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// EDUARD BRÜLL

Restrictions of Fixed Term Employment Contracts: Evidence from a German Reform

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Restrictions of Fixed Term Employment Contracts: Evidence from a German Reform

Eduard Brüll*
ZEW Mannheim

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Abstract

This paper examines the impact of legal restrictions on fixed-term contracts on employment, wages and the careers of labour market entrants. Specifically, I analyse a 2001 German reform that made it more difficult for establishments that are not subject to employment protection to hire workers on fixed-term contracts. Using a Difference-in-Differences approach, which compares establishments subject to employment protection with those that are not both before and after the reform, I find that the reform has reduced the use of fixed-term contracts, but has not markedly changed net employment. However, the reform has had positive effects on the career stability of post-reform labour market entrants.

Keywords: Fixed-Term Contracts, Employment Protection, Labour Market Segmentation, Germany
JEL codes: J21, J41, J68

*Eduard Brüll, ZEW - Leibniz Centre for European Economic Research, L7, 1 D-68161 Mannheim, E-mail: eduard.bruell@zew.de. I thank Christina Gathmann, Ole Monscheuer, Melanie Arntz and the participants at the internal seminar at the University of Heidelberg, the research colloquium at the University of Freiburg and the 2018 EEA/ESEM Annual Conference for their valuable comments. I am solely responsible for all remaining mistakes and inaccuracies.

1 Introduction

In recent years, governments in Spain, Italy and Germany have discussed policy proposals that would tighten the rules on fixed-term employment contracts (e.g. The Economist, 2018; Reuters, 2016; Zeit, 2018). This new interest in restricting temporary work marks the reversal of a long trend of liberalisation across Europe.[1] Since the 1980s, several governments have lifted limitations on fixed-term contracts to increase firms' flexibility to respond to economic changes. At the same time, the strict dismissal protection rules for permanent employees have remained largely unchanged.

An extensive economic literature examines the availability of fixed-term contracts, in cases where employment protection is strict for open-ended contracts. Remarkably, the assessment of the benefits of fixed-term employment is mixed from a theoretical perspective. Although temporary contracts could offer firms the opportunity to hire less skilled workers without risking high firing costs in case of non-performance (Bentolila and Saint-Paul, 1992), there may be some adverse effects. For instance, employers might substitute from permanent to fixed-term jobs, when both types of contract are easily available (Blanchard and Landier, 2002; Cahuc and Postel-Vinay, 2002; Cahuc et al., 2016). Consequently, a combination of strict legal requirements for the termination of permanent jobs and weak restrictions on the creation of temporary contracts may contribute to a segmentation of the labour market; wherein those in permanent employment enjoy stable careers, and others repeatedly take temporary contracts while working in lower paid entry-level positions

In this paper I analyse a 2001 German reform that made it more difficult for small establishments to use fixed-term contracts. Before the reform, larger firms that are subject to employment protection had to provide an objective reason why a new job was temporary. [2] Small firms below the employment protection threshold of 5 employees were exempt from this rule. The reform changed that by introducing a fixed list of objective reasons why a contract could be fixed-term. These objective grounds were largely identical to those considered by the courts for larger firms prior to the reform, but now applied regardless of firm-size. Thus, after the reform, the legal barriers when hiring employees on fixed-term contracts increased only for those firms that were not subject to employment protection, while nothing changed for larger firms.

To derive empirical predictions for this reform, I use a theoretical search and matching model

[1] Such flexibility measures were often motivated by fears that strict employment protection has rendered European labour markets rigid, and was thereby detrimental to employment. These fears were linked to an economic debate that has examined the institutional differences between the US and several European countries (Bertola, 1999; Nickell, 1997). In this debate, increases in European unemployment in the 1980s were often attributed to more rigid labour markets in Europe when compared with the US.

[2] Both the reform variation and my data are at the local establishment level. For the sake of better readability, I will use the terms establishment and firm interchangeably throughout this paper.

by Cahuc et al. (2016) that explicitly describes the contract choice of firms. In the model, firms hire workers to take advantage of production opportunities of varying expected lengths. Depending on the expected duration of a production possibility, firms choose between fixed-term or open-ended contracts. If a production opportunity ends before its anticipated duration, a permanent job can be terminated for a layoff cost. However, fixed-term jobs can only be terminated after their official term date has expired and not before.[3] Thus, firms face a trade-off between the potential dismissal costs of an open-ended contract and the risk of continued wage payments for a fixed-term job if it becomes unproductive before its term expires. As the reform increases the costs of writing fixed-term contracts, some temporary jobs with very short production horizons should no longer be created. Moreover, more jobs should be converted from fixed-term to permanent contracts after their term has ended. Together, this implies that both job creation and job destruction should decrease after the reform is implemented.

I then use a difference-in-differences research design to analyse, how the reform did influence take-up of fixed-term contracts, job creation and job destruction. Moreover, I examine the reform effect's on wages and the careers of labour market entrants.

In a first step, I show that the share of fixed-term workers decreased in treated establishments, both for new contracts and overall. For new contracts, the fixed-term share decreased by approximately 3 percentage points whereas the overall post-reform fixed-term share decreased by 0.7 percentage points. This 3 percentage point decrease is a sizeable effect, as it amounts to 13 % of the average German usage of fixed-term contracts prior to the reform.

In a next step, I analyse how the reform has affected employment flows. Consistent with the theoretical predictions, I find declines in both job creation and destruction. However, the net impact on employment is only modest.

For earnings, the theoretical model predicts that a temporary contract restriction may have a positive wage effect; if the bargaining power of workers is worse in fixed-term contracts, and the likelihood that a prospective new contract is temporary decreases. This effect should emerge in particular when the overall employment response to a reform is small, and the reduction in the fixed-term share is large. Consistent with this prediction and my other empirical results, I find a 2.39 % increase in the wages of new jobs.

Lastly, I also examine some long-term effects for those labour market entrants who joined affected establishments after the reform. The core result of this analysis is a sizeable increase in the

[3]This represents the legal default for temporary contracts in much of Central Europe including Germany.

cumulative wages over the the individuals' first years in the labour market and both a decrease in time out of work and the number of jobs. Moreover, I provide suggestive evidence that the likelihood to remain fixed-term decreased for this group of labour market entrants.

Together these results imply that the reform had little effect on net employment but a positive effect on wages and the longer-term job security of labour market entrants.

This article contributes to a broad economic literature that examines, how regulating fixed-term contracts affect labour markets. While much of the preceding work is based on aggregate cross-country data (e.g. Kahn, 2010), only a few more recent studies use within country reform variation and micro data to identify the impact of fixed-term contracts on employment and wages (Cappellari et al., 2012; Aguirregabiria and Alonso-Borrego, 2014; García-Pérez et al., 2018; Saggio et al., 2018). However, these studies focus exclusively on the deregulation of temporary contracts in Southern European countries. Both García-Pérez et al. (2018) and Aguirregabiria and Alonso-Borrego (2014) rely on a 1984 Spanish reform which lifted restrictions on temporary contracts. Based on a dynamic labour demand model and firm-level data, Aguirregabiria and Alonso-Borrego (2014) find that the reform led to strong increases in employment and job turnover. García-Pérez et al. (2018), on the other hand, examine the long-term impact of the reform for new labour market entrants and conclude that it has reduced both the number of days worked and their income over the first ten years in the labour market.

Similarly, Cappellari et al. (2012) and Saggio et al. (2018) both examine a staggered deregulation of temporary contracts across industries in Italy. While Cappellari et al. (2012) find that the deregulation reduced capital intensity of firms and generated productivity losses, Saggio et al. (2018) show that the reform had only a small impact on net employment and lowered the earnings of new jobs.

In contrast to the previous literature, I am the first to analyse a restriction of fixed-term contracts in Germany. This is particularly interesting, since there are considerable institutional differences between Southern European countries and Germany (see Boeri et al., 2011). For example, employment protection rules for permanent contracts are more stringent in Spain than in Germany (OECD, 2013) and temporary contracts are used much more intensively.[4] This could, for instance, lead to differences in the long-term impact of fixed-term contracts between Germany and Spain if the much higher incidence of fixed-term contracts in Spain makes the transition into permanent employment more difficult for labour market entrants.

[4]About 30 % of all employment contracts in Spain are fixed-term, compared to only 12 % in Germany (see figure A1 in the appendix).

Likewise, analysing a restriction instead of a deregulation is also a distinctive contribution, as the effects of a deregulation and a restriction of fixed-term contacts need not be symmetric. For example, the substitution possibilities of firms between fixed-term and permanent contracts for pre-existing jobs may be different for restrictions and liberalisations. A particularly interesting comparison in this context is Hunt (2001), who analyses a West German liberalisation of fixed-term employment contracts in 1985 using aggregate industry data. Contrary to my findings, Hunt (2001) does not find any effects on employment adjustments.

Lastly, I also contribute to a growing literature that analyses whether fixed-term contracts offer long-term opportunities or if they bind employees into low-paid entry-level positions (Booth et al., 2002; Ichino et al., 2008; Autor and Houseman, 2010; García-Pérez et al., 2018). I find only minor effects on net employment, but relatively strong increases in cumulative wages and job security for labour market entrants. This assessment of the impact of the reform on later outcomes for labour market entrants also relates to the literature on the long-term effects of labour market conditions at the time of entry (Altonji et al., 2016; Oreopoulos et al., 2012).

The article proceeds as follows. The next section discusses the institutional background of the reform and describes how employment protection and fixed-term work are regulated in Germany. Section 3 provides a discussion of the theoretical mechanisms that determine how limiting fixed-term contracts might affect the use of different contract types, employment and wages. Section 4 describes the data, I use for the analyses. Section 5 presents the empirical strategy. Section 6 reports the results, and section 7 provides a range of robustness checks for my findings. Finally, section 8 concludes.

2 Institutional Background

I will now briefly summarize the institutional background of fixed-term work in Germany, and give an overview of the variation induced by the reform I analyse here. As a first step, I describe employment protection and the laws regarding fixed-term contracts in Germany in section 2.1. I then outline the changes introduced by the 2001 Part-Time and Fixed-Term Contracts Act in section 2.2.

2.1 Employment protection law and fixed-term contracts in Germany

Most employment contracts in Germany are typically open-ended. If a firm dismisses a permanent employee significant firing costs are incurred, in the form of notice periods, severance payments or administrative effort. In most cases, half a month's salary per year of employment is paid as a

severance payment. The firm must also observe notice periods ranging from 2 weeks to 6 months, dependent upon the seniority of the employee.

In addition, larger establishments have to comply with further firing restrictions from the Dismissal Protection Act (Kündigungsschutzgesetz). Specifically, employers have to provide evidence that one of the reasons for dismissal named in the law is satisfied. The act only allows dismissals related to the personal situation of the individual (e.g. long-term sickness), breach of contractual duties (e.g. fraud or theft), or operational requirements of the employer. For a dismissal based on operational reasons, which is the most common type of dismissal, the employer must show that the job position will permanently cease to exist and no other appropriate vacant job exists in the entire firm. Since the burden of proof for a dismissal is relatively high, employees and employers often agree on severance payments to avoid lengthy legal disputes.

Alternatively, firms can hire employees on fixed-term contracts. Once a fixed-term contract reaches its termination date, it can be dissolved without any dismissal costs. However, it is more difficult to justify the termination of a temporary contract before it expires. In general, fixed-term contracts are only permitted if employers state an objective reason why a job could not be permanent (e.g. project work or replacement during sick leave).[5]

2.2 The 2001 Part-Time and Fixed-Term Contracts Act

In January 2001, the Part-Time and Fixed-Term Employment Act was signed into to law as an implementation of the EU Directive 1999/70/C, which sought to harmonise fixed-term contracts across the European Union. This new law changed the rules regarding fixed-term contracts for small establishments, and provides the main variation for my identification strategy.

I present a short overview of the variation introduced by the law in Table 1. The main change is that, prior to the reform, treated establishments were exempt from justifying why a contract was fixed-term, but had to follow the same rules as the control establishments after the reform was implemented. This increased the cost of using fixed-term contracts for treated establishments.

Before 2001 only establishments in the control group had to prove the legal admissibility of their fixed-term contracts. This was due to case law which only applied to the control group. For

[5]There is an exception to this rule for contracts shorter than two years. Since 1985, firms are allowed to use these shorter temporary contracts without naming an objective reason. After 2 years a firm cannot legally offer workers a further fixed-term contract without naming an objective reason. This exception from the default is based on a temporary exemption to boost employment that was introduced in 1985. This rule for short fixed-term contracts was renewed two times and on a latter occasion made permanent. However, these rules remained unaffected by the 2001 reform. See Hunt (2000) for an analysis of the 1985 law that introduced short-term fixed-term contracts without objective reason.

Table 1: Reform variation

	Before 2001	**After 2001**
Treated	No restriction	Objective reasons listed in law
Control	Objective reasons (EPL circumvention)	Objective reasons listed in law

NOTE.- This table summarizes the relevant variation from the 2001 Part-Time and Fixed-Term Contracts Act.

these establishments, courts evaluated a list of objective reasons why a contract could be fixed-term (Column 1). The reform changed this as the list of objective reasons was written into law and now applied to both treatment and control firms (Column 2).[6]

Whether an establishment is treated depends entirely on whether it is subject to employment protection. Establishments that are not subject to employment protection were exempt from the requirement to justify the use of fixed-term contracts. Case law had merely developed the list of objective criteria for the justification of fixed-term contracts to analyse whether a temporary contract could possibly be used to circumvent employment protection. Consequently, before the reform treated establishments were by definition exempted, and so were not restricted in their use of fixed-term contracts.

Employment protection itself depends only on the size of the local establishments, where size requirements have differed over time. I encode treatment along the contemporaneous establishment size bounds for employment protection for each year. Before 1997, the threshold for dismissal protection was 5 employees. For the period from 1997 to 1998 it was increased to 10 employees. Between 1999 and 2004 it was reduced back to 5 employees, and since then it has again been raised to 10 employees.[7]

Since the employment protection threshold changes over time, I analyse whether this affects my results in the robustness section 7.3. To this end, I restrict the sample to the time frame from 1999 to 2004, where the threshold was 5 employees, and find no major differences to my main results.

The same law also introduced new rules, which allowed longer fixed term contracts without objective reasons for workers over the age of 58. However only 1.6 % of new employment contracts are for workers over the age of 58 and exclude them from my data to address this issue.

[6]The reasons include temporary job-role requirements on the part of the employer, trial periods or fixed-term periods following training or studies, employment to substitute another employee, reasons related to the person of employee or the nature of the job and the limited availability of public funds.

[7]The 1996 and 1999 employment protection reforms are analysed by Bauer et al. (2007), while Bauernschuster (2013) discusses the 2004 reform.

3 Theory

Intuitively, treated firms should use relatively fewer fixed-term contracts after the reform, as the legal requirements for their admissibility increase. However, the effects on other outcomes, such as overall employment or wages, are less clear a priori. To derive further predictions on the impact of the reform I introduce a search and matching model based on Cahuc et al. (2016), which explicitly examines firms' choices between permanent and fixed-term jobs.[8] I then use this model to qualitatively study how an increase in the costs of fixed term contracts should affect employment, the type of contracts chosen and wages in treated firms.

In the model, production possibilities have different anticipated durations. When firms and workers meet, they learn the expected duration of their job match and then decide whether they want to take up a job and what type of contract they will use. Such an approach has some advantages over theoretical models which consider fixed-term contracts to be screening devices, used to learn more about worker productivity (e.g. Faccini, 2014; Blanchard and Landier, 2002). In particular, this approach allows for a better representation of some empirical observations on fixed-term contracts than screening models. First, there exist both contracts that are either shorter than the legal probation period or longer than the typically estimated time needed for screening. Second, a learning perspective ignores the higher prevalence of fixed-term contracts in industries with short production opportunities (e.g. Bassanini and Marianna, 2009). Lastly, the model also provides a very accurate description of the German legal framework for fixed-term contracts.

All production possibilities are ultimately limited in time in the model. Jobs differ in the arrival rate of shocks that render them unproductive. Firms and workers jointly maximize a match surplus that depends on this rate, and share it through Nash bargaining. Since fixed-term and open-ended contracts have different termination rules, the match surplus for a given shock arrival rate varies between contract types.

Firms have to pay a firing cost to dismiss an employee on an open-ended contract if he or she becomes unproductive. However, employees on fixed-term contracts can be dismissed free of charge after the contract term, yet not before. If a fixed-term employee becomes unproductive before the contract has ended, the firm must keep paying the employee's salary until the contract term expires. Jobs that start fixed-term can be converted to permanent contracts when the original fixed-term contract expires. Alternatively, fixed-term contracts can be terminated free of charge after the expiration date. Renewing the job with another fixed-term contract is not possible.

[8] Additionally I also incorporate the extensions of the model from Saggio et al. (2018) into my theoretical framework.

In a first step, I can differentiate between threshold values for the shock arrival rate, which determine whether jobs are created and which type of contract is used. This allows me to distinguish four different cases: first, jobs with very low shock arrival rates can start directly with a permanent contract. Secondly, some jobs with higher shock arrival rates begin with a fixed-term contract and then continue with an open-ended contract after their term expires. Third, shorter expected production possibilities lead to fixed-term jobs that end after their term limit. Lastly, jobs with very high shock arrival rates are not created at all.

In a second step, I can derive how a change in the costs of writing a fixed-term contract affects the shock arrival rate thresholds, and thus the proportion of job matches in each of the four cases. This yields predictions about job creation, job destruction and the share of new permanent contracts. Moreover, I can analyse how this increase in contract-writing costs affects wages.

For an increase in the cost of establishing fixed-term contracts, the model predicts two countervailing influences on overall employment. On the one hand, fewer fixed-term jobs are created. On the other hand, it increases the incentive to retain fixed-term employees in permanent contracts, which reduces job destruction.[9]

Beyond these predictions on employment, I also study how the reform should affect wages. A special feature of the model is that it allows positive wage effects of the reform when the bargaining power of workers in temporary contracts is lower. This is due to the employees' outside option. As the reform increases the cost of fixed-term contracts, it is less likely that the next job match of a worker will result in a fixed-term contract. This also makes it more likely that his or her bargaining power will be greater in the next job, thus increasing the outside option and wages.

I will now outline the basic premise of the model, and describe the equilibrium conditions for job creation, job destruction and wages.

3.1 Model Setup

The model economy consists of identical, infinitely-lived, risk neutral workers and firms, who face the same discount rate r. Since workers are identical, their total mass is normalized to 1. Labour is the only input used by perfectly competitive firms. All jobs produce the same quantity of output $y > 0$ per unit of time, but production opportunities differ in their expected duration. This difference between the expected durations is modelled as shocks, which reduce the output produced per time

[9]This basic result has been obtained by a large part of the theoretical literature on fixed-term contracts (e.g. Alonso-Borrego et al., 2005; Blanchard and Landier, 2002; Cahuc and Postel-Vinay, 2002)

unit to $y = 0$ and arrive at the Poisson rate λ.[10] Job seekers and vacancies meet according to a standard constant returns to scale matching technology, and the job-type $\lambda \in [\underline{\lambda}; \bar{\lambda}]$ is randomly drawn from a distribution with $\lambda \sim G(\lambda)$ on match.

Firms and workers maximize a job-type dependent match surplus of $S(\lambda)$ and share it using Nash bargaining. Depending on the size of this match surplus, they choose between permanent and temporary contracts. Permanent contracts are open ended, but are terminated if the job becomes unproductive. At termination the employer pays a firing cost f to dissolve an unproductive permanent contract.[11] Temporary contracts have an endogenous duration $D(\lambda)$ until they expire and can not be ended early. If a job becomes unproductive before the end of its entire term, the company must continue to pay the employee's wage until the contract expires.[12] If the contract stays productive for the whole duration $D(\lambda)$, workers and firms decide whether to dissolve the employment relationship free of any firing cost or whether to establish a permanent contract with a new wage. Agreeing upon another fixed-term contract after the term ends is not possible.[13] Firms pay contract-writing costs that differ between fixed term (c_{FT}) and permanent contracts (c_P).[14] The reform is later modelled as an increase in the contract-writing costs for fixed-term contracts c_{FT}.

The difference between the surplus of a temporary contract with optimal duration $S_{FT}(\lambda, D^*(\lambda))$ and the surplus of a permanent contract $S_P(\lambda)$ determines the contract type choice in equilibrium. I provide a detailed definition of the surplus by contract type in appendix B.1.

3.2 Equilibrium Conditions

Cahuc et al. (2016) show that, given that both types of contract exist in an equilibrium, there are three unique endogenously determined levels of λ that determine job creation, job destruction and the initial type of contract.

[10] There is empirical evidence that fixed-term contract use depends strongly on the length of production opportunities. For example, Dräger and Marx (2017) find that workload fluctuations increase the likelihood of hiring fixed-term workers in countries with less flexible labour markets.

[11] f is assumed to be a red-tape cost and not a transfer from the firm to the worker (such as a severance pay) as such transfers can be neutralized by appropriately designed contracts (Lazear, 1990). Garibaldi and Violante (2005) discuss to what extent employment protection induces transfers from firms to employees or creates red tape firing costs.

[12] This represents the default in German fixed-term contract law, as a jointly determined dismissal provision between the employee and the firm is required for the premature termination of fixed-term contracts. Deviations from this basic rule are only possible in special cases like fraud or theft. Moreover, not all jointly determined termination provisions are legally justified. Similar rules also apply in other European countries like France, Belgium and Italy.

[13] Although it is theoretically possible to establish consecutive fixed term contracts with a valid objective reason in Germany, the law regards this as the default. Renewing a fixed-term contract requires a special new objective reason for an extension. Moreover, the courts evaluate the number of past contracts and the total employment duration to establish whether a new fixed-term contract is valid.

[14] Both terms represent legal costs of writing contracts. c_{FT} is higher than c_P as firms need to provide an admissible objective reason to establish fixed-term contracts.

First, the level λ_P with $S_P(\lambda_P) = 0$ determines whether fixed-term jobs are continued after the termination date. Second, the value λ_{FT} with $S_{FT}(\lambda_{FT}) = 0$ specifies a bound for temporary job creation. Lastly, λ_E with $S_P(\lambda_E) = S_{FT}(\lambda_E)$ defines a level of λ at which firms are indifferent to whether they should start a job with a fixed-term or permanent contract. The necessary condition for the existence of such a type of equilibrium with $\lambda_E > \lambda_P > \lambda_{FT}$ is that $S_{FT}(\lambda_P) > 0$.[15]

Figure 1: Choice Between Fixed-Term and Permanent Contracts

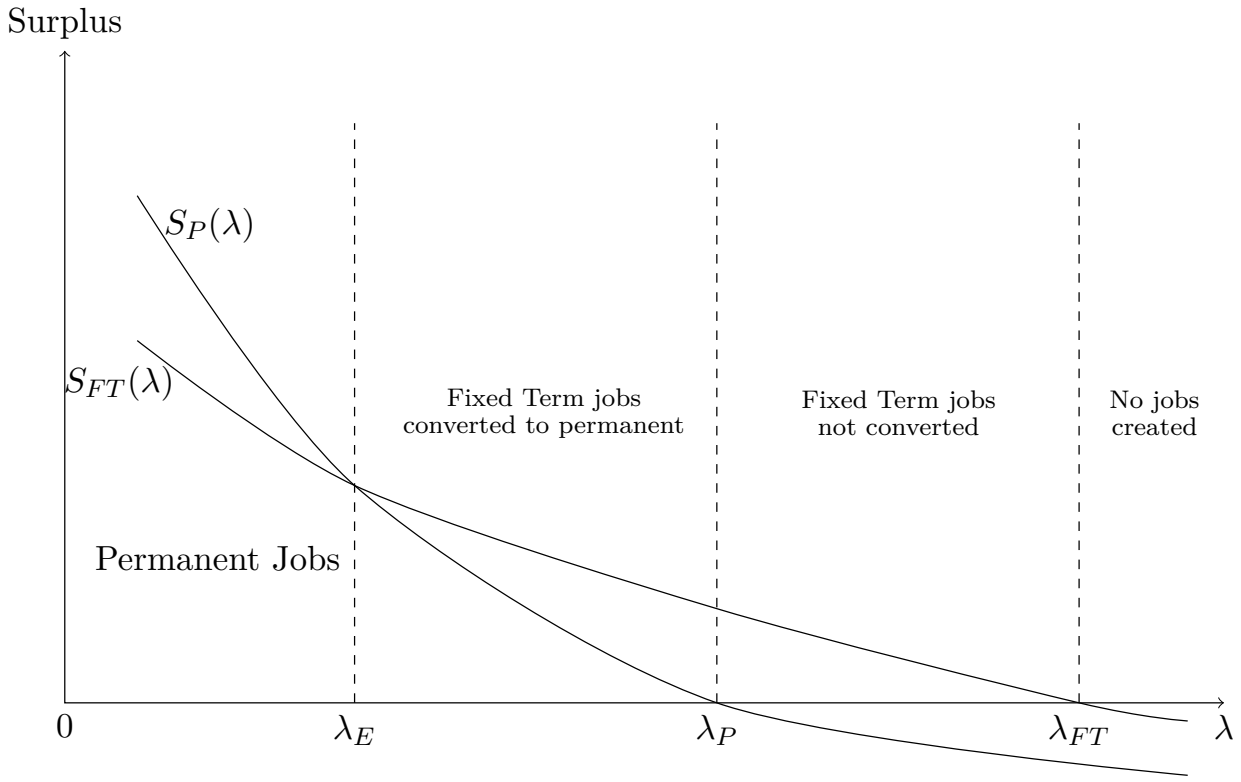


Figure 1 illustrates the contract choice for different levels of λ.[16] For values of λ below λ_E, the expected duration of a production opportunity is sufficiently large to create jobs with permanent contracts. For values of $\lambda \in [\lambda_E, \lambda_P]$, jobs are created with a fixed-term contract but converted to permanent contracts if they stay productive until $D^*(\lambda)$. As the surplus of continuing a job in a permanent contract is below zero for $\lambda \in [\lambda_P, \lambda_{FT}]$, jobs in this range begin fixed-term and are destroyed once the contract expires. Jobs with very high shock arrival rates above λ_{FT} generate negative surplus even for fixed-term contracts, and are therefore not created at all.

The equilibrium in the labour market is defined by the conditions which determine parameters λ_{FT},λ_P and λ_E, and one further condition on the matching between workers and vacancies. Unemployed workers u find vacancies v according to a standard constant returns to scale aggregate

[15] A more detailed overview of the equilibrium conditions can be found in the appendices B.2 and B.3.
[16] The figure is based on figure 6 in Cahuc et al. (2016)

matching function $m(v,u)$ (Pissarides, 1979). Therefore, the vacancy fill rate $q(\theta)$ and the employment finding rate $\theta q(\theta)$ solely depend on the ratio of the number of vacancies v over the number of unemployed workers u, which is the labour market tightness $\theta = \frac{v}{u}$. Not filling a vacancy implies a cost of $\kappa > 0$. If there is a match, both parties learn the true value of λ and use the contract-type rules for λ to decide whether they enter an employment relationship. If the worker and the firm sign a contract, they negotiate a wage using Nash bargaining. The share of the surplus retained by workers is $\gamma_c \in [0,1)$ with $c = \{FT, P\}$.

$$\kappa = q(\theta)\left[(1-\gamma_P)\int_{\underline{\lambda}}^{\lambda_E} S_P(\lambda)dG(\lambda) + (1-\gamma_T)\int_{\lambda_E}^{\lambda_{FT}} S_{FT}(\lambda)dG(\lambda)\right] \tag{1}$$

If all profitable opportunities for job creation are exploited, the expected profit for vacant jobs is equal to the cost κ. This yields the free entry condition in equation (1), which specifies the labor market tightness in the equilibrium.

Moreover, this condition can be used to pin down the value of the outside option in the equations that define λ_E, λ_P and λ_{FT}. The value of this outside option is simply given by the sum of a flow utility of unemployment z and the expected surplus share of a job evaluated at the job finding rate $\theta q(\theta)$.

$$rU = z + \theta q(\theta)\gamma_P \int_{\underline{\lambda}}^{\lambda_E} S_P(\lambda)dG(\lambda) + \theta q(\theta)\gamma_{FT}\int_{\lambda_E}^{\lambda_{FT}} S_{FT}(\lambda)dG(\lambda) \tag{2}$$

Rearranging the free entry condition to get an expression for $\int_{\lambda_E}^{\lambda_{FT}} S_{FT}(\lambda)dG(\lambda)$, and substituting this into equation (2), yields

$$rU = z + \theta q(\theta)\frac{\gamma_{FT}\kappa}{1-\gamma_{FT}} + \theta q(\theta)\frac{\gamma_P - \gamma_{FT}}{1-\gamma_{FT}}\int_{\underline{\lambda}}^{\lambda_E} S_P(\lambda)dG(\lambda). \tag{3}$$

Note that the last term in equation (3) becomes zero if the rent-sharing parameter does not differ between contract types. This term is the valuation of the additional rent that workers can extract under a permanent contract. Thus, for the case of equal bargaining power in fixed-term and open ended contracts ($\gamma_{FT} = \gamma_P$) the value of the outside option solely depends on the utility flow of unemployment z, the cost of creating a job κ, the bargaining parameter γ and the labour market tightness θ. However, for differential rent-sharing the outside option additionally depends on λ_E, which determines whether a job is directly started with a permanent contract.

Substituting the value of the outside option from equation (3) into the equations defining γ_{FT}, γ_P and γ_E provides a system of equations that specifies the equilibrium $(\theta^*, \lambda_E^*, \lambda_P^*, \lambda_{FT}^*)$.

3.3 Wages

In equilibrium, the firing costs f are sufficiently low to ensure that permanent jobs are terminated if their productivity drops to zero. Thus, permanent jobs that are continued have a productivity of y, whereas fixed-term jobs have a productivity of either y or zero, but must be continued until the end of their term date. Therefore, wages are negotiated only when a new contract is signed, since none of the parties can provide a credible threat to enter into wage negotiations during an employment relationship. Consequently, the only two cases of wage negotiations are new job matches and conversions of fixed-term into permanent contracts.

The negotiated wages of a contract of type c are the outcome of a Nash bargaining problem, which is given by

$$w_c = \text{argmax} \left\{ (W_c - U)^{\gamma_c} (S_c - (W_c - U))^{1-\gamma_c} \right\},$$

where W_c is the workers valuation of wage payments in the contract, U is the value of the outside option, S_c is the total surplus of the contract and γ_c is the Nash bargaining parameter.

Solving this bargaining problem implies that the wage for a permanent contract is given by

$$w_p = \gamma_p(y + rf) + (1 - \gamma_p)rU, \tag{4}$$

which would reduce to the outside option rU if the workers bargaining power γ_P was zero. Analogously, the wage for a fixed-term contract is given by the following equation:

$$w_{FT} = \gamma_{FT} \left(\frac{ry}{r+\lambda} \frac{1 - e^{-(r+\lambda)D^*(\lambda)}}{1 - e^{-rD^*(\lambda)}} \right) + (1 - \gamma_{FT})rU \tag{5}$$

The term $\frac{1-e^{-(r+\lambda)D^*(\lambda)}}{1-e^{-rD^*(\lambda)}}$ evaluates the odds of a shock not occurring before the duration $D^*(\lambda)$ has elapsed and is decreasing in D^* for a fixed λ.

This is because, at a given expected productive duration of $1/\lambda$, the probability that a job will become unproductive during the contract term increases with the length of D^*. Thus, as D^* decreases in U for a fixed λ, an increase in the value of U leads to an increase in both the first and the second term of w_{FT}.

3.4 Model predictions

How does an increase in the cost of establishing a fixed-term contract c_{FT} affect job creation, job destruction and wages? I now discuss the reform effects on the main outcomes that I analyse. A

more detailed description of the comparative statics for the equilibrium values (θ^*,λ_E^*,λ_P^* and λ_{FT}^*) is provided in the appendices B.2 and B.3. Table 2 outlines the main model predictions I will examine in the results section.

Table 2: Model predictions

	Prediction
Job creation	Less job creation ($\lambda_{FT} \downarrow$) as jobs with very short expected productive durations are no longer created
Job destruction	Less job destruction as more temporary jobs are converted to permanent contracts once their term expires ($\lambda_P \updownarrow$)
Wages	Only negative wage effects ($w_{FT} \downarrow, w_P \downarrow$) possible for equal rent-sharing through decrease in the value of outside option as labor market tightness declines. Positive wage effects ($w_{FT} \uparrow, w_P \uparrow$) possible for differential rent sharing, if more jobs start directly with a permanent contract ($\lambda_E \uparrow$).

NOTE.- This table contains a short overview of the main model predictions.

Fewer jobs are created after the reform. Raising contract writing costs for fixed-term contracts c_{FT} reduces their potential surplus, rendering fixed-term jobs with very short-term expected production opportunities less desirable ($\lambda_{FT} \downarrow$). Such a decrease in job creation subsequently reduces labour market tightness ($\theta \downarrow$). Although a lower labour market tightness reduces the value of the workers' outside option, and thus works in the opposite direction to the main effect, the direct negative effects on job creation dominates.

The reduction in the value of the outside option of workers, combined with the increase in the cost of new fixed-term contracts, reduces the relative cost of continuing temporary jobs with a permanent contract after they have reached the end of their term. Hence, more temporary jobs are converted to open ended contracts (λ_P increases), which in turn reduces job destruction.

The overall impact of the reform on employment depends on whether the decline in job destruction outweighs the decrease in job creation. Consequently, the net effect of the reform on employment remains an empirical question.

If bargaining power is identical across contract types, only negative wage effects are possible, as the reform decreases the outside option of workers. However, if the bargaining power in fixed-term contracts is lower than in permanent contracts, positive wage effects are also possible. For the case of differential rent sharing, the value of the outside option not only depends on labour market tightness θ but also on the parameter λ_E that determines whether a new job is started with a fixed-term or

a permanent contract. If more jobs start directly with permanent contracts, which allow workers to extract a higher share of the match surplus, the net effect of the reform on the workers' outside option $U(\lambda_E, \theta)$ may be positive.

However, the impact of the reform on the share of contracts that directly begin with an open-ended contract is a priori indeterminate ($\lambda_E \updownarrow$) due to two countervailing reform effects. First, for a given level of labour market tightness, creating a job directly with an open-ended contract becomes more attractive as using a fixed-term contract instead now incurs higher costs. Secondly, a a decline in labour market tightness due to lower job creation could offset this effect through a reduction in the outside option of workers. The overall effect is not clear from the onset and depends on different model parameters.

However, a positive wage effect of the reform would indicate that workers' bargaining power is lower in fixed-term contracts.

4 Data Sources

4.1 Mikrozensus

To analyse the 2001 reform in fixed-term employment rules, I use the Mikrozensus, a repeated cross-sectional survey of 1% of the German population. My analysis is based on data between 1996 and 2010 as information on the contract type is only available for survey waves after 1996. Multiple characteristics make the data particularly suitable for examining fixed-term employment.

First, the data contain information on whether a contract is temporary and also include its official duration for time periods before and after the reform. In contrast, German Social Security data only include the fixed-term status of employees for years after 2011. Secondly, the data include questions on the establishment size, which can be used to determine whether a workers' firm is subject to employment protection laws. Finally, the Mikrozensus is a large representative sample of the German population, including about 14000 new employment relationships per year, allowing me to analyse the effect of fixed term employment legislation on hiring behaviour.

I distinguish three skill groups based on the highest educational qualification obtained. An individual is medium-skilled if he or she has completed an apprenticeship or graduated from high school (*Abitur*). A person is high-skilled if he or she graduated from college or university. However, high-skilled workers in the treated firms, in particular those with temporary contracts, are a highly

selective group with very few observations. [17] Therefore, I exclude high-skilled workers from my sample.

The Mikrozensus also provides information on net personal income that combines wage income with earnings from self-employment, rental properties, pensions as well as other public transfers (like welfare or child benefits). I convert net personal income to 2014 prices using the national consumer price index, and use this information to analyse the wage effects of the reform.

For the analysis, I restrict the sample to West German individuals between the ages of 20 and 58. I further exclude individuals that are either self-employed, in civilian or military service, or in vocational training, since the legal rules regarding employment protection and fixed term work do not apply to these groups.

4.2 Social Security Data and Establishment History Panel

Although the Mikrozensus contains detailed information on contract types, I cannot use it to analyse employment effects because I cannot track individuals in my sample over time.

However, administrative social security data allow for a detailed analysis of the theoretical predictions on job-creation and destruction, as it is possible to follow individuals over time. Although contract types are not directly observable in the social security data for the reform period, this allows for both analyses on the transition between unemployment and employment and an analysis on the long-term reform effects for labour market entrants. Consequently, I use a 2% random sample of the population of workers and firms covered by the social security system in Germany in order to study employment effects and long-term reform outcomes in greater detail.

I apply the same sample restrictions as for the Mikrozensus data to make the results comparable across data sets. Since the education variable in the social security data is missing for about 20% of the observations, and exhibits some inconsistencies over time, I use the panel structure of the data to impute education in the current year from past and future spells following Fitzenberger et al. (2006).

The data provide information on each individual's employment status in the social security system as of June 30th for each year. Moreover, the wage variable reports the average daily wage for the employment spell that contains this reference date. As with virtually all social security data, the wage variable is right-censored at the social security threshold. I impute censored wages under the assumption that the error term in the wage regression is normally distributed, allowing for separate

[17] For new contracts, there are less than 29 observations of high-skilled individuals in treated firms in 2001, of which only 6 are fixed-term.

variances by year and gender (Gartner, 2005). However, given that the data-set is restricted to low- and medium-skilled individuals less than 4% of wage observations are affected by the imputation procedure. Furthermore, I also convert wages to 2014 prices using the national consumer price index.

4.3 Descriptive statistics

Table 3: Summary Statistics for the Mikrozensus data

	Workers in treatment firms			
	Pre reform period (1996-2000)		Post reform period (2001-2010)	
	Mean	St. Dev.	Mean	St. Dev.
Fixed Term	0.05	0.21	0.06	0.24
Real net monthly income	1937.95	1078.41	2030.50	1408.37
Hours worked per week	30.79	13.19	28.58	13.46
Female	0.65	0.48	0.66	0.47
Age	38.61	10.32	39.82	10.36
Service Sector	0.68	0.47	0.72	0.45
Medium Education	0.78	0.41	0.84	0.37
Observations	27,300		131,125	
	Workers in control firms			
	Pre reform period (1996-2000)		Post reform period (2001-2010)	
	Mean	St. Dev.	Mean	St. Dev.
Fixed Term	0.06	0.23	0.08	0.27
Real net monthly income	2016.02	958.00	2100.03	1226.88
Hours worked per week	35.14	9.23	34.55	9.99
Female	0.44	0.50	0.46	0.50
Age	39.39	10.07	40.32	10.16
Service Sector	0.54	0.50	0.62	0.49
Medium Education	0.78	0.41	0.83	0.38
Observations	232,288		712,009	

NOTE.- This table provides means and standard deviations for fixed-term status, real income, hours worked and age by treatment status both for the pre-reform and post-reform periods. Moreover it also reports the proportion of female workers, medium educated individuals and employees in the service industry.
Source: Mikrozensus sample for West German employees aged 20 to 58

Table 3 presents summary statistics of the main variables by treatment status for the time periods before and after the reform. Matching the stronger incentive to use temporary contracts, the proportion of fixed-term employees is higher in the control firms that are affected by employment protection.[18] Average wages are only slightly higher for control firms. Average working hours are

[18]Centeno and Novo (2012) and Hijzen et al. (2017) both analyse how the employment protection of permanent contracts affects the uptake of fixed-term contracts, and find positive effects of stricter employment protection on the use of fixed-term contracts.

however larger. Moreover, the proportion of female workers is higher for treated firms.

While the average age is nearly identical for the two groups, treated firms tend to employ more low skilled individuals and are more likely to be active in the services sector of the economy.

Overall, employees in treatment and control firms are quite similar along observable characteristics.

5 Empirical Strategy

The 2001 reform increased restrictions on fixed-term contracts only for establishments that are not subject to employment protection. I use this variation in the relative costs of fixed-term contracts between treatment (i.e. not subject to employment protection) and control firms (i.e. subject to employment protection) to implement a difference-in-differences approach. Specifically, I compare employees in treatment firms to those in control firms, before and after the reform.

The main estimation equation is given by

$$\textsc{Outcome}_{ift} = \alpha \textsc{Treated}_{if} \times \textsc{Post}\ 2001_t + \beta \textsc{firm-size}_f + \gamma \textsc{year}_t + \lambda \mathrm{X}_{ift} + \varepsilon_{ift}, \qquad (6)$$

where i indexes individuals, f indexes firm-size categories, and t indexes years. I include year and firm-size dummies, as well as a set of control variables X_{ift} for the age, education and the gender of the individual and the industry of the firm at the 2-digit level.

The variable $\textsc{Treated}_{if} \times \textsc{Post}\ 2001_t$ is an interaction effect between the treatment status of a workers' firm and an indicator variable for years after 2001. Workers in firms that are not subject to employment protection are treated. Employment protection itself is entirely determined by firm-size and I use the respectively applicable firm-size limit for each year to determine, whether a firm is subject to employment protection or not. As the firm-size rules for employment protection change over time, I also restrict the sample to a time-frame in which the same rules applied as in the reform year in the robustness section.

Since I control for the firm-size categories that determine a firms' employment status and year fixed effects, the effect of the reform α is identified by the change in the respective outcome variable in treatment firms relative to control firms, in 2001 or later relative to 2000 or earlier.

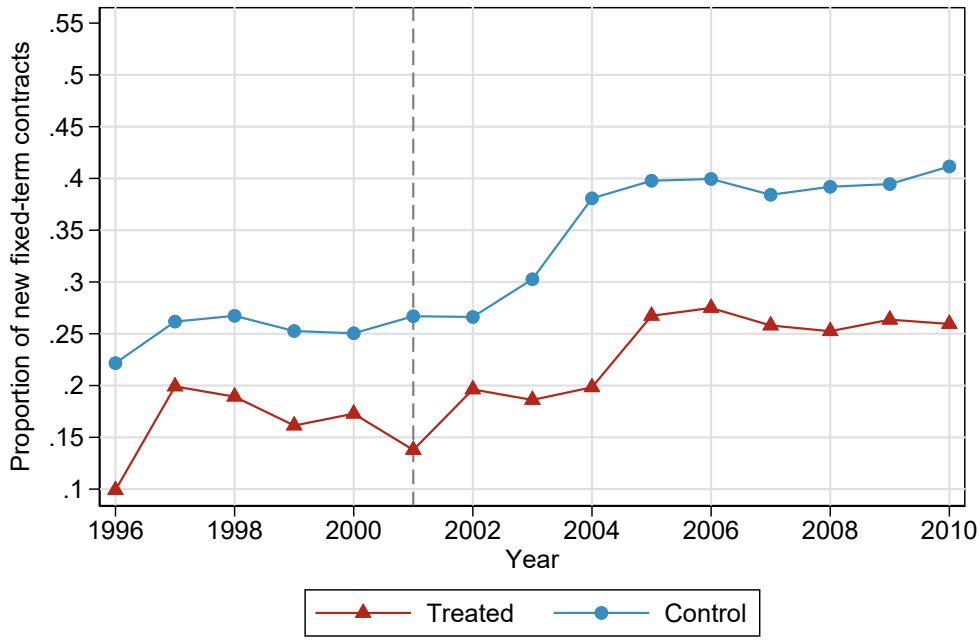
The firm-size categories for all regressions are plants of 1 to 5 employees, 6 to 9 employees, 10

to 19 employees and 20-49 employees. The range of the firm-size category increases due to data limitations for firm-sizes above 10 Employees. However, I analyse different firm-size restrictions and different levels of fixed-effects in the robustness section. For example, I restrict the maximum plant-size to 10 employees and include detailed firm-size fixed-effects in the smallest available increment.

To account for within firm-size dependence of hiring behaviour, I cluster standard errors at the level of firm-size categories. Since the number of firm-size clusters is relatively small I also report wild bootstrap confidence intervals (Cameron et al., 2008) where appropriate.

Parallel trends between treatment and control group is the central assumption for the validity of my identification strategy. I address this issue in several ways. First, I graphically plot the development of the proportion of new fixed-term contracts in Figure 2 for all available years before and after the reform. Secondly, in the next section I also extend the above regression to account for timing of the effects, to see whether there are any significant pre-reform differences once I account for fixed group characteristics and controls. Lastly, I also compute placebo reform effects in the robustness section.

Figure 2: Trends for the proportion of fixed-term contracts by employment protection status



NOTE.- The figure plots the proportion of new fixed term contracts in the treatment and control groups.
Source: Mikrozensus sample for West German employees aged 20 to 58

Figure 2 shows that up to 2001 the trends in hiring fixed-term employees moved roughly in parallel for both treatment and control firms and diverged afterwards. After 2001, there is an

increase in the use of fixed-term contracts for new hires in both the treatment and the control group. However, the increase is considerably larger in the control group.

My identification strategy also requires that firms do not deliberately change their size in response to the reform and move from the control into the treatment group. However, I can use information on the firm-size from the social security data, to analyse whether firms changed their size around the employment protection threshold after the reform. Moreover, I can later also further alleviate these concerns by excluding firm-sizes that are close to the employment protection threshold.

6 Results

6.1 Use of fixed-term contracts

The baseline of the theoretical model predicts both a decline in temporary job creation, and an increase in the conversion of fixed-term contracts into permanent jobs. Thus, if the reform had any effect, I should observe a response in the share of fixed-term contracts in the treatment group.

Table 4: Reform effects on new fixed-term contracts

	(1)	(2)	(3)	(4)	(5)	(6)
	New contracts			All contracts		
Treated×Post 2001	−0.0406***	−0.0304***	−0.0319***	−0.0092***	−0.0052**	−0.0072**
	(0.0062)	(0.0059)	(0.0060)	(0.0015)	(0.0017)	(0.0024)
Female	−0.0049	−0.0512***	−0.0503***	0.0046	−0.0152***	−0.0154***
	(0.0063)	(0.0032)	(0.0032)	(0.0027)	(0.0020)	(0.0020)
Medium Education	−0.0659***	−0.0708***	−0.0710***	−0.0421***	−0.0422***	−0.0423***
	(0.0069)	(0.0067)	(0.0070)	(0.0020)	(0.0021)	(0.0021)
Age	−0.0197***	−0.0188***	−0.0186***	−0.0199***	−0.0196***	−0.0196***
	(0.0025)	(0.0020)	(0.0021)	(0.0018)	(0.0017)	(0.0017)
Age^2/100	0.0225***	0.0215***	0.0212***	0.0207***	0.0203***	0.0203***
	(0.0031)	(0.0025)	(0.0027)	(0.0019)	(0.0018)	(0.0018)
Wild Bootstrap CI	[-0.056;-0.016]	[-0.045;-0.012]	[-0.044;-0.012]	[-0.013;-0.005]	[-0.009;-0.002]	[-0.013;-0.001]
Firm-size Fixed Effects	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES
Industry Fixed Effects	NO	YES	YES	NO	YES	YES
Industry Trends	NO	NO	YES	NO	NO	YES
Observations	44,630	44,166	44,166	489,395	483,655	483,655
R^2	0.0425	0.0696	0.0729	0.0369	0.0474	0.0485

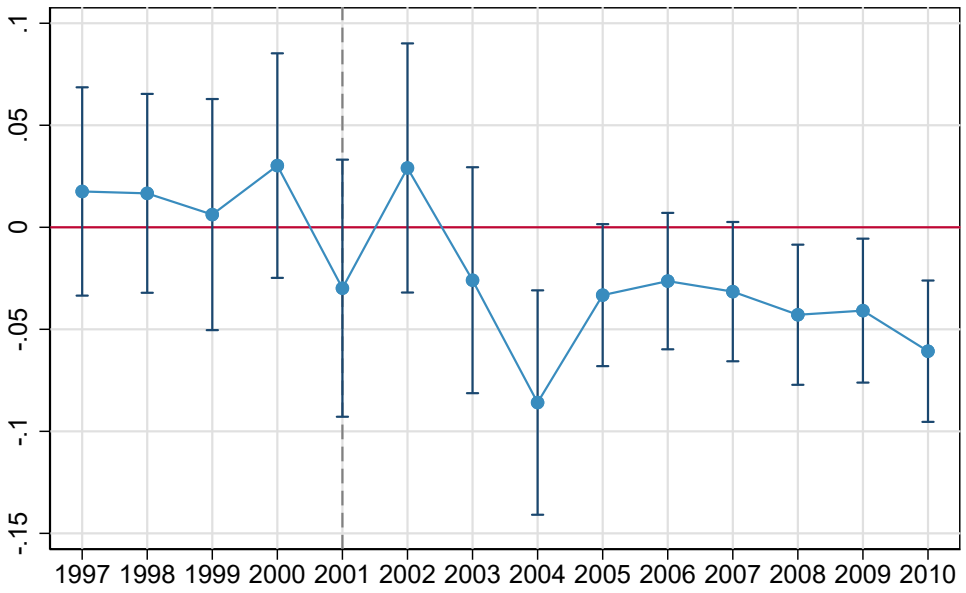
Note.- This table contains the results of regressions of the main difference-in-differences estimation equation for the likelihood that a contract is fixed-term. Columns (1) to (3) restrict the sample to new contracts, while columns (4) to (6) are computed for the sample of all employment relationships.
Cluster-robust standard errors for firm-size clusters in parentheses. $***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

Table 4 contains the results of difference-in-difference regressions for the fixed-term share in both new contracts and all employment relationships. Columns (1) to (3) limit the sample to

new contracts, while columns (4) to (6) are calculated for the whole sample of all employment relationships. Each column presents a regression of the fixed-term status of an employment contract on firms-size category and year fixed effects.

The coefficient of −0.0406 in column (1) indicates a 4.06 p.p. decrease in the fixed-term share of new contracts in treated firms. Adding additional industry fixed effects reduces the estimated effect to roughly 3 percentage points. Standard errors also remain stable and the estimated effect is highly statistically significant in all regressions. Furthermore, the effect remains stable at 3 percentage points even if I allow for differential industry-specific trends over time in column (3). Since the average fixed-term share for new contracts before the reform was 22.9%, the effect represents an economically substantial 13 % decrease.

Figure 3: Coefficient plot for the dynamics of the reform



NOTE.- The figure plots the coefficient of interactions between years and the employment protection threshold indicator in a regression of the fixed-term status of a new contract on plant-size fixed effects, year-fixed effects, the respective interactions and a all control variables included in table 4.
Source: Mikrozensus sample for West German employees aged 20 to 58

The overall fixed-term share of contracts in treated firms also declines accordingly. The coefficient of −0.0072 in Column (6) indicates a 0.72 p.p. decrease in affected firms. This effect is also sizeable as the average fixed-term share of all contracts for treated firms was 5% prior to the reform.

Next, I examine whether the effect of the reform is persistent or reverses after some time. For this I re-estimate the same regression as in Column (1) of Table 4, but replace the single difference-

in-differences indicator with interactions between each calendar year and the treatment indicator. The coefficients for these interactions are plotted in figure 3.

For pre-reform years, the coefficients of the interactions of treatment status and the calendar year are close to zero and statistically insignificant. Consequently, the trends in the treatment and control group are parallel prior to the reform.

Interestingly, the reaction to the reform is slightly delayed, as the main decrease is from 2002 to 2003. After 2003 there is a clear difference in the development of the uptake of fixed-term contracts by employment protection status. The difference slightly recedes after 2005 but stays stable at about 3 percentage points. Thus, the reform is largely persistent at effect sizes similar to the simple difference-in-differences specification.

6.2 Employment

For employment, the main theoretical prediction is a decrease in fixed-term job creation and an increase in the conversion of jobs from fixed-term into open-ended contracts (see Table 2). A decline in temporary job creation implies a decrease in the likelihood that a jobseeker will move out of unemployment. At the same time, the increase in the conversion of fixed-term contracts into permanent contracts should reduce the likelihood that employed individuals will become unemployed. Hence, I next examine the impact of the reform on flows into and out of unemployment.

As it is not possible to track individuals over time in my main sample, I use social security data to estimate the reform effects on employment flows. I report these results in table 5. While column (1) reports results on the likelihood to switch from non-employment to employment, column (2) contains the results for the probability to switch from an employment into an non-employment spell. Thus, the first column is indicative of overall job creation under the new rules, while the second represents the impact on job destruction. The columns (3) and (4) are based on the same specification, but here the official unemployment status is used as the base category instead of non-employment. Therefore only job-seekers, who are officially registered as unemployed, are considered in these columns.

The $-0,0033$ coefficient in the (1) column indicates a 0.33 percentage point decrease in the likelihood that non-employed individuals moves into employment into treated firms. Given that, on average, 5.9 % of the non-employed switch into employment per year, the effect of the reform on the transition from non-employment to employment is quite large.

In addition column (2) also reports a 0.28 p.p. decrease in the probability of moving from a job

Table 5: Employment effects of the reform - IEB Results

	(1)	(2)	(3)	(4)
	Flows from/to non-employment		Flows from/to unemployment	
	Job creation	Job destruction	Job creation	Job destruction
Treated×Post2001	−0.0033*	−0.0028*	−0.0013	−0.0037***
	(0.0018)	(0.0015)	(0.0008)	(0.0012)
Female	−0.0045*	−0.0078***	−0.0113***	−0.0092***
	(0.0024)	(0.0015)	(0.0010)	(0.0010)
Medium Education	0.0267***	0.0199***	−0.0100***	0.0108***
	(0.0010)	(0.0013)	(0.0005)	(0.0005)
Age	−0.0089***	−0.0006***	−0.0039***	0.0014***
	(0.0002)	(0.0003)	(0.0002)	(0.0001)
$Age^2/1000$	0.0694***	0.0059***	0.0536***	−0.0200***
	(0.0028)	(0.0036)	(0.0016)	(0.0007)
Firm-size Fixed Effects	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES
Observations	3,228,829	3,228,829	2,401,018	3,228,829
R^2	0.028	0.005	0.007	0.007

Note.- This table contains the results of regressions of the main difference-in-differences specification on flows from non-employment to employment, employment to non-employment, unemployment to employment and employment to unemployment .
Cluster-robust standard errors for firm-size clusters in parentheses.
$***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

in a treated firm into non-employment after the reform. Together with the results from column (1), this implies that the net effect on employment of the reform was close to zero, since the effects for both job creation and job destruction are within a standard error range.

The results are very similar when I restrict the analysis to flows to and from unemployment. Here however, the coefficient for the flow from unemployment to employment is insignificant, while the decrease in the transition from employment to unemployment is similarly sized at 0.37 percentage points.

In sum, the reform has only a small impact on net employment. This suggests that the overall decrease in the temporary contract share is not only due to a decrease in job creation and an increase in contract conversion, but also that the likelihood of starting a new job with a permanent contract has increased.[19]

6.3 Wages

Next, I estimate the wage effects of the reform. The results of log wage regressions for fixed-term and permanent employees, both for new contracts and for all employees, are shown in table 6. The first

[19]In the theoretical framework this represents an increase in the parameter λ_E

Table 6: Wage effects of the reform

	(1)	(2)	(3)	(4)	(5)	(6)
		New contracts			All contracts	
	Overall	Permanent	Fixed Term	Overall	Permanent	Fixed Term
Treated×Post 2001	0.0239***	0.0140	0.0387***	0.0066**	0.0047	0.0104
	(0.0064)	(0.0087)	(0.0123)	(0.0026)	(0.0033)	(0.0083)
Female	0.0939***	0.1169***	0.0653**	0.0137	0.0468	0.0314
	(0.0240)	(0.0316)	(0.0217)	(0.0215)	(0.0311)	(0.0176)
Medium Education	0.2232***	0.2136***	0.2045***	0.1936***	0.1976***	0.1454***
	(0.0051)	(0.0064)	(0.0118)	(0.0039)	(0.0052)	(0.0075)
Age	0.0367***	0.0471***	0.0024	0.0453***	0.0512***	−0.0233***
	(0.0055)	(0.0028)	(0.0051)	(0.0027)	(0.0012)	(0.0026)
$\text{Age}^2/100$	−0.0431***	−0.0548***	−0.0033	−0.0462***	−0.0547***	0.0321***
	(0.0071)	(0.0039)	(0.0065)	(0.0033)	(0.0016)	(0.0035)
Wild Bootstrap CI	[0.009;0.041]	[-0.004;0.037]	[0.015;0.078]	[0.001;0.011]	[-0.003;0.013]	[-0.012;0.036]
Firm-size Fixed Effects	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES
Observations	50,521	29,897	15,123	580,403	432,814	50,367
R^2	0.0444	0.0589	0.0286	0.0564	0.0598	0.0149

Note.- This table contains the results of regressions of the main difference-in-differences estimation equation for log net personal income. Cluster-robust standard errors for firm-size clusters in parentheses.
$***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

three columns contain regressions for workers in new contracts, with regression results for the overall wage effect of the reform in column (1). Furthermore, the regression in column (2) is restricted to new permanent contracts, while column (3) displays results for new temporary employees. Column (4) contains the results of a wage regression for all employees, while the sample for column (5) is confined to all permanent employees. Lastly, column (6) is computed for the sample of all fixed-term employees.

Overall, the results in column (1) indicate a highly statistically significant 2.39 % increase in wages for new contracts in the treatment group after the reform. Since the effect for the entire work-force reported in columns (4) is significantly smaller, the wage effects of the reform seam to be driven by new contracts.

The point estimates in columns (2) and (3) suggest that this wage increase is larger for fixed-term employees at 3.87 % than for permanent employees at 1.40 %. However, the coefficient for new permanent contracts is not measured precisely and it cannot be ruled out that the effect for open-ended contracts is zero. Moreover, the standard error is so large that the coefficient for open-ended contracts is within a distance of two standard errors from the coefficient for fixed-term contracts. Consequently, it can not be ruled out that the effect sizes do not differ between the contract types.

These positive wage effects of the reform are consistent with my theoretical predictions when workers have less bargaining power in fixed-term contracts. In this case, a lower probability that

a new employment contract is fixed-term after the reform implies an increase in the value of the workers' outside option, and thus also their wages. Given that the observed effects of the reform on net employment are small, while the decrease of the temporary contract share is large, the prerequisites for this case are also fulfilled. Thus, the post-reform increase in wages in treated firm also suggests lower bargaining power in fixed-term contracts.

However, a caveat remains to these results. The difference-in-differences estimates for wage effects differ from the theoretical predictions in one important respect. While wage effects in the theoretical model are due to changes in the workers' outside option, the empirical approach only analyses wage divergences between firms along their employment protection status. If the outside option of workers' generally decreases or increases across the whole labour market due to the reform, the difference-in-differences estimation would not yield any wage effect. Insofar, the wage results considered here are net of spill-overs from affected firms to the control group. Given that a proportion of the wage effect likely spills over into the control group due to workers who face common outside options, the approach likely underestimates the wage effects.

6.4 Long-term outcomes for labour market entrants

The long-term impact of starting a career in fixed-term work is often at the center of the policy debate on temporary contracts. In particular, the debate focuses on whether fixed-term contracts are a stepping stone to a permanent job or a dead-end for labour market entrants. Thus, I extend my analysis to examine how entry into the labour market under the new fixed-term employment policy affects the long-term outcomes for post-reform entrants.

Once again, I use social security data since it allows me to track individuals over time who entered the labour market at the time of the reform. In a similar difference-in-differences setup as before, I compare the difference in the outcomes of post and pre-reform entrants, who entered the labour market in treatment and control firms. This allows me to analyse outcomes such as the likelihood of still being employed by the same employer as in the entry year, the cumulative earnings, time out of employment and the number of jobs for entrants in the first five years after entry.

I report the results of the respective regressions in Table 7. All regressions in this table are based on difference-in-differences specifications that compare outcomes of entry cohorts in the first 5 years after entry based on their treatment status in the entry year.

The outcome variable for the regression in column (1) is the number of weeks without employment in the first 5 years in the labour market, while column (2) reports the effect on the official duration

Table 7: Longterm effects of the reform

	(1) Weeks non-employed	(2) Weeks unemployed	(3) Log cumulative earnings	(4) Likelihood Same Employeer	(5) Number of Jobs
Treated × Post 2001 Entry	−3.0777***	−0.3138***	0.1404***	−0.0099***	−0.2288***
	(1.0957)	(0.0592)	(0.0091)	(0.0038)	(0.0207)
Female	−10.4925***	−0.636***	−0.2898***	0.0328***	−0.5964***
	(0.5215)	(0.0292)	(0.0039)	(0.0017)	(0.01)
Medium Education	−31.2032***	−0.5778***	0.524***	0.0495***	0.3866***
	(0.8681)	(0.041)	(0.005)	(0.002)	(0.0114)
Age	−3.858***	−0.0178*	0.0337***	0.0131***	0.2128***
	(0.2313)	(0.0095)	(0.0012)	(0.0005)	(0.0027)
Age^2/100	6.1784***	0.0375***	−0.0453***	−0.0157***	−0.2881***
	(0.3324)	(0.0136)	(0.0016)	(0.0006)	(0.0035)
Firm-size at Entry Fixed Effects	Yes	Yes	Yes	Yes	Yes
Entry Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	214,833	214,833	214,804	214,833	214,833
R^2	0.0305	0.0147	0.0899	0.0230	0.0549

Note.- This table contains the results of regressions of the difference-in-differences specification for long-term effects of the reform on post-reform entrants. All columns refer to outcomes in the first five years after labor market entry.
Cluster-robust standard errors for firm-size at entry clusters in parentheses.
$***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

of unemployment over the same period. Column (3) contains results on log cumulative earnings. Moreover, the regression results for the probability of remaining at the same employer over the entire 5 year period are in column (4). Lastly, column (5) reports the reform effect on the number of jobs.

Column (1) and (2) suggest that both the time out of employment and weeks of unemployment declined considerably for post-reform entrants. Moreover, column (3) indicates a sizeable increase in the earnings in the first five years of employment. Although the likelihood to remain at the same employer marginally declines, column (5) shows that the average number of jobs also declines. Together, the results suggest that the stricter fixed-term hiring rules for small firms led to an increase in job security and long-term wages for affected labour market entrants.

Since the social security data do not contain information on the type of contract for the relevant time-frame, I have to resort to information from the Mikrozensus to assess how the reform has affected the long-term likelihood of remaining fixed-term. Similarly to the other long-term outcomes, I use information on the first year in the labour market to compare entry cohorts across their treatment status in later years. However, the Mikrozensus does not contain information on the firm-size at entry and I can not track individuals across time. Therefore, I compare individuals across their contemporaneous employment protection status and not the employment protection status at labour market entry.

Although this has the disadvantage that firms in later years could differ from the establishment

Table 8: Likelihood of long-term fixed-term employment for labor market entrants

	(1) Overall	(2) 2001 to 2005	(3) 2005 to 2010
Treated × Entry after 2001	−0.0311***	−0.0403***	−0.0320***
	(0.0048)	(0.0089)	(0.0038)
Female	0.0218***	0.0166***	0.0222***
	(0.0023)	(0.0043)	(0.0019)
Age	−0.8422***	−0.6831*	−0.8918***
	(0.2262)	(0.3461)	(0.2087)
Age^2/100	0.0001***	0.0001*	0.0001***
	(0.0000)	(0.0000)	(0.0000)
Medium Education	−0.0381***	−0.0217	−0.1271***
	(0.0114)	(0.0172)	(0.0318)
Wild Bootstrap CI	[-0.041;-0.020]	[-0.062;-0.023]	[-0.039;-0.023]
Plant-size Fixed Effects	YES	YES	YES
Year Fixed Effects	YES	YES	YES
Entry Year Fixed Effects	YES	YES	YES
Observations	95,288	22,907	78,396
R^2	0.0334	0.0398	0.0368

Note.- This table contains the results for the likelihood to stay fixed-term in later years than the reform year.
Cluster-robust standard errors for firm plant-size clusters in parentheses.
$***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

at entry with regard to the employment protection status, this still provides suggestive evidence for the long-term likelihood of remaining in temporary employment. The estimates for the long-term probability of staying fixed-term are in table 8.

The first column suggests that the probability to be fixed-term has decreased by 3.12 percentage points for workers who entered the labour market after 2001 and work in firms that are not subject to employment protection. Columns (2) and (3) contain the same estimation for two distinct time-periods. For column (1) this time-period is 2001 to 2005, whereas it is 2005 to 2010 for column (2). Splitting the sample over time shows that this decrease in the likelihood to be fixed-term is roughly persistent.

7 Robustness

Next, I conduct some additional specification checks to assess the robustness of my findings. First, I provide a more direct analysis for the parallel trend assumption between treatment and control firms in section 7.1. Second, I examine, whether deliberate changes in the firm-size in response to the reform pose a threat to my identification strategy in section 7.2. Finally, I discuss in section 7.3, how changed sample restrictions affect my results.

7.1 Parallel pre-trends

Table 9: Placebo tests for the main specification

	(1) 1997	(2) 1998	(3) 1999	(4) 2000
Treated × Post Placebo Year	0.0166	0.0103	0.0100	0.0175
	(0.0121)	(0.0125)	(0.0111)	(0.0187)
Treated × Post 2001	−0.0501***	−0.0478***	−0.0490***	−0.0566***
	(0.0109)	(0.0130)	(0.0122)	(0.0165)
Female	−0.0049	−0.0049	−0.0049	−0.0049
	(0.0063)	(0.0063)	(0.0063)	(0.0063)
Age	−0.0196***	−0.0196***	−0.0196***	−0.0196***
	(0.0025)	(0.0025)	(0.0025)	(0.0025)
$\text{Age}^2/100$	0.0225***	0.0225***	0.0225***	0.0225***
	(0.0031)	(0.0031)	(0.0032)	(0.0031)
Medium Education	−0.0658***	−0.0659***	−0.0659***	−0.0659***
	(0.0069)	(0.0069)	(0.0069)	(0.0069)
Wild Bootstrap CI (Placebo)	[-0.012;0.048]	[-0.018;0.037]	[-0.010;0.081]	[-0.023;0.063]
Firm-size Fixed Effects	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES
Observations	44,630	44,630	44,630	44,630
R^2	0.0425	0.0425	0.0425	0.0425

Note.- This table contains difference-in-differences regressions for placebo reforms for the years 1997 to 2000. Cluster-robust standard errors for firm-size clusters in parentheses. $***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

In addition to estimating leads and lags of the treatment indicator (see figure 3), I calculate placebo reform regressions on the likelihood of being fixed-term, where I shift the introduction year of the reform to years prior to the reform.

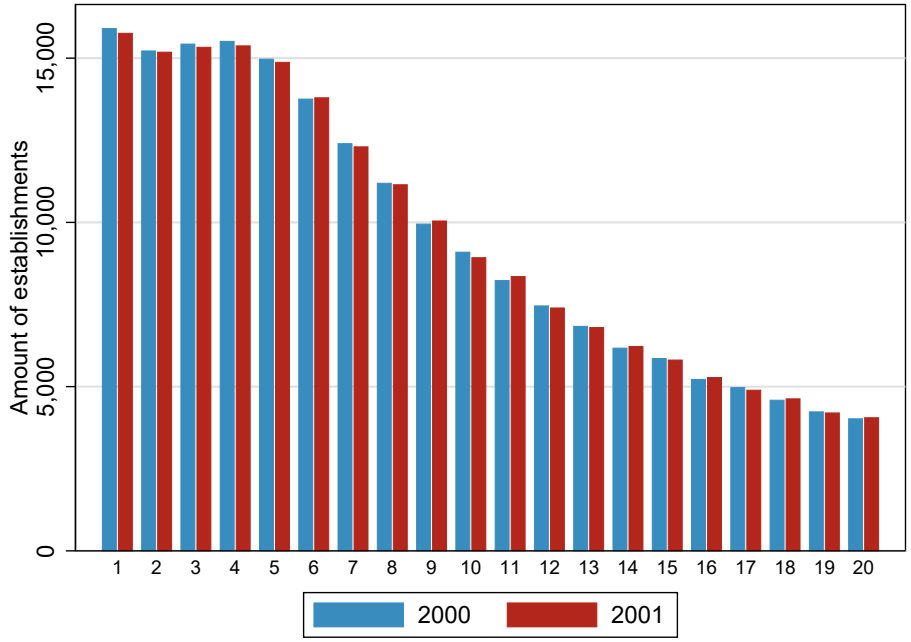
Table 9 shows the results for these regressions. All placebo reform indicators, regardless of the placebo year, are statistically insignificant and very close to zero. Taken together, this is further evidence for the validity of the parallel trend assumption.

7.2 Plant-size response of the reform

If establishments react to the reform by changing their size around the employment protection threshold, there would be contagion of the treatment group into the control group. To assess this concern, I use data from the establishment history panel to analyse how the establishment-size distribution changed around the reform year.

I start by plotting the establishment-size distribution for the pre-reform year 2000 and the year 2001 in figure 4. For the sake of clarity, only establishments up to a size of 20 employees are taken into account in this figure. This is also reasonable, since the relevant threshold at which manipulation was possible is at 5 employees. In total, 60 % of all establishments in the data set have fewer than

Figure 4: Establishment-size distribution before and after reform



NOTE.- The figures plots the establishment-size distribution for the years 2000 and 2001 for establishments with less than 20 employees.
Source: Establishment history panel

20 employees.

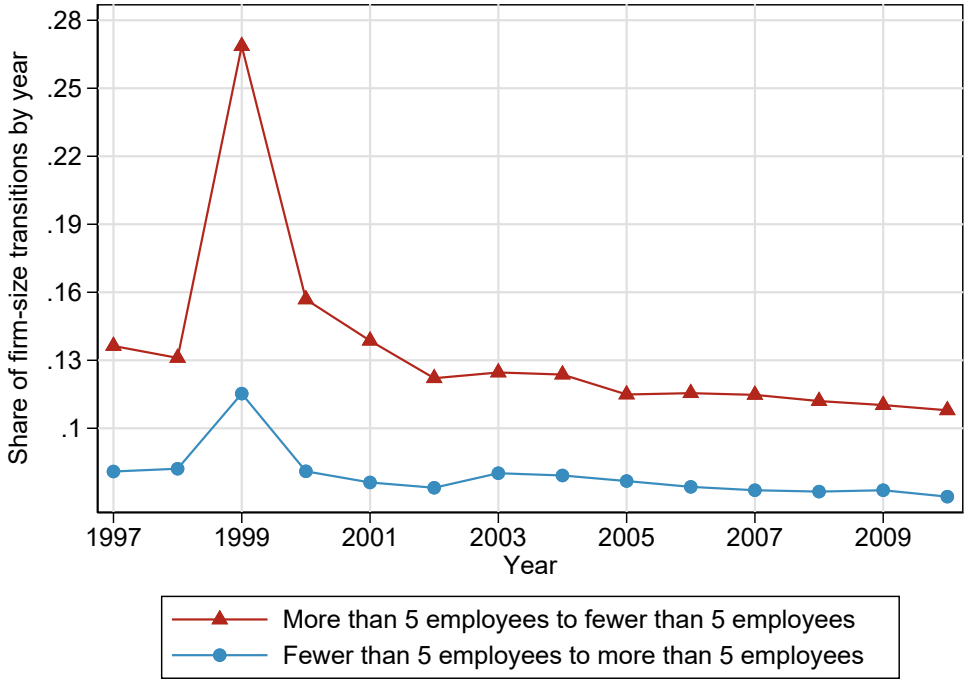
The distribution is almost identical for the two years, hinting at only small changes in firm-size for existing establishments. However, since changes are still possible that would not be evident in the overall firm-size distribution, I next use the panel structure of the data, to examine whether the rates of transition around the firm-size threshold of 5 Employees was different in the reform year.

I plot two transition rates for the 2001 employment protection threshold of 5 employees in figure 5. The first is the share of all firms that had fewer than 5 employees in the previous year and more than 5 employees in the current year. The second is the share of firms which had more than 5 employees in the previous year but are smaller in the current year.

While there were substantial adjustments during the reform of the employment protection threshold in 1999, the transition rates in 2001, the year of the fixed-term contract reform, are roughly at their average level. Therefore, changes in firm-size around the fixed-term reform are not a cause for concern.

Nevertheless, I also compute some of my main results for subsamples that exclude firm-size-categories right at the employment protection threshold to further alleviate concerns about potential firm-size manipulation. For this I estimate the main regressions for the likelihood of being fixed-

Figure 5: Firm-size transitions



NOTE.- The figures plots the transitions rates of firms with fewer than 5 employees in the previous year to more than 5 employees in the current year and vice versa.
Source: Establishment history panel

term with two restrictions. The first restriction is that I exclude firm-sizes of 4 or 9 employees. These are the firm-sizes right below the employment protection thresholds for different years in the sample. For the second restriction, I only consider the time period around the reform during which the employment protection threshold was at 5 workers, and exclude firms right at the threshold. The estimates for these regressions are in table 10.

Both restrictions have little effect on the outcome of the likelihood that a new contract is fixed-term. The difference-in-differences coefficient changes very little and is close to the original estimate. Moreover, the statistical significance of the results also remains largely unchanged.

This mostly also applies to results for the wage effect on new contracts. However, for the second restriction the wage effect becomes statistically insignificant. This is likely due to large loss of observations in this sub sample.

In summary, deliberate manipulation of the plant-size around the reform does not seem to impair the validity of my estimates.

Table 10: Robustness: Excluding firms right at the threshold

	(1)	(2)	(3)	(4)	(5)	(6)
		Fixed Term			Log net income	
	All	Restriction 1	Restriction 2	All	Restriction 1	Restriction 2
Treated × Post 2001	−0.0406***	−0.0407***	−0.0557***	0.0238***	0.0249***	0.0101
	(0.0062)	(0.0067)	(0.0159)	(0.0057)	(0.0067)	(0.0208)
Age	−0.0197***	−0.0198***	−0.0200***	0.0392***	0.0396***	0.0392***
	(0.0025)	(0.0027)	(0.0032)	(0.0037)	(0.0042)	(0.0045)
Age^2/100	0.0225***	0.0227***	0.0229***	−0.0463***	−0.0470***	−0.0430***
	(0.0031)	(0.0035)	(0.0039)	(0.0048)	(0.0053)	(0.0057)
Female	−0.0049	−0.0054	0.0032	0.1113***	0.1065***	0.0633**
	(0.0063)	(0.0069)	(0.0068)	(0.0292)	(0.0301)	(0.0266)
Medium Education	−0.0659***	−0.0663***	−0.0595***	0.2271***	0.2253***	0.1776***
	(0.0069)	(0.0073)	(0.0135)	(0.0040)	(0.0044)	(0.0178)
Wild Bootstrap CI	[-0.054;-0.016]	[-0.056;-0.024]	[-0.123;-0.025]	[0.010;0.039]	[0.008;0.037]	[-0.074;0.060]
Observations	44,630	40,814	11,675	42,510	38,870	11,276
Firm-size Fixed Effects	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES
R^2	0.0425	0.0433	0.0305	0.0531	0.0520	0.0512

Note.- This table contains difference-in-differences regressions with different sample restrictions for firms right at the employment protection threshold. For Restriction 1 firms with 4 and 9 employees are excluded. For Restriction 2 the sample is limited to time-periods, when the employment protection bound was at 5 Employees and firms with 4 Employees are excluded. Cluster-robust standard errors for firm-size clusters in parentheses.
$***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

7.3 Additional Specification Checks

Finally, I also examine how the restriction of the sample to certain maximum firm-sizes and the specification of the firm-size category fixed effects affect my findings. For this purpose, I explore two modifications to the main specification. For one, I limit the maximum firm-size in the sample to 20 or 10 employees.[20] And secondly, I calculate all estimates with more detailed fixed effects for firm-size categories.

In the baseline estimation the fixed-effects on firm-size categories are defined in size steps of 5 employees, whereas the detailed fixed-effects directly represent firm-sizes in the smallest available increment.

The results for these specification checks for the likelihood that a new contract is fixed-term are in table 11. None of the specification changes has a large impact on the coefficient of interest. The estimated effect size remains roughly at 3 to 4 percentage points for any of the specifications. However the standard errors are larger in the sample, where I reduce the maximum plant size to 10 employees and the observed time-frame accordingly.

[20]When I restrict the maximum firm-size to 10 employees, I also restrict the observation period to years where the employment protection threshold was at 5 employees, as this restriction excludes all firms above the employment protection threshold for years where the threshold was at 10 employees.

Table 11: Robustness: Firm-size restrictions (Fixed-term share)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Standard Sample			Under 20 Employees			Under 10 Employees		
Treated × Post 2001	−0.0406***	−0.0400***	−0.0302***	−0.0366***	−0.0360***	−0.0259**	−0.0362*	−0.0357*	−0.0425**
	(0.0062)	(0.0061)	(0.0060)	(0.0096)	(0.0095)	(0.0092)	(0.0177)	(0.0178)	(0.0172)
Female	−0.0049	−0.0041	−0.0511***	−0.0107	−0.0097	−0.0537***	−0.0116	−0.0118	−0.0402***
	(0.0063)	(0.0060)	(0.0032)	(0.0069)	(0.0065)	(0.0031)	(0.0093)	(0.0095)	(0.0059)
Age	−0.0197***	−0.0197***	−0.0187***	−0.0180***	−0.0180***	−0.0172***	−0.0250***	−0.0248***	−0.0246***
	(0.0025)	(0.0025)	(0.0020)	(0.0024)	(0.0024)	(0.0018)	(0.0053)	(0.0053)	(0.0048)
Age^2/100	0.0225***	0.0226***	0.0215***	0.0203***	0.0205***	0.0197***	0.0290***	0.0288***	0.0289***
	(0.0031)	(0.0031)	(0.0025)	(0.0030)	(0.0030)	(0.0022)	(0.0067)	(0.0067)	(0.0060)
Medium Education	−0.0659***	−0.0666***	−0.0713***	−0.0599***	−0.0609***	−0.0650***	−0.0423**	−0.0430**	−0.0434***
	(0.0069)	(0.0066)	(0.0066)	(0.0076)	(0.0072)	(0.0069)	(0.0136)	(0.0135)	(0.0131)
Wild Bootstrap CI	[-0.054;-0.020]	[-0.053;-0.021]	[-0.043;-0.013]	[-0.057;-0.005]	[-0.057;-0.003]	[-0.045;0.011]	[-0.076;0.011]	[-0.076;0.011]	[-0.080;-0.007]
Firm-size Fixed Effects	Standard	Detailed	Detailed	Standard	Detailed	Detailed	Standard	Detailed	Detailed
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry Fixed Effects	NO	NO	YES	NO	NO	YES	NO	NO	YES
Observations	44,630	44,630	44,166	32,839	32,839	32,504	5,996	5,996	5,862
R^2	0.0425	0.0434	0.0702	0.0321	0.0333	0.0612	0.0206	0.0212	0.0462

Note.- This table contains difference-in-differences regressions for the probability that a new contract is fixed term for different sub-samples of the data and different fixed effects specifications. For columns (4) to (7) the sample is restricted to firms with less than 20 Employees. The regressions in the last three columns include only firms with less than 10 Employees and time-periods, when the employment protection bound was at 5 Employees. Cluster-robust standard errors for firm-size clusters in parentheses. $*** p < 0.01$, $** p < 0.05$, $* p < 0.1$

Moreover, the wage effect for new contracts also differs only minimally across the various specification checks. I present the results for these effects in table A1 in the appendix. Again, there are only marginal changes in the effect size across specifications.

In general, though, the specification checks suggest that my results are robust to changes in firm-size constraints and the use of more detailed fixed-effects.

8 Conclusion

This article examines how a 2001 reform that raised the legal requirements for the use of temporary contracts by small establishments has affected employment, wages and the careers of labour market entrants.

I find a sizeable decrease in the use of fixed-term contracts for affected firms and a reduction in both job creation and job destruction. However, the net effects on employment are relatively small. In line with my theoretical predictions for a scenario in which workers have less bargaining power in fixed-term contracts and a decrease in the proportion of new fixed-term contracts, I also find positive reform effects on wages.

Furthermore, I present some evidence that the reform has contributed to a reduction in labour market segmentation for new entrants. For labour market entrants, who joined affected firms after the reform year, I find an increase in cumulative wages in the first 5 years, as well as a reduction in the time out of work. In addition, I find suggestive evidence that the likelihood of remaining in temporary employment for longer periods of time is diminishing.

Together, my findings contribute to an economic literature that is critical of fixed-term employment as an exception to otherwise strict employment protection in permanent jobs. My findings suggest that restrictions on fixed-term contracts could have some beneficial effects for workers, at least in the short term. However, this abstracts from the longer-term adaptations of firms to changed legislation. For example, firms could respond to increased labour costs by replacing labour with capital. Thus, longer-term adjustments are an interesting avenue for future research.

Lastly, my findings provide further evidence for the various current proposals across Europe to limit temporary employment. The two main issues in the public debates on these proposals are the impact of temporary work on the careers of young workers, and the possible negative employment effects of restricting fixed-term contracts.

Although some of the current plans to limit fixed-term employment are more extensive than the reform I examine, my results suggest that fears of large negative employment effects seem less warranted. In addition, I provide some evidence that imposing such restrictions on fixed-term work could be beneficial to the stability of new labour market entrants' careers. However, whether the more broad new reforms will have similar outcomes will ultimately be a question for further research.

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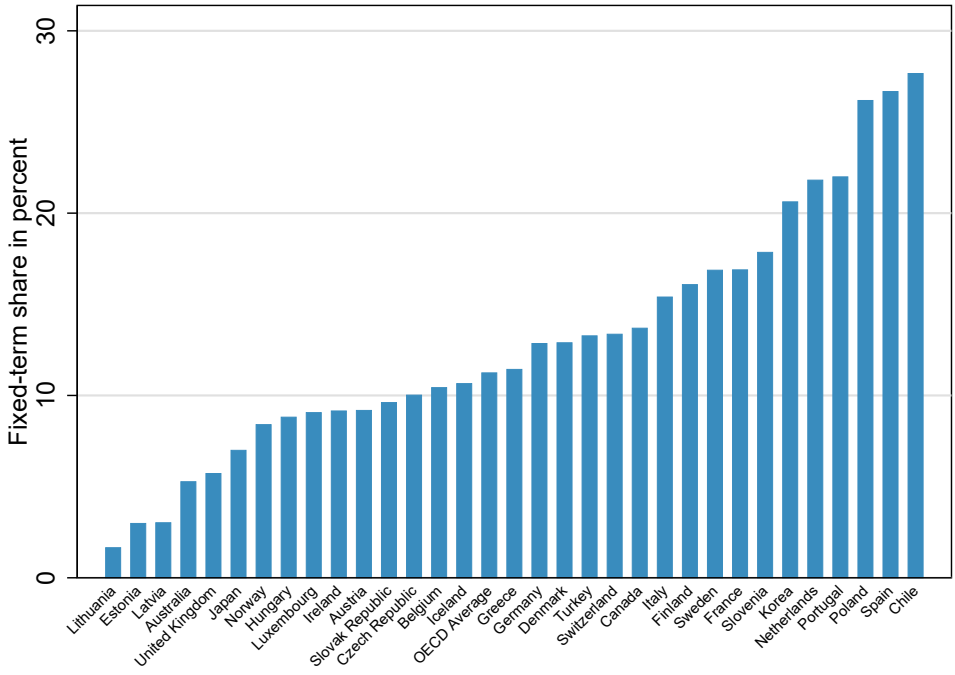
A Additional tables and figures

Figure A1: Share of fixed-term employment in OECD countries 2017



NOTE.- The figures plots the share of employment contracts that are fixed-term for OECD countries
Source: OECD (2018), Temporary employment (indicator)

Table A1: Robustness: Firm-size restrictions (Wages)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Standard Sample			Under 20 Employees			Under 10 Employees		
Treated × Post 2001	0.0238***	0.0239***	0.0155**	0.0263***	0.0264***	0.0171**	0.0333**	0.0326**	0.0239*
	(0.0057)	(0.0057)	(0.0056)	(0.0064)	(0.0064)	(0.0068)	(0.0105)	(0.0102)	(0.0111)
Female	0.1113***	0.1120***	0.1603***	0.1397***	0.1407***	0.1886***	0.1256***	0.1270***	0.1454***
	(0.0292)	(0.0295)	(0.0280)	(0.0311)	(0.0315)	(0.0292)	(0.0232)	(0.0235)	(0.0281)
Age	0.0392***	0.0391***	0.0367***	0.0427***	0.0426***	0.0406***	0.0460***	0.0459***	0.0439***
	(0.0037)	(0.0037)	(0.0039)	(0.0034)	(0.0033)	(0.0034)	(0.0050)	(0.0050)	(0.0049)
Age^2/100	−0.0463***	−0.0462***	−0.0435***	−0.0508***	−0.0506***	−0.0484***	−0.0513***	−0.0510***	−0.0490***
	(0.0048)	(0.0047)	(0.0050)	(0.0044)	(0.0043)	(0.0043)	(0.0064)	(0.0063)	(0.0061)
Medium Education	0.2271***	0.2268***	0.2018***	0.2290***	0.2283***	0.1956***	0.1641***	0.1626***	0.1249***
	(0.0040)	(0.0041)	(0.0064)	(0.0045)	(0.0048)	(0.0087)	(0.0260)	(0.0265)	(0.0254)
Wild Bootstrap CI	[0.010;0.037]	[0.012;0.039]	[0.001;0.031]	[0.014;0.048]	[0.013;0.046]	[0.006;0.041]	[0.010;0.062]	[0.010;0.062]	[-0.001;0.051]
Firm-size Fixed Effects	Standard	Detailed	Detailed	Standard	Detailed	Detailed	Standard	Detailed	Detailed
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry Fixed Effects	NO	NO	YES	NO	NO	YES	NO	NO	YES
Observations	42,510	42,510	42,053	31,287	31,287	30,955	5,820	5,820	5,687
R^2	0.0531	0.0535	0.0787	0.0606	0.0612	0.0895	0.0594	0.0606	0.0958

Note.- This table contains difference-in-differences regressions for the log-personal income for new contracts for different sub-samples of the data and different fixed effects specifications. For columns (4) to (7) the sample is restricted to firms with less than 20 Employees. The regressions in the last three columns include only firms with less than 10 Employees and time-periods, when the employment protection bound was at 5 Employees. Cluster-robust standard errors for firm-size clusters in parentheses. $***$ $p < 0.01$, $**$ $p < 0.05$, $*$ $p < 0.1$

B Model derivations

In this appendix I will provide further details of the theoretical framework set out in section 3. In particular, I provide detailed definitions of the surplus of creating jobs by contract type (B.1). Moreover, I discuss the comparative statics of an increase in contract writing costs for temporary contracts for both the case of equal bargaining power (B.2) across contract types, and lower bargaining power (B.3) in fixed-term contracts.

B.1 Expected surplus for job creation

The expected surplus for creating a job of type λ is given by the sum of the associated expected profit of the firm and the workers valuation of the match minus the worker's outside option. For a permanent contract, the expected profit of the firm is given by

$$\Pi_P(\lambda) = \int_0^\infty \left[\int_0^\tau [y - w(\lambda)] e^{-rt} dt - f e^{-r\tau} \right] \lambda e^{-\lambda\tau} d\tau - c_P = \frac{y - w(\lambda) - \lambda f}{r + \lambda} - c_P, \tag{7}$$

where the inner integral represents the discounted sum of expected profits until a random termination date τ, while the term $fe^{-r\tau}$ is the discounted value of the firing costs at this date. The whole expression in the brackets is then integrated over the poisson process density $\lambda e^{-\lambda\tau}$ that determines at which date τ a job of type λ becomes unproductive.

Similarly, a workers valuation of a job is given by

$$V_P(\lambda) = \int_0^\infty \left[\int_0^\tau w(\lambda) e^{-rt} dt + U e^{-r\tau} \right] \lambda e^{-\lambda\tau} d\tau = \frac{w(\lambda) + \lambda U}{r + \lambda}, \tag{8}$$

where U is the workers valuation of the outside option of the match. The surplus for a permanent contract is then given by

$$S_P(\lambda) = \frac{y - rU - \lambda f}{r + \lambda} \tag{9}$$

The expected profit of a firm for a fixed term contract is the sum of the discounted profit flow up to an endogenous date $D(\lambda)$ and the discounted value of continuing in a permanent contract at time D less the cost of writing the contract c_{FT}.

$$\Pi_{FT}(\lambda, D) = \int_0^D [y e^{-\lambda\tau} - w(\lambda, D)] e^{-r\tau} d\tau + \max\left(\Pi_P(\lambda), 0\right) \cdot e^{-\lambda D} \cdot e^{-rD} - c_{FT} \tag{10}$$

Note that in the discounted flow of profits up to D only the productivity y is evaluated at

its survival probability, while wages are only discounted with r. This reflects that employers have to keep paying the wage until D if the productivity shock arrives before the expiration date of the contract. Moreover, the continuation value is simply the discounted maximum of either the permanent contract profit for the same job-type λ or 0.

Similarly, a worker's valuation of of a new fixed-term contract is given by

$$V_T(\lambda, D) = \int_0^D w(\lambda, D)e^{-r\tau}d\tau + \max\left(V_P(\lambda), U\right) \cdot e^{-\lambda D} \cdot e^{-rD} + U(1 - e^{-\lambda D})e^{-rD} \tag{11}$$

Lastly, just as for an open-ended contract, the expected surplus for a fixed-term contract is defined as the sum of the firm's expected profit and the worker's valuation less his or her outside option.

$$S_{FT}(\lambda, D) = \int_0^D \left[ye^{-\lambda\tau} - rU\right] e^{-r\tau}d\tau + \max\left(S_P(\lambda), 0\right) \cdot e^{-(r+\lambda)D} - c_{FT} \tag{12}$$

At the equilibrium, workers and firms will choose the optimal duration $D^*(\lambda)$, since it maximizes the expected surplus for a contract given λ. Hence, $D^*(\lambda)$ is obtained from the first order condition of equation 12 with regard to duration D:

$$\frac{\partial S_T(\lambda, D)}{\partial D} = ye^{-\lambda D} - rU - (r + \lambda)e^{-\lambda D}\max\left(S_P(\lambda), 0\right) = 0 \tag{13}$$

B.2 Case 1: Equal bargaining power across contract types

At the equilibrium four conditions are satisfied.

First, jobs are only created if the surplus of a temporary employment contract is greater than zero. The parameter λ_{FT} is the maximum shock arrival rate at which jobs can be created, and is determined by the point at which the surplus of a temporary contract with optimal contract duration is zero. To obtain this expression, I substitute the first-order condition of the fixed-term contract surplus with regard to the duration D from equation 13 into $S_{FT}(\lambda_{FT}) = 0$. As a result, the equilibrium condition on the fixed-term job creation $h^{FTJCR}(\lambda_{FT}, \theta, c_{FT})$ is given by

$$h^{FTJCR}(\lambda_{FT}, \theta, c_{FT}) = \frac{y - rU(\theta)}{r + \lambda_{FT}} + \lambda_{FT}\frac{U(\theta)\left(e^{-rD^*(\lambda_{FT})} - 1\right)}{r + \lambda_{FT}} - c_{FT} = 0. \tag{FTJCR}$$

Secondly, as jobs with a shock arrival rate above λ_P are not continued after the end of a temporary contract, the fixed-term job destruction is obtained from the point at which the surplus of a

permanent contract is zero $S_P(\lambda_P) = 0$.

$$h^{FTJDR}(\lambda_P, \theta) = \lambda_P - \frac{y - rU(\theta) - rc_P}{c_P + f} = 0 \tag{FTJDR}$$

Thirdly, the parameter λ_E, which specifies whether jobs start with a fixed-term or permanent contract, is obtained by equating the surplus of a fixed-term contract at optimal duration with the surplus of an unlimited contract.

$$h^{PvsFT}(\lambda_E, \theta, c_{FT}) = \lambda_E \frac{U(\theta)\left(e^{-rD^*(\lambda_E)} - 1\right)}{r + \lambda_E} + \frac{\lambda_E f}{r + \lambda_E} + (c_P - c_{FT}) = 0 \tag{PvsFT}$$

Lastly, the fourth condition is a free entry condition for firms and equalizes the expected surplus of a job with the costs of its creation κ.

$$h^{EC}(\theta, c_{FT}) = \kappa - q(\theta)(1 - \gamma)\left[\int_{\underline{\lambda}}^{\lambda_E} S_p(\lambda) dG(\lambda) + \int_{\lambda_E}^{\lambda_T} S_T(\lambda) dG(\lambda)\right] = 0 \tag{EC}$$

The valuation of unemployment in the first three equations $U(\theta)$ is an increasing function in the labour market tightness θ and is given by equation 2. By substituting this into the three conditions, I obtain an equilibrium with the parameters $(\theta^*, \lambda_E^*, \lambda_P^*, \lambda_{FT}^*)$.

Next, I obtain the effect on an increase in c_{FT} on these parameters by using total differentials of the equilibrium conditions

1. *Impact on job creation:* The effect on λ_{FT} is calculated from total differentials of the free-entry condition and the fixed-term job creation rule.

$$\frac{\partial \lambda_{FT}}{\partial c_{FT}} = \underbrace{-\frac{\frac{\partial h^{FTJCR}}{\partial c_{FT}}}{\frac{\partial h^{FTJCR}}{\partial \lambda_{FT}}}}_{\text{Direct Effect}} + \underbrace{\frac{\frac{\partial h^{FTJCR}}{\partial \theta}}{\frac{\partial h^{FTJCR}}{\partial \lambda_{FT}}} \times \frac{\frac{\partial h^{EC}}{\partial c_{FT}}}{\frac{\partial h^{EC}}{\partial \theta}}}_{\text{Equilibrium Feedback Effect}} \tag{14}$$

First, note that the derivatives for the fixed-term job creation rule with regard to c_{FT}, θ and

λ_{FT} are:

$$\begin{aligned}
\frac{\partial h^{FTJCR}(\lambda_{FT},\theta,c_{FT})}{\partial c_{FT}} &= -1 < 0 \\
\frac{\partial h^{FTJCR}(\lambda_{FT},\theta,c_{FT})}{\partial \theta} &= \frac{\partial U}{\partial \theta}\left[\frac{-r}{r+\lambda_{FT}} + \frac{\lambda_{FT}(e^{-rD^*}-1)}{r+\lambda_{FT}}\right] \\
&= -\frac{\partial U}{\partial \theta}\left[\frac{r}{r+\lambda_{FT}} + \frac{\lambda_{FT}(1-e^{-rD})}{r+\lambda_{FT}}\right] < 0 \\
\frac{\partial h^{FTJCR}(\theta,\lambda_{FT},c_{FT})}{\partial \lambda_{FT}} &= \frac{ye^{-(r+\lambda_{FT})D^*(\lambda_{FT})}[(r+\lambda_{FT})D^*(\lambda)+1]-y}{(r+\lambda_{FT})^2} < 0
\end{aligned}$$

The expression for $\frac{\partial h^{FTJCR}(\theta,\lambda_{FT},c_{FT})}{\partial \lambda_{FT}}$ is calculated using a derivative of $S_{FT}=0$ with regard to $\lambda_F T$ and $\lambda_P > \lambda_{FT}$. The negative sign of the expression stems from the fact that $e^{-x} < \frac{1}{x+1}$ for $x > 0$:

$$\frac{ye^{-(r+\lambda_{FT})D^*(\lambda_{FT})}[(r+\lambda_{FT})D^*(\lambda)+1]-y}{(r+\lambda_{FT})^2} < \frac{y\,\frac{1}{(r+\lambda_{FT})D^*(\lambda)+1}\,[(r+\lambda_{FT})D^*(\lambda)+1]-y}{(r+\lambda_{FT})^2} = 0$$

Second, the derivatives of the free-entry condition are:

$$\begin{aligned}
\frac{\partial h^{EC}(\theta,c_{FT})}{\partial c_{FT}} &= -q(\theta)(1-\gamma)\left[\int_{\underline{\lambda}}^{\lambda_E}\frac{\partial S_P(\lambda)}{\partial c_{FT}}dG(\lambda) + \int_{\underline{\lambda}}^{\lambda_E}\frac{\partial S_{FT}(\lambda)}{\partial c_{FT}}dG(\lambda)\right] \\
&= -q(\theta)(1-\gamma)\left[\int_{\lambda_E}^{\lambda_{FT}} -1\; dG(\lambda)\right] \\
&= q(\theta)(1-\gamma)\Big[G(\lambda_{FT}) - G(\lambda_E)\Big] > 0 \\
\frac{\partial h^{EC}(\theta,c_{FT})}{\partial \theta} &= -(1-\gamma)q'(\theta)\Big[\int_{\underline{\lambda}}^{\lambda_E} S_p(\lambda)dG(\lambda) + \int_{\lambda_E}^{\lambda_{FT}} S_{FT}(\lambda)dG(\lambda)\Big] \\
&\quad -(1-\gamma)q(\theta)\Big[\int_{\underline{\lambda}}^{\lambda_E}\frac{\partial S_P(\lambda)}{\partial \theta}dG(\lambda) + \int_{\lambda_E}^{\lambda_{FT}}\frac{\partial S_{FT}(\lambda)}{\partial \theta}dG(\lambda)\Big] > 0
\end{aligned}$$

The positive sign in the derivative with regard to θ follows from the definition of $q(\theta)$ as decreasing function of θ and from the negative signs of both $\frac{\partial S_P(\lambda)}{\partial \theta}$ and $\frac{\partial S_{FT}(\lambda)}{\partial \theta}$.[21]

Together, this implies a negative direct effect of an increase of c_{FT} and a positive feedback effect. Saggio et al. (2018) show that the direct effect dominates. Thus, the overall effect of an increase of the contract writing costs on job creation is negative.

[21] Since $S_P(\lambda) = \frac{y-rU-\lambda f}{r+\lambda}$ it follows that $\frac{\partial S_p(\lambda)}{\partial U} < 0$. Moreover the value of U is increasing in labor market tightness. Therefore it holds that: $\frac{\partial S_p(\lambda)}{\partial \theta} = \frac{\partial S_p(\lambda)}{\partial U(\theta)} \cdot \frac{\partial U(\theta)}{\partial \theta} < 0$. Similarly, it also applies that $\frac{\partial S_{FT}(\lambda)}{\partial U(\theta)} < 0$, which implies $\frac{\partial S_{FT}(\lambda)}{\partial \theta} < 0$

2. *Impact on job destruction:* The total differential of the fixed-term job destruction rule is

$$\frac{\partial h^{FTJDR}}{\partial \lambda_P} d\lambda_P + \frac{\partial h^{FTJDR}}{\partial \theta} d\theta = 0 \tag{15}$$

Since $\frac{\partial h^{FTJDR}}{\partial \lambda_P} = 1$ and $\frac{\partial h^{FTJDR}}{\partial \theta} = \frac{r}{c_P + f} \frac{\partial U(\theta)}{\partial \theta}$ this implies that the effect of the reform on the conversion of fixed-term contracts to permanent is given by:

$$\frac{\partial \lambda_P}{\partial c_{FT}} = \frac{r}{c_P + f} \frac{\partial U(\theta)}{\partial \theta} \frac{\partial \theta}{\partial c_{FT}} > 0 \tag{16}$$

As more temporary jobs are converted into open-ended contracts when λ_P rises, job destruction decreases.

3. *Impact on the type of contract at job start:* From the total differential of $h^{PvsFT}(\lambda_E, \theta, c_{FT})$ it follows that:

$$\frac{\partial \lambda_E}{\partial c_{FT}} = - \underbrace{\frac{\frac{\partial h^{PvsFT}}{\partial c_{FT}}}{\frac{\partial h^{PvsFT}}{\partial \lambda_E}}}_{\text{Direct Effect}} - \underbrace{\frac{\frac{\partial h^{PvsFT}}{\partial \theta}}{\frac{\partial h^{PvsFT}}{\partial \lambda_E}} \times \frac{\partial \theta}{\partial c_{FT}}}_{\text{Feedback Effect}} \tag{17}$$

The derivatives used in equation 17 are

$$\begin{aligned}
\frac{\partial h^{PvsFT}(\lambda_E, \theta, c_{FT})}{\partial c_{FT}} &= -1 < 0 \\
\frac{\partial h^{PvsFT}(\lambda_E, \theta, c_{FT})}{\partial \lambda_E} &= \frac{-\lambda_E f r \frac{\partial D^*}{\partial \lambda}(\lambda_E) e^{-rD^*(\lambda_E)}}{(1 - e^{-rD^*(\lambda_E)})(r + \lambda_E)} \geq 0 \\
\frac{\partial h^{PvsFT}(\lambda_E, \theta, c_{FT})}{\partial \theta} &= -\lambda_E \frac{\partial U(\theta)}{\partial \theta} \frac{1 - e^{-rD^*(\lambda_E)}}{r + \lambda_E} < 0
\end{aligned}$$

Again, there is both a direct effect and a feedback effect on λ_E, albeit the overall effect remains undetermined this time. However, Saggio et al. (2018) show that the direct effect is larger then the feedback effect if

$$\lambda_T < \frac{y - r c_{FT}}{f + c_{FT}} \tag{18}$$

4. *Effect on wages:* Wages are only affected through the valuation of the outside option. The derivatives of wages with regard to the cost of writing fixed-term contracts are:

$$\begin{aligned}
\frac{\partial w_P}{\partial c_{FT}} &= (1 - \gamma) r \frac{\partial U(\theta)}{\partial \theta} \frac{\partial \theta}{\partial c_{FT}} < 0 \\
\frac{\partial w_{FT}}{\partial c_{FT}} &= \left(\frac{ry}{r + \lambda} \left[-\frac{r e^{rD^*} (1 - e^{-(r+\lambda)D^*})}{(1 - e^{rD^*})^2} + \frac{(r + \lambda) e^{-(r+\lambda)D^*}}{(1 - e^{rD^*})} \right] \frac{\partial D^*}{\partial U} + (1 - \gamma) r \right) \frac{\partial U(\theta)}{\partial \theta} \frac{\partial \theta}{\partial c_{FT}} < 0
\end{aligned}$$

B.3 Case 2: Differential bargaining power across contract types

I now consider the case in which workers in temporary contracts can only extract a lower rent from the match-surplus compared to permanent contracts. In this case, the equilibrium conditions remain largely similar to the case of equal rent sharing. However, as discussed in the main text, the valuation of the outside option in equation 3 now also depends on λ_E. Thus the changed equilibrium conditions for this case are

$$h^{FTJCR2}(\lambda_{FT}, \lambda_E, \theta, c_{FT}) = \frac{y - rU(\theta, \lambda_E)}{r + \lambda_{FT}} + \lambda_T \frac{U(\theta, \lambda_E)\left(e^{-rD^*(\lambda_{FT})} - 1\right)}{r + \lambda_{FT}} - c_{FT} = 0 \quad \text{(FTJCR2)}$$

$$h^{FTJDR2}(\lambda_P, \lambda_E, \theta) = \lambda_P - \frac{y - rU(\theta, \lambda_E) - rc_P}{c_P + f} = 0 \quad \text{(FTJDR2)}$$

$$h^{PvsFT2}(\lambda_E, \theta, c_{FT}) = \lambda_E \frac{U(\theta, \lambda_E)\left(e^{-rD^*(\lambda_E)} - 1\right)}{r + \lambda_E} + \frac{\lambda_E f}{r + \lambda_E} + (c_P - c_{FT}) = 0 \quad \text{(PvsFT2)}$$

$$h^{EC2}(\theta, \lambda_E, c_{FT}) = \kappa - q(\theta)\left((1 - \gamma_P)\int_{\underline{\lambda}}^{\lambda_E} S_P(\lambda)dG(\lambda) + (1 - \gamma_{FT})\int_{\lambda_E}^{\lambda_{FT}} S_{FT}(\lambda)dG(\lambda)\right) = 0 \quad \text{(EC2)}$$

Next, I again use total differentials of these conditions to derive the effects of the reform on the parameters of interest. In a first step, I separately evaluate the reform effect on λ_E and θ if the respective other parameter remains constant. I use a total differential of $h^{EC2}(\theta, \lambda_E, c_{FT})$ to evaluate the reform effect on labour market tightness. For a given level of λ_E the effect of an increase in c_{FT} on θ is negative as

$$\frac{\partial \theta}{\partial c_{FT}} \mid_{\lambda_E \text{ is constant}} = -\frac{\frac{\partial h^{EC2}}{\partial c_{FT}}}{\frac{\partial h^{EC2}}{\partial \theta}} < 0 \quad (19)$$

This results from the positive sign of both the numerator and the denominator

$$\frac{\partial h^{EC2}(\theta, \lambda_E, c_{FT})}{\partial c_{FT}} = q(\theta)(1 - \gamma)\Big[G(\lambda_{FT}) - G(\lambda_E)\Big] > 0$$

$$\frac{\partial h^{EC2}(\theta, \lambda_E, c_{FT})}{\partial \theta} = -q'(\theta)\left[(1 - \gamma_P)\int_{\underline{\lambda}}^{\lambda_E} S_P(\lambda)dG(\lambda) + (1 - \gamma_{FT})\int_{\lambda_E}^{\lambda_{FT}} S_{FT}(\lambda)dG(\lambda)\right]$$
$$+ q(\theta)\Big[(1 - \gamma_P)\int_{\underline{\lambda}}^{\lambda_E} \frac{\partial S_P(\lambda)}{\partial \theta}dG(\lambda) + (1 - \gamma_{FT})\int_{\lambda_E}^{\lambda_{FT}} \frac{\partial S_{FT}(\lambda)}{\partial \theta}dG(\lambda)\Big] > 0$$

Similarly, a total differential of $h^{PvsFT2}(\lambda_E, \theta, c_{FT})$ can be used to evaluate the change in λ_E for an unchanged level of labor market tightness.

$$\frac{\partial \lambda_E}{\partial c_{FT}} \mid_{\theta \text{ is constant}} = -\frac{\frac{\partial h^{PvsFT2}}{\partial c_{FT}}}{\frac{\partial h^{PvsFT2}}{\partial \lambda_E}} > 0 \quad (20)$$

This results stems from the different signs of the two derivatives used

$$\frac{\partial h^{PvsFT2}(\lambda_E, \theta, c_{FT})}{\partial c_{FT}} = -1 < 0$$
$$\frac{\partial h^{PvsFT2}(\lambda_E, \theta, c_{FT})}{\partial \lambda_E} = \frac{-\lambda_E f r \frac{\partial D^*}{\partial \lambda}(\lambda_E) e^{-rD^*(\lambda_E)}}{(1 - e^{-rD^*(\lambda_E)})(r + \lambda_E)} > 0$$

Thus, an increase in the contract writing costs for fixed-term contracts leads to a higher proportion of jobs that start in permanent contracts, if the effect of labour market tightness is not taken into account. Together with the above result on the effect of changes on c_{FT} on labour market tightness, this shows that that reactions to the reform on λ_E and θ counteract each other. However, labour market tightness still overall decreases, while the effects on the valuation of unemployment and λ_E are not clear from the outset.[22]

Next, I use these results to analyse job creation, job destruction, the likelihood that a job starts on a permanent contract and wages.

1. *Impact on job creation:* Using the total differential of $h^{FTJCR2}(\lambda_{FT}, \lambda_E, \theta, c_{FT})$ reveals a direct effect of c_{FT} on λ_{FT} and two feedback effects through λ_E and θ.

$$\frac{\partial \lambda_{FT}}{\partial c_{FT}} = -\frac{\frac{\partial h^{FTJCR2}}{\partial c_{FT}}}{\frac{\partial h^{FTJCR2}}{\partial \lambda_{FT}}} - \frac{\frac{\partial h^{FTJCR2}}{\partial \theta}}{\frac{\partial h^{FTJCR2}}{\partial \lambda_{FT}}} \frac{\partial \theta}{\partial c_{FT}} - \frac{\frac{\partial h^{FTJCR2}}{\partial \lambda_E}}{\frac{\partial h^{FTJCR2}}{\partial \lambda_{FT}}} \frac{\partial \lambda_E}{\partial c_{FT}} \tag{21}$$

with

$$\frac{\partial h^{FTJDR2}(\lambda_{FT}, \lambda_E, \theta, c_{FT})}{\partial \lambda_{FT}} = \frac{y e^{-(r+\lambda_{FT})D^*(\lambda_{FT})}[(r + \lambda_{FT})D^*(\lambda) + 1] - y}{(r + \lambda_{FT})^2} < 0$$
$$\frac{\partial h^{FTJDR2}(\lambda_{FT}, \lambda_E, \theta, c_{FT})}{\partial \lambda_E} = -\frac{\partial U}{\partial \lambda_E}\left[\frac{r}{r + \lambda_{FT}} + \frac{\lambda_{FT}(1 - e^{-rD})}{r + \lambda_{FT}}\right] < 0$$
$$\frac{\partial h^{FTJDR2}(\lambda_{FT}, \lambda_E, \theta, c_{FT})}{\partial \theta} = -\frac{\partial U}{\partial \theta}\left[\frac{r}{r + \lambda_{FT}} + \frac{\lambda_{FT}(1 - e^{-rD})}{r + \lambda_{FT}}\right] < 0$$
$$\frac{\partial h^{FTJDR2}(\lambda_{FT}, \lambda_E, \theta, c_{FT})}{\partial c_{FT}} = -1 < 0$$

Thus, the first two terms still represent a direct effect that leads to a decrease in λ_{FT} for an increase in c_{FT} and a feedback effect that increases λ_{FT} through a decline in the outside option as the labour market becomes less tight. However, the third term is a feedback effect that acts through changes in the likelihood that new jobs are started with permanent contracts. If the reform leads to an increase in λ_E this second term acts in the same direction as the direct

[22] See Saggio et al. (2018) for a detailed discussion and a graphical representation of the interaction of λ_E and θ.

effect, as the increase in the share of new permanent contracts increases the outside option which in turn leads to a decline in λ_{FT}. Moreover, Saggio et al. (2018) show that in this case the expected overall effect is the same both for differential and equal rent-sharing. However, if λ_E decreases the overall effect on λ_{FT} is not clear.

2. *Impact on job destruction:* Using a total differential of the fixed-term job destruction rule yields an indeterminate effect of the reform on λ_P

$$\frac{\partial \lambda_P}{\partial c_{FT}} = \frac{r}{c_P + f}\left[\frac{\partial U(\theta, \lambda_E)}{\partial \theta}\frac{\partial \theta}{\partial c_{FT}} + \frac{\partial U(\theta, \lambda_E)}{\partial \lambda_E}\frac{\partial \theta}{\partial \lambda_E}\right] \lesseqgtr 0 \tag{22}$$

3. *Impact on the type of contract at job start:* Similarly to the other case, a combination of the free-entry condition (EC2) and the contract type rule (PvsFT2) allow for a wide range of decreases and increases in λ_E.

4. *Effect on wages:* Wages are only affected through the valuation of the outside option. The derivatives of wages with regard to the cost of writing fixed-term contracts are:

$$\begin{aligned}
\frac{\partial w_P}{\partial c_{FT}} &= (1-\gamma)r\left[\frac{\partial U(\theta, \lambda_E)}{\partial \theta}\frac{\partial \theta}{\partial c_{FT}} + \frac{\partial U(\theta, \lambda_E)}{\partial \lambda_E}\frac{\partial \theta}{\partial \lambda_E}\right] \lesseqgtr 0 \\
\frac{\partial w_{FT}}{\partial c_{FT}} &= \left(\frac{ry}{r+\lambda}\left[-\frac{re^{rD^*}(1-e^{-(r+\lambda)D^*})}{(1-e^{rD^*})^2} + \frac{(r+\lambda)e^{-(r+\lambda)D^*}}{(1-e^{rD^*})}\right]\frac{\partial D^*}{\partial U} + (1-\gamma)r\right) \\
&\quad \times \left[\frac{\partial U(\theta, \lambda_E)}{\partial \theta}\frac{\partial \theta}{\partial c_{FT}} + \frac{\partial U(\theta, \lambda_E)}{\partial \lambda_E}\frac{\partial \theta}{\partial \lambda_E}\right] \lesseqgtr 0
\end{aligned}$$

Contrary to the other case, if the overall reform effect on the outside option is positive, positive wage effects are also possible.

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ZEW – Leibniz-Zentrum für Europäische Wirtschaftsforschung GmbH Mannheim
ZEW – Leibniz Centre for European Economic Research

L 7,1 · 68161 Mannheim · Germany
Phone +49 621 1235-01
info@zew.de · zew.de