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Do Fixed-Term Contracts Increase the Long-Term Employment Opportunities of the Unemployed?

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Non technical summary

One important argument in the political debate on dismissal protection is that firing costs reduce the re-employment probabilities of the unemployed, and especially for those with adverse signals such as long-term unemployment or low qualification. Since the mid-1980s it is relatively easy possible for employers to hire workers on fixed-term contracts (FTC) in order to avoid costs due to dismissals. Thus FTCs or temporary work in general may increase the employment opportunities of (long-) term unemployed which are harmed by the firing costs due to the employment protection for permanent workers. The rationale is simple: employers may be more willing to hire if they can fire easily.

The paper investigates whether (unsubsidised) fixed-term contracts (FTCs) are a means of integration or a so-called ‘stepping stone’ for the unemployed towards (permanent) employment relationships. This is done by analysing whether entering into an FTC improves the employment opportunities of an unemployed person in terms of the probability of subsequent permanent contracts and subsequent periods of employment and unemployment and periods out-of-labour-force.

The empirical analysis is based on propensity score matching methods, obtaining the effects of FTCs by comparing the future situation of (‘treated’) unemployed entering into FTCs after a particular duration of unemployment with a suitable control group of individuals who do not so (‘non-treated’). The data set used is the German Socio Economic Panel for West Germany for the period 1991 until 2001. First, it is discussed that there are at least two reasonable counterfactuals for individuals entering into FTC jobs after a certain number of months in unemployment. One counterfactual, most commonly applied in evaluation studies, is to compare a ‘world with FTCs’ with a ‘world without FTCs’ and define non-treated persons as unemployed who never (during the period covered by the data set) enter into FTCs. A second counterfactual which may be more in line with the idea of sequential job search (unemployed enter into FTCs after having failed to find a permanent job) is not to take up an FTC job up to a certain unemployment duration, but possibly in a later month. This implies a comparison of unemployed entering into FTCs in a certain month of unemployment with those unemployed who do not enter into FTCs up to the end of that month but possibly in a later month. Both definitions are analysed in the paper.

The hazard rate analysis shows that typical characteristics which have in other studies been found to prolong unemployment duration such as disabilities, being a foreigner, or being female and having children do not affect the FTC hazard rate, but have a negative effect on the permanent contract hazard rate. Thus FTCs may be “entry jobs” for unemployed with low employment chances. Entering into FTCs increases the future employment probability (including FTC and permanent contract jobs) and the probability of holding permanent jobs and decreases the probability of being out-of-labour-force. These findings are compatible with the hypothesis that FTCs may be stepping-stones towards permanent employment relationships. However, some results

of the hazard rate model and the matching estimator are in line with dual labour market theories. Having held an FTC in the past increases the probability of holding an FTC in the future. Furthermore, entering into an FTC does not reduce the risk of being unemployed in the long-run, as the effect vanishes 18 months after the transition to the FTC. Consequently, the positive employment effect is not accompanied by a lower probability of registered unemployment but by a reduced probability of being-out-of labour-force.

Do Fixed-Term Contracts Increase the Long-Term Employment Opportunities of the Unemployed?*

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Abstract

The paper investigates whether (unsubsidised) fixed-term contracts (FTCs) are a means of integration for the unemployed in the West German labour market. This is done by analysing whether entering into an FTC improves the employment opportunities of an unemployed person in terms of the probability of subsequent permanent contracts and subsequent periods of employment and unemployment. The empirical analysis is based on propensity score matching methods, obtaining the effects of FTCs by comparing the future situation of ('treated') unemployed entering into FTCs after a particular unemployment duration with a suitable control group of 'non-treated' individuals. In principal different counterfactual situations for treated persons entering into FTCs after a certain number of month of unemployment are reasonable. A first counterfactual is never to enter into an FTC. A second counterfactual is not to take up an FTC job in this month but possibly in a later month. These two possible counterfactuals imply different definitions for the group of non-treated individuals and impose different policy questions. Both definitions are analysed in the paper. The propensity score is estimated by a discrete hazard rate model, which seems to be an appropriate way of taking into account the potential endogenous effect of the unemployment duration on the selection into the type of contract. Further insights are gained by comparing the determinants of the transition to FTC and permanent contract jobs. There is some evidence that FTCs may serve as 'stepping stones' towards permanent employment for the unemployed. However, the hypothesis that FTCs lead to dual labour markets cannot be rejected.

Keywords: Fixed-term Contracts, Propensity Score Matching, Hazard Rate Model, Unemployment Duration, Stepping Stones, Dual Labour Markets

JEL Classification: C14, C41, J41, J64

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1 Introduction

One important argument in the political debate on dismissal protection is that firing costs reduce the re-employment probabilities of the unemployed, especially for those with adverse signals such as long-term unemployment or low qualification. Since the mid-1980s it is relatively easy for employers to hire workers on fixed-term contracts (FTC) in order to avoid dismissal costs.[1] Thus FTCs, or temporary work in general, may increase the employment opportunities of the (long-term) unemployed which are harmed by the firing costs due to employment protection for permanent workers. The rationale is simple: employers may be more willing to hire if they can fire easily.

However, objections are raised to this view. Temporary work may create a segmented labour market where the employment stability of permanent contract workers is raised by firms' using temporary workers as a kind of buffer against transitory changes in the business environment (see SAINT-PAUL, 1996). This may imply for specific groups of workers that they are 'trapped' in a cycle of recurrent periods of unemployment and temporary work. The temporary nature of the employment relationship becomes the cause of subsequent unemployment and temporary work (see TAUBMAN and WACHTER, 1986). This phenomenon may be fostered by inferior access to training and lower promotion prospects in temporary jobs (see BOOTH, FRANCESCONI and FRANK, 2002a). Thus the central issue is whether or not temporary work really increases the long-run employment prospects of the unemployed entering into temporary work in terms of future permanent employment relationships or employment in general. In other words: Should unemployed job searchers take up FTC jobs or should they keep on searching for permanent positions? Are FTC jobs 'stepping stones' towards permanent jobs?

There are only few studies currently available trying to identify the causal effects of FTC on future employment opportunities. On the one hand, there are studies evaluating the employment effects of subsidised temporary employment relationships which are promoted by public employment offices (see LECHNER et al. 2001; GERFIN, LECHNER and STEIGER, 2002). On the other hand, there are studies analysing the determinants of the duration of (unsubsidised[2]) temporary employment relationships and the determinants of the transition to other labour market states (see, for example, ALBA-RAMÌREZ, 1998; GÜELL and PETRONGOLO, 2003; PEETERS, 2000; GIESECKE and GROß, 2002). Although the latter studies may shed light on the determinats of "successful" temporary employment relationships (in terms of transition to permanent contract jobs or the duration of the temporary job), they are not informa-

1 FTCs define temporary employment relationships, which expire automatically without dismissal at the end of the agreed term. After the expiration of the contract the employment relationship is terminated, or the employer can decide to offer the worker a permanent position or, under certain circumstances, another FTC. FTCs have become important in many European Countries since the mid of 1980s (see for example the Special Issue of the Economic Journal in June 2002). A detailed description of the institutional background in Germany can be found in SCHÖMANN, ROGOWSKI and KRUPPE (1995) as well as BOOCKMANN and HAGEN (2001).

2 Here unsubsidised means that the temporary jobs are not active labour market programmes in the sense that they are sponsored by the public employment office.

tive with regard to the question whether it is beneficial for an unemployed person (in terms of the subsequent employment probability) to take up a temporary job.

To the best of my knowledge there are only two studies which analyse the causal effects of *unsubsidised* temporary jobs on future employment opportunities. The study by BRODATY, CRÉPON and FOUGÈRE (2001) compares the employment effects of youth employment programmes with FTCs in France by matching estimators. It turns out that FTC jobs are more effective than the employment programmes. There is, however, no comparison to unemployed who do not participate in any programme and who do not enter into FTCs, respectively. Second, VAN DEN BERG, HOLM and VAN OURS (2002) analyse a special kind of temporary job scheme in the Netherlands which is open for medical students searching for trainee positions to get work experience. The question is whether these temporary jobs help students to get a position as a trainee, i.e. whether the temporary job is a stepping-stone towards the trainee position compared to the situation in which the students do not take up the temporary job. In order to deal with the selection problem, all possible transitions between the three states (searching, temporary job and trainee position) are simultaneously estimated in a multivariate duration model. It is found that the temporary job helps to get a trainee position. The authors explain the positive effect by positive signals for potential employers.

This paper attempts to investigate the employment effects of FTCs for the unemployed by using matching methods which have been developed in the econometrics of evaluation of active labour market programes (see HECKMAN, LALONDE and SMITH, 1999 for a survey). Matching estimators are based on the "potential outcome approach" to causality (ROY, 1951; RUBIN, 1974), i.e. the hypothetical (and, therefore, unobservable) future outcome of a treated person in a "non-treated state" is estimated by a control group of non-treated persons. The methodological contribution of the paper is to estimate the propensity score by a discrete hazard rate model, which has been done only rarely so far.[3] It will be argued that this may have some advantages – at least in this application.

3 To the best of my knowledge there are only two papers available which use hazard rate models for the estimation of the propensity score (see BRODATY, CRÉPON and FOUGÈRE, 2001 and SIANESI, 2001a). Recently this approach has been formalized by FREDRIKSSON and JOHANSSON (2003).

2 Theoretical Considerations

Workers entering into FTC jobs face higher unemployment and income risks than workers entering into permanent contract jobs. Descriptive statistics on the duration of employment spells after the transition from unemployment to FTCs or permanent contracts, respectively, which are in line with this statement, can be found in Table A1 in the Appendix. Available empirical studies suggest, furthermore, that FTC workers do not receive wage premiums to compensate for these risks, but even contemporaneously lower wages (see, for example, HAGEN, 2002). These empirical results do not conflict with economic theory. For example, in the dual labour market model by REBITZER and TAYLOR (1991) wages for temporary workers may be lower, even if temporary and permanent workers are perfect substitutes. Furthermore, one can argue that temporary employment relationships are associated with a loss of returns to job seniority.[4]

Under which conditions do job searcher enter into FTC jobs? [5]

In order to answer this question one can argue in the framework of job search theory. The probability that an unemployed worker *i* leaves unemployment to a specific job after a certain unemployment duration *t* (hazard rate) $h_i(t)$ is equal to the probability of receiving a specific job offer[6] ξ_i times the probability that the offer is acceptable $\left(1-F_i\left(w_i^R(t)\right)\right)$, where $F_i\left(w_i^R(t)\right)$ is the cumulated wage offer distribution and $w_i^R(t)$ is the reservation wage (see MORTENSEN 1986: 862). How can job search theory be applied to the case that there are FTC and permanent contract job offers?

BURDETT and MORTENSEN (1980) augment the standard sequential job search model with a job-specific random dismissal probability, without taking FTCs and permanent contracts explicitly into account. If one assumes that failing to get the contract renewed is associated with an adverse signal for potential future employers (for example, if non-renewal due to unfavourable business development or due to bad worker's performance is not distinguishable) then the reservation wage is increasing with the dismissal probability.[7] Therefore, the

4 A result often found in wage regressions is that job seniority has a significantly positive effect on wages. This is interpreted as accumulation of firm specific human capital during employment. Based on empirical evidence PISSARIDES (1994: 458) concludes: "*Thus although quitters generally benefit from a job change, those who are separated because of an exogenous shock and find a new job after an unemployment spell generally suffer.*" BOOTH, FRANCESCONI and FRANK (2002a) find that men having had an FTC employment spell ten years earlier suffer a 5% wage penalty compared to workers who have always worked on a permanent contract in the U.K. For women there is no comparable long-term wage penalty.

5 Firms' reasons for employing temporary workers are analysed in BOOCKMANN and HAGEN (2001).

6 This arrival rate of job offers in standard search models can be decomposed further into the flow of vacancies times the probability that the worker becomes aware of the vacancy times the probability that the worker actually is offered the job.

7 Burdett and Mortensen's general proposition is that the reservation wage is increasing in the dismissal probability, if the return to search after being dismissed is less than the expected return to search before a job was found.

reservation wage with regard to FTC jobs is ceteris paribus higher than the reservation wage with regard to permanent contract jobs (see GROOT, 1990; BOVER and GÓMEZ, 2003). The reservation wage with regard to FTC jobs decreases, however, with the expected probability that the contract is transformed into a permanent one by the employer. Nevertheless, the acceptability of FTC job offers is ceteris paribus lower than the acceptability of permanent contract job offers, given that the latter are more stable.

There are various reasons to assume the reservation wage to be decreasing in unemployment duration, for example, due to ageing within a finite time horizon model (see FRANZ, 2003: 213). Therefore, the probability that an FTC job is acceptable increases with unemployment duration. There may be further reasons for job searchers to reduce their reservation wages and accept FTC job offers. They may, for example, have to meet temporary declines in family income, particularly when other family members have also been dismissed.

Unemployment compensation is interpreted within the job search framework as a reduction of the opportunity costs of unemployment (see MORTENSEN, 1986). By increasing the reservation wage it reduces the acceptability of job offers. Given the assumptions about the reservation wages with regard to FTCs and permanent contracts, the probability of accepting an FTC job offer is reduced more than the probability of accepting a permanent contract job offer (see BOVER and GÓMEZ, 2003). In other words: Unemployed without unemployment compensation are more willing to accept FTCs, given a certain probability of permanent contract job offers, than unemployed receiving unemployment compensation.[8] This result can also be derived, outside the framework of search theory, from the model by VAN DE KLUNDERT (1990). In the model, unemployment results from the assumption that queuing for a ‘primary’ job is preferred to a ‘secondary’ job if the utility derived from unemployment compensation, combined with the status of searching for a proper, that is, primary job, exceeds the utility derived from a secondary job. Besides the effects of subjective factors such as “status in the labour market” the model highlights the effects of unemployment compensation: If the amount is relatively high and depends on the previous wage rate, unemployed persons may prefer to wait for a suitable primary job. Hence, previous high-wage, “high status”, primary (permanent) jobs increase unemployment duration and decrease the probability of entering into low-wage, “low status”, secondary (temporary) jobs. Since in particular entrants into the labour market (younger workers or women after maternity leave) are often not entitled to unemployment compensation and have no determined idea about their “labour market status”, they are more likely to enter into FTCs. Unemployed who have previously been employed under a permanent contract will hesitate to accept an FTC job offer. This may lead to various types of state dependence.

Additional explanations are possible if one assumes incomplete information with respect to job searchers’ characteristics. For example, the searchers’ abilities are only incompletely observable for potential employers.[9] Temporary contracts may be a kind of prolonged probationary period which allows firms to obtain information that is unavailable before hiring and

8 According to this argumentation, unemployment compensation could be interpreted as an subsidy for the search for ‘good’ (permanent) jobs.

9 SPENCE (1973) formulated ‘hiring as investment under uncertainty’.

that serves as a check on the quality of the match between worker and job (*screening*).[10] Obviously, screening becomes even more important if institutions (dismissal protection) generate additional firing-costs.

A somewhat surprising result is generated by the model of LOH (1994), which is based on self-selection due to incomplete information and probationary periods. Loh's model predicts that in a competitive market setting, workers with greater ability – given other observable characteristics such as age, qualification and gender – migrate to firms offering jobs with probationary period, and those with lower ability migrate to firms offering jobs with no probationary period, since the former face lower risks of losing their jobs. Lower wages during the probationary period are compensated disproportionately after the probationary period. Again, if one interprets FTCs (at the start of a career path within a firm) as a kind of probationary period, the model predicts that, given the observable characteristics, high-ability job searchers enter into FTCs whereas low-ability job searchers enter into permanent contracts or keep on searching for permanent jobs, respectively.

If incomplete information plays an important role, job searchers' employment histories may serve as signals. References from previous employers and the reputation of previous employers may reveal information on the unobservable characteristics of the worker. If the previous employment history involves adverse signals, employers with permanent contract vacancies will hesitate to offer these job-searchers permanent jobs (see PEETERS, 1999). If, for example, unemployment duration is an adverse signal[11] then, within the job search framework, the offer rate for permanent contract jobs may decrease relative to the offer rate for FTC jobs with increasing unemployment duration. Given that the *relative* FTC job offer rate and the acceptability of FTC offers increase with unemployment duration, the duration dependence of the FTC hazard rate should be empirically more positive than the duration dependence of the permanent contract hazard rate (see GROOT, 1990).[12]

Dual labour market theories predict that the temporary nature of an employment relationship is the cause of subsequent unemployment and temporary jobs (see TAUBMANN and WACHTER, 1986). Thus, interpreting FTC jobs as 'secondary' and permanent contract jobs as 'primary' implies that having been employed with an FTC in the past may be an adverse signal for future employers, at least if the jobs were associated with unfavourable attributes.[13] This statement is also compatible with theoretical models on signalling effects (see MA and WEISS, 1993; MCCORMICK, 1990). Hence, workers who previously held an FTC job, have a higher probability of re-entering into an FTC job, since they receive fewer permanent contract job offers.

10 This argument is in line with the concept of matches as "experience goods" (see JOVANOVIC, 1979: 973): "*...the only way to determine the quality of a specific match is to form the match and 'experience it'.*"

11 LOCKWOOD (1991) shows in a theoretical model that when it is costly for employers to test workers they may use unemployment duration as a signal on which the employment decision is based.

12 If the individual hazard rate depends (after controlling for observed and unobserved heterogeneity) on unemployment duration then this is called (true) duration dependence. Negative (positive) duration dependence means that the hazard rate decreases (increases) with unemployment duration.

13 *"...secondary employment may be regarded as a kind of stigma that bars access to the primary sector.*" (MCDONALD and SOLOW, 1985: 1124).

A further explanation besides dual labour market theories is that the drawback of the temporary nature of the jobs is compensated by other features of the jobs which correspond to the job searchers' preferences. For example, flexible schedules or part-time jobs in order to meet family, school or other non-work responsibilities may be only available with FTCs or there are scarce permanent contract job offers with these features (in the local labour market concerned), respectively. This would imply that women with children enter into FTCs with a higher probability.

So far, the possibility of on-the-job search has been neglected. It is, however, likely that FTCs promote on-the-job search in comparison to permanent contract jobs, since rational workers will anticipate the higher risk of job losses (see BOERI, 1999). As FTC workers would, therefore, start earlier searching for a (permanent contract) job, FTCs may be interpreted as notice period (see BOERI, 1999; SWAIM and PODGURSKY, 1990). If one further assumes that FTCs increase the arrival rate of job offers (due to networking etc., see next subsection) and/or improves the wage offer distribution by enhancing human capital (see next subsection), entering into an FTC job may be an optimal search strategy.[14] On-the-job search may also render re-entering into FTCs after a previous FTC and a subsequent unemployment spell as an optimal strategy. Whether entering into an FTC job is a rationale strategy for job searchers depends on whether FTCs really improve the parameters mentioned above.

Why should FTCs be stepping stones towards permanent positions?

As argued by BOERI (1999) within a matching model with on-the-job search, temporary jobs may be interpreted as intermediate and transitory labour market status between employment and unemployment. If so, the question arises, why and under which conditions FTCs may be stepping stones towards permanent positions, that is, increase the long-term employment opportunities of those entering into FTCs.

First, during FTC jobs there will be more investments in (general and specific) *human capital* (in comparison to the situation in which the person had stayed unemployed), even if there is no formal training. This may raise the employment opportunities at the same or other employers. The latter may be explained by an induced shift in the wage offer distribution. However, firms will invest less in FTC workers than in permanent contract workers, since they recognize the shorter expected job tenure (see BOOTH, FRANCESCONI and FRANK, 2002b). Therefore, the opposite effect may also be possible: If the unemployed person had not accepted the FTC job offer in a certain period, she or he might have got a permanent contract job offer in the next period with better training opportunities and career prospects.

As mentioned above, FTCs may be used as a prolonged probationary period in order to overcome the problem of asymmetric information. Thus FTCs may serve as a *screening device*. This may help unemployed persons with adverse signals, who would otherwise get permanent contract job offers only with a low probability (see PEETERS, 1999). After the

14 The argument is based on HECKMAN, LALONDE and SMITH (1999), who present a model which interprets public sponsored labour market training as an optimal form of job search.

expiration of the FTC and after sufficient information on the worker's productivity is collected, the worker may get a permanent contract job offer from the same employer. This may be especially true if FTCs induce a sorting mechanism like in the model of LOH (1994) described above. Given identical observable characteristics, workers with (unobservable) 'high ability' select themselves into FTCs and get their contract transformed into a permanent contract with higher wages afterwards.

As mentioned above, if FTC jobs are associated with reduced employment stability, rational workers will already anticipate the higher risk of job losses and will, therefore, start searching for a (permanent contract) job earlier. In other words: temporary jobs may promote *on-the-job search* (see BOERI, 1999). So it seems to be plausible to state that the job search intensity may not be lower than during unemployment and strictly higher than in permanent contract jobs. If, however, search intensity is lower than during unemployment, FTCs may also decrease the probability of permanent contract job offers compared to unemployment. Furthermore, FTC workers may be in the position to enlarge their social network within the firm or even the industry in which they are employed (see VAN DEN BERG, HOLM and VAN OURS, 2002). This may increase the workers' knowledge of (future) vacancies and may again help other employers to collect (otherwise unobserved) information on the workers' productivity.

For on-the-job-searchers an FTC job may also be a *positive signal* to *other* employers, again, in comparison to the situation in which the person had stayed unemployed and thus had possibly been affected by negative 'stigma effects' due to unemployment. However, in order to be a credible positive signal, temporary jobs must be more costly to be found (in broad terms of search costs) for low ability workers (see GERFIN, LECHNER and STEIGER, 2002). The harder it is to get a temporary job the better is the signal for potential future employers. But again, entering into an FTC job may have the opposite effect, that is, it may also serve as a signal that the person had not received any offers for permanent contract jobs. Hence, temporary jobs may be 'stigmatized' (see MA and WEISS, 1993). Given that FTCs are associated with a negative wage differential it may be an adverse signal especially for highly qualified workers (see MCCORMICK, 1990).

3 Estimation Methods

What effect do FTCs have on employment opportunities of the unemployed? This question can be restated: How do the employment opportunities of unemployed persons change due to the fact they enter into FTCs instead of keeping on searching? In order to answer this question, one can apply methods which are used for the evaluation of active labour market policy (ALMP).[15]

First of all, a careful definition of the 'policy measure' or 'intervention' that should be actually evaluated is necessary. In other words: what is the '*treatment'*?[16] In the following "*treatment*" is defined as *transition from unemployment in an FTC job.* Further attributes of the FTC job, such as the duration of the contract, the wage, or the working conditions, are not taken into account, i.e. many very heterogeneous jobs are pooled together into one 'treatment'.[17]

3.1 The Evaluation Problem and its Solution in General

Evaluation problem and the parameter of interest

What is the causal effect of a treatment 1 (take up an FTC), relative to another treatment 0 or non-treatment, respectively (stay unemployed), on an outcome variable (future employment status) Y?

Let Y^1 be the outcome (future employment probability in a permanent contract) that would result if the individual was exposed to treatment 1 (FTC) and Y^0 the outcome that would result if the same individual received no treatment (stayed unemployed).[18] $C \in \{0,1\}$ is a dummy variable indicating if the treatment is actually received (C=1).

For an individual *i*, the actually observed employment probability is $y_i = y_i^0 + c_i\left(y_i^1 - y_i^0\right)$. However, the *individual* causal effect $y_i^1 - y_i^0$ cannot be estimated, since an individual can never be observed in two different states $\left(y_i^1, y_i^0\right)$ at the same point in time. To put it another

15 For a survey see HECKMAN, LALONDE and SMITH (1999); BLUNDELL and COSTAS DIAS (2000); ANGRIST and KRUEGER (1999).

16 Following the literature, the terms "treatment" and "participation" are used interchangeably throughout this paper.

17 Note, however, that focusing on previously unemployed individuals reduces the heterogeneity in comparison to other studies dealing with the effects of FTCs. Furthermore, also in studies which evaluate different labour market programmes it is usual to pool at least some measures, since it is, for example, in case of training programmes impossible to interpret every type and topic of training as a separate 'treatment'. Within an extension of the used methodological framework it would be possible to differentiate between different types of FTC jobs, for example between jobs with long and short FTCs or jobs with low or high skills requirement (see LECHNER, 2001b for the foundation of the so-called 'multiple-treatments' approach). The scale of the dataset is, however, too small for the identification of multiple treatments.

18 In the following, upper case letters denote random variables and lower case letters denote specific values of those variables. Furthermore, the subscript for the time dimension is omitted in order to keep notation simple.

way, the counterfactuals $\left(y_i^1, c_i = 0\right)$ as well as $\left(y_i^0, c_i = 1\right)$ are not observable. While estimation of the causal effect for an individual is never possible, it is possible for the mean (or other quantities) in samples of the population (see LECHNER, 1999).

The parameter of interest in most evaluation studies is the *average effect of the treatment on the treated*,

$$TT = E\left(Y^1 - Y^0 \middle| C = 1\right) = E\left(Y^1 \middle| C = 1\right) - E\left(Y^0 \middle| C = 1\right), \tag{1}$$

which is the average effect for those who actually receive the treatment. In the application in this paper, the TT defined in equation (1) measures the change in the future employment prospects of unemployed entering into FTCs which is caused by the fact that they actually entered into FTCs (C=1). The last term in (1) describes the hypothetical average employment probability *if the FTC workers had stayed unemployed.* Of course, this term is not observable and has to be estimated using a control group of unemployed workers. However, the average future employment probability of unemployed workers is typically not suitable since unemployed entering into FTCs and unemployed who do not enter into FTCs differ in characteristics which affect the future employment probability,

$$E\left(Y^0 \middle| C = 1\right) \neq E\left(Y^0 \middle| C = 0\right). \tag{2}$$

Equation (2) states that using the future employment probability of unemployed individuals as an estimate for the hypothetical situation in which an FTC worker stayed unemployed is in general not valid, since both groups differ due to observable and unobservable characteristics giving rise to a selection bias: the workers entering into FTCs are not a random sample of the population, but they may select themselves or may be selected on the basis of characteristics, which also influence their outcome (i.e. their future employment prospects).[19]

Statistical matching

Let X be a vector of variables that are unaffected by the treatment, such as gender, age and qualification. The statistical matching estimator may solve the problem of selection bias (due to differences in observable characteristics) by imposing the *Conditional Independence Assumption* (*CIA*)

$$Y^0 \perp C \middle| X \tag{3}$$

where $\perp$ denotes independence.[20] The assumption states that the outcomes of the non-treated individuals are independent of the participation status C, once one controls for observable variables X (see BLUNDELL and COSTAS DIAS, 2000).

19 Using $E\left(Y_i^0 \middle| C_i = 0\right)$ as an estimate for $E\left(Y_i^0 \middle| C_i = 1\right)$ is occasionally termed “naïve control group”.

20 The CIA is also termed ‘ignorability of treatment’ or ‘selection on observables’. Basic statistical matching estimators solve the problem of selection bias if and only if the selection bias is completely determined by selection on the observable variables X. So if one applies the method of matching one has either to assume

The *CIA* justifies the use of matched non-treated persons (unemployed) to measure which future employment opportunities treated (workers entering into FTCs) would have, on average, if they had not participated (had stayed unemployed in order to keep on searching). Obviously, the vector X should contain all the variables that are thought to simultaneously influence participation and outcome. If this condition is fulfilled, one can assume

$$E\left(Y^0 \middle| C=1, X\right) = E\left(Y^0 \middle| C=0, X\right). \tag{4}$$

By using this expression it is possible to estimate the TT expressed in equation (1) consistently.

A fundamental requirement for the validity of most microeconometric studies is the *stable unit-treatment value assumption* (*SUTVA*; see RUBIN, 1980): there is no interference between units (persons) leading to different outcomes depending on treatments other units received, that is, the treatment and non-treatment outcomes as well as the treatment effects are not affected by who is treated or how many individuals are treated.[21] In context of the evaluation of ALMP these effects on the non-participants are called *indirect effects* of the treatment (see HECKMAN, LALONDE and SMITH, 1999). If the *SUTVA* is violated the causal effect estimated by the partial analysis at the microeconomic level is not informative with regard to the impact on the economy at large. What does this assumption mean for the analyses at hand? The fact that some individuals enter into FTCs must not affect the labour market situation of those individuals who do not so. For example, if entering into FTCs increases the individuals' labour market chances this should not be at the expense of those who do not enter into FTCs. It is, however, conceivable that a positive effect for participants estimated at the microeconomic level is based solely on redistribution of employment chances between treated and non-treated individuals. Furthermore, the whole range of possibilities how permanent contract jobs are substituted by temporary jobs and the impacts on wage formation and productivity are not taken into account. Obviously, the *SUTVA* is very likely to be violated in reality, a statement which is also predicted by theoretical models explaining the macroeconomic impact of FTCs, such as BOERI (1999) or BLANCHARD and LANDIER (2002). Nevertheless, in line with previous literature, the microeconometric analysis is regarded here as a complementary starting-point for further analyses using other (macroeconomic or general equilibrium) methods.

A further necessary assumption is that the possible phenomenon of perfect predictability of the participation status C given X is ruled out. This is done by assuming that $0 < \Pr\left(C=1 \middle| X\right) < 1$, which guarantees that persons with the same X values have a positive probability of being both participants and non-participants, i.e. any individual constitutes a possible participant and possible non-participant (see HECKMAN, LALONDE and SMITH, 1999:

that there is no selection on unobservables or that by conditioning on X also mean differences in unobservables are balanced.

21 If the *SUTVA* is violated the "no-treatment" benchmark is contaminated by treatment (see HECKMAN, LALONDE and SMITH, 1999: 2035).

1920). Every individual entering into an FTC could in general also stay unemployed in order to keep on searching and vice versa.

Particularly if the vector X is large and contains many continuous variables, it may be quite unlikely that a match between all people of the treatment and non-treatment groups will be found for every combination of X ('curse of dimensionality'; HECKMAN, ICHIMURA and TODD, 1997). However, as ROSENBAUM and RUBIN (1983) show, it is sufficient to match treated and non-treated persons on the conditional probability of participation given the vector of observed characteristics. This conditional probability of participation $e(X) \equiv \Pr(C=1|X)$ is called the *propensity score*. By definition, treatment and non-treatment observations with the same value of the propensity score have the same distribution of the full vector of X. So (4) can be rewritten as

$$E\left(Y^0 \middle| C=1, e(X)\right) = E\left(Y^0 \middle| C=0, e(X)\right). \tag{5}$$

Equation (5) allows to reduce the high-dimensional vector X to a one-dimensional probability $e(X)$ and eases the problem of finding appropriate matches. The propensity score $e(X)$ can be estimated by standard parametric approaches like the probit or logit model (see DEHEJIA and WAHBA, 1999). Following the literature, the predicted linear index rather than the predicted conditional probability is used (see LECHNER, 1998). The reason is that individuals in the tails of the distribution can be distinguished more exactly. Nevertheless, in the following the term propensity score is also used for the linear index.

In concrete terms the matching estimator works as follows: For each person i in the group of unemployed entering into FTCs, a (group of) comparable unemployed person(s) has to be found. Matches are constructed on the basis of a neighbourhood $\mathbb{C}(e_i)$, where e_i is the estimated propensity score for treated person i. Let N_0 denote the number of observations in the sample of unemployed and N_1 is the number of observations in the sample of unemployed entering into FTCs. Thus, the persons in the unemployed sample who are neighbours to i, are individuals $j \in \{C=0\}$ for whom $e_j \in \mathbb{C}(e_i)$, i.e. the set of persons $A_i = \left\{ j \middle| e_j \in \mathbb{C}(e_i) \right\}$.

The effect of the treatment (FTC) for each observation i in the FTC workers group is estimated by subtracting the weighted average of the outcome of the unemployed workers group observations from the outcome of the treatment observation i (see HECKMAN, LALONDE and SMITH, 1999). Hence the *TT* is estimated by

$$\frac{1}{N_1} \sum_{i=1}^{N_1} \left(y_i^1 - \sum_{j=1}^{N_0} w(i,j)\, y_j^0 \right). \tag{6}$$

Different matching estimators differ in the weights $w(i,j) \in [0,1]$ with $\sum_{j=1}^{N_0} w(i,j) = 1$ for the members of the comparison group.

Nearest-neighbour matching

Nearest-neighbour matching (*NN*-matching) defines the neighbourhood A_i of the individual *i* entering into an FTC in such a way that only the unemployed *j* is selected as a control that is closest to *i* in terms of e_i and e_j:

$$A_i = \left\{ j \mid \min_{j \in \{1,\dots,N_0\}} \left\| e_i - e_j \right\| \right\}, \tag{7}$$

where $\| \ \|$ is a metric measuring the distance between e_i and e_j. Equation (7) states that the unemployed worker *j* with the value of e_j that is nearest to e_i is selected as a match and is defined as a control for the FTC worker *i*. This selected unemployed worker is attached with the weight $w(i,j)$=1, i.e. there is only one control per treated individual.

NN-matching can be performed with or without replacement. With replacement means that the non-treated individuals can be used more than once. This can improve the matching quality, but it increases the related standard error of the estimated effect. Therefore, the standard errors have to be adjusted (for the calculation of the standard errors see subsection 3.3). In order to reduce the risk of 'bad matches', a modified version of *NN*-matching called 'caliper matching' is used (see COCHRAN and RUBIN, 1973). For a pre-specified level of tolerance $\Psi > 0$, the FTC worker *i* is matched to the unemployed worker *j* so that:

$$\Psi > \left\| e_i - e_j \right\|. \tag{8}$$

If none of the unemployed persons is within the interval Ψ around the treated individual *i*, the individual *i* is left unmatched and is not used for the estimation. This is one possible method for imposing the *common support condition* (see subsection 3.3 for the discussion of this issue).

Kernel-based matching

Simple *NN*-matching uses only a fraction of the information on the non-treated individuals since only one non-treated person is matched to one treated person. Therefore, it is associated with a loss of efficiency (see SIANESI, 2001a; FRÖLICH, 2001). Kernel-based matching estimators construct matches by calculating weighted averages of the outcomes of all individuals in the non-treated sample with the weights depending on the similarity of the non-treated persons in terms of distance between e_i and e_j. Thus the variance of the estimate is reduced (efficiency gain), which may, however, be associated with an increased bias (imbalance in observable characteristics). Therefore, there is in general a trade-off between the minimising of bias and minimising of variance (see the discussion SIANESI, 2001a: 27). As discussed in detail by BERGEMANN, FITZENBERGER and SPECKESSER (2001) as well as FRÖLICH (2001) the non-treated outcome of the treated individuals $E\left(Y^0 \middle| C=1, X\right)$ can be estimated using a nonparametric kernel regression as a weighted outcome of all non-treated individuals. Kernel-based matching sets $A_i = \{C = 0\}$, that is, all non-treated observations (within the common support) are used. In this application, kernel-based (as well as local linear) matching estimators always performed worse than the simple *NN*-matching in terms of balancing out pre-treatment differences in the outcome variables. Therefore, only the *NN*-matching results are discussed and reported.

3.2 The Counterfactuals of Interest, the Strategy of Job Searchers and the Policy Questions

To apply the methods described in the last subsection to the evaluation of the effects of FTCs one has to take the following features of the dataset into account (see section 4 for description of the dataset used):

- Due to the fact that an unemployed person who has entered into an FTC (and is, therefore, 'treated') can become unemployed again, she or he can also become a 'control' for another treated person.
- If an unemployed person does not enter into an FTC job after a certain number of months of unemployment she or he can enter into an FTC job in a later month or in a following unemployment spell. Thus a person may be a potential control after particular duration of unemployment as well as a treated person at a later point in time.
- The starting date of the treatment is not unique, i.e. FTC jobs can be entered in every month between 1991 and 2000.
- The date of the inflow into unemployment is not unique. Hence, not only the starting date of the treatment differs but also unemployment duration before the treatment.
- There is not only one treatment per person possible but persons can enter more than once into FTCs (after becoming unemployed again).

The implications of these issues will be discussed in the following sections. It will be explained how the application of hazard rate models for the estimation of the propensity score may be a suitable approach to these issues. Recently some of the ideas presented in the following have been formalised by FREDRIKSSON and JOHANSSON (2003).

Existence versus non-existence of fixed-term contracts

In general one can think of two different counterfactuals which are linked to the decisions of job searchers and, therefore, imply different policy questions (see the summary in Table 1).[22]

A first possible job searcher's decision may be – depending on the characteristics of the individual and the attributes of the (desired) job offers – never to enter into any FTC job. This would imply a non-treatment group of unemployed individuals who never enter into FTCs. This definition is in line with the design in almost every evaluation study where the control group consists of people who never enter into the evaluated programme (during the

22 See HECKMAN, LALONDE and SMITH (1999: Section 3.2) for a general discussion of this issue.

period time covered by the dataset). The corresponding policy question is, whether the existence of the institution "fixed-term contract" helps unemployed to find stable employment relationships. This kind of non-treatment definition is referred to as *Definition 1* (DEF.1) in the following. Non-treated individuals according to DEF.1 are individuals who never enter into FTCs within the whole period of time they are observed in the dataset used.

Entering into an FTC job versus continuing to search for a permanent job

A second possible behaviour of job searchers, which is in line with some of the theoretical discussion in section 2, may be to enter into FTCs after having tried to find a permanent contract job (or waiting for permanent contract job offers, respectively) for several months of unemployment. This means that a non-treated person after a particular unemployment duration can become a treated person at a later point of time in the unemployment spell and can, therefore, be a treated as well as a control person. SIANESI (2001a) proposed this definition of non-treatment in the context of the evaluation of active labour market policy in Sweden. She does not differentiate between different kinds of measures for the unemployed, i.e. there is only one treatment (all type of programmes) compared to one non-treatment (unemployment). The methodological problem is that almost every unemployed person in Sweden does participate in any programme sooner or later, so the non-treatment DEF.1 introduced in the last section seems not to be a reasonable concept. She states that the reason that an unemployed individual does not participate in a programme is because she or he has found a job before. Therefore, the participation decision is *sequential over time*. This, however, implies that individuals who have never participated are those who were successful in finding a job before. Non-treated DEF.1 may, therefore, bias the estimated treatment effect towards negative values (see FREDRIKSSON and JOHANSSON, 2003).[23]

The idea of a sequential participation decision can be applied to the evaluation of the effects of FTCs: Let T^1 denote the duration of an unemployment spell before an individual exits into an FTC. Is it beneficial (in terms of future employment opportunities) to enter into an FTC after a certain duration of unemployment T^1 in comparison to keep on searching for a permanent position? The corresponding policy question is whether or not it is beneficial for unemployed job-searchers to enter into FTC jobs in comparison to keeping on searching for a permanent contract job. In the following this definition of non-treated persons is referred to as *Definition 2* (DEF.2). According to this definition non-treated persons can be control persons after a particular unemployment duration as well as treated persons in later months or unemployment spells.[24] To the best of my knowledge, besides SIANESI (2001a) and this paper, there is no study available yet, in which DEF.2 is applied to define the group of non-

23 FREDRIKSSON and JOHANSSON (2003: 3) state about non-treated DEF.1: "*By defining the comparison group in this way one is implicitly conditioning on the outcome variable since those who do not enter in future time periods to a large extent consist of those who have had the luck of finding a job. Therefore, the conditional independence assumptions* (…) *do not hold and studies that define the comparison group in this way will generate estimates that are biased towards finding negative treatment effects when, in fact, none exist.*"

24 A justification can be found in HECKMAN, LALONDE and SMITH (1999: 83): "*The same individual may be in both groups if that person is treated at one time and untreated at another.*"

treated persons. It can be seen in Table 1 that DEF.2 implies an increased number of potential control unemployment spells in comparison to DEF.1.

Table 1: Definitions of non-treatment

	Counterfactual of interest	*Non-treated persons*	*Number of non-treated **	*Policy question*
DEF.1	World without FTCs for individual *i*	Unemployed who do never enter into FTC jobs	Spells: 1,271 Persons: 1,041	Does the existence of FTC jobs increase the long-term employment prospects of those unemployed who enter into FTCs?
DEF.2	World without FTC job offers up to the unemployment duration T^1+1 for individual *i*	Unemployed who do not enter into FTC jobs before unemployment duration T^1+1	Spells: 1,826 Persons: 1,447	Do unemployed persons taking the first opportunity (FTC job offers) enhance their future employment prospect in comparison to those who continue searching (for perm. jobs)?

Note: * the numbers refer to spells with non-missing explanatory variables in the estimation of the propensity score and the period 1991-2000 which do not end in transitions to FTCs.

Unknown 'start of non-treatment'

A problem in every evaluation study using longitudinal data is that for the group of nonparticipants important time varying variables like 'unemployment duration prior to the treatment' are not defined (see LECHNER, 2002 as well as FREDRIKSSON and JOHANSSON, 2003). The unemployment duration before entering into the treatment (FTC) T^1 is by definition for non-treated an 'unobserved counterfactual' T^0, thus it is not possible to include it into the estimation of the propensity score (see SIANESI, 2001a).[25] For example, GERFIN, LECHNER and STEIGER (2002) approach this problem by predicting for each non-participant a hypothetical programme starting date using information which is available at the date of the inflow into unemployment. SIANESI (2001a) as well as BRODATY, CRÉPON and FOUGÈRE (2001) use a simple but intuitive approach. The propensity score is estimated by a duration model, which derives the probabilities of transiting from unemployment in the treatment conditional on having stayed unemployed for a certain number of months.[26] In the following analyses a discrete (logistic) duration model is used for the estimation of the propensity score.

25 Again the counterfactuals (T_i^1, c_i=0) and (T_i^0, c_i=1) are not observable.
26 To be exact, SIANESI (2001a) estimates for every period a probit conditional on having reached an unemployment duration, which corresponds to the period. "*This approach is equivalent to a discrete hazard model, with all the estimated parameters allowed to be duration-specific*." SIANESI (2001a: 17).

Repeated treatments (transitions to FTCs)

As mentioned above, a person may enter more than once into an FTC job.[27] In the analyses it will, however, not be considered whether a treatment is the person's first one or not. A repeated treatment is interpreted as if the person had never entered into an FTC job from unemployment before.

One alternative approach is to model repeated participation explicitly and to allow the repeated participation to have a different (additional) effect. This is done, for example, by BERGEMANN, FITZENBERGER and SPECKESSER (2001) for East German labour market programmes. Since repeated participation is a rare event in the dataset used, it does not seem to be possible to perform separate analyses for repeated treatments in this study. There are *349* treatments (transitions to FTCs) observed in the dataset. *295* of them are by persons who enter into an FTC only once, *25* persons enter twice and *one* person takes up FTCs four times. A second alternative approach, which seems to be common practice, is to focus on the first treatment and to exclude a repeated treatment from the analysis or to include only those individuals who participate only once. This, however, may induce a selection bias, since 'unsuccessful' FTC jobs (in terms of repeated transitions from unemployment to FTCs and the other way round) are systematically excluded. A similar line of argument applies to the non-treated: using only the first unemployment spell leads to sample selection towards "above average" controls (see HAM and LALONDE, 1996: 184).

Illustration of the definition of treated and non-treated persons

An example which documents the considerations in the previous subsections is depicted in Table 2. A monthly time scale, which may correspond to a certain calendar time, is assumed.[28] Person 1 becomes unemployed (U) in month 2, after having been employed in period 1 (**E**). In month 7 the person is treated (𝕋), i.e. it enters into an FTC. (Remember that treatment is defined as *entering into* an FTC from unemployment but not *being employed* under an FTC).

Table 2: Definition of treated and non-treated individuals

Month	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Person 1	**E**	U	U	U	U	U	𝕋	**E**	**E**	**E**	**E**	**E**	**E**	U	U	U	𝕋	**E**	**E**	**E**
Person 2	·	·	·	·	·	**E**	U	U	𝕋	**E**	**E**	**E**	**E**	**E**	**E**	**E**	**E**	**E**	**E**	**E**
Person 3	**E**	U	U	U	U	U	U	**P**	**E**	**E**	**E**	**E**	**E**	**E**	**E**	**E**	**E**	**E**	**E**	**E**
Person 4	**E**	**E**	**E**	U	U	U	U	U	**P**	**E**	**E**	**E**	**E**	U	U	U	U	U	𝕋	**E**

Notes: **E** = Employed; U = Unemployed ; 𝕋 = Entering into an FTC (Treatment); **P** = Entering into a permanent contract; · = missing observation

27 This is called 'multiple treatments' or 'dynamic treatments' in the literature. So-called 'dynamic treatments' are discussed in LECHNER and MIQUEL (2002) and are applied in BERGEMANN, FITZENBERGER and SPECKESSER (2001).

28 Month 1 may be, for example, January 1993.

Person 1 is employed (without taking the type of contract into account) until month 13 and becomes unemployed again in month 14. In month 17 she or he again enters into an FTC, i.e. she or he is treated. As stated in the last subsection, a person can be in the treatment group for more than one time.[29]

Person 2 is not in the sample (or the survey, respectively) until month 6, when she or he is employed. After having been unemployed for two months she or he enters into an FTC in month 9. Is person 2 a potential member of the control group for the first treatment of person 1, assuming DEF.2 for non-treated persons? It is a *potential* control person. The estimated propensity score will, however, probably be too different since the unemployment durations of both persons are very different in month 7. Person 3 is a potential control person for the first treatment of person 1. She or he can be used as a non-treated person in terms of both definitions of treatment, since she or he never enters into an FTC. Finally, person 4 is a potential control group member of the second treatment of person 1, if DEF.2 is applied.

3.3 Implementation of the Propensity Score Matching Estimator

Taking into account the particularities of the dataset and the definitions of treated and non-treated persons as presented in the previous subsections, it is now described how the matching estimator is implemented.

The main modification of the standard propensity score matching approach is to estimate the propensity score by a discrete hazard rate model. Thus treated and non-treated individuals are matched on the basis of the predicted transition probabilities from unemployment to FTCs, conditional on having stayed unemployed for a certain number of months.[30]

Within the framework of discrete hazard rate models time is divided into intervals (months). The amount of time spent in unemployment is denoted by the random natural number T which is a realisation of a nonnegative and continuous random variable t. If an unemployment spell ends within the interval $\left[I_{t-1}, I_t\right]$, then $T = t$. Besides transitions to several labour market states, the unemployment spell can end also due to right-censoring. If $T = t$, a transition to another state occurs; if $T>t$ the spell is right-censored. The discrete hazard rate is the probability of a transition of an individual in it's k-th unemployment spell to labour market state C during month t, conditional unemployment has lasted until the beginning of t (see FAHRMEIER and TUTZ, 2001). Hence, the destination-specific hazard rate for the transition from unemployment in FTC jobs is given by $e_k^{FTC}\left(t \middle| X(t)\right) \equiv \Pr\left(T_k = t, C = 1 \middle| X(t), T_k \geq t\right)$.

In order to keep the notation simple, the propensity score is again denoted by e.

Additionally to matching on the propensity score, it is imposed that individuals are matched only within the same *calendar month* τ, which ensures that treated and controls face

29 The person is then, however, considered as another person. This implies that it is equivalent for the analysis whether one observes two unemployed persons which each enter once into an FTC or whether one observes one person who enters into FTCs twice (after being unemployed).

30 A more formal justification for the consistency of this approach can be found in SIANESI (2001a) as well as FREDRIKSSON and JOHANSSON (2003).

the same economic environment (business cycle, season). In the following, some further issues are discussed in detail.

Choice-based sampling

The problem of choice-based sampling occurs if in the data used the probability of sampling a treated individual is not the population probability that an individual is a treated. This is certainly the case in the empirical analyses of this paper since the information on FTCs in the data imply that transitions to FTCs are likely to be systematically underrepresented in comparison to the population (see section 4). What does choice-based sampling implies for the estimated treatment effect?

If there is choice-based sampling, weights are required to estimate consistently the propensity score and thus the causal effect (see SMITH and TODD, 2003). When the weights are unknown it can be shown that propensity score matching methods can still be applied by transforming the estimated propensity scores e to odds ratios $e/(1-e)$ or log odds ratios, respectively, and by matching on these instead.[31] In case of *NN*-matching choice-based sampling does not seem to be a severe problem: It does not matter whether matching is performed on the odds ratio or on the propensity score, because the ranking of the observations is the same and, therefore, the same neighbours are selected (see SMITH and TODD, 2003).[32] However, for methods using the absolute distance between observations, such as kernel-based matching, it does matter.

Common support condition

Unlike parametric estimators, the consistency of matching depends crucially on the so-called *common support condition* (see HECKMAN, ICHIMURA and TODD, 1997). In case of propensity score matching the condition requires that the distribution of the estimated propensity of the treated e^1 is overlapped entirely by the distribution of the propensity score of the non-treated e^0. Here it means that for every unemployed person entering into an FTC a sufficiently similar person (in terms of the estimated propensity score) who stays unemployed in the same calendar month has to be available.

There are two common approaches to the problem of lacking common support (see LECHNER, 2001a): Either matching is performed only for the sub-population within the common support or the problem is simply ignored. The latter means, for example, in case of *NN*-matching using non-treated neighbours who are very different from the treated persons.[33] This approach can obviously lead to biased estimates due to 'bad' matches. Although the first approach is appropriate for obtaining a consistent estimate for the *region of common support*, that is, the region of the distribution where for every treated person there is a sufficient similar non-treated individual in terms of the propensity score, it may be misleading:

31 Ignoring choice-based sampling in the estimation of the propensity score is consistent since the odds ratio estimated using the incorrect weights (or no weights, respectively) is a scalar multiple of the true odds ratio, which is itself a monotonic transformation of the propensity score.

32 Any transformation of the propensity score which preserves the order of the observations does not affect consistency of the *NN*-matching estimator (see LECHNER, 1998).

33 LECHNER (2001a) proposes a further approach.

"*When treatment effects are heterogeneous inside and outside the common support, then the estimated effect does no longer correspond to the original parameter of interest.*" (LECHNER, 2001a: 21). Otherwise, if the treatment effect is homogeneous, at least within the treatment group, no additional problems appear besides the loss of information, that is, loss of efficiency of the estimator (see BLUNDELL and COSTAS DIAS, 2000: 449).

In this study the common support condition is imposed for every single calendar month τ. In concrete terms, for every month between 1991 and 2000 it is checked whether there is an overlap in the distribution of the estimated propensity score of treated and non-treated persons. Treated persons outside the range of the distribution of the propensity score of non-treated persons are excluded. Furthermore, the caliper is an additional safeguard against the violation of the common support. The caliper is chosen by taking into account the trade-off between unbiasedness (reduction in the error variance) in case of narrow calipers and loss of observations. The matching estimator 'psmatch' implemented by SIANESI (2001b) for STATA is used and is modified accordingly.

Standard errors

From equation (1) it can be seen that the estimated *TT* is the difference of two means. A statistical test on the significance of this difference is a simple *t*-test. For this purpose the variance of the TT has to be calculated as well. It has to be taken into account that matching with replacement is performed, i.e. a non-treated person can serve for more than one treated person as a control. Following SIANESI (2001a) and LECHNER (2001b) independent observations, fixed weights and homoskedasticity of the outcome variable within the treated and the control group are assumed. Furthermore, the variance is assumed not to depend on the fact that the propensity score is estimated and the estimated probabilities are applied for a reduced sample due to the common support condition (see LECHNER, 2002: 29). Given these assumptions the variance of *TT* in case of *NN*-matching is calculated as

$$Var\left(TT\right)=\frac{1}{N_1}Var\left(Y^1\middle|C=1\right)+\frac{\sum_{j=1}^{N_0}\omega_j^2}{\left(N_1\right)^2}Var\left(Y^0\middle|C=0\right), \tag{9}$$

where ω_j is the number of times a non-treated person is used as a control (see also HUJER, CALIENDO and THOMSEN, 2003, footnote 12). As mentioned above, the variance increases with ω. A potential problem is the assumption in (9) that the variance is not influenced by the fact that the propensity score is an estimated variable. A possible solution to the problem is to calculate the variance based on bootstrapping (see LECHNER, 2002). However, in this paper the problem is ignored in order to avoid time-consuming calculations. This may be justified by the result in LECHNER (2002) that there seems to be little difference between the bootstrapped variance and the variance calculated using equation (9).

Anticipatory effects (Ashenfelter's Dip)

A common knowledge in evaluation of ALMP is that treatments may influence not only the outcome *after* the participation but also already *before* participations. It has been observed in a number of studies, that shortly before participating in a certain training programme earnings and employment situations of the future participants deteriorated, which has been termed “Ashenfelter’s Dip” (see HECKMAN and SMITH 1999; FITZENBERGER and PREY 2000; BERGEMANN, FITZENBERGER and SPECKESSER 2001).[34] Usually, this *transitory decline* in employment and wages of participants is explained by anticipatory effects: In expectation of participation, search activities are reduced, leading on average to reduced employment probabilities and wages. Since treatment affects pre-treatment variables, the *CIA* is violated.

It is likely that *anticipatory effects* are also relevant for the transition to FTC jobs. Many job searchers know several months before the actual transition occurs that they will take up a job soon and some will already have signed the employment contract. Anticipatory effects lead ceteris paribus to reduced FTC hazard rates months before the transition occurs. Thus it is likely that control persons with too low estimated propensity scores are matched. If one assumes that persons with higher FTC hazard rates are those with better characteristics (causing better employment opportunities), the estimated effect of FTCs (on future employment opportunities) is biased upwards, since the group of control persons has worse characteristics on average.

There is no clear cut solution to this problem. One straightforward but arbitrary approach is to match treated and non-treated persons on the basis of the estimated propensity score not by the month of transition but some months earlier.[35] Although I could not find any evidence for a decline in the employment probability before the treatment in an earlier version of this paper, I will try to avoid the violation of the *CIA* by assuming that anticipatory effects start not earlier than two months before the transition to the FTCs. Thus, treated and control persons are matched two months before the treatment occurs. If anticipatory effects are strong and start ‘early’ this is obviously only an incomplete solution to the problem. However, the results turn out to be quite robust with respect to different specifications concerning the month of matching.

Check on matching quality

A simple and intuitive test for whether the matching approach is able to balance the selection bias consists of *t*-tests on the differences in the outcome variables between the group of treated and the group of control persons *before* the treatment (the transition to the FTC).[36] This is tested up to 24 months before the treatment for the monthly measured outcome variables and up to 3 years for the yearly measured outcome variables (for the description of the outcome variables see section 4). For this application it turns out, that *NN*-matching seems to be more suitable than different types of kernel-based matching procedures for generating appropriate control groups in the sense of this test.

34 This phenomenon was observed for the first time by ASHENFELTER (1978) for the earnings effects of a training programme.

35 This approach is in line with interpretation of anticipatory effects as an earlier start of the treatment.

36 Note that this is a variant of the pre-programme test introduced in HECKMAN and HOTZ (1989) for regression models.

4 Data base and Definition of Variables

Data base and estimation sample

The data base is the German Socio-Economic Panel (GSOEP) for 1991 until 2001. The GSOEP is a representative household survey of the German population, conducted on an annual basis.[37] A useful feature of the GSOEP is the availability of monthly information between yearly interviews (so-called calendar). Different employment states are covered. These information are collected by retrospective questions about what happened in particular months within the previous year.

Unfortunately, the type of contract is not collected on a monthly basis in the GSOEP. The type of contract is only asked for the current job at the date of the interview. It is, however, possible to derive monthly information, taking legal regulations on FTCs into account. It is not allowed for employers to employ a person with an FTC after the same person was employed with a permanent contract. Thus, while it is allowed to transform a temporary contract into a permanent one the other direction is not permitted. This means that if a person is currently employed with an FTC at a certain employer that she/he was already hired into an FTC.

Using this information, the type of contract (FTC vs permanent contract vs self-employed) is not defined for approximately 30 % of all hirings. These undefined spells are obviously more often short-term (that is the reason why they are not observed in the months of interview), so it is likely that there are many FTC spells within this category, which may induce a severe selection problem. It is possible to reduce the amount of undefined spells to approximately 18% by using information concerning the reason of the end of the last employment spell (due to the expiration of an FTC or an apprenticeship contract). However, using this information one identifies "unsuccessful" FTCs with a higher probability, i.e. those FTCs which do not lead to a long-term employment relationship at the same employer. For this reason, the latter information is not used for the definition of employment spells.

To check the significance of this problem, a maximum-likelihood probit model with sample selection was estimated (see VAN DE VEN and VAN PRAGG, 1981). In the first probit equation, the transition to any type of employment (including "undefined spells") is analysed, in the second probit equation the FTC hazard rate. The error terms of both equations are assumed to be jointly normally distributed. At least one exclusion restriction is needed in order to avoid that identification is solely achieved from distributional assumptions. If the correlation of the error terms is not zero, a separate estimation of the FTC hazard rate leads to biased results. This is checked by performing LR-tests comparing the likelihood of the full model (the simultaneous estimation of the FTC hazard and overall hazard) with the sum of the likelihoods for the separate estimated FTC hazard and overall hazard.

37 Details on the GSOEP can be obtained from the web-server of the German Institute of Economic Research (DIW) in Berlin (http://www.diw-berlin.de/soep/).

Various estimations with different exclusion restrictions were performed. The lowest *p*-value of the LR-test was 0.3825. Since all LR-tests in all specifications show that the correlation of the error terms is not significant, one can conclude that estimation of the FTC hazard rate model is probably not biased by the sampling scheme, since the selection effects are either captured by observable variables or there is no selection bias at all. One should, however, keep in mind that the probit with sample selection model is based on some restrictive distributional assumptions as well as the choice of appropriate exclusion restrictions which may drive the results to a large extent. Therefore, all the results should be interpreted keeping in mind that short FTC spells are likely to be underrepresented. Insofar as the sampling scheme corresponds to choice-based sampling as described in subsection 3.3, *NN*-matching does not yield biased results. For the analyses, those unemployment spells ending in employment spells with undefined employment contracts are simply dropped.

Participants in Public Employment Measures (ABM) hold FTCs by definition. In order to distinguish regular FTCs from these types of subsidised employment, ABM spells are defined as unemployment spells in the analyses. This definition seems to be reasonable since a certain duration of unemployment is usually a necessary condition for participation.[38]

The estimation sample consists of individuals, *registered* as unemployed for at least one month between January 1991 and December 2000. In order to obtain an inflow sample only spells are used which start after January 1991, i.e. left-censored spells are excluded. Since there exist a number of formal and informal early-retirement measures in Germany only persons not older than 58 years are included. The minimum age for being in the sample is 18 years. One may argue that this is – given the comparatively long period of education in Germany – too young to ensure that only unemployed persons are included which are really job searchers. However, it is interesting to include younger workers since temporary jobs may be important for the transition from school or apprenticeship to work (see RYAN, 2001).

Definition of the outcome variables

An overview of the definition of the outcome measures used in the analysis can be found in Table 3. The *first* and probably most important outcome is the future probability of being employed with a permanent contract. Since – as mentioned above – the type of contract is only observed by the date of interview in the raw data, this outcome variable is only annually measured at the dates of interviews. So the effect will be the *difference in the probability of being employed under a permanent contract between the treated and control group in the year following the next.* The year of interview following the transition is not taken into account because in many cases it is exactly the information which is used to define the start of the employment spell as an FTC job or a permanent contract job. The question whether the actual employment contract is temporary or permanent is not available for all waves of the

38 Public Employment Measures (ABM) cannot be distinguished from 'regular' FTCs in 1996. For this year FTC workers are defined as public employment measure participants if they are employed in the public sector and declare to be unemployed. Nevertheless, it is likely that there are still participants among those entering FTCs. However, the proportion of Public Employment Measures in FTC employment is about 5 % in West Germany, thus, the problem may not be too severe (see RUDOLPH, 2000).

panel study. Until 1995 only those who reported job changes were asked about the type of contract. From 1995 onwards all necessary information is available. This is no problem for the identification of transitions to FTCs or permanent contracts, that is, the estimation of the propensity score. It implies, however, that the first outcome variable is only measured from 1996 until 2001 (at least one year after the transition to the FTC).

Table 3: Definition of the outcome variables

Outcome 1 *1996-2001* *annual information*	Probability of being employed under a permanent contract in the year following the next, collected by the months of interviews.
Outcome 2 *1991-2000* *monthly information*	Probability of employment (all types of contracts apart from training-on-the-job) within each of the following 36 months after the treatment.
Outcome 3 *1991-2000* *monthly information*	Probability of (registered) unemployment within each of the following 36 months after the treatment.
Outcome 4 *1991-2000* *monthly information*	Probability of being out-of-labour-force (including school/ university but without training-on-the-job) within each of the following 36 months after the treatment.
Outcome 5 *1996-2001* *annual information*	Probability of being employed under an FTC in the year following the next, collected by the months of interviews. (DEF.2 only).

The *second* outcome measure is derived from the monthly calendar. The effect is the difference in the employment probability between treated and controls within each of the 36 months following the treatment (transition). Monthly data are only available until 2000 (the monthly information is collected retrospectively) so that the other outcome measures are only available up to this date. Employment is now defined broadly as FTC and permanent contract employment as well as self-employment but not training-on-the-job. The *third* outcome variable is again derived from the monthly calendar. The effect is the difference in the probability of registered unemployment between treated and controls within each of the 36 months following the treatment. The *fourth* outcome variable is the risk of being out-of-labour-force. Finally, the *fifth* outcome variable is the probability of being employed under an FTC in the year following the next. Note that this is an annually measured variable comparable with outcome 1. Furthermore, outcome 5 is not defined for non-treated DEF.1, since DEF.1 requires that non-treated individuals never enter into an FTC, i.e. the mean outcome of the non-treated is fixed at zero.

A potential problem arises due to the fact that the outcome variables are not observable for all 36 months after the transitions to FTCs. Some persons do not answer all questions (item non-response) and others drop out from the whole survey (sample attrition). For the outcome variables sample attrition is particularly important. Generally, there are two approaches to deal with this problem. Either, one uses only the balanced panel, i.e. one excludes all persons with incomplete information in the outcome variables, or one uses all available information and accepts a decreasing number of observations with increasing time-lag to the month of treatment (unbalanced panel).

Using the first approach, one has to assume that selection into the sample (the balanced panel) is random, i.e. the sample still represents the underlying population. For the second approach one has to assume that the probability of missing values in the outcome variables are the same for the treated and the control group, i.e. the matching estimator balances the differences out. This is unlikely if the probability of missing outcomes does not depend on the covariates which are included in the propensity score estimation.

In order to get an impression of the problem of missing values, a variable for the unbalanced panel is defined, which includes the number of months with missing values during the following 36 months.[39] Using the same *NN*-matching procedure as for the second outcome variable (see section 6) it is checked whether there are differences in the number of missing observations in the treated and matched control sample. It turns out that there is a mean difference which is, however, not statistical significant at the 10 % level. The average number of missing months is 8.9 in the treated sample and 8.3 in the control sample. The corresponding t-statistic of 0.59 indicates that the difference is not statistically significant different from zero.

One can conclude that there is sample attrition in the outcome variable but no strong evidence in favour of induced sample selection bias. Thus the "unbalanced panel approach" is used since the bias generated by simply dropping incomplete data may be worse than the bias due to a declining number of observations with an increasing time-lag to the treatment. Furthermore, using the unbalanced panel ensures a sufficient sample size.

39 Of course it would be more appropriate to check the problems of missing values for every single month. Unfortunately (or rather fortunately) there are not enough missing values to do this.

5 Specification and Estimation of the Discrete Hazard Rate Model – Estimation of the Propensity Score

5.1 Specification

As mentioned above the propensity score is estimated by a discrete hazard rate (duration) model. Since the unemployment spells in the GSOEP are measured in months it seems to be a natural approach to specify a discrete time model instead of a continuous time hazard rate model.[40] The method applied and the specification is not discussed in detail. A further description can be found in HAGEN (2003).

Four different destination (exit) states are taken into account: FTC jobs, permanent contract jobs (including self-employment)[41], a category termed "training" consisting of on-the-job training and apprenticeship as well as an out-of-labour-force status (including school and university). A further distinction between the types of contracts implies that a differentiation between full- and part-time is not feasible due to the limited sample size. Table A2 in the Appendix displays the number of transitions and the average duration of the prior unemployment spells.

In order to compare the determinants of the transition to FTCs and to permanent contracts a discrete independent competing risk model is estimated by a multinomial logit model without controlling for unobserved heterogeneity (see FAHRMEIER and TUTZ, 2001).[42] The propensity score is, however, estimated by a simple logistic model (again without controlling for unobserved heterogeneity) for the exit into FTCs only. By doing this, it is avoided that the estimated parameters corresponding to the exit into FTC jobs are biased by specification errors with regard to the other exit states.

Due to the limited sample size, the analysis is performed for men and women together, which is, given the well-known substantial differences in labour force behaviour, a serious restriction. However, differences are taken into account as far as possible by allowing important variables to have a gender-specific impact.

The base-line hazard is specified non-parametrically in the form of piece-wise constant dummy variables.[43] Besides the usual characteristics (qualification, age, gender, nationality, disabilities, marital status), variables on previous labour market experience are included, which may be important signals for employers and which are likely to capture individual heterogeneity of the unemployed job searchers. The duration of the last employment and

40 Other examples of discrete unemployment duration models with the GSOEP are, for example, STEINER (2001) as well as HUJER and SCHNEIDER (1996).

41 Due to the sample size a further differentiation between self-employment and paid employment seems to be not feasible. For an analysis of the hazard rate into self-employment in West Germany see REIZE (2000).

42 An extended multinomial model which controls for unobserved heterogeneity (HECKMAN and SINGER, 1984) and further duration analyses can be found in HAGEN (2003).

43 The number of observations is too small for defining for every single month of unemployment duration a separate dummy variable.

unemployment spells is included ("lagged duration dependence", see HECKMAN and BORJAS, 1980). Furthermore, the number of previous unemployment spells is included in order to control for "occurrence dependence". For checking whether there is state dependence in the type of the employment contract, a dummy variable indicating whether the person has ever held an FTC before and dummy variables describing the reason for the end of the last employment contract ('due to end of an FTC or apprenticeship contract' and 'due to dismissal') are included.

The monthly federal state unemployment rate is included to control for regional labour demand.[44] After controlling for unemployment the federal state dummies become insignificant and are, therefore, excluded. Descriptive statistics can be found in Table A3 in the Appendix.

5.2 Estimation Results

The estimation results of the multinomial logistic hazard rate model are depicted in Table A4 in the Appendix. The log-likelihood ratio tests proposed by CRAMER and RIDDER (1991) confirm that the differences in the destination-specific regression coefficients are statistically significant at the 1% level.[45] Thus FTC and permanent contract jobs are indeed behaviourally distinct states. The marginal effects of the estimated coefficients for the FTC and the permanent contract hazard rates, evaluated at the means of the X covariates and multiplied by 100, can be found in Table 4. The marginal effects of dummy variables are calculated as discrete changes in the expected value of the hazard rates as the dummy changes from 0 to 1. Due to the very time consuming calculations it was not feasible to calculate standard errors for the marginal effects. Therefore, the t-values of the corresponding coefficients (Table A4 in the Appendix) are reported. Furthermore, the relative risk (odds ratio) to exit into an FTC instead of a permanent contract is reported.

The following results are of particular interest:

- There is no clear cut pattern of duration dependence in the transition to FTCs and permanent contracts. If one focuses on long-term unemployed (more than 12 months), then there is a tendency for positive duration dependence in the FTC hazard rate and negative duration dependence in the permanent contract hazard rate. This result is in line with the theoretical considerations but should be interpreted with care since neglected unobserved heterogeneity can bias the results of the baseline hazard.
- The variables 'end of previous job due to expiration of an FTC or apprenticeship contract' and 'never an FTC before' have a significantly positive effects on the transition to FTCs. In the same way, 'end of previous job due to dismissal' has a positive effect and 'end of previous job due to expiration of an FTC or apprenticeship contract' has a negative effect on the permanent contract hazard rate. This is in line with the predictions of

44 Obviously, it would be more suitable to use regional units, which approximate regional labour markets more accurate, such as, for example, travel-to-work-areas. Unfortunately the necessary information on the individuals' place of residence is not included in the GSOEP.

45 Furthermore a Hausman test on the IIA was performed. The null hypothesis of independence could not be rejected.

dual labour market theory.[46] There is, however, an alternative interpretation: individuals who did not pass the transition from apprenticeship (which may also be interpreted as a kind of probationary period) to permanent contract employment within a firm have to enter again into an additional probationary period, that is, an FTC.

- Typical characteristics having been found to prolong unemployment duration in other studies, such as disabilities, being a foreigner, being female and having children, do not affect the FTC hazard rate, but have a significantly negative effect on the permanent contract hazard rate. To put it in another way: The relative risk of entering into an FTC instead of a permanent contract is 2 for disabled persons (relative to persons without disabilities), 1.5 for foreigners (relative to German) and 1.9 for women with children (relative to men without children).
- Opposed to theoretical predictions, the positive effect of not receiving unemployment compensation is stronger for the permanent contract hazard rate. The odds ratios are, however, not statistically significant. Furthermore, it is well-known that the actual receipt of unemployment compensation cannot be treated as exogenous, i.e. the estimated coefficients are not reliable.

A further interpretation of the results is beyond the scope of this paper (see for a more detailed discussion HAGEN, 2003).

In order to estimate the propensity score, the risk of leaving unemployment is estimated separately for the exits into FTCs, keeping the other exits as right censored at the time of completion.[47] Those variables which have no statistically significant effects on the FTC hazard rate are excluded from estimation. Remember that the vector of covariates X should contain all the variables that are thought to simultaneously influence participation and outcome. If a variable is insignificant in the FTC hazard rate estimation, this does not mean that it does not affect the outcome, but there are no significant differences in the variable between treated and non-treated individuals. The results are depicted in Table A5 in the Appendix.

46 Note that these coefficients are also significant in specifications controlling for unobserved heterogeneity (see HAGEN, 2003).

47 See NARENDRANATHAN and STEWART (1993: 68) for a justification of this approach.

Table 4: Estimation Results of the (multinomial logistic) competing risk hazard rate model – marginal effects and relative risk of entering into FTC versus entering into perm. contract

	$\overline{X}$	Exit into FTC		Exit into PERM		Relative Risk of entering into FTC versus PERM	
		M.E. x 100	"*t*-stat"	M.E. x 100	"*t*-stat"	odds ratio	*t*-stat
Baseline hazard							
Month 2 – 3	0.153	0.539	3.39	2.424	8.04	0.680	-1.91
Month 4 – 6	0.168	0.481	3.01	1.802	6.15	0.775	-1.20
Month 7 – 9	0.121	0.286	1.65	2.225	6.29	0.584	-2.12
Month 10 – 12	0.086	0.933	3.88	2.270	5.51	0.948	-0.19
Month 13 – 18	0.106	0.544	2.43	2.833	6.57	0.628	-1.62
Month $\geq$19	0.169	0.853	3.50	1.688	4.08	1.068	0.22
Age	39.212	-0.129	-0.76	-0.278	-1.09	0.996	-0.02
Age^2 / 1,000	17.092	0.520	1.10	1.084	1.55	1.038	0.05
Age^3 / 100,000	80.778	-0.067	-1.60	-0.137	-2.23	0.993	-0.11
No occupational qualification	0.421	-0.317	-2.96	-0.782	-4.73	1.045	0.27
Master craftsman	0.045	-0.127	-0.58	0.311	0.90	0.729	-0.97
University graduate	0.064	0.487	2.61	0.538	1.89	1.234	0.95
Female	0.492	-0.019	-0.06	0.215	0.54	0.871	-0.35
Disabled	0.117	0.093	0.54	-0.895	-3.36	2.004	2.63
Foreigner	0.365	0.063	0.44	-0.630	-3.78	1.527	2.61
Married	0.633	0.188	0.94	-0.072	-0.25	1.306	0.95
Married x female	0.311	-0.297	-1.13	-0.121	-0.27	0.734	-0.80
No partner	0.299	-0.198	-1.10	-0.539	-1.96	1.061	0.22
No partner x female	0.147	0.537	1.65	0.629	1.38	1.249	0.58
Children < 16	0.398	-0.082	-0.56	0.007	-0.02	0.903	-0.46
Children x female	0.217	0.003	0.01	-0.983	-3.51	1.855	2.06
Prev. job: end of FTC or apprenticeship	0.104	0.819	3.60	0.472	1.49	1.625	1.89
Prev. job: end of FTC or appr. x female	0.053	-0.352	-1.86	-0.340	-0.91	0.725	-0.89
Prev Job: dismissed	0.260	0.217	1d.31	1.391	5.41	0.680	-1.74
Prev Job: dismissed x female	0.119	-0.044	-0.19	-0.471	-1.61	1.253	0.70
Out-of-labour-force before	0.196	0.526	1.93	-0.247	-0.59	1.936	1.91
Out-of-labour-force before x female	0.128	-0.420	-1.82	-0.716	-1.67	0.845	-0.39
Training or school before	0.127	0.449	2.39	0.414	1.62	1.274	1.06
Duration of prev. unemployment spell	3.300	-0.015	-1.68	-0.018	-1.46	0.992	-0.62

… Table 4 continued

	$\bar{X}$	Exit into FTC		Exit into PERM		Relative Risk of entering into FTC versus PERM	
		M.E. x 100	"*t*-stat"	M.E. x 100	"*t*-stat"	odds ratio	*t*-stat
Prev. employment spell 3-5 months	0.047	0.953	3.43	0.822	2.27	1.500	1.43
Prev. employment spell 6-8 months	0.055	0.642	2.53	-0.206	-0.51	2.002	2.32
Prev. employment spell 9-11 months	0.029	0.282	1.09	2.055	4.44	0.631	-1.38
Prev. employment spell 12-20 months	0.047	0.241	1.05	0.874	2.43	0.875	-0.45
Prev. employment spell > 20 months	0.182	0.199	1.30	0.756	3.35	0.870	-0.69
Already ever an FTC before	0.115	0.403	2.69	-0.280	-1.34	1.757	3.02
Public sector before	0.102	0.358	2.25	-0.128	-0.53	1.544	2.13
Numb. of prev. unemployment spells	0.818	-0.057	-0.78	-0.033	-0.28	0.952	-0.50
Numb. of prev. unemployment spells^2	2.602	0.017	1.90	0.031	2.20	1.003	0.28
Log net household income	7.253	-0.049	-2.65	-0.042	-1.38	0.965	-1.35
No unemployment benefit	0.449	0.256	2.62	0.874	5.59	0.857	-1.04
No unemployment assistance	0.808	0.619	5.19	1.690	8.40	0.744	-1.22
Log regional unemployment rate	2.222	-0.046	-0.28	-1.108	-3.65	1.709	1.88
Spell started in first quarter	0.347	-0.517	-4.46	-0.870	-4.69	0.845	-0.92
Spell started in second quarter	0.273	-0.454	-3.88	-0.690	-3.77	0.813	-1.09
Spell started in third quarter	0.209	-0.269	-2.27	-0.270	-1.39	0.819	-1.07
1992	0.069	0.044	0.13	0.310	0.65	0.902	-0.19
1993	0.104	-0.008	-0.00	-0.339	-0.72	1.203	0.34
1994	0.134	0.415	0.94	0.058	0.16	1.472	0.75
1995	0.135	0.237	0.58	-0.031	-0.02	1.312	0.52
1996	0.141	0.214	0.50	-0.360	-0.78	1.549	0.83
1997	0.139	0.196	0.48	0.214	0.45	1.110	0.20
1998	0.116	0.894	1.71	0.042	0.16	2.108	1.42
1999	0.088	0.751	1.51	0.967	1.79	1.265	0.45
2000	0.048	1.924	2.93	3.254	4.56	1.209	0.37
Predicted (relative) prob. of exit (in %)		0.843		1.872		45.032	

Note: The marginal effects of dummy variables are calculated as discrete changes in the expected value of the dependent variable. The "*t*-stat" is from the corresponding coefficients in Table A4 in the Appendix. The odds ratio is the exponential of the corresponding coefficient defining "exit into permanent contracts" as base category.

Reference category: men, cohabit with a partner, vocational training, no partner, no children, not disabled, German nationality, previous job quit, never an FTC job before, previous employment spell 1-2 month, not out-of-labour before, not training or school before, receives unemployment benefit or assistance, unemployment spell started in fourth quarter, calendar year is 1991, unemployed for only one month.

6 Results: Effects of Entering into Fixed-Term Contracts

For both definitions of 'non-treated' individuals, simple *NN*-matching (with caliper) is performed (see Table A6 in the Appendix).[48] The command 'psmatch' implemented by SIANESI (2001b) in STATA 7.0 is used in a modified form, which imposes the common support condition for every calendar month. The caliper is chosen by taking into account the trade-off between reducing the bias, on the one hand, and minimizing the loss of (treated) observations, on the other. The caliper is set in most cases to $\Psi = 0.03$ which turned out to be a reasonable compromise. In subsection 6.2 the mean effects are reported while subsection 6.3 presents effects for particular groups of individuals. The next subsection presents some checks on the matching quality.

6.1 Matching Quality

Is it plausible to assume that selection into FTC jobs is caused only by observable variables, that is, to consider the *CIA* to be fulfilled? Of course it seems unrealistic to assume that all variables affecting the FTC hazard and outcome variables simultaneously are included in the propensity score. Nevertheless, one may argue that the variables relating to individuals previous labour market experiences may capture unobserved characteristics of the unemployed. This statement seems to be confirmed by the estimation results.

Is the *NN*-matching estimator able to balance out observable pre-treatment differences between the treated and the control group of the non-treated? In order to answer this question some results for non-treated DEF.1 and outcome 2 one month after the transition to the FTC is presented in detail. The reason is that it is obviously more complicated to balance out differences in case of DEF.1 since there are fewer non-treated individuals available.

As mentioned above, the *common support condition* requires that for every unemployed individual entering into an FTC a sufficiently similar non-treated person in terms of the predicted propensity score should be available *for each single month*. The latter is necessary since matching is conditioned on the same calendar month, i.e. treated and control persons are matched only within the same calendar month. If for a treated person there is no sufficiently similar non-treated person available she or he cannot be used in the analysis and is excluded. Unfortunately, this procedure leads to a significant loss of observations (see Table 5). A further substantial reduction of observations is a result of imposing the non-treated to be within the caliper. As expected, less observations are lost in case of DEF.2. The difference of the samples of DEF.1 and DEF.2 implies that a comparison of the effects with regard to the non-treated definition is possibly not meaningful (see section 3.3 for a discussion).

48 Furthermore, several kernel-based matching estimators where checked (see sections 3.1 and 3.3). It turns out that the performance in terms of balancing out pre-treatment differences of *NN*-matching is much better than that of kernel-based matching estimators.

Table 5: Loss of treated observations due to common support requirement and lack of similar non-treated within the caliper (*NN*-matching, outcome 2)

Number of treatments (transitions to FTCs)	DEF.1	DEF.2
Before matching	349	349
Within common support	304	339
Within the caliper (after *NN*- matching)	239	282

Table 6: Means of important pre-treatment (X) variables before and after *NN*-Matching (DEF.1)

	Before matching				After matching			
	$\overline{X_1}$	$\overline{X_0}$	p-value[a]	std. diff. %[b]	$\overline{X_1}$	$\overline{X_0}$	p-value[a]	std. diff. %[b]
Propensity score	-3.695	-5.055	0.000	132.47	-3.998	-3.998	0.994	0.04
Dur. of unemployment	8.799	12.392	0.000	32.02	9.703	10.226	0.598	4.66
Female	0.453	0.512	0.028	11.92	0.477	0.498	0.648	4.19
Married	0.507	0.668	0.000	33.08	0.519	0.523	0.927	0.86
Married × female	0.206	0.335	0.000	29.36	0.222	0.238	0.665	3.80
Age	32.673	41.650	0.000	76.53	32.979	32.971	0.993	0.07
Prev job: end of FTC or apprenticeship	0.244	0.055	0.000	54.67	0.172	0.134	0.253	10.94
Prev job: end of FTC or apprenticeship × female	0.097	0.033	0.000	26.35	0.092	0.075	0.510	6.83
Dur. of previous unempl. spell	2.739	3.207	0.310	6.30	3.285	2.707	0.367	7.78
Dur. of previous empl. spell	13.146	16.579	0.091	10.43	13.431	15.489	0.473	6.25
Out-of-labour-force before	0.129	0.225	0.000	25.42	0.146	0.197	0.146	13.25
Training or school before	0.192	0.115	0.000	21.49	0.176	0.205	0.416	8.17
Public service before	0.206	0.073	0.000	38.89	0.172	0.106	0.018	19.42
University degree	0.163	0.042	0.000	40.83	0.084	0.059	0.287	8.44
No occupational qualification	0.301	0.439	0.000	28.84	0.360	0.377	0.705	3.50
Log household net income	6.793	7.302	0.000	20.48	7.008	6.939	0.765	2.77
No unemployment benefit	0.476	0.439	0.175	7.30	0.444	0.531	0.055	17.64
No unemployment assistance	0.883	0.815	0.001	18.87	0.849	0.870	0.511	5.86
Log unemployment rate	2.256	2.205	0.006	18.82	2.257	2.236	0.318	7.99
Number of obs	349	16,120			239	239		

Notes: The 16,120 observations in the non-treated group before matching correspond to 1,271 spells. [a]Two-sample *t*-test with unequal variance: H_0: $\overline{X}_1 - \overline{X}_0 = 0$. [b]Standardised differences defined as $\left|\bar{X}_1 - \bar{X}_0\right| \Big/ \left(\sqrt{\left(V_1(X_1)+V_0(X_0)\right)/2}\right)$.

In Table 6 the means of characteristics of treated $\overline{X_1}$ and non-treated workers $\overline{X_0}$, as well as the matched FTCs and controls are depicted. *T*-tests indicate that the differences in the means of nearly all covariates X are not significantly different from zero in the matched sample, while there are important differences before matching. This is confirmed by the *standardised differences* which are defined as the absolute difference of the sample means in the treated $\bar{X}_1$ and non-treated $\bar{X}_0$ sub-samples as a percentage of the square root of the average of the sample variances in the treated V_1 and non-treated V_0 groups: The standardized differences of all variables decline. Most important, there are no significant differences in the estimated propensity score anymore.

As mentioned in section 3.3 a further check on whether matching was successful in generating a suitable control group is to perform *t*-tests on the differences between the means of the outcome variables in both groups *before* the treatment. These differences are shown in the subsequent sections.

6.2 Mean Effects

In the following, the *TT* for outcome variables 1 up to 5 and for both definitions of non-treated are reported. First of all, the monthly measured outcome variables (2, 3, and 4) are reported, since they are useful to document the dynamics of the effects.

Outcome 2 – Employment probability 1991-2000

Figure 1 and Figure 2 present the difference in employment probabilities between treated and control persons including a 95 % confidence interval for non-treated DEF.1 and non-treated DEF.2, respectively. Note that employment includes permanent contract jobs and FTC jobs but excludes training. Zero on the time axis represents the month of treatment, i.e. the transition from unemployment to an FTC job. The insignificant effect before the treatment (-24 up to -3) indicates that the matching approach balanced pre-treatment differences in the employment status to a large extent. The matching quality seems to be slightly better in case of DEF.2, which can be explained by the fact that there are more potential controls available (see Table 1 in section 3.2). The figures suggest for both definitions that entering into FTC jobs increases the employment probability for up to 36 months after the transition. The similarities between the two definitions are surprising, given the dissimilarity of the concepts. The effects in case of DEF.2 seem to be slightly more positive.[49] Whether the estimated positive employment effect is due to FTC or permanent contract jobs is assessed in the following.

49 Note, that this is in line with the hypothesis that DEF.1 leads to an underestimation of positive effects (see the discussion in section 3.2 as well as FREDRIKSSON and JOHANSSON, 2003)

Figure 1: Employment effects – DEF.1

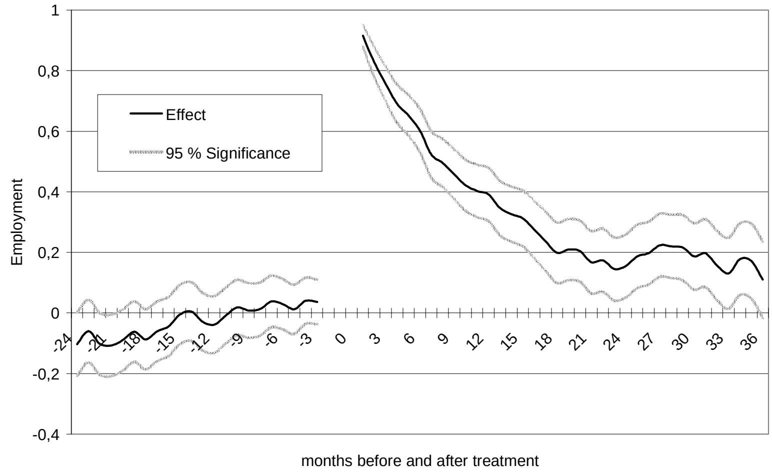
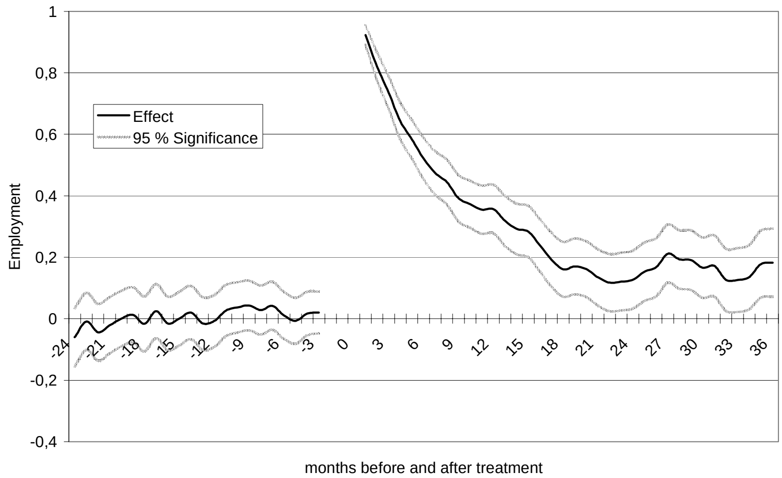


Figure 2: Employment effects – DEF.2



Outcome 3 – Unemployment probability 1991-2000

Again it can be seen in Figure 3 and Figure 4 that in the matched samples there are almost no significant differences in the pre-treatment probability for being registered unemployed for both non-treated definitions. The result that approximately 17 months after the transition there are no significantly negative effect on the unemployment probability anymore while Figure 2 and Figure 3 indicate that there are positive employment effects seems to be puzzling. The explanation has obviously to be found in the effects on the probability of being out-of-labour-force.

Figure 3: Unemployment effects – DEF.1

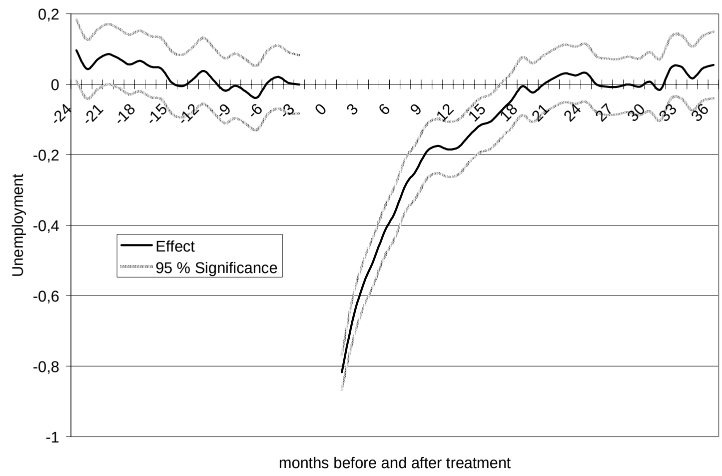
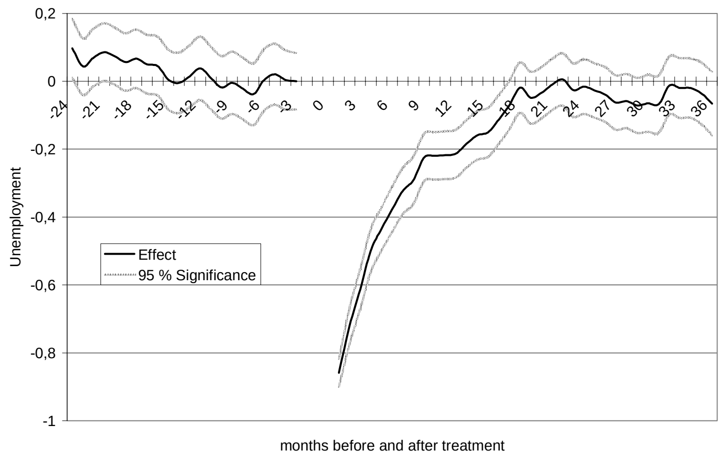


Figure 4: Unemployment effects – DEF.2



Outcome 4 – Probability of being out-of-labour-force 1991-2000

Indeed Figure 5 and Figure 6 indicate that entering into an FTC reduces the probability of being out-of-labour-force within the following 36 months. Therefore, one can conclude that the positive employment effect (outcome variable 2) is not accompanied by a lower probability of registered unemployment but by a reduced probability of being-out-of labour-force. Obviously this effect is likely to be driven by women, younger or elderly workers. It is likely that FTCs are used for people (re-)entering the labour market. If they become unemployed after the expiration of the contract they are more strongly attached to the labour market since they have qualified for unemployment compensation and have possibly enhanced their prospects which reduces the risk of being out-of-labour-force.

Figure 5: Out-of-labour-force effect – DEF.1

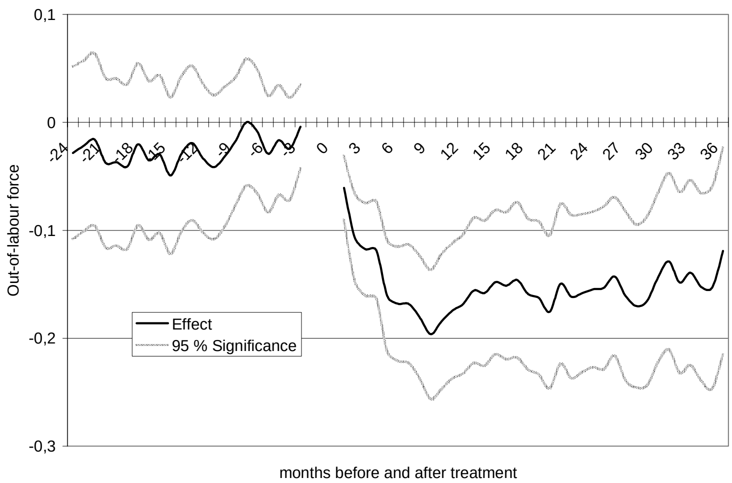
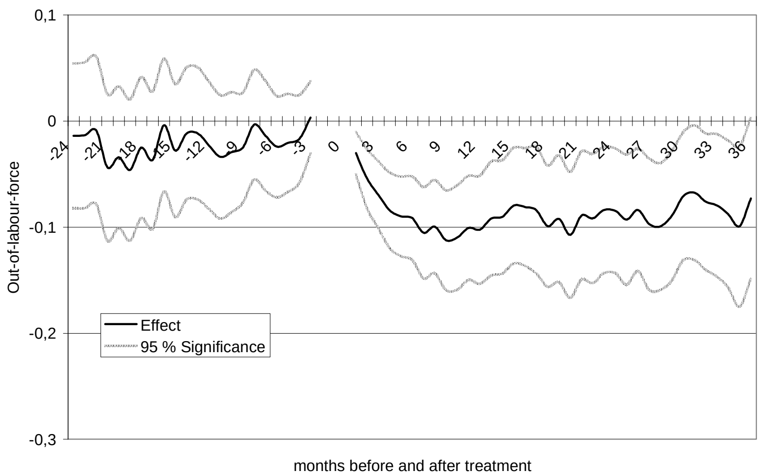


Figure 6: Out-of-labour-force effect – DEF.2



Outcome 1 – Probability of holding a permanent contract 1996-2001

Is there a positive employment effect only for FTC jobs or are individuals who have entered into an FTC also more likely to be employed as permanent contract workers after a given time? The effect on the probability of holding a permanent contract for non-treated DEF.1 is depicted in Table 7. First, it can be seen that even after matching there are some pre-treatment differences in the probability of holding a permanent contract between the treated and the control group, which are, however, not statistically significant. Nevertheless, one should keep in mind that there may be some selection bias left. Up to 3 years after the transition to an FTC, the formerly unemployed raise their probability of holding a permanent contract. But only after 3 years the positive effect of 11 percentage points is statistically significant. The effect four years after the transition is reported but should be interpreted with highest caution due to the loss of observations.

Table 7: Probability of being employed under a permanent contract – DEF.1

	All persons				
Year	Mean treated	Mean control	Effect	*t*-stat	Pairs
- 3	0.439	0.426	0.012	0.156	82
- 2	0.333	0.433	-0.100	-1.574	120
- 1	0.194	0.250	-0.056	-1.199	160
+ 2	0.448	0.437	0.011	0.214	174
+ 3	0.578	0.468	0.110	1.926	154
+ 4	0.575	0.500	0.075	1.090	106

Notes: Ψ = 0.03.

In Table 8 the effects for DEF.2 are reported. Again there are still some minor pre-treatment differences in the probability of holding a permanent contract, which are, however, smaller as in case of DEF.1. The effect is now more clear-cut: After three years 59.7% of all unemployed who had entered into FTCs hold permanent contracts. Only 43.5% of unemployed who had not entered into FTCs in a certain month (but possibly later) hold permanent contracts. The difference (*TT*) of 16.1 percentage points is highly significant. In year +4, the effect is still significantly positive at the 5% level.

Table 8: Probability of being employed under a permanent contract – DEF.2

	All persons				
Year	Mean treated	Mean control	Effect	*t*-stat	Pairs
- 3	0.377	0.349	0.028	0.424	106
- 2	0.312	0.347	-0.035	-0.619	144
- 1	0.194	0.246	-0.052	-1.230	179
+ 2	0.441	0.412	0.028	0.587	211
+ 3	0.597	0.435	0.161	3.119	186
+ 4	0.603	0.481	0.122	1.976	131

Notes: Ψ = 0.03.

Outcome 5 – Probability of being employed under an FTC 1996-2001

Outcome variable 5, i.e. the probability of holding an FTC, is the counterpart to outcome variable 1.[50] The matching procedure is able to balance out pre-treatment differences in the outcome variable to a large extent (see Table 9). It can be seen that entering into an FTC not only increases the probability of holding a permanent contract in the future but also to hold again an FTC.

Note that in the third and fourth year after the treatment the effect on the probability of holding a permanent contract (Table 7) is higher than the probability of holding an FTC (Table 9). The sum of the two effects (probability of holding an FTC and probability of holding a permanent contract) corresponds approximately to the overall employment effect (see Figure 2).

Table 9: Probability of being employed under an FTC – DEF.2

	All Persons				
Year	Mean treated	Mean control	Effect	*t*-stat	Pairs
- 3	0.104	0.075	0.028	0.715	106
- 2	0.118	0.083	0.035	0.969	144
- 1	0.136	0.115	0.021	0.614	191
+ 2	0.313	0.142	0.171	4.239	211
+ 3	0.134	0.097	0.037	1.123	186
+ 4	0.137	0.061	0.076	2.060	131

Notes: Ψ = 0.03.

6.3 Heterogeneous Effects

Although it is not necessary for the application of matching estimators to assume the *TT* of the transition to FTCs to be the same for all individuals, so far only mean effects of all individuals have been presented. It is, however, reasonable to suppose that the individual effects are heterogeneous across individuals.

Therefore, sub-samples are selected in advance and the estimation steps described above are performed on these samples. For many sup-samples, this approach turned out to be not suitable since the number of non-treated individuals per month became too small. Thus only a differentiation by a single characteristic is possible, i.e. the effects for women, individuals with formal qualification and individuals who are at least 32 years old, are estimated. Furthermore, only non-treated DEF.2 is used in order to maximise the number of non-treated individuals, which may be justified by the fact that there have not been great differences in the effects between the two definitions of non-treatment. Nevertheless, the number of pairs becomes quite small, as many treated persons have to be dropped since no suitable non-treated could be found within the support and the caliper. For this reason, the results should be interpreted with greatest caution and only as a "tendency".

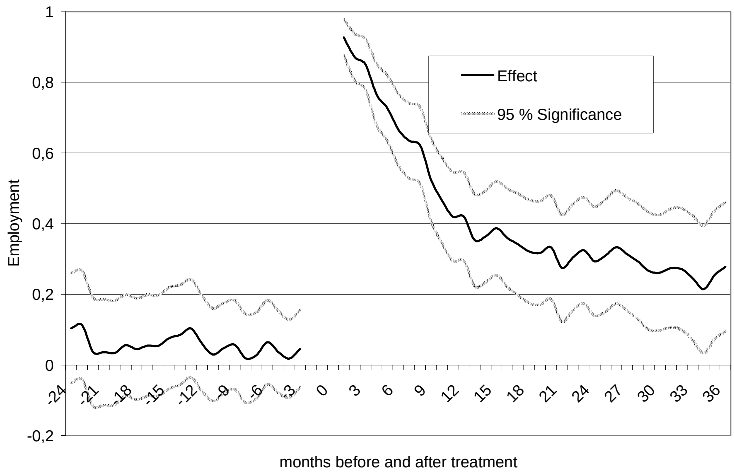
50 Remember that the outcome variable 5 is only defined for non-treated DEF.2 (see section 4).

Women

It can be seen in Figure 7 that the pre-treatment employment probability of those women entering into FTCs is always between 2 and 10 percentage points higher, which is, however, not statistically significant. Nevertheless, one can conclude that treated women (workers entering into FTCs) have ‘unobservable’ characteristics[51] which lead to on average slightly higher employment probabilities. Keeping this caveat in mind and comparing Figure 7 with Figure 2 one may conclude that the employment effects are higher for women than for men. Whether the same can be stated about the unemployment effects (Figure 8) is unclear.

A matter of particular interest are the effects on the probability of being-out-of-labour-force. Comparing Figure 9 with Figure 6 and taking only the point estimates into account, one may conclude that the negative effect is at least temporarily stronger for women. The same seems to be true for the probability of holding a permanent contract: While for the whole population the effect after three years is 16 percentage points (Table 8) the effect for women is approximately 22 percentage points (Table 10).

Figure 7: Employment effects – women (DEF.2)



51 Unobservable characteristics refers to a potential violation of the CIA: There are omitted (unobserved) variables which influence the selection into the FTC and the outcome.

Figure 8: Unemployment effects – women (DEF.2)

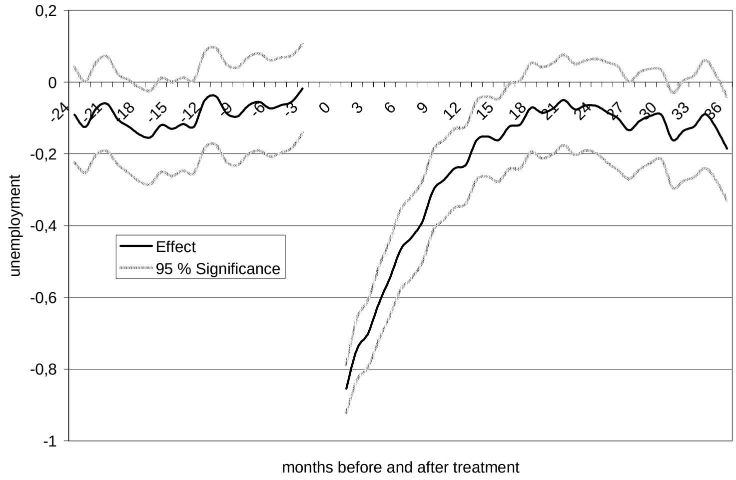


Figure 9: Out-of-labour-force effect – women (DEF.2)

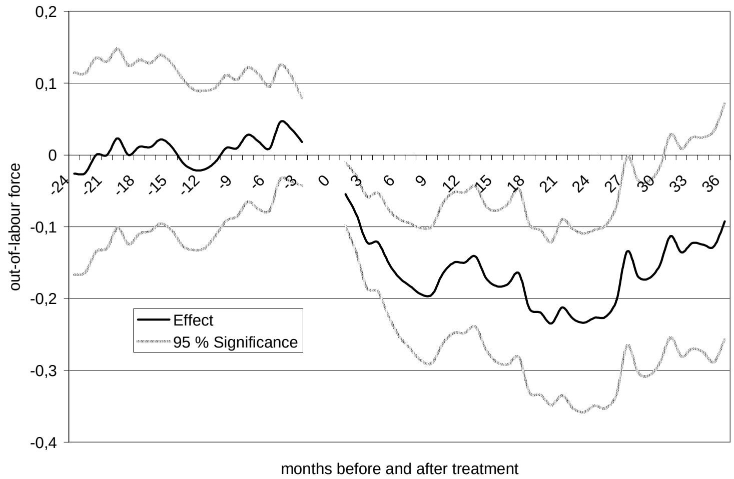


Table 10: Probability of being employed under a permanent contract / FTC – DEF.2

	Women								
	Permanent contract				FTC				
Year	Mean treated	Mean control	Effect	*t*-stat	Mean treated	Mean control	Effect	*t*-stat	Pairs
- 3	0.290	0.258	0.032	0.274	0.097	0.065	0.032	0.451	31
- 2	0.266	0.267	0.000	0.000	0.200	0.067	0.133	1.859	45
- 1	0.161	0.194	-0.032	-0.467	0.210	0.048	0.161	0.058	62
+ 2	0.537	0.373	0.164	1.903	0.194	0.030	0.164	3.087	67
+ 3	0.563	0.333	0.229	2.295	0.125	0.042	0.083	1.479	48
+ 4	0.600	0.429	0.171	1.435	0.114	0.029	0.086	1.392	35

Notes: Ψ = 0.03.

Age ≥ 32

It would be interesting to evaluate the effects of entering into an FTC on the employment situation of workers older than 58 years since they can be hired according to legal regulation without justification on FTCs. Unfortunately, there are not enough observations available since the mean age of unemployed entering into FTCs is about 32 years. Therefore, only workers who are at least 32 years old are used in the analysis. The results in comparison to the whole sample can be summarised as follows.

The employment effects seem to be similar to the effects for the whole sample (Figure 10 and Figure 2), even though the positive effects are often not significant which may be explained by the small sample size. Also the effects on the probability of being unemployed (Figure 11 and Figure 4) and the probability of being out-of-labour force are very similar (Figure 12 and Figure 6), whereas, again, the effects for the sub-sample are not statistically significant. There are some differences with respect to the effects on the probability of holding permanent contracts (Table 11 and Table 7) as well as probability of holding FTCs (Table 12 and Table 9). It seems, however, not to be possible to derive any clear-cut conclusion from this comparison.

Figure 10: Employment effects – age ≥ 32 (DEF.2)

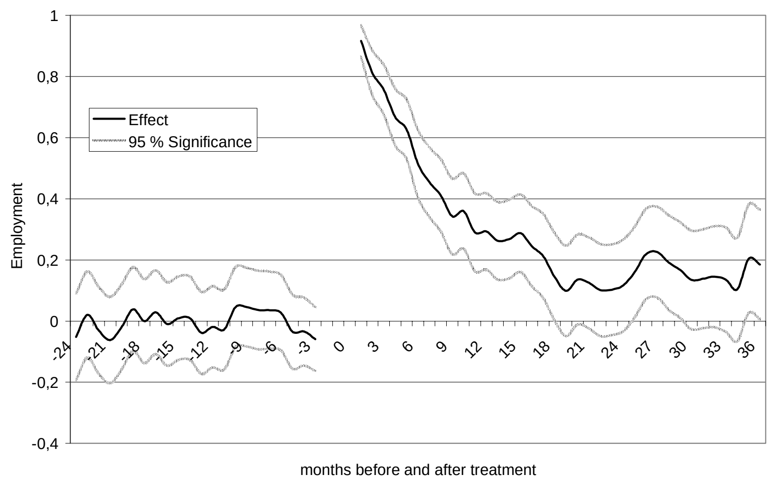


Figure 11: Unemployment effects – age ≥32 (DEF.2)

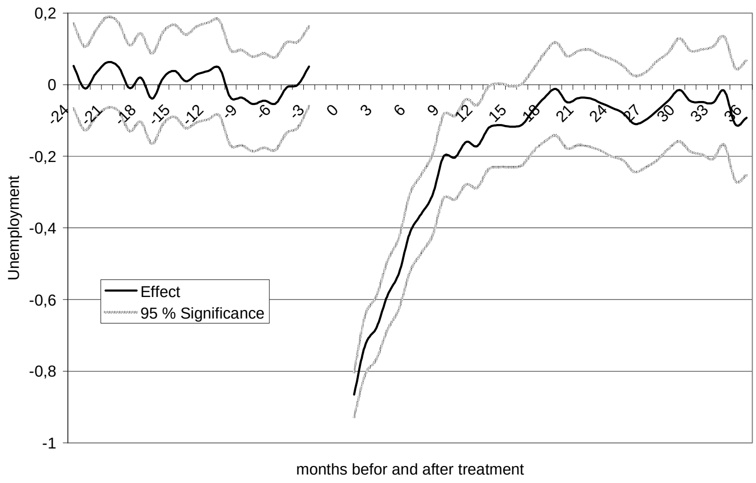


Figure 12: Out-of-labour-force effects – age ≥32 (DEF.2)

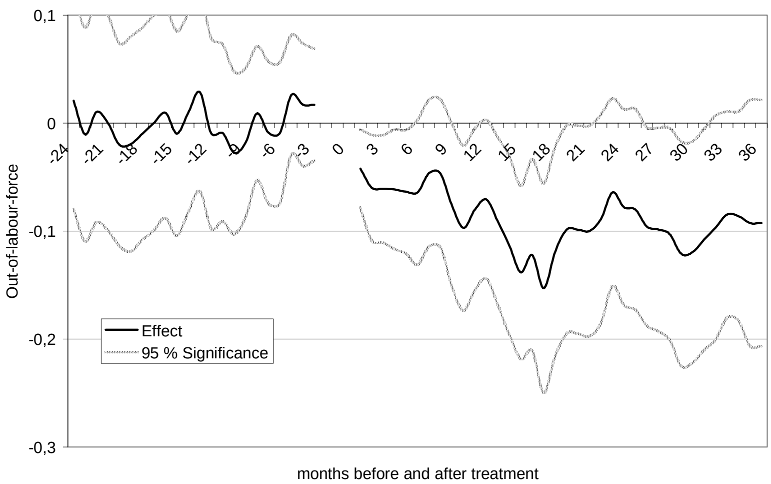


Table 11: Probability of being employed under a permanent contract / FTC – DEF.2

	Age ≥32								
	Permanent contract				FTC				
Year	Mean treated	Mean control	Effect	*t*-stat	Mean treated	Mean control	Effect	*t*-stat	Pairs
- 3	0.551	0.367	0.184	1.837	0.082	0.082	0.000	0.000	49
- 2	0.452	0.339	0.113	1.284	0.081	0.065	0.016	0.343	62
- 1	0.238	0.250	-0.013	-0.183	0.075	0.05	0.025	0.650	80
+ 2	0.533	0.373	0.160	1.981	0.147	0.067	0.080	1.589	75
+ 3	0.517	0.466	0.052	0.553	0.138	0.052	0.086	1.588	58
+ 4	0.541	0.405	0.135	1.159	0.108	0.054	0.054	0.844	37

Notes: Ψ = 0.03.

With formal qualification

In this subsection only those individuals are analysed which have completed at least a vocational training. This amounts to approximately 70% of all individuals entering into FTCs. Obviously, due to the number of observations it is not possible to focus on the unemployed without formal qualification which would be more interesting from a policy-orientated point of view. Comparing again the point estimates of the monthly measured outcome variables (Figure 14 with Figure 3, Figure 15 with Figure 5, and Figure 15 with Figure 7) one may conclude that the effect for the sub-sample is slightly better in terms of enhancing employment prospects. The conclusions from a comparison with respect to the type of contract are again unclear (see Table 12).

Figure 13: Employment effects – with formal qualification (DEF.2)

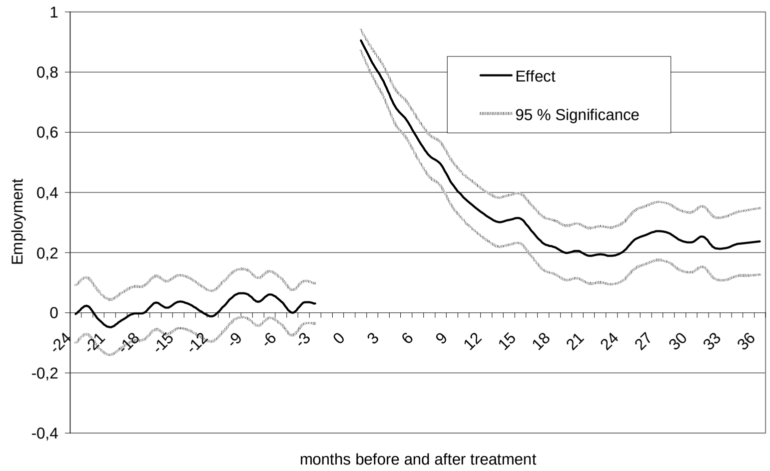


Figure 14: Unemployment effects – with formal qualification (DEF.2)

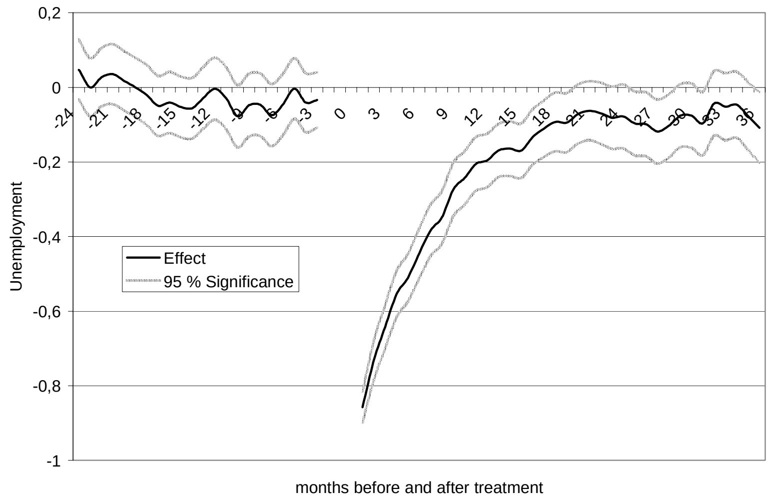


Figure 15: Out-of-labour-force effects – with formal qualification (DEF.2)

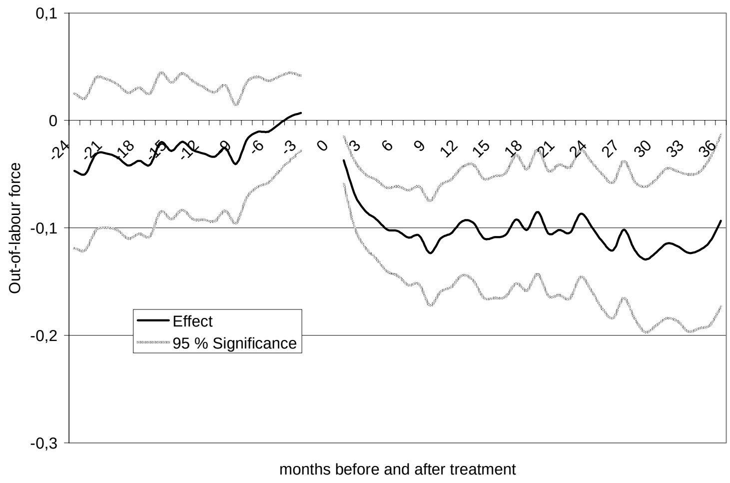


Table 12: Probability of being employed under a permanent contract / FTC – DEF.2

	At least vocational training								
	Permanent contract				FTC				
Year	Mean treated	Mean control	Effect	*t*-stat	Mean treated	Mean control	Effect	*t*-stat	Pairs
- 3	0.400	0.400	0.000	0.000	0.100	0.100	0.000	0.000	40
- 2	0.328	0.466	-0.138	-1.520	0.086	0.052	0.034	0.728	58
- 1	0.203	0.278	-0.076	-1.098	0.114	0.114	0.000	0.000	79
+ 2	0.599	0.401	0.198	3.624	0.138	0.060	0.078	2.363	167
+ 3	0.585	0.492	0.093	1.424	0.136	0.059	0.076	1.967	118
+ 4	0.533	0.453	0.080	0.976	0.147	0.004	0.106	2.268	75

Notes: Ψ = 0.016.

7 Conclusion

This paper has investigated the effects of the transition from unemployment to fixed-term contract (FTC) jobs on the individuals' future employment opportunities in the West German labour market. First, it has been discussed that there are at least two reasonable counterfactuals for individuals entering into FTC jobs after a certain unemployment duration. One counterfactual most commonly applied in evaluation studies is to compare a 'world with FTCs' with a 'world without FTCs' and define non-treated persons as unemployed who never (during the period covered by the data set) enter into FTCs. A second counterfactual which may be more in line with the idea of sequential job search (unemployed enter into FTCs after having failed to find a permanent job) is not to enter into an FTC up to a certain duration of unemployment, but possibly in a later month. This implies a comparison of unemployed entering into FTCs in a particular month of unemployment duration with those unemployed who do not enter into FTCs up to the end of this month. Both definitions have been analysed in the paper. Contrary to expectation, the estimation results differ only slightly which may, however, be driven by the small sample size. Second, it has been shown that the estimation of the propensity score by a hazard rate model is a suitable approach to address various practical problems since it is able to balance pre-treatment differences between treated and non-treated individuals in most cases and seems to be again more in line with sequential job search than the more common 'static' propensity score estimation.

The empirical findings can be summarised as follows. First of all, it should be kept in mind, that the results may be biased due to the design of the estimation sample which is caused by the way the information on the type of contract is collected in the GSOEP. Short FTC employment spells are likely to be underrepresented. While *NN*-matching is robust against the general case that treated units are over- or underrepresented with regard to the underlying population (choice-based sampling), the results may still be biased by the selectivity with regard to the duration of the employment spells.

The competing risk hazard rate model shows that typical characteristics which have been found in other studies to prolong unemployment duration such as disabilities, being a foreigner, being female and having children do not affect the FTC hazard rate, but have a negative effect on the permanent contract hazard. Thus FTCs may be "entry jobs" for unemployed with low employment chances.

Entering into FTCs increases the future employment probability (including FTC and permanent contract jobs) and the probability of holding permanent contract jobs and decreases the probability of being out-of-labour-force. These findings are compatible with the hypothesis that FTC may be stepping-stones towards permanent contract jobs.

Some results of the hazard rate model and the matching approach are in line with dual labour market theories: Having held an FTC increases the probability of entering into an FTC and holding an FTC in the future. Entering into an FTC has no long-term negative effect on the unemployment probability, i.e. the effect vanishes after 18 months.

Some more detailed analyses with regard to the heterogeneity of the effects revealed that unemployed women may benefit more from entering into FTCs than unemployed men. Slightly more positive effects have also been found for unemployed with formal qualification. These results should, however, be interpreted with caution since the samples for the analyses were quite small. More detailed analyses would be useful. To the best of my knowledge, there is, however, no larger panel data set for Germany available including information on the type of contract.

When interpreting the results one should, furthermore, keep in mind that this is a microeconometric study assuming that there are neither general equilibrium effects nor indirect effects from unemployed entering into FTCs on individuals who do not do so (see HECKMAN, LALONDE and SMITH, 1999). If employers were not allowed to hire workers on FTCs they would possibly hire the identical workers on permanent contracts. This behaviour is called *deadweight effects* in the literature on the evaluation of active labour market policy. This type of deadweight effects cannot be taken into account by the microeconometric approach applied here. In almost the same manner it would be misleading to conclude that FTCs reduce unemployment duration from a macroeconomic point of view. It is also possible that the re-employment probabilities of unemployed are *reduced,* since unemployed have to compete with FTC workers on existing vacancies (see BOERI, 1999), a mechanism which could be termed (analogously to active labour market programmes) *substitution effect*.

Thus, besides more detailed microeconometric research, studies based on aggregate data or general equilibrium models would be useful to gain further insight into the labour market effects of FTCs.

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Appendix

Table A1: Duration of continuous employment spells after the transition from unemployment to FTCs and permanent contracts

Employment spell starts with	Mean	Min	Max	Standard Deviation	25 percentile	Median	75 percentile
FTC	9.488	1	89	9.632	5	7	11
Permanent contract	18.141	1	107	19.733	5	10	23

Notes: The employment spells may include FTC and permanent contracts at different employers. The figures are based on the estimation sample of the duration models in Section 5. Right-censored employment spells are included which may bias the results.

Table A2: Duration of unemployment by kind of transition

Unemployment spell ends in	Mean	Min	Max	Standard Deviation	25 percentile	Median	75 percentile
FTC	8.766	1	79	9.078	3	6	11
Permanent contract	8.087	1	79	7.891	3	6	10
Training	8.936	1	60	9.027	3	6	11
Out-of-labour-force	12.589	1	85	12.486	4	9	15

Notes: The figures are based on the estimation sample of the duration models in Section 5.

Table A3: Means of explanatory variables by kind of transition

Variables	Right-censored unemployment spell	Exit into FTC	Exit into PERM	Exit into Training	Exit into out-of-labour-force
Baseline hazard					
Month 2 – 3	0.235	0.224	0.260	0.191	0.142
Month 4 – 6	0.115	0.198	0.196	0.221	0.176
Month 7 – 9	0.090	0.099	0.134	0.157	0.131
Month 10 – 12	0.038	0.093	0.081	0.089	0.144
Month 13 – 18	0.051	0.074	0.094	0.072	0.119
Month ≥19	0.056	0.105	0.066	0.094	0.174
Age	36.205 (10.436)	32.654 (9.562)	33.541 (10.103)	32.183 (9.988)	35.871 (0.463)
No occupational qualification	0.338	0.300	0.290	0.332	0.429
Master craftsman	0.047	0.040	0.065	0.064	0.018
University graduate	0.073	0.164	0.105	0.089	0.049
Female	0.397	0.456	0.437	0.498	0.621
Disabled	0.038	0.088	0.051	0.119	0.139
Foreigner	0.261	0.314	0.264	0.179	0.310

… Table A3 continued

Variables	Right-censored unemploy-ment spell	Exit into FTC	Exit into PERM	Exit into Training	Exit into out-of-labour-force
Married	0.620	0.504	0.528	0.426	0.600
Married x female	0.261	0.204	0.224	0.238	0.439
No partner	0.316	0.408	0.360	0.519	0.322
No partner x female	0.111	0.210	0.160	0.247	0.132
Children < 16	0.551	0.453	0.403	0.353	0.430
Children x female	0.244	0.218	0.152	0.213	0.327
Prev job: end of FTC or apprenticeship	0.103	0.242	0.142	0.136	0.112
Prev job: end of FTC or apprenticeship x female	0.073	0.097	0.068	0.068	0.054
Prev Job: dismissed	0.256	0.245	0.366	0.255	0.159
Prev Job: dismissed x female	0.077	0.103	0.136	0.123	0.104
Out-of-labour-force before	0.043	0.127	0.087	0.140	0.300
Out-of-labour-force before x female	0.030	0.071	0.049	0.098	0.219
Training or school before	0.081	0.190	0.170	0.302	0.161
Duration of prev. unemployment spell	2.560 (6.348)	2.711 (6.042)	2.666 (6.293)	2.732 (6.404)	3.104 (8.496)
Prev. employment spell 3-5 months	0.056	0.085	0.064	0.051	0.062
Prev. employment spell 6-8 months	0.103	0.085	0.049	0.034	0.060
Prev. employment spell 9-11 months	0.137	0.045	0.068	0.055	0.022
Prev. employment spell 12-20 months	0.081	0.065	0.077	0.055	0.033
Prev. employment spell > 20 months	0.218	0.181	0.237	0.149	0.129
Already ever an FTC before	0.150	0.269	0.151	0.115	0.102
Public sector before	0.085	0.204	0.108	0.089	0.118
Numb. of prev. unemployment spells	1.650 (2.408)	1.031 (1.714)	0.971 (1.680)	0.864 (1.320)	0.692 (1.156)
Log net household income	6.379 (2.987)	6.801 (2.703)	7.005 (2.471)	7.401 (2.016)	7.545 (1.961)
No unemployment benefit	0.449	0.473	0.445	0.468	0.628
No unemployment assistance	0.910	0.884	0.925	0.898	0.930
Log regional unemployment rate	2.217 (0.271)	2.252 (0.271)	2.193 (0.284)	2.242 (0.294)	2.228 (0.270)
Spell started in first quarter	0.269	0.261	0.263	0.260	0.397
Spell started in second quarter	0.217	0.215	0.261	0.187	0.240
Spell started in third quarter	0.215	0.241	0.242	0.370	0.191
Number of transitions	234	349	767	235	597
Number of persons	219	321	683	214	533

Notes: The figures are based on the estimation sample of the duration models in Section 5. Standard deviations of metric variables are in parentheses.

Table A4: Estimation Results of the multinomial logistic duration model

	Exit into FTC		Exit into PERM		Exit into Training		Exit into out-of-labour-force	
	Coef.	*t*-stat.	Coef.	*t*-stat.	Coef.	*t*-stat.	Coef.	*t*-stat.
Baseline hazard								
Month 2 – 3	0.573	3.39	0.958	8.04	0.603	2.74	0.600	3.60
Month 4 – 6	0.527	3.01	0.782	6.15	0.790	3.66	0.780	4.85
Month 7 – 9	0.355	1.65	0.893	6.29	0.964	4.05	0.875	5.05
Month 10 – 12	0.869	3.88	0.922	5.51	0.916	3.25	1.453	8.38
Month 13 – 18	0.593	2.43	1.059	6.57	0.613	2.01	1.211	6.65
Month $\geq$19	0.835	3.50	0.770	4.08	0.808	2.67	1.320	7.36
Age	-0.160	-0.76	-0.156	-1.09	-0.423	-1.73	-0.101	-0.70
Age^2 / 1.000	0.639	1.10	0.602	1.55	1.403	2.04	0.067	0.17
Age^3 / 100.000	-0.083	-1.60	-0.076	-2.23	-0.151	-2.45	0.005	0.14
No occupational qualification	-0.399	-2.96	-0.444	-4.73	-0.362	-2.21	-0.148	-1.55
Master craftsman	-0.168	-0.58	0.147	0.90	0.228	0.79	-0.863	-2.76
University graduate	0.474	2.61	0.263	1.89	0.155	0.60	-0.246	-1.18
Female	-0.017	-0.06	0.119	0.54	-0.651	-1.61	0.345	1.24
Disabled	0.111	0.54	-0.584	-3.36	0.461	2.08	0.527	3.92
Foreigner	0.060	0.44	-0.364	-3.78	-0.696	-3.74	-0.320	-3.13
Married	0.226	0.94	-0.041	-0.25	-0.125	-0.43	-0.202	-0.92
Married x female	-0.370	-1.13	-0.060	-0.27	0.386	0.95	0.371	1.33
No partner	-0.252	-1.10	-0.310	-1.96	0.024	0.09	0.058	0.24
No partner x female	0.532	1.65	0.310	1.38	0.816	2.09	-0.559	-1.94
Children < 16	-0.104	-0.56	-0.003	-0.02	-0.494	-2.02	-0.206	-1.23
Children x female	0.002	0.01	-0.616	-3.51	0.197	0.63	0.365	1.83
Prev job: end of FTC or apprenticeship	0.738	3.60	0.252	1.49	0.020	0.07	0.478	2.29
Prev job: end of FTC or appr. x female	-0.535	-1.86	-0.213	-0.91	-0.194	-0.47	-0.626	-2.18
Prev Job: dismissed	0.252	1.31	0.638	5.41	0.049	0.21	-0.430	-2.10
Prev Job: dismissed x female	-0.053	-0.19	-0.279	-1.61	0.153	0.47	0.232	0.93
Out-of-labour-force before	0.536	1.93	-0.128	-0.59	-0.262	-0.70	0.400	2.02
Out-of-labour-force before x female	-0.631	-1.82	-0.462	-1.67	0.033	0.08	-0.281	-1.29
Training or school before	0.460	2.39	0.217	1.62	0.435	2.00	0.080	0.50

… Table A4 continued

	Exit into FTC		Exit into PERM		Exit into Training		Exit into out-of-labour-force	
	Coef.	*t*-stat.	Coef.	*t*-stat.	Coef.	*t*-stat.	Coef.	*t*-stat.
Duration of prev. unemployment spell	-0.018	-1.68	-0.010	-1.46	-0.020	-1.57	-0.015	-2.41
Prev. employment spell 3-5 months	0.805	3.43	0.399	2.27	0.131	0.39	0.518	2.54
Prev. employment spell 6-8 months	0.595	2.53	-0.099	-0.51	-0.363	-0.91	0.598	2.91
Prev. employment spell 9-11 months	0.320	1.09	0.781	4.44	0.585	1.78	0.128	0.42
Prev. employment spell 12-20 months	0.267	1.05	0.401	2.43	0.084	0.25	0.078	0.30
Prev. employment spell > 20 months	0.229	1.30	0.368	3.35	-0.018	-0.08	0.027	0.18
Already ever an FTC before	0.405	2.69	-0.159	-1.34	-0.333	-1.44	-0.039	-0.25
Public sector before	0.363	2.25	-0.071	-0.53	-0.397	-1.57	-0.109	-0.77
Numb. of prev. unemployment spells	-0.066	-0.78	-0.016	-0.28	0.216	1.58	0.090	1.02
Numb. of prev. unemployment spells^2	0.020	1.90	0.016	2.20	-0.028	-1.09	-0.009	-0.56
Log net household income	-0.058	-2.65	-0.022	-1.38	0.050	1.40	0.046	2.01
No unemployment benefit	0.328	2.62	0.482	5.59	0.209	1.35	0.946	9.52
No unemployment assistance	0.997	5.19	1.293	8.40	1.065	4.33	1.842	10.49
Log regional unemployment rate	-0.068	-0.28	-0.604	-3.65	-0.044	-0.15	-0.104	-0.56
Spell started in first quarter	-0.682	-4.46	-0.514	-4.69	-0.318	-1.57	-0.172	-1.36
Spell started in second quarter	-0.625	-3.88	-0.418	-3.77	-0.378	-1.75	-0.253	-1.87
Spell started in third quarter	-0.356	-2.27	-0.155	-1.39	0.512	2.69	-0.169	-1.20
1992	0.065	0.13	0.168	0.65	0.369	0.75	0.398	0.99
1993	-0.002	-0.00	-0.186	-0.72	-0.978	-1.76	0.680	1.75
1994	0.428	0.94	0.041	0.16	-0.031	-0.06	0.301	0.76
1995	0.266	0.58	-0.006	-0.02	0.140	0.29	0.408	1.04
1996	0.231	0.50	-0.207	-0.78	-0.230	-0.47	0.053	0.13
1997	0.226	0.48	0.121	0.45	0.031	0.06	0.379	0.94
1998	0.789	1.71	0.043	0.16	0.117	0.24	0.522	1.30
1999	0.706	1.51	0.471	1.79	0.582	1.20	0.838	2.11
2000	1.342	2.93	1.152	4.56	1.366	2.93	1.381	3.49
Constant	-3.719	-1.45	-1.874	-1.07	-1.501	-0.50	-4.663	-2.55
Numb. of Spells	349		767		235		597	
Numb. of Persons	321		683		214		533	
Numb. of Observations	23,151							
Log-likelihood	-8166.4056							
χ^2 (216)	2096.67							

Table A5: Logistic hazard rate model (propensity score estimation)

	Exit into FTC	
	Coef.	*t*-stat.
Baseline hazard		
Month 2 – 3	0.475	2.82
Month 4 – 6	0.429	2.46
Month 7 – 9	0.231	1.08
Month 10 – 12	0.710	3.20
Month 13 – 18	0.420	1.74
Month $\geq$19	0.691	3.00
Age	-0.188	-0.92
Age^2 / 1.000	0.730	1.29
Age^3 / 100.000	-0.090	-1.80
No occupational qualification	-0.329	-2.51
Master craftsman	-0.165	-0.58
University graduate	0.460	2.59
Female	0.294	1.61
Married	0.327	2.01
Married x female	-0.698	-3.06
Prev job: end of FTC or Apprenticeship	0.554	2.97
Prev job: end of FTC or Apprenticeship x female	-0.457	-1.69
Out-of-labour-force before	0.612	2.28
Out-of-labour-force before x female	-0.725	-2.16
Training or school before	0.445	2.38
Duration of prev. unemployment spell	-0.016	-1.54
Prev. employment spell 3-5 months	0.763	3.31
Prev. employment spell 6-8 months	0.596	2.56
Prev. employment spell 9-11 months	0.300	1.03
Prev. employment spell 12-20 months	0.291	1.16
Prev. employment spell > 20 months	0.247	1.41
Already ever an FTC before	0.495	3.38
Public sector before	0.393	2.48
Numb. of prev. unemployment spells	-0.048	-0.59
Numb. of prev. unemployment $spells^2$	0.015	1.48
Log net household income	-0.058	-2.71
No unemployment benefit	0.281	2.31
No unemployment assistance	0.935	4.93

… Table A5 continued

	Coef.	*t*-stat.
Log regional unemployment rate	0.117	0.57
Spell started in first quarter	-0.684	-4.48
Spell started in second quarter	-0.645	-4.01
Spell started in third quarter	-0.402	-2.58
Constant	-3.523	-1.46
Numb. of Spells		349
Numb. of Persons		321
Numb. of Observations (person month)		23,151
Log-likelihood		-1645.004
χ^2 (37)		330.62

Table A6: Implementation of the propensity score matching estimator

1.	Estimate a discrete (logistic) hazard rate model for the transition from unemployment in FTC (independent competing risk). Include all covariates X and a non-parametric (piece-wise constant) specification for the base-line hazard
2.	Predict the propensity score $e \equiv \Pr\left(T_k = t, C = 1 \middle| X(t), T_k \geq t\right)$ for every individual i (with T = duration of the unemployment spell and k=number of unemployment spells)
3.	For non-treatment DEF.1: Exclude individuals who are treated at any time during the period from the pool of 'non-treated' persons
4.	Impose common support condition for every month: Drop observations outside the support and the caliper
5.	*NN-Matching*: For every calendar month τ to each treated person the non-treated person with the closest propensity score e two months before the actual transition to FTC occurs is matched (with replacement). *Kernel-Based Matching*: For every calendar month τ to each treated person all non-treated person two months before the actual transition to FTC occurs are matched. Assign to every matched non-treated person a weight which depends on the distance between the propensity scores e.
6.	The effect is the difference in the (weighted) mean outcomes of the two groups