Discussion Paper No. 01-71

Do Temporary Workers Receive Risk-Premiums?

Assessing the Wage Effects of Fixed-Term Contracts in West-Germany by Matching Estimators Compared with Parametric Approaches

Tobias Hagen

ZEW
Zentrum für Europäische
Wirtschaftsforschung GmbH
Centre for European
Economic Research

Discussion Paper No. 01-71

Do Temporary Workers Receive Risk-Premiums?

Assessing the Wage Effects of Fixed-Term Contracts in West-Germany by Matching Estimators Compared with Parametric Approaches

Tobias Hagen

Non-technical summary

The theory of compensating differentials states that higher risk of unemployment or uncertain prospects in general are compensated by higher wages. If workers with fixed-term contracts (FTCs) are bearing a higher risk they may obtain higher wages. On the other hand the theory of dual labour markets specifies conditions under which temporary workers earn less than permanent workers even if they have the same productivity.

Until now there does not exist any German empirical study which investigates the wage effects of FTCs and takes into account the selection bias. Since the selection into permanent or temporary contracts is not random, it is important to account for the selection mechanism in order to estimate unbiased effects of FTCs on wages. Furthermore, the 'classical' selection problem due to the fact that wages can only be observed for those persons who participate in the labour market has to be considered.

Another intention of the paper is to compare propensity score matching estimators with parametric approaches. Matching estimators, which are usually applied for the evaluation of active labour market policies, turn out to be well suited, since they allow to include variables in the balancing score, which are endogenous with regard to the type of contract. The estimation results differ between the methods. Small negative effects of FTCs which are not statistically significant are found with matching, while the effects estimated by the parametric approach are unrealistically large. Another interesting result of this study is that important characteristics of the workers like human capital or sex do not determine the probability of FTCs. The workers' individual (un-)employment histories seem to be more important.

Do Temporary Workers Receive Risk-Premiums?

Assessing the Wage Effects of Fixed-Term Contracts in West-Germany by Matching Estimators Compared with Parametric Approaches

Tobias Hagen

Centre for European Economic Research (ZEW)
P.O. Box 10 34 43
D- 68034 Mannheim

Abstract

The wage effects of fixed-term contracts (FTCs) are analysed with the GSOEP for West Germany. Different estimators which take into account selection bias are used. It is shown that propensity score matching estimators which are usually applied for the evaluation of active labour market programmes are well-suited for the analysis of this topic, whereas parametric approaches find unrealistically large negative effects. The empirical evidence rejects compensating wage differentials as well as strong negative wage effects of FTCs.

***Key Words*:** Fixed-term Employment, Wage Differentials, Propensity Score Matching
***JEL classification*:** C14, C35, J31

Acknowledgement: This paper would not have been possible without the helpful comments on econometric issues by Frank Reize. I also thank Susanne Steffes for her assistance.

* Phone: +49 / 621 / 1235 -288, Fax: +49 / 621 / 1235 -225, e-mail: hagen@zew.de

1 Introduction

Fixed-term contracts (FTCs) define temporary employment relationships, which expire automatically without dismissal at the end of the agreed term.[1] Therefore it seems to be plausible to expect FTC workers to bear a higher risk of unemployment and discontinuity than permanent workers.

Which effect do FTCs have on individual wages? The well-known theory of compensating differentials states that disadvantages among work activities are equalised by wage differentials (Rosen 1986). Higher risk of unemployment or general uncertain prospects of the future working life due to an FTC may be compensated by a higher wage. For example, a worker with an FTC may receive a higher wage that equalizes the loss of the expected value of the redundancy pay.

On the other hand there are also reasons for negative effects of FTCs on wages. For example, the dual labour market theory predicts that workers with higher turnover are paid less. Temporary contracts may also serve as a prolongation of the probationary period. In this case low-paid FTC jobs may be a kind of sorting mechanism: only workers with high productivity will accept temporary contracts since they have a higher probability of getting a permanent position with higher wages afterwards.

While available studies find some evidence for compensating wage differentials for jobs with higher unemployment risk in the U.S. (see Rosen 1986), so far only negative effects on wages have been found for temporary contracts. Booth et al. (2000) find that FTC workers in Britain earn less than permanent workers (men 8.9% and women 6%). However, for some workers FTCs seem to be a ‘stepping stone’ for a permanent job, i.e. there is some compensation in the long-run.

Although there are some German empirical studies available which investigate the wage differentials between FTC and permanent workers they ignore potential selection bias (see for example Schömann / Kruppe 1994; Groß 1999). Since the selection into permanent or temporary contracts is not random it is important to account for the selection mechanism in order to estimate unbiased effects of FTCs on wages. Furthermore, the ‘classical’ selection problem due to the fact that wages can only be observed for those persons who participate in the labour market should be considered (Heckman 1979). To my knowledge, this paper is the first empirical attempt to examine the effect of FTCs on wages taking the selection bias into account. Therefore, I do not analyse whether FTCs are associated with higher or lower wages but whether the effect of FTCs are higher or lower wages.

Another intention of the paper is to compare matching estimators with traditional parametric techniques. Matching Estimators, which are usually applied for the evaluation of active labour market policies, turn out to be well suited, since they allow to include variables in the balancing score, which are endogenous with re

1 See Schömann et al. (1995) and Boockmann / Hagen (2001) for a further description of the institutional background in Germany

gard to the type of contract. The estimation results differ between the methods. With propensity score matching small negative effects of FTCs, which are not statistically significant, are found, while the effects estimated by traditional maximum likelihood models are unrealistically large. Another interesting result of this study is that important characteristics of the workers like human capital or sex do not determine the probability of FTCs. The workers' individual (un-)employment histories seem to be more important.

2 Theoretical Considerations

This section provides theoretical explanations for wage effects of temporary contracts and discusses reasons why workers enter FTCs.

2.1 Wage effects

A worker with an FTC will – in a competitive labour market with mobility between jobs and perfect information – receive a higher wage that just offsets the loss of the expected value of the redundancy pay (Booth et al. 2000). The compensating differential depends on the probability that the worker receives a permanent contract afterwards, the worker's attitude towards risk, the unemployment insurance and the formation of expectations (see Abowd / Ashenfelter 1981; Topel 1984). Also firms may be willing to pay temporary workers the present value of the expected institutional firing costs which would be incurred if these workers had a permanent contract. However, there are a number of reasons why temporary workers may not receive a compensating differential in the form of higher wages than permanent workers.

- The dual labour markets theory derives conditions under which the wages for temporary workers may be lower even if temporary and permanent workers are perfect substitutes. In the tradition of the Shapiro / Stiglitz (1984) efficiency wage models, Rebitzer / Taylor (1991) show, by assuming that the monitoring of the workers is costly and the product demand is uncertain, that wages paid to permanent workers exceed those paid to temporary workers.[2]
- FTC workers and their employers have lower incentives to invest in firm-specific human capital which leads to lower wages (and probably also other long-term negative prospects). In contrast to the formal qualification like schooling and apprenticeship, the effective investment in firm-specific human capital can be observed only incompletely in most data sources, so it might be difficult to distinguish between the direct effect of temporary employment and the indirect effect via the lack of investment in human capital.

2 Daniel / Sofer (1998) combine the dual labour market theory with the theory of compensating wage differentials. They find empirical evidence for compensating differentials in low unionized sectors.

- Firms view the initial temporary contract as a probationary stage. Depending on the job performance and labour demand, workers will move into permanent employment within the firm. Low-paid temporary jobs can even be attractive to workers with high ability. As pointed out by Loh (1994) probationary periods (with lower wages) may induce self-selection of those workers with higher ability because they have a higher probability of getting permanent contracts. Temporary contracts with lower wages are therefore a sorting instrument for firms. Low wages during the temporary contract period will be compensated for by higher future wages at the same employer.
- Temporary workers may be outsiders since their bargaining position is weakened due to the lack of institutional firing costs.

2.2 Why do workers accept job offers with temporary contracts?

Assuming that temporary workers are not contemporaneously compensated by higher wages and that they might therefore be worse off, the question arises why workers accept job offers with temporary contracts.[3]

A first explanation is that particular jobs are only available with temporary contracts. For example flexible schedules or part-time jobs in order to meet family, school or other non-work responsibilities may be only available as temporary jobs. Regional immobility may force persons to accept FTCs even if they would get permanent contracts in other regions. In Germany many academic positions, especially for young people, are only available in combination with FTCs. Persons may accept job offers with FTCs to meet temporary declines in family income, particularly when other family members may be laid off. Those reasons for "involuntary" temporary contracts are obviously more decisive in economic downturns.

Additional explanations are possible if one assumes asymmetric information, i.e. important characteristics of the worker are unobservable for the employer. A temporary contract may be a kind of prolonged probationary period which allows firms to obtain information that is unavailable before hiring and that serves as a check on the quality of the match between workers and job. As already mentioned, this may induce a positive selection. The model of Loh (1994) predicts that in a competitive market setting, workers with greater ability (which is unobservable before hiring) migrate to firms offering jobs with probationary period, and those with lower ability migrate to firms offering jobs with no probationary period, since the formers face lower risks of losing the job.

If employers are uncertain about the unobservable characteristics of the employees, the individuals' employment history may serve as signal. References from previous employers but also the reputation of previous employers may include information on the unobservable characteristics of the worker. If the previ

3 Firms' reason of employing temporary workers are analysed in Boockmann / Hagen (2001).

ous employment history involves 'bad' signals and there are no alternative applicants available the employer will hire the worker on a temporary contract. Important signals may be labour market experience and the duration of previous employment or unemployment spells.

These considerations are compatible with empirical evidence from the German Microcensus for 1997 (see Hagen 2001). Workers are questioned about the reasons for being employed with an FTC. 20.7% of all FTC workers are in the probationary period, 19.1% cannot find a permanent job, 5.0% do not want a permanent job and 55.3% have other reasons.[4]

3 Methodological Issues

Which effect do FTCs have on wages? This question can be restated: How much would workers with permanent contracts earn if they had FTCs instead? In order to answer these questions one can apply methods which are used for the evaluation of active labour market programs (for a survey see Heckman et al. 1999; Hagen / Steiner 2000).

3.1 The Evaluation Problem

What is the causal effect of a treatment 1 (an FTC), relative to another treatment 0 (permanent contract), on the outcome variable Y?

Let Y_1 be the outcome (hourly wage) 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 (permanent contract). $C \in \{0,1\}$ is a dummy variable indicating if the treatment is actually received. The outcome Y may be employment, employment stability, satisfaction, state of health or wages, for example. For an individual i, the actually observed outcome is therefore $Y_i = Y_{i0} + C_i\left(Y_{i1} - Y_{i0}\right)$.

The parameter of interest is the *average effect of the treatment*, which is given by

$$E\left(Y_1 - Y_0 \mid C=1\right) = E\left(Y_1 \mid C=1\right) - E\left(Y_0 \mid C=1\right). \tag{1}$$

This measures the change in the outcome of the participants which is caused by the fact that they participate ($C=1$). The last term in (1) describes the hypothetical average outcome *if the participants had not participated*. Of course, this term

4 Without the public sector and employees in vocational training. There are interesting differences between skilled and unskilled workers: While 24.2% of all unskilled FTC workers have their temporary contract due to the probationary period only 14.5% of the skilled FTC workers are in the probationary period (see Hagen 2001).

is not observable and has to be estimated either by a before-after comparison using only participants or a control group of non-participants or a combination of both. In this paper only a control group of non-participants is generated. However, the average value of the outcome of the non-participants typically does not represent the correct average of non-treatment outcome since participants and non-participants differ in characteristics which influence the outcome variable,

$$E(Y_0 \mid C=1) \neq E(Y_0 \mid C=0). \tag{2}$$

Equation (2) states that using non-participants as an estimate for the hypothetical situation a participant had not participated is in general not valid, since both groups differ due to observable and unobservable characteristics giving rise to a selection bias. The treated individuals 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. The following sections shows how matching (section 3.2) and parametric approaches (section 3.3) deal with this problem.

3.2 Statistical Matching

Let Z be a vector that describes observable attributes of the individuals which are not affected by the treatment, like sex and age. The statistical matching estimator may solve the problem of selection bias (due to differences in observable characteristics) by imposing the *Conditional Independence Assumption* (CIA)

$$(Y_0, Y_1) \perp C \mid Z \tag{3}$$

where $\perp$ denotes independence. This assumption justifies the use of matched non-participants (workers with permanent contracts) to measure what participants (workers with FTC) would earn, on average, if they had not participated (had a permanent contract). Obviously, the vector Z should contain all the variables that are thought to simultaneously influence participation and outcome. If this condition is fulfilled one can assume

$$E(Y_0 \mid C=1, Z) = E(Y_0 \mid C=0, Z). \tag{4}$$

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

Especially if the vector Z is large and contains many continuous variables it may become very unlikely to find for every combination of Z a match between all persons of the treatment with a person of the non-treatment group (‘curse of dimensionality’; Heckman et al. 1997).

However, as Rosenbaum / Rubin (1983) show it is sufficient to match participants and non-participants on the conditional probability of participation given the vector of observed characteristics. This conditional probability of participation $e(Z) \equiv \Pr\{C = 1|Z\}$ is called *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 Z. So (4) can be written as

$$E\big(Y_0 \mid C = 1, e(Z)\big) = E\big(Y_0 \mid C = 0, e(Z)\big) \tag{5}$$

The propensity score $e(Z)$ can be estimated by standard parametric approaches like the probit or logit model (Dehejia / Wahba 1999). In this paper it is estimated by probit.

Since any transformation of the propensity score which preserves the order of the observations is sufficient for the matching estimator, the predicted linear index $Z\hat{\beta}$ rather than the predicted probability $\Pr(C = 1|Z)$ is used (see Lechner 1998, 115). Thereby individuals in the tails of the distribution can be distinguished more exactly. Nevertheless, in the following I will also use the term propensity score for the linear index $Z\hat{\beta}$.

Implementation of the propensity score matching estimators

For each person *i* in the treatment group, a (group of) comparable persons is found. Matches are constructed on the basis of a neighbourhood $\mathbb{C}(e_i)$, where e_i is the propensity score for person *i*. It is assumed that N_0 is the number of observations in the comparison sample and N_1 is the number of observations in the treatment sample. Thus the persons in the comparison sample who are neighbours to *i*, are persons *j* for whom $e_j \in \mathbb{C}(e_i)$, i.e. the set of persons $A_i = \{j | e_j \in \mathbb{C}(e_j)\}$.

The effect of treatment for each observation *i* in the treatment group is estimated by subtracting the weighted average of the outcome of control group observations from the outcome of the treatment observation *i* (see Heckman et al. 1999, 1953):

$$Y_{1i} - \sum_{j=1}^{N_0} w(i,j)\, Y_{0j} \tag{6}$$

Matching estimators differ especially 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-neighbours matching

Nearest-neighbours matching defines A_i such that only the control *j* is selected that is closest to e_i in some metric:

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

where $\| \ \|$ is a metric measuring the distance in the Z. Equation (7) states that the non-participant with the value of e_j that is nearest to e_i is selected as a match and is defined as a control. This selected non-participant is attached with the weight $w(i,j)$=1.

Nearest-neighbours matching can be executed 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 (Sianesi 2001). It is also possible to use more than one nearest neighbour ('oversampling'). Nearest-neighbours matching has generally the disadvantage that 'bad matches' are likely if the closest neighbour is far away.

Caliper matching

Caliper matching – a version of nearest-neighbours matching – may reduce the risk of 'bad matches' (see Cochran / Rubin 1973). For a pre-specified level of tolerance $\Psi > 0$, the treated individual *i* is matched to the non-treated unit *j* such that:

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

The corresponding neighbourhood is $A_i = \left\{ X_j \mid \left\| e_i - e_j \right\| \right\}$.

If none of the non-treated units is within the Ψ of th treated individual *i*, the individual *i* is left unmatched and is not used for the estimation. This leads to a modified sample, since treated persons are left out. Therefore, the estimated effect should be interpreted only on the basis of the sample used.

Mahalanobis Metric Matching

Using additional variables (a subset of variables included in *Z* and assumed to be important) separately besides the propensity score may decrease the selection bias and may be an additional protection against any impact due to inconsistent estimation of the propensity score (Lechner 1998). The propensity score in combination with the additional variables is called *balancing score* $b(Z)$ (Rosenbaum /

Rubin 1983). The matching on the balancing score is performed by the Mahalanobis distance (see Rubin 1980; Lechner 1998):

$$\left\| b_i - b_j \right\| = \left(b_i - b_j \right)' \Sigma^{-1} \left(b_i - b_j \right) , \tag{9}$$

where b_i and b_j are the balancing scores and Σ is the covariance matrix formed from the $C = 1$ sample.

Advantages of Matching

What are the advantages of matching estimators over parametric estimators? A basic requirement for a bias-removing implementation of the matching algorithm is a sufficiently large overlap between the distribution of the propensity scores of the treated and non-treated persons. This is called the *common support condition* and means that for every treated person a sufficient similar non-treated person is available.[5] The condition ensures that one does not compare the 'incomparable' (Heckman et al. 1997, 647). This condition is always met in social experiments. As stressed by Heckman et al. (1997) an unbiased application of matching methods is only possible inside the range of common support of the distribution of $Z\hat{\gamma}$ of the treatment and non-treatment group.[6] The common support condition is essential for the matching estimator but not for traditional parametric techniques. Parametric approaches can be used to predict the expected outcome even in regions of the variable space where no observation can occur (Lechner 2000). This fact is assessed as an advantage of matching.

The most obvious advantages of matching estimators in comparison to other methods is that it is a non-parametric technique which avoids the definition of a specific form for the outcome equation and the selection equation and which does not require to assume any specification of unobservables in both equations. This also means that variables on which the matching is performed directly may be endogenous with regard to the participation decision. In the empirical application of this paper the duration of the actual employment (job tenure) is an endogenous variable in a probit estimation for the probability of being employed with an FTC. Therefore in section 6.4 matching is performed directly on this variable (besides the propensity score) and job tenure is omitted form the propensity score estimation.

However, because of the parametrical estimation of the propensity score (with probit or logit) these advantages are reduced in practise. The whole estimation

5 More formally this means $0 < \Pr(C=1 \mid Z) < 1$.

6 According to Heckman et al. (1999) there are in general three possible sources of selection bias. The first one is the difference in the support of Z in the treated and control group, the second bias appears due to the difference between the two groups of the distribution of Z over its common support and the reason for the third bias is selection on unobservables.

becomes inconsistent if there are any specification errors in the selection equation. Even so, propensity score matching has still the advantage not to impose any functional forms and not to rely on any distributional assumptions in the outcome equation. This also means that heterogeneous individual treatment effects are allowed, i.e. the functional form of the effect is not assumed to be a constant additive term for every individual like in the parametric model which is presented in the next section.

3.3 Control Function Estimator

The so called control function approach motivated by Heckman (1978, 1979) is a full maximum likelihood estimator which evaluates the effect of an endogenously chosen dummy variable (indicating any 'treatment' like training, union membership or temporary contract) on another endogenous continuous variable, conditional on two sets of independent variables.

The primary equation is

$$Y_i = \beta' x_i + \delta C_i + \varepsilon_i \tag{10}$$

where C_i is the binary variable indicating whether the individual *i* belongs to the treatment group (have an FTC; C_i = 1) or belongs to the control group (have a permanent contract; C_i = 0). C_i is assumed to be derived from an unobservable latent variable

$$C_i^* = Z_i \gamma + u_i \quad . \tag{11}$$

The decision to be in the treatment group (to have an FTC) is determined by the rule

$$C_i = \begin{cases} 1, & \text{if } C_i^* > 0 \\ 0, & \text{otherwise} \end{cases} \tag{12}$$

where the error terms ε and u are bivariate normally distributed with mean zero and correlation ρ.

The expected outcome for the treatment group can be written as

$$E\left[Y_i \mid C_i = 1\right] = \beta' x_i + \delta + E\left[\varepsilon_i \mid C_i = 1\right] \tag{13}$$

$$= \beta' x_i + \delta + \rho \sigma_\varepsilon \lambda\left(-Z_i \gamma\right),$$

where σ_ε is the standard deviation of ε and λ is the hazard (see Maddala 1983).

For non-participants the counterpart to equation (13) is

$$E\left[Y_i \mid C_i = 0\right] = \beta' x_i + \rho\sigma_\varepsilon \left[\frac{-\phi(Z_i\gamma)}{1-\Phi(Z_i\gamma)}\right], \tag{14}$$

where ϕ is the standard normal density, and Φ is the standard normal cumulative distribution function. The difference in expected outcome between participants and non-participants is,

$$E\left[Y_i \mid C_i = 1\right] - E\left[y_i \mid C_i = 0\right] = \delta + \rho\sigma_\varepsilon \left[\frac{\phi(Z_i\gamma)}{\Phi(Z_i\gamma)\left(1-\Phi(Z_i\gamma)\right)}\right]. \tag{15}$$

It can be seen that if the selection correction is omitted like in OLS regressions which treat the type of contract as exogenous, the second term on the right-hand side drops out. If the correlation between the error terms, ρ, is zero, there is no selection bias (the effect can be estimated by OLS) and the difference is simply δ.

Although this model seems to have the advantage to be logically specified since it can be derived as a structural approach it has been criticised for some of its assumptions which are crucial for identification. One needs at least one w variable in the selection equation (11) which is not included in the x variables of the outcome equation (10).[7] Furthermore, the model's consistency is based on the joint normal distribution of ε and u which is arbitrary. Another critical point is that the endogenous dummy variable is assumed to be a shift-parameter in a given outcome function. Therefore, it is assumed that the functional form of the outcome equation is the same for the participants and non-participants and that the treatment (the contract) has a linear effect.[8]

3.4 Probit Model with Sample Selection

In order to estimate the effect of FTCs on wages consistently it may be necessary to take another source of selection bias into account: The decision whether to have a permanent or an FTC job can only be observed for those individuals who participate in the labour market. Therefore, the estimation of the determinants of FTCs may be biased if unobserved characteristics influencing the participation in the labour market are the same as those affecting the selecting into FTCs.

7 In theory the model can still be estimated in the absence of exclusion restrictions through identification by functional form (Fitzenberger / Prey 1998). However, identification of the model then rests entirely on the assumptions on the joint distribution of the error terms.

8 An extension of this model which allows different outcome functions for the participants and the non-participants is the endogenous swichting model. An comparable approach – which produces generally similar estimates – is one with instrumental variables (see Vella / Verbeek 1999).

To check the significance of this problem, a maximum-likelihood probit model with sample selection – similar to that proposed by Heckman (1979) for continuous variables – is estimated (see Van de Ven / Van Pragg 1981). In addition to the probit estimation for the FTCs a selection equation for participation in the labour market is simultaneously estimated. The error terms of both equations are assumed to be normally distributed. If the correlation of the error terms ρ is not zero a separate estimation of the probit for FTCs leads to biased results. This is checked by performing a likelihood-ratio test comparing the likelihood of the full model (the simultaneous estimation of the probit and the participation equation) with the sum of the likelihoods for the separate estimated probit and participation model. Similar to the model described in the last section for identification, one variable in the participation equation is needed which is not included in the probit estimation.

4 Dataset: The German Socio-Economic Panel

The analysis is mainly based on the wave 1999 of the German Socio-Economic-Panel (GSOEP). The GSOEP is a representative household survey of the German population, conducted on a yearly basis.[9] It contains information about the kind of labour contract (fixed-term vs. permanent) and whether the FTC is due to public employment measure. The latter persons are excluded from the analysis. Besides, in order to restrict the heterogeneity, only those persons who are working in West-Germany and have the German citizenship are used. Self-employed, military servants and those who are on an apprenticeship training are removed from the sample. Furthermore, the sample is restricted to persons not younger than 30 years and not older than 60 years to secure that the labour force participation is sufficiently large.

Since average hours worked differ slightly between FTC and permanent contracts, hourly wages rather than (monthly) earnings are analysed. Hourly wages are calculated from the information on individual gross earnings and actually worked hours in the previous month. Fringe benefits like 13th month pay, holiday or Christmas bonuses are not taken into account because this information cannot consistently be combined with the information on hourly wages (Steiner / Wagner 1997).

A very useful feature of the GSOEP is the availability of monthly information between yearly interviews. Different employment and income states are covered. This information is collected by retrospective questions about what happened in particular months of the previous year. From these data the duration of the previous employment spell as well as the previous unemployment spell are generated. I

9 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/).

use only the last employment or unemployment spell which ended within the previous two years.

5 Determinants of FTCs

In this section the determinants of FTCs are analysed by estimating a probit model. This is used in the subsequent sections as a propensity score or for the control function estimation, respectively. Besides the usual characteristics like gender, age, marital status, children, human capital and profession, variables controlling for the employment history are included.

Unfortunately there are not enough observations available to perform the analysis of wage effects for men and women separately. However, gender differences are considered in the propensity score and the selection equation of the control function approach estimation by including interaction terms of important variables with the women dummy. Besides, as the following results show, gender seems to have no important impact on the probability of being employed with an FTC.

As already mentioned, previous unemployment and employment spells may include important unobserved variables like 'ability' or 'motivation' (see Heckman et al. 1997 in the context of labour market training). Furthermore, these variables may be signals for employers. In order to capture these effects the duration of the previous unemployment spell and the duration of the previous employment, which ended within the last two years, are included in the selection equation. Furthermore, a dummy variable indicating if a person has never had a job before or has been out of labour force before and a dummy variable for a change of employer within the previous two years are included.

An important variable which may represent the probationary period is the duration of the actual employment. However, this variable is obviously endogenous, since the duration of a FTC is ceteris paribus shorter than the duration of a permanent contract. For this reason, the variable is omitted from the probit regression and is included directly in the balancing score of the matching estimator and the outcome equation of the parametric approach instead.

The probit model is augmented by a sample selection equation for the labour market participation. The labour market participation equation includes human capital variables, gender, the number of children, age, the employment status of the spouse, dummy variables indicating the attitude to work and a dummy variable indicating whether the person has to pay interest payments. Additionally there are interaction terms with the female dummy included. Descriptive statistics for all the variables can be found in Table A1 in the appendix.

The estimation results can be seen in Table 1. Especially the results for the formal qualification are unexpected. The coefficients are insignificant and joint tests also show that formal qualification does not affect the probability of FTCs. The results for the dummy variables indicating blue collar workers and professionals[10] support the conjecture that FTC employees are very heterogeneous. Both variables have a positive impact.

The individual employment history seems to be very important: The longer the previous unemployment spell the higher is the probability of an FTC, and the longer the previous employment spell (at one employer) the lower the probability of an FTC. More than one explanation seems to be suitable for this result. Short previous employment spells may be a proxy for previous FTCs. If this was true there would be a kind of state dependence which would reject a stepping stone theory. The individual (un-)employment history may also capture characteristics like ability or motivation which cannot be observed in the data directly. This would mean that persons with higher ability have longer (shorter) employment (unemployment) spells and a lower probability of being employed with an FTC. A further explanation suggests that employers hesitate to hire workers with an unstable labour market history on a permanent position since they suppose that these workers have only low ability, given the observable characteristics like qualification and age.[11] Compatible with this explanation are the results for the dummy variables indicating if the person has changed his or her employer or entered new employment after having been out of labour force within in the previous year. The highly significant results of these dummy variables can also be explained by the probationary period, which has always been a legally accepted reason for the use of FTCs.

Although the estimated positive effect of the dummy variable for women is not significant at 10 percent level there is some (also insignificant) evidence that experience lowers the probability of FTCs for women, but not for men. The probability of civil servants to be employed with an FTC decreases significantly with their experience. The three variables for experience (the experience variable and the interaction with women and civil servants) are significant at 1 per cent in a joint likelihood-ratio test. One may conclude that labour market experience serves as a positive signal for civil servants (with regard to their unobservables), which increases their probability of being employed with a permanent contract. There are also some differences between different firm sizes. Workers in firms with 200 to 1999 employees seem to have the highest probability for FTCs.

10 Professionals correspond with the International Standard Classification of Occupation from the International Labour Office in Geneva and include physical, mathematical and engineering science professionals, life science and health professionals, teaching professionals, business, legal and social science professionals as well as writers and artists.

11 This result is also compatible with the result of Farber (1999) for the U.S., who finds that job losers are significantly more likely than non-losers to be employed in temporary jobs. However, in this analysis dummy variables indicating whether the worker was dismissed from the last job or quit the last job voluntarily have no significant effect.

Table 1: Determinants of FTCs taking participation decision into account

FTC			Labour market participation		
	Coeff.	t-stat		Coeff.	t-stat.
Constant	-2.193	-5,71	Constant	-6.055	-5.20
Women	0.512	1.35	Women	1.765	1.14
qualification			*Qualification*		
(Reference: No formal qualification and low school degree)			(Reference: No formal qualification and low school degree)		
Intermediate school degree	0.066	0.47	Intermediate school degree	-0.185	-2.19
High school	0.082	0.39	High school	-0.101	-0.73
Apprenticeship	-0.126	-0.83	Apprenticeship	-0.084	-0.82
Graduate	-0.149	0.224	Graduate	0.779	4.84
			Intermediate school × women	0.009	2.44
Dur. of previous unemployment	0.024	2.56	High school × women	0.016	2.14
Dur. of previous employment	-0.004	-3.50	Apprenticeship × women	0.003	0.74
Experience	0.000	0.02	Graduate × women	-0.020	-2.32
Blue collar worker	0.315	2.17	*Attitude to work*		
Professionals	0.280	0.163	Work is very important	0.165	3.07
Civil service	0.874	2.27	Work is unimportant	-0.850	-10.85
Experience × civil service	-0.029	-1.90	Interest payments	0.340	3.28
Experience × women	-0.018	-1.26	Spouse employed	0.089	1.07
New employer	1.171	7.39	Spouse employed × women	0.152	1.33
Out of labour force before	0.804	3.55	# of children	-0.031	-0.65
			# of children × women	-0.338	-5.31
Firm size			Married	0.448	3.38
(Reference: 1-19 employees)			Married × women	-0.930	-4.87
Firm size 20 – 199	0.266	1.71	Divorced	-0.282	-1.61
Firm size 200 – 1999	0.450	2.65	Divorced × women	0.220	0.95
Firm size 2000 –	0.061	0.18	Age	0.350	6.68
			Age^2	-0.004	-7.74
			Age × women	-0.103	-1.49
			Age^2 × women	0.001	1.79
LR tests of Joint Significance	χ^2	*P-val.*	*LR tests of Joint Significance*	χ^2	*P-val.*
Experience	9.83	0.021	Attitude to work	153.96	0.000
Firm size	2.39	0.496	# of children	77.60	0.000
			Qualification	70.20	0.000
			Age	229.27	0.000
			Divorced	2.76	0.252
			Spouse employed	10.71	0.005
Number of uncensored observations: 2,114			Number of observations: 3,501		

Log-Likelihood –2109.258

Wald test : χ^2 (20) = 136.69 (p-value: 0.000)

$\hat{\rho}$ = 0.03 (Std. Err.: 0.206)

LR test of independence of the equations (ρ =0) : χ^2 (1) = 0.19 (p-value: 0.6650)

Notes: The joint tests include all interaction terms.
Sources: German Socio-Economic Panel

A number of other variables like income of the spouse, marital status, number of children, distance between job and place of residence and regional unemployment rate were included in the equation for FTC and turned out to be insignificant.[12]

12 The distance between the job and the place of residence may be a proxy for a utility increasing or decreasing attribute of a job. A utility gain of a job which is near the place of residence may be traded-off

Finally, an important finding for the further analysis is the result of the likelihood-ratio test of the independence of the equations (and therefore the significance of the correlation of the disturbances ρ). Since the null hypothesis of no correlation is not rejected, the estimated probit for FTCs is not significantly different from the outcome which would be obtained by estimating a probit for FTCs not taking into account the participation decision. Therefore, one can conclude that it is not inconsistent to estimate the determinants of FTCs separately.

Table 2: Determinants of FTCs

	Coeff.	t-stat
Constant	-2.041	-7.44
Dur. of previous unemployment	0.023	2.49
Dur. of previous employment	-0.004	-3.66
Experience	-0.007	-0.74
Blue collar worker	0.319	2.36
Professionals	0.277	1.85
Civil service	0.952	2.50
Experience × civil service	-0.031	-2.08
Experience × women	0.003	0.56
New employer	1.167	7.42
Out of labour force before	0.839	3.74
Firm size		
(Basis: 1-19 employees)		
Firm size 20 – 199	0.255	1.66
Firm size 200 – 1999	0.445	2.65
Firm size 2000 –	0.011	0.03
LR tests of Joint Significance	χ^2	P-val.
Experience	8.35	0.040
Firm size	7.49	0.058
Log-Likelihood –696.198		
Wald test : χ^2 (28) = 1585.57 (p-value: 0.000)		
Number of Observations: 1,914		

Sources: German Socio-Economic Panel

The separate probit estimation for FTCs, which will be used for the further analysis, is depicted in Table 2. The groups of variables which are in joint tests insignificant in the probit with sample selection are excluded. Again, several additional variables were tested but they were all insignificant. As expected, the results are only slightly altered in comparison to the probit model with sample selection in Table 1.

against the utility loss of a temporary contract. The regional unemployment rate is a proxy for aggregate labour demand. Low labour demand may force workers to enter FTCs.

6 Wage Effects of FTCs

6.1 Empirical Wage Equation

For the parametric estimation of the wage effects of FTCs one has to specify an outcome equation. For this purpose a standard Mincer equation which is augmented by a dummy variable for FTCs and interaction terms is used:[13]

$$
\begin{aligned}
y_i = \alpha_0 + \beta_1' SKILL_i + \beta_2 EXP_i + \beta_3 EXP_i^2 + \beta_4' \left(SKILL_i \times EXP_i \right) \\
+\beta_5 FEM_i + \beta_6 FEM_i \times EXP_i + \beta_7 TEN_i + \beta_8 TEN_i^2 + \Gamma' Z_i + \delta C_i + \varepsilon_i \qquad (16)
\end{aligned}
$$

where y_i = natural log of gross hourly wages of individual i
$SKILL$ = vector of educational and vocational dummy variables
EXP = labour market experience (years)
EXP^2 = labour market experience (years) squared
FEM = dummy for women
TEN = job tenure (months)
TEN^2 = job tenure (months) squared
Z = vector of firm size and industry dummies
C = dummy for fixed-term contracts
ε = error term with $\varepsilon_i \sim N\left(0, \sigma^2\right)$, $E\left(X\varepsilon\right) = 0$
and X = all explanatory variables.

Several additional variables and interaction terms were checked. But they turned out to be insignificant.[14]

The skill structure is captured by dummy variables for the educational and vocational qualification and years of labour market experience. After performing several likelihood-ratio tests, the following skill dummy variables are used. (1) no vocational training and low school degree (reference category), (2) no vocational training and intermediate school degree, (3) no vocational training and high school, (4) vocational training or master craftsman and (5) polytechnical or university degree. As usual, labour market experience is defined as *age – years of schooling* – 6, where years of schooling are derived from information on the highest educational and vocational degree (see Rosen 1992).[15]

13 For further explanations of this equation see Franz (1999) and Card (1999).

14 For example regional dummies and interactions between civil servants and labour market experience were checked.

15 The years of schooling are assumed to be as follows. Without any educational degree 7 years, low school degree (Hauptschule) 9 years, intermediate school degree (Realschule) 10 years, qualification for studies at a polytechnic (Fachhochschulreife) 12 years, high school (Abitur) 13 years, apprenticeship (Lehre) 1,5 years, polytechnic (Fachhochschule) 3 years, university 5 years.

Labour market experience is viewed as a proxy for human capital accumulation through training and learning on the job. Since a hypothesis of the human capital theory states that earnings increase with experience with a decreasing rate, one should estimate $\beta_2>0$ and $\beta_3<0$ (see Lauer / Steiner 2000). Interaction terms between educational and vocational qualification reflect that experience effects may depend on the skill-level. For the interaction term between woman and experience it is expected that $\beta_6<0$ (see Mincer / Polachek 1974).

Additionally, the duration of the actual employment relationship with the employer (job tenure) is included. This may – more than labour market experience – capture the effects of the accumulation of firm-specific human capital (Topel 1991), and, more important, the effects of (formal and informal) probationary periods. As already mentioned it is not possible to include this variable in the probit model for FTCs. By including job tenure in the wage equation directly, the bias due to the omission of this variable from the probit equation may be reduced. Again, it is plausible to expect $\beta_7>0$ and $\beta_8<0$.

6.2 Estimating the wage effects of FTCs treating the type of contract as exogenous

In this section a simple OLS Regression treating the contract as exogenous is performed. The estimation results are presented in Table 3. Descriptive statistics for the variables can be found in Table A2 in the appendix. The main finding is that FTCs have significant negative effects on wages. FTC workers earn about 9.97 percent less per hour than workers with FTCs.[16] The results for human capital and experience are comparable with those found in other studies for Germany (see Steiner / Lauer 2000).

However, the results may not only be biased due to the endogeneity of the type of contract but also due to the 'classical' sample selection bias (Heckman 1979). This may occur for the same reason as described in section 3.4 for the choice of the type of contract: The wage is only observed for those persons who participate in the labour force. It is likely that the error terms of the labour market participation equation and the wage equation are correlated. If so, the sample of individuals observed as working do not represent the underlying population. If the selection bias is not recognised, the estimation of the parameters in the wage equation (and also the estimated effect of FTCs) are inconsistent.

16 $-9.97 = [(\exp(-0.105)-1)\times 100]$.

Table 3: Wage effects of FTCs

	Coeff.	t-stat
Constant	2.717	20.73
Women	-0.001	-0.03
Qualification		
(reference: No formal qualification and low school degree)		
Intermediate school degree	-0.093	-1.43
High school	0.101	1.04
Apprenticeship	-0.037	-0.57
Graduate	0.219	2.39
Experience / 100	1.270	1.70
Experience² / 100	-0.026	-2.39
Experience / 100 × women	-0.590	-3.32
Experience / 100 × intermediate school degree	0.519	2.36
Experience / 100 × high school	0.301	0.84
Experience / 100 × apprenticeship	0.356	1.61
Experience / 100 × graduate	0.162	0.48
Job tenure / 100	0.135	6.21
Job tenure² /1000	-0.014	-3.04
Firm size		
(Reference: 1-4 employees)		
Firm size 5 – 19	0.134	3.16
Firm size 20 – 199	0.234	6.02
Firm size 200 – 1999	0.291	7.28
Firm size 2000 –	0.392	4.79
FTC	-0.105	-2.48
F tests of Joint Significance	*F*	*p-Val*
Industry dummies	14.17	0.000
Qualification	9.59	0.000
Experience	2.35	0.051
Firm size	20.66	0.000
R^2	0.454	
Number of Observations	1,914	

Notes: 10 industry dummies are included in the wage equation but not reported. The joint tests include all the interaction terms. Robust Standard Errors.

Sources: German Socio-Economic Panel

Therefore, the wage equation (16) is re-estimated by the Heckman (1979) estimator (full maximum-likelihood) taking into account the labour market participation. For the participation equation the same variables are used as in section 5. The results are depicted in Table 4. Since the likelihood-ratio test does not reject the null hypothesis of independence of the equations sample selection bias seems to be no problem. The result of this test is in line with the finding that the estimated coefficients are not altered in comparison with the OLS estimation. The results are also compatible with other German studies which do not find evidence for sample selection bias (Lauer / Steiner 2000). For this reason, labour market participation is not considered in the following analyses.

Table 4: Wage effects of FTCs taking labour market participation into account

log wages			**Labour market participation**		
	Coeff.	t-stat		Coeff.	t-stat.
Constant	2.753	20.12	Constant	-6.383	-5.36
Women	0.011	0.21	Women	1.597	1.00
Qualification			*Qualification*		
(Reference: No formal qualification and low school degree)			(Reference: No formal qualification and low school degree)		
Intermediate school degree	-0.091	-1.40	Intermediate school degree	-0.190	-2.19
High School	0.095	1.00	High school	-0.085	-0.60
Apprenticeship	-0.042	-0.67	Apprenticeship	-0.094	-0.89
Graduate	0.205	2.33	Graduate	0.786	4.81
Experience / 100	1.042	1.27	Intermediate school × women	0.010	2.64
Experience^2 / 100	-0.022	-1.70	High school × women	0.016	2.11
Experience / 100 × women	-0.603	-3.38	Apprenticeship × women	0.004	0.98
Experience / 100 × interm. school	0.516	2.32	Graduate × women	-0.019	-2.15
Experience / 100 × high school	0.323	0.88	*Attitude to work*		
Experience / 100 × apprenticeship	0.371	1.72	Work is very important	0.156	2.86
Experience / 100 × graduate	0.201	0.58	Work is unimportant	-0.842	-10.42
Job tenure / 100	0.135	6.47	Interest payments	0.349	3.28
Job tenure^2 /1000	-0.015	-3.04	Spouse employed	0.103	1.20
Firm Size			Spouse employed × women	0.168	1.43
(Reference: 1-4 employees)			# of children	-0.022	-0.45
Firm size 5 – 19	0.135	4.32	# of children × women	-0.349	-5.36
Firm size 20 – 199	0.234	8.54	Married	0.429	3.18
Firm size 200 – 1999	0.291	9.72	Married × women	-0.930	-4.80
Firm size 2000 –	0.392	6.25	Divorced	-0.274	-1.55
FTC	-0.105	-2.87	Divorced × women	0.214	0.91
			Age	0.350	6.76
			Age^2	-0.004	-7.78
			Age × women	-0.098	-1.37
			Age^2 × women	0.001	1.65
LR tests of Joint Significance	χ^2	p-Val	*LR tests of Joint Significance*	χ^2	p-Val
Industry dummies	144.72	0.000	Attitude to work	139.07	0.000
Qualification	330.04	0.000	# of children	72.26	0.000
Experience	9.17	0.057	Qualification	73.61	0.000
Firm Size	120.61	0.000	Age	224.84	0.000
			Divorced	2.55	0.2796
			Spouse Employed	12.53	0.0019

Number of uncensored observations: 1,914 Number of observations: 3,301

Log-Likelihood –2167.843

Wald test : χ^2 (29) = 1,347.08 (p-value: 0.000)

= 0.03 (Std. Err.: 0.206)

LR test of independence of the equations (=0) : χ^2 (1) = 0.19 (p-value: 0.6650)

Notes: 10 industry dummies are included in the wage equation but not reported.
The joint tests include all interaction terms. The results are described in the text.

Sources: German Socio-Economic Panel

6.3 Control function estimator

The contract type in equation (16) is now treated as endogenous. Therefore, the control function estimator described in section 3.3 is applied. The estimation results are depicted in Table 5.

Table 5: Wage effects of FTCs

log wages			FTC		
	Coeff.	t-stat		Coeff.	t-stat.
Constant	2.747	21.42	Constant	-2.050	-7.02
Women	-0.013	-0.26	Dur. of previous unemployment	0.025	1.99
Qualification			Dur. of previous employment	-0.005	-3.72
(reference: No formal qualification			Experience / 100	-0.62	-0.65
and low school degree)			Blue collar worker	0.443	2.91
Intermediate school degree	-0.095	-1.46	Professionals	0.265	1.69
High school	0.097	1.04	Civil service	1.164	2.96
Apprenticeship	-0.040	-0.64	(Experience /100) × civil service	-3.547	-2.28
Graduate	0.217	2.53	(Experience /100) × women	0.101	0.20
Experience	0.012	1.65	New employer	1.163	6.87
Experience² / 100	-0.025	-2.20	Out of labour force before	0.703	2.70
Experience/100 × women	-0.600	-3.33			
Experience/100 × intermed. school	0.525	2.37	*Firm size*		
Experience/100 × high school	0.302	0.83	(Basis: 1-19 employees)		
Experience/ 100 × apprenticeship	0.359	1.67	Firm size 20 – 199	0.246	1.49
Experience / 100 × graduate	0.168	0.49	Firm size 200 – 1999	0.448	2.52
Job tenure / 100	0.126	5.95	Firm size 2000 –	-0.205	-0.54
Job tenure² / 1000	-0.013	-2.70			
Firm size					
(Basis: 1-4 employees)					
Firm size 5 – 19	0.136	4.38			
Firm size 20 – 199	0.237	8.62			
Firm size 200 – 1999	0.297	9.85			
Firm size 2000 –	0.399	6.33			
FTC	-0.247	-3.09			
LR tests of Joint Significance	X^2	*P-val.*	*LR tests of Joint Significance*	X^2	*P-val.*
Industry Dummies	144.02	0.000	Experience	9.77	0.021
Qualification	35.11	0.000	Firm Size	7.89	0.048
Experience	19.98	0.001			
Firm size	123.13	0.000			

Log-Likelihood –696.198

Wald test : χ^2 (29) = 1585.57 (p-value: 0.000)

$\hat{\rho}$ = 0.248 (Std. Err.: 0.124), $\hat{\sigma}_\varepsilon$=0.306 (Std. Err.: 0.005), $\hat{\lambda}$=0.076 (0.038)

LR test of independence of the equations ($\hat{\rho}$=0) : χ^2 (1) = 2.95 (p-value: 0.086)

Number of Observations: 1,914

Notes: The joint tests include all interaction terms. 10 industry dummies are included in the wage equation but not reported. The results are described in the text.

Sources: German Socio-Economic Panel

The first important finding is the result of the likelihood-ratio test which indicates at the 10 percent level that the wage equation and the probit equation for

FTCs are not independent and that the selection into FTCs should not be treated as exogenous. Therefore, one can conlude that the estimated effects in the last section are biased.

The main finding is that FTCs reduce the hourly earnings of workers by about 21.9 percent, given their characteristics and the selection into FTCs.[17] This large amount seems to be fairly unrealistic and may be a result of specification errors as well as the fact that job tenure could not be included into the selection equation.

6.4 Matching

Two different matching estimators are applied using the propensity score (respectively the linear index) from the probit estimation presented in section 5 (Table 2). First, nearest-neighbours matching with replacement (a given control unit can be matched to more than one treated unit) but without oversampling (only one neighbour is matched to one treated) is performed. Second, one-to-one caliper matching with replacement is performed.[18]

Nearest-neighbours matching uses all workers with FTCs (within the common support) while the caliper matching drops FTC workers for which no control worker is found within the maximum absolute distance given by the caliper. Therefore, selection-bias is more probable to remain in the first case due to differences in the distribution of the *Z* variables over its common support. In the second case some FTC workers are left out so that the sample is reduced and cannot be compared with the other estimations. The caliper is set to $\Psi = 0.05$, which is a compromise between "bad matches" and the loss of units.

The number of FTC workers for which all variables are available is not very large with 90 observations (the corresponding number of permanent workers is 1826) in the sample. However, there are several evaluation studies for the German active labour market policy using matching estimators with a comparable number.

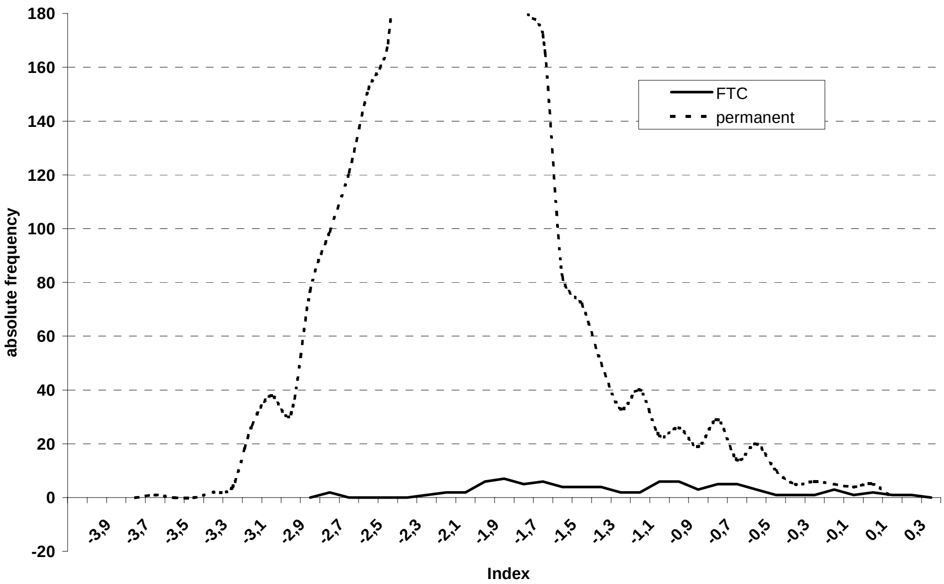
Matching Quality

Figure 1 shows the overlap of $Z\hat{\gamma}$ of the sample of permanent workers and the sample of FTC workers. Although the mass of the distribution of the controls is to the left of the treated, there is an overlap for a large part of the distribution of the persons with FTCs. However, there is a minor lack of overlap in the right tail of the distribution.

17 $-21.9=[(\exp(-0.290)-1)\times 100]$.

18 I apply the matching estimators for STATA 7.0 implemented by Barbara Sianesi.

Figure 1: Distribution of $Z\hat{\gamma}$ for FTCs and Permanent Workers



Note: Mean (standard deviation) in treated / control sample is -1.18 (0.74) / -2.07 (0.50)

Table 6: Means and standardised differences of important variables (%)

	Before matching			After nearest- neighbours matching			After one-to-one caliper matching		
	FTC	perm	diff. %	FTC	perm	diff. %	FTC	perm	diff. %
Propensity score $Z\hat{\gamma}$	-1.19	-2.10	142.5	-1.19	-1.21	2.3	-1.29	-1.29	0.02
Job tenure	54.67	162.08	98.2	54.67	54.03	0.6	44.90	45.33	0.4
Dur. of prev. employment	43.59	93.07	86.7	43.59	44.65	1.8	39.17	38.60	1.0
Dur. of prev. unemployment	2.68	0.350	45.5	2.68	3.63	18.5	2.31	3.83	29.4
New employer	0.34	0.05	80.0	0.34	0.37	6.9	0.27	0.29	4.3
Out of labour force before	0.11	0.02	37.2	0.11	0.14	10.2	0.13	0.16	12.8
Civil service	0.34	0.30	9.8	0.34	0.24	21.7	0.30	0.19	23.8
Blue collar worker	0.42	0.33	17.2	0.42	0.37	10.4	0.44	0.40	9.8
Professionals	0.34	0.33	27.9	0.34	0.29	11.3	0.33	0.27	14.2
Experience	22.44	27.45	61.5	22.45	23.97	18.6	22.56	24.03	18.6
Experience × civil service	6.79	8.27	12.2	6.79	4.01	22.8	6.24	3.51	22.4
Experience × women	10.97	12.06	8.0	10.97	10.97	7.6	10.88	11.52	4.7
Women *	0.49	0.43	13.0	0.49	0.46	7.6	0.49	0.48	3.2
Years of schooling *	12.55	12.23	11.2	12.55	12.29	9.1	12.52	12.30	3.2

Notes: * Women and years of schooling (educational and vocational level) do not determine the type of contract and are therefore not included in the propensity score.

In order to increase the matching quality, additional variables are included so that the propensity score is augmented to a 'balancing score' (Rosenbaum / Rubin 1983). Besides the duration of the actual employment spell (job tenure) which could not be included into the participation equation due to endogeneity, other variables which turn out to be important for selection are included. The inclusion of job tenure secures that only persons with the same duration of their actual employment spell are compared. The duration of previous employment spells may capture unobservables as well as signals for the employee. Furthermore it may be a proxy for previous temporary contract jobs.

In Table 6 the means of characteristics Z of the workers with FTCs and the workers with permanent contracts, as well as the matched FTCs and controls are depicted. In addition the means of the propensity score (respectively the index) and the *standardised difference* before and after matching is included. The standardised difference is defined as (Rosenbaum / Rubin 1985),

$$\frac{\left|\bar{Z}_1 - \bar{Z}_0\right|}{\sqrt{\left(V_1(Z)+V_0(Z)\right)/2}},$$

i.e. the absolute difference of the sample means in the treated $\bar{Z}_1$ and non-treated Z_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. It can be seen that the standardised differences of most variables is reduced. Most important, the standardised difference of $z\hat{\gamma}$ is reduced from 142.5 % to 2.3 % and 0.02%, respectively.

The standardised difference should not be confused with the remaining selection bias after the estimation. It should be kept in mind that matching on the propensity score is sufficient for an unbiased estimation of the effect.[19]

Wage Effects

The estimated wage effects from the matching estimators are depicted in Table 7. The first part of the table contains the results of the nearest-neighbours matching. In the first row are the results of nearest-neighbours matching with the balancing score consisting only of the estimated propensity score. In this case the average wage of the FTC workers is 23.3 DM and the average wage of the permanent workers is 26.5 DM. This implies that the wage effect (the average treatment effect) of FTCs is -12.0 %, which is, however, not statistically significant at the 10 per cent level. Also after the inclusion of job tenure and the duration of the previous employment spell, the wage effects are insignificant.

The estimated effects of the caliper matching are smaller and also insignificant. It can be seen in the last column that the number of matched pairs is reduced since it is not always possible to find corresponding matches within the caliper.

Assuming that the effects would become significant with a larger sample size it is insightful to compare the effects. It can be seen in the results of both matching

19 Given that all relevant Z are included in the selection equation, there are no specification errors in the probit, there is no further selection on unobservable variables and there is a sufficiently large overlap between treatment and potential control group.

estimators that the negative wage effect becomes smaller after the inclusion of job tenure (in months). The preferred specification is one-to-one caliper matching with the balancing score consisting of the propensity score, job tenure and the duration of previous employment. In this case the effect of -5.2% is far from being significant.

Table 7: Estimation result of matching estimators

Nearest-neighbours matching					
Balancing score	average wage of FTC	perm.	Effect in %	t-stat	matched pairs
$Z\hat{\gamma}$	23.32	26.50	-12.0%	-1.60	88
$Z\hat{\gamma}$, *TEN*	23.32	25.56	-8,6%	-1.16	88
$Z\hat{\gamma}$, *TEN, Durat. of prev. employment*	23.32	25.03	-6,8%	-0.98	88
One-to-one caliper matching					
Balancing score	average wage of FTC	perm.	Effect in %	t-stat	matched pairs
$Z\hat{\gamma}$	23.61	26.64	-11.4%	-1.54	86
$Z\hat{\gamma}$, *TEN*	23.51	25.33	-7.2%	-0.94	84
$Z\hat{\gamma}$, *TEN, Durat. of prev. employment*	23.83	25.12	-5.2%	-0.66	72

The results are not altered significantly if instead of the duration of the previous employment other variables are included in the balancing score, like dummies for blue collar workers or civil servants or the duration of previous unemployment. Also the inclusion of variables not included in the estimation of the propensity score, like years of schooling does not alter these findings.

The difference between the matching estimators and the control function approach which yields a significant effect of -22 percent may be explained by the assumptions concerning distribution and functional forms of the parametric model. Furthermore, as already mentioned, the variable job tenure could not be included in the estimation of the propensity score, respectively in the selection equation in the control function estimator in section 6.3.

7 Conclusion

Although the sample size may be too small to obtain significant results with the matching estimators one can conclude that there is no evidence for a compensating differential for FTC workers. Far from it, negative wage effects are found. However, these negative effects of FTCs on wages are not too important.

Assuming that there is a negative wage differential for FTCs, an interesting question for further research is whether those workers are compensated in subsequent positions by higher wages.

FTCs may be an instrument to increase the reemployment probability of workers with 'bad' signals and unobserved characteristics. It is shown that FTC workers have – given their observable characteristics like qualification, experience and gender – longer previous unemployment spells and shorter employment spells than permanent workers. However, the shorter employment spells may also be interpreted as evidence for state dependence: workers with temporary jobs have a higher probability of getting another temporary job than permanent workers. Especially in this area research is needed.

Another important finding is that matching estimators seem to be well-suited for the analysis of wage differentials. Further research should apply extensions like kernel-based matching. Also the longitudinal dimension of the GSOEP should be used for matching combined with conditional difference-in-difference estimators in order to take unobserved heterogeneity explicitly into account (see Heckman et al. 1998).

Literature

Abowd, J.M. and O. Ashenfelter (1981), Anticipated Unemployment, Temporary Layoffs, and Compensating Wage Differentials, in: Sherwin Rosen (ed.), *Studies in Labor Markets*, University of Chicago Press, Chicago, 141-170.

Boockmann, B. and T. Hagen (2001), The Use of Flexible Working Contracts in West Germany: Evidence from an Establishment Panel, ZEW Discussion Paper, No. 01-33, Mannheim.

Booth, A., M. Francesconi and J. Frank (2000), Temporary Jobs: Stepping Stones or Dead Ends?, IZA Discussion Paper No. 205.

Card, D. (1999), The Causal Effect of Education on Earnings, in: Ashenfelter, O. and D. Card (eds.), *Handbook of Labor Economics* 3, Amsterdam.

Cochran, W. and Rubin, D.B. (1973), "Controlling Bias in Observational Studies", *Sankyha* 35, 417-446.

Daniel, C. and C. Sofer (1998), Bargaining, Compensating Wage Differentials, and Dualsim of the Labor Market: Theory and Evidence for France, *Journal of Labor Economics* 16(3), 546-575.

Dehejia, R.H and S. Wahba (1999), Causal Effects in Non-Experimental Studies: Re-Evaluating the Evaluation of Training Programmes, *Journal of the American Statistical Association* 94, 1053-1062.

Farber, H. S. (1999), Alternative and part-time employment arrangements as a response to job loss, NBER Working Paper 7002, M.A.

Franz, W. (1999), *Arbeitsmarktökonomik*, 4. Aufl., Berlin, Heidelberg.

Fitzenberger, B. and H. Prey (1998), Beschäftigungs- und Verdienstwirkungen von Weiterbildungsmaßnahmen im ostdeutschen Transformationsprozess: eine Methodenkritik, in: F. Pfeiffer and M. Lechner (eds.), *Qualifikation, Weiterbildung und Arbeitsmarkterfolg*, ZEW-Wirtschaftsanalysen, Bd. 31, Baden-Baden, 39-96.

Groß, Martin (1999), Die Folgen prekärer Arbeitsverhältnisse für das Ausmaß sozialer Ungleichheit: Einkommensbenachteiligung befristeter Arbeitsverträge, in: P. Lüttering (ed.): *Sozialstrukturanalysen mit dem Mikrozensus*, ZUMA Nachrichten Spezial, Bd. 6, Mannheim, S. 323-353.

Hagen, T. and V. Steiner (2000), *Von der Finanzierung der Arbeitslosigkeit zur Förderung der Arbeit – Analysen und Handslungsempfehlungen zur Arbeitsmarktpolitik in Deutschland*, ZEW Wirtschaftsanalysen, Bd. 51, Baden-Baden.

Hagen, T. (2001), Does Fixed-Term Contract Employment Raise Firms' Adjustment Speed? Evidence from an Establishment Panel for West Germany, ZEW Discussion Paper No. 01-57, Mannheim.

Heckman, J. J. (1978), Dummy Endogenous Variables in a Simultaneous Equation System, *Econometrica* 46(4), 937-959.

Heckman, J. J. (1979), Sample Selection Bias as a Specification Error, *Econometrica* 47(1), 153-162.

Heckman, J.J., Ichimura, H. and Todd, P.E. (1997), Matching As An Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Programme, *Review of Economic Studies* 64, 605-654.

Heckman, J.J., Ichimura, H. and Todd, P.E. (1998), Matching as an Econometric Evaluation Estimator, *Review of Economic Studies* 65, 261-294.

Heckman, James J. and Lalonde, Robert J. and Smith, Jeffrey A. (1999): The Economics and Econometrics of Active Labor Market Programs, in: A. Ashenfelter and D. Card (eds.), *Handbook of Labour Economics* (3), S. 1865-2097

Lauer, C. and V. Steiner (2000), Returns to Education in West Germany - An Empirical Assessment, ZEW Discussion Paper No. 00-04, Mannheim.

Lechner, M. (1998), *Training the East German Labour Force, Microeconometric Evaluations of Continuous Vocational Training after Unification*, Physika-Verlag, Heidelberg.
Lechner, M. (2000), A note on the common support problem in applied evaluation studies, Swiss Institute for International Economics and Applied Economic Research, University of St. Gallen.
Loh, E. S. (1994), Employment Probation as a Sorting Mechanism, *Industrial and Labor Relations Review* 47 (3), 471-486.
Maddala, G. S. (1983), *Limited Dependent and Qualitative Variables in Econometrics*, Cambridge University Press, Cambridge.
Mincer, J. and S. Polachek (1974), Family Investments in Human Capital: Earnings of Women, *Journal of Political Economy* 82(2), 76-108.
Rebitzer, J. B. and L. J. Taylor (1991): A Model of Dual Labor Markets When Product Demand is Uncertain, *Quarterly Journal of Economics* 106(4), 1374-1383.
Rosen, S. (1986), The Theory of Equalizing Differences, in: Ashenfelter, O. and R. Layard (eds.), *Handbook of Labor Economics* 1, Amsterdam.
Rosen , S. (1992), Distinguished Fellow: Mincering Labor Economics, *Journal of Economic Perspectives* 6(2), 157-170.
Rosenbaum, P.R. and D.B. Rubin (1983), The Central Role of the Propensity Score in Observational Studies for Causal Effects, *Biometrika* 70(1), 41-55.
Rosenbaum, P.R. and D.B. Rubin (1985), Constructing a Control Group Using Multivariate Matched Sampling Methods that Incorporate the Propensity Score, *The American Statistician* 39(1), 33-38.
Rubin, D.B. (1980), Bias Reduction Using Mahalanobis-Metric Matching, *Biometrics* 36, 293-298.
Schömann, K. and T. Kruppe (1994), Who Enters Fixed-Term Contracts: Evidence from East and West Germany, *DIW-Vierterljahresheft* 1-2, 69-74.
Schömann, K., R. Rogowski, T. Kruppe (1995), Fixed-Term Contracts and Labour Market Efficiency in the European Union, Discussion Paper FS I 95-207.
Shapiro, C. and J. E. Stiglitz (1984), Equilibrium Unemployment as a Worker Discipline Device, *American Economic Review* 754, 892-893.
Sianesi, B. (2001), An Evaluation of Active Labour Market Programmes in Sweden, IFAU-Working Paper 2001:5, Office of Labour Market Policy Evaluation, Uppsala.
Steiner, V. and K. Wagner (1997), East-West German Wage Convergence - How far have we got ?, ZEW Discussion Paper No. 97-25, Mannheim.
Topel, R. (1984), Equilibrium Earnings, Turnover, and Unemployment: New Evidence, *Journal of Labor Economics* 2(4), 500-522.
Topel, R. (1991), Specific Capital, Mobility and Wages: Wages Rise with Job Seniority, *Journal of Political Economy* 99(1), 145-176.
Van de Ven, W. P. M. M. and B. M. S. Van Pragg (1981), The Demand for Deductibles in Private Health Insurance: A Probit Model with Sample Selection, *Journal of Econometrics* 17, 229-252.
Vella, F. and M. Verbeek (1999), Estimating and Interpreting Models with Endogenous Treatment Effects, *Journal of Business and Economic Statistics* 17(4), 473-478.

Appendix

Table A1: Summary statistic for the labour market participation equation

	mean.	standard deviation	min	max
Participation*	0.604	0.489	0	1
Women*	0.550	0.498	0	1
Work is very important *	0.354	0.478	0	1
Work in unimportant *	0.124	0.329	0	1
Interest Payments *	0.067	0.250	0	1
Spouse employed *	0.576	0.494	0	1
# Children	0.689	1.027	0	9
Married *	0.800	0.400	0	1
Divorced *	0.098	0.298	0	1
Age	46.664	8.581	30	60
Intermediate School Degree *	0.362	0.481	0	1
High school *	0.167	0.373	0	1
Apprenticeship *	0.755	0.430	0	1
Graduate *	0.132	0.339	0	1

Notes: * Dummy Variables, Number of observation is 3,501.

Table A2: Summary statistic for the wage equation

	mean	standard deviation	min	max
Hourly wage	3.259	28.231	3.69	123.25
Intermediate school degree*	0.362	0.481	0	1
High school*	0.216	0.412	0	1
Apprenticeship*	0.753	0.431	0	1
Graduate *	0.182	0.386	0	1
Women *	0.432	0.495	0	1
Experience (years)	27.244	8.369	4.5	45
Job tenure (months)	157.651	123.154	1	547
New employer *	0.059	0.236	0	1
Out of labour force before *	0.024	0.152	0	1
Civil service *	0.299	0.458	0	1
Firm size 5 – 19 *	0.144	0.351	0	1
Firm size 20 – 199 *	0.500	0.500	0	1
Firm size 200 – 1999 *	0.261	0.439	0	1
Firm size 2000 – *	0.016	0.124	0	1
FTC	0.041	0.199	0	1
Dur. of previous unemployment	0.425	2.854	0	51
Dur. of previous employment	91.112	67.793	0	202
Blue collar worker	0.339	0.473	0	1
Professional	0.222	0.416	0	1

Notes: * Dummy Variables, Number of observation is 1,914.