny.

However, when using the survey data to look at the probability of having a regular open-ended contract we find strong positive effects. Furthermore, there are indications of an increased probability of staying on with the same employer. When separately identifying the effect of open-ended contracts with the own employer and with other employers we find that the entire effect comes from within the workplace of the replacement contract.

Overall the results show that temporary contracts do serve as stepping-stones for the unemployed, which is consistent with previous research. The fact that longer contracts do not appear to strengthen this effect suggests that it is the contact with the employer that is important. This notion is also supported

³² It is e.g considerably larger than the corresponding number in Forslund et al (2004) that study the effects of subsidised employment in Sweden.

by estimates being relatively larger for those without a previous contact with that very employer. The results are also consistent with reduced search effort counteracting the effects of tenure and experience when contracts are longer: If a longer contract both increases the probability of staying on and reduces the time spent on searching for other jobs, we could have a zero net effect on the overall labour market outcomes, but a positive effect within the given workplace, as is suggested by the results.

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Appendix

Table A1 shows background variables from the Händel database for all replacement workers who received a temporary contract before April 2003 (total population) together with the same description for the responses and non-response of the survey sample.

The non-response group is in some extent biased towards younger individuals, males and non-Swedish citizens. A possible explanation for this might be that younger persons to a greater extent exclusively rely on mobile phones.

Table A1 Responses to the survey

	Total population	Survey sample responses	Survey sample non-responses
Women	66.0 %	70.1 %	63.3 %
<i>Age</i>			
Mean age	35 years	35 years	34 years
Median age	33 years	34 years	32 Years
<30	39.4 %	35.9 %	43.7 %
30–49	48.6 %	51.6 %	47.3 %
≥50	12.0 %	12.5 %	9.1 %
<i>Education</i>			
Primary (< 10 years)	14.0 %	11.0 %	14.6 %
Upper secondary	56.1 %	50.8 %	55.3 %
Post gymnasium	29.9 %	38.1 %	30.2 %
<i>Citizenship</i>			
Nordic (Non Swedish)	1.7 %	1.3 %	2.7 %
Non –Nordic	5.5 %	4.7 %	7.8 %
Work related disability	3.4 %	2.4 %	3.4 %
Number of observations	2549	538	478
Response rate		57 %	

Table A2 Descriptive statistics for the survey data

	Mean	Standard deviation	Min	Max	N
Hours worked last week	24.67	17.98	0	60	485
Unemployed (self reported)	0.274	--	0	1	485
Monthly wage	9.814	0.215	8.854	10.915	363
Open ended contract	0.497	--	0	1	485
Working at same job	0.573	--	0	1	485
Previous contact with the employer:					
Worked for the employer some time previously*	0.516	--	0	1	535
Worked for the employer when of- fered replacement contract*	0.370	--	0	1	535

Note: *From a survey in the fall of 2003. See Lindqvist (2004) and Skans & Lindqvist (2005) for details about the survey questions.

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