Employment Protection and Fixed Term Contracts: Evidence from a German Reform

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Abstract

Governments across Europe have liberalized temporary labor contracts to stimulate employment. However, due to worries about the long-term outcomes of these reforms, there are several recent policy proposals advocating renewed restrictions of fixed-term employment. This paper evaluates the effects of a 2001 reform in Germany, which has changed the ability of smaller firms to establish fixed-term contracts. After the reform hiring employees on temporary contracts became relatively harder for plants below the employment protection threshold of 5 employees. Using a basic differences-in-differences approach that compares plants below and above the official employment protection threshold, I find that the reform has led to a decrease in the use of fixed-term contracts by small plants but has not markedly changed their employment behavior. Furthermore, for post-reform labor market entrants who joined affected plants, I note an increase in cumulated wages and a reduction in the time out of work in the first 5 years as well as suggestive evidence that implies a reduced likelihood to remain fixed-term employed.

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

1 Introduction

Over the last three decades, the use of fixed-term contracts has increased considerably in large parts of Europe. This trend has been driven by several reforms that have removed restrictions on fixed-term work. The aim of these reforms was to increase the flexibility of firms to respond to economic changes.[1] As the political costs for a more wide-ranging revision of employment protection were high, the strict dismissal protection rules for permanent employees remained largely unchanged, while fixed-term jobs were liberalized. However, concerns about the long-term effects of these reforms have recently led to increased political efforts in several European countries to reverse some of these liberalizations.[2]

An extensive economic literature studies the effects of firms' access to temporary work when strict employment protection provisions apply to open-ended contracts. Remarkably, the assessment of the benefits of fixed-term employment is mixed in the related economic theory. Although temporary contracts could offer companies the opportunity to hire less skilled workers without risking high firing costs in case of non-performance (Bentolila and Saint-Paul, 1992), there could be some adverse effects. For instance, employers might exhibit a different hiring behavior, when both types of contract are easily available. As firms face high firing costs for open-ended contracts, while fixed-term contracts without firing costs are available, employers might substitute from permanent to fixed-term contracts. (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 with weak restrictions on the creation of temporary contracts could contribute to a segmentation of the labor market with protected insiders in permanent employment and outsiders who remain in repeated temporary contracts in lower paid entry level jobs.

In this paper I analyze a 2001 German reform that made it more difficult for small establishments to use fixed-term contracts. Before the reform plants below the employment protection threshold of 5 employees did not have to provide a legal justification for the use of temporary contracts. Prior to the reform, jurisdiction had only considered the possible circumvention of employment protection as the

[1] Such flexibility measures were often motivated by fears that strict employment protection has rendered European labor 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 labor markets in Europe compared with the US.

[2] Political proposals to again restrict fixed-term contracts were discussed in Spain, Italy and Germany (see The Economist, 2018; Reuters, 2016; Zeit, 2018). These political debates focused both on worries about the circumvention of employment protection through fixed-term contracts and the impact on the stability of young workers' careers .

sole criterion for the admissibility of a fixed-term contract. Thus, only plants affected by employment protection had to provide a justification, why a job was fixed-term. The reform introduced a list of objective grounds, why a contract could be fixed-term. These objective grounds were largely identical to those considered by the courts for larger plants before the reform, but were evaluated by the courts regardless of plant-size after the reform. Thus, the legal barriers to hire employees on fixed-term contracts for plants below the employment protection threshold rose compared with those above the threshold.

To derive empirical predictions for this reform, I use a theoretical search and matching model by Cahuc et al. (2016) that explicitly describes the contract-type choice of firms. In the model firms hire workers to take advantage of production opportunities with different expected length. I first present the equilibrium conditions for job creation, job destruction and the share of jobs that are started with a permanent contract. Moreover, I outline, how these conditions are related to employment and wages. Then I present the results of a comparative static analysis to show, how these variables change as the use of fixed-term contracts becomes more difficult.

I then use a basic differences-in-differences research design to answer the following questions: (1) How did the reform influence the take-up of fixed-term contracts? (2) What were the effects on flows to and out of employment? (3) How did the reform affect wages? (4) What impact did the reform have on the careers of labor market entrants?

As the first step, I show that the share of fixed-term workers decreased in treated plants, 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. The decrease of 3 percentage points amounts to 10 % of the average use of fixed-term contracts in Germany. Thus, these effects have considerable economic significance.

In a next step, I analyze how the reform has affected employment flows. Contrary to the large effects on the share of fixed-term contracts, I only find very minor employment effects associated with the reform. Both the decline in job creation and the increase in the conversion from temporary to permanent contracts are quite small. This suggests that the strong effect on the share of fixed-term contracts is partly due to more jobs starting directly with a permanent contract.

For earnings, the theoretical model predicts that a temporary contract restriction can 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 particularly emerge if the employment response to a reform is small and the reduction in the fixed-term share is large. Consistent with this model prediction and my other empirical results, I find a 2.3 % increase in the wages of new contracts.

Lastly, I also examine some long-term effects for labor market entrants, who joined affected firms after the reform. The core result of this analysis is a sizable increase in cumulated wages over the first years in the labor market and a decrease in the 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 labor market entrants.

Together these results imply that the reform had only little effect on employment but a positive effect on new contract wages and increased longer term job security for labor market entrants.

This article contributes to several strands of economic literature. First, it adds further evidence to the literature that studies, how employment protection rules relate to the use of fixed-term contracts and how reforms of temporary contracts affect their share in total employment (Centeno and Novo, 2012; Bassanini and Garnero, 2013; Hijzen et al., 2017). While much of this work is based on aggregate cross country data (Lazear, 1990; Kahn, 2010; Garibaldi and Violante, 2005; Bassanini and Garnero, 2013), several more recent studies have examined the effects of changes in fixed-term legislation using with-in country variation and micro data. However, many of these studies (Autor et al., 2007; Aguirregabiria and Alonso-Borrego, 2014; Cappellari et al., 2012) are at the firm level, whereas certain interesting outcomes of reforms such as long-term wage effects require employee data. Consequently, this article adds further evidence to a smaller set of empirical work that combines employee data with reform variation (e.g. García-Pérez et al., 2018; Hijzen et al., 2017; Saggio et al., 2018).

Second, in contrast to most other literature, I analyze a scenario in which fixed-term rules became more stringent in Germany. Most other studies are concerned with fixed-term contract liberalizations in Southern European countries like Spain and Italy. This is particularly interesting, since Southern European countries traditionally have a different approach to employment protection than Germany (see. Boeri et al., 2011). For example, temporary contracts are used much more intensively in some countries such as Spain.[3] Moreover, the rigor of employment protection laws also differs between Southern Europe and Germany (OECD, 2013). Thus, the evidence in this paper is obtained under

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

different labor market conditions and allows or a better understanding of the effects of fixed-term work restrictions in countries where temporary employment is used less.

Third, my work documents novel positive wage effects for a restriction of fixed-term contracts. This is evidence for lower negotiating power for workers in temporary employment relationships.

Lastly, the article also contributes to a growing literature that analyzes 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; Saggio et al., 2018). I find only minor effects on employment but relatively strong increases in labor market entrants' cumulated wages and job security. This assessment of the impact of the reform on later outcomes for labor market entrants also relates to a literature on the long-term effects of labor market conditions at 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 a limitation of fixed-term contracts might affect the use of different contract types, employment and wages. Section 4 describes the datasets, I use for the analyses. Section 5 presents the empirical strategy Section 6 reports the results. Section 7 provides a range of robustness checks for my findings. Finally, section 8 concludes.

2 Institutional Background

2.1 Employment protection law and fixed-term contracts in Germany

Most employment contracts in Germany are commonly unlimited. 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 severance payment. The firm must also observe notice periods ranging from 2 weeks to 6 months, depending upon the seniority of the employee.

In addition, larger establishments have to comply with further firing restrictions from the Dismissal Protection Act (Kündigungsschutzgestz). Specifically, employers have to provide evidence that one of the particular dismissal reasons named in the law is satisfied. The act only allows dismissals related to the personal situation of the person to be dismissed (e.g. long-term sickness), breach of contractual

duties (e.g. fraud or theft), or operational reasons related of the business of the employer. For a dismissal due to operational reasons, which is the most common type of dismissal, the employer must show, that the job position permanently ceases 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. Whether these stricter firing rules apply is entirely determined by the number of full-time employees.[4] In 2001 the size threshold for the employment protection law was at 5 employees.[5]

Alternatively firms can hire employees under 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).[6] However, before the 2001 reform, which I analyze in this paper, smaller firms were exempt from providing a justification for using fixed-term contracts.

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

Table 1: Reform variation

	Before 2001	**After 2001**
Subject to EPL ($\geq$5 Employees)	Objective reasons (EPL circumvention)	Objective reasons listed in law
Not subject to EPL ($<$5 Employees)	No restriction	Objective reasons listed in law

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

In January 2001, the Part-Time and Fixed-Term Employment Act was signed into to law to im-

[4]More precisely, newer versions of this law are based on the number of full-time equivalents, where workers up to 20 hours per week are counted with a factor of 0.50, and workers up to 30 hours with a factor of 0.75

[5]Before 1997 the threshold for dismissal protection was at 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 set at 10 employees. The 1996 and 1999 employment protection reforms are analyzed by Bauer et al. (2007), while Bauernschuster (2013) discusses the 2004 reform.

[6]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 can not legally offer workers a further fixed-term contract without naming an objective reason. This exception from the default is based an a temporary exemption to boost employment that was introduced in 1985. This rule for short fixed-term contracts was renewed two times and lastly made permanent. However, these rules remained unaffected by the reform. See Hunt (2000) for an analysis of the 1985 law that introduced short-term fixed-term contracts without objective reason.

plement the EU Directive 1999/70/C. This new law changed the rules concerning the justification of fixed-term contracts.[7] Prior to the reform, the judiciary assessed the admissibility of grounds for the use of a temporary contract by examining whether the contract could possibly be used to circumvent dismissal protection. By definition, establishments below the employment protection threshold could not circumvent dismissal protection. As a result, these establishments were not restricted in their use of fixed-term contracts. This changed as the new law specified a list of legal justifications for a contract limitation regardless of the employment protection status.[8]

The objective reasons listed in the law are largely similar to those courts evaluated to determine whether the use of fixed-term contracts did constitute a employment protection circumvention before the reform. However, these new reasons are now evaluated by the courts independently of their potential for the circumvention of employment protection laws and plants below the employment protection threshold also have to providence evidence that these reasons are satisfied. Thus, after the reform hiring workers in fixed-term contracts based on an objective reason became comparatively harder for firms below the employment protection threshold (see Table 1 for a short overview of the variation introduced by the law).

This introduces variation in the potential to use fixed term contracts between the firms that are subject to employment protection rules and the firms that are not. Thus, I can compare workers in firms above and below the employment protection threshold before and after the reform.

3 Theory

Intuitively, plants below the employment protection threshold 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 not clear from the outset. 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.[9] I

[7] The same law also introduced new rules, which allowed longer fixed term contracts without objective reasons for workers above the age of 58. However, these rules should act in the same direction as the rest of the law, as firms above the employment protection threshold have a higher incentive to make use of temporary contracts.

[8] The reasons include temporary need of the job 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.

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

use this model to qualitatively study, how a increase in the costs of fixed term contracts should affect the employment behavior, the type of contracts chosen and wages in establishments below the employment protection threshold.

In the model, firms hire workers to take advantage of production opportunities with different anticipated durations. Specifically, jobs differ in the arrival rate of shocks that renders them unproductive. Firms and employees jointly maximize a match surplus that depends on this rate and share it through Nash bargaining. Fixed-term and open-ended contracts differ in their termination rules.

Firms have to pay a firing cost to dismiss an employee on an open-ended contract if 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 has to keep paying the employees salary until the contract term expires. Jobs that start fixed-term can be converted to permanent when the contract expires. Alternatively fixed-term contract can be terminated free of charge after the term date. Renewing the job with another fixed-term contract is not possible. These rules lead to different surpluses for permanent and temporary contracts for a given expected productive duration of a job.

In a first step, this allows me to distinguish different thresholds on the shock arrival rate that determine whether jobs are started permanent, started fixed-term and then converted to open-ended contracts or started on temporary contracts but terminated after the term or not created at all. In a second step, I can derive how a change in the costs of writing a fixed-term contract affects these thresholds. This also yields predictions about job creation, job destruction and the share of new contracts that are started permanent. Lastly, I can analyze 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. For one, fewer fixed-term jobs are created. At the same time, it increases the incentive to retain fixed-term employees in permanent contracts, which reduces job destruction.[10]

[10]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). However, much of the theoretical literature on fixed-term contracts models them as screening device to learn about productivity (e.g. Faccini, 2014; Blanchard and Landier, 2002), which is at odds with two empirical observations. 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)

Beyond these predictions on employment, I also study, how the reform should affect wages of permanent and fixed-term employees. In the baseline that abstracts from differences in bargaining power between contract types, there is only a negative wage effect on wages through a decrease in labor market tightness. However, once I assume that employees have less bargaining power in temporary jobs, a positive effect on wages is possible. This effect results from an increase in the value of the outside option of the employee, as the probability that a prospective new job is permanent and she therefore is able to exercise greater negotiating power in this job increases.

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. Labor 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 modeled as shocks, which reduce the output produced per time unit to $y = 0$ and arrive at the Poisson rate λ.[11] 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.[12] Temporary contracts have an endogenous duration $D(\lambda)$ till they expire and can not be ended before. If a job becomes unproductive before the end of its entire term, the company must continue to pay the

[11] 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 labor markets.

[12] 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.

employee's wage until the contract expires.[13] 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 is not possible.[14] Firms pay contract writing costs that differ between fixed term (c_{FT}) and permanent contracts (c_P).[15] The reform is later modeled 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.

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 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 $S_{FT}(\lambda_P) > 0$.[16]

Figure 1 illustrates the contract choice for different levels of λ.[17] 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 the job in a permanent con-

[13] This represents the standard case 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.

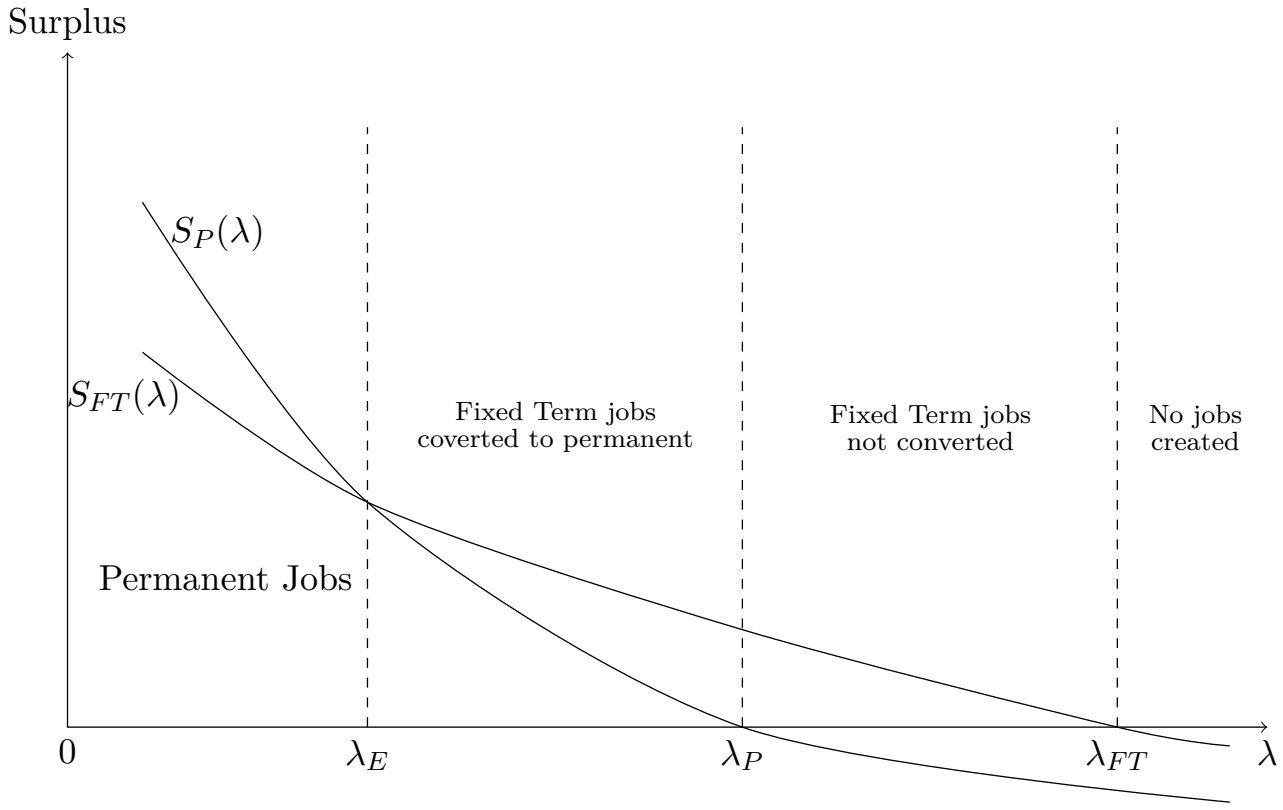
[14] Although it is theoretically possible to establish consecutive fixed term contracts with a valid objective reason in Germany, the law regards this as the default case. Renewing a fixed-term contracts requires a special new objective reason for an extension. Moreover the courts evaluate the number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

[15] 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.

[16] A more detailed overview of the equilibrium conditions can be found in the appendices B.2 and B.3.

[17] The figure is based on figure 6 in Cahuc et al. (2016)

Figure 1: Choice Between Fixed-Term and Permanent Contracts



tract is below zero for $\lambda \in [\lambda_P, \lambda_{FT}]$, jobs in this range are started fixed-term and 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 not created.

The equilibrium in the labor market is defined by the conditions that determine the parameters λ_{FT},λ_P and λ_E and a condition on the matching between workers and vacancies. Unemployed workers u find vacancies v according to a standard constant returns to scale aggregate 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 labor 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 relation. 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 $c = \{FT, P\}$. Similarly to Saggio et al. (2018) I will later discuss the reform effects both for an equal bargaining power ($\gamma_{FT} = \gamma_P = \gamma$) for fixed-term and permanent workers and for a case were fixed-term workers have a

lower bargaining power ($\gamma_{FT} < \gamma_P$) .

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.

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

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 $\gamma_{FT} = \gamma_P$ the value of the outside option solely depends on z, γ, κ and the labor market tightness θ, while it additionally depends on λ_E for differential rent-sharing.

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

Continuing wage payments under a permanent contract satisfy the following Nash bargaining first order condition:

$$\gamma_P \left[\Pi_P(\lambda) + f \right] = (1-\gamma_P) \left[V_p(\lambda) - U \right] \tag{4}$$

Consequently this implies that the wage for a permanent contract is given by

$$w_p = \gamma_p(y + rf) + (1 - \gamma_p)rU, \tag{5}$$

which would reduce to the outside option rU if the workers bargaining power γ_P was zero. The continuing wage for fixed-term contract can be obtained in similar way and is given by the following wage equation:

$$w_{FT} = \gamma_{FT}\left(\frac{ry}{r+\lambda}\frac{1 - e^{-(r+\lambda)D^*(\lambda)}}{1 - e^{-rD^*(\lambda)}}\right) + (1 - \gamma_{FT})rU \tag{6}$$

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 Comparative statics

I now summarize how an increase in the cost of establishing a fixed-term contract c_{FT} affects the equilibrium values $(\theta^*, \lambda_E^*, \lambda_P^*, \lambda_{FT}^*)$ and the equilibrium wages for permanent and fixed-term contracts, if rent-sharing is identical for both contract types. A more detailed overview of the comparative statics is in the appendices B.2 and B.3.

For a fixed level of labor market tightness, creating jobs with high shock arrival rates becomes less desirable as λ_{FT} declines. However, there is also a feedback channel to this effect. As less jobs are created after the cost increases, market tightness decreases, which in turn leads to a decrease in the value of the outside option. Lastly a decline in the outside option shifts λ_{FT} upwards. However, it can be shown that the direct effect dominates, and overall λ_{FT} declines for an increase in c_{FT}.

Furthermore, more fixed-term contracts are converted to permanent contracts as λ_P increases with an increase in c_{FT}. This comes from both a direct effect of costs on λ_P and an indirect effect through decreasing labor market tightness.

There are two countervailing effects on the parameter λ_E that determines whether jobs are started with fixed-term or permanent contracts. First, there is a positive direct effect on λ_E, since for a fixed θ

it is more costly for firms to establish fixed-term contracts. Second, a declining labor market tightness could possibly offset this effect by a decrease of $U(\theta)$, which negatively affects λ_E. The overall effect on λ_E is not clear from the onset and depends on different model parameters.

Generally the only possible wage effect on permanent wages in the setup with identical bargaining across contract types is negative, as decreasing labor market tightness leads to a decrease of the outside option and hence also to a decrease of the permanent wage $w_p(\lambda)$. A decline in $U(\theta)$ can also lead to a decrease in fixed-term wages. However, there is a second effect in the opposite direction for fixed-term wages as the reform leads to an increase in the optimal duration of fixed-term contracts, which in turn leads to an increase in the the first wage term in equation 6.

For the case of differential rent sharing, the value of the outside option does not only depend on labor market tightness θ but also on the parameter λ_E that determines whether a job is created on a fixed-term or a permanent contract. Intuitively, a higher λ_E raises the value of unemployment, as the chance that a new employment contract is permanent and lets employees keep a larger share of the match surplus increases.

This influence of the parameter λ_E on the value of unemployment adds additional indirect effects to the reform effects on the parameter λ_{FT} and λ_P. Interestingly, these additional influences can act in different directions, as the reform effect on λ_E is ex-ante ambiguous. For example, if the reform increases λ_E, this leads to a decrease of $U(\lambda_E, \theta)$, which has a negative impact on λ_{FT}, whereas in the case where the reform decreases the λ_E, this raises $U(\lambda_E, \theta)$ and thus also λ_{FT}.

Overall the direct effect of the reform on λ_T still dominates provided that λ_E increases, which leads to lower job-creation in affected firms after the reform. However, the effect on job-destruction is now indeterminate as λ_P can be affected in either direction depending on the basic parameters of the model.

Interestingly, differential rent sharing between permanent and fixed-term contracts now also allows for positive wage effects. This happens if the overall effect of an increase in c_{FT} on $U(\lambda_E, \theta)$ is positive. As in the case with equal rent sharing, this effect is again clear for permanent wages, but can be counteracted by a reaction through the optimal duration for fixed-term wages.

Table 2 provides a short summary of the main model predictions.

Table 2: Model predictions

1. Equal bargaining power $\gamma_{FT} = \gamma_P$		**2. Less bargaining power in temporary jobs** $\gamma_{FT} < \gamma_P$	
$\lambda_{FT} \downarrow$	Less job creation as jobs with very short expected productive durations are no longer created	$\lambda_{FT} \downarrow$	Less job creation as jobs with very short expected productive durations are no longer created
$\lambda_P \uparrow$	More temporary jobs are converted to permanent contracts once their term expires	$\lambda_P \updownarrow$	Effect on contract conversion remains a priori indeterminate
$\lambda_E \updownarrow$	A priori indeterminate effect on contract-type at start	$\lambda_E \updownarrow$	A priori indeterminate effect on contract-type at start
$w_{FT} \downarrow, w_P \downarrow$	Negative wage effect as labor market tightness decreases	$w_{FT} \updownarrow, w_P \updownarrow$	Indeterminate wage effect as an increasing λ_E can lead to a higher outside option.

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

4 Data Sources

4.1 Mikrozensus

To analyze 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 includes its official duration for time periods before and after the reform. Contrary to that, German Social Security data only includes the fixed term status of employees for years after 2011. Second, the data includes questions on the plant size, which can be used to determine whether a workers plant 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 analyze the effect of fixed term employment legislation on hiring behavior.

I distinguish three skill groups based on the highest educational qualification. An individual is medium-skilled if she has completed an apprenticeship or graduated from high school (*Abitur*). A person is high-skilled if she graduated from college or university. However, the proportion of high

skilled workers in treated plants is very small.[18] 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 analyze the wage effects of the reform.

For the analysis, I restrict the sample to West German individuals between the ages of 20 and 62. I further exclude people, 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

While the Mikrozensus contains detailed information on contract types and wages, it is not suitable for analyzing employment effects. Since it is a repeated cross-sectional data set, each person is observed at only one point in time.

However, administrative social security data allows for a more 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 labor market entrants. Consequently, I use a 2% random sample of the population of workers and plants covered by the social security system in Germany, to study employment effects and long-term reform outcomes in more detail.

I apply the same sample restrictions as for the Mikrozensus data to make the results more 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 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

[18] For new contracts, there are less than 41 observations in 2001, and ca. 50 observations per year on average

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 variances by year and gender (Gartner, 2005). However, since 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: Proportions of fixed-term employees by gender, age and education

Women								
	20 to 26	27 to 32	33 to 38	39 to 43	44 to 48	49 to 54	55 to 59	60 to 65
Low Education	14.7%	7.5%	6.5%	5.9%	4.6%	3.4%	2.7%	2.7%
Middle Education	20.9%	7.9%	5.4%	5.3%	4.6%	3.3%	2.6%	3.2%
Men								
	20 to 26	27 to 32	33 to 38	39 to 43	44 to 48	49 to 54	55 to 59	60 to 65
Low Education	18.3%	7.9%	4.8%	3.8%	3.2%	2.7%	2.6%	2.9%
Middle Education	25.9%	10.7%	4.8%	3.3%	3.1%	2.8%	3.1%	3.8%

NOTE.- This table displays the proportion of fixed-term employees with-in gender, age and education groups
Source: Mikrozensus sample for West German employees aged 20 to 65

In the following, I display some summary statistics from the Mikrozensus sample. Table 3 shows the proportions of fixed-term employees for 8 different age and education groups separately for women and for men over the entire observation period. The proportion of fixed term contracts in the two youngest age groups is much higher than in all other age groups. Additionally in these two age groups the fixed-term proportion increases with education, while the difference between different education types is much smaller for older age groups. Gender differences are relatively small.

The fixed-term shares for younger workers is relatively high reaching almost 26 % for male middle-educated workers under 26. For workers above the age of 26 the fixed-term shares are drastically smaller. However, 9.9 % of all contracts observed are fixed-term, which is close to the OECD average of 11.2 %. (OECD, 2017).

Figure A2 in the appendix plots the time trend of the proportion of fixed term contracts both for the overall working population and for new contracts. Both measures have increased markedly over the sample period. Between 1996 and 2010 the proportion of fixed term contracts for new employment

relationships increased by 15 percentage points.

Lastly, I report some summary statistics by employment protection status and time period in table 4.

Table 4: Summary Statistics for the Mikrozensus data

	Workers in firms that are not affected by employment protection			
	Pre reform period (1996-2000)		Post reform period (2001-2010)	
	Mean	St. Dev.	Mean	St. Dev.
Fixed Term	0.044	0.205	0.060	0.238
Real net monthly income	1931.946	1082.619	2025.035	1421.328
Hours worked per week	30.477	13.382	28.213	13.617
Female	0.653	0.476	0.658	0.474
Age	40.093	11.429	41.200	11.334
Service Sector	0.680	0.466	0.717	0.450
Middle Education	0.769	0.422	0.773	0.419
Observations	30,423		149,592	
	Workers in firms that are subject to employment protection			
	Pre reform period (1996-2000)		Post reform period (2001-2010)	
	Mean	St. Dev.	Mean	St. Dev.
Fixed Term	0.053	0.225	0.075	0.264
Real net monthly income	2019.406	967.815	2101.062	1238.247
Hours worked per week	35.077	9.340	34.477	10.082
Female	0.436	0.496	0.456	0.498
Age	40.624	10.967	41.428	10.923
Service Sector	0.539	0.498	0.615	0.486
Middle Education	0.832	0.374	0.828	0.378
Observations	247,605		752,799	

NOTE.- This table provides means and standard deviations for fixed-term status, real income, hours worked and age by employment protection 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 65

Matching the stronger incentive to use temporary contracts, the proportion of fixed-term employees is higher in firms that are affected by employment protection. Average wages are only slightly higher for

firms above the employment protection threshold. Average working hours are however larger. Moreover, the proportion of female workers is higher for firms below the employment protection bound.

While the average age is nearly identical for the two groups, firms that are not affected by employment protection tend to employ more low skilled individuals and are more likely to be active in the services sector of the economy.

Altogether, employees in firms above and below the employment protection threshold are generally quite similar.

5 Empirical Strategy

The Part-Time and Fixed-Term Contracts Act of 2001 has raised the regulatory requirements for the use of temporary contracts for plants below the employment protection threshold. This induces variation in the relative costs of writing fixed term contracts between firms above and below the employment protection threshold. Thus, I can use a simple difference-in-differences research design, which compares employees in treated plants (i.e. not subject to employment protection) to those in control plants (i.e. subject to employment protection) before and after the reform.

For outcome variables such as the likelihood of a new contract to be fixed-term, the transition from unemployment to employment and net wages the estimation equation is given by

$$\textsc{Outcome}_{ipt} = \alpha \textsc{No EPL}_{ip} \times \textsc{Post } 2001_t + \beta \textsc{Plant-size}_p + \gamma \textsc{year}_t + \lambda \mathrm{X}_{ipt} + \varepsilon_{ipt}, \qquad (7)$$

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

The variable $\textsc{No EPL}_{if} \times \textsc{Post } 2001_t$ is an interaction effect between the employment protection status of a workers firm and an indicator variable for years after 2001. I use the respectively applicable plant-size limit for each year to determine, whether a firm is subject to employment protection or not. Since I control for plant-size and year fixed effects, the effect of the reform α is identified by the change in the respective outcome variable in firms above the employment protection threshold, relative to the other firms, in 2001 or later relative to 2000 or earlier.

My identification strategy requires that plants 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 plant size, to analyze whether plants changed their size around the threshold after the reform. Moreover, I can later also further alleviate these concerns by comparing plants that are farther away from the actual employment protection threshold.

Furthermore, other legal changes that are happening at the same time and affecting firms along the employment protection threshold differentially can not be distinguished from the reform effect. However, there were no major changes to employment legislation at the same time and the other rules introduced in the same law were unrelated to the employment protection status of firms and fixed-term employment.

Lastly, 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. I graphically plot the development for all available years before and after the reform. Moreover, 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.

6 Results

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.

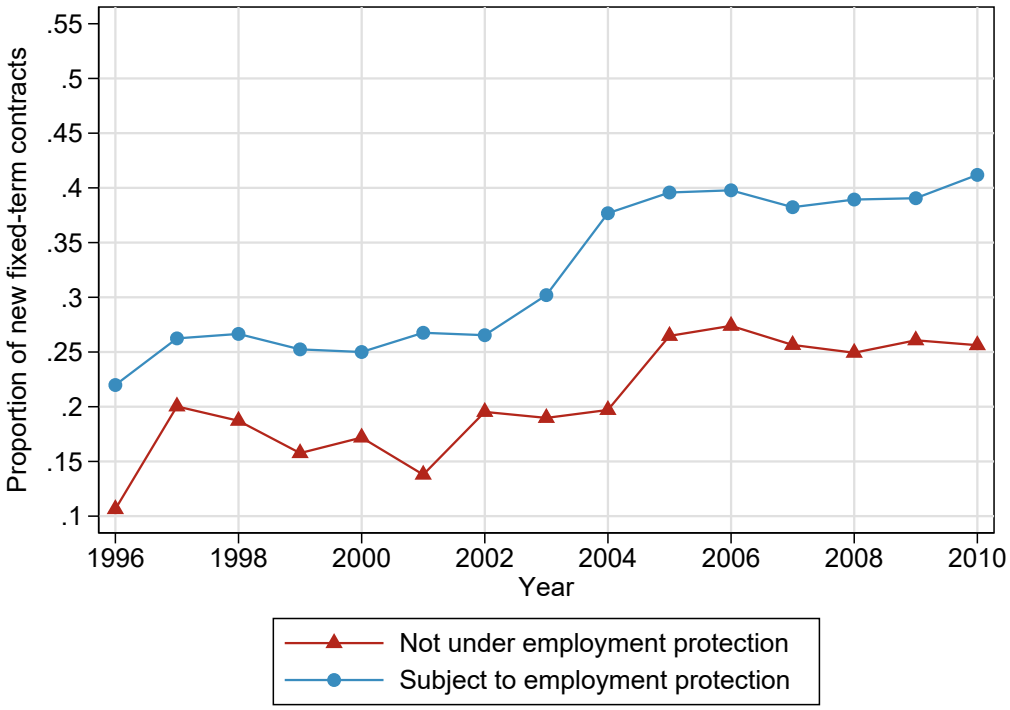
6.1 Use of fixed-term contracts

Consequently, I begin by assessing the impact of the reform on the uptake of fixed-term contracts by plants that are not subject to employment protection rules. As a first step, I plot the proportion of new contracts that are fixed term.

Figure 2 shows that up to 2001 the trends in hiring fixed-term employees moved roughly in parallel for both types of plants 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 for plants that are subject to employment protection.

Besides, this first overview is in line with the expected incentives for the use of fixed-term contracts.

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



NOTE.- The figur plots the proportion of new fixed term contracts by employment protection status.
Source: Mikrozensus sample for West German employees aged 20 to 65

Plants larger than the employment protection threshold have a stronger incentive to make use of temporary employment and consequently have a larger share of fixed-term contracts pre-reform. As the 2001 reform has made it more difficult for plants not covered by employment protection to use fixed-term contracts, the trends between the two groups diverge.

Next, I explore the effect of the reform more formally. Table 5 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 plant-size category and year fixed effects. The plant-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 plant-size category increases due to data limitations for firms above 10 Employees.[19]

To account for within plant-size dependence of hiring behavior, I cluster standard errors at the level

[19]I analyze plant-size restrictions and different levels for fixed-effects in the robustness section.

Table 5: Difference-in-Difference Equations for likelihood of being fixed-term (New contracts)

	(1)	(2)	(3)	(4)	(5)	(6)
	New contracts			Overall		
No EPL×Post 2001	-0.0404***	-0.0302***	-0.0316***	-0.00895***	-0.00510**	-0.00695**
	(0.00566)	(0.00533)	(0.00535)	(0.00157)	(0.00175)	(0.00237)
Female	-0.00534	-0.0510***	-0.0500***	0.00363	-0.0153***	-0.0155***
	(0.00648)	(0.00366)	(0.00376)	(0.00265)	(0.00203)	(0.00201)
Medium Education	-0.0656***	-0.0702***	-0.0704***	-0.0392***	-0.0394***	-0.0395***
	(0.00730)	(0.00711)	(0.00738)	(0.00203)	(0.00208)	(0.00208)
Age	-0.0151***	-0.0144***	-0.0142***	-0.0170***	-0.0167***	-0.0167***
	(0.00210)	(0.00178)	(0.00186)	(0.00151)	(0.00148)	(0.00147)
Age^2	0.000159***	0.000152***	0.000149***	0.000167***	0.000163***	0.000163***
	(2.63e-05)	(2.17e-05)	(2.28e-05)	(1.53e-05)	(1.48e-05)	(1.47e-05)
Constant	0.493***	0.595***	65.21**	0.451***	0.476***	12.04**
	(0.0413)	(0.0381)	(23.26)	(0.0318)	(0.0331)	(4.271)
Wild Bootstrap CI	[-0.054;-0.022]	[-0.041;-0.012]	[-0.043;-0.017]	[-0.012;-0.005]	[-0.011;-0.001]	[-0.013;-0.001]
Plant-size fixed effects	YES	YES	YES	YES	YES	YES
Year Fixed Effcts	YES	YES	YES	YES	YES	YES
Industry Fixed Effects	NO	YES	YES	NO	YES	YES
Industry Trends	NO	NO	YES	NO	NO	YES
Observations	45,571	45,103	45,103	519,207	513,112	513,112
R^2	0.042	0.069	0.072	0.036	0.046	0.047

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. $***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

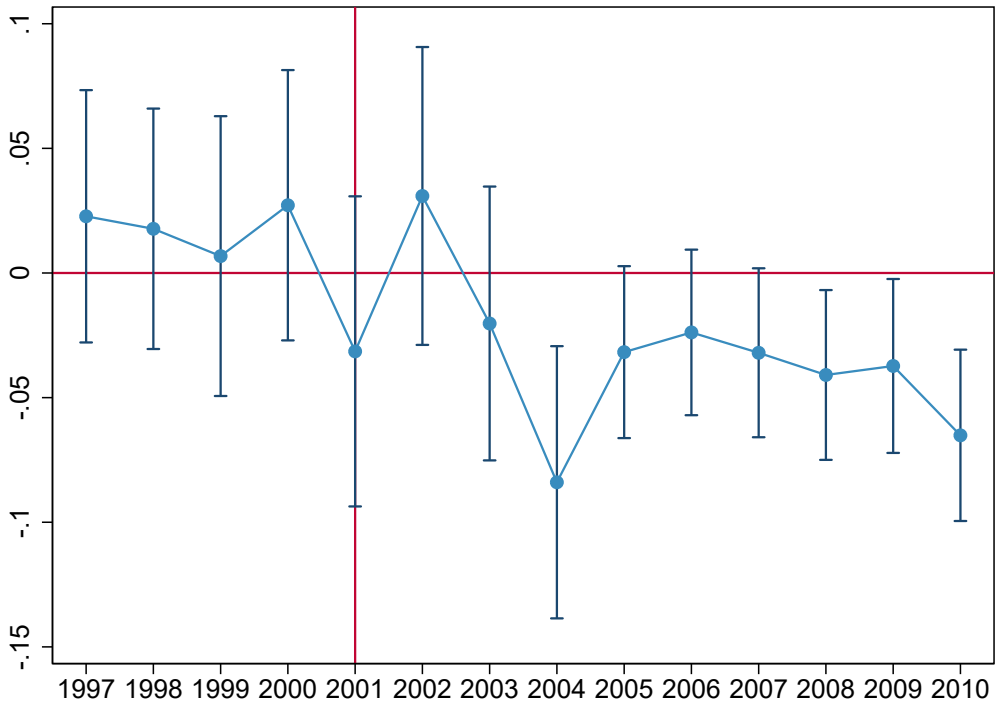
of plant-size categories. Since the number of plant-size clusters is relatively small I also report wild bootstrap confidence intervals (Cameron et al., 2008) where appropriate.

The coefficient of -0.0404 in column (1) indicates a 4.04 p.p. decrease in the fixed-term share of new contracts in firms not subject to employment protection. Adding additional industry fixed effects reduces the estimated effect to 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 35%, the effect represents an economically substantial 10 % decrease.

The overall fixed-term share of contracts in treated firms is also declining accordingly. The coefficient of -0.00695 in Column (6) indicates a 0.696 p.p. decrease in firms affected by the reform. This effect is also sizable, as the average fixed-term share of all contracts in the sample is 9.9 %.

Next, I examine whether the effect of the reform is persistent or reverses after some time. For

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 firm-size fixed effects, year-fixed effects, the respective interactions and a all control variables included in table 5.
Source: Mikrozensus sample for West German employees aged 20 to 65

this I re-estimate the same regression as in Column (1) of Table 5, but replace the single difference-in-difference indicator with interactions between each calendar year and an indicator for plants that are not subject to employment protection. The coefficients for these interactions are plotted in figure 3.

For pre-reform years, the coefficients of the interactions of employment protection status and the calendar year are close to zero and statistically insignificant. Consequently the trends in both types of firms before the reform are largely parallel.

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 a about 3 percentage points. Thus the reform is largely persistent at effect sizes similar to the simple difference-in-differences specification.

6.2 Employment

The main predictions of the theoretical model discussed in section 3 concern the destruction and creation of jobs. The basic model with equal bargaining power across both contract types, predicts both a decrease in the creation of fixed-term jobs 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 the Mikrozensus lacks the panel structure that would be necessary to analyze detailed employment flows, I use social security data, to estimate the reform effects on employment flows. I report these results in table 6. 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 about 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.

The $-0,0091$ coefficient in the (1) column indicates a 0.9 percentage point decrease in the likelihood that non-employed individuals moves into employment in plants that are not affected by employment protection legislation. 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 relatively small.

In addition column (2) also reports a 0.79 p.p. decrease in the probability to move from a job in a plant below the employment protection threshold into non-employment after the reform. Together with the results from column (1), this implies a negligible overall effect on the transition from non-employment to employment, since the effects in both columns are within a standard error range.

The results are very similar when I restrict the analysis to flows from and to unemployment. Here however, the flow from unemployment to employment exhibits a smaller decrease of only 0.37 percentage points, while the decrease in the transition from employment to unemployment is similarly sized at 0.74 percentage points. Although this suggests an overall positive effect on employment, the effect is again

Table 6: 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
No EPL×Post2001	-0.0091***	-0.0079***	-0.0037**	-0.0074***
	(0.00302)	(0.0023)	(0.00155)	(0.00162)
Female	-0.00436*	-0.00771***	-0.01102***	-0.00911***
	(0.00236)	(0.0015)	(0.00102)	(0.00096)
Medium Education	0.02646***	0.01971***	-0.01004***	0.01067***
	(0.00102)	(0.0013)	(0.00049)	(0.00049)
Age	-0.00891***	-0.0006**	-0.00393***	0.00141***
	(0.00023)	(0.00027)	(0.00015)	(0.00006)
Age^2	0.0000695***	0.0000059	0.0000539***	-0.00002***
	(0.0000028)	(0.0000035)	(0.0000016)	(0.0000007)
Constant	0.3104***	0.09301***	0.14889***	0.01401***
	(0.00494)	(0.00474)	(0.0032)	(0.00184)
Plant-size fixed effects	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES
Observations	3228829	3228829	2401018	3228829
R^2	0.02729	0.00459	0.00714	0.00683

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.
$***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

not economically significant.

In sum, the reform has only a minimal impact on employment flows. 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 likely increased.[20]

6.3 Wages

I can use an identical approach to compute the wage effects of the reform. It is not clear from the outset, what wage effect should be expected for the reform. While theory predicts negative wage effects in a baseline model with equal rent sharing in both contract types, positive wage effects of the reform are also possible if workers have less bargaining power in fixed-term contracts. In this case, a lower

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

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 employment effects of the reform are small, while the decrease of the temporary contract share is large, the basic prerequisites for this case appear to be fulfilled. A post-reform increase in wages in treated plants would therefore suggest lower bargaining power in fixed-term contracts.

Table 7: Wage effects of the reform

	(1)	(2)	(3)	(4)	(5)	(6)
		New contracts			All contracts	
	Overall	Permanent	Fixed Term	Overall	Permanent	Fixed Term
No EPL×Post 2001	0.0233***	0.0146	0.0340**	0.00896***	0.00605*	0.00888
	(0.00701)	(0.00931)	(0.0116)	(0.00234)	(0.00301)	(0.00855)
Female	0.0931***	0.116***	0.0652**	0.00376	0.0409	0.0312
	(0.0248)	(0.0326)	(0.0227)	(0.0201)	(0.0317)	(0.0184)
Medium Education	0.225***	0.215***	0.205***	0.197***	0.199***	0.145***
	(0.00491)	(0.00528)	(0.0111)	(0.00427)	(0.00483)	(0.00700)
Age	0.0341***	0.0448***	0.00193	0.0450***	0.0505***	-0.0193***
	(0.00396)	(0.00163)	(0.00520)	(0.00259)	(0.00163)	(0.00260)
Age^2	-0.000394***	-0.000516***	-2.74e-05	-0.000458***	-0.000538***	0.000260***
	(4.95e-05)	(2.12e-05)	(6.70e-05)	(3.06e-05)	(2.04e-05)	(3.43e-05)
Constant	6.394***	6.162***	6.882***	6.301***	6.160***	7.394***
	(0.0842)	(0.0409)	(0.102)	(0.0660)	(0.0513)	(0.0575)
Wild Bootstrap CI	[0.008;0.043]	[-0.007;0.041]	[0.007;0.056]	[0.004;0.014]	[-0.002;0.014]	[-0.015;0.036]
Plant-size fixed effects	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES
Observations	51,647	30,623	15,328	622,565	460,470	51,237
R^2	0.044	0.058	0.029	0.054	0.058	0.015

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.
$***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

Consequently, I analyze the impact of the reform on the wages for fixed-term and permanent employees, both for new contracts and for all employees. The results of the corresponding log wage regressions are shown in table 7. The first 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 2.33 % 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 3.4 % than for permanent employees at 1.46 %. However, the coefficient for new permanent contracts is not measured precisely and it cannot even be excluded 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, comparisons of the effect size between the contract types are not feasible.

6.4 Long-term outcomes for labor market entrants

So far I have analyzed the reform effects on the fixed-term share, employment and wages. Next, I abstract from the predictions of the theoretical framework and examine the long-term impact of the reform on labor market entrants. Questions about the long-term impact of starting a career in fixed-term work are often at the center of the policy debate on temporary work. In particular, the debate focuses on whether fixed-term contracts are a stepping stone to a permanent job or a dead-end for labor market entrants. Thus, I extend my analysis to examine how entry into the labor 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 who have entered the labour market around the reform over time. In a similar Differences-in-Differences setup as before, I compare the difference in the outcomes of post and pre-reform entrants, who entered the labor market in plants below and above the 5 employee threshold. This allows me to analyze outcomes such as the likelihood to still be employed at the same employer as in the entry year, the cumulative earnings and times out of employment for entrants in the first five years after entry and the number of jobs in the first 5 years after entry.

I report the results of the respective regressions in Table 8. 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 across the plant-size threshold 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 labor market, while column (2) reports the effect on the official duration of

Table 8: Longterm effects of the reform

	(1) weeks non-employed	(2) weeks unemployed	(3) Log cumulative earnings	(4) Likelihood Same Employeer	(5) Number of Jobs
No EPL Entry × 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	0.0618	0.0004	-0.0005	-0.0002	-0.0029
	(0.00332)	(0.00014)	(0.00002)	(0.00001)	(0.00004)
Constant	123.205	3.7773	10.3459	-0.0979	-0.8147
	(4.0647)	(0.1762)	(0.022)	(0.0091)	(0.0527)
Plant-size at Entry Fixed Effects	Yes	Yes	Yes	Yes	Yes
Entry Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	214833	214833	214804	214833	214833
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 clusters in parentheses.
$***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

unemployment over the same period. Column (3) contains results on log cumulative earnings. Moreover, the regression results for the probability to remain 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 sizable 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 plants led to an increase in job security and long-term wages for labor 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 labor market to compare entry cohorts across firm sizes for later years. However, the Mikrozensus does not contain information on the firm-size at entry and I can not track

Table 9: Likelihood of long-term fixed-term employment for labor market entrants

	(1) Overall	(2) 2001 to 2005	(3) 2005 to 2010
No EPL × Entry after 2001	-0.0312***	-0.0405***	-0.0321***
	(0.00481)	(0.00889)	(0.00380)
Female	0.0218***	0.0165***	0.0222***
	(0.00229)	(0.00428)	(0.00185)
Age	-0.00748***	-0.00634*	-0.00789***
	(0.00213)	(0.00321)	(0.00192)
Age^2	9.19e-05***	7.44e-05*	9.61e-05***
	(2.52e-05)	(3.90e-05)	(2.32e-05)
Medium Education	-0.0375***	-0.0209	-0.127***
	(0.0115)	(0.0173)	(0.0318)
Constant	0.227***	0.199***	0.338***
	(0.0489)	(0.0613)	(0.0406)
Wild Bootstrap CI	[-0.041;-0.019]	[-0.065;-0.023]	[-0.040;-0.023]
Plant-size fixed effects	YES	YES	YES
Year fixed effects	YES	YES	YES
Entry Year fixed effects	YES	YES	YES
Observations	95,423	22,930	78,520
R^2	0.033	0.040	0.037

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 size clusters in parentheses.
$***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

individuals across time. Therefore, I compare individuals across their contemporaneous employment protection status and not the employment protection status at labor market entry.

Although this has the disadvantage that plants in later years could differ from the establishment at entry with regard to the employment protection status, this still gives me 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 9.

The first column suggests that the probability to be fixed-term has decreased by 3.12 percentage points for workers who entered the labor market after 2001 and work in plants 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 plants that are affected by employment protection and those that are not in section 7.1. Second, I examine, whether deliberate changes in the plant-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 10: Placebo tests for the main specification

	(1) 1997	(2) 1998	(3) 1999	(4) 2000
No EPL × Post Placebo Year	0.0156	0.00880	0.00827	0.0176
	(0.0118)	(0.0114)	(0.0113)	(0.0203)
No EPL × Post 2001	-0.0493***	-0.0465***	-0.0473***	-0.0564***
	(0.0101)	(0.0118)	(0.0117)	(0.0180)
Female	-0.00535	-0.00536	-0.00535	-0.00535
	(0.00648)	(0.00649)	(0.00649)	(0.00648)
Age	-0.0151***	-0.0151***	-0.0151***	-0.0151***
	(0.00210)	(0.00210)	(0.00210)	(0.00209)
Age^2	0.000159***	0.000159***	0.000159***	0.000159***
	(2.62e-05)	(2.63e-05)	(2.63e-05)	(2.62e-05)
Medium Education	-0.0655***	-0.0656***	-0.0656***	-0.0656***
	(0.00731)	(0.00730)	(0.00730)	(0.00730)
Constant	0.488***	0.491***	0.492***	0.492***
	(0.0415)	(0.0413)	(0.0422)	(0.0412)
Wild Bootstrap CI	[-0.012;0.045]	[-0.033;0.033]	[-0.014;0.074]	[-0.023;0.076]
Plant-size fixed effects	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES
Observations	45,571	45,571	45,571	45,571
R^2	0.042	0.042	0.042	0.042

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 to be fixed-term, where I shift the introduction year of the reform to years prior to the reform.

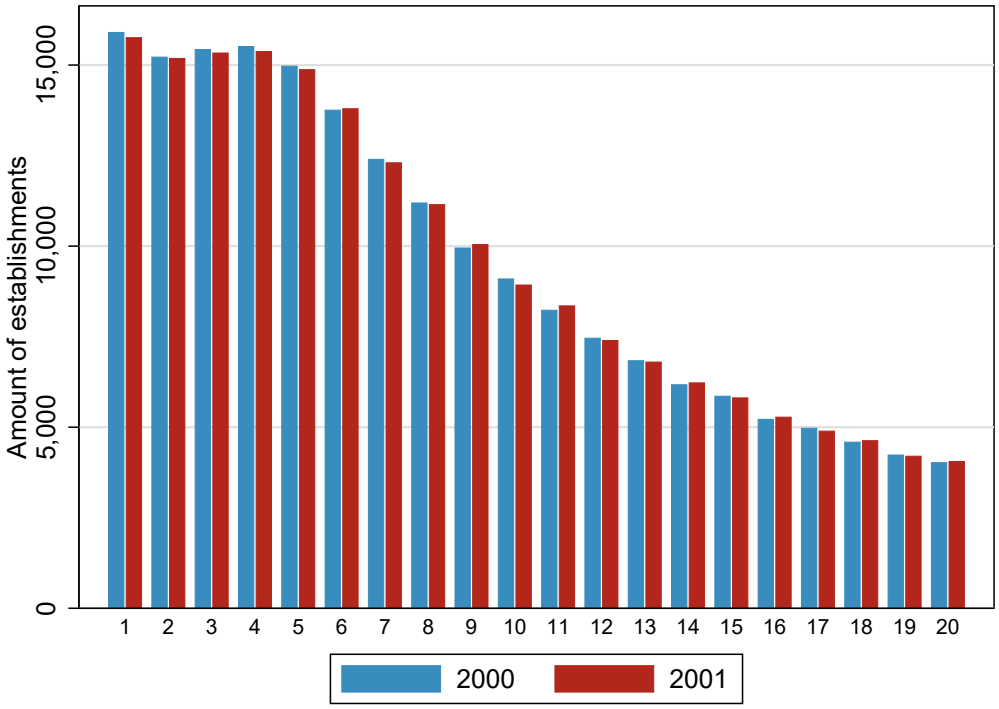
Table 10 shows the results for these regressions. All placebo reform indicators regardless of the

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 plants 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 analyze, how the plant-size distribution changed around the reform.

Figure 4: Plant-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

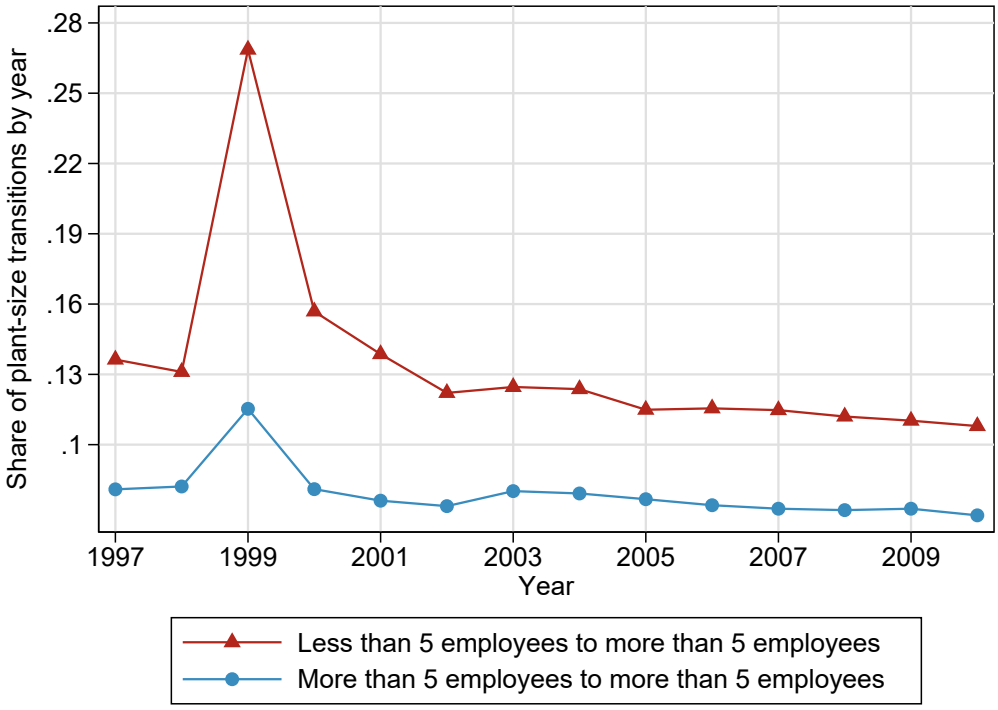
I start by plotting the plant-size distribution for the pre-reform year 2000 and the year 2001 in figure 4. For the sake of clarity, only firms up to a size of 20 employees are taken into account in this figure. That is sensible, as the relevant threshold at which manipulation was possible is 5 employees.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

The distribution is almost identical for the two years, hinting at only small changes in plant-size

for existing establishments. However, since changes are still possible that would not be evident in the overall plant-size distribution, I next use the panel structure of the data, to examine whether the rates of transition around the plant-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 less than 5 employees in the previous year and more than 5 employees in the current year. The second one is the share of plants, which had more than 5 employees in the previous year but are smaller in the current year.

Figure 5: Firm-size transitions



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

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 plant-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 plant-size-categories right at the employment protection threshold to further alleviate concerns about potential plant-size manipulation. For this I estimate the main regressions for the likelihood of being fixed-term

with two restrictions. The first restriction is that i exclude plant-sizes of 4 or 9 employees. These are the plant-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 plants right at the threshold. The estimates for these regressions are in table 11.

Table 11: 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
No EPL × Post 2001	-0.0404***	-0.0405***	-0.0516***	0.0227***	0.0242***	0.00503
	(0.00566)	(0.00610)	(0.0143)	(0.00579)	(0.00642)	(0.0216)
Age	-0.0151***	-0.0155***	-0.0172***	0.0367***	0.0368***	0.0404***
	(0.00210)	(0.00233)	(0.00269)	(0.00300)	(0.00332)	(0.00463)
Age^2	0.000159***	0.000165***	0.000187***	-0.000428***	-0.000430***	-0.000449***
	(2.63e-05)	(2.91e-05)	(3.04e-05)	(3.70e-05)	(4.09e-05)	(5.83e-05)
Female	-0.00534	-0.00558	0.00273	0.111***	0.105***	0.0648**
	(0.00648)	(0.00695)	(0.00706)	(0.0302)	(0.0310)	(0.0279)
Medium Education	-0.0656***	-0.0658***	-0.0572***	0.228***	0.226***	0.176***
	(0.00730)	(0.00780)	(0.0137)	(0.00432)	(0.00481)	(0.0178)
Constant	0.493***	0.494***	0.565***	6.306***	6.301***	6.261***
	(0.0413)	(0.0467)	(0.0602)	(0.0757)	(0.0811)	(0.0990)
Wild Bootstrap CI	[-0.055;-0.023]	[-0.056;-0.025]	[-0.116;-0.019]	[0.010;0.036]	[0.012;0.042]	[-0.062;0.074]
Plant-size fixed effects	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES
Observations	45,571	41,670	11,903	43,398	39,675	11,494
R^2	0.042	0.043	0.030	0.053	0.051	0.050

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.
$***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

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.

7.3 Additional Specification Checks

Finally, I also examine how the restriction of the sample to certain maximum plant-sizes and the specification of the plant fixed effects affect my findings. For this purpose, I explore two modifications to the main specification. For one, I limit the maximum plant-size in the sample to 20 or 10 employees.[21] And secondly, I calculate all estimates with more detailed fixed effects for plant-size categories.

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

Table 12: Robustness: Plant-size restrictions (Fixed-term share)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Standard Sample			Under 20 Employees			Under 10 Employees		
No EPL × Post 2001	-0.0404***	-0.0397***	-0.0300***	-0.0363***	-0.0357***	-0.0255**	-0.0350*	-0.0345*	-0.0406**
	(0.00566)	(0.00545)	(0.00536)	(0.00879)	(0.00863)	(0.00821)	(0.0168)	(0.0169)	(0.0162)
Female	-0.00534	-0.00455	-0.0509***	-0.0114	-0.0104	-0.0542***	-0.0128	-0.0129	-0.0413***
	(0.00648)	(0.00613)	(0.00370)	(0.00678)	(0.00632)	(0.00319)	(0.00952)	(0.00963)	(0.00564)
Age	-0.0151***	-0.0152***	-0.0144***	-0.0138***	-0.0138***	-0.0131***	-0.0203***	-0.0202***	-0.0203***
	(0.00210)	(0.00210)	(0.00178)	(0.00206)	(0.00205)	(0.00158)	(0.00364)	(0.00363)	(0.00338)
Age^2	0.000159***	0.000160***	0.000152***	0.000142***	0.000143***	0.000137***	0.000221***	0.000221***	0.000226***
	(2.63e-05)	(2.63e-05)	(2.18e-05)	(2.56e-05)	(2.56e-05)	(1.96e-05)	(4.17e-05)	(4.16e-05)	(3.81e-05)
Medium Education	-0.0656***	-0.0663***	-0.0707***	-0.0593***	-0.0602***	-0.0640***	-0.0398**	-0.0405**	-0.0406**
	(0.00730)	(0.00700)	(0.00696)	(0.00784)	(0.00746)	(0.00716)	(0.0139)	(0.0139)	(0.0135)
Constant	0.493***	0.457***	0.566***	0.464***	0.428***	0.538***	0.632***	0.627***	0.747***
	(0.0413)	(0.0416)	(0.0412)	(0.0377)	(0.0393)	(0.0383)	(0.0852)	(0.0849)	(0.116)
Wild Bootstrap CI	[-0.055;-0.020]	[-0.054;-0.021]	[-0.044;-0.013]	[-0.060;-0.005]	[-0.054;-0.007]	[-0.046;0.004]	[-0.076;0.005]	[-0.072;0.004]	[-0.075;-0.001]
Plant-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	45,571	45,571	45,103	33,593	33,593	33,255	6,132	6,132	5,996
R^2	0.042	0.043	0.070	0.032	0.033	0.061	0.019	0.020	0.044

Note.- This table contains difference-in-differences regressions for the likelihood of a new contract being 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. $*** p < 0.01$, $** p < 0.05$, $* p < 0.1$

The results for these specification checks for the likelihood that a new contract is fixed-term are in table 12. 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.

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.

[21] When I restrict the maximum plant-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 plants above the employment protection threshold for years where the threshold was at 10 employees.

In general, though, the specification checks suggest that my results are robust against changes in plant-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 for small firms affected employment, wages and the take-up of fixed-term contracts.

I find a sizable decrease in the utilization of fixed-term contracts for affected plants, yet only negligibly small effects on employment. In line with the theoretical predictions for a scenario in which workers have less bargaining power in fixed-term contracts and the proportion of new fixed-term contracts decreases, I also find positive reform effects on wages.

Furthermore, I also find some evidence that the reform has contributed to a reduction in labor market segmentation for new entrants. For labor market entrants, who joined affected firms after the reform year, I find an increase in cumulated 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 be beneficial to 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 labor costs by replacing labor with capital. Thus, longer-term adjustments are an interesting point for future research.

Lastly, my findings for a German restriction of fixed-term contracts from 2001 offer further insights for the current plans of the German government to limit fixed-term employment. Recently, the ruling coalition parties have agreed to reduce the maximum permissible duration of fixed-term contracts, to limit the number of renewals and to set a new quota for the maximum proportion of fixed-term employees for larger firms (Coalition Agreement, 2018). The debate that preceded these plans often focused on possible negative employment effects of a significant restriction on firms' use of temporary jobs.

Although the current German plans to limit the use of fixed-term contracts are more extensive than the previous reform, the results of this article at least suggest that fears about detrimental employment

effects seem less warranted. However, whether the more broad new reforms will have similar outcomes will ultimately be a question for further research.

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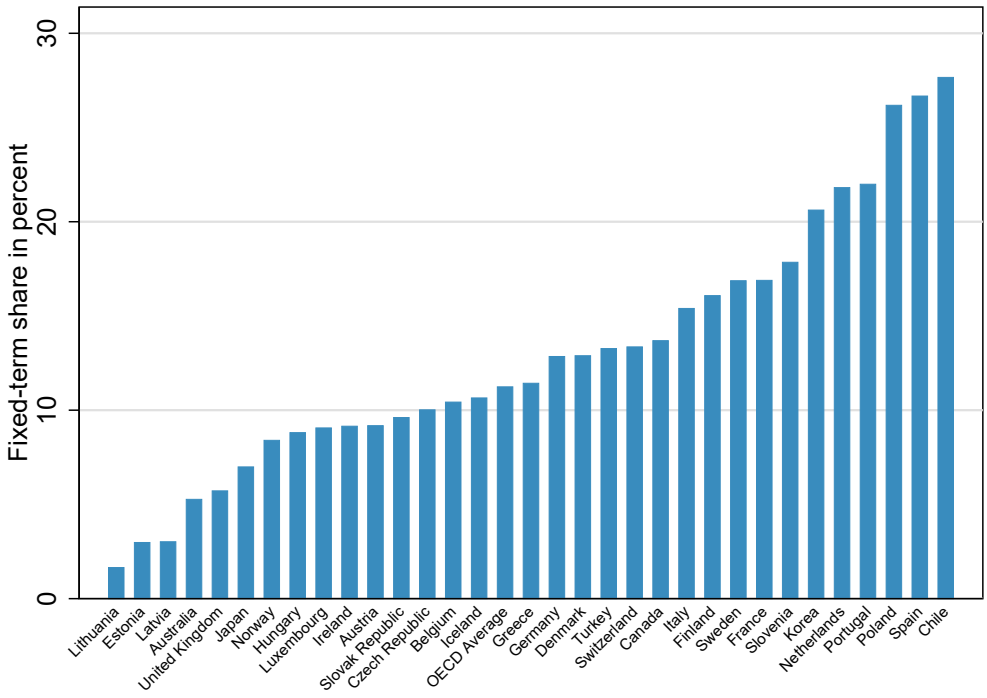
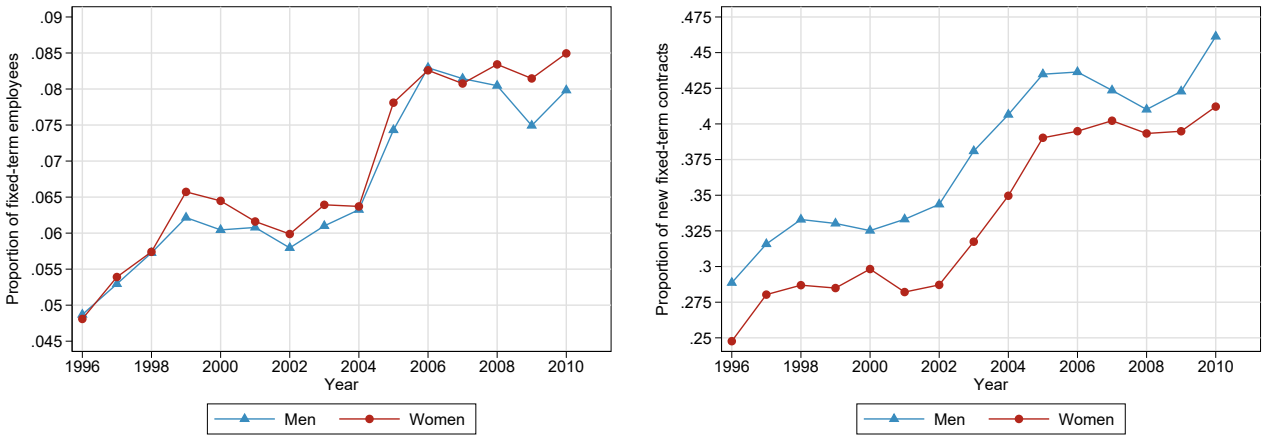
A Additional tables and figures

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)

Figure A2: Proportion of fixed-term contracts over time



NOTE.- The figures plots the proportion of fixed term contracts for all employees (left panel) and for all new contracts seperately for men and women
Source: Mikrozensus sample for West German employees aged 20 to 65

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Standard Sample			Under 20 Employees			Under 10 Employees		
No EPL × Post 2001	0.0227***	0.0227***	0.0150**	0.0257***	0.0256***	0.0173**	0.0302**	0.0292**	0.0223*
	(0.00579)	(0.00581)	(0.00582)	(0.00716)	(0.00715)	(0.00739)	(0.0102)	(0.00999)	(0.0110)
Female	0.111***	0.111***	0.160***	0.140***	0.141***	0.189***	0.130***	0.131***	0.151***
	(0.0302)	(0.0305)	(0.0292)	(0.0324)	(0.0328)	(0.0307)	(0.0239)	(0.0242)	(0.0285)
Age	0.0367***	0.0367***	0.0342***	0.0398***	0.0397***	0.0374***	0.0477***	0.0473***	0.0459***
	(0.00300)	(0.00297)	(0.00313)	(0.00259)	(0.00255)	(0.00281)	(0.00441)	(0.00429)	(0.00412)
Age^2	-0.000428***	-0.000427***	-0.000400***	-0.000465***	-0.000463***	-0.000438***	-0.000538***	-0.000532***	-0.000521***
	(3.70e-05)	(3.63e-05)	(3.84e-05)	(3.28e-05)	(3.18e-05)	(3.55e-05)	(5.52e-05)	(5.33e-05)	(5.17e-05)
Medium Education	0.228***	0.227***	0.203***	0.229***	0.228***	0.196***	0.160***	0.159***	0.120***
	(0.00432)	(0.00447)	(0.00727)	(0.00527)	(0.00565)	(0.00988)	(0.0253)	(0.0257)	(0.0248)
Constant	6.306***	6.278***	6.212***	6.226***	6.193***	6.116***	6.116***	6.091***	6.033***
	(0.0757)	(0.0791)	(0.0972)	(0.0640)	(0.0660)	(0.0771)	(0.0860)	(0.0854)	(0.0878)
Wild Bootstrap CI	[0.011;0.038]	[0.010;0.038]	[0.003;0.030]	[0.011;0.052]	[0.013;0.053]	[0.002;0.045]	[0.007;0.057]	[0.005;0.057]	[-0.004;0.050]
Plant-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	43,398	43,398	42,937	31,996	31,996	31,661	5,953	5,953	5,818
R^2	0.053	0.053	0.078	0.060	0.060	0.089	0.059	0.060	0.096

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 workers outside option. For a permanent contract the expected profit of the firm is given by

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

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

$$V_P(\lambda) = \int_0^\infty \left[\int_0^\tau w(\lambda) e^{-rt} dt + U e^{-r\tau} \right] \lambda e^{-\lambda\tau} d\tau = \frac{w(\lambda) + \lambda U}{r + \lambda}, \tag{9}$$

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

$$S_P(\lambda) = \frac{y - rU - \lambda f}{r + \lambda} \tag{10}$$

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}.

$$\Pi_{FT}(\lambda, D) = \int_0^D [y e^{-\lambda\tau} - w(\lambda, D)] e^{-r\tau} d\tau + \max\left(\Pi_P(\lambda), 0\right) \cdot e^{-\lambda D} \cdot e^{-rD} - c_{FT} \tag{11}$$

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 workers valuation of of a new fixed-term contract is given by

$$V_T(\lambda, D) = \int_0^D w(\lambda, D)e^{-r\tau}d\tau + \max\left(V_P(\lambda), U\right) \cdot e^{-\lambda D} \cdot e^{-rD} + U(1 - e^{-\lambda D})e^{-rD} \tag{12}$$

Lastly, just as for an open-ended contract, the expected surplus for a fixed-term contract is defined as the sum of the firms expected profit and the workers' valuation less her external option.

$$S_{FT}(\lambda, D) = \int_0^D \left[ye^{-\lambda\tau} - rU\right] e^{-r\tau}d\tau + \max\left(S_P(\lambda), 0\right) \cdot e^{-(r+\lambda)D} - c_{FT} \tag{13}$$

In 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 13 with regard to duration D:

$$\frac{\partial S_T(\lambda, D)}{\partial D} = ye^{-\lambda D} - rU - (r + \lambda)e^{-\lambda D}\max\left(S_P(\lambda), 0\right) = 0 \tag{14}$$

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

In 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 14 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

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

Second, 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 $S_P(\lambda_P) = 0$.

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

Third, 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 labor market tightness theta and is given by equation 2. By substituting this in 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.

$$\frac{\partial \lambda_{FT}}{\partial c_{FT}} = \underbrace{-\frac{\frac{\partial h^{FTJCR}}{\partial c_{FT}}}{\frac{\partial h^{FTJCR}}{\partial \lambda_{FT}}}}_{\text{Direct Effect}} + \underbrace{\frac{\frac{\partial h^{FTJCR}}{\partial \theta}}{\frac{\partial h^{FTJCR}}{\partial \lambda_{FT}}} \times \frac{\frac{\partial h^{EC}}{\partial c_{FT}}}{\frac{\partial h^{EC}}{\partial \theta}}}_{\text{Equilibrium Feedback Effect}} \tag{15}$$

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 $\frac{\partial S_{FT}(\lambda)}{\partial \theta}$.[22]

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

[22]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 $\frac{\partial S_{FT}(\lambda)}{\partial \theta} < 0$

of the contract writing costs on job creation is negative.

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

$$\frac{\partial h^{FTJDR}}{\partial \lambda_P} d\lambda_P + \frac{\partial h^{FTJDR}}{\partial \theta} d\theta = 0 \tag{16}$$

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:

$$\frac{\partial \lambda_P}{\partial c_{FT}} = \frac{r}{c_P + f} \frac{\partial U(\theta)}{\partial \theta} \frac{\partial \theta}{\partial c_{FT}} > 0 \tag{17}$$

As fewer temporary jobs are converted into open-ended contracts and are terminated instead when λ_P rises, job destruction also increases.

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:

$$\frac{\partial \lambda_E}{\partial c_{FT}} = \underbrace{- \frac{\frac{\partial h^{PvsFT}}{\partial c_{FT}}}{\frac{\partial h^{PvsFT}}{\partial \lambda_E}}}_{\text{Direct Effect}} \underbrace{- \frac{\frac{\partial h^{PvsFT}}{\partial \theta}}{\frac{\partial h^{PvsFT}}{\partial \lambda_E}} \times \frac{\partial \theta}{\partial c_{FT}}}_{\text{Feedback Effect}} \tag{18}$$

The derivatives used in equation 18 are

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

There are again a direct effect and 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

$$\lambda_T < \frac{y - r c_{FT}}{f + c_{FT}} \tag{19}$$

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:

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

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

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

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

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

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

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 labor market tightness. For a given level of λ_E the effect of an increase in c_{FT} on θ is negative as

$$\frac{\partial \theta}{\partial c_{FT}} \mid_{\lambda_E \text{ is constant}} = -\frac{\frac{\partial h^{EC2}}{\partial c_{FT}}}{\frac{\partial h^{EC2}}{\partial \theta}} < 0 \qquad (20)$$

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

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

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

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.

$$\frac{\partial \lambda_E}{\partial c_{FT}} \mid_{\theta \text{ is constant}} = -\frac{\frac{\partial h^{PvsFT2}}{\partial c_{FT}}}{\frac{\partial h^{PvsFT2}}{\partial \lambda_E}} > 0 \tag{21}$$

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 labor market tightness is not taken into account. Together with the above result on the effect of changes on c_{FT} on labor market tightness this shows that that reactions to the reform on λ_E and θ counteract each other. However, labor market tightness still overall decreases, while the effects on the valuation of unemployment and λ_E are not clear from the outset.[23]

Next, I use these results to analyze 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.

$$\frac{\partial \lambda_{FT}}{\partial c_{FT}} = -\frac{\frac{\partial h^{FTJCR2}}{\partial c_{FT}}}{\frac{\partial h^{FTJCR2}}{\partial \lambda_{FT}}} - \frac{\frac{\partial h^{FTJCR2}}{\partial \theta}}{\frac{\partial h^{FTJCR2}}{\partial \lambda_{FT}}}\frac{\partial \theta}{\partial c_{FT}} - \frac{\frac{\partial h^{FTJCR2}}{\partial \lambda_E}}{\frac{\partial h^{FTJCR2}}{\partial \lambda_{FT}}}\frac{\partial \lambda_E}{\partial c_{FT}} \tag{22}$$

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

with

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

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 labor 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 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

$$\frac{\partial \lambda_P}{\partial c_{FT}} = \frac{r}{c_P+f}\left[\frac{\partial U(\theta,\lambda_E)}{\partial \theta}\frac{\partial \theta}{\partial c_{FT}} + \frac{\partial U(\theta,\lambda_E)}{\partial \lambda_E}\frac{\partial \theta}{\partial \lambda_E}\right] \lessgtr 0 \tag{23}$$

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:

$$\frac{\partial w_P}{\partial c_{FT}} = (1-\gamma)r\left[\frac{\partial U(\theta,\lambda_E)}{\partial \theta}\frac{\partial \theta}{\partial c_{FT}} + \frac{\partial U(\theta,\lambda_E)}{\partial \lambda_E}\frac{\partial \theta}{\partial \lambda_E}\right] \lesseqgtr 0$$

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

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