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Fixed-term Contracts as Sorting Mechanisms: Evidence From Job Durations in West Germany

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Non-technical summary

This paper provides empirical evidence for the role of fixed-term contracts FTCs as prolonged probationary periods, which allow the employer to better screen workers before promoting them to a permanent position. Screening becomes necessary if the quality of the job match or the quality of the worker cannot be observed by the employer at the time of hiring. Institutionally, the screening period may be a legal probationary period during which employment protection does not apply. If this legal period is too short to provide information on the quality of the job-worker match, the firm may offer the worker an FTC in order to extend the screening period.

We derive predictions from a job matching model concerning the time pattern of exit from two types of employment spells: spells that start with an initial episode of FTC work (but are possibly converted into permanent employment later on) and those that are started on a permanent contract from the outset. We define the first kind of spell as being subject to a 'treatment' in the sense of the statistical evaluation literature and compare them to spells that have not been 'treated'. Using statistical matching, we make workers in both kinds of spells comparable as regards their observed characteristics and compare their job exit rates.

According to the results, FTCs accelerate exit within the first two years of an employment spell by 55 to 80 per cent. However, employment spells started on the basis of FTCs are significantly more stable than jobs started on permanent contracts in the following years. These results are consistent with a role of FTCs as probationary periods. If firing costs are low initially, bad matches are dissolved earlier than under high firing costs. Ultimately, however, a firm will dismiss all the bad matches. Concerning other theories of temporary work, the result of the reversion of the relative exit rates for the two types of contracts after two years is inconsistent with any of them.

A striking empirical result is that the chances that an employment spell will last longer than five years are not influenced negatively if the worker is initially employed on a fixed-term contract (FTC). Hence, the sorting processes taking place on both types of contracts lead to the same outcome. While this result, unlike the acceleration of the sorting process, is not an implication of our theoretical model, it is consistent with it under certain parameter constellations. Substantially, it suggests that the view that many good job-worker matches are terminated accidentally due to the use of FTCs is not supported empirically.

Fixed-term contracts as sorting mechanisms: Evidence from job durations in West Germany

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Abstract: We estimate the effect of initial episodes under fixed-term contracts (FTCs) on job duration in the further course of the employment spell, using data from the German Socio-Economic Panel (SOEP) from 1985 to 2002. Using a statistical matching approach, we find that job exit rates are initially much higher if the employment spell began with an FTC. However, exit rates fall below those of comparable spells spent entirely in permanent employment after a few years time. This suggests that FTCs accelerate a sorting process and that they may at least to some part be understood as prolonged probationary periods. Strikingly, the probability of long-term employment of more than five years duration is not lower in spells that are initially concluded as FTCs. Hence, the sorting processes taking place in both forms of contracts seem to be of similar intensity.

JEL-Codes: C41, J41, J60

Key Words: Fixed-term employment, probationary periods, job matching, duration analysis

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1 Introduction

As a consequence of labour market reforms in many European countries, researchers have paid considerable attention to the effects of legally allowing workers and firms to conclude fixed-term employment contracts (FTCs). To date, however, empirical knowledge of the role of FTCs in individual employment histories remains limited. On one hand, FTCs are seen as a trap, fostering a division between 'ins' and 'outs' in the labour market and resulting in an inefficient excess of short-term employment relationships. On the other hand, FTCs are regarded as a stepping stone towards regular employment, at least for certain groups in the labour market.

There are two different exit routes from FTCs into regular positions: within the company, and across the borders of the company. In this paper, we are concerned with the internal route only. The hypothesis we concentrate on in the following is that FTCs may serve as prolonged probationary periods, allowing the employer to better screen workers before employing them on a permanent contract. Screening becomes necessary if the quality of the job match or the quality of the worker cannot be observed by the employer at the time of hiring. Institutionally, the screening period may be a legal probationary period during which employment protection does not apply. If this legal period is too short to provide information on the quality of the job-worker match, the firm may offer the worker an FTC in order to have a longer period available for screening. In Germany, for instance, the legal probationary period of 6 months appears to be long. However, it is shortened substantially by collective framework agreements in many industries, in particular for blue-collar workers.

We derive predictions from a job matching model concerning the time pattern of exit from two types of employment spells: spells that started with an initial episode of FTC work (but were possibly converted into permanent employment later on) and those that were started with a permanent contract from the outset. We define the first kind of spell as being subject to a 'treatment' in the sense of the statistical evaluation literature and compare them to spells that have not been 'treated'. Using statistical matching, we make workers in both kinds of spells comparable as regards their observed characteristics and compare their job exit rates.

A number of papers have addressed transitions from FTCs into permanent employment relationships. The Spanish case has found most attention in the literature

because the proportion of FTC workers is higher than in any other European country. Alba-Ramírez (1998), Amuedo-Durantes (2000) and Güell and Petrongolo (2003) analyse the transitions out of FTCs and into other labour market states. These studies mostly conclude that temporary employment is not an effective route for entry into permanent positions, in particular for women and workers with low qualifications. By contrast, evidence from Portugal (Portugal and Verejão, 2002) and the Netherlands (van den Berg, Holm and van Ours, 2002; Zijl, van den Berg and Heyma, 2004) does suggest a springboard role of FTC employment. For the German case, Boockmann and Hagen (2004), Giesecke and Groß (2002) and McGinnity and Mertens (2002) have empirically analysed transitions into permanent employment. Again, the likelihood of transition to permanent work is much higher than in the Spanish labour market.

None of these papers, however, deals directly with the question of the consequences of FTC spells for job durations. The approach of the present study is, therefore, more similar to studies investigating job exit in general. For the German case, Bellmann, Bender and Hornstein (2000), Bender, Konietzka and Sopp (2000), Bergemann and Mertens (2000), Bergemann and Schneider (2001), Schasse (1991) and Wolff (2004) have addressed the determinants of job exits. However, with the exception of the descriptive study by Bergemann and Schneider, these studies have not investigated the role of FTC work for job exit.

In the following section, we discuss the implications of job matching theory for the time profile of the probability of exit from FTC and permanent jobs. Next, we introduce our data. In the fourth section, we describe the statistical matching approach used to identify the effects of FTCs and provide detailed information on our empirical approach. In the fifth section, we present evidence regarding the determinants of entry into FTCs and their consequences for job exit. A final section draws conclusions. To get round the homonymy in the word ‘matching’, we use the term ‘job matching’ when referring to the theoretical concept and ‘statistical matching’ for the estimation technique wherever a danger of confusion arises.

2 Effects of FTCs on the time pattern of job exit

The ‘screening’ or ‘probationary periods’ property of temporary work arises in models of job matching. In the classic contribution by Jovanovic (1979), job searchers’ abilities

are only incompletely observable for potential employers and revealed only after the worker's performance on the job has been monitored for some time. Thus, the quality of the match between worker and job is an experience good. The job matching argument constitutes a reason why firms hire workers initially on FTCs. On the one hand, long-term or permanent contracts are desirable because they provide incentives for investment into job-specific human capital. On the other hand, firms need to protect themselves from unproductive employment relationships by testing the worker during an initial FTC period. Only if matching quality is sufficiently high, the contract is converted into a permanent one.

2.1 A simple model of job matching

Consider a partial model[1] in which a firm hires a number of workers who stay in the firm forever unless they are dismissed.[2] Once hired or retained, in each period workers either excel or fail. In the first case, the period payoff to the firm is $E_t = 1$, in the second case it is $E_t = -1$. Wages are determined outside of the firm and do not reflect the performance of the individual worker. We assume that there are two types of matches between workers and the firm. If the match is bad, the worker fails with (known) probability 1 in each period. If the match is good, the worker fails with (known) probability $1-q$. The quality of the match is unknown to the firm at the moment of hiring, but the firm has an initial belief concerning the quality of the match, denoted by p_0. Firms update their beliefs using Bayes' rule. For instance, if the worker has failed in period *t*, beliefs change according to

$$\Pr(G \mid E_0, \ldots E_{t-1,} E_t = -1) = p_{t+1} = \frac{1-q}{1-p_t q}\, p_t < p_t \ , \tag{1}$$

1 Theoretical models that allow for a screening effect of FTCs have been developed by Blanchard and Landier (2002) and Portugal and Verejão (2003). Our model focuses on the learning process while keeping other aspects, such as wage determination, outside the analysis. Moreover, the model is partial in the sense that we do not address why some firms choose permanent contracts to begin with while other hire workers only on FTCs.

2 Voluntary quits are not considered in this model, the reason being that FTCs influence dismissals and redundancies more directly than quits. To the extent that FTCs induce a higher intensity of search while still on the job and, thus, a higher quit rate, they could induce an acceleration of the sorting process similar to the one induced by the effect on dismissals.

where p_{t+1} is the firm's belief at the end of period *t*. Firms discount future payoffs with discount factor δ.

We now introduce firing costs due to employment protection. We assume that FTCs can be prolonged or terminated at zero costs after each period. Later, we will introduce a maximum number of times an FTC can be prolonged. If workers on permanent contracts are dismissed, firms have to pay a fixed amount of money *F*. Let D_t be a dummy variable, with $D_t = 0$ if the worker is employed with an FTC in period *t* and $D_t = 1$ otherwise. It follows that the expected value of continuing a match at the beginning of period *t* is given by the following recursive equation:

$$EV_t(p_t, D_{t+1}) = (2-q)p_t - 1 + \delta \max\left\{EV_{t+1}(p_{t+1}, D_{t+2}), -D_{t+1}F\right\}. \quad (2)$$

This value must be higher than the costs of terminating the match, otherwise the worker will be dismissed.

We now derive employment durations under alternative types of contracts. Consider first the case without firing costs ($D_t = 0, \forall t$). We denote by *ta* the time when a worker who always failed is dismissed. This duration depends on the prior probability and the speed of the updating process determined by *q* and is found by solving $EV_{ta}(p_{ta}, 0) = 0$. Consider next the case of a permanent contract ($D_t = 1, \forall t$). In this case, the time at which a worker who always failed is dismissed, *tb*, is obtained by solving $EV_{tb}(p_{tb}, -F) = -F$. It can be easily checked that $ta < tb$: Because $EV_t(p_t, 0)$ and $EV_t(p_t, -F)$ differ only with respect to the amount of expected future firing costs arising in case of dismissal, they cannot differ by more than *F* at any duration. Therefore, $EV_{ta}(p_{ta}, 0) - F < EV_{ta}(p_{ta}, -F)$. Following from the definition of *ta*, the first term is zero. The remaining inequality implies that bad matches will not (yet) be dissolved at *ta* if firms face firing costs. Hence, as expected, firing costs protect against dismissal.

Now consider the case of a fixed-term contract that cannot be prolonged at date $t_{\max}$. At that period, the firm has to decide whether the worker shall be promoted to a permanent position. Otherwise, the worker will leave the company. The firm compares the value of retaining the worker (and possibly incurring future firing costs) with the costs of dismissal, which are zero at this period. Thus, the worker will be promoted to a

permanent position if $EV_t(p_t,-F)>0$ and dismissed otherwise. Suppose that the maximum period of successive FTCs, $t_{\max}$, is just below *ta*. Will a worker who always failed be retained? The answer is no, because at $t_{\max}$ the expected value of the match makes a discrete jump downwards from $EV_t(p_t,0)$ to $EV_t(p_t,-F)$. The worker will only be retained if $t_{\max}$ is below *s*, where *s* is defined by $EV_s(p_s,-F)=0$. Because $EV_t(p_t,-F)<EV_t(p_t,0)$ for all *t*, it must be that $s<ta<tb$.

This implies the following ordering: if $t_{\max}<s$, FTCs are ineffective as probationary periods. All workers are retained up to *tb* and bad matches are dissolved at that duration. If $s<t_{\max}<ta$, bad matches are dissolved at $t_{\max}$. Finally, if $ta<t_{\max}$, the situation is as in the case without the maximum number of prolongations, and bad matches end at *ta*.

Comparing FTCs and permanent contracts, there is a period of time from $\min(t_{\max},ta)$ to *tb* when all bad matches have been dissolved under FTCs but have not yet been dissolved under permanent contracts. Thus, while there is initially less employment stability among workers hired on FTCs, from $\min(t_{\max},t')$ employment stability in this group is at a maximum, because all workers in this group are good matches. By contrast, among workers hired on permanent contracts, bad matches exist in the firm until *tb*, when all of them are dissolved. Intuitively, the use of FTCs serves to accelerate a sorting process. It changes the time pattern of dismissals so that job exits occur more frequently early on in the employment spell but less frequently later. This is the prediction to be tested in the empirical part of this paper.

2.2 Selectivity into FTC jobs

When confronting this simple idea with the data, we need to take into account that match quality may not be the same for all matches ex ante. In reality, some matches may have a higher probability of turning out as good matches. For instance, a worker's formal qualifications or physical capabilities may or may not meet the requirements of the job. If this is observed either by the firm or by the job candidate, problems of selection into jobs with probationary periods or FTC jobs will arise.

A first selection problem consists in the fact that employers' ex ante assessments of match quality differ over potential matches. In this case, employers' hiring behaviour is influenced by the availability of probationary periods or FTCs. Employers may hire persons on FTCs who are more likely to turn out as bad matches and who would not be hired on permanent contracts. If these matches are dissolved more quickly than

permanent ones, this may more be due to hiring behaviour than to the effect of firing costs on dismissal behaviour.

A second problem arises when workers self-select into FTCs and permanent contracts. In the model of Loh (1994), workers with higher ability – given other observable characteristics such as age, qualification and gender – select into jobs with probationary periods (or jobs initially on FTCs).[3] Workers with lower ability prefer jobs with full employment protection from the beginning. The reason for self-selection is that workers with higher ability face lower risks of losing their jobs once their quality has been revealed. Firms use the self-selection mechanism by offering wage contracts that imply low initial wages and steep wage growth.[4] Note that the argument is phrased in terms of worker quality, not in terms of match quality. As a consequence of the Loh (1994) model, separation rates need not be higher during probationary periods. If only individuals who expect to perform well enter jobs with probationary periods (i.e. if probation succeeds in inducing self-selection), exit rates may actually turn out to be lower on these jobs (Loh 1994: 485). However, if worker quality were controlled for, layoffs would still be more frequent in jobs with probationary periods.

According to this model, one expects high-ability job searchers to enter into FTCs whereas low-ability job searchers either take up permanent employment or keep on searching for a permanent job. In the empirical estimates, we will attempt to control for self-selection of high-ability workers using, in particular, information on individuals' previous work histories. We will indicate in which direction our results may be biased if this attempt fails and either form of selection is present.

2.3 Alternative accounts of FTCs and their implications for job duration

Some alternative accounts of FTC employment also have implications for job durations. Search theory, when allowing for on-the job search, implies that job exit will be more frequent in FTC jobs (see Boeri, 1999). Given productivity and given the wage

3 For a similar model, see Wang and Weiss (1998).

4 See also Gibbons and Murphy (1992). A worker at the beginning of his/her career is more willing to spend more effort or to accept lower wages if this investment leads to a better assessment of his/her ability by (potential) employers and thus a higher lifetime earnings or utility. Using Swiss data, Engellandt and Riphan (2003) show that, in line with the prediction of Gibbons and Murphy (1992), workers on FTCs invest more effort by working far more hours of unpaid overtime.

earned in the job, the reservation wage that induces individuals to move is lower than in the case of permanent contracts. Once again, mobility is accelerated by the use of FTCs. In contrast to the matching approach, however, once FTCs have been converted into permanent employment relationships, there should be no difference in exit behaviour between the two groups.

The use of FTCs is also often explained by the desire of employers to adjust to firm-specific or macroeconomic shocks. FTC workers serve as a buffer stock allowing firms to choose their labour input flexibly (see, for instance, Bentolila and Saint-Paul, 1992). If this motive is relevant, we would once again expect to see a higher job exit rate of FTC workers. However, once they have made the transition to permanent work, former FTC workers should not have higher or lower job stability. Thus, the prediction for job exit is the same as in the search model.

According to human capital theory (Booth, Francesoni and Frank, 2002a), workers invest into firm-specific human capital only if they expect to stay in the company for some time. Most workers hired on FTCs, however, expect to leave the firm early and avoid the investment expenditures. Since human capital accumulation takes time, this effect may persist even after the contracts have been converted in permanent ones. As a consequence, wages rise more slowly for workers hired on FTCs, they are promoted later than workers hired on permanent contracts, and they may be the first who are made redundant if the need arises. Similar to matching theory, human capital theory predicts long-lasting differences between the group of former FTC workers and workers hired on permanent contracts, but in contrast to matching theory, the groups of former FTC workers consists of individuals whose characteristics are worse than those of other workers and who exit earlier rather than later.

In the following, our focus is on whether the distinct time pattern of job exit implied by the interpretation of FTCs as sorting mechanisms can be confirmed, but we also address whether the evidence conforms to any of the competing theories.[5] Our empirical approach is to identify effects of an initial FTC episode in the employment spell. We are interested in how workers whose employment spell began with an FTC fare in

5 Given the great heterogeneity of FTCs found by other authors (see the previous section), these theories may be true for particular segments of the labour market while they may not hold for others. However, due to the limitation of the data there is little scope for testing the implications for particular groups of workers.

comparison with those who have always been on a permanent contract. Therefore, the 'treatment' does not consist in current job status (FTC or permanent) but in contract status of individuals in the first year of their employment spell. Hence, we use only information on contract type from the first year of employment in a particular company.

While we interpret FTCs as a discrete treatment, the approach could be extended to account for the duration of treatment, i.e. length of the FTC until the time of conversion. As the next section shows, however, it is impossible to retrieve current contract status from our data for a substantial part of the data. In Germany, the vast majority of FTCs last only for up to two years, after which the employment relationship is either terminated or renewed on the basis of a permanent contract (Boockmann and Hagen, 2004). With relatively little variation in the time spent on FTCs, the additional insight gained from considering current contract status as opposed to initial contract status is likely to be limited.

3 Description of the sample

The data base of our study is the German Socio-Economic Panel (SOEP), a representative household survey of the German population conducted on an annual basis.[6] We restrict our analysis to West Germany because a large share of FTC employment is subsidised by active labour market programmes in the East. Moreover, general labour market conditions are very different in the two parts of the country. For these reasons, East Germany would merit a separate analysis.

The key section from the questionnaire for our purpose is worded as follows: '*Do you have an employment relationship with a fixed duration from the outset, or do you have a permanent employment contract?*'[7]. This question is part of the annual questionnaire used from 1995. Before that year, the question was only asked to individuals having started a new job in the previous year. In order to use information prior to 1995, our research is based on contract status in the first year of the employment spell only.

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

7 '*Haben Sie ein von vornherein befristetes Arbeitsverhältnis oder haben Sie einen unbefristeten Arbeitsvertrag?*'

The character of the SOEP as an annual survey introduces the problem that short durations are under-sampled. We limit interpretation to jobs that have lasted at least for a period of one year. This seems also expedient from an economic point of view: many FTCs are of a seasonal or otherwise short-term nature. A number of features (such as part-time nature or frequent recalls to the previous employer) distinguish seasonal or casual work from more stable fixed-term contracts. Moreover, due to probationary periods workers hired on the basis of permanent contracts do not enjoy employment protection immediately. Hence, firing costs will differ only after any probationary periods have ended, which is the case after a maximum of 6 months according to German law.

As we define it, fixed-term work does not include apprenticeships, which are always based on fixed-term contracts in Germany. The sample is limited to spells started after age 21, and we drop all information after 57 years of age. Employment spells in the public sector are excluded because FTCs are often used as an integral part of the career paths of certain occupations in public employment, such as doctors and university teachers. Moreover, there are sometimes restrictions on the conversion of fixed-term to permanent positions in the public sector, in particular in higher education.

We define an employment spell to be an uninterrupted period of employment by the same employer. A condition for inclusion of a spell is that we must observe the beginning of the spell and contract status at the beginning. Hence, only spells started after 1985 are included, and the maximum duration of a spell in the sample is 18 years.[8] Extensive care has been given to the consistency of information on employment spells over survey dates. The variable containing the start of the spell is based on a question asked annually about the exact calendar month and year of the start of current employment. Inconsistent information between years concerning the start date of the spell leads to elimination from the data. However, if the inconsistency is minor (<6 months), we substitute the earlier information for the subsequent spells that deviate from this information, in order to minimise recall error. If information on spells is interrupted, we code spells as right-censored at the date of interruption (January of the year in which non-reporting occured). Otherwise, the end of the spell is defined as the month prior to which the subsequent episode (employment, unemployment or non-

employment) began. In cases in which the beginning of the next episode is not reported, we fix the end of spell at six months after the month of interview.

The first two rows of Table 1 show that there are over 4700 employment spells for men in the sample and close to 4000 for women. Of these, between 16 and 18 per cent are started on FTCs.[9] This number is, of course, much higher than the ratio in the stock of employment, which figures around 7 per cent for West Germany in the observation period. At the same time, it is an under-estimate of the proportion of FTCs in all new employment spells. This is due to the fact that spells with durations of less than a year are under-sampled in the SOEP.[10]

The median duration for spells started in FTC employment is 22 months for men and 21 for women, while the corresponding numbers are 39 months and 32 for spells that have always been permanent. Figure 1 presents Kaplan-Meier estimates of the empirical survivor curve for job duration, distinguishing between contract type and between men and women but not accounting for any other characteristics. Log-rank tests clearly reject equality of the empirical survivor curves across contract types at very high significance levels (see table 1). However, the shapes of the survivor functions are much more distinct for men as compared to women. In the latter case, the functions converge after 50 months duration.

Table 1 also contains median durations according to educational categories and age. If hired on permanent contracts, low-skilled workers tend to have shorter employment durations than workers with completed vocational education or university education. The differences are less pronounced for workers hired on FTCs. As expected, job durations rise with the age at start of spell. Of all workers in the youngest category hired on FTCs, half of them have left their jobs 15 months after entry.

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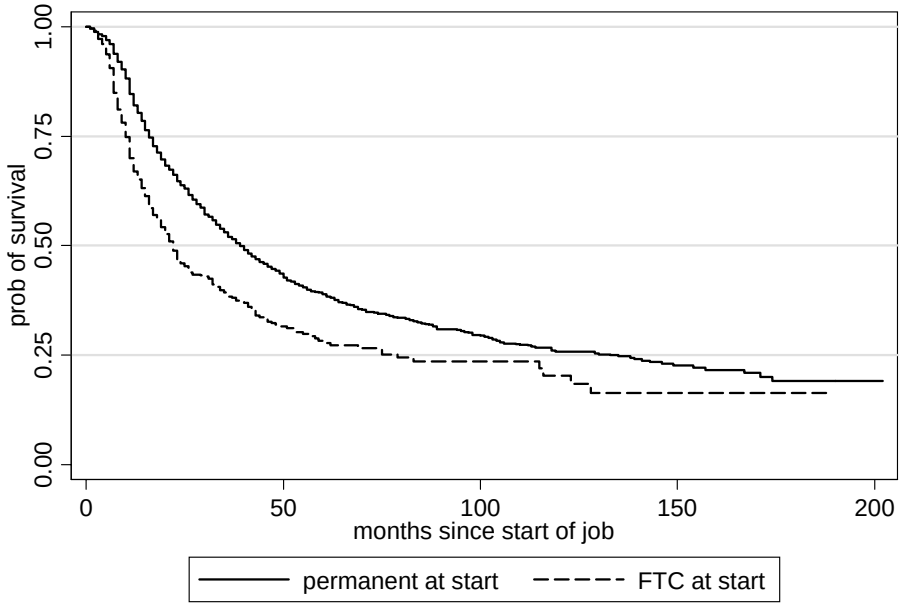
8 In the matching procedure, we also require information from the period before the spell started, which further limits the number of spells that can be used.

9 Or, more exactly, are observed to be FTCs in the first year of observation. Some spells may have been started on an FTC but have been converted into a permanent contract before the first observation.

10 On the basis of the IAB Establishment Panel, which contains all employment starts in the first half of each year in the panel firms, we estimated that the share of hirings on FTC jobs in all hirings is about 33 per cent for West Germany during the period from 1997 to 2003 (Boockmann and Hagen, 2005).

Figure 1: Kaplan-Meier estimates of the empirical survivor curve for job duration

a) Men



b) Women

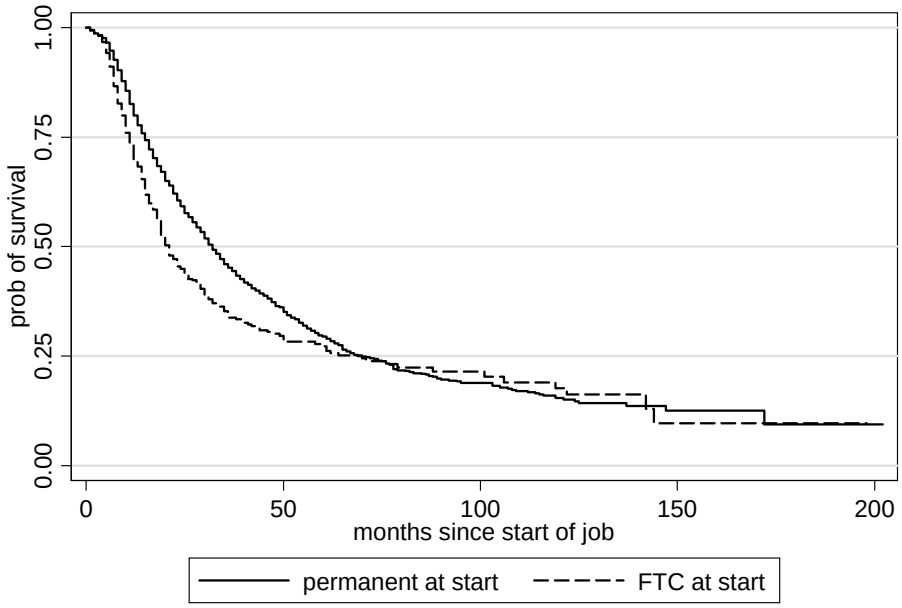


Table 1: Number of employment spells and median durations

	Started on an FTC	Started on a permanent contract	log-rank χ^2 for differences in survivor curves
Men	847	3902	61.93
	(22)	(39)	
Women	644	3342	18.48
	(21)	(32)	
Unskilled	456	1717	19.75
	(18)	(28)	
Vocational education	888	4851	39.73
	(23)	(37)	
University education	185	869	12.28
	(25)	(47)	
< 25 years	368	1363	30.68
	(16)	(26)	
25 – 35 years	588	3094	35.94
	(21)	(35)	
> 35 years	541	2813	13.29
	(30)	(42)	

Note: Median durations in parentheses

4 Using Statistical Matching to Control for Selection into FTC Jobs

In the following, we aim to identify the causal effect of contract status at the start of the employment spell on survival and wages in subsequent years within the same spell. To the extent that observed job durations are due to unobservable match quality, the estimated effects on exit rates will be unconditional with respect to this influence. Hence, estimated survival rates reflect the changing composition of the two groups with respect to match quality over time as well as genuine duration effects. In our case, this is not a disadvantage because the predictions of job matching and alternative theories were phrased in terms of outcomes unconditional on the quality of worker-firm matches (see sections 2.1 and 2.3).

The parameter to be estimated in the following is the average treatment effect on the treated (ATT). The ATT indicates how 'treated' workers (i.e. workers initially on FTCs) have fared relative to a situation in which the same workers have not been treated (i.e. if their employment contracts had been permanent from the start). The ATT is interesting

from a policy perspective because the 'treated' group is often seen as a group put at a disadvantage by labour market regulation. If Y_1 and Y_2 are the outcomes with and without treatment, T is treatment and Z are observed covariates, the ATT is

$$\begin{aligned}\theta &= E\left[Y_1 - Y_0 \mid T = 1, Z\right] \\ &= E\left[Y_1 \mid T = 1, Z\right] - E\left[Y_0 \mid T = 1, Z\right]\end{aligned}$$

The second term in the second line is the counterfactual outcome which must be constructed from the data. To do so, a statistical matching approach is used in order to control for observed heterogeneity between treated and untreated workers. Statistical matching models have previously been used to deal with the selection into temporary contracts by Hagen (2002, 2003).

Matching entails the conditional independence assumption (CIA): conditional on the covariates, contract form at the start of the job and outcomes are assumed to be independent. In particular, we assume that conditionally on the covariates, the outcome for a worker initially on an FTC in the state of non-treatment is the same as the outcome for a non-treated worker:

$$E\left[Y_0 \mid T = 1, Z\right] = E\left[Y_0 \mid T = 0, Z\right] = E\left[Y_0 \mid Z\right] .$$

The question of whether this assumption is justified in our context is a delicate one. There are two kinds of heterogeneity, heterogeneity with respect to the quality of the worker and heterogeneity with respect to the quality of the match. We control for the first kind of heterogeneity by the inclusion of suitable covariates. In particular, we control for variables of the employment history. The assumption here is that worker quality is reflected in past employment history and other characteristics. In case the assumption is violated, there will be an upward bias of the effect of FTC if there is negative selection bias (the less able workers are given FTCs) and a downward bias if selection bias is positive (the career concerns model).

We cannot control for the second kind of heterogeneity. But according to the job matching literature, the second kind of heterogeneity is not observed by the actors at the moment of contract conclusion. Rather, it is experienced while the employment contract

is fulfilled. Following this assumption, the quality of the match is uncorrelated with contract type. In case the latter assumption is violated, there may be selection bias on unobservables so that workers expected to be better matches are given permanent contracts while matches with lower expected quality are made on the basis of FTCs. We do not observe some of the circumstances an employer can observe when hiring a worker, such as references or the exact specialisation of the job candidate. If the employer chooses a permanent contract for matches expected to be good and an FTC for matches that are likely to turn out to be bad, and if these expectations are correct on average, possible negative effects of initial FTC status on job duration and wages will be over-estimated.[11]

Statistical matching is implemented by performing a Probit estimate of the propensity score first. Since the number of potential control observations is high in our sample, we use simple one-to-one nearest neighbour matching without replacement. Results were affected only little when replacement was allowed for, more neighbours were chosen or kernel-based matching was applied.[12] Since individuals taking up employment from different labour market states may differ in their further prospects regarding job stability, we match only individuals with the same labour market state in the period before the start of a new employment spell. A calliper of 0.1 was chosen to remove matches whose propensity scores are relatively far apart. This appeared to be a reasonable compromise between avoiding bias and minimising the loss of observations.

5 Estimation Results

5.1 Estimation of the propensity score

In the estimation of the propensity score, we use information from the interview prior to the first observation in the new spell. The variables used for explaining contract status are all related to the person. They consist, first, of demographic characteristics such as age, education, nationality and disability. Second, we use variables relating to

11 In interpreting our results, we abstract from general equilibrium effects of FTC contracts. If these contracts stabilise permanent employment relationships as suggested by Bentolila and Saint-Paul (1992), Saint-Paul (1996) and others, a possible positive effect of FTC status on the exit rate may be over-estimated.

12 We used the `psmatch2` routine implemented in Stata 8.2 by Leuven and Sianesi (2003).

the household context. Third, we use information on previous spells wherever available, such as tenure in the previous job, the reason why the previous job was terminated (dismissed, expiry of a previous FTC job, quits and other reasons), and the number and duration of previous unemployment spells. Furthermore, we include the employment state (FTC, permanent, vocational training, self-employment, unemployment, out of the labour force) in the period in which matching takes place and in the period before. We also include the unemployment rate of the federal state interacting it with employment state, as well as annual and federal state dummies. The exact definition of the variables can be found in table A1.

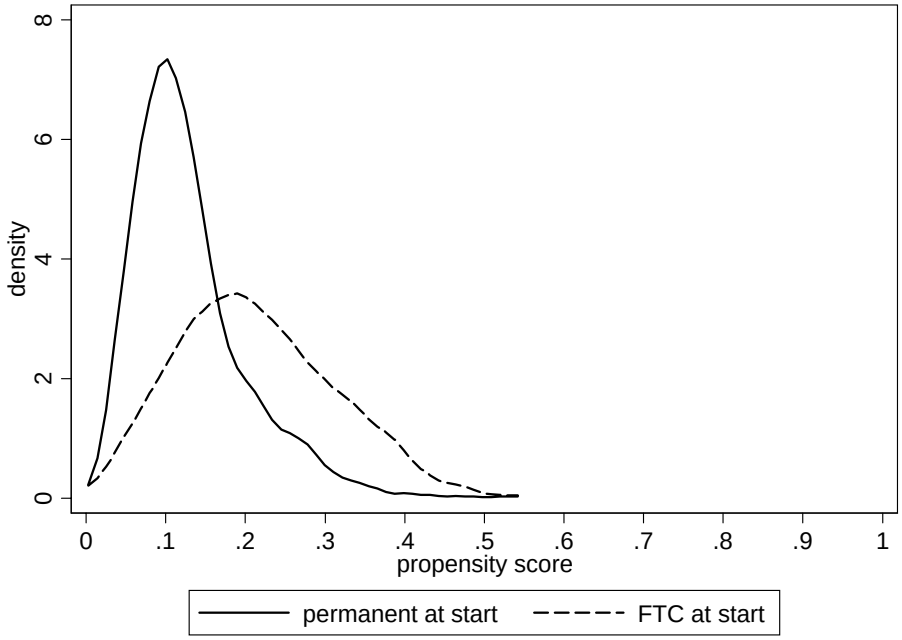
Table A2 in the appendix shows the results from probit estimation. The performance of the estimation is quite different for men and women. Only a few variables are significant in explaining contract status for female workers. Women with university education are more likely to be hired on FTCs than women with vocational education, and they are more likely to take up FTC work if their previous employment ended due to the expiration of an FTC spell. Among men, there is a significant effect of age and nationality on contract form. A number of the variables of the individual employment history, such as previous permanent employment, the duration of a spell out of the labour force or previous FTC experience also have significant coefficients.

Figure 2 displays estimated kernel density functions of the propensity score. The fact that contract form is better explained for men than for women is clearly visible in the distribution of predicted FTC probabilities. The distributions are much more distinct for male as compared to female workers. As a result, we expect that the matching procedure will introduce more changes compared to the descriptive evidence for men than for women. In both cases, the two groups do not have separate regions of support. Imposing the common support condition does, therefore, not lead to many losses in the data.

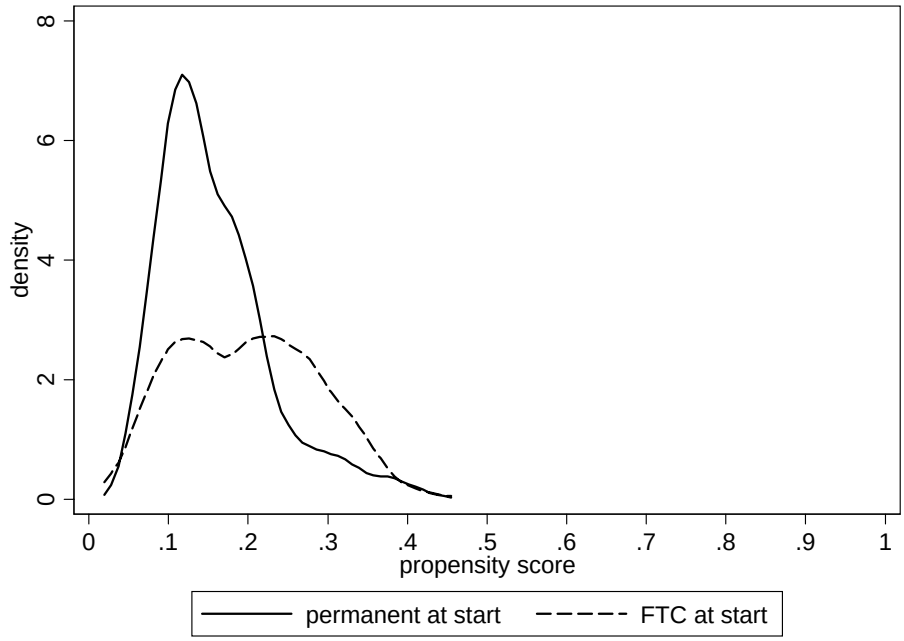
Matching is performed using the predicted linear index from probit estimation. Table A3 in the appendix shows results from a balancing test. We observe that matching is able to reduce the differences in the values of the covariates between the treatment and the control group. With one exception, there remain no significant differences in covariates, and their magnitudes are reduced substantially both for men and women. Most importantly, there are no significant differences in the propensity score.

Figure 2: Kernel density estimates of the distribution of the predicted FTC probability

a) Men



b) Women



Another check on the quality of matching is the pre-programme test of outcomes before the date of treatment. We will discuss this test in section 5.3 below. After estimation of the propensity score and imposition of the common support condition, there remain 478 matched pairs for men and 449 pairs for women.

5.2 Effects of FTC episodes on job stability

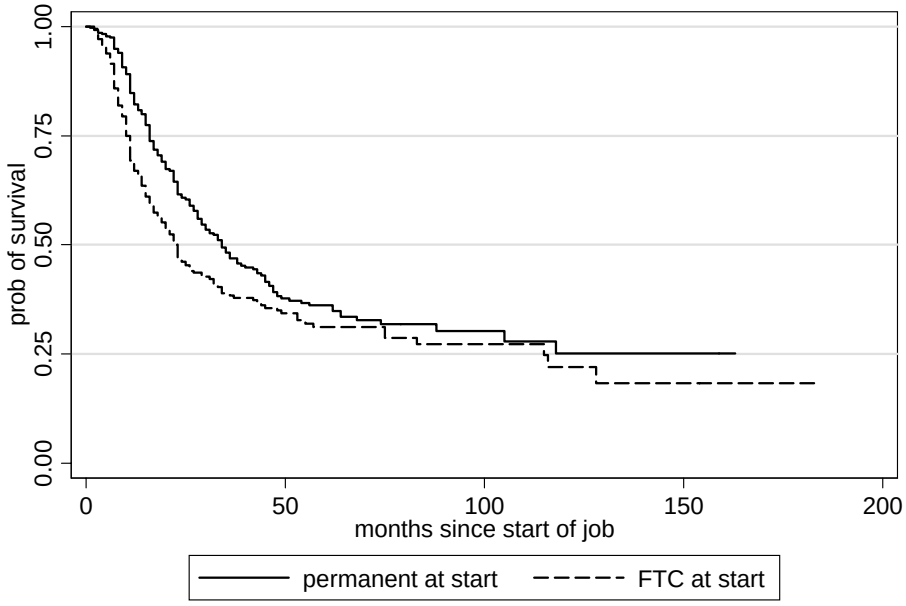
In this section, we test the prediction, derived from the sorting or probationary periods model of section 2, that employment stability in jobs started with FTCs is initially lower, but higher after the sorting process has been concluded. As an outcome variable, we first use the empirical Kaplan-Meier survival rates on the matched samples. This measure is better suited than average durations or a dummy variable indicating survival on the job at a certain time because it deals appropriately with right-censoring. The parameter of interest is the survivor function at time *t* after the start of the job. Figure 3 displays Kaplan Meier estimates of the survivor function for the treatment and the control group.

Due to matching, the differences between spells with and without initial FTC episodes are smaller than the descriptive evidence suggests, but they are still significant: logrank tests yield χ^2-statistics of 6.29 and 4.37 for men and women, respectively, both of which are significant at the five per cent level. We observe that for both men and women, there is a downward effect of initial FTC status on survival in the job for the first two years. From this period on, however, survival rates converge. Strikingly, the survivor curves come very close or even touch after about four and a half years and are hardly distinguishable from that time on. This implies that the probability of being in long-term employment of more than five years is not affected negatively by working initially on the basis of an FTC. This finding holds for women, where the empirical survivor curve already shows such a development, but equally for men.

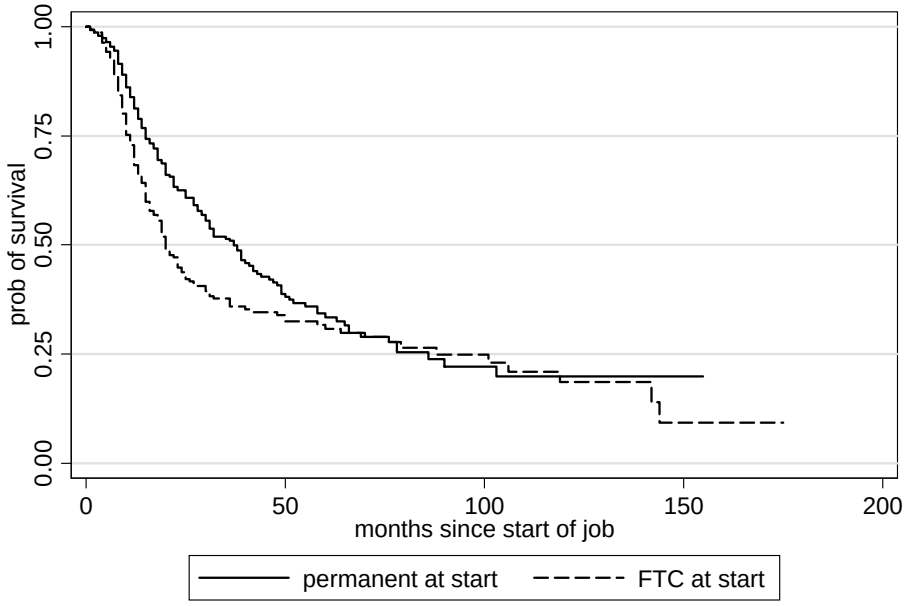
A caveat is that, due to earlier exit and due to right-censoring occurring at earlier durations, the number of spells with long durations is limited. For men, there remain 165 untreated and 115 treated spells after two years duration; of these, 60 untreated but only 29 treated spells are observed to end in the following three years. For women, 150 untreated and 90 treated spells are still on-going after two years, and the numbers of job exits during the following three years are 52 and 22, respectively.

Figure 3: Kaplan-Meier survivor functions for matched samples

a) Men



b) Women



In the next step, we use a proportional hazard model on the matched sample in order to reduce the variance of the estimates and to obtain parameters of a baseline hazard, the second quantity of interest in this part of the analysis. In addition to the information included in the propensity score, we can account for time-varying characteristics influencing the exit rate. Moreover, we can condition on job-related characteristics such as industry and firm size that are excluded from estimating the propensity score. In line with the prediction of Topel and Ward (1992), we also control for the role of wages in determining job exit.[13] We specify the model as a proportional hazard model. The baseline hazard is specified as a piecewise constant function with annual intervals.

Table 2 contains hazard ratios, since these also allow a quantitative assessment of the covariate influences.[14] Values larger than one indicate a positive influence, values of less than one a negative influence.[15] Concerning the covariates, firm size matters for job exit, with significantly lower exit rates from employment in larger firms. In line with the results of Topel and Ward (1992), the wage exerts a negative effect on exit rate which is, however, only significant at the ten per cent level for women. Married women and divorced or widowed men have higher exit rates than unmarried women or married men, respectively. Female workers of non-German nationality have lower exit rates, and dismissal from a previous job has a positive effect on exit from the current job for men. One would expect higher mobility rates for younger workers due to job shopping (Johnson, 1978) and a decline of mobility with age except close to the upper age bound of 57 where the phenomenon of early retirement becomes relevant. Surprisingly, however, age is insignificant in determining job exit.

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13 In our estimation design, wages are also potentially influenced by treatment. Hence, there may be an indirect effect of treatment via wages which may to some extent bias the influence of treatment on the hazard rate (see Simonsen and Skipper, 2003).

14 The hazard ratio minus one gives the per cent change of the hazard induced by a unit change of the covariate. Formally, the hazard ratios are given by

$$\frac{\lambda(t; z_1\gamma)}{\lambda(t; z_0\gamma)} = \exp\left[(z_1 - z_0)\gamma\right],$$

so they are the exponentiated coefficients if we consider a unit change in the covariate z.

15 We use standard errors uncorrected for the effect of matching and imposition of the common support condition. While this is only an approximation of the true standard errors, for practical purposes the differences between the approximation and bootstrapped standard errors appear to be small (Lechner, 2002).

Table 2: Results from proportional hazards estimation on the matched sample

	Men		Women	
	Coeff.	t-Stat.	Coeff.	t-Stat.
Baseline hazard (reference group: <12)				
12-23	*** 0.350	6.21	*** 0.308	6.62
24-35	*** 0.509	3.36	*** 0.422	3.77
36-47	*** 0.437	3.40	*** 0.485	2.64
48-59	*** 0.158	3.61	** 0.469	2.24
60+	*** 0.180	4.72	*** 0.266	3.72
<12, start as FTC	*** 1.652	3.30	*** 1.544	2.80
12-23, start as FTC	** 1.538	2.51	*** 1.772	2.95
24-35, start as FTC	0.587	1.49	0.875	0.43
36-47, start as FTC	* 0.414	1.87	** 0.215	2.10
48-59, start as FTC	1.057	0.08	* 0.368	1.69
60+, start as FTC	0.813	0.42	0.803	0.47
Wage	0.969	1.53	* 0.955	1.77
Match position/qualification	1.022	0.21	0.920	0.63
Firm size 20-199	** 0.775	2.08	** 0.762	1.99
Firm size 200-1999	*** 0.619	3.15	** 0.691	2.07
Firm size > 2000	*** 0.618	2.61	0.876	0.76
No education	* 1.224	1.79	1.094	0.76
Master	0.735	1.17	0.523	1.01
University	1.195	0.91	1.232	0.91
Age	0.960	0.14	1.120	0.36
Age squared	1.030	0.04	0.667	0.47
Age cubic	1.042	0.06	1.436	0.48
Foreigner	1.009	0.08	** 0.739	2.20
Disabled	1.476	1.55	1.203	0.81
Children	1.063	0.51	0.941	0.52
Married	0.942	0.41	*** 1.562	3.00
Divorced/widowed	* 1.464	1.84	1.186	0.79
Dismissed	** 1.380	2.40	0.928	0.43
Expiry of previous FTC	1.052	0.24	1.257	0.74
Tenure last job	0.969	1.56	0.983	0.77
Never on FTC	0.890	1.10	0.868	1.14
Number of UE spells	0.958	1.30	1.055	1.29
UR * year	** 0.877	1.97	1.017	0.25

Table 2 (continued) : Results from proportional hazards estimation (continued)

Number of observations	1529	1245
Number of spells	626	532
Number of observed exits	311	284
Log-likelihood initial	-707.23	-616.96
Log-likelihood final	-580.86	-506.76

Notes: The table includes hazard ratios. Values smaller than 1 indicate a negative influence of the independent variable on exit rates. Time, regional and industry dummies included. Standard errors allow for clustering at the person level. ***, ** and * denote significance at the one, five and ten per cent levels.

Regarding employment relationships started on permanent contracts, the baseline hazard suggest a highly significant drop of 65 per cent for men and 70 per cent for women in the duration dependence after one year. From that time, the baseline hazard remains stable but falls again markedly after four years of tenure. The coefficients for spells with an initial FTC episode represent the differences as compared to the coefficients for permanent contracts in the respective duration interval. They show that job exit occurs significantly more frequently during the first two years of a 'treated' spell; the increase is in the range of 54 to 77 per cent. Most FTCs are either terminated or have been converted into permanent contracts after this time. The baseline hazard for the following two or three years shows that the job exit probability is actually lower for treated than for untreated spells. This holds in tendency both for men and women, but the effect is less pronounced for men, where only one of the coefficients is significant at the ten per cent level.

Since we do not control for unobserved person-specific effects in the estimation, the estimated baseline reflects true duration dependence (for instance, due to accumulation of human capital) as well as changes in the pool of matches and individuals. Since the only possible explanation for greater job stability in treated spells is that the composition of workers and matches changes with duration, we interpret this finding as evidence in favour of a sorting process. In jobs with an initial FTC contract, mobility that would occur even under permanent contracts is accelerated in the first two years of an employment spell. After two years, the sorting process slows down in treated spells but continues in untreated spells, so that the survivor functions converge.

7 Conclusions

According to our results, FTCs accelerate exit within the first two years of an employment spell by 55 to 80 per cent. However, employment spells started on the basis of FTCs are more stable than jobs started on permanent contracts in the following years. These results are consistent with a role of FTCs as probationary periods. If firing costs are low initially, bad matches are dissolved earlier than under high firing costs. Ultimately, however, a firm will dismiss all the bad matches. Concerning other theories of temporary work, the result of the reversion of the relative exit rates for the two types of contracts after two years is inconsistent with any of them.

Another result is that the chances that an employment spell will last longer than five years are not influenced negatively if the worker is initially employed on a fixed-term contract (FTC). While this result, unlike the acceleration of the sorting process, is not an implication of the theoretical model presented in section 2, it is consistent with it under certain parameter constellations. The result must, however, be interpreted with care, since FTCs with very short durations are under-represented in our data.

A normative conclusion from the results is that FTCs are likely to improve the average quality of job-employee matches because matches that are likely to be bad are terminated earlier. A potential drawback could be that many good matches on FTCs are terminated accidentally. Our results do not corroborate this view, since both types of contracts lead to the same proportion of long-term employment. This also contradicts the prediction of Blanchard and Landier (2002) that FTC contracts are terminated at an inefficiently high rate.

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Table A1: Defintion of the variables, sample means and standard deviations

Variable	Definition	Mean	Std.Dev.
No education	Without completed vocational education or A-levels (dummy)	0.284	0.451
Master	Allowed to teach apprentices	0.054	0.226
University	Completed university or polytechnic education (dummy)	0.089	0.284
Age		38.072	10.382
Foreigner	Any nationality other than German (dummy)	0.260	0.4389
Disabled	Legally recognised as handicapped or partially incapable of work	0.042	0.200
Children	At least one child below age 18 (dummy)	0.487	0.500
Married		0.697	0.459
Divorced/widowed		0.067	0.250
Dismissed	Dismissed from previous job (dummy)	0.033	0.180
Expiry of previous FTC	Previous job ended with expiration of FTC employment (dummy)	0.006	0.076
Tenure last job	Months spent in previous job	0.573	2.572
Never on FTC	Work history does not include FTC spell (dummy)	0.869	0.337
Number of UE spells	Number of years in unemployment recorded in data	0.327	0.968
Duration of UE spell	Length of previous unemployment spell	0.562	4.896
Duration of OLF spell	Length of previous spell out of labour force	3.020	16.006
UR	Official unemployment rate of federal state	8.802	2.558
FTC	Fixed-term contract employment	0.024	0.153
Permanent	Permanent contract employment	0.587	0.492
Vocational training		0.015	0.122
Self-employed		0.077	0.266
Unemployed		0.061	0.239
OLF	Out of labour force	0.225	0.417
Match position/qualification	Position is the same as the profession for which worker was educated	0.329	0.470
Wage	Monthly wage divided by 4.35 time actual hours worked per week	11.304	5.819
Firm size < 20		0.336	0.473
Firm size 20-199		0.256	0.436
Firm size 200-1999		0.209	0.407
Firm size > 2000		0.198	0.399

Table A2: Probit estimates of the propensity score

	Men		Women	
	Coeff	Std.Err.	Coeff	Std.Err.
No education	0.056	0.074	0.053	0.072
Master	** -0.270	0.136	-0.096	0.179
University	-0.032	0.101	*** 0.439	0.113
Age	* -0.282	0.149	-0.160	0.161
Age squared	** 0.852	0.418	0.445	0.442
Age cubic	** -0.809	0.376	-0.395	0.393
Foreigner	*** 0.193	0.070	0.061	0.077
Disabled	0.254	0.168	0.213	0.179
Children	-0.019	0.069	0.071	0.074
Married	0.043	0.082	-0.091	0.093
Divorced/widowed	0.141	0.143	0.185	0.128
Dismissed	* 0.155	0.094	-0.001	0.123
Expiry of previous FTC	0.130	0.132	*** 0.540	0.154
Tenure last job	-0.016	0.011	-0.012	0.013
Never on FTC	* -0.142	0.075	0.049	0.086
Number of UE spells	0.004	0.030	-0.041	0.038
Duration of UE spell	-0.003	0.005	0.001	0.007
Duration of OLF spell	*** -0.042	0.015	-0.001	0.001
FTC*UR	-0.057	0.047	-0.009	0.051
Permanent*UR	-0.032	0.028	0.016	0.033
UE*UR	0.022	0.032	0.021	0.037
OLF*UR	* 0.066	0.040	0.012	0.035
Permanent	-0.520	0.407	-0.603	0.445
Vocational training	* -0.814	0.482	-0.169	0.515
Self-employed	-0.736	0.469	-0.602	0.514
Unemployed	-0.681	0.445	-0.340	0.485
OLF	* -0.851	0.498	-0.511	0.457
Permanent-1	*** -0.440	0.126	-0.191	0.161
Vocational training-1	-0.334	0.220	-0.307	0.259
Self-employed-1	-0.146	0.165	-0.158	0.200
Unemployed-1	-0.093	0.148	0.144	0.181
OLF-1	-0.101	0.157	-0.043	0.171
Number of observations	2957		2582	
Log-likelihood initial	-1390.48		-1267.55	
Log-likelihood final	-1266.64		-1198.88	

Notes: Time dummies and regional dummies included. Standard errors allow for clustering at the person level. ***, ** and * denote significance at the one, five and ten per cent level.

Table A3: Balancing test

Variable		Men		Women	
		%bias	t-Stat.	%bias	t-Stat.
No education	Unmatched	17.00	3.57	2.30	0.45
	Matched	-1.40	0.08	-1.40	-0.31
Master	Unmatched	-16.30	-3.02	-3.00	-0.57
	Matched	-0.90	-0.17	5.10	0.81
University	Unmatched	-7.50	-1.47	19.00	4.08
	Matched	1.40	0.09	-6.40	-0.23
Age	Unmatched	-2.00	-0.40	-0.70	-0.14
	Matched	-2.70	-0.68	-7.10	-1.20
Foreigner	Unmatched	18.90	3.94	5.60	1.11
	Matched	-4.60	-0.29	-2.10	-0.20
Disabled	Unmatched	12.50	2.88	5.20	1.06
	Matched	4.70	1.01	4.00	0.52
Children	Unmatched	1.60	0.33	2.50	0.48
	Matched	-0.40	0.06	8.80	1.39
Married	Unmatched	-2.90	-0.59	-11.80	-2.31
	Matched	-3.40	-0.56	7.70	0.84
Divorced/widowed	Unmatched	4.00	0.83	12.70	2.62
	Matched	2.90	0.52	-8.90	-0.93
Dismissed	Unmatched	16.90	3.65	1.30	0.26
	Matched	-4.30	-0.47	-7.40	-1.04
Expiry of previous FTC	Unmatched	12.70	2.84	22.90	5.38
	Matched	-5.80	-0.73	1.00	1.14
Tenure last job	Unmatched	-2.50	-0.49	-4.20	-0.77
	Matched	-3.10	-0.71	5.70	1.16
Never on FTC	Unmatched	-26.00	-5.46	-3.30	-0.64
	Matched	-9.00	-2.04	-4.60	-0.70
Number of UE spells	Unmatched	25.90	5.79	15.50	3.11
	Matched	0.00	0.17	1.60	0.59
Duration of UE spell	Unmatched	19.40	4.15	11.90	2.54
	Matched	-5.40	-0.48	1.80	0.15
Duration of OLF spell	Unmatched	-3.60	-0.62	-2.50	-0.48
	Matched	3.70	1.54	-1.60	-0.31
Propensity score	Unmatched	32.8	6.99	-11.9	1.62
	Matched	0.00	0.64	0.00	0.85