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in Europe:  
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# Chapter 1

## Introduction

### 1.1 Motivation

In the late 1980s and early 1990s, Southern and Continental European labor markets were characterized by worse labor market conditions, e.g. increasing unemployment and failure to absorb disadvantaged groups, in comparison to Anglo-Saxon labor markets. In the public debate, high employment protection legislation (EPL) was considered to be one major factor for this adverse development (OECD, 1994). On the one hand, EPL can improve market efficiency in the case of imperfect capital markets by providing insurance against income losses (Pissarides, 2001). On the other hand, given risk-averse workers or inflexible wages, EPL decreases employment according to Lazaer (1990).

In general, EPL is a labor market institution which formally regulates the dismissal and hiring of employees,<sup>1</sup> and has two major dimensions: employment protection for temporary contracts (EPLT)<sup>2</sup> and employment protection for permanent contracts (EPLP)<sup>3</sup>. After an agreed upon period, temporary contracts are terminated without any (or with low) costs for employers. In contrast, permanent

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<sup>1</sup>For an overview of the extensive literature on theoretical and empirical effects of EPL as well as on measuring EPL, see Cahuc and Koeniger (2007), Venn (2009), Cahuc and Zylberberg (2004), Boeri (2011), OECD (2013b), OECD (2004), OECD (1999) or Boeri and van Ours (2013).

<sup>2</sup>I refer to temporary workers as fixed-term workers, and temporary agency workers. The former are employees with a working contract between the worker and the firm. The latter are employees with a working contract between the worker and an agency rather than the firm.

<sup>3</sup>The terms *regular*, *permanent* and *open-end* are used equivalently.

contracts can only be terminated from the employer by paying firing costs. These costs consist of firing taxes (i.e. payments from the employer to a third party such as the court fees), and transfers (i.e. payments from the employer to the employee such as severance payments). Overall, employment protection for regular workers aims to protect workers from unfair dismissals.

Liberalizing EPLP, in order to improve labor market outcomes, however, was perceived to be politically harmful as the majority of voters had a regular contract and would be adversely affected (Bentolila, Cahuc, Dolado and Le Barbanchon, 2012; Rueda, 2005; Saint-Paul, 2002; Saint-Paul, 1996b). Therefore, in the 1980s and 1990s, labor market reforms were characterized by decreasing protection for temporary workers (the small labor market segment) as well as by continued protection for regular workers (the large labor market segment). Saint-Paul (2002) shows that such reforms were a promising mechanism to win the political support of regular workers for liberalizing EPL reforms. Boeri (2011) labels these reforms as *two-tier* reforms in employment protection legislation.

Hence, two-tier labor reforms made it more attractive to employ temporary workers as adjustment costs for regular workers remained in place while it became less costly to adjust by temporary workers (e.g. Boeri and Garibaldi, 2007; Blanchard and Landier, 2002). According to Boeri and Garibaldi (2007), two-tier reforms increase the share of temporary employment over time. Indeed, over the last decades, the share of temporary workers rose from 11.5 percent in the EU15 in 1995 to the considerable share of 14.9 percent in 2007 (Eurostat, 2014).<sup>4</sup> Hence, European labor markets became more segmented<sup>5</sup> (e.g. Boeri and van Ours, 2013; European Commission, 2010; OECD, 2013b; OECD, 2014). In the aftermath of the 2007 financial crisis, temporary employment decreased to 13.8 percent in 2013 (Eurostat, 2014) as firms decreased the number of workers through the more preferable and lower cost route of terminating temporary contracts.

On the one hand, temporary employment provides important advantages for firms in countries with high EPLP, e.g. cheap buffering function for firms in re-

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<sup>4</sup>Technological and organizational change are also important factors for the increasing share of temporary workers (OECD, 2014).

<sup>5</sup>*Segmented labor markets* are characterized by strong differences in the job quality in labor market segments, e.g. job security, with low transition between segments (Boeri and van Ours, 2013; OECD, 2014).

sponse to product demand shocks or screening at lower costs in the presence of imperfect information. From the perspective of workers, temporary employment has advantages, too. Temporary contracts provide easier access into employment for people who are less attached to labor markets, e.g. long-term unemployed or young workers. Furthermore, workers might choose temporary contracts voluntarily if they prefer an employment relationship with less commitment. In the EU27 in 2013, only 11.8 percent of temporary workers, however, did not want a permanent job (Eurostat, 2014).

On the other hand, a strong use of temporary employment is associated with adverse equity effects in terms of job quality and efficiency effects in terms of economic growth. Job quality in the segment of temporary contracts is worse than that of regular contracts, especially with regard to job security, wages, or training (OECD, 2014; European Commission, 2010; Booth, Francesconi and Frank, 2002). This is often accompanied by low transition rates into permanent contracts. Thus, disadvantages seem to outweigh advantages: In the EU27, 61.8 percent of temporary workers were in a temporary job because they could not find a permanent one in 2013 (Eurostat, 2014). From the perspective of economic growth, there is an ongoing debate whether high shares of temporary work adversely affect productivity growth, for instance, because firms usually invest less in training for non-regular jobs (European Commission, 2010). Boeri and Garibaldi (2007) and Cahuc and Postel-Vinay (2002) found that, indeed, two-tier labor market reforms led to a decrease in productivity growth. Eslava, Haltiwanger, Kugler and Kugler (2014), however, show an increase in productivity in response to Colombian reforms.

In order to decrease the adverse effects of a strong reliance on temporary employment, liberalizing reforms in EPLP were often proposed in the public debate. In fact, in the aftermath of the 2007 financial crisis, policy makers specifically in Southern European countries started to react to this critique. For instance, in Portugal, Spain, Italy, and Greece, EPLP was liberalized between 2008 and 2013 (OECD, 2013b, p. 94). Therefore, it is highly relevant to understand the effects of a decrease in EPLP on economic outcomes.

This dissertation contributes to the quantitative literature on the effects of EPLP as well as the conditioning effects of EPLP on temporary employment and

well-being, and can be divided in two parts<sup>6</sup>: In the first part, the effect of EPLP and the conditioning role of EPLP for the effect of product demand shocks on the use of temporary employment at the establishment-level is investigated. This part consists of Chapter 2 *Employment protection reform effects on temporary employment* and Chapter 3 *The effect of shocks on temporary employment conditional on EPLP*. After finding that reforms increasing (decreasing) EPLP have a positive (negative) effect on temporary employment and that EPLP strengthens the positive effect of shocks on temporary employment, I analyze the effect of EPLP reforms on well-being, proxied by life satisfaction at the individual-level, in Chapter 4 *Employment protection reform effects on well-being*.

Hypotheses are built upon the literature in labor economics and on the literature on determinants of well-being proxied by life satisfaction. I refer to the literature on search and matching models and labor demand models which are the workhorses for modeling EPLP.<sup>7</sup> Several models predict a negative impact of EPL on labor market flows, e.g. hiring or firing, but the effects on levels, e.g. unemployment rate, is ambiguous. Within the literature on determinants of well-being, I mainly refer to the concept of relative social positions as an important determinant of well-being.<sup>8</sup>

All research questions are answered empirically. I employ econometric methods and utilize micro data in order to identify effects and conditioning effects of EPLP rather than *exploring* or *describing* the social phenomena (e.g. Angrist and Krueger, 1999; Diekmann, 2003; Heckman, 2010). Concretely, I explore EPLP reforms in a difference-in-difference approach (Chapter 2 and Chapter 4) and regression analyzes (Chapter 3). In particular, the latter method is very demanding in its assumptions for the identification of effects rather than correlations, which I account for by discussing these assumptions in detail.

The remainder of the introduction is organized as follows. In the next section, I present the research questions and contributions. This is followed by a description

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<sup>6</sup>I would like to acknowledge here my coauthors Paul Marx (Chapter 3), Steffen Künn (Chapter 2) and Ulf Rinne (Chapter 2).

<sup>7</sup>For search and matching models, see for instance: Mortensen and Pissarides (1994), Cahuc and Postel-Vinay (2002), Cahuc, Charlot and Malherbert (2012), Bentolila, Cahuc, Dolado and Le Barbanchon (2012). For labor demand models, see for instance: Bentolila and Bertola (1990), Bentolila and Saint-Paul (1992), Nunziata and Staffolani (2007), or Boeri and Garibaldi (2007).

<sup>8</sup>See Easterlin (1974), Luttmer (2005), Clark and Senik (2010), and Karacuka and Zaman (2012).

of the empirical strategy in Section 1.3. Finally, in Section 1.4, I summarize the main findings and discuss their external validity.

## 1.2 Research questions and contributions

### 1.2.1 The effect and conditioning effect of EPLP on temporary employment

In the public debate, high shares of temporary work are considered to have adverse effects on equity and efficiency. Thus, one popular proposed solution suggests that reducing EPLP would lead to a reduction in the share of temporary workers. Chapter 2 and Chapter 3 contribute to a better understanding of the effect and the conditioning effect of EPLP on temporary employment.

## Chapter 2<sup>9</sup>

This chapter focusses on the demand for temporary workers at the establishment-level in Germany when EPLP is increased and decreased. The empirical literature which is based on within-country subgroup (and time) variation has shown that EPLP is positively related to temporary work (Boockmann and Hagen, 2001; Centeno and Novo, 2012; Hijzen, Mondauto and Scarpetta, 2013). Studies which employ within-country time variation but not subgroup variation can, however, not consistently confirm an impact of EPLP on temporary employment (Kahn, 2010; Nunziata and Staffolani, 2007). An asymmetric effect of increases and decreases of EPLP on temporary employment might contribute to a deeper understanding of the aforementioned findings.

Building upon this literature and to the best of my knowledge, my contribution is to answer the novel research question: Does a symmetric increase or decrease in EPLP have a symmetric effect on the share of temporary workers at the establishment-level? Germany provides a unique opportunity to investigate this in a quasi-experimental approach based on two reforms in EPLP in 1999 and

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<sup>9</sup>This chapter is circulated as "The asymmetric effects of employment protection reforms on temporary employment" (joint with Steffen Künn and Ulf Rinne).

2004. In line with Boeri (2011) and Cahuc et al. (2012), I expect that the increase in EPLP in 1999 increased the share of temporary jobs at the establishment-level, because temporary workers became relatively more attractive for firms. When EPLP increases, I expect that firms hire more on permanent workers as these contracts might be associated with advantages, e.g. higher job filling rates.

### Chapter 3<sup>10</sup>

This chapter contributes to the research on determinants of temporary employment in Europe by studying firms' demand for temporary workers in response to product demand shocks in different institutional settings regarding EPLP. Next to employment protection and other factors, firm-level shocks are one major reason for employing temporary workers (Eslava et al., 2014; Morikawa, 2010; Houseman, 2001). The 2007 financial crisis might have increased shocks at the firm-level (Buch, Döpke and Stahn, 2008). The importance of volatilities for employment, however, depend on adjustment costs. Eslava et al. (2014) and Bentolila and Saint-Paul (1992) showed this in single-country, firm-level studies, and to a minor extend, Nunziata and Staffolani (2007) in a country-level study.

Based on this literature, I ask: Is the effect of shocks on firms' decision to employ temporary workers stronger in countries that impose strict rules on the dismissal of permanent workers? In line with a recently developed search and matching model by Cahuc et al. (2012), I expect that it is more likely that firms employ temporary workers when the demand shocks are of short duration. The effect, however, is expected to depend on sufficiently high EPLP.

Employing European establishment-level data for 2009 and 2004/2005 and to the best of my knowledge, the contribution of this chapter is threefold. First, I add a broad cross-country perspective to the single-country firm-level study of Eslava et al. (2014), thus, contributing to a much broader generalization of previously found relations. Second, in comparison to previous research (Eslava et al., 2014; Nunziata and Staffolani, 2007; Bentolila and Saint-Paul, 1992), I

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<sup>10</sup>This chapter is based on a revised and resubmitted version to the Industrial and Labor Relations Review (ILRRReview) of "Do firms demand temporary workers when they face workload fluctuation? Cross-country firm-level evidence" (joint with Paul Marx). An earlier version circulates as Dräger and Marx (2012).

am the first investigating the relevance of different durations in product demand shocks for temporary employment, which is theoretically relevant for temporary employment (Cahuc et al., 2012). Third, I add the firm-level perspective in comparison to Nunziata and Staffolani (2007), and thereby, accounting for firm-level compositional effects.

## 1.2.2 The effect of EPLP on well-being

### Chapter 4<sup>11</sup>

A broad literature strand investigates the effect of EPLP on objective labor market outcomes in cross-country studies (e.g. Nunziata and Staffolani, 2007; Kahn, 2010), and more recently, in studies employing evaluation techniques (e.g. Bauer, Bender and Bonin, 2007; Kugler and Pica, 2008; Martins, 2009; Centeno and Novo, 2012; Leonardi and Pica, 2013). Furthermore, a fast growing literature strand is on determinants of well-being (e.g. Benjamin, Heffetz, Kimball and Rees-Jones, 2014; Frey and Stutzer, 2012; Clark and Senik, 2010; Kassenboehmer and Haisken-DeNew, 2009; Clark, Diener, Georgellis and Lucas, 2008; Frey and Stutzer, 2002; Easterlin, 1974) and its importance as an ingredient for measuring social progresses (e.g. OECD, 2013c; OECD, 2013a; OECD, 2011; Oswald, 2010). Surprisingly, only a few papers investigate the effect of employment protection on well-being proxied by life satisfaction, job satisfaction or perceived job security (Lepage-Saucier and Wasmer, 2012; Boarini, Comola, Keulenaer, Manchin and Smith, 2013; Ochsens and Welsch, 2012; Kuroki, 2012; Clark and Postel-Vinay, 2009; Salvatori, 2010).

Building upon this research, I ask: Does employment protection for regular workers affect well-being of workers? Based on the literature in labor economics and on the literature on determinants of well-being, I derive a set of hypotheses how EPLP affects life satisfaction (e.g. Falk and Knell, 2004; Kugler and Pica, 2008; Clark and Senik, 2010; Boeri and van Ours, 2013). Overall, the effects are ambiguous.

Thereby and to the extent of my knowledge, I contribute to the literature in two

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<sup>11</sup>This chapter is circulated as "Does employment protection legislation affect well-being? A quasi experiment".

ways. First, in comparison to Busk, Jahn and Singer (2015), Kuroki (2012) and Lepage-Saucier and Wasmer (2012) who employ evaluation techniques, I add life satisfaction as a new outcome. Second, in comparison to Boarini et al. (2013) and Ochsen and Welsch (2012) who conduct cross-country studies for which omitted variable bias is not easily ruled out, I add evaluation tools and include the distinction between employment protection legislation for permanent and for temporary workers.

### 1.3 Empirical strategy

This section presents the corresponding empirical strategies to investigate above research questions. The aim is to provide the intuition guiding the crucial assumptions in the empirical approaches rather than to provide an in-depth econometric discussion. For this purpose, I start by locating the empirical strategies within the range of social science methods and continue by introducing the concrete empirical strategies which are applied.

In each chapter of the dissertation, I focus on the effect (or conditioning effect) of EPLP on labor market outcomes rather than exploring or describing social phenomena. The well-known fundamental evaluation problem is that I do not observe the dependent variable for the same unit with and without EPLP or labor demand shocks. In order to gain insights in counterfactual worlds, different techniques can be applied. According to Angrist and Krueger (1999), Diekmann (2003), and Heckman (2010) and with reference to the EPLP literature, I broadly categorize them into *calibration*, *structural*, and *experimental/quasi experimental* approaches. These approaches differ, among others, with regard to the identification of the causal effect. In the calibration approach, mathematical economic models are calibrated to a benchmark of economic data. This is done by choosing some model parameters from the literature, e.g. elasticities, while calibrating other model parameters in such a way that they replicate a benchmark of economic data. Changes in the model parameter, e.g. firing costs, can then be used in order to derive key indicators of an economy with hypothetical reforms in EPLP. Therefore, EPLP reforms can be evaluated ex-ante. The workhorse models with respect to EPLP are search and matching models (e.g. Cahuc and Postel-Vinay, 2002; Blan-

chard and Landier, 2002; Cahuc et al., 2012; Bertola, Dabusinskas, Hoerberichts, Izquierdo, Kwapil, Montornès and Radowski, 2012) and labor demand models (e.g. Bentolila and Bertola, 1990).

In the structural approach, researchers more directly try to transmit economic theory into an empirical model through full parametrization of empirical models. The aim is then to estimate the behavioral parameters of an economic model. Based on these parameters, they simulate the effects of hypothetical reforms. Reforms can be evaluated ex-ante again. EPLP reforms are investigated in dynamic structural models of labor demand (e.g. Hamermesh, 1996; Hamermesh and Pfann, 1996; Cooper and Willis., 2004). For instance, Aguirregabiria and Alonso-Borrego (2014) analyzing a two-tier labor market reform in Spain. Based on a structural labor demand, they estimate hiring and firing costs of permanent and temporary workers before and after the reform. They use these estimates in order to predict the effect of this two-tier labor market reform keeping other institutions constant and finding a positive effect on total employment and the share of temporary workers as well as negative effects on firms productivity. An example from public economics is structural labor supply modelling (e.g. van Soest, 1995; Bargain, Orsini and Peichl, 2015). Estimated labor supply elasticities enable the simulation of a counterfactual world of labor supply when e.g. taxes change. In general, if assumptions of these model are not met, results might be biased. An example is the assumption of voluntary unemployment (e.g. Bargain, Caliendo, Haan and Orsini, 2010; Haan and Uhlenborff, 2013).

In the experimental/quasi-experimental approach, researchers rely on experiments with controlled random assignments of participants to the control and treatment group in order to specify their empirical model and identify the causal effect. Thereby, the treatment, e.g. a reform, is not evaluated ex-ante but ex-post. Hence, the ideal strategy for estimating a causal effect is a real experiment. Although controlled randomization is common in life sciences, such as medicine, it is less common in social sciences. In my case, I would need random selection of individuals/firms to EPLP. As these kinds of real experiments are difficult to conduct, researchers often rely on observational data and statistical methods. Based on this, one can at least proxy real experiments. The identification strategies are broadly categorized into those dealing with observational differences between treatment

and control group, e.g. regression analyses and propensity score matching, and those handling unobservable differences, e.g. instrumental variables, difference-in-difference approaches or selection models. If identifying assumptions are not met, the estimated ex-post effects are biased.

In this dissertation, I employ the experimentalist approach in order to investigate effects and test hypotheses. For this purpose, I make use of *regression analyses* and *difference-in-difference* approaches, both of which are briefly presented in the following sections.

### 1.3.1 Regression approach

The identifying assumption for a causal effect in regression analyzes is called the *conditional independence assumption*. This means that controlling for observable covariates ensures that the error is not correlated with the variable of interest. If one does not control for all relevant covariates, the error might be correlated with the variable of interest and yields an omitted variable bias. A special case is a bias due to reversed causality. This occurs when the dependent variable (e.g. share of temporary workers) has an effect on the covariate (e.g. EPLP) of interest.

### Application: Chapter 3

I employ regression analysis in order to test the hypothesis that a firm's propensity to employ at least one temporary worker is higher if the firm is exposed to annual workload fluctuations, but only if dismissal protection for regular workers is sufficiently high. I employ cross-country and within-country variation (firm-level) in order to obtain sufficient variation in EPLP and workload fluctuation, respectively.

Assuming that the profit of firm  $i$  in country  $j$  of employing at least one temporary worker can be presented in a latent variable approach, I estimate the following specification:

$$Y_{ij}^* = EPLP_j * WF_{ij}\gamma_1 + WF_{ij}\gamma_2 + \beta' X_{ij} + R_{ij} + U_j \quad (1.1)$$

with

$$Y_{ij} = 1[Y_{ij}^* > c] \quad (1.2)$$

and the latent variable ( $Y_{ij}^*$ ), employment protection legislation for regular workers ( $EPLP_j$ ), annual workload fluctuations ( $WF_{ij}$ ), a vector of controls ( $X_{ij}$ ) and the error terms (idiosyncratic term  $R_{ij}$ , country fixed effect  $U_j$ ). If the profit of employing at least one temporary worker ( $Y_{ij}^*$ ) is larger than  $c$ , firms employ at least one.

The conditional independence assumption means that all relevant covariates need to be observed and controlled for in order to avoid an omitted variable bias. Vector  $X_{ij}$  controls for a large battery of covariates at the firm-level (Salvatori, 2009; Böheim and Zweimüller, 2012; Kahn, 2007; Houseman, 2001; Bentolila and Dolado, 1994; Bentolila and Saint-Paul, 1992) and at the country-level (Kahn, 2010; Polavieja, 2005; Lazaer, 1990) which are relevant for the decision of a firm to employ temporary workers. In some models, any country-specific unobserved heterogeneity is account for by including country fixed-effects ( $U_j$ ). Due to clustering, errors are likely not to be independent which I allow for by estimating cluster-robust standard errors at the country-level.<sup>12</sup>

There, however, might still be concerns in terms of the conditional independence assumption due to reversed causality. First, high shares of temporary employment might volatilize domestic demand, which, in turn, might yield workload fluctuation at the firm-level. I argue that this point is less relevant in my case as temporary work is also strongly used by firms in sectors which depend less on domestic demand. I cannot, however, rule out reversed causality and account for this in my interpretation. Second, EPLP might be endogenous to the hiring behavior of firms and to the share of temporary workers. The hiring of temporary workers as well as the existence of high shares of temporary workers in the labor market in general might induce liberalizing reforms (Marx, 2012; Bentolila, Dolado and Jimeno, 2012). If these political-economy arguments hold, however, the positive conditioning effect of protection on employing temporary workers would bias be towards zero.

I combine establishment-level data with country-level data for 2004/2005 and 2009 where each cross-section consists of around 18,000 observations in up to 20 European countries. Establishment-level data are from the European Company

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<sup>12</sup>Cameron and Miller (2015) suggest other strategies, too, e.g. hierarchical models (Snijders and Bosker, 2012). I follow (Kahn, 2007).

Survey, which is a four-year survey starting in 2004/2005. It is conducted by the European Foundation for the Improvement of Living and Working Conditions (Eurofound). The survey is representative for establishments with more than ten employees in the European Union. The dependent variable is operationalized by a binary variable which equals one if an establishment employed temporary workers in the last 12 months. I proxy labor demand shocks of short duration with an item on whether an establishment normally faces workload shocks within a year. Employment protection legislation is measured at the country-level using a well-known indicator<sup>13</sup> from the OECD (Venn, 2009).

### 1.3.2 Difference-in-difference approach

In contrast to regression analyzes, the difference-in-difference approach is less demanding and allows for unobservable differences between control and treatment group. The identifying assumption which is employed here, however, is that these unobservable differences are constant over time conditional on the controls. This means that the dependent variable for the treatment and the control group is allowed to differ in its level due to unobservables but the difference in the level between the groups is not allowed to change over time. This assumption is called the *common trend assumption*.

This assumption is violated if the unobservable composition of the treatment and control group would change over time or if groups would differ in time-varying unobservable covariates. For instance, a reform decreases protection for small firms and induces workers with a (unobserved and uncontrolled) preference for high job security to select into large firms after the reform in order to be protected again. If those workers are also less satisfied with life, the effect of a decrease in employment protection for regular workers on life satisfaction would be downward biased due to changing compositions.

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<sup>13</sup>This indicator allows to distinguish between protection for regular workers, fixed-term contract workers and temporary agency workers.

### Application: Chapter 2 and Chapter 4

I employ a difference-in-difference approach to test the effect of EPLP on the share of temporary employees at the firm-level and on life satisfaction at the individual-level. The identification is based on within-country time and subgroup variation (Boeri and Jimeno, 2005). Employing this combination of variation became a standard tool in the evaluation of the effect of employment protection on objective outcomes (e.g. Boeri and Jimeno, 2005; Bauer et al., 2007; Kugler and Pica, 2008; Martins, 2009; Centeno and Novo, 2012; Leonardi and Pica, 2013).

In Germany, small firms are exempted from EPLP. The small firm-size threshold below which the firm is exempted from EPLP changed in the last two decades three times. In 1996, the small firm size threshold changed from 5 to 10 full-time-equivalent workers. In 1999, the threshold decreased from 10 to 5 workers. In 2004 the threshold increased again from 5 to 10 workers. Thereby, small firms (treatment group) faced a decrease in protection for permanent workers in 1996 and 2004 and an increase in 1999 while larger firms (control group) did not face changes. Thereby, one can compare the change in the dependent variable for the treatment group to that of the control group. When the common trend assumption is met, a difference-in-difference regression gives the causal effect of employment protection legislation.

I use a standard conditional difference-in-difference model:

$$Y_{it} = TG_{it}\gamma_1 + R_t\gamma_2 + TG_{it}R_t\gamma_3 + \beta'X_{it} + \epsilon_{it} \quad (1.3)$$

$$R_t = 1[\textit{year} \geq \textit{reform year}_t] \quad (1.4)$$

$$\epsilon_{it} = u_{it} + a_i \quad (1.5)$$

with  $Y_{it}$  as the dependent variable of individual/firm  $i$  in time  $t$ ,  $TG_{it}$ <sup>14</sup> is defined for each worker/firm depending on the number of workers in the firm in the year of observation (one for small firms (treatment group), zero for large firms (control group)),  $R_t$  is the reform dummy (zero before the reform takes place and one afterwards);  $\gamma_3$  gives the effect of employment protection on the dependent variable if the common trend assumption is met.

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<sup>14</sup>Depending on the specification, the treatment group dummy is time-invariant  $TG_i$ .

In order to avoid violation of the common trend assumption, I control for time-variant observables ( $X_{it}$ ) as well as for unobservable time-invariant individual/firm characteristics:  $\epsilon_{it} = \alpha_{it} + u_i$  with the idiosyncratic term ( $\alpha_{it}$ ) and the individual/firm fixed effect ( $u_i$ ). Hence, I control for compositional changes when observed and for unobservable time-invariant characteristics. Therefore, I checked whether the common trend assumption is plausible by employing a battery of indirect tests, e.g. a pre-reform trend test, a placebo treatment group test, a placebo reform test, and an anticipation test. Similar tests are conducted, e.g., by Leonardi and Pica (2013). The common trend assumption could still be violated. For instance, I would not capture selection if the selection process into the control or treatment group is due to unobservable time-variant variables. I discuss these issues in Chapter 2 and 4, separately.

## Chapter 2

I estimate the impact of dismissal protection on the share of temporary workers at the establishment-level in the difference-in-difference approach. The data source is the IAB establishment panel (IAB EP), an establishment-level survey of 4,000 to 19,000 establishments conducted annually since 1993. The survey is conducted by the research institute of the Federal Employment Agency. The population of establishments are all German establishments with at least one worker who is subject to social insurance contributions. The dependent variable is the share of temporary workers at the establishment-level. The major independent variable is the treatment group dummy which indicates whether a firm is in the treatment or in the control group. The allocation to the treatment or control group is based on the definition of full-time equivalents in the German EPLP. The IAB EP allows for a sound approximation of these full-time equivalents according to the German EPLP.

I allow the error term ( $\epsilon_{it}$ ) to consist of  $\alpha_{it}$  and  $u_i$  by estimating firm fixed-effects. As firms are clustered over time, it is reasonable to allow for  $\alpha_{it}$  to be correlated over time. I account for this by estimating cluster-robust standard errors clustered at the establishment-level. Furthermore, the share in temporary workers ( $Y_{it}$ ) is assumed to be continuous and unlimited.

## Chapter 4

I employ the reforms in German protection law in a difference-in-difference approach as described above to estimate the effect of employment protection legislation for regular workers on life satisfaction at the individual-level. I use the German Socio-Economic Panel (GSOEP), a representative annual household survey of around 11,000 households and 20,000 individuals conducted since 1984. The dependent variable well-being is proxied by life satisfaction. Life satisfaction is measured based on a standard life satisfaction question (0 to 10 scale). The major independent variable is the treatment group dummy. Due to data limitations, it can only be proxied; therefore, the effect of employment protection on life satisfaction is likely biased towards zero, which has to be accounted for.

The error term ( $\epsilon_{it}$ ) consists of  $\alpha_{it}$  and  $u_i$ . It is assumed that life satisfaction ( $Y_{it}$ ) is cardinal which is based on Ferrer-i-Carbonell and Frijters (2004). They show that the cardinality versus ordinality assumption is less relevant for the estimates in life satisfaction equations. In contrast, they pronounce the importance of considering individual fixed effects in life satisfaction equations ( $u_i$ ). Hence, my preferred model is an OLS fixed effect model. I allow the error terms to be correlated at the individual-level by estimating cluster-robust standard errors.

## 1.4 Summary and discussion

### 1.4.1 The effect and conditioning effect of EPLP on temporary employment

## Chapter 2

I test whether an almost perfectly symmetric increase and decrease in EPLP has a symmetric effect on the share of temporary workers. Applying within-country time and subgroup variation in a difference-in-difference regression approach with establishment-level panel data, the main result is that the EPLP reforms had an asymmetric effect. In line with expectations, the increase (decrease) in EPLP increased (decreased) the share of temporary workers. The effect, however, was stronger in terms of statistical and economical significance for the increase. Pre-

reform trend tests do not support a failure of the common trend assumption in the preferred specifications. The asymmetric pattern is relatively robust with reference to a time invariant definition of the treatment group dummy and to the size of the control group.

The asymmetric effects of changes in EPLP might contribute to explain less clear effects of EPLP on temporary employment in studies not allowing for asymmetries. Policy makers should consider this in designing EPLP reforms because they might under- or over-expect effects on temporary employment. Future research could built up on these findings by investigating the mechanism behind the asymmetry as well as the magnitude of an asymmetry.

### Chapter 3

The aim of Chapter 3 is to test whether short-term product demand shocks only increase the propensity to employ temporary workers when EPLP is sufficiently large in the respective country. Employing pooled cross-country firm-level data, I confirm this. Workload fluctuations within a year have no positive relation with the propensity to employ temporary workers in countries with low EPLP but an economically and statistically relevant relation in countries with sufficient large EPLP: The propensity to employ temporary workers for firms without annual fluctuations is around 70 percent, while for firms with fluctuations it is eight percentage points higher. I show that the results are robust with respect to fixed-term contract and - although less - to temporary agency workers. The second novel findings are related to the duration of shocks. I show that daily and weekly shocks are less positively related to temporary employment for the first time. Furthermore, I provide first findings on the distribution of the duration of shocks in European establishments which indicates that specifically annual fluctuations play a major role in Europe.

Thereby, I can generalize previous findings of single-country analyses and country-level analyses, while at the same time providing first insights in the role of the duration of shocks. Furthermore, I show that a considerable portion of firms in European economies are in the need of flexibility. Therefore, high EPLP might have severe effects on economic development and, at the same time, polarize job

quality, which is important for policy makers when reforming EPLP. Future research can build upon the findings by investigating adjustment mechanisms behind daily and weekly shocks. Furthermore, my empirical strategy is demanding in terms of its identifying assumptions and some concerns remain after discussing reversed causality and omitted variable bias in detail. Future research could investigate other sources of variation in order generalize these findings.

### 1.4.2 The effect of EPLP on well-being

#### Chapter 4

In Chapter 2 and 3, I show that employment protection legislation for regular workers is an important determinant of temporary employment. Reducing EPLP could decrease temporary employment. The aim of Chapter 4 is to shed light on the effect of such reforms on well-being. Employing a difference-in-difference approach based on German reforms in EPLP, I find a temporarily negative effect of the reduction in EPLP on the life satisfaction of temporary workers. An explanation would be that temporary workers who remain in a temporary job after the reform suffer from the comparison to colleagues who benefited from the decrease in EPLP by transitioning into a permanent job. This interpretation is in line with the finding of Centeno and Novo (2012) that EPLP adversely affects the transition probabilities from a temporary into a permanent job and is in line with the literature on social comparison. Another potential explanation, however, is that EPLP reforms induced selection into the treatment group. If this is the case and is driven by time-invariant unobservables or observables, I account for this. I cannot fully rule out selection due to unobserved time-invariant heterogeneity. I show, however, that pre-reform trends do not differ between treatment and control group. Thereby, I can at least reduce some concerns. For an increase in protection, I do not find significant effects which would be in line with loss aversion. The analysis focuses on temporary workers because the majority of permanent workers did not face major changes in EPLP. Pre-reform trend tests, anticipation tests, placebo reform and placebo group tests as well as group-specific linear trends do not support a violation of the common trend assumption.

This chapter provided first evidence on the effect of EPLP reforms on life satisfaction by employing evaluation tools. Thereby, I showed that temporary workers, on average, do not necessarily benefit from a decrease in EPLP (Saint-Paul, 2002). Policy makers liberalizing EPLP should consider that such reforms might affect temporary workers adversely, too. A deeper analyses of the mechanisms behind the negative effect which I discuss in the paper (e.g. comparison and anticipation hypotheses) remains open for future research, too. A drawback of this study is, that I can only reduce concerns about unobserved time-variant heterogeneity but cannot fully rule it out. Hence, future research investigating other sources of variation in EPLP would be beneficial in order to investigate the relevance of this concern. In general, research combining the literature on determinants of well-being and evaluation techniques when it comes to labor markets institutions is still rare but seems to be a promising field.

### 1.4.3 External validity

My samples consist either of establishments in the European Union, of small establishments in Germany or of workers in small German establishments. Hence, it is relevant whether results are externally valid beyond these cases (Cook, Campbell and Shadish, 2002). For generalization, it is important to account for relevant interactions of country-level as well as establishment- and worker-level variables with EPLP. There are at least four of those interactions which are important.

First, for the effect of EPLP on the behavior of firms, differential enforcement due to governance differences is relevant (e.g. Micco and Pages, 2007; Venn, 2009; Haltiwanger, Scarpetta and Schweiger, 2014). For instance, if corruption is high, legislation on employment protection might not be followed by the employers. Similar to aforementioned studies, I also consider governance indicators (government effectiveness, rule of law, control of corruption) in Chapter 3, too. I, however, do not find that countries with a low governance indicator have a different conditioning effect of EPLP compared to countries with a high indicator. This is explained by the fact that differential enforcement is specifically relevant in countries with very low enforcement, which is the case in e.g. developing countries (Venn, 2009). Employing a sample of developing and non-developing countries, Micco and Pages

(2007) have an average of -0.18 in the rule of law indicator, while in my sample of European countries the rule of law average is 1.26 on a scale from -2.5 to 2.5. Thus, I expect my results on the abolishment of EPLP and on the conditioning effect of EPLP on temporary employment to hold in countries with similar levels in the governance indicator but not necessarily in countries with low levels in this indicator, such as in developing countries.

Second, the effect of EPLP may also differ with respect to the level of EPLP itself. With reference to the literature on varieties of capitalism (Hall and Soskice, 2001; Estevez-Abe, Iversen and Soskice, 2001), coordinated economies (CME), e.g. Germany, Netherlands, and mixed market economies (MME), e.g. Spain, Italy, are usually characterized by more rigid labor markets with high levels of employment protections compared to liberal market economies (LME), e.g. Ireland, United States. Indeed, EPL as measured by the OECD was 65% smaller in LME compared to CME in the period from 1990 to 2002 (Hall and Gingerich, 2009). MME were even more regulated (1.65 times) compared to CME. Hence, abolishing EPLP in CME or MME makes a strong difference in firing costs for firms compared to LME. Germany is generally categorized as a CME and is characterized by relatively strong protection. Hence, in LME, I would expect only small effects of abolishing EPLP on firms behavior with regard to temporary workers.

Third, and related to the second, firm-specific skills - as well as industry-specific skills - are considered to be important in CME (Hall and Soskice, 2001). This is related to the level of EPLP and the rationale behind the investment in human capital by employees (e.g. Estevez-Abe et al., 2001; Wasmer, 2006a; Wasmer, 2006b). Hence, the substitution of permanent workers by temporary workers might be limited (Hijzen et al., 2013). This, however, would less likely be the case in LME, which are assumed to rely more on general skills (e.g. Hall and Soskice, 2001; Estevez-Abe et al., 2001; Wasmer, 2006a; Wasmer, 2006b). Hence, the effect of abolishing EPLP on temporary workers is expected to be stronger in LME such as in the United States compared to Germany. The conditioning effect of EPLP is also expected to be stronger in those countries - keeping other institutions constant. Furthermore, larger firms might rely more heavily on firm-specific skills (Pfeifer, Schönfeld and Wenzelmann, 2011). This may result in limits concerning the substitution of permanent workers by temporary workers in larger

firms. I would expect that the effect of abolishing EPLP is smaller in firms which are relatively large in Germany. In Chapter 3, I do not find that the conditioning effect of EPLP is driven by large or small firms.

Fourth, wage rigidity might be relevant for the effect of EPLP on temporary employment at the establishment-level as well as on life satisfaction (via temporary employment). According to Lazaer (1990), if wages are flexible, firms could at least compensate for EPLP related transfers, e.g. severance payments, by lower wages. This is empirically supported by Leonardi and Pica (2013). They show that an increase in EPLP has a negative effect on wages of new workers, specifically when they are in a weak bargaining position, e.g. young blue-collar workers or workers in labor markets with low employment rates. If firms can compensate EPLP by lower entry wages, numerical reactions are expected to be less strong (Leonardi and Pica, 2013; Lazaer, 1990). Hence, the effect of abolishing EPLP on temporary employment might be stronger in German establishments which are larger than those in the treatment group. This is because larger German firms employ, on average, more white-collar workers, and according to Leonardi and Pica (2013), blue-collar workers are associated with low bargaining power. As already mention, in Chapter 3, I do not find that the conditioning effect of EPLP is driven by large or small firms. Furthermore, the conditioning effect of EPLP for the effect of shocks and the effect of EPLP might be an upper bound estimate as other countries beyond the European Union and beyond Germany might have labor market institutions which are more supportive in terms of wage flexibility, i.e. less collective wage bargaining and lower unemployment benefits. In developing countries, for instance, effects are expected to be less strong.

I show that workload fluctuations are only positively related to temporary employment at the establishment-level if EPLP is sufficiently large. Next to the discussion on generalization above, I show that these results are robust with respect to country subsamples, to different periods (2009 and 2004/2005), as well as to sector subsamples. Thereby, this provides empirical support that one specific set of national institutions, one specific sectoral production function, or one specific year drives the results.

Finally, if the negative effect of abolishing EPLP on life satisfaction of temporary workers is due to the comparison to former temporary worker colleagues

who transitioned into a permanent job, the behavior of firms in terms of transforming temporary into permanent contracts is important for the effect of EPLP on life satisfaction. Hence, the aforementioned arguments apply to the results of the effect of EPLP on life satisfaction, too. Additionally, from the perspective of workers, the perception of individuals of income mobility might also play a role in the effect of EPLP on life satisfaction. For instance, Senik (2008) shows that the effect of social comparison (here income) is adverse in "old" European countries but that the relation is positive in the United States as well as in post-transition countries. Hence, in countries with a high perceived mobility, abolishing EPLP might not have a negative but rather, a positive effect on well-being of temporary workers.

## Chapter 2

# Employment protection reform effects on temporary employment

### 2.1 Introduction

In the public debate, employment protection legislation for permanent workers (EPLP) is considered to be a structural source of high incidences in temporary<sup>1</sup> employment, a high youth unemployment rate and low transition rates from temporary to permanent work. Thus, reform proposals for labor market institutions often suggest a decrease in EPLP. For instance, in Portugal, Spain, Italy, and Greece, EPLP was liberalized in the aftermath of the 2007 financial crisis (OECD, 2013b). Therefore, it is highly relevant to understand the effects of a decrease in EPLP on economic outcomes.

EPLP is generally modeled via firing costs of workers with a permanent contract (e.g. Bentolila and Bertola, 1990; Cahuc et al., 2012). Boeri (2011) predicts that by increasing these costs, the share of temporary employment increases, while the effect on the unemployment level is ambiguous. This finding is in line with papers employing within-country time and subgroup variation. Centeno and Novo (2012) show that the increase in EPLP has a positive effect on the share of temporary workers at the firm-level. Boockmann and Hagen (2001) show that a decrease in

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<sup>1</sup>Temporary is referred to as fixed-term contract workers (FTC) or and/or to temporary agency workers. The former holds a direct fixed-term contract with a firm, while the latter holds a contract with an employment agency and works on a fixed-term basis for a user firm.

EPLP has a negative effect on the binary decision to employ at least one temporary worker. Literature using within-country time variation but no subgroup variation, however, show that the effect of EPLP on temporary employment is less clear (Nunziata and Staffolani, 2007; Kahn, 2010).

Building upon this literature, we investigate for the first time the symmetry of the effect of symmetric EPLP reforms (increase, decrease) on temporary employment. None of the aforementioned papers analyzed, whether a decrease in EPLP has a similar effect on temporary employment as an increase in EPLP. If a decrease in EPLP affects temporary employment less strongly compared to an increase, this should be accounted for in theoretical models as well as by policy makers when they design reforms in EPLP.

Germany provides the unique opportunity to investigate a symmetric increase and decrease in EPLP. In 1999, German EPLP increased for new hires in firms with 6 to 10 full-time equivalent employees (FTE) but not for other firms. In 2004, EPLP decreased for new hires for the same group of firms. Both reforms were almost perfectly symmetric. These reforms provide within-country time and subgroup variation which we employ in a difference-in-difference approach (DID). This became a standard tool in the evaluation of EPLP<sup>2</sup> and was also applied in the case of German EPLP reforms (e.g. Boockmann and Hagen, 2001; Bauer et al., 2007; Boockmann, Gutknecht and Steffes, 2008). By focusing on firms which are close to the threshold (6-12 full-time equivalent employees), we conduct the DID in a regression discontinuity design (Leonardi and Pica, 2013).

The main result is that symmetric EPLP reforms have asymmetric effects in terms of statistical and economical significance. The increase in EPLP in 1999 raised the share of fixed-term workers in firms with 6 to 10 FTE by 1.7 percentage points. The decrease in EPLP in 2004 decreased the share of fixed-term workers. These effects, however, were asymmetric. The effect of the increase in EPLP was more economically (58% versus 12% of the mean) and statistically significant. The sign of the effects as well as the asymmetric pattern is robust to different definitions of the treatment group dummy, different controls and different samples. The change in share of fixed-term workers was driven by changes in the number of fixed-term workers, while the employment level remains almost unaffected. For

<sup>2</sup>For instance, Leonardi and Pica (2013), Scoppa (2010), Martins (2009), Kugler and Pica (2008).

our main results, we do not find pre-reform trend differences between treatment and control group, which supports the common trend assumption.

The article is organized as follows: In the next section, we explain the institutional setting with regard to EPLP and the degree of symmetry of the reforms in EPLP. In Section 2.3, we describe the theoretical and empirical background. A section on the empirical strategy follows. It presents the identification strategy, data sources, sample selection and the definition of the treatment group dummy as well as of the dependent variables. In Section 2.5, we present some descriptive statistics and the difference-in-difference results, and a conclusion is drawn in Section 2.6.

## **2.2 Institutional background**

### **2.2.1 Employment protection legislation for permanent workers**

Employment protection legislation regulates the hiring and firing of workers. Different employment protection legislation exists for permanent (open-ended or regular) and for temporary workers. One major difference between these two types of workers is the cost which is associated with the termination of a contract. Temporary contracts are of limited duration and end automatically after a specified period at no (or low) costs. In contrast, permanent contracts are of unlimited duration, and the termination of such a contract is costly. Employment protection for permanent workers increases firing costs of permanent workers in terms of transfers from the employer to employee (e.g. severance payments) and in terms of taxes (e.g. legal advice costs, court costs). In this paper, we focus on the effect of employment protection legislation for permanent workers on the employment of temporary workers.

In Germany, protection for permanent workers ranked among the top fifth of OECD countries in 2013 according to the OECD indicator (Venn, 2009; OECD, 2015). To give some perspective, this is comparable to the EPLP levels of Portugal and France but contrasts to that of the United Kingdom and the United States. German protection for permanent workers varies across subgroups of firms. All

firms have to meet minimum criteria for a fair dismissal e.g. no dismissal due to discriminatory reasons and laws for specific groups e.g. disabled workers. In addition, firms above a specific threshold in terms of number of employees have to apply more restrictive legislation when they terminate a permanent contract. We refer to this German legislation as employment protection for permanent workers (EPLP). EPLP exempts small firms. This small firm size exemption also exists/existed in other countries such as Italy (Leonardi and Pica, 2013) and Portugal (Centeno and Novo, 2012).<sup>3</sup>

Dismissals are only considered fair under EPLP regulations if: 1) the cause lies in the worker (e.g. long-term incapacity); 2) the worker's behavior is deemed damaging or unacceptable; 3) it is an economic necessity. Furthermore, a fair dismissal has several other conditions as well, for instance, to meet the criteria of a specific form, the obligation to properly inform about causes for the dismissal, and, in the case of collective dismissals, to give announcements. After being dismissed, a worker has the right to go to court in order to challenge the validity of the dismissal. If the dismissal is not valid, the employer has to continue the working contract or has to pay severance payments. Finally, since the German law is characterized by a high legal uncertainty (Eichhorst and Marx, 2011b), costs for legal advice increase and firing costs become highly uncertain.

### 2.2.2 EPLP reforms

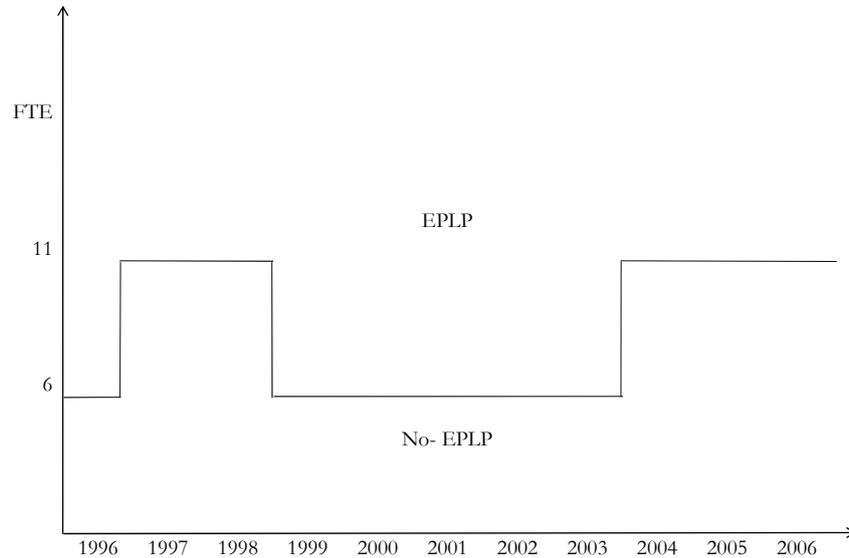
The specific design of EPLP reforms in Germany over the last 20 years provides the unique opportunity to evaluate whether a symmetric increase and decrease in EPLP has a symmetric effect on temporary employment in a quasi-experimental design. Figure 4.1 presents EPLP reforms in Germany in 1996, 1999 and 2004. The changes in legislation were mainly restricted to changes in the threshold of the number of full-time equivalent workers (FTEs) that, once passed, firms are required to adhere to the regulations of EPLP. The threshold (solid line) went up in 1996 (from 6 to 11 FTEs), down in 1999 (from 11 to 6 FTEs), and up in 2004 (from 6 to 11 FTEs). Thereby, for firms with 6 to 10 FTEs, EPLP increased (1999) and decreased (1996, 2004) symmetrically. Due to data limitations concerning

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<sup>3</sup>Venn (2009) gives an overview of small firm size exemptions in the OECD.

temporary employment, however, we only focus on the 1999 and 2004 reforms.

Figure 2.1: EPLP reforms in Germany from 1996 to 2005



Note: Own presentation; FTE: full-time equivalent workers; EPLP: employment protection legislation for permanent workers.

Before the reform in 1996, firms with 6 or more FTEs had to follow EPLP. During a period of economic downturn and flexibilization (Eichhorst and Marx, 2011a), the Christian Democratic Union/Free Democratic Party government increased the threshold from 6 to 11 FTEs in October 1996.<sup>4</sup> Hence, firms with 6-10 (FTEs) faced a decrease in EPLP for new hires but not for incumbents, who signed the labor contract before the reform took place (incumbents) and were exempted from the reform until 1999.

The second reform followed in a period of economic recovery and re-regulation (Eichhorst and Marx, 2011a) in the late 1990s. In January 1999, the newly elected

<sup>4</sup>EPLP for firms above 10 FTEs decreased as social selection criteria in the case of economic redundancies were loosened (Eichhorst and Marx, 2011a). Counting of FTEs changed, too.

Social Democratic/Green government revoked the reform from 1996 (Eichhorst and Marx, 2011a) by decreasing the threshold from 11 to 6 FTEs.<sup>5</sup> Thereby, firms with 6-10 (FTEs) faced an increase in EPLP for new hires but again not for incumbents hired at least two years before. Concerning anticipation, it is important to note that revoking the EPLP reform was already proposed by the Social Democrats during the election campaign in the fall of 1998 (Bauer et al., 2007). This has to be accounted for when interpreting results. From the empirical analyses, we can conclude, however, that anticipation is less relevant for our main results.<sup>6</sup>

The third reform took place in a period of an economic downturn and as being part of several liberalizing reforms in the 2000s. Among others, the Social-Democratic/Green government revoked the EPLP reform in 1999 and increased the threshold from 6 to 11 FTEs again.<sup>7</sup> Once more, incumbents were exempted from these reforms. Thereby, firms with 6 to 10 FTEs faced another decrease in EPLP for new hires. Unlike the previous reform, this reform was quite unforeseeable, thus anticipation is likely not a major factor (Bauer et al., 2007).<sup>8</sup>

Finally, our interest remains focused on the symmetry of EPLP reform effects. Hence, it is relevant whether the increase and decrease in EPLP are indeed symmetric. First, the reform in 1999 affected incumbents with a job tenure of less than two years, while the 2004 reform affected no incumbents. The share of incumbents with a job tenure below 3 years in total employment (including permanent and temporary workers) was around 27.9% in 1999 (OECD, 2015). We discuss potential implications for the symmetry of EPLP reforms in Section 2.3, which we summarize to be of minor relevance in our case. Second, the macro-economic conditions between the reforms differed. The 1999 reform took place in a period of good economic performance (1997 to 2001: GDP growth was 2.1 percent, and the unemployment rate growth rate was -2 percent), while the 2004 reform took

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<sup>5</sup>EPLP for firms above 10 FTEs increased again as the government strengthened selection criteria. We account for changes in the counting of FTEs in detail in Section 2.4.2.

<sup>6</sup>See Section 2.5 for a discussion.

<sup>7</sup>Social selection criteria increased again. Concerning regulations about severance payments, in the case of an economic reason for dismissal, employees can now exchange the right to go to the court for severance payments by elapsing the period for filing an action. As determining a permanent contract, however, is still related to uncertainty within the period for filing an action, we assess this change as irrelevant for the symmetry of EPLP reforms.

<sup>8</sup>This is supported in the empirical analyses in Section 2.5.2.

place in a period of lower economic performance (2002 to 2006: GDP growth was 1 percent, and unemployment rate growth rate was 2 percent). We discuss this issue in Section 2.3, and we conclude that this difference is less relevant in our case. Third, parallel reforms took place, e.g. liberalizing fixed-term employment. They are, however, considered to be not relevant for inducing trend differences in the treatment and control group, as we choose a very small neighborhood for our treatment and control group. In order to mitigate any concerns, however, we discuss potential relevant reforms in Section 2.5.2.<sup>9</sup>

## 2.3 Relevant literature

We are interested in how firms change their behavior in terms of employing temporary workers in responses to symmetric changes in EPLP. EPLP increases firing cost for firms, which are then modeled in the profit function of a firm. If EPLP only consists of transfers, if wages are flexible and if workers are risk-neutral, then EPLP is neutral in terms of employment as the transfer is entirely set off by lower wages (Lazaer, 1990). In most economies, however, wages are not perfectly flexible (e.g. due to unions), and firing costs also consist of taxes (Leonardi and Pica, 2013). In this case, EPLP clearly negatively affects labor market flows with less clear effects on employment levels (e.g. for a labor demand model, see Bentolila and Bertola, 1990). To uncover the effect of EPLP on temporary employment, no (or low) firing costs for temporary jobs and higher firing costs for permanent jobs need to be modeled. Blanchard and Landier (2002) and Boeri and Garibaldi (2007) present the effects of an introduction of entry jobs with lower firing costs (two-tier reforms).

Closer related to the institutional setting and EPLP reforms in Germany are studies which investigate an increase in EPLP in a two-tier regime. Boeri (2011) predicts - based on a search and matching model - that such a reform increases the

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<sup>9</sup>Fixed-term work was liberalized in 2001, 2003, 2004 and slightly re-regulated in 2005. Temporary agency work was liberalized in 1997, 2002 and 2003. Furthermore, we also checked parallel reforms in laws with firm size thresholds similar to the thresholds in EPLP. In August 2004, a reform took place which reduced the costs for break rooms only for firms with above 10 employers. These costs are, however, not related to the employment of fixed-term versus permanent workers.

share of temporary jobs in total employment, while unemployment is less clearly affected. Cahuc et al. (2012) - search and matching model - find that an increase in EPLP induces substitution of permanent jobs in favor of temporary jobs, while the employment level is less clearly affected.

In line with Cahuc et al. (2012) and Boeri (2011), we expect that the increase in EPLP in 1999 increased the share of temporary employment at the establishment-level. In 1999, firms faced increased EPLP for new hires and incumbents with less than two years of job tenure. Profit maximizing firms circumvent increased firing costs for permanent workers by hiring temporary workers instead of permanent workers.<sup>10</sup> In contrast, in 2004, firms faced a decrease in EPLP for new hires with a permanent contract. Firms might now reduce hiring temporary workers and increase the hiring of permanent workers as, e.g., costs associated with recruiting might be lower for permanent contracts.<sup>11</sup> We expect that a decrease in EPLP decreases the share of temporary employment.

Concerning the symmetry of reforms, first, the difference in the affected incumbents in the 1999 and the 2004 reform seems to be less relevant in our case - no permanently employed incumbents were affected from the decrease in EPLP in 2004, while permanently employed with less than two years of tenure were affected from the increase in EPLP in 1999. At the utmost, a reform which would also decrease EPLP for permanently employed incumbents with a job tenure of less than two years but not for other permanent workers would induce substitution of permanently employed incumbents with a tenure more than two years by new permanent contracts. This substitution, however, has no effect on the share of temporary workers. Second, in poor economic conditions the effect of EPLP reforms might be negligible (Boeri and Garibaldi, 2007, p. F371). In economic bad conditions, firms might stop hiring. Hence, a decrease in EPLP for newly hired workers would have no effect. In the case of an increase in EPLP, the similar

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<sup>10</sup>In addition, they might try to diminish incumbents with permanent contracts (e.g. through early retirement) and substitute them with temporary workers. A reform which would not increase EPLP for incumbents with tenure less than two years might have a slightly less strong effect on the share of temporary employment as those incumbents would not needed to be substituted by temporary workers.

<sup>11</sup>In addition, they might try to reduce incumbents with permanent contracts and substitute them with new permanent contracts at a lower EPLP level. This substitution, however, does not affect the share of temporary employment.

argument would apply.

To the best of our knowledge, recent empirical contributions do not investigate whether effects of symmetric EPLP reforms are symmetric themselves.<sup>12</sup> Employing within-country time variation and European individual data, Kahn (2010) studies the propensity of being a temporary worker and, after controlling for country-specific trends, does not find significant effects of EPLP reforms in this respect. In his specification, he assumes that increasing and decreasing reforms have a symmetric effect. Nunziata and Staffolani (2007), also exploiting within-country time variation in Europe, analyze the effect of EPLP on the share of temporary employment using macro-level data. Controlling for other institutions,<sup>13</sup> they find a positive relation between EPLP and the share of temporary workers. This relation becomes non-significant, however, when they control for economic recessions. Applying cross-country variation in EPLP, Dräger and Marx (2012) show that workload fluctuations at the firm-level increase the propensity to employ temporary workers only if EPLP is sufficiently large. They control for several firm-level and institutional variables, e.g. union coverage. Hence, EPLP has no robust effect on temporary employment in the literature that does not employ within-country subgroup variation. This literature is known for being prone to, e.g., omitted variable bias.

Employing within-country time and subgroup variation in a DID approach is less demanding in its identifying assumptions, and became a standard tool to obtain causal evidence on the effect EPLP reforms on objective outcomes. Within-country subgroup and time variation in EPLP is obtained, for instance, from reforms in EPLP variation across firm size (e.g. Leonardi and Pica, 2013; Centeno and Novo, 2012; Cappellari, Dell’Aringa and Leonardi, 2012; Martins, 2009; Kugler and Pica, 2008; Bauer et al., 2007), or across tenure (Marinescu, 2009). Leonardi and Pica (2013) combine the DID with a regression discontinuity design. When it comes to the effect of EPLP on the share of temporary workers, Centeno and Novo (2012) and Boockmann et al. (2008) are the only studies which investigate

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<sup>12</sup>Next to EPLP, screening, workload fluctuations, or parental leave are important determinants of temporary employment (e.g. Houseman, 2001; Cahuc et al., 2012; Dräger and Marx, 2012; Eslava et al., 2014).

<sup>13</sup>They control for union density, bargaining co-ordination, unemployment benefit replacement ratios and duration as well as for the tax wedge.

this identification strategy. Centeno and Novo (2012) employ within-country time and subgroup variation and find that the share of temporary workers increased by 1.6 percentage points after an increase in Portuguese EPLP. Employing a similar identification strategy, Boockmann and Hagen (2001) estimate that the decrease in German EPLP in 1996 decreased the probability of employing temporary workers at the firm-level. Applying subgroup variation in a regression discontinuity design, Hijzen et al. (2013) show that firms subject to EPLP employ 2-2.5 percentage points more temporary workers than firms not subject to EPLP in Italy. Thus, EPLP has a positive relation with the share of temporary employment. None of these studies, however, investigated whether effects of symmetric reforms in EPLP are symmetric. Germany provides the unique opportunity to investigate this in a quasi-experimental setting.

Although not studying symmetry, several papers have already investigated reforms in German EPLP. Verick (2004) shows that the rise in EPLP decreased the propensity of employment growth and increased the propensity of firms to remain below the new FTE threshold (IAB Establishment Panel). He concludes, however, that results might be driven by other factors than the reform. Bauer et al. (2007) estimate the effect of the 1996 and 1999 EPLP reforms on worker flows based on social security records provided by the IAB. They find no effects. Burgert (2006) shows that a rise in EPLP decreases hiring of older employees (IAB Linked-Employee-Employer-Data (LIAB)). Boockmann et al. (2008) find that the increase in EPLP in 1999 raised the job duration (LIAB), and Boockmann and Hagen (2001) show that the decrease in EPLP in 1996 decreased the probability of employing temporary workers. Results indicate that stocks and workers flows are less affected by reforms in German EPLP but that atypical employment might be affected.

## 2.4 Empirical strategy

### 2.4.1 Identification strategy

To investigate the effects of symmetric EPLP changes on temporary employment, we rely on variations in how the reforms affected firms of different sizes through

a difference-in-difference approach. Due to the fact that the reforms changed the regulations for the threshold at which firms must comply with EPLP, these changes only affected small firms but not large firms. Thereby, we can compare the difference over time for small (affected) versus large (not affected) firms with respect to temporary employment.

We use the conditional difference-in-difference model which is estimated for each reform separately:

$$y_{it} = \beta_0 + TG_{it}\beta_1 + R_t\beta_2 + TG_{it}R_t\beta_3 + X_{it}\beta_4 + \epsilon_{it}, \quad (2.1)$$

with  $y_{it}$  as the dependent variable of firm  $i$  in time  $t$ .  $TG_{it}$  is defined for each firm in each period of observation and is one if a firm is in the treatment group (small firm) and zero if a firm is in the control group (large firm). In the baseline specification, this dummy is time-variant.<sup>14</sup> Hence, it alters with firm size.  $R_t$  is the reform dummy and zero before the reform takes place (1997-1998 or 2002-2003) and one afterwards (1999-2001 or 2004-2006). The coefficient  $\beta_3$  gives us the reform effect. We allow the error term ( $\epsilon_{it}$ ) to include firm fixed effects ( $\epsilon_{it} = \alpha_{it} + u_i$ , with  $\alpha_{it}$  as the idiosyncratic term and  $u_i$  as the firm fixed effect). The estimated standard errors allow for potential error correlation within firms and for heteroscedasticity.

In order to estimate whether the reform effect fades or grows and whether any pre-reform differences between treatment and control group exist, we add additional reform dummies into a dynamic specification of the DID:

$$R_{t-1} = 1[\text{year} \geq \text{reform year}_{t-1}] \quad (2.2)$$

$$R_{t+1} = 1[\text{year} \geq \text{reform year}_{t+1}] \quad (2.3)$$

First, we include a pre-reform dummy ( $R_{t-1}$ ) which is zero and turns one in the year before the reform takes place. The interaction between  $R_{t-1}$  and  $TG_{it}$  is also added. This measures whether the treatment group has a different pre-reform trend. If this is the case, the common trend assumption might be violated. Second, we also include a post-reform dummy ( $R_{t+1}$ ), which is zero and turns one in the

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<sup>14</sup>Robustness checks are conducted for other specifications too.

year after the reform takes place. The interaction with  $TG_{it}$  is added and measures whether the treatment group has a different trend one year after the reform took place.

The difference-in-difference approach only provides unbiased estimates of the reform effect if the common trend assumption is met. In other words, control and treatment group are allowed to differ but these differences are not allowed to change over time. In order to avoid violation of this assumption, we take the following steps. First, we generalize the assumption. Differences between control and treatment are not allowed to change over time under the condition that we control for time-variant observables and time invariant unobservables. Time-variant observable firm characteristics ( $X_{it}$ ) are the share of blue and white color workers, share of part-time workers, share of women, industry dummies, federal state dummies as well as the share of workers in dual apprenticeship.<sup>15</sup> Time invariant firm characteristics are accounted for by estimating firm fixed-effects ( $u_i$ ). Second, we restrict the sample to firms close to the threshold. Thereby, we keep the firms as similar as possible. Leonardi and Pica (2013) call this a combination of a DID with a regression discontinuity design. Third, we conduct pre-reform trend tests. For the main results, we do not find differences in the pre-reform trend.

## 2.4.2 Data

We use the IAB Establishment Panel (IAB EP), which has been conducted since 1993 (Fischer, Janik, Müller and Schmucker, 2008). Each wave of the IAB EP consists of around 4,000 to 16,000 observations.<sup>16</sup> The IAB EP questionnaire is answered with reference to June 30th. The population of establishments consists of all German establishments with at least one employee who is subject to social insurance contributions. The sample is drawn within size (i.e. employees) and industry cells. Establishments with large numbers of employees are over-represented.

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<sup>15</sup>Results are robust when we control for temporary agency work (Appendix 2.7.4, Table 2.18). Due to data limitation, we are only able to conduct this test for the EPLP reform in 2004).

<sup>16</sup>Observations increase over time (Bellmann, Kohaut and Lahner, 2002): in 1996 from 4,000 to 8,000 cases (survey extension to East Germany), in 2000 from 10,000 to 14,000 (sample size increase).

We chose waves from 1997 to 2006. Data access was provided via on-site use at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) followed by subsequent remote data access.

## Variables

### Dependent variables

We employ different dependent variables which are related to fixed-term workers (summarized in Table 2.1<sup>17</sup>).<sup>18</sup> Fixed-term workers are those who hold a contract of limited duration with the interviewed establishment.<sup>19</sup> The main dependent variable is the share of fixed-term workers within total employment (*ShFTC*)<sup>20</sup>:

$$ShFTC = \frac{FTC}{E}, \quad (2.4)$$

with  $E$  as the total number of employees (i.e. including workers liable to social security as well as workers not-liable to social security<sup>21</sup> and others) and  $FTC$  as the number of fixed-term contract workers. An effect of EPLP on *ShFTC* could be due to changes in the number of employees ( $E$ ), in the number of fixed-term workers ( $FTC$ ), or due to changes in both. In order to disentangle the driver, we estimate in addition the effects of EPLP reforms on the number of employees ( $E$ ) and the number of fixed-term workers ( $FTC$ ), separately. Furthermore, we analyze the effect of EPLP on the propensity to employ any fixed-term worker ( $Any\ FTC = 1[ShFTC > 0]$ ) - i.e. at the extensive margin - as well as on the share of fixed-term workers in those companies employing at least one fixed-term worker ( $ShFTC\ if > 0$ ) - i.e. at the intensive margin.

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<sup>17</sup>For an overview of the definitions and the original questions in the IAB EP, see Appendix 2.7.1.

<sup>18</sup>We refer to fixed-term workers rather than temporary workers as we focus on this type of temporary workers.

<sup>19</sup>Establishments might also substitute by hiring temporary agency workers. Results are, however, robust controlling for temporary agency work in the 2004 reform (Appendix 2.7.4, Table 2.18).

<sup>20</sup>The IAB EP does not contain the hiring of fixed-term workers for the relevant period.

<sup>21</sup>For instance, these are workers in a so called "Mini-Job" in Germany.

Table 2.1: Dependent variables ( $y_{it}$ )

$y_{it}$	Definition
Share of fixed-term workers ( $ShFTC$ )	$ShFTC = \frac{FTC}{E}$
Total number of employees ( $E$ )	$E$
Total number of fixed-term contract workers ( $FTC$ )	$FTC$
Employing at least one fixed-term worker ( $Any\ FTC$ )	$Any\ FTC = 1[ShFTC > 0]$
Share of fixed-term workers in establishments with at least one fixed-term worker ( $ShFTC$ if $> 0$ )	$ShFTC = ShFTC$ if $Any\ FTC == 1$

### Treatment group dummy

The treatment group dummy is based on the number of full-time equivalent workers (FTE). According to the German EPLP, FTEs are defined as the sum of regular (excluding workers in training) full-time and weighted part-time employees. As we do not observe working hours (WH) in the IAB EP, we are not able to calculate the number of FTEs perfectly as defined by the German EPLP (Table 2.12 (column 2)). We approximate the German EPLP definition by assuming that part-time workers are equally distributed among the categories within the working hours listed in Table 2.12 (column 2) and simply apply the average of the working hour category specific weights (Table 2.12 (column 3)). In order to calculate FTEs, we subtract trainees from the total number of employees ( $E$ ) and weight<sup>22</sup> the number of part-time workers according to Table 2.12 (column 3).

FTEs are the sum of regular (excluding workers in training) full-time and weighted part-time employees. Due to data limitations, we have a minor measurement error in FTEs, which might bias the estimates towards zero. The time-variant treatment group dummy ( $TG_{it}$ ) is one (treatment group) if establishments employ 6 to 10 FTEs and zero (control group) if establishments employ 11 to 12 FTEs<sup>23</sup>. With 6 to 12 FTEs, we chose a small neighborhood of establishments, which is

<sup>22</sup>See for a detailed description of how we deal with changes in the weighting key in Appendix 2.7.2.

<sup>23</sup>We choose larger establishments as the control group because Bauer et al. (2007) show that small establishments exhibit different dynamics in terms of insolvencies. We conduct, however, robustness checks with 11 to 15 FTEs and discuss the results in Section 2.5.2.

common in a regression discontinuity design and which supports keeping the treatment and control groups similar.  $TG_{it}$  is defined for each period separately.

Establishments, however, might select the number of FTEs endogenously to reforms in EPLP. For instance, if some establishments are in the need of high levels of numerical flexibility in the workforce, these establishments also have a high share of fixed-term workers in comparison to other establishments. After the 2004 reform, establishments with the need for flexibility and with 11 FTEs might try to decrease FTEs to 10. Thereby, they could circumvent EPLP but might bias the negative effect of a decrease in EPLP on fixed-term employment towards zero. If the selection process is driven by unobserved time invariant heterogeneity or by observed time-variant heterogeneity, we account for this. If this is, however, not the case, we do not capture endogenous selection. In order to check whether changes in the treatment group status after the reform affect our estimates, we define a time invariant treatment group dummy ( $TG_i$ ). The treatment group dummy is one if establishments employ 6 to 10 FTEs in the year before the reform and zero if establishments employ 11 to 12 FTEs in the year before the reform (same vein as Centeno and Novo (2012) and Kugler and Pica (2008)).

## Sample selection

We construct samples for each evaluated reform. For the 1999 reform, the sample is from 1997 to 2001, and for the 2004 reform, from 2002 to 2006. We do not choose a longer pre-reform period for the 1999 reform due to the reform in October 1996. Furthermore, we do not choose a longer post-reform period for the 1999 reform because of the reform in 2004. For the sake of comparability, we choose the same length for the 2004 reform sample. We exclude units in the public sector, establishments without any worker who is subject to social insurance contributions,<sup>24</sup> units above the 95th percentile of the share of trainees<sup>25</sup> and establishments with major

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<sup>24</sup>We apply this restriction in order to be able to weight the descriptive statistics. The IAB provides weights, however, only for cross-sections which exclude establishments with no worker who is subject to social insurance contributions. They also provide weights for longitudinal data sets which are constructed by the IAB. As we construct our own panel, we rely on cross-sectional weights for the descriptive statistics.

<sup>25</sup>These are educational institutions which employ almost only trainees. We excluded those because they yielded skewed distribution of the number of employees. Results are, however,

changes to their production function<sup>26</sup>. Finally, the estimation samples for the difference-in-difference analyses are restricted to establishments which are either in the treatment or in the control group, i.e. between 6 to 12 FTEs.

## 2.5 Empirical results

### 2.5.1 Descriptive statistics

Table 2.2 presents representative summary statistics of German establishments employing 6 to 12 FTEs in the periods 1997-2001 (reform in 1999) and 2002-2006 (reform in 2004).<sup>27</sup> On average, in both periods 1997-2001 and 2002-2006, establishments employed around 8 FTEs. This is consistent with the fact that around 85% of establishments were in the group of establishments employing 6 to 10 FTEs. The number of employees is higher as part-time workers do not count fully and trainees do not count at all: Establishments with 6 to 12 FTEs employ on average around 10 workers with a maximum of 29 workers. Part-time work increased, on average, in the periods 1997-2001 versus 2002-2006. This is also reflected in the larger maximum number of employees in the 1997-2001 versus 2002-2006 period (26 versus 29 workers).

On average, the share of fixed-term workers was 3 and 4%, and the median was zero in both periods. Hence, only a small share of establishments (6 to 12 FTEs) employed at least one fixed-term contract worker - between 13 and 17%. The trend in employing fixed-term workers is positive. The share of fixed-term workers at the establishment-level increased over time from 3% to 4%. This is due to increases at the extensive margin rather than at the intensive margin. The share of establishments employing at least one fixed-term contract worker increased from 13% to 17%. The share of fixed-term workers in establishments which employ at least one fixed-term worker decreased from 24% to 22%.

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robust when including those cases (Appendix 2.7.4, Table 2.17).

<sup>26</sup>The establishment number is based on a local production unit. If a unit changes the production, e.g. from micro-chip production to ice cream production, the establishment number remains the same; however, the establishment faced fundamental changes. Fischer et al. (2008) do not consider those cases as IAB EP panel cases. We follow their suggestion.

<sup>27</sup>Number of observations increased, specifically due to the sample size increase in 2000 from 10,000 to 14,000 (Bellmann et al., 2002).

Table 2.2: Summary statistics

<b>Reform 1999</b>						
Variable	Mean	Median	Min	Max	SD	N
FTE	8.00	8.00	6.00	12.00	1.83	6190.00
$TG_{it}$	0.87	1.00	0.00	1.00	0.34	6190.00
Employees	9.87	9.00	6.00	26.00	2.80	6190.00
FTC	0.29	0.00	0.00	24.00	1.08	6190.00
Part-time (%)	0.18	0.11	0.00	1.00	0.22	6190.00
Trainees (%)	0.04	0.00	0.00	0.25	0.07	6190.00
Wage per head	1771.75	1591.98	89.22	9713.06	896.95	5615.00
$ShFTC$ (%)	0.03	0.00	0.00	1.00	0.11	6190.00
$ShFTC$ if > 0 (%)	0.24	0.14	0.05	1.00	0.23	915.00
<i>Any FTC</i>	0.13	0.00	0.00	1.00	0.33	6190.00
Low qualified (%)	0.25	0.14	0.00	1.00	0.28	6190.00
High qualified (%)	0.58	0.64	0.00	1.00	0.28	6190.00
<b>Reform 2004</b>						
Variable	Mean	Median	Min	Max	SD	N
FTE	8.10	8.00	6.00	12.00	1.89	10490.00
$TG_{it}$	0.85	1.00	0.00	1.00	0.35	10490.00
Employees	10.44	10.00	6.00	29.00	3.22	10490.00
FTC	0.41	0.00	0.00	24.00	1.44	10490.00
Part-time (%)	0.23	0.14	0.00	1.00	0.24	10490.00
Trainees (%)	0.04	0.00	0.00	0.23	0.06	10490.00
Wage per head	1932.19	1714.29	133.33	15151.52	1057.20	9058.00
$ShFTC$ (%)	0.04	0.00	0.00	1.00	0.12	10490.00
$ShFTC$ if > 0 (%)	0.22	0.13	0.04	1.00	0.21	2037.00
<i>Any FTC</i>	0.17	0.00	0.00	1.00	0.37	10490.00
Low qualified (%)	0.22	0.11	0.00	1.00	0.27	10490.00
High qualified (%)	0.63	0.70	0.00	1.00	0.27	10490.00

Note: Establishments from 1997-2001 or 2002-2006 of non-public sectors with 6-12  $FTE_{its}$ ,  $TG_{it}$  and excluding outliers (share of trainees) and no missing values in model relevant variables (estimation sample). Cross-sectional weights. Source: Own calculation based on IAB EP.

Our estimation sample consists of 6,190 observations for the 1999 reform and 10,490 observations for the 2004 reform. First, Table 2.3 presents how these establishments are distributed between treatment and control group. Around 80% of these establishments were in the treatment group. The share of small establishments is smaller compared to the weighted descriptives. This is due to over representation of larger establishments in the IAB EP.

Table 2.3: Distribution of  $TG_{it}$  in % by year

<b>Reform 1999</b>						
	1997	1998	1999	2000	2001	Total
Control group ( $TG_{it} = 0$ )	18	19	20	20	18	19
Treatment group ( $TG_{it} = 1$ )	82	81	80	80	82	81
Total	100	100	100	100	100	100
<b>Reform 2004</b>						
	2002	2003	2004	2005	2006	Total
Control group ( $TG_{it} = 0$ )	21	20	20	21	20	20
Treatment group ( $TG_{it} = 1$ )	79	80	80	79	80	80
Total	100	100	100	100	100	100

Note: Establishments from 1997-2001 or 2002-2006 of non-public sectors with  $TG_{it}$  and excluding outliers (share of trainees) and no missing values in model relevant variables (estimation sample). Source: Own calculation based on IAB EP.

Second, threshold effects in Germany are not confirmed in the literature: Bauer et al. (2007) do not find EPLP threshold effects on full-time equivalents; Kölling, Schnabel and Wagner (2001) show that disability law threshold effects on employment do not exist; Verick (2004) finds a decreased probability for employment growth for establishments below the EPLP threshold but concludes that this finding might be due to omitted factors. Hence, selection of FTEs due to the EPLP reforms should not be a major issue.

We show, however, some descriptives on the dynamics in FTE of establishments with 6 to 10 FTEs. Table 2.4 presents the share of these establishments which remained in the category of 6 to 10 FTEs ( $t+1$  6-10), decreased FTEs below 6 ( $t+1 < 6$ ), increased FTEs above 10 ( $t+1 > 10$ ), and the share of establishments in which FTE was not defined in the next period (other), e.g. due to panel attrition or missing values. Around 60% of establishments remained in the treatment group. Concerning the 1999 reform, we observe a lower share of establishments which

stay in the treatment group in the pre-reform period compared to the post-reform period. This is also true for the 2004 reform. If selection is the reason, for example, this could be explained by establishments which aim to avoid EPLP by decreasing FTEs after the reform and by establishments which avoided EPLP prior to the 1999 reform but grow now. As already mentioned, however, threshold effects were not confirmed for Germany (e.g. Kölling et al., 2001; Bauer et al., 2007).

Table 2.4: Do firms with 6-10 FTEs change FTEs over time?

<b>Reform 1999</b>					
	t+1 <6	t+1 6-10	t+1 >10	other	Total
1997	15.95	56.98	14.81	12.25	100.00
1998	16.93	53.94	12.47	16.67	100.00
1999	14.65	62.67	12.24	10.44	100.00
2000	17.08	58.69	11.62	12.62	100.00
2001	15.30	58.18	10.72	15.80	100.00
Total	15.99	58.25	12.04	13.72	100.00
<b>Reform 2004</b>					
	t+1 <6	t+1 6-10	t+1 >10	other	Total
2002	12.73	56.61	10.34	20.31	100.00
2003	12.37	62.37	12.15	13.10	100.00
2004	15.47	62.29	11.06	11.18	100.00
2005	16.57	60.99	10.10	12.35	100.00
2006	10.53	66.46	11.22	11.79	100.00
Total	13.56	61.76	10.99	13.68	100.00

Note: Share of establishments which remained in the category of 6 to 10 FTEs (t+1 6-10), decreased FTEs below 6 (t+1 <6), increased FTEs above 10 (t+1 > 10), and the share of establishments in which FTE was not defined in the next period (other). Establishments from 1997-2001 or 2002-2006 of non-public sectors with 6-12  $FTE_{it}$ s,  $TG_{it}$  and excluding outliers (share of trainees) and no missing values in model relevant variables (estimation sample). Source: Own calculation based on IAB EP.

## 2.5.2 Difference-in-difference results

### Main results

The main result is that the almost perfectly symmetric increase in 1999 and decrease in 2004 in EPLP have symmetric effects on the share of fixed-term employment in terms of sign, but they have asymmetric effects in terms of economic and statistical significance.<sup>28</sup> The direction of the effects is as expected. The increase in EPLP in 1999 increased the share of fixed-term contract workers significantly, both statistically and economically. The effect of the decrease in EPLP in 2004 was less economically and statistically significant. Table 2.5 presents the reform effects of the standard specification ( $\beta_3$ ) - the interaction between treatment group dummy and reform dummy. Table 2.6 presents the dynamic specification with pre-reform effects and post-reform dynamics.

### Standard DID specification

The increase in EPLP in 1999 gave rise to the share of fixed-term workers by 1.73 percentage points in the post-reform period compared to the pre-reform period (Table 2.5, column 1). This is similar to Centeno and Novo (2012) who also employ within-country subgroup and time variation in EPLP. They found that EPLP increased fixed-term employment by 1.6 percentage points. The increase in Table 2.5 is not only statistically but also economically significant. The share of fixed-term workers in the estimation sample is 3%, thus, an increase of 58% of the mean.

In order to disentangle the effect of EPLP on the share of fixed-term workers ( $ShFTC$ ), we estimate the effect of the EPLP reforms on the number of employees (column 2,  $E$ ) as well as on the number of fixed-term workers (column 3,  $FTC$ ). The effect on  $ShFTC$  seems to be driven by  $FTC$ . The policy coefficient for the total number of employees (0.6% of employees) is economically less significant compared to the effect on the number of fixed-term workers (58% of the mean). They are, however, not statistically significant. The policy coefficient for the binary decision to employ any fixed-term worker and for the share of fixed-term workers

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<sup>28</sup>Full models are presented in Appendix 2.7.3.

Table 2.5: EPLP effect on fixed-term work

	(1)	(2)	(3)	(4)	(5)
	<i>ShFTC</i>	<i>E</i>	<i>FTC</i>	<i>Any FTC</i>	<i>ShFTC</i> if $> 0$
<b>Reform 1999</b>					
Reform x Treat	0.0173**	0.0595	0.169	0.0400	0.0811
	(2.00)	(0.53)	(1.44)	(1.21)	(1.62)
<i>N</i>	6190	6190	6190	6190	915
<i>R</i> <sup>2</sup>	0.031	0.487	0.027	0.021	0.092
<i>Mean y<sub>it</sub></i>	0.03	9.87	0.29	0.13	0.24
<b>Reform 2004</b>					
Reform x Treat	-0.00447	0.133*	-0.160*	-0.00103	-0.0185
	(-0.79)	(1.95)	(-1.93)	(-0.04)	(-0.78)
<i>N</i>	10490	10490	10490	10490	2037
<i>R</i> <sup>2</sup>	0.014	0.411	0.023	0.013	0.037
<i>Mean y<sub>it</sub></i>	0.04	10.44	0.41	0.17	0.22

Note: Fixed effect estimators with clustered standard errors; Dependent variables: share of fixed-term workers (*ShFTC*), establishment employs any fixed-term worker (*Any FTC*), *ShFTC* in establishments which employ at least one fixed-term worker (*ShFTC* if  $> 0$ ), number of employees (*E*), number of fixed-term employees (*FTC*); Controls: TG dummy, reform dummies, share of low qualified, high qualified, part-time workers, female workers and trainees, branch and federal state dummies; Establishments from 1997-2001 or 2002-2006 of non-public sectors with 6-12 *FTE<sub>it</sub>*s, *TG<sub>it</sub>* and excluding outliers (share of trainees) and no missing values in model relevant variables (estimation sample); *t* statistics in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 2.6: EPLP effect on fixed-term work

	(1)	(2)	(3)	(4)	(5)
	<i>ShFTC</i>	<i>E</i>	<i>FTC</i>	<i>Any FTC</i>	<i>ShFTC</i> if > 0
<b>Reform 1999</b>					
Reform[t-1] x Treat	-0.00313 (-0.25)	0.00816 (0.06)	-0.141 (-1.01)	-0.0404 (-0.80)	0.0437 (0.86)
Reform x Treat	0.0325** (2.48)	-0.0151 (-0.11)	0.304* (1.66)	0.0948** (2.10)	0.0958 (1.32)
Reform[t+1] x Treat	-0.0230*** (-2.79)	0.118 (0.97)	-0.128 (-0.92)	-0.0630* (-1.72)	-0.0488 (-1.01)
<i>N</i>	6190	6190	6190	6190	915
<i>R</i> <sup>2</sup>	0.033	0.487	0.028	0.022	0.098
<i>Mean y<sub>it</sub></i>	0.03	9.87	0.29	0.13	0.24
<b>Reform 2004</b>					
Reform[t-1] x Treat	0.0108 (1.23)	0.104 (1.02)	0.115 (1.05)	0.0299 (0.93)	0.0496 (1.54)
Reform x Treat	-0.0128* (-1.66)	0.100 (1.29)	-0.258** (-2.26)	-0.0143 (-0.47)	-0.0448 (-1.40)
Reform[t+1] x Treat	0.00690 (1.03)	-0.0135 (-0.19)	0.0889 (0.89)	0.00249 (0.10)	0.0164 (0.57)
<i>N</i>	10490	10490	10490	10490	2037
<i>R</i> <sup>2</sup>	0.014	0.411	0.024	0.013	0.041
<i>Mean y<sub>it</sub></i>	0.04	10.44	0.41	0.17	0.22

Note: Fixed effect estimators with clustered standard errors; Dependent variables: share of fixed-term workers (*ShFTC*), establishment employs any fixed-term worker (*Any FTC*), *ShFTC* in establishments which employ at least one fixed-term worker (*ShFTC* if > 0), number of employees (*E*), number of fixed-term employees (*FTC*); Controls: TG dummy, reform dummies, share of low qualified, high qualified, part-time workers, female workers and trainees, branch and federal state dummies; Establishments from 1997-2001 or 2002-2006 of non-public sectors with 6-12 *FTE<sub>it</sub>*s, *TG<sub>it</sub>* and excluding outliers (share of trainees) and no missing values in model relevant variables (estimation sample); *t* statistics in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

in establishments with at least one fixed-term worker is positive but not significant in statistical terms.

The decrease in EPLP in 2004 had no statistically significant effect on *ShFTC* (Table 2.5). Also, the economic significance is less strong compared to the 1999 reform: The policy coefficient is only around 11% of the mean (compared to 58% of the mean in 1999). The number of fixed-term workers decreased by 0.16, which is 39% of the mean. This is less significant in economic terms compared to the 1999 reform (58% of the mean). At the same time, employment increased, but only by 1% of the mean which is economically less relevant. All other coefficients are negative but not significant.

### **Dynamic specification**

The dynamic specification is presented in Table 2.6. For the 1999 reform, the EPLP effect in the year of the reform is quite substantial for the share of fixed-term workers. When EPLP increased, establishments employed significantly more fixed-term employees (3.25 percentage points), which is also significant in economic terms (108% of the mean). This is again due to the increase in the total number of fixed-term workers (0.304 persons), which is significant statistically and economically (105% of the mean). Furthermore, it was 9.48 percentage points more likely that an establishment employed any fixed-term worker six months after the reform than in the pre-reform year. The increase in EPLP does not seem to have affected the share of fixed-term workers in establishments employing at least one fixed-term worker. The effect is positive but not significant. As the number of observations is quite small, however, the standard errors are quite high.

Effects on the share of fixed-term workers and on the propensity to employ at least one fixed-term worker, however, decrease 1.5 years after EPLP increased. In comparison to six months after the reform, the share of fixed-term employment increased by 2.3 percentage points. This could be explained by fixed-term employment being used as a screening device. In this case, after the increase in EPLP, establishments hire fixed-term workers rather than permanent workers. Then after one year, they transform the fixed-term contract into a permanent contract if the match quality is good.

The decline in EPLP in 2004 (lower panel) decreased the share of fixed-term employment six months after the reform significantly by 1.28 percentage points compared to the previous year. This is a decrease by 32% of the mean, which is much smaller compared to the 108% from the 1999 reform. The effect of the decrease in EPLP on the share of fixed-term workers is driven by the decrease in the total number of fixed-term workers: They increased by 0.258 persons, which is 63%. The effect in 1999 was economically more significant (105% of the mean), but the total number of employees is not significantly affected. Similar to the standard DID specification, the propensity to employ any fixed-term worker as well as the share of fixed-term workers in establishments with at least one fixed-term worker do not show any significant effects. The effect on the share of fixed-term workers does not fade.

### **Symmetry of reforms**

Overall, we consider the reforms to be almost perfectly symmetric. We discuss, however, potential challenges, which are shown to be less relevant for our main conclusion on the asymmetric effects of EPLP reforms. First, concerning parallel reforms, we chose a small neighborhood in order to avoid very different establishments and violations of the common trend assumption. Therefore, we do not expect that parallel reforms are crucial in our case. Furthermore, if the reforms would be relevant, the main conclusion of asymmetry would be not affected. In 2001, liberalizing reforms of fixed-term employment took place. Thus, if large establishments have in general more positive trends in the share of FTCs, the reform could have strengthened the trend difference. In such a case, we would actually underestimate the positive effect of the increase in EPLP on the fixed-term employment.

In 2004, fixed-term work was liberalized parallel to the EPLP reform. One could argue again that large establishments employ more fixed-term workers and that reforms might result in a more positive trend in fixed-term employment for the control group. In this case, the small negative effect of the EPLP increase in 2004 would again be overestimated in absolute terms. To summarize, the two reforms would result in an underestimation of the asymmetry of the effects. Therefore, our

main conclusion concerning the asymmetry is still valid even if parallel reforms were relevant.

Second, EPLP reforms might have negligible effects in economic downturns as firms might stop to hire new workers. Although the second reform in 2004 took place in a period of an economic downturn, firms hired in both reform periods workers. Firms with 6-10 (11-12) FTEs hired, on average, 0.6 (0.7) persons in the first half of each year between 1997-2001 and 0.9 (1.07) persons between 2002-2006.<sup>29</sup> Thus, the economic conditions on the establishment is considered to be not problematic in our case.

## Robustness

### Larger control group size

We diverge now from the DID in a regression discontinuity design, by increasing the neighborhood for the control group. Establishments are defined to be in the control group if they employ 11 to 15 FTEs rather than 11 to 12 FTEs. The signs of the policy effects on the share of fixed-term employment, however, are as expected, and the asymmetric effects in terms of statistical and economic significance are confirmed. This is, specifically, the case in the dynamic specification (Table 2.8). After the increase in EPLP in 1999, establishments employed significantly 2.08 percentage points more fixed-term contract workers, while the decrease in EPLP in 2004 did not result in a significant decrease. Furthermore, the policy effect on the number and share of fixed-term workers of the increase in EPLP is economically more relevant (69% versus 10% and 44% versus 12%), which is also true for the standard DID specification of the share of fixed-term workers (19% versus 2% of the mean).

In the standard DID specification, no policy effect on the share and number of fixed-term contracts for the 1999 reform is significant (Table 2.7). This, however, is consistent with a negative pre-reform trend difference in the share and the number of fixed-term contract workers (Table 2.8). The trend in the share of fixed-term contract workers in the control group seems to be more strongly positive, which is supported by a stronger unconditional trend for establishments with 13 to 15

<sup>29</sup>Source: Own calculation based on IAB EP. Annual data are not available.

Table 2.7: EPLP effect on fixed-term work: larger control group

	(1)	(2)	(3)	(4)	(5)
	<i>ShFTC</i>	<i>E</i>	<i>FTC</i>	<i>Any FTC</i>	<i>ShFTC</i> if $> 0$
<b>Reform 1999</b>					
Reform x Treat	0.00572	-0.000549	-0.0529	0.0241	-0.0108
	(0.90)	(-0.01)	(-0.56)	(0.98)	(-0.33)
<i>N</i>	7907	7907	7907	7907	1302
<i>R</i> <sup>2</sup>	0.025	0.536	0.026	0.023	0.148
<i>Mean y<sub>it</sub></i>	0.03	10.01	0.33	0.14	0.23
<b>Reform 2004</b>					
Reform x Treat	0.000602	0.130*	-0.0159	-0.0141	0.0101
	(0.15)	(1.94)	(-0.24)	(-0.87)	(0.63)
<i>R</i> <sup>2</sup>	0.011	0.452	0.024	0.011	0.036
<i>N</i>	13057	13057	13057	13057	2707
<i>R</i> <sup>2</sup>	0.011	0.452	0.024	0.011	0.036
<i>Mean y<sub>it</sub></i>	0.04	10.00	0.46	0.18	0.21

Note: Fixed effect estimators with clustered standard errors; Dependent variables: share of fixed-term workers (*ShFTC*), establishment employs any fixed-term worker (*Any FTC*), *ShFTC* in establishments which employ at least one fixed-term worker (*ShFTC* if  $> 0$ ), number of employees (*E*), number of fixed-term employees (*FTC*); Controls: TG dummy, reform dummies, share of low qualified, high qualified, part-time workers, female workers and trainees, branch and federal state dummies; Establishments from 1997-2001 or 2002-2006 of non-public sectors with 6-15 *FTE<sub>it</sub>*s (*TG<sub>it</sub>*) excluding outliers (share of trainees) and no missing values in model relevant variables (estimation sample); *t* statistics in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 2.8: EPLP effect on fixed-term work: larger control group

	(1)	(2)	(3)	(4)	(5)
	<i>ShFTC</i>	<i>E</i>	<i>FTC</i>	<i>Any FTC</i>	<i>ShFTC</i> if > 0
<b>Reform 1999</b>					
Reform[t-1] x Treat	-0.0151 (-1.57)	0.131 (1.06)	-0.359** (-2.46)	-0.0330 (-0.94)	-0.0117 (-0.25)
Reform x Treat	0.0208** (2.44)	-0.0162 (-0.14)	0.144 (1.23)	0.0695** (2.11)	-0.00524 (-0.14)
Reform[t+1] x Treat	-0.0154** (-2.30)	-0.0672 (-0.65)	-0.0840 (-0.84)	-0.0553** (-2.02)	-0.00409 (-0.14)
<i>N</i>	7907	7907	7907	7907	1302
<i>R</i> <sup>2</sup>	0.028	0.536	0.029	0.024	0.148
<i>Mean y<sub>it</sub></i>	0.03	10.01	0.33	0.14	0.23
<b>Reform 2004</b>					
Reform[t-1] x Treat	0.00311 (0.57)	0.0405 (0.46)	-0.0298 (-0.38)	0.00403 (0.19)	0.00248 (0.12)
Reform x Treat	-0.00385 (-0.78)	0.109 (1.48)	-0.0555 (-0.68)	-0.0229 (-1.10)	-0.00268 (-0.14)
Reform[t+1] x Treat	0.00546 (1.20)	0.00823 (0.12)	0.0880 (1.26)	0.0122 (0.63)	0.0206 (1.29)
<i>N</i>	13057	13057	13057	13057	2707
<i>R</i> <sup>2</sup>	0.011	0.452	0.025	0.011	0.038
<i>Mean y<sub>it</sub></i>	0.04	10.00	0.46	0.18	0.21

Note: Fixed effect estimators with clustered standard errors; Dependent variables: share of fixed-term workers (*ShFTC*), establishment employs any fixed-term worker (*Any FTC*), *ShFTC* in establishments which employ at least one fixed-term worker (*ShFTC* if > 0), number of employees (*E*), number of fixed-term employees (*FTC*); Controls: TG dummy, reform dummies, share of low qualified, high qualified, part-time workers, female workers and trainees, branch and federal state dummies; Establishments from 1997-2001 or 2002-2006 of non-public sectors with 6-15 *FTE<sub>it</sub>*s (*TG<sub>it</sub>*) excluding outliers (share of trainees) and no missing values in model relevant variables (estimation sample); *t* statistics in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

FTEs compared to 11 to 12 FTEs. These results show that a small neighborhood is an important feature in our case with establishment-level data.

### **Time invariant $TG_i$**

In order to check whether dynamics endogenous to the reforms play a fundamental role, the time invariant definition of the treatment group dummy ( $TG_i$ ) is employed. Such a test is also conducted by, e.g., Centeno and Novo (2012).<sup>30</sup> For this purpose, the sample is restricted to establishments with 6-12 FTEs in the year prior to the reform. Overall, the direction of the effects of EPLP reforms is as expected, and the asymmetry is confirmed in the dynamic specification again. The share of fixed-term contract workers increased significantly by 2 percentage points after the increase in EPLP in 1999, which is 67% of the mean (Table 2.10) and statistically and economically stronger compared to the decrease in EPLP in 2004 (not significant and 33% of the mean). This picture is repeated for the number of fixed-term contract workers (114% of the mean (significant) versus 38% of the mean (not significant)). Hence, selection induced by the reforms might not play a fundamental role for the dynamic specification.

The effects in the standard DID specification (Table 2.9) are as expected in terms of the sign of the policy effects, but they do not confirm the asymmetry. This is due to a negative pre-reform effect in the sample for the 1999 reform (Table 2.10). Prior to the increase in EPLP in 1999, establishments which employed 6-10 FTEs in 1998 decreased the share in fixed-term workers in relation to the control group. Anticipation could explain this because, before the reform, establishments might have tried to stay below 11 FTEs in order to circumvent EPLP but then grow after expecting the reform to begin. Threshold effects on employment could, however, not be confirmed (Kölling et al., 2001; Verick, 2004; Bauer et al., 2007). Overall, we prefer the estimation sample with the time-variant treatment group dummy as the common trend assumption seems to be better met.

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<sup>30</sup>Please see Section 2.4.2.

Table 2.9: EPLP effect on fixed-term work: time invariant  $TG_i$

	(1)	(2)	(3)	(4)	(5)
	<i>ShFTC</i>	<i>E</i>	<i>FTC</i>	<i>Any FTC</i>	<i>ShFTC</i> if $> 0$
<b>Reform 1999</b>					
Reform x Treat	0.00361	0.829*	0.133	0.00705	-0.0193
	(0.43)	(1.74)	(0.80)	(0.24)	(-0.66)
<i>N</i>	3643	3643	3643	3643	552
<i>R</i> <sup>2</sup>	0.028	0.140	0.012	0.018	0.163
<i>Mean y<sub>it</sub></i>	0.03	9.93	0.25	0.11	0.24
<b>Reform 2004</b>					
Reform x Treat	-0.00879	0.721	-0.202*	-0.0181	-0.0138
	(-1.55)	(1.35)	(-1.66)	(-0.83)	(-0.61)
<i>N</i>	8623	8623	8623	8623	1595
<i>R</i> <sup>2</sup>	0.010	0.058	0.014	0.011	0.029
<i>Mean y<sub>it</sub></i>	0.03	10.32	0.36	0.16	0.20

Note: Fixed effect estimators with clustered standard errors; Dependent variables: share of fixed-term workers (*ShFTC*), establishment employs any fixed-term worker (*Any FTC*), *ShFTC* in establishments which employ at least one fixed-term worker (*ShFTC* if  $> 0$ ), number of employees (*E*), number of fixed-term employees (*FTC*); Controls: TG dummy, reform dummies, share of low qualified, high qualified, part-time workers, female workers and trainees, branch and federal state dummies; Sample is restricted to establishments observed in the year before the reform either in the control or in the treatment group; Time invariant  $TG_i$ : TG is kept constant based on the observation in the year before the reform; Establishments from 1997-2001 or 2002-2006 of non-public sectors excluding outliers (share of trainees) and no missing values in model relevant variables (estimation sample); *t* statistics in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 2.10: EPLP effect on fixed-term work: time invariant  $TG_i$ 

	(1)	(2)	(3)	(4)	(5)
	<i>ShFTC</i>	<i>E</i>	<i>FTC</i>	<i>Any FTC</i>	<i>ShFTC</i> if $> 0$
<b>Reform 1999</b>					
Reform[t-1] x Treat	-0.0189*	0.968	-0.0616	-0.0711*	-0.0389
	(-1.74)	(1.51)	(-0.17)	(-1.71)	(-0.71)
Reform x Treat	0.0200**	0.528	0.284*	0.0553	0.0234
	(2.03)	(1.42)	(1.90)	(1.39)	(0.57)
Reform[t+1] x Treat	-0.0153	-0.174	-0.222	-0.0336	-0.0668
	(-1.23)	(-0.32)	(-1.05)	(-0.92)	(-1.12)
<i>N</i>	3643	3643	3643	3643	552
<i>R</i> <sup>2</sup>	0.030	0.142	0.013	0.020	0.172
<i>Mean y<sub>it</sub></i>	0.03	9.93	0.25	0.11	0.24
<b>Reform 2004</b>					
Reform[t-1] x Treat	-0.00183	0.839	-0.188	0.0355	-0.0331
	(-0.28)	(1.13)	(-1.17)	(1.35)	(-1.19)
Reform x Treat	-0.0100	0.506	-0.138	-0.0401	-0.00436
	(-1.50)	(1.50)	(-1.27)	(-1.49)	(-0.20)
Reform[t+1] x Treat	0.00332	-0.214	0.0211	0.0128	0.00681
	(0.50)	(-1.03)	(0.22)	(0.52)	(0.27)
<i>N</i>	8623	8623	8623	8623	1595
<i>R</i> <sup>2</sup>	0.010	0.059	0.014	0.011	0.031
<i>Mean y<sub>it</sub></i>	0.03	10.32	0.36	0.16	0.20

Note: Fixed effect estimators with clustered standard errors; Dependent variables: share of fixed-term workers (*ShFTC*), establishment employs any fixed-term worker (*Any FTC*), *ShFTC* in establishments which employ at least one fixed-term worker (*ShFTC* if  $> 0$ ), number of employees (*E*), number of fixed-term employees (*FTC*); Controls: TG dummy, reform dummies, share of low qualified, high qualified, part-time workers, female workers and trainees, branch and federal state dummies; Sample is restricted to establishments observed in the year before the reform either in the control or in the treatment group; Time invariant  $TG_i$ : TG is kept constant based on the observation in the year before the reform; Establishments from 1997-2001 or 2002-2006 of non-public sectors excluding outliers (share of trainees) and no missing values in model relevant variables (estimation sample); *t* statistics in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## 2.6 Conclusion and discussion

This paper studies for the first time whether the impact of an almost perfectly symmetric increase and decrease in employment protection legislation for permanent workers (EPLP) has a symmetric effect on the share of fixed-term contract workers at the establishment-level. The particular structure of reforms in German EPLP offer the unique opportunity to evaluate an increase and a decrease in EPLP in a quasi-experimental approach. Reforms in Germany increased EPLP in 1999 and then decreased EPLP in 2004 for small establishments almost perfectly symmetric, while large establishments were not affected. Therefore, a difference-in-difference approach in a regression discontinuity design based on within-country time and subgroup variation can be employed. We account for observable and time-invariant unobservable establishment characteristics. Pre-reform trend tests support the common trend assumption for our main results.

The main result is that the effect of the EPLP reforms on the share of fixed-term workers is symmetric in its sign but asymmetric with regard to its economical and statistical significance. The direction of the EPLP effects is as expected and is in line with Cahuc et al. (2012) and (Boeri, 2011). The share of fixed-term workers increased by 1.7 percentage points (58% of the mean) due to the increase in EPLP in 1999, while the decrease of EPLP in 2004 had no significant effect. In the dynamic specification, this asymmetric pattern is repeated. The increase in EPLP in 1999 increased the share of fixed-term contract workers by 3 percentage points 6 months after the reform (108% of the mean), while the decrease in EPLP in 2004 decreased the share by only 1.3% (32% of the mean). Concerning the 1999 reform, the effect decreased by 2 percentage points, which might be due to fixed-term contracts being used as a screening device. The effect on the share of fixed-term workers can be explained by changes in the number of fixed-term workers. The asymmetry of the increase and decrease in EPLP is relatively robust to different definitions of the treatment group dummy and to larger control groups.

The asymmetry might contribute to explain why empirical studies which assume symmetry do not find a robust relation between EPLP and temporary employment. Further research can be built upon these findings. The potential mechanism, for instance, remains subject to future research. Potential explanations

for the asymmetry could be that employing permanent workers is not beneficial compared to employing temporary workers, or that the reform sequence (decrease, increase, decrease) plays a role. Finally, the magnitude of the asymmetry requires further research, too. For instance, the magnitude might depend on the share of incumbents affected by an increase in EPLP.

This study provides new insights into the symmetry of EPLP effects. Up to now, there has been no paper which has investigated whether symmetric increases and decreases in EPLP have symmetric effects. There are studies employing within-country time and subgroup variation which already showed that an increase in EPLP has a substantial effect on the share of fixed-term employment at the establishment-level. For policy purposes, however, it is highly relevant whether reforms which increase EPLP have similar effects as reforms that decrease EPLP on the share of fixed-term employment because decreasing EPLP is often advocated as a tool to decrease temporary employment in labor markets. We showed, however, that the effect of an almost perfectly symmetric increase and decrease in EPLP was asymmetric in its magnitude. Hence, reforms which revoke previous increases in EPLP do not necessarily have similarly strong effects on the share of fixed-term work at the establishment-level. This should be considered by policy makers when liberalizing EPLP.

## 2.7 Appendix

### 2.7.1 Definition of fixed-term employment variables

Table 2.11: Definition of fixed-term employment variables

$y_{it}$	Definition	IAB EP Question
Number of employees ( $E$ )	$E$	$E$ : "How many persons, categorized according to the employment groups listed, were employed by this establishment/office on 30 June 1999 [...]? Employees liable to social security (Workers and employees, Trainees/ apprentices), Employees not liable to social security (Civil servants incl. candidates for civil service, Working Proprietors and unpaid family workers), Others (E.g. marginal part time workers, 630 DM job holders)"
Number of fixed-term employees ( $FTC$ )	$FTC$	$FTC$ : "Does the total number of employees mentioned in Question 47 also include [...] b. Fixed-term employment? [...] If so, please indicate the total number of fixed-term employment [...]"

### 2.7.2 Definition of full-time equivalents

The weighting key for part-time workers changed with the reform of German EPLP in October 1996 and January 1999. Table 2.12 shows that the part-time worker weights after the reform in 1999 (FTE1999) are smaller compared to those after the reform in 1996 (FTE1996). Hence, establishments' FTEs decreased after the reform even when regular part-time and full-time workers remained the same. Therefore, the time-variant treatment group dummy ( $TG_{it}$ ) for the 1999 reform is defined to be 1 if an establishment employs between 6 FTE1999 and 10 FTE1996 and 0 if an establishment employs 11 or 12 FTE1996. If we would employ FTE1999 for the threshold at 10 FTEs, the control group would include establishments which actually faced a change in EPLP.

Table 2.12: Full-time equivalent workers (FTEs)

	German EPLP	Chosen weights
FTE1996 (since 1.10.1996)	Weights: WH < 11 – > 0,25 WH < 20 – > 0,5 WH < 30 – > 0,75 WH > 29 – > 1	Weights: part-time workers – > 0,5 full-time workers – > 1
FTE1999 (since 1.1.1999)	Weights: WH < 11 – > 0 WH < 20 – > 0,5 WH < 30 – > 0,75 WH > 29 – > 1	Weights: part-time workers – > 0,436 full-time workers – > 1

Note: WH is working hours.

### 2.7.3 Full models

Table 2.13: EPLP effect on fixed-term work (Reform 1999)

	(1)	(2)	(3)	(4)	(5)
	<i>ShFTC</i>	<i>E</i>	<i>FTC</i>	<i>Any FTC</i>	<i>ShFTC</i> if > 0
Reform x Treat	0.0173** (2.00)	0.0595 (0.53)	0.169 (1.44)	0.0400 (1.21)	0.0811 (1.62)
TG	-0.0121 (-1.30)	-2.902*** (-25.71)	-0.312*** (-2.75)	-0.0560* (-1.83)	-0.00647 (-0.18)
Low Qualified (%)	0.00902 (0.25)	3.905*** (6.46)	0.112 (0.29)	0.134 (1.10)	0.126 (0.75)
High Qualified (%)	0.0230 (0.76)	3.679*** (6.22)	0.291 (0.92)	0.150 (1.31)	0.179 (1.16)
Part-Time (%)	0.0107 (0.73)	5.017*** (14.66)	0.529 (1.53)	0.0811 (1.35)	-0.00976 (-0.15)
Women (%)	0.00659 (0.33)	-0.271 (-1.06)	0.110 (0.55)	-0.0119 (-0.21)	-0.0731 (-0.40)
Trainees (%)	-0.0816 (-1.53)	11.50*** (14.27)	-0.415 (-0.85)	-0.107 (-0.64)	-0.152 (-0.48)
Public Trainees (%)	0.0500 (1.10)	2.455** (2.16)	0.948* (1.79)	0.147 (0.55)	
Year	-0.000218 (-0.10)	-0.0539* (-1.90)	-0.00844 (-0.28)	-0.00151 (-0.18)	-0.00391 (-0.32)
1997 (reference)					
1998	0.0134*** (2.93)	0.0817 (1.41)	0.137*** (2.66)	0.0527*** (2.97)	0.0553** (2.42)
1999	0.00835* (1.68)	0.0219 (0.33)	0.0841 (1.24)	0.0311* (1.67)	-0.00235 (-0.08)
2000	0.000216 (0.06)	0.0321 (0.72)	-0.00604 (-0.15)	-0.00823 (-0.63)	-0.00310 (-0.18)
Constant	0.311 (0.07)	116.5** (2.05)	16.11 (0.27)	2.901 (0.17)	7.876 (0.32)
Branch Dummies	yes	yes	yes	yes	yes
State Dummies	yes	yes	yes	yes	yes
<i>N</i>	6190	6190	6190	6190	915
<i>R</i> <sup>2</sup>	0.031	0.487	0.027	0.021	0.092

Note: Fixed effect estimators with clustered standard errors; Dependent variables: share of fixed-term workers (*ShFTC*), establishment employs any fixed-term worker (*Any FTC*), *ShFTC* in establishments which employ at least one fixed-term worker (*ShFTC* if > 0), number of employees (*E*), number of fixed-term employees (*FTC*); Controls: TG dummy, reform dummies, share of low qualified, high qualified, part-time workers, female workers and trainees, branch and federal state dummies; Establishments from 1997-2001 or 2002-2006 of non-public sectors with 6-12 *FTE<sub>it</sub>*s, *TG<sub>it</sub>* and excluding outliers (share of trainees) and no missing values in model relevant variables (estimation sample); *t* statistics in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 2.14: EPLP effect on fixed-term work (Reform 2004)

	(1)	(2)	(3)	(4)	(5)
	<i>ShFTC</i>	<i>E</i>	<i>FTC</i>	<i>Any FTC</i>	<i>ShFTC</i> if > 0
Reform x Treat	-0.00447 (-0.79)	0.133* (1.95)	-0.160* (-1.93)	-0.00103 (-0.04)	-0.0185 (-0.78)
TG	-0.00206 (-0.41)	-2.487*** (-32.89)	-0.157** (-2.35)	-0.0243 (-1.14)	-0.0147 (-0.71)
Low Qualified (%)	0.0860*** (3.18)	5.213*** (10.85)	0.994*** (3.01)	0.247*** (2.60)	0.188* (1.85)
High Qualified (%)	0.0850*** (3.20)	4.796*** (10.16)	0.975*** (3.13)	0.199** (2.15)	0.191* (1.82)
Part-Time (%)	0.0149 (1.23)	4.402*** (17.64)	0.810*** (3.22)	0.0524 (1.17)	0.0144 (0.37)
Women (%)	0.0182 (1.47)	-0.196 (-0.98)	0.212 (1.43)	0.0210 (0.46)	0.0290 (0.48)
Trainees (%)	0.0203 (0.57)	13.19*** (22.44)	0.723* (1.81)	0.0445 (0.30)	-0.168 (-1.10)
Public Trainees (%)	0.0513 (0.89)	13.97*** (7.44)	1.144** (2.15)	-0.104 (-0.32)	-0.170 (-1.07)
Year	0.00109 (0.70)	-0.0714*** (-3.91)	0.0343 (1.55)	0.000839 (0.14)	0.00175 (0.28)
2002 (reference)					
2003	0.000768 (0.28)	-0.0587* (-1.68)	-0.00184 (-0.06)	0.0222** (2.07)	-0.0152 (-1.28)
2004	0.00538 (1.41)	-0.0790* (-1.79)	0.138** (2.47)	0.0277* (1.92)	0.00574 (0.36)
2005	0.00619** (2.30)	-0.0952*** (-2.82)	0.0752** (2.14)	0.0331*** (2.98)	-0.00939 (-0.80)
Constant	-2.263 (-0.73)	149.1*** (4.07)	-69.59 (-1.57)	-1.743 (-0.14)	-3.571 (-0.29)
Branch Dummies	yes	yes	yes	yes	yes
State Dummies	yes	yes	yes	yes	yes
<i>N</i>	10490	10490	10490	10490	2037
<i>R</i> <sup>2</sup>	0.014	0.411	0.023	0.013	0.037

Note: Fixed effect estimators with clustered standard errors; Dependent variables: share of fixed-term workers (*ShFTC*), establishment employs any fixed-term worker (*Any FTC*), *ShFTC* in establishments which employ at least one fixed-term worker (*ShFTC* if > 0), number of employees (*E*), number of fixed-term employees (*FTC*); Controls: TG dummy, reform dummies, share of low qualified, high qualified, part-time workers, female workers and trainees, branch and federal state dummies; Establishments from 1997-2001 or 2002-2006 of non-public sectors with 6-12 *FTE<sub>it</sub>*, *TG<sub>it</sub>* and excluding outliers (share of trainees) and no missing values in model relevant variables (estimation sample); *t* statistics in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 2.15: EPLP effect on fixed-term work (Reform 1999): dynamic specification

	(1)	(2)	(3)	(4)	(5)
	<i>ShFTC</i>	<i>E</i>	<i>FTC</i>	<i>Any FTC</i>	<i>ShFTC if &gt; 0</i>
Reform[t-1] x Treat	-0.00313 (-0.25)	0.00816 (0.06)	-0.141 (-1.01)	-0.0404 (-0.80)	0.0437 (0.86)
Reform x Treat	0.0325** (2.48)	-0.0151 (-0.11)	0.304* (1.66)	0.0948** (2.10)	0.0958 (1.32)
Reform[t+1] x Treat	-0.0230*** (-2.79)	0.118 (0.97)	-0.128 (-0.92)	-0.0630* (-1.72)	-0.0488 (-1.01)
TG	-0.00888 (-0.87)	-2.914*** (-20.05)	-0.222* (-1.79)	-0.0288 (-0.69)	-0.0396 (-0.80)
Low Qualified (%)	0.00909 (0.25)	3.904*** (6.46)	0.108 (0.28)	0.133 (1.09)	0.114 (0.63)
High Qualified (%)	0.0230 (0.76)	3.679*** (6.21)	0.282 (0.90)	0.148 (1.29)	0.165 (1.01)
Part-Time (%)	0.0103 (0.70)	5.019*** (14.66)	0.532 (1.52)	0.0812 (1.35)	-0.0110 (-0.17)
Women (%)	0.00695 (0.34)	-0.274 (-1.07)	0.105 (0.53)	-0.0128 (-0.23)	-0.0640 (-0.36)
Trainee (%)	-0.0819 (-1.54)	11.50*** (14.27)	-0.428 (-0.88)	-0.111 (-0.66)	-0.133 (-0.42)
Public Trainee (%)	0.0364 (0.76)	2.526** (2.23)	0.890 (1.59)	0.115 (0.42)	
Year	0.00208 (0.89)	-0.0648* (-1.83)	0.0193 (0.61)	0.00864 (0.82)	-0.00586 (-0.57)
1997 (reference)					
1998	0.0137 (1.34)	0.0856 (0.78)	0.225* (1.85)	0.0758* (1.83)	0.0220 (0.57)
1999	-0.00630 (-0.90)	0.0988 (0.92)	0.0328 (0.25)	-0.00131 (-0.04)	-0.0463 (-0.98)
2000	0.00227 (0.62)	0.0225 (0.47)	0.0200 (0.52)	0.00116 (0.08)	-0.00604 (-0.35)
Constant	-4.289 (-0.91)	138.2* (1.95)	-39.34 (-0.62)	-17.39 (-0.82)	11.88 (0.58)
Branch Dummies	yes	yes	yes	yes	yes
State Dummies	yes	yes	yes	yes	yes
<i>N</i>	6190	6190	6190	6190	915
<i>R</i> <sup>2</sup>	0.033	0.487	0.028	0.022	0.098

Note: Fixed effect estimators with clustered standard errors; Dependent variables: share of fixed-term workers (*ShFTC*), establishment employs any fixed-term worker (*Any FTC*), *ShFTC* in establishments which employ at least one fixed-term worker (*ShFTC if > 0*), number of employees (*E*), number of fixed-term employees (*FTC*); Controls: TG dummy, reform dummies, share of low qualified, high qualified, part-time workers, female workers and trainees, branch and federal state dummies; Establishments from 1997-2001 or 2002-2006 of non-public sectors with 6-12 *FTE<sub>it</sub>s*, *TG<sub>it</sub>* and excluding outliers (share of trainees) and no missing values in model relevant variables (estimation sample); *t* statistics in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 2.16: EPLP effect on fixed-term work (Reform 2004): dynamic specification

	(1)	(2)	(3)	(4)	(5)
	<i>ShFTC</i>	<i>E</i>	<i>FTC</i>	<i>Any FTC</i>	<i>ShFTC</i> if > 0
Reform[t-1] x Treat	0.0108 (1.23)	0.104 (1.02)	0.115 (1.05)	0.0299 (0.93)	0.0496 (1.54)
Reform x Treat	-0.0128* (-1.66)	0.100 (1.29)	-0.258** (-2.26)	-0.0143 (-0.47)	-0.0448 (-1.40)
Reform[t+1] x Treat	0.00690 (1.03)	-0.0135 (-0.19)	0.0889 (0.89)	0.00249 (0.10)	0.0164 (0.57)
TG	-0.00845 (-1.06)	-2.547*** (-24.83)	-0.225** (-2.19)	-0.0416 (-1.46)	-0.0460 (-1.42)
Low Qualified (%)	0.0855*** (3.17)	5.209*** (10.84)	0.989*** (2.99)	0.246*** (2.59)	0.194* (1.90)
High Qualified (%)	0.0847*** (3.19)	4.793*** (10.15)	0.971*** (3.13)	0.198** (2.14)	0.195* (1.84)
Part-Time (%)	0.0145 (1.19)	4.402*** (17.64)	0.806*** (3.20)	0.0520 (1.16)	0.0141 (0.37)
Women (%)	0.0188 (1.51)	-0.193 (-0.97)	0.218 (1.48)	0.0221 (0.48)	0.0326 (0.54)
Trainees (%)	0.0205 (0.58)	13.19*** (22.43)	0.725* (1.81)	0.0443 (0.30)	-0.161 (-1.05)
Public Trainees (%)	0.0504 (0.87)	13.97*** (7.45)	1.133** (2.13)	-0.105 (-0.32)	-0.164 (-1.03)
Year	-0.000823 (-0.40)	-0.0831*** (-3.42)	0.0126 (0.44)	-0.00306 (-0.40)	-0.00620 (-0.78)
2002 (reference)					
2003	-0.00611 (-0.96)	-0.132* (-1.77)	-0.0739 (-0.91)	0.00176 (0.07)	-0.0463** (-1.98)
2004	0.00713 (1.05)	-0.114* (-1.68)	0.166 (1.56)	0.0219 (0.88)	0.00244 (0.08)
2005	0.00429 (1.43)	-0.107*** (-2.86)	0.0536 (1.37)	0.0292** (2.43)	-0.0172 (-1.37)
Constant	1.546 (0.37)	172.8*** (3.55)	-26.37 (-0.46)	5.869 (0.38)	12.45 (0.78)
Branch Dummies	yes	yes	yes	yes	yes
State Dummies	yes	yes	yes	yes	yes
<i>N</i>	10490	10490	10490	10490	2037
<i>R</i> <sup>2</sup>	0.014	0.411	0.024	0.013	0.041

Note: Fixed effect estimators with clustered standard errors; Dependent variables: share of fixed-term workers (*ShFTC*), establishment employs any fixed-term worker (*Any FTC*), *ShFTC* in establishments which employ at least one fixed-term worker (*ShFTC* if > 0), number of employees (*E*), number of fixed-term employees (*FTC*); Controls: TG dummy, reform dummies, share of low qualified, high qualified, part-time workers, female workers and trainees, branch and federal state dummies; Establishments from 1997-2001 or 2002-2006 of non-public sectors with 6-12 *FTE<sub>its</sub>*, *TG<sub>it</sub>* and excluding outliers (share of trainees) and no missing values in model relevant variables (estimation sample); *t* statistics in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

### 2.7.4 Robustness: outliers and temporary agency workers

Table 2.17: EPLP effect on fixed-term work: outliers in share of trainees included

	(1)	(2)	(3)	(4)	(5)
	<i>ShFTC</i>	<i>E</i>	<i>FTC</i>	<i>Any FTC</i>	<i>ShFTC</i> if > 0
<b>Reform 1999</b>					
<b>Standard Specification</b>					
Reform x Treat	0.0155*	0.221	0.107	0.0407	0.0785
	(1.87)	(0.32)	(0.88)	(1.28)	(1.60)
$R^2$	0.031	0.077	0.024	0.019	0.138
<b>Dynamic Specification</b>					
Reform[t-1] x Treat	-0.00871	-1.220	-0.302*	-0.0614	0.00635
	(-0.69)	(-1.41)	(-1.70)	(-1.26)	(0.08)
Reform x Treat	0.0298**	-0.400	0.248	0.0920**	0.0904
	(2.38)	(-0.64)	(1.40)	(2.11)	(1.35)
Reform[t+1] x Treat	-0.0175**	1.865	-0.0227	-0.0418	-0.0249
	(-2.23)	(1.19)	(-0.16)	(-1.18)	(-0.58)
$R^2$	0.033	0.078	0.025	0.020	0.139
<i>N</i>	6559	6559	6559	6559	988
<b>Reform 2004</b>					
<b>Standard Specification</b>					
Reform x Treat	-0.00641	0.401	-0.185**	-0.0113	-0.0182
	(-1.19)	(0.80)	(-2.32)	(-0.51)	(-0.85)
$R^2$	0.013	0.061	0.023	0.012	0.031
<b>Dynamic Specification</b>					
Reform[t-1] x Treat	0.00832	0.540	0.0917	0.0187	0.0441
	(1.01)	(0.89)	(0.89)	(0.61)	(1.48)
Reform x Treat	-0.0132*	0.668**	-0.257**	-0.0135	-0.0455
	(-1.78)	(2.07)	(-2.35)	(-0.46)	(-1.55)
Reform[t+1] x Treat	0.00584	-0.815	0.0617	-0.00893	0.0187
	(0.93)	(-1.62)	(0.65)	(-0.36)	(0.71)
$R^2$	0.013	0.062	0.023	0.012	0.035
<i>N</i>	11128	11128	11128	11128	2176

Note: Fixed effect estimators with clustered standard errors; Dependent variables: share of fixed-term workers (*ShFTC*), establishment employs any fixed-term worker (*Any FTC*), *ShFTC* in establishments which employ at least one fixed-term worker (*ShFTC* if > 0), number of employees (*E*), number of fixed-term employees (*FTC*); Controls: TG dummy, reform dummies, share of low qualified, high qualified, part-time workers, female workers and trainees, branch and federal state dummies; Establishments from 1997-2001 or 2002-2006 of non-public sectors with 6-15 *FTE<sub>it</sub>* (*TG<sub>it</sub>*) excluding outliers (share of trainees) and no missing values in model relevant variables (estimation sample); *t* statistics in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 2.18: EPLP effect on fixed-term work: controlling for temporary agency work (TAW)

	(1)	(2)	(3)	(4)	(5)
	<i>ShFTC</i>	<i>E</i>	<i>FTC</i>	<i>Any FTC</i>	<i>ShFTC</i> if > 0
<b>Reform 2004: not controlling for TAW</b>					
<b>Standard Specification</b>					
Reform x Treat	-0.00487	0.126*	-0.167**	-0.00380	-0.0219
	(-0.86)	(1.84)	(-2.00)	(-0.16)	(-0.94)
$R^2$	0.011	0.409	0.023	0.012	0.038
<b>Dynamic Specification</b>					
Reform[t-1] x Treat	-0.00871	-1.220	-0.302*	-0.0614	0.00635
	(-0.69)	(-1.41)	(-1.70)	(-1.26)	(0.08)
Reform x Treat	0.0298**	-0.400	0.248	0.0920**	0.0904
	(2.38)	(-0.64)	(1.40)	(2.11)	(1.35)
Reform[t+1] x Treat	-0.0175**	1.865	-0.0227	-0.0418	-0.0249
	(-2.23)	(1.19)	(-0.16)	(-1.18)	(-0.58)
$R^2$	0.033	0.078	0.025	0.020	0.139
$N$	10399	10399	10399	10399	2019
<b>Reform 2004: controlling for TAW</b>					
<b>Standard Specification</b>					
Reform x Treat	-0.00507	0.127*	-0.170**	-0.00466	-0.0218
	(-0.89)	(1.85)	(-2.03)	(-0.20)	(-0.93)
TAW	0.00496*	-0.0112	0.0670**	0.0215**	-0.000564
	(1.92)	(-0.42)	(1.97)	(2.38)	(-0.09)
$R^2$	0.011	0.409	0.024	0.013	0.038
<b>Dynamic Specification</b>					
Reform[t-1] x Treat	0.0108	0.0976	0.115	0.0276	0.0528*
	(1.23)	(0.96)	(1.04)	(0.86)	(1.66)
Reform x Treat	-0.0129	0.0954	-0.263**	-0.0152	-0.0475
	(-1.64)	(1.22)	(-2.26)	(-0.49)	(-1.47)
Reform[t+1] x Treat	0.00633	-0.0132	0.0858	0.000765	0.0130
	(0.92)	(-0.18)	(0.83)	(0.03)	(0.44)
TAW	0.00487*	-0.0114	0.0658*	0.0214**	0.000139
	(1.90)	(-0.43)	(1.94)	(2.36)	(0.02)
$R^2$	0.011	0.409	0.023	0.012	0.042
$N$	10399	10399	10399	10399	2019

Note: Fixed effect estimators with clustered standard errors; Dependent variables: share of fixed-term workers (*ShFTC*), establishment employs any fixed-term worker (*Any FTC*), *ShFTC* in establishments which employ at least one fixed-term worker (*ShFTC* if > 0), number of employees (*E*), number of fixed-term employees (*FTC*); Controls: TG dummy, reform dummies, share of low qualified, high qualified, part-time workers, female workers and trainees, branch and federal state dummies; Establishments from 1997-2001 or 2002-2006 of non-public sectors with 6-15  $FTE_{it}$ s ( $TG_{it}$ ) excluding outliers (share of trainees) and no missing values in model relevant variables (estimation sample);  $t$  statistics in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## Chapter 3

# The effect of shocks on temporary employment conditional on EPLP

### 3.1 Introduction

Temporary employment accounts for a considerable part of the EU27 workforce - around 14 per cent, 60 per cent of which is involuntary (Eurostat, 2012). Workers on temporary contracts are one of the most vulnerable groups to economic downturns (Boeri, 2011, p. 1207), which implies a large risk of unemployment incurring well-being losses (Clark et al., 2008; Lucas, 2007; Frey and Stutzer, 2002). Moreover, temporary employment has a direct negative impact on well-being compared to permanent workers, as it comes with fewer training opportunities, lower wages and lower job satisfaction (Booth et al., 2002; De Cuyper, De Jong, De Witte, Isaksson, Rigotti and Schalk, 2008). Hence, gaining deeper insights into the mechanisms that generate temporary employment is relevant for policy making.

This paper contributes to such insights by studying firms' demand for temporary workers induced by product demand shocks in different institutional contexts. Studying the firm-level is important for two reasons. First, firms' shocks in product demand are a main reason for the use of temporary workers: Houseman (2001) presents evidence on the motivation to employ temporary workers based on employer interviews; Morikawa (2010) shows a positive relation between firms'

volatility in sales growth on the share of non-standard employment; Eslava et al. (2014) present a positive relation between job destruction and construction on changes in the share of temporary employment at the plant-level. Second, firm-level product demand shocks are positively related to macro-economic business cycle volatilities (Buch et al., 2008). Hence, in the aftermath of the 2007 financial crisis, macro-economic volatilities increased in many European countries, which might have raised volatilities at the firm-level too. Thereby, the employment of temporary workers might have increased generally in its importance.

Firm-level shocks in product demand, however, do not determine the use of temporary employment in isolation. Research shows that labor market institutions, such as regulations of temporary work (e.g. Cappellari et al., 2012; Kahn, 2010; Boeri and Garibaldi, 2007; Blanchard and Landier, 2002) are an important determinant of temporary employment. Employment protection legislation for permanent workers also matters: Applying within-country subgroup and time variation, EPLP was shown to have a positive effect on employing temporary workers (Centeno and Novo, 2012; Boockmann and Hagen, 2001); Colombian within-country increase in EPLP had a similar effect (Eslava et al., 2014); focusing on within-country time variation, however, the results of cross-country studies are less clear (Kahn, 2010; Nunziata and Staffolani, 2007).

Furthermore, research has shown that real shocks interact with institutions in determining (temporary) employment. At the firm-level, Eslava et al. (2014) show that temporary workers are shock absorbers and that the share of temporary workers became more responsive to job construction (destruction) when non-wage labor costs for permanent workers increased in Colombia after institutional changes; focusing on employment rather than temporary employment, Bentolila and Saint-Paul (1992) find for Spanish firms that employment elasticities to firms' real shocks, which are measured via changes in sales, are higher after the liberalization of legislation for temporary workers. At the country-level, results are less clear. Nunziata and Staffolani (2007) show that the decrease in the aggregated share of temporary workers is to a statistically non-significant extent stronger in a macro-economic recession when employment protection for permanent workers is high.

Building upon this literature, this paper asks: Is the effect of shocks in product

demand on firms' decision to employ temporary workers stronger in countries that impose strict rules on the dismissal of permanent workers? In line with a recent search and matching model by Cahuc et al. (2012), we expect that firms are more likely to employ temporary workers, when they face workload shocks of short duration. The effect, however, depends on sufficiently high employment protection for permanent workers.

To the best of our knowledge, ours is the first study to investigate the impact of the duration of shocks on employing temporary workers conditional on employment protection empirically from a cross-country establishment-level perspective. Hence, our contribution to the literature is threefold: First, compared to Eslava et al. (2014), who employ firm-level data and are closest related to our study, we can add a broad cross-country perspective. Second, in comparison to Nunziata and Staffolani (2007), Eslava et al. (2014) and Bentolila and Saint-Paul (1992), we employ a different measure of shocks that is more closely related to the theoretical model by Cahuc et al. (2012). The model emphasizes the role of the duration of shocks. Our measure captures information on the duration. Thereby, we are the presenting the role of the duration of shocks for temporary employment and present their distribution in Europe for the first time. Third, compared to Nunziata and Staffolani (2007), who use macro-data, we add the micro-perspective by combining institutional data with establishment-level data, thus accounting for composition effects.

Our empirical strategy uses novel data from two waves of the European Company Survey (each wave with around 18,000 establishments) clustered in up to 20 European countries in combination with macro-data. We rely on cross-country variation in employment protection legislation (Boeri and Jimeno, 2005). Although the cross-sectional character clearly limits our analyzes, the broad international scope of our data represents an almost unique opportunity to analyze firms' hiring decisions in different institutional contexts. Acknowledging the non-negligible limitations of our empirical strategy, we discuss the issue of correlations versus effects in detail.

We estimate a binary choice model on the pooled data, with clustered standard errors and country dummies. Our main result is that establishments normally facing workload fluctuations within a year in flexible regimes are not more likely to

employ temporary workers compared to establishments without fluctuations. In countries with a sufficient high level of employment protection legislation, however, establishments are significantly more likely to employ temporary workers (70 per cent versus 78 per cent). This is also true for the subgroups of temporary agency and fixed-term contract workers. Our results are robust if we account for differential enforcement of employment protection. Furthermore, they are also robust in different country subsamples, years of observation, and model specifications.

We begin with our theoretical argument based upon labor demand and search and matching models (Section 3.2). From this, we derive our empirical model and discuss the empirical strategy in Section 3.3. After describing data sources and central concepts (Section 3.4), we present our results and discuss endogeneity as well as robustness issues in Section 3.5. In the final Section 3.6, we conclude.

## 3.2 Theoretical and empirical background

Our interest is in shocks interacted with employment protection for permanent workers as determinants on temporary employment. European labor markets are characterized by heterogeneous employment protection for permanent and temporary workers. Thereby, employers face different adjustment costs for temporary and permanent workers. Importantly, temporary contracts can be terminated at no (or low) costs if the contract ends after pre-specified period, while permanent contracts are costly in their termination. Protection for temporary and permanent workers are typically modeled as workforce adjustment costs in either dynamic labor demand models under uncertainty (e.g. Boeri and Garibaldi, 2007; Nunziata and Staffolani, 2007; Hamermesh, 1996; Bentolila and Saint-Paul, 1992)<sup>1</sup> or search and matching models (e.g. Bentolila, Cahuc, Dolado and Le Barbanchon, 2012; Blanchard and Landier, 2002; Cahuc and Postel-Vinay, 2002).

With these kind of models, the effect of two-tier labor market reforms, i.e. liberalization of temporary work, on economic outcomes such as average employment and unemployment levels (e.g. Blanchard and Landier, 2002) but also on the distribution of permanent and temporary jobs (e.g. Boeri and Garibaldi, 2007).

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<sup>1</sup>Labor demand models with heterogeneous workers are often based upon the classical labor demand model developed by Bentolila and Bertola (1990).

As Berton and Garibaldi (2012) note, the literature on two-tier labor market reforms in rigid labor markets often assumes (or implies) that after reforms at the margin, firms rely on temporary employment exclusively when filling vacancies.<sup>2</sup> It is, however, more realistic to assume a continuing coexistence of permanent and temporary contracts. For employers, the choice between contract types entails a trade-off: Permanent contracts may exhibit a higher job-filling rate, but temporary contracts provide flexibility in case of productivity shocks (Berton and Garibaldi, 2012). Given that employers continue to hire permanent workers, the important question is what determines employers' choice between permanent and temporary employment contracts when filling vacancies.

Cahuc et al. (2012) and Eslava et al. (2014) explicitly model the choice between contract types. Cahuc et al. (2012, p. 2) point to the relevance of the "heterogeneity of expected duration of jobs" for the choice. In general, search and matching models or labor demand models include stochastic shocks modeled for instance as Geometric Brownian motion (Lotti and Viviano, 2012), but not heterogeneity in the duration of jobs. Intuitively, the choice of employment contracts is most likely based upon the durability of a product demand shock. Permanent contracts are associated with high firing costs, while temporary contracts can be terminated - after a pre-determined duration - at no cost. If dismissal protection imposes sufficiently high turnover costs on permanent workers and employers have jobs which are limited in time, temporary contracts are chosen. When employment protection is low for permanent workers, permanent contracts are always chosen - even for jobs with a low duration. Hence, firing costs and the probability of a worker becoming unproductive (the job's shock arrival rate) interact in determining the choice of employment contracts.

The above mentioned model on the use of temporary work points to the paramount importance of the duration of shocks in interaction with institutional features determining firing costs. Taking the view of the firm<sup>3</sup> (and leaving workers'

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<sup>2</sup>Theoretical model, however, do not necessarily imply that the stock of permanent contracts is completely crowded-out over time, because they allow for the conversion of temporary into permanent contracts (e.g. Nunziata and Staffolani, 2007; Blanchard and Landier, 2002).

<sup>3</sup>We use the words firms and establishments interchangeable. Thereby, we assume that firms with more than one establishment operate these establishments independently when it comes to employment decisions.

decisions aside)<sup>4</sup>, we can formulate the following hypothesis for the choice to employ temporary workers: Firms' propensity to offer temporary contracts increases with the existence of jobs which become unproductive with a higher shock arrival rate conditional upon sufficiently high adjustment costs for permanent workers.

Empirical research analyzing this specific interaction at the firm-level in a cross-country design does not exist, but empirical studies have already shown that shocks are important and that firing costs are relevant for the impact of shocks on the workforce. First, single-country firm-level studies on the use of temporary work show that shocks in product demand are important to determine firms' choice whether to (at least partially) hire on temporary contracts (Eslava et al., 2014; Morikawa, 2010; Houseman, 2001; Abraham and Taylor, 1996) or to determine the size of the workforce (Bentolila and Saint-Paul, 1992). This is in line with Cahuc et al. (2012), as workers become unproductive with production opportunities of different lengths.

Second, in the vein of Cahuc et al. (2012), some studies support that adjustment costs are relevant for the effect of firm-level shocks or cyclical elements on employment. Eslava et al. (2014) show that when Colombian firms create (destroy) jobs the share of temporary workers increases (decreases) and that this relation is stronger when firing costs for permanent workers increased after 2001. Bentolila and Saint-Paul (1992) find for Spain in the 1980s that firm-level cyclical elasticity to sales increased with the availability of temporary contracts. At the aggregated level, Nunziata and Staffolani (2007) show that temporary employment rates vary more strongly over the business cycle than permanent employment rates and that this cyclical response is even stronger when temporary agency workers are well protected. The cyclical elasticity of temporary employment, however, does only change to a minor extent with protection for permanent workers at the aggregated level. While these papers are strongly related to ours, we add a broad cross-country perspective compared to Eslava et al. (2014), the measurement of duration of shocks in comparison to all three papers, and the firm-level compared to Nunziata and Staffolani (2007).

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<sup>4</sup>As most temporary contracts are involuntary, we expect firms to be the more powerful actor in the bargaining process, and hence we focus on their behavior. In the EU27, 60.4 per cent of temporary workers preferred a permanent job over a temporary job in 2009 (Eurostat, 2012).

In order to assess our hypotheses, we require variation in employment protection legislation for permanent workers as well as the duration of shocks at the firm-level.<sup>5</sup> This has two implications for the empirical research on the choice of employing temporary workers. First, to obtain variation in firing costs, cross-country or within-country variation can be exploited (Boeri and Jimeno, 2005). There is major cross-country variation in employment protection even within Europe. In comparison, within-country time variance is relatively small, because employment protection legislation is historically grown, and specifically for permanent workers, it has been quite stable in Europe. Furthermore, subgroup variation exists and comes from the variable enforcement of employment protection for permanent workers across firm size, for instance, in Germany and Italy. There is an important literature on employing within-country time and subgroup variation (e.g. Leonardi and Pica, 2013). Second, aggregated (e.g. national) data disguises heterogeneity in shock arrival rates across sectors and firms. Hence, we meet these requirements by employing a relatively new data set of European firms (protection varies across countries, shocks across firms) with the limitation of not having job-specific but firm-specific shocks.

### 3.3 Empirical specification

Our hypothesis is that firms' propensity to offer temporary contracts is high when the job-specific shock is of a relatively short duration and employment protection for permanent workers is sufficiently high. To link our theoretical argument to an empirical model, we make simplifying arguments that are partly driven by pragmatic reasons and data availability (see Section 3.4). The propensity to offer temporary contracts is ideally measured with flow data - the composition of hiring - and the duration of a productive job refers to job-specific characteristics - aspects of jobs that can differ within firms. For this, we would need linked employer-employee data, although such indicators are difficult to obtain in a comparative

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<sup>5</sup>Theoretically, the shock arrival rate is specific to jobs not to firms. This makes sense, since temporary and permanent work coexist in many firms. As we argue below, however, characteristics determining choice of employment contract are easier to observe at the firm-level than at the level of specific jobs. Estimating job-specific shock arrival rates would require comparable linked employer-employee data.

framework. Hence, we use the binary variable concerning whether establishments employ at least one temporary worker in our main analyzes and for additional analyzes the composition of the stock of employees by contract type. We utilize workload fluctuations of different duration at the firm-level. Finally, adjustment costs are partly determined by employment protection legislation for permanent workers at the national level. This allows us to rephrase our hypothesis as follows: Firms' likelihood of having temporary workers in their workforce is higher (*ceteris paribus*) if the firm is exposed to workload fluctuations of short duration and this is only the case if the costs for dismissal of permanent contracts (as stipulated by law or collective agreement) are sufficiently high.

In our baseline specification, we assume that the profit of firm  $i$  in country  $j$  employing at least one temporary worker ( $y_{ij}$ ) can be characterized by a latent variable ( $y_{ij}^*$ ):

$$y_{ij}^* = \beta_0 + \beta_1 EPLP_j * WF_{ij} + \beta_2 WF_{ij} + \beta_3' \mathbf{C} + R_{ij} + U_j \quad (3.1)$$

with

$$y_{ij} = 1[y_{ij}^* > c] \quad (3.2)$$

$$y_{ij} = 0 \text{ otherwise} \quad (3.3)$$

with employment protection legislation for permanent workers  $EPLP_j$ , short-term workload fluctuation  $WF_{ij}$ , a vector of controls  $\mathbf{C}$ , and the error term components  $R_{ij}$  and  $U_j$ .

There is at least one temporary worker in the workforce of a firm ( $y_{ij}$ ), when the profit of employing the worker exceeds the threshold  $c$ . We also replace the dependent variable by fixed-term contract and temporary agency workers, to which our theoretical argument similarly applies. Finally, we extend the analysis by employing the share of fixed-term contract workers at the date of the interview as a dependent variable. For this purpose, we estimated a two-component model accounting for corner solutions and different processes for the intensive and extensive margin (Eslava et al., 2014; Cameron and Trivedi, 2009, pp. 538).

The main variable of interest is the effect of  $WF_{ij}$  in different institutional contexts (employment protection legislation for permanent workers) on the propensity

that a firm employs temporary workers. For this, we require variation on the institutional level. As already mentioned above, there are two options: within and between country variance (Boeri and Jimeno, 2005). Due to data limitations, we are mainly restricted to variation of EPLP across countries rather than within countries.

Concerning our estimation strategy, due to cross-sectional data (see Section 3.4), we are unable to control for firm fixed-effects. Any national unobserved difference, however, in the propensity to employ temporary workers is dealt with by including country fixed-effects in some models ( $U_j$ ). Furthermore, firms are clustered within countries, and hence, the firm-specific error terms might be correlated within countries. We correct for this by estimating cluster-robust standard errors at the country-level in each model (Cameron and Miller, 2015). Thereby, we follow Kahn (2007) who employs the similar structure of data while investigating the effect of EPL on temporary employment. He pools cross-sectional individual data from seven countries.<sup>6</sup> Furthermore, we estimate a battery of robustness checks for different subsamples and specifications. Overall, the results are quite robust, which is specifically true for annual fluctuations.

The vector  $\mathbf{C}$  includes several controls at the firm-level, such as firm size and industry dummies as well as country-level variables and country dummies, depending on the specification. First, for the firm-level, various strands of literature argue that workplace representation may have an impact on the use of temporary jobs (Salvatori, 2012; Böheim and Zweimüller, 2012; Bentolila and Dolado, 1994). Empirical results, however, are ambiguous, and it is theoretically unclear in which direction the effect of workplace representation proceeds. Given that works councils are not at the core of our argument, we refrain from making an explicit theoretical claim, but include a control dummy variable measuring whether there is workplace representation in the establishment. Second, Houseman (2001) found in a company survey that temporary workers are employed to fill positions of absent

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<sup>6</sup>In order to deal with clustered data, Cameron and Miller (2015) propose OLS with cluster-robust standard errors, feasible generalized least squares or hierarchical models which, e.g., allow for random slopes. Hierarchical models are often referred to as multilevel models which are extensively applied in social science. For a methodological background, see Rabe-Hesketh and Skrondal (2012) or Snijders and Bosker (2012). We provide robustness checks in Appendix 3.7.6 but follow Kahn (2007) in the main analyses.

regular workers who are sick on family leave or vacation, or to screen employees for regular workers. Therefore, we control for the rates of absent workers and new workers (increased number of workers). Third, in empirical labor demand models, labor costs, costs of intermediate goods, capital stocks and performance indicators are usually controlled for (Bentolila and Saint-Paul, 1992). We include the change in the number of employees. For the others, we control indirectly by firm size and economic sector. Fourth, further controls are the rate of female and high-skilled employees in the workforce, since these groups differ in their likelihood of holding temporary contracts (Kahn, 2007). Finally, and similar to Kahn (2007) in his individual-level studies, we control for gender and skill level, as well as whether the firm makes use of flexible working time.

Depending on whether we included country dummies, we also include control variables at the country-level. First, it is argued in the literature that wage rigidity exacerbates the effect of employment protection legislation, since higher turnover costs cannot be compensated for by lower wage costs (Lazaer, 1990). Empirical studies on wage rigidity found that downward real wage rigidity depends on labor market institutions such as collective bargaining (e.g. Babecký, Du Caju, Kosma, Lawless, Messina and Røöm, 2010). Therefore, we include the proportion of eligible workers covered by collective agreements (collective bargaining coverage rate) in our model (single and in interaction with EPL for permanent and EPL for temporary workers as well as in interaction with annual workload fluctuations). Second, high EPLP often goes hand-in-hand with high EPL for temporary workers, and high EPL for temporary workers decreases the probability of being a temporary worker (Kahn, 2010). Therefore, we control for EPL for temporary workers and its interaction with annual workload fluctuation, as well as its interaction with EPLP. Finally, we control for the national unemployment rate to account for higher pressure for job seekers to accept temporary jobs (Polavieja, 2005).

### 3.4 Stylized facts and data sources

We utilize establishment-level data with around 18,000 establishments in up to 20 European countries for two years and merge them with country-level data. The European Company Survey (ECS) provides our data at the establishment-level

(Eurofound, 2010a; Gensicke, Hajek and Tschersich, 2009; Eurofound, 2006).<sup>7</sup> The ECS [former Establishment Survey on Working Time (ESWT)] started in 2004/2005 and is comparable across countries.<sup>8</sup> It is conducted every four years by the European Foundation for the Improvement of Living and Working Conditions (Eurofound). Our analysis mainly focuses on the most recent wave, with data collected in spring 2009, although we provide robustness analyzes with data collected starting in autumn 2004 and ending in spring 2005.<sup>9</sup> The 2004/2005 and the 2009 ECS are representative for establishments with more than ten employees.<sup>10</sup>

The original 2009 ECS comprises around 27,000 establishments from 30 European countries. A considerable number of countries had to be excluded due to missing data on either the micro-level or the institutional level.<sup>11</sup> The final sample comprises 20 European countries and 18,407 establishments.<sup>12</sup> The 2004/2005 ECS is employed for robustness checks across years. In the original 2004/2005 ECS, 21,031 establishments from 21 European countries participated.<sup>13</sup> The final sample comprises 17 countries and 17,923 establishments.<sup>14</sup>

To the best of our knowledge, only one data source at the establishment-level exists that is comparable to the ECS in its broad coverage of European establishments in combination with the details on contract types. This is the purely

<sup>7</sup>The unit of observation in the European Company Survey (ECS) is the establishment. Establishments are local production sites and firms may consist of multiple local production sites.

<sup>8</sup>Comparability over country for the ECS 2009 is large regarding questionnaire translation and the fieldwork period but the sampling frame differs slightly with regard to large firms for Belgium, Denmark, Greece, France, Luxembourg, Hungary, Poland and Turkey (Eurofound, 2011).

<sup>9</sup>Data for 2013 were not available at the time of the empirical analyzes.

<sup>10</sup>The survey covers all relevant sectors (NACE Rev. 1.1), excluding NACE A (agriculture, hunting, forestry), NACE B (fishing), NACE P (private household with employed persons) and NACE Q (extra-territorial organizations and bodies). The latter two sectors are both of negligible size (Eurofound, 2010b, p. 3).

<sup>11</sup>Since the loss in countries and observations is considerable, we provide a detailed description of the original sample, as well as the reason for dropping countries and observations in Appendix 3.7.3.

<sup>12</sup>Austria, Belgium, the Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Ireland, Luxembourg, the Netherlands, Poland, Portugal, Slovakia, Slovenia, Sweden, Turkey and the United Kingdom.

<sup>13</sup>Austria, Belgium, Cyprus, the Czech Republic, Denmark, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Latvia, Luxembourg, the Netherlands, Poland, Portugal, Slovenia, Spain, Sweden, the United Kingdom.

<sup>14</sup>The OECD EPL indicator for January 2004 was not available for Cyprus, Latvia, Luxembourg and Slovenia. Hence, we restricted the sample to the other 17 countries. Excluding observations with missing values in the relevant variables.

cross-sectional firm survey of the Wage Dynamics Network (Bertola et al., 2012). It covers 14 countries and 15,235 responses in total. In comparison to this survey, the advantage of the ECS is a larger sample of countries and of establishments. A second advantage is that the unit of observations is establishments. This provides us with a much disaggregated perspective and a broader sample. Other firm-level databases such as the AMADEUS also cover a broad sample and provide information on the stock of the overall workforce, although variables on the composition of the workforce are not available. One possible limitation of the ECS is that the sampling procedure excludes agriculture and forestry and these are sectors with major seasonal fluctuation, which is one of our main variables. We, however, do not believe that this biases our results, although we lose important observations and thus the estimates are less precise.

### 3.4.1 Establishment-level variables

The ECS asks separately whether temporary agency workers or fixed-term workers were employed within the last 12 months by the establishment, i.e. between spring 2008 and spring 2009 in the case of the 2009 sample and between autumn 2003 and spring 2005 in the case of the 2004/2005 sample. Temporary agency workers (TAWs) are workers who signed a contract with an employment agency. Establishments can hire these workers on a fixed-term basis as a third party. Next to TAWs, establishments can employ workers directly on a fixed-term (FTCs). In this case, the establishment has a contract with the worker. We code a dummy variable *Temp*, which is zero when the establishment employs neither FTC nor a TAW. The dummy variable *FTC (TAW)* is one, when the establishment employs at least one FTC (TAW) and zero otherwise.

Furthermore, the 2009 survey includes the proportion of employees holding a fixed-term contract in the respective establishment.<sup>15</sup> The question is: "About what proportion of your employees is holding a fixed-term contract?". We, however, do not expect the mediating role of EPL for permanent workers to be as clear as in the case of the variables *Temp*, *FTC* and *TAW*, for the following reasons. First, the variable on the share of FTCs refers to the date of the interview, but

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<sup>15</sup>Unfortunately, the share of FTCs is not available for the 2004/2005 sample.

our main explanatory variable (annual workload fluctuation) does not provide any information on the workload at the date of the interview. Second, our main explanatory variable is also binary and thus less suitable for predicting precise shares of temporary contracts in an establishment. Third, data was collected in spring 2009, during which time most countries experienced a severe economic crisis. The precise share of fixed-term contracts is arguably more sensitive to asymmetric adjustments of staff levels in the crisis than the binary variable. Fourth, the wording of the question from which our binary dependent variable is derived refers to the entire previous year rather than only the time of the interview. Therefore, it should be less affected by the crisis (Eurofound, 2010b, p. 2).

The descriptives for both variables are shown in Figure 3.1 and Table 3.1.<sup>16</sup> In our sample, around 61 per cent of the establishments use temporary contracts, although the value strongly differs across countries. It varies from 27 per cent in Slovakia to almost 85 per cent in the Netherlands (Figure 3.1). Around 53 per cent of establishments employ FTCs, although only 22 per cent use TAWs. Furthermore, TAWs have only a minor share in the total workforce - less than 3 per cent of total employment in Denmark, Finland, France, Germany, Netherlands, and Sweden around 2002 (Eurofound, 2007b). Hence, FTCs is what drives the results. The average share of fixed-term workers at the establishment-level (including establishments without any such contracts) is 10 per cent, ranging from 2.7 per cent in Austria to 19 per cent in Poland.

According to our theoretical argument, firms facing shocks of short duration in the productivity of jobs anticipate that some workers will become unproductive, and thus, hire (partly) on temporary contracts. In labor demand models shocks are usually modeled as stochastic processes (e.g. Geometric Brownian motion). One can distinguish between uncertainty of product demand and actual shocks in product demand. First, some empirical work or calibrations focus on the uncertain part of shocks such as Lotti and Viviano (2012) (squared difference of upper and lower bound of expected output) or Bentolila and Saint-Paul (1994) (variance of the stochastic process of the shock). Second, a major part of the empirical work proxies shocks in labor demand models by real shocks such as Bentolila and Saint-

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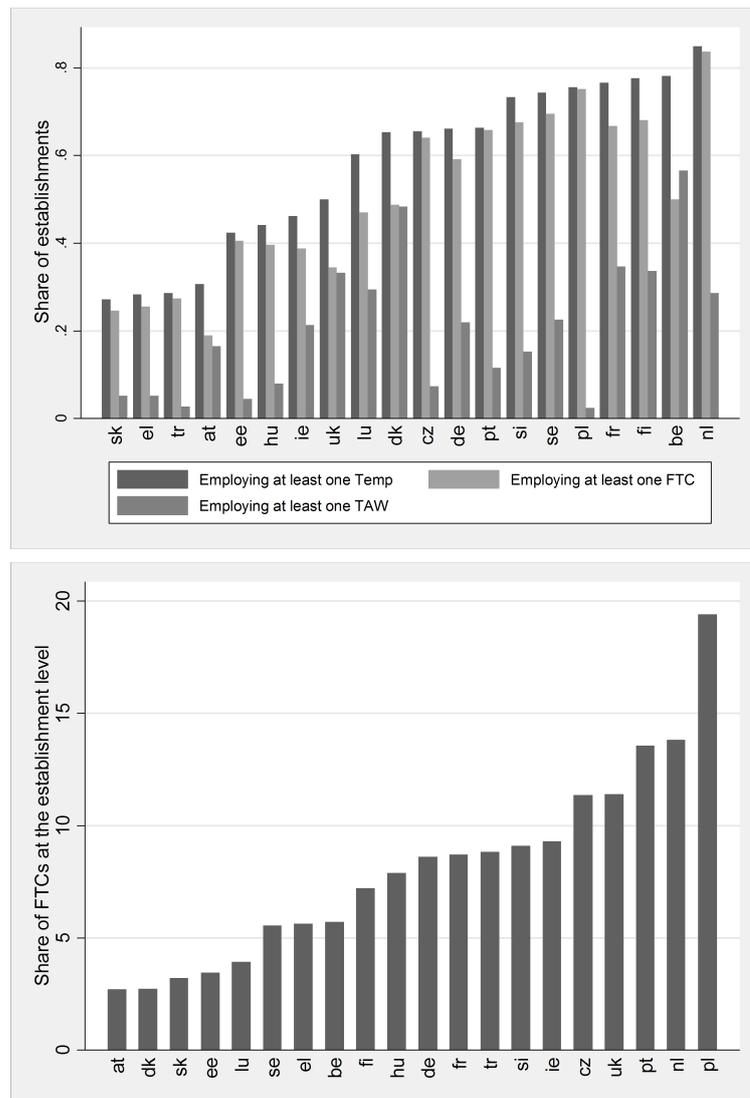
<sup>16</sup>Summary statistics of establishment-level variables of the original ECS 2009 sample are presented in Appendix 3.7.3.

Table 3.1: Summary statistics of establishment characteristics (2009)

Variable	Mean	SD	Min	Max	N
If any temp	0.61	0.49	0	1	18450
If any TAW	0.22	0.42	0	1	18450
If any FTC	0.53	0.50	0	1	18450
Share of FTC	9.80	21.63	0	100	18036
If any WF daily	0.32	0.47	0	1	18450
If any WF weekly	0.44	0.50	0	1	18450
If any WF annual	0.64	0.48	0	1	18450
If any freelancer	0.19	0.39	0	1	18407
If any works council	0.37	0.48	0	1	18450
No. of workers increased between 2006 and 2009	0.34	0.47	0	1	18450
No. of workers decreased between 2006 and 2009	0.22	0.42	0	1	18450
If high absenteeism and/or sickness rates (absent)	0.14	0.34	0	1	18450
Gender share (centered)	5.59	31.82	-41	59	18450
High-skilled share (centered)	0.94	29.70	-24	76	18450
If flexible working time schemes	0.57	0.50	0	1	18450
Establishment size (1-10)	1.90	1.51	1	10	18450
NACE C-E	0.19	0.39	0	1	18450
NACE F	0.07	0.26	0	1	18450
NACE G	0.19	0.40	0	1	18450
NACE H	0.04	0.21	0	1	18450
NACE I	0.05	0.21	0	1	18450
NACE J	0.03	0.16	0	1	18450
NACE K	0.13	0.34	0	1	18450
NACE L	0.05	0.22	0	1	18450
NACE N	0.09	0.29	0	1	18450
NACE O	0.06	0.24	0	1	18450

Note: Source is ECS 2009 (Eurofound, 2010b). Descriptive statistics with employer weights. Temporary workers (temp), temporary agency worker (TAW), fixed-term contract worker (FTC), workload fluctuation (WF), number (no.). Centered variables are centered based upon summary statistics of the sample without employer weights. High absenteeism means that an establishment encounters a human resource problem due to absenteeism and/or sickness. The share of high-skilled means the proportion of employees working in high-skilled jobs which usually require an academic degree. NACE Rev. 1.1: C-E Manufacturing and energy; F Construction; G Wholesale and retail trade, repair of goods; H Hotels and restaurants; I Transport and communication; J Financial intermediation; K Real estate and business activities; L Public administration; M Education; N Health and social work; O Other community, social and personal services.

Figure 3.1: To what extent do European establishments employ temporary and fixed-term contract workers?



Note: Source is ECS 2009 (Eurofound, 2010b). Temporary workers (temp), fixed-term contract workers (FTC), temporary agency workers (TAW). Descriptive statistics with employer weights.

Paul (1992) (change in sales) or Nunziata and Staffolani (2007) (Hodrick-Prescott-Filter based GDP recession measure). If a firm faces an actual positive shock but is highly uncertain about its duration, the firm would prefer hiring temporary workers, too. Our hypothesis, however, is stronger related to the notion of real shocks - and their duration - rather than to uncertainty about shocks.

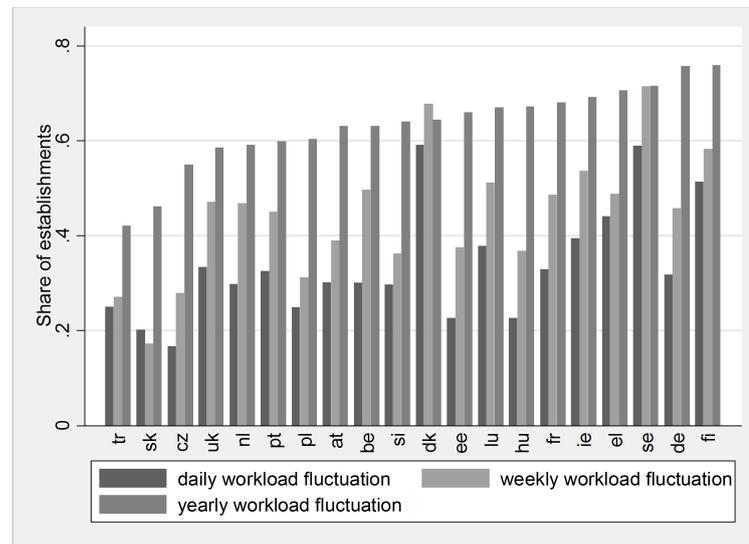
The ECS contains an item that can be directly related to the duration of shocks at the establishment-level. The survey question asks: "Does your establishment normally have to cope with major variations of the workload 1) within a day, 2) within a week or 3) within a year?". Thereby, our variable provides information on the duration of a shock which is relatively certain in its occurrence ("normally"). The measure does not provide any information about the uncertainty - upper and lower bound - of the shock in the future. Hence, we interpret this measurement to be closer related the concept of real shocks - and the duration - rather than to uncertainty.

We include all three variables as dummies in the model. FTC and TAW, however, should be more relevant for fluctuations within a year than for fluctuations within days and weeks. TAW might be a little more important for weekly fluctuation, because establishments can obtain staff at short notice (the typical recruitment procedures for FTCs should not allow being responsive to unforeseen weekly fluctuations). Fluctuations within a week or a day, however, should be dealt with by hiring on part-time or relying on flexible working time rather than temporary contracts.

Figure 3.2 shows that workload variations within a year are the dominant form of fluctuations in most countries. In our sample, 64 per cent of the establishments have to deal with such fluctuations, while the values range from 42 per cent in Turkey to 76 per cent in Finland. Yearly fluctuations are specifically strong in sectors that have to deal with seasonal variations, such as hotels and restaurants, construction and other community, social and personal activities. They are less relevant in sectors with a constant workload, such as health, social work, education and manufacturing and energy.

In countries with low shares of establishments facing yearly fluctuation such as Turkey and Slovakia, high shares of establishments are active in sectors C-E, involving activities such as manufacturing and energy. For instance in Turkey, the

Figure 3.2: Which workload fluctuations dominate in Europe?



Note: Source is ECS 2009 (Eurofound, 2010b). Descriptive statistics with employer weights.

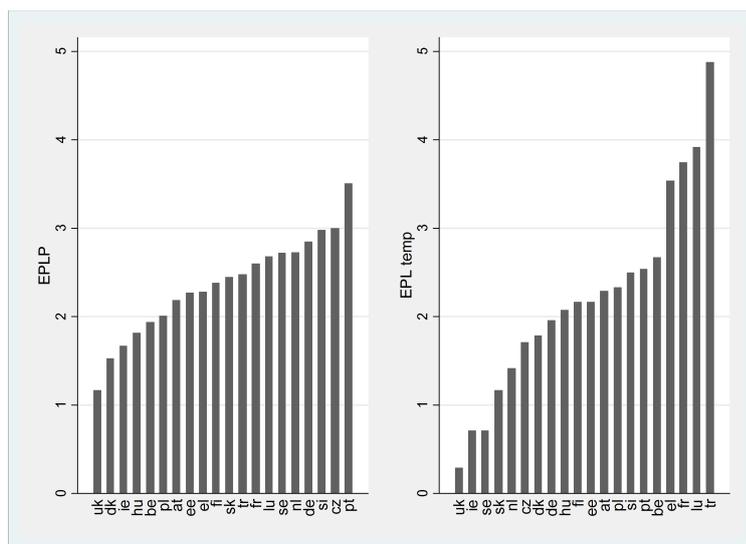
textile industry might play a crucial role and textile production is probably less affected by seasonal fluctuation. By contrast, hotels and restaurants, as well as other community, social and personal activities, play a stronger role in Finland and Germany but less so in Slovakia and Turkey. Overall, in Slovakia and Turkey the industry sector plays a more relevant role in relation to the service sector compared to Finland and Germany, where the structural change towards the service sector is already more progressed. Hence, the structural change might explain at least some of the variation in the share of establishments facing yearly fluctuation.

### 3.4.2 Country-level variables

The effect of fluctuation on the likelihood of employing temporary contracts should be conditional upon sufficiently high firing costs for permanent workers. Hence, we need data on EPLP which is modeled as firing costs in economic models. For this purpose, we employ the well-established OECD indicator on the strictness of employment protection legislation for 2004 and 2008 (Venn, 2009). Venn (2009)

provides an in depth overview of the indicator.<sup>17</sup> The indicator has various subcomponents measuring how strictly different contract types are regulated. In our case, the main important indicator is the sub-indicator for dismissal of employees on permanent contracts (EPLP). We also include the sub-indicator for the strictness of regulation of temporary contracts, which we call the employment protection for temporary workers (EPL temp). This is necessary, because it has been shown to interact with regulation of permanent contracts (Nunziata and Staffolani, 2007). We, however, expect the effect of institutions to be dominated by regulation of permanent contracts. Even if temporary contracts are strictly regulated by comparison, they are usually still more flexible than permanent contracts. Hence, irrespective of their level of regulation, temporary contracts should be attractive if firing costs for permanent workers are high.

Figure 3.3: How strong are European permanent workers and temporary workers protected?



Note: Source is EPL 2008 (OECD, 2012). EPL for permanent workers (EPLP), EPL for temporary workers (EPL temp).

The OECD sub-indicator for EPL for temporary worker is an aggregate of two sub-sub-indicators: EPL for fixed-term contracts (EPL FTC) and EPL for

<sup>17</sup>Bentolila, Cahuc, Dolado and Le Barbanchon (2012) recently criticized the OECD indicator for Spain for being too high for regulations on temporary contracts and too lax for regulations on permanent contracts. To the best of our knowledge, however, this indicator is the most commonly employed for comparative studies.

temporary agency work (EPL TAW). Fixed-term contracts are those that are signed between the worker and the establishment, while temporary work refers to contracts between agencies and workers. The EPL FTC is a summary indicator of measures of the maximum number of successive contracts and cumulated duration, for instance. The EPL TAW summaries indicators such as equal treatment issues, maximum cumulated duration and types of work for which TAW is legal.

Table 3.2: Summary statistics for country-level variables (2009)

Variable	Mean	SD	Min	Max
EPLP (centered)	0	0.563	-1.193	1.147
EPL temp (centered)	0	1.147	-1.940	2.650
EPL FTC (centered)	0	1.370	-1.663	2.338
EPL TAW (centered)	0	1.320	-2.213	2.954
Bargaining coverage rate (centered)	0	30.374	-51.580	37.120
Unemployment rate (centered)	0	2.079	-3.815	3.785
N countries	20			
N establishments	18450			

Note: Temporary workers (temp), temporary agency worker (TAW), fixed-term contract worker (FTC), EPL for permanent workers (EPLP), EPL for temporary workers (EPL temp), EPL for temporary agency workers (EPL TAW), EPL for fixed-term workers (EPL FTC). Data sources: EPL 2008 from OECD (2012), national unemployment rate in the first quarter of 2009 Eurostat (2012), bargaining coverage rate Hayter and Stoevska (2011) and Eurofound (2007a).

The OECD indicator for January 2008 is shown in Figure 3.3 and Table 3.2 for EPL for temporary and permanent workers across Europe.<sup>18</sup> Typically, southern European countries such as Portugal are relatively strong regulated for permanent workers, while Ireland and United Kingdom are quite flexible. High EPL for temporary workers means that the employment of temporary workers is very restrictive. Countries with low restrictions are again Anglo Saxon countries such as United Kingdom and Ireland. Some countries with high EPLP decreased EPL temp to make their labor markets more flexible. The pattern, however, remains similar with Portugal and France having relative high protection in both dimensions and with the Anglo Saxon countries having relatively low protection. Overall, there is a positive and significant correlation between EPLP and EPL temp in our sample. Furthermore, EPL FTC is lowest in Slovakia and highest in Greece, while EPL TAW is lowest in United Kingdom and highest in Turkey.

<sup>18</sup>Summary statistics of country-level variables of the original ECS 2009 sample are presented in Appendix 3.7.3.

## 3.5 Empirical results

### 3.5.1 Workload fluctuation and temporary contracts

#### Temporary workers

Theoretically, we expect establishments facing workload fluctuations of short duration to be more likely to hire temporary workers than establishments without such fluctuations. This effect, however, should be conditional upon sufficiently high employment protection legislation for permanent workers (EPLP). The results presented in Table 3.3 largely confirm this for our binary choice model.<sup>19</sup>

Concerning specifications, Table 3.3 shows models for different covariates. All Models [(1) to (3)] are logistic regressions with cluster-robust standard errors at the country-level (Cameron and Miller, 2015).<sup>20</sup> Model (1) allows for unobserved country fixed effects by including country dummies while Model (2) explicitly models theoretical relevant country-level variables for temporary employment. Thereby, the latter is more directly related to theoretical arguments (Section 3.3). Methodologically, Model (2) is similar to Kahn (2007). Model (3) is nested in Model (1), while the former excludes establishment size and sector dummies.

The coefficients on annual workload fluctuation and the interaction between annual workload fluctuation and EPLP are both quiet robust with respect to different covariates.<sup>21</sup> First, comparing models with country dummies but different covariates [Model (1) and Model (3)], workload fluctuation within a year as well as the interaction between EPLP and annual fluctuation are both positively significant and quite similar in terms of the magnitude. Second, comparing Models (1) and (2), annual fluctuation is again robust positively related to the decision to employ at least one temporary worker. The odds of employing at least one temporary

<sup>19</sup>The full model is presented in Appendix 3.7.1 in Table 3.4.

<sup>20</sup>Based on Cameron and Miller (2015), we provide estimations of different strategies which deal with clustered data. We present a logistic model with cluster-robust standard errors, a feasible generalized least square model (FGLS) with cluster-robust standard errors and a random slope model in Appendix 3.7.6. Coefficients differ not fundamentally, although FGLS and the random slope model have a smaller estimate on *EPLP \* WF annual* and standard errors are smaller. For the main analyses, we follow Kahn (2007) who investigates an EPLP question with similar data to ours.

<sup>21</sup>Model (2) is robust to the use of employer weights. See Appendix 3.7.2.

Table 3.3: Do workload fluctuations increase odds ratios (logistic model) of hiring Temps?

Dependent variable	If any temporary worker		
	(1)	(2)	(3)
WF annual	1.329*** (6.25)	1.316*** (3.95)	1.241*** (4.92)
WF weekly	1.148* (2.07)	1.198* (2.26)	1.076 (1.22)
WF daily	0.895 (-1.94)	0.857* (-2.16)	0.846** (-2.82)
EPLP* WF annual	1.144* (2.13)	1.283* (2.35)	1.129* (2.21)
EPLP		1.349 (0.9)	
Establishment variables	yes	yes	yes
Establishment size fixed effect	yes	yes	no
Sectors fixed effect	yes	yes	no
Cross-level interactions	yes	yes	yes
Country fixed effect	yes	no	yes
Country variables	no	yes	no
Establishments	18407	18407	18407
Countries	20	20	20
LL	-8612	-9213	-9299

Note: \*\*\* significant at 0.1 per cent, \*\* significant at 1 per cent, \* significant at 5 per cent. Coefficients are reported as odds ratios and are from logistic regression models with clustered standard errors, z-values in parentheses. Continuous variables are centered. Temps is temporary workers. WF stands for workload fluctuation; EPLP is employment protection legislation for permanent workers. Country variables: EPL for temporary workers, bargaining coverage, EPLP\*bargaining coverage, EPL for temporary workers\*bargaining coverage, EPLP\*EPL for temporary workers, unemployment rate. Establishment variables: freelancer, works council, number of workers increased, number of workers decreased, high absenteeism and/or sickness rates (i.e. absenteeism and/or sickness causes human resource problems), gender share, share of high-skilled workers (i.e. the proportion of employees working in high-skilled jobs which usually require an academic degree), flexible working time. Interaction between country and establishment variables: WF annual\*EPL for temporary workers, WF annual\*bargaining coverage. Establishment-level variables described in Table 3.1. Country-level variables described in Table 3.2.

worker are 32 [Model (2)] to 33 [Model (1)] per cent higher for establishments with annual fluctuations. As the coefficients are quite similar, we do not expect that the estimate on workload fluctuation in Model (2) captures much unobserved heterogeneity at the country-level. Furthermore, for both models, the odds are higher when EPLP increases.  $EPLP * WF\ annual$  is in both specifications positive and statistically significant, although accounting for unobserved country heterogeneity yield slightly smaller odds. Interpreting the differences between Model (1) and Model (2) as minor, we prefer Model (2) as it is more directly related to theoretical arguments.

In detail, Model (2) shows that the odds of employing at least one temporary worker are 32 per cent higher for establishments with annual fluctuations when EPLP is held constant at the mean. This effect is highly significant and shows that shocks are a main motive to employ temporary workers in Europe (Houseman, 2001; Boockmann and Hagen, 2001). In line with our argument, the odds ratio is even higher when EPLP increases by one unit (1.69).<sup>22</sup>

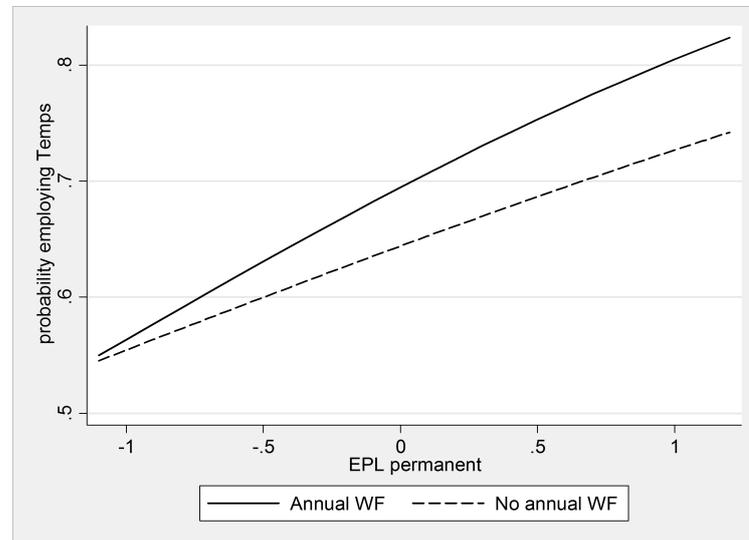
As expected, other types of fluctuation have little or no effect on the likelihood of employing temporary workers. While annual fluctuation is robust, this is less true for weekly and daily fluctuation. Establishments with weekly (daily) fluctuation have a significant higher probability of employing temporary workers in Model (1) and Model (2) [Model (2) and (3)] but not in Model (3) [Model (1)]. This pattern is also revealed in subsample estimations and individual country<sup>23</sup> regressions, which find robust positive coefficients for annual fluctuation but not for daily and weekly fluctuation. This is in line with the argument that establishments adjust for weekly and daily fluctuations by part-time or working time accounts.

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<sup>22</sup>This interpretation is corroborated by the average marginal effects depicted in Figures 3.5 and 3.6. Presented in odds ratios, the interaction term in this model tells us by how much the effect differs, but they do so in a multiplicative way (Buis, 2010, p. 87). Hence, the relevant odds ratio for annual fluctuation is obtained by multiplying its odds ratios with the coefficient of the interaction term (Buis, 2010).

<sup>23</sup>Results are available upon request.

Figure 3.4: Do workload fluctuations increase the probability of hiring Temps and does this relation even becomes stronger with an increase in EPLP?



Note: Average of predicted probabilities, Model (2), calculated at zero for all institutions (EPL for temporary workers (Temps), bargaining coverage rate), except EPL permanent.

To present the substantive effect of our explanatory variables, Figure 3.4 plots the average of predicted probabilities of employing at least one temporary worker at different values of EPL for permanent workers (broken down by establishments with and without annual fluctuations).<sup>24</sup> These predicted probabilities are based upon Model (2) in Table 3.3. The figure confirms that the gap between the two types of establishments increases with strictness of EPLP and that this gap is relevant in substantive terms. In a flexible regime such as in the United Kingdom, establishments employ temporary workers to 59 per cent- the establishment types do not differ. In rigid regimes, however, we find that the probability of employing temporary workers is 78 per cent for establishments with annual fluctuations, compared to 70 per cent for those without fluctuations.

Are the differences between the two types of establishments significant? To answer this question, we calculated the average marginal effects of annual workload fluctuation on the probability of employing at least one temporary worker

<sup>24</sup>Confidence intervals are not presented here, as the significance of the average marginal effect of workload fluctuation on the probability to employ temporary workers is presented in the Figure 3.5.

at different values of EPL for permanent workers and at the mean of the other interaction terms, as well as their confidence intervals:<sup>25</sup>

$$DP = P(y_{ij} = 1|WF = 1, EPL = x) - P(y_{ij} = 1|WF = 0, EPL = x) \quad (3.4)$$

The results are plotted in Figure 3.5 against the level of EPL for permanent workers. Irrespective of the level of protection for temporary workers, the average marginal effect of annual workload fluctuation requires a sufficient level of EPL for permanent workers to become significant. In rigid labor markets, the probability is 8 percentage points higher for establishments with annual workload fluctuation. In average regimes (such as in Finland), these two groups of establishments still differ by 5 percentage points. In both cases, probability differences between establishment types are significant. In flexible labor markets, however, workload fluctuations cease to make a significant difference in the probability of hiring temporary workers.

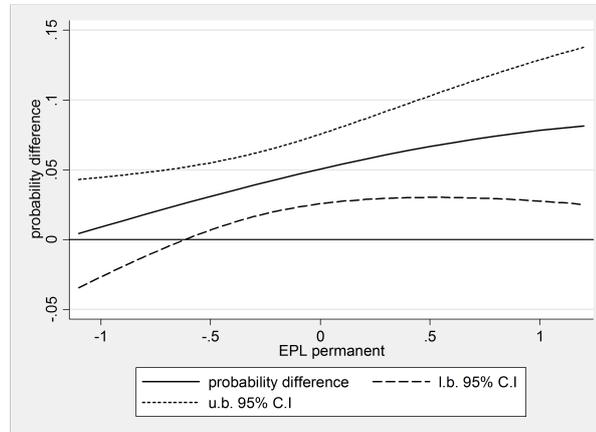
As marginal effects in logistic models depend on covariates, we calculated DP for low, and high values (one standard deviation difference from the mean) of employment protection for temporary workers (Figure 3.6), whereby increasing DP with EPL for permanent workers also holds for low and high employment protection for temporary workers. Interestingly, we find that the threshold for EPL for permanent workers, where DP becomes significantly different from zero, is higher when temporary work is strongly regulated. Furthermore, DP becomes largest when EPL for temporary workers is low. This result is quite intuitive: The less it costs to hire and terminate temporary workers, the more often they are used to circumvent the numerical adjustment to production shocks by firing permanent workers.

Overall, we find that the effect of workload fluctuations on establishments' demand for temporary workers depends on employment protection for permanent workers. This is in line with the results of Bentolila and Saint-Paul (1992), who study whether the impact of sales shocks on employment differs with the availab-

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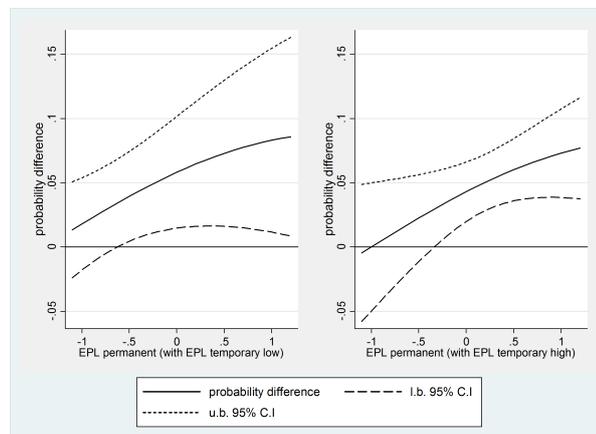
<sup>25</sup>We sometimes refer to the average marginal effect as the difference in the predicted probabilities of establishments with annual fluctuation versus establishments without fluctuation.

Figure 3.5: Does the positive relation of fluctuation with the probability of employing Temps differ significantly with EPLP?



Note: Difference in the predicted probabilities of employing at least one temporary worker of establishments with annual fluctuation versus establishments without fluctuation, Model (2), calculated at zero for all institutions (EPL for temporary workers (Temps), bargaining coverage rate), except EPL permanent. 95% confidence interval (C.I.) presented with the lower bound (l.b.) and upper bound (u.b.).

Figure 3.6: Does the positive relation of fluctuation with the probability of employing Temps differ significantly with EPLP even for different values of EPL temporary?



Note: Difference in the predicted probabilities of employing at least one temporary worker of establishments with annual fluctuation versus establishments without fluctuation, Model (2), calculated at zero for all institutions (bargaining coverage rate), except EPL permanent and EPL for temporary workers (Temps) (+/- one standard deviation from zero). 95% confidence interval (C.I.) presented with the lower bound (l.b.) and upper bound (u.b.).

ility of temporary workers. They find - similar to lower firing costs for permanent workers - an increased pro-cyclical response to sale shocks when temporary workers are available. While our study evidently differs in terms of the dependent and independent variables, in both studies institutions change the impact of labor demand shocks (either measured as sales shocks or as the "normal" duration of shocks) on employment. This is also in line with results from Eslava et al. (2014) who find job destruction (creation) to be stronger related to changes in the share of temporary employment when employment protection for permanent workers increased. In contrast, our findings differ slightly in comparison to Nunziata and Staffolani (2007). They find a significant negative impact of recessions on the aggregated share of temporary employment, although employment protection for permanent workers does not change this effect strongly. The divergence from our results is not very surprising. First, the micro-composition of the economy is not accounted for by macro-data. Second, we employed different concepts in the sense that we study the impact of the duration of "normal" workload fluctuations and not the impact of the current state of the economy.

### **Fixed-term contract and temporary agency workers**

In this paper, temporary workers are distinguished between FTCs and TAWs. We examined the probability of employing at least one FTC or TAW in the establishment, and hence, this time controlling for EPL FTC and EPL TAW instead of EPL Temp. One might be concerned about high correlations between indicators with only 20 countries. Correlations, however, seem relatively modest between EPLP and EPL FTC (0.31) or EPL TAW (0.43). The estimation results are shown in Appendix 3.7.1 in Table 3.5. The focus is on Model (5) and Model (8).<sup>26</sup>

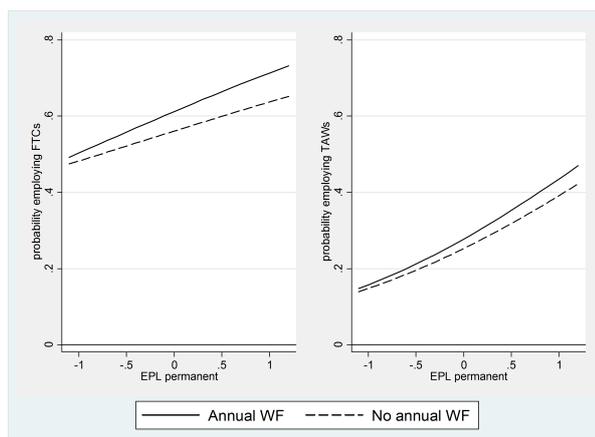
Theoretically, we do not expect differences between these two subgroups concerning the impact of annual fluctuation at different levels of rigidity. We generally find this to be the case. The direct effect of annual workload fluctuation on employing TAW or FTC is positive significant in Model (5) and Model (8). For TAWs, the annual workload effect is slightly lower, which might be due to the fact that TAWs play a minor macro-economic role. Another explanation is that

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<sup>26</sup>See Section 3.5.1.

annual workload fluctuations measures mainly fluctuations that were foreseeable and hence FTCs could be employed. TAWs might be more relevant in the case of unforeseeable fluctuations. Concerning weekly and daily fluctuation, we do not find a strong and robust effect which is as expected. Part-time workers and flexible working time should be more relevant for these kinds of variation.

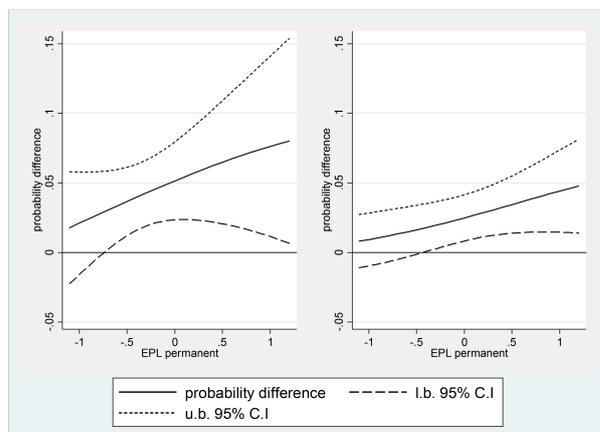
Figure 3.7: Do workload fluctuations increase the probability of hiring FTCs (or TAWs) and does this relation become even stronger with an increase in EPLP?



Note: Predicted probabilities of employing at least one TAW/FTC of establishments with annual fluctuation against establishments without fluctuation, Model (5) and (8) in Table 3.5 (Appendix 3.7.1), calculated at zero for all institutions (bargaining coverage rate, EPL for fixed-term contract (FTC) workers, EPL for temporary agency workers (TAW)), except EPL permanent.

We, however, are mainly interested in the marginal effect of workload fluctuations in different institutional contexts. We expect the impact of annual fluctuation to differ at different levels of rigidity. This is also what we find, with the probabilities of employing TAWs or FTCs significantly higher for establishments with annual workload fluctuations given a sufficiently strong regulation for permanent workers (Figures 3.7 and 3.8). The results, however, seem to be more strongly driven by FTCs rather than TAWs. Again, this could be explained by the relatively small macro-economic relevance of TAWs. The other explanation was that annual workload fluctuations mainly measures fluctuations that were foreseeable and that TAWs might be more relevant to cope with unforeseeable fluctuations.

Figure 3.8: Does the positive relation of fluctuation with the probability of employing FTCs (or TAWs) differ significantly with EPLP?



Note: Difference in the predicted probabilities of employing at least one TAW/FTC of establishments with annual fluctuation versus establishments without fluctuation, Model (5) and (8) in Table 3.5 (Appendix 3.7.1), calculated at zero for all institutions (bargaining coverage rate, EPL for fixed-term contract (FTC) workers, EPL for temporary agency workers (TAW)), except EPL permanent. 95% confidence interval (C.I.) presented with the lower bound (l.b.) and upper bound (u.b.).

## Share of fixed-term workers

Finally, we estimate the share of FTCs at the date of the interview as the dependent variable. The share of FTCs cumulates at zero. Following the idea of different processes for the extensive and intensive margin (e.g. Eslava et al., 2014), we estimate a two-component model [probit model and OLS model (subsample with values in the share of FTCs larger than zero)].<sup>27</sup> We find robust and expected relations for the extensive margin but to a lesser extent for the intensive margin. Establishments in rigid labor markets with annual fluctuations are more likely to employ at least one FTC at the date of the interview compared to establishments without annual fluctuation (Figure 3.9). This is not observed in flexible labor markets.

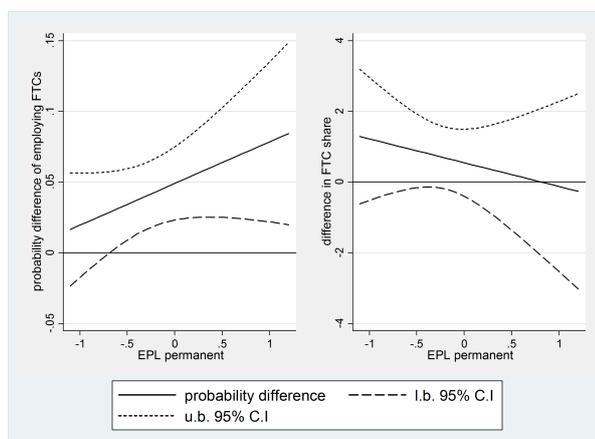
In terms of the share of FTCs, the rigidity does not seem to play a fundamental role in the relation between workload fluctuation and the share of FTCs (Figure 3.9).<sup>28</sup> The non-significant result at the intensive margin can be explained by the

<sup>27</sup>Estimation results are available upon request.

<sup>28</sup>This is independent of whether we control for establishment weights. Results with establish-

measurement period. The share of FTCs refers to the date of the interview, while the dummy variable of annual workload fluctuation refers to a time period. The fluctuation dummy does not provide any information about the workload at the date of the interview. As the precise share of FTCs at the date of the reform is expected to be much more sensitive to the workload at a specific point of time compared to the decision to employ any FTC, the non-significant result is less surprising.

Figure 3.9: Do the relations of fluctuation with the probability and the share of employing FTCs at the interview date differ significantly with EPLP in 2009?



Note: Difference in the predicted probabilities of employing at least one FTC (Graph 1) or FTC shares at the date of the interview (Graph 2) of establishments with annual fluctuation versus establishments without fluctuation, Model (2) with different dependent variables and estimators (Graph 1: dummy for employing at least one FTC at the date of the interview, probit model; Graph 2: share of FTC at the date of the interview for firms with at least one FTC, OLS model) without employers' weight, calculated at zero for all institutions (bargaining coverage rate, EPL for fixed-term contract (FTC) workers), except EPL permanent. 95% confidence interval (C.I.) presented with the lower bound (l.b.) and upper bound (u.b.).

### 3.5.2 Correlation versus effect

In summary, establishments facing annual fluctuations are more likely to employ temporary workers, and the likelihood increases with EPL for permanent workers. To what extent, however, can we talk about effects rather than correlations? Our

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ment weights are available upon request.

identifying assumption is that workload fluctuation and employment protection legislation for permanent workers are pre-determined to the hiring behavior of establishments. Given that our empirical identification is relatively weak, we discuss in the following the extent to which our estimators could be interpreted as effects rather than correlations. We discuss three issues: endogeneity of workload fluctuations, endogeneity of EPL for permanent workers and omitted unobservables at the country-level.

### **Endogeneity of workload fluctuations**

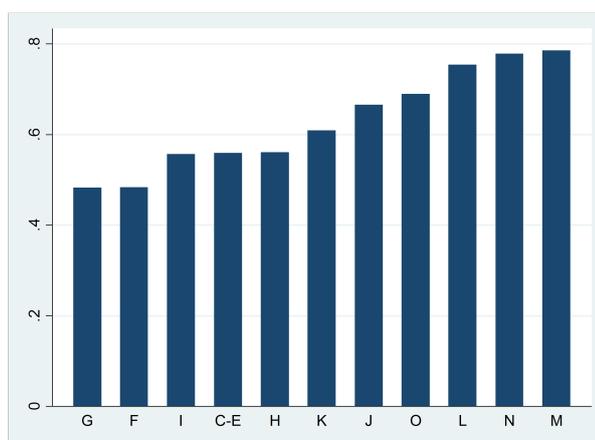
Concerning expected workload fluctuations, we assume that workload fluctuations are exogenous to hiring temporary workers. We interpret workload fluctuation as being a characteristic of the product (1) itself, but also induced by macro-economic variations (2). First, considering the product characteristic, for instance, the workload in restaurants fluctuates with peaks in the summer, while hotels also face workload fluctuation due to seasonal fluctuations in demand. The establishment also might invest in another product, although this is relatively costly compared to hiring and firing decisions. Therefore, the product characteristic component of workload fluctuation is interpreted as being pre-determined to hiring decisions.

Second, next to the product characteristic, workload fluctuation might also change due to macro-economic variations (B). In this case, reversed causality might be an issue. Recent matching models (Sala, Silca and Toledo, 2012; Costain, Jimeno and Thomas, 2010) show that a dual labor market structure - including high shares of temporary workers - yields higher unemployment volatility. This, in turn, might jeopardize private domestic demand. Increased volatility in private domestic demand results in more workload volatility at the firm-level for firms producing for the domestic sector. To assess the potential relevance of this mechanism, Figure 3.10 shows the share of establishments employing at least one temporary worker by sector. The highest share is observed in the sector of education (M), health and social work (N) and public administration (L). Domestic private demand might play a role for sector N (health and social work), as well as sector M (education). Public domestic demand and export, however, induced demand also play a crucial role: Demand for health and social work (N), as well as public admin-

istration is predominantly induced by the public (L); research for export-oriented establishments might induce demand for higher education (M). Therefore, the use of temporary workers is not restricted to private domestic demand and concerns about reversed causality are at least weakened.

In addition, the notion that workload variations determine and motivate the hiring of temporary workers and not solely reverse is also supported by Lotti and Viviano (2012), as well as Houseman (2001). Lotti and Viviano (2012) show that the positive relation between uncertainty of product demand shocks as a covariate and the share of temporary employment on overall workforce remains when uncertainty is lagged over more than one year. Finally, Houseman (2001) finds in her survey that a main motive to employ temporary workers in American establishments is expected variation in the workload (40 per cent on average). Overall, we do not rule out reversed causality, although it seems to be of limited relevance in our case. Hence, we account for this by interpreting the positive workload fluctuation estimator on temporary employment as an upper bound estimate.

Figure 3.10: Share of establishments' employing at least one temporary worker by sector



Note: Source is ECS 2009 (Eurofound 2010a). NACE Rev. 1.1: C-E Manufacturing and energy; F Construction; G Wholesale and retail trade, repair of goods; H Hotels and restaurants; I Transport and communication; J Financial intermediation; K Real estate and business activities; L Public administration; M Education; N Health and social work; O Other community, social and personal services.

### **Endogeneity of EPL for permanent workers**

We do not employ exogenous variation in EPL for permanent workers to test our hypothesis on the conditioning effect of EPL for permanent workers. Thereby, reversed causality could be an issue. Reversed causality means that the hiring behaviour of establishments would have an effect on EPL for permanent workers. We, however, argue that plausible reversed causality would even underline our interpretation of the results. First, for France, Marx (2012) found that changing hiring behaviour of employers - an increase in hiring of temporary workers - yielded a decrease in EPL for temporary workers. If this mechanism was also applicable to EPL for permanent workers, EPLP would be negatively correlated to the employment of temporary workers. Hence, our positive significant relation between EPLP and employing temporary workers would be underestimated, thus reflecting a lower bound estimate.

Second, Bentolila, Dolado and Jimeno (2012) point to the relation between the share of outsiders in a country and reforms in EPL for permanent workers - while outsiders are those workers who are in flexible contracts and not in open-end contracts like insiders (Saint-Paul, 1996a). The higher the share of temporary workers is, the higher the share of the so-called outsiders, who are assumed to benefit - or at least not to suffer - from lower EPLP. This argument is related to Saint-Paul (1996b). Therefore, liberalizing reforms are more likely when the share of temporary workers is high. This again implies that our positive estimator between EPLP and the employment of temporary workers suffers from a downward bias. In sum, reversed causality between hiring temporary workers and EPL for permanent workers leads to a lower bound estimate of our positive estimator.

### **Omitted variables at the country-level**

Third, concerning unobserved heterogeneity at the country-level, we argue to control for relevant other factors. For instance, employment protection for permanent workers is positively correlated to the protection of temporary workers in our estimation sample and EPL for temporary workers is negatively related to the employment of temporary workers. Therefore, we include the interaction between workload fluctuation and employment protection for temporary workers in our re-

gressions. This rules out the notion that the increase in the effect of workload fluctuation with employment protection for permanent workers is not attenuated by employment protection legislation for temporary workers.

### 3.5.3 Robustness analyses

Our results for annual workload fluctuation and the interaction term between fluctuation and EPL for permanent workers are quite robust with respect to differential enforcement of EPL for permanent workers, country subsample estimations, and different year of observation.

#### Differential enforcement of EPL for permanent workers

EPL for permanent workers might be differentially enforced across firm size. First, more than half of the countries in our sample face some kind of exemptions from EPL for permanent workers for small firms (Venn, 2009).<sup>29</sup> Controlling for establishment size dummies in Model (2) does not account for this. Hence, we estimate Model (2) (without controlling for establishment size dummies) for subsamples that exclude one establishment size category. No major differences in the estimates, however, are observed when small establishments are excluded (Figure 3.11).

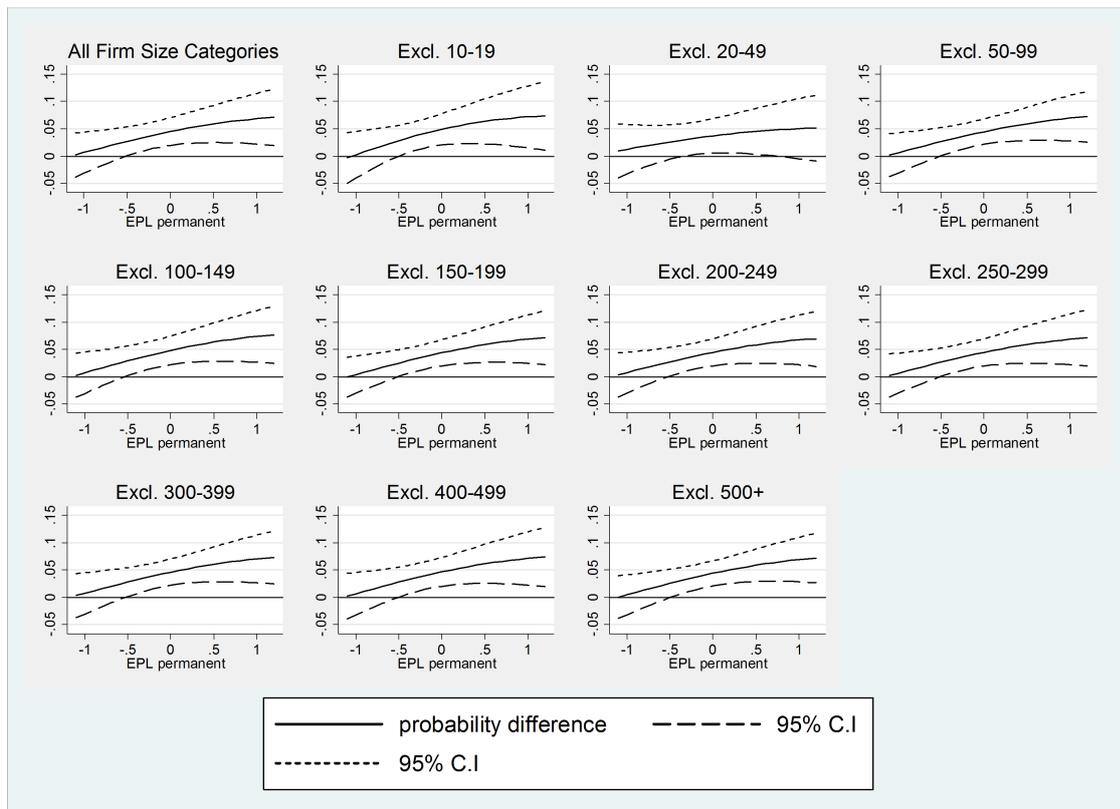
Second, it was found that differential enforcement due to governance differences is relevant for the effect of EPL in samples of industrialized and developing countries and thus it is often accounted for in EPL analyses (Micco and Pages, 2007; Haltiwanger et al., 2014). We employ governance indicators (government effectiveness, rule of law, control of corruption) as proxies for the enforcement of regulations (Micco and Pages, 2007; Kaufmann, Kraay and Mastruzzi, 2004).<sup>30</sup> Figure 3.12 shows that the relation does not significantly differ between countries with high and low respective levels of enforcement. We explain this by the fact that in European countries these indicators are relatively high and that enforcement is specifically relevant in developing countries (Venn, 2009). For instance, Micco and Pages (2007) found that their EPL indicator had no effect on job flows in countries

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<sup>29</sup>Austria, Belgium, the Czech Republic, Denmark, Finland, Germany, Hungary, Portugal, Slovenia, Sweden and Turkey.

<sup>30</sup>A description of the governance indicators can be found in Appendix 3.7.4.

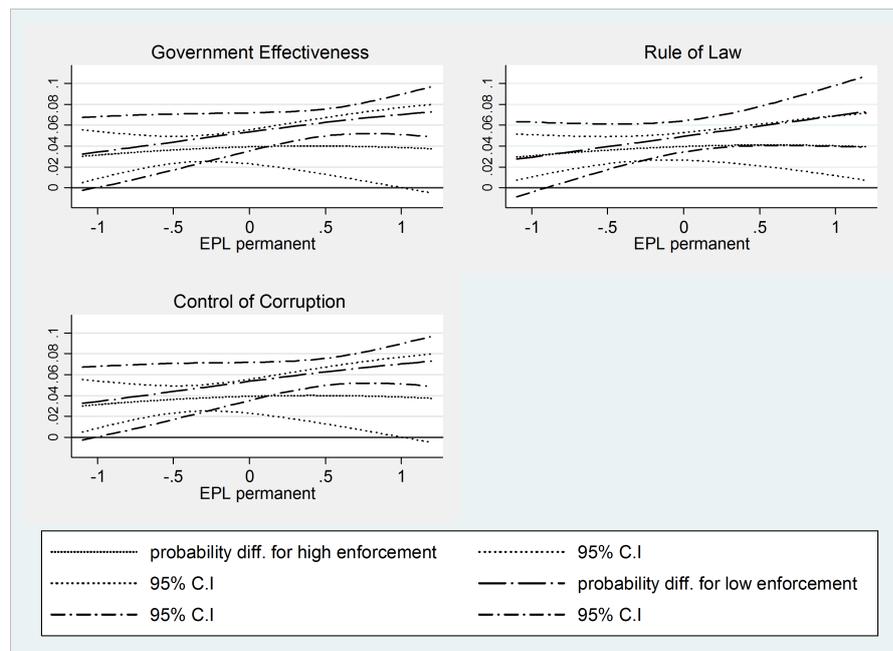
Figure 3.11: Is the relevance of EPLP for the relation of fluctuation with the probability of employing Temps in 2009 underestimated due to small firm exemptions from EPL?



Note: Difference in the predicted probabilities of employing at least one temporary worker of establishments with annual fluctuation versus establishments without fluctuation, Model (2) without firm size dummies, calculated at zero for all institutions (bargaining coverage rate, EPL for temporary workers (Temps)), except EPL permanent. Samples exclude (excl.) firms with the size of employees mentioned in the title. 95% confidence interval (C.I.) presented with the lower bound (l.b.) and upper bound (u.b.).

with low scores in the rule of law indicator. The mean in their sample of industrial and developing countries is around -0.18, with a minimum at -1.27, while the mean in our sample of non-developing countries is at 1.26, with a minimum at 0.13.

Figure 3.12: Differential enforcement: Does the positive relation of fluctuation with the probability of employing Temps differ significantly with EPLP in 2009?



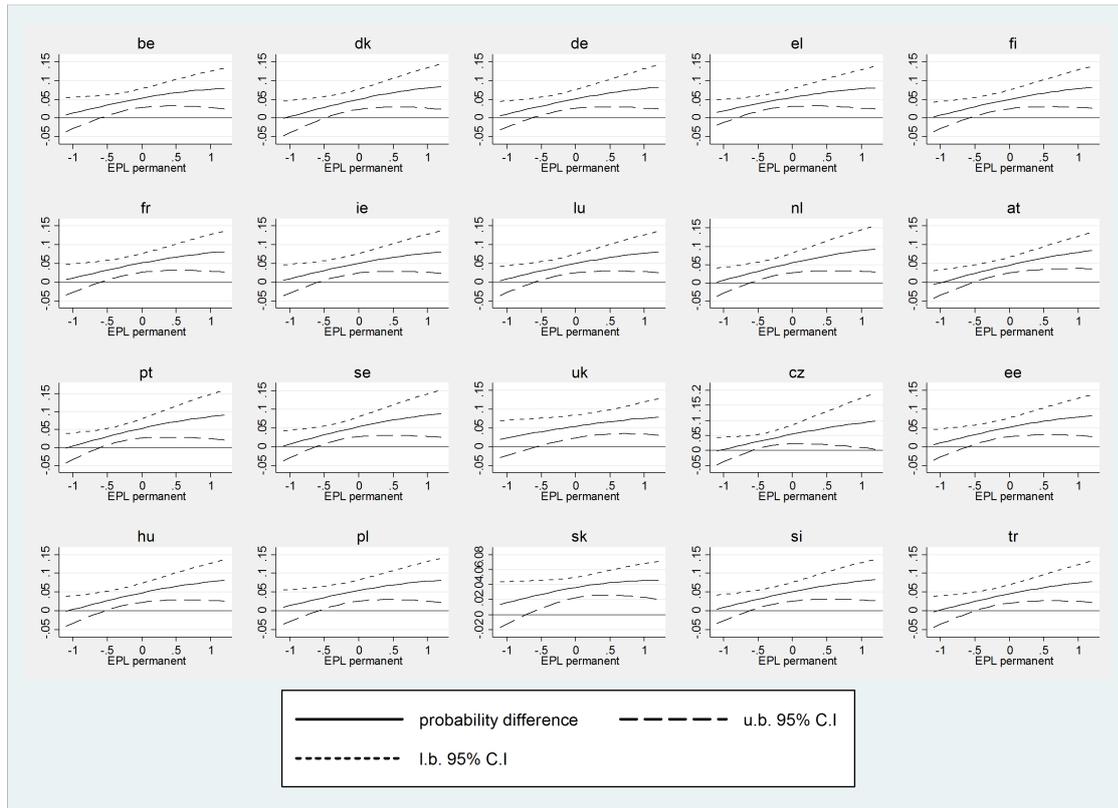
Note: Difference (diff.) in the average of predicted probabilities of employing at least one temp of establishments with annual fluctuation against establishments without fluctuation, Model (2), calculated at zero for all institutions (bargaining coverage rate, EPL for temporary workers (Temps)), except EPL permanent. 95% confidence interval (C.I.) presented with the lower bound (l.b.) and upper bound (u.b.). High (low) enforcement means sample is restricted to firms in countries with governance indicators larger than (smaller or equal to) the mean.

## Other robustness analyses

In terms of subsample estimations, one might be concerned that the results are driven by a single country. Therefore, we provide subgroup estimations. Figure 3.13 presents the average marginal effect of annual workload fluctuation on the probability to employ at least one temporary worker and their confidence intervals. Figure 3.13 is based upon Model (2) in Table 3.3, albeit for subsamples, i.e. excluding one country from the sample. For each subsample, the average

marginal effect of annual workload fluctuation requires a sufficient level of EPL for permanent workers to be significant. Hence, the results are not driven by one specific country.<sup>31</sup> Furthermore, we checked whether results differ with sectors and conclude that they are not driven by a specific sector too (Appendix 3.7.5).

Figure 3.13: Is the relevance of EPLP for the relation of fluctuation with the probability of employing Temps in 2009 driven by one specific country?



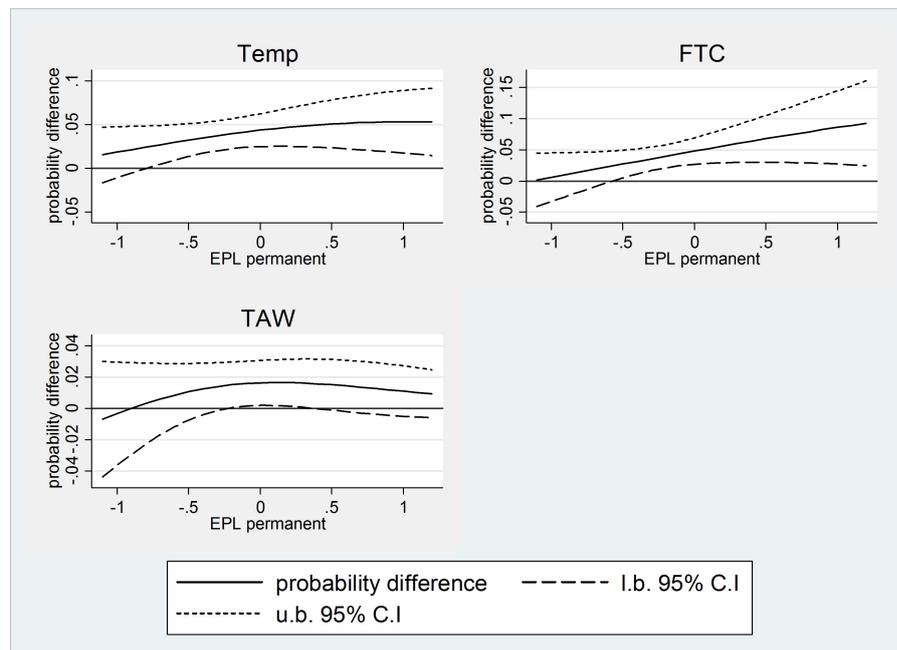
Note: Difference in the predicted probabilities of employing at least one temporary worker of establishments with annual fluctuation versus establishments without fluctuation, Model (2), calculated at zero for all institutions (bargaining coverage rate, EPL for temporary workers (Temps)), except EPL permanent. Samples exclude the country which is mentioned in the title. 95% confidence interval (C.I.) presented with the lower bound (l.b.) and upper bound (u.b.).

The survey for the main analyses was conducted in spring 2009, when most European countries were experiencing a severe economic crisis. Results might be sensitive to this. Therefore, we estimated Model (2) in Table 3.3 with establishment data of the 2004/2005 ECS. Figure 3.14 shows that the relation is as expected

<sup>31</sup>The results for employing at least one TAW or FTC are similarly robust.

for temporary workers. Examining FTCs and TAWs separately, it becomes evident that the relation is driven by FTCs and not by TAWs. This is similar to the 2009 data.<sup>32</sup> The expected relationship is less clear when Germany is included, which is unsurprising, considering that extensive German labor market reforms, e.g. temporary agency work, took place between 2003 and 2006.

Figure 3.14: Does the positive relation of fluctuation with the probability of employing Temps (or FTCs, TAWs) differ significantly with EPLP in 2004/2005?



Note: Difference in the predicted probabilities of employing at least one temp (FTC or TAW) of establishments with annual fluctuation versus establishments without fluctuation, Model (2), (5) and (8) but with the 2004/2005 sample with employers' weight, calculated at zero for all institutions (bargaining coverage rate, EPL for temporary workers (Temps) (Model (2)), EPL for fixed-term contract (FTC) workers, EPL for temporary agency workers (TAW)), except EPL permanent. Sample excludes Germany. 95% confidence interval (C.I.) presented with the lower bound (l.b.) and upper bound (u.b.).

Concerning the measurement of EPL, the difference between EPL for permanent and temporary workers might be the more relevant measure rather than EPL for permanent workers itself. Taking the difference indicates that establishments in Ireland and United Kingdom face higher incentives to employ temporary workers compared to establishments in France. The share of temporary workers in France,

<sup>32</sup>The results of the 2004/2005 ECS are a bit more sensitive to employer weights compared to results of the 2009 sample.

however, is higher compared to Ireland and United Kingdom. Controlling for other institutions and establishments-level variables, we do not find a clear positive role of the difference for the relation between workload fluctuations and the probability of employing temporary workers.<sup>33</sup> Hence, even when temporary work is relatively high regulated, temporary workers still seem to be more attractive compared to permanent workers in highly regulated countries concerning permanent workers. While we do not conclude that relative costs do not play any role, in our case they are less relevant.

### 3.6 Conclusions

The intention of our paper was to bring in the interaction between shocks of short duration and employment protection as an important element explaining firms' demand for temporary work in Europe. This is pressing as the financial crisis in 2007 increased macro-economic fluctuations, which are positively related to firm-level fluctuations. Therefore, shocks at the firm-level became even more relevant for the use of temporary work. In line with recent theories, we have hypothesized a higher propensity to hire temporary workers if a firm is exposed to shocks of short duration. But we have expected this effect to be conditioned by the regulatory framework. Shocks should only matter if firing costs for workers with permanent contracts are sufficiently high.

We constructed a novel data set combining establishment-level variables of around 18,000 establishments from up to 20 European countries with institutional variables for 2004/2005 and 2009. The establishment-level data provide the unique opportunity to investigate the duration of labor demand shocks and temporary employment at the same time. Furthermore, these data offer the novel opportunity to investigate labor demand shocks at the establishment-level within a broad cross-country study. The data, however, are limited in terms of controlling for firm fixed-effects. Due to the demanding assumptions of our identification strategy, we extensively discuss the issue of correlations versus effects.

Using cross-country establishment data, we were able to confirm our hypothesis

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<sup>33</sup>The results are available upon request.

across a number of robustness checks, having extensively discussed potential endogeneity issues. First, we are able to generalize results from single country studies to European countries (Eslava et al., 2014): Our main result is that establishments with workload fluctuation within a year are more likely to hire workers on temporary contracts, with this effect being conditional upon a certain level of employment protection legislation - our measure of firing costs. The results are not only statistically significant, but they also matter in substantive terms. While we do not observe a significant effect of workload fluctuations in flexible labor markets, the difference between establishments with and without fluctuations is eight percentage points in heavily regulated labor markets. This is also true for the employment of fixed-term and temporary agency workers, although to a lesser extent for the latter. This might be explained by a still minor macro-economic role of agency workers in Europe. Another explanation is that agency workers might be more used to cope with uncertain shocks, while our shock measure emphasizes the short duration of a shock and not the uncertainty of a shock.

Second, we provide first evidence on the relevance of the duration of labor demand shocks for temporary employment: While annual fluctuation is robustly positively related to employing temporary workers, this is less true for weekly and daily fluctuation. Firms might adjust for weekly and daily fluctuations by part-time or working time accounts but less by temporary workers. Finally, we are the first who descriptively show that annual workload fluctuation is widely spread in Europe: 64 per cent of establishments in 20 European countries had to deal with annual fluctuations in the workload in 2008 and was the dominant form (compared to weekly and daily).

Future research could be built up on this by investigating adjustment mechanisms for weekly and daily fluctuations in more detail as well as by studying heterogeneity in fluctuations which take longer than one year. To investigate motives for the differences in the use of temporary agency work versus fixed-term contracts as a buffer seems to be a fruitful task, too. Employing different methods in order to mitigate remaining concerns about reversed causality (fluctuation and temporary employment) would be beneficial in order to cross-validate these findings. Moreover, generally speaking, our results are in line with initial findings that labor market institutions mediate the effect of firm-characteristics. Bringing

the different levels together is a fruitful task for future research. Depending on the research question, the data for this would be readily available. More difficult, regarding data requirements, would be to analyse the link between institutions, establishments and workers' characteristics. The improved availability of linked employer-employee data sets may make this possible in the future.

As we have shown, firing costs encourage the use of temporary contracts for establishments with yearly workload fluctuations. Our descriptive results indicate that annual workload fluctuations are widely spread in Europe, and thereby, that the need for flexibility is inherent to some establishments' production processes. Reforms ignoring the fundamental role of economic volatilities in the context of sufficiently large EPLP for temporary employment might produce a strengthening of segmented labor markets.

## 3.7 Appendix

### 3.7.1 Full models

Table 3.4: Are workload fluctuations associated with higher odds ratios (logistic model) of hiring Temps and does this relation become even stronger with an increase in EPLP?

Dependent variable	If any temporary worker		
	(1)	(2)	(3)
Establishment controls			
WF annual	1.329*** (6.25)	1.316*** (3.95)	1.241*** (4.92)
WF weekly	1.148* (2.07)	1.198* (2.26)	1.076 (1.22)
WF daily	0.895 (-1.94)	0.857* (-2.16)	0.846** (-2.82)
Freelancer	1.830*** (7.08)	1.814*** (4.51)	2.067*** (7.83)
Works council	1.416*** (5.1)	1.833*** (3.74)	2.892*** (13.62)
Number of workers up	1.305*** (7.48)	1.350*** (7.14)	1.575*** (12.2)
Number of workers down	0.987 (-0.25)	1.016 (0.26)	1.067 (1.15)
Absent	1.474*** (5.87)	1.624*** (5.33)	1.805*** (8)
Gender share	1.004** (3.06)	1.004** (3.16)	1.006*** (3.88)
High-skilled share	1.001 (0.48)	1 (0.05)	1.003 (1.81)
Flexible working time	1.206*** (3.92)	1.290*** (4.39)	1.204*** (3.59)
Cross-level interactions			
EPLP*WF annual	1.144* (2.13)	1.283* (2.35)	1.129* (2.21)
EPL temp*WF annual	1.019 (0.65)	0.946 (-0.98)	1.026 (1.18)
Bargainin*WF annual	0.999	0.996	1.000

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Table 3.4 – *Continued from previous page*

Dependent variable	If any temporary worker		
	(1)	(2)	(3)
	(-0.78)	(-1.26)	(-0.20)
Country controls			
EPLP		1.349 (0.9)	
Bargaining coverage		0.994 (-0.53)	
EPLP*bargaining		1.016 -1.4	
Unemployment rate		0.859 (-1.45)	
EPL temp		0.704 (-1.26)	
EPLP*EPL temp		0.745 (-0.91)	
EPL temp*bargaining		1.006 -1.45	
Establishment size fixed effect	yes	yes	no
Sectors fixed effect	yes	yes	no
Country fixed effect	yes	no	yes
Establishments	18407	18407	18407
Countries	20	20	20
LL	-8612	-9213	-9299

Note: \*\*\* significant at 0.1 per cent, \*\* significant at 1 per cent, \* significant at 5 per cent. Coefficients are reported as odds ratios and are from logistic regression models with clustered standard errors, z-values in parentheses. Continuous variables are centered. Temps is temporary workers. WF stands for workload fluctuation; EPLP is employment protection legislation for permanent workers. Country variables: EPL for temporary workers, bargaining coverage, EPLP\*bargaining coverage, EPL for temporary workers\*bargaining coverage, EPLP\*EPL for temporary workers, unemployment rate. Establishment variables: freelancer, works council, number of workers increased, number of workers decreased, high absenteeism and/or sickness rates (i.e. absenteeism and/or sickness causes human resource problems), gender share, share of high-skilled workers (i.e. the proportion of employees working in high-skilled jobs which usually require an academic degree), flexible working time. Interaction between country and establishment variables: WF annual\*EPL for temporary workers, WF annual\*bargaining coverage. Establishment-level variables described in Table 3.1. Country-level variables described in Table 3.2.

Table 3.5: Are workload fluctuations associated with higher odds ratios (logistic model) of hiring FTCs (or TAWs) and does this relation become even stronger with an increase in EPLP?

Dependent variable	If any FTC		If any TAW			
	(4)	(5)	(6)	(7)	(8)	(9)
Establishment controls						
WF annual	1.316*** (6.11)	1.319*** (3.73)	1.225*** (4.91)	1.126* (2.07)	1.163** (3.01)	1.078 (1.21)
WF weekly	1.085 (1.44)	1.137* (2.03)	1.018 (0.37)	1.05 (1.06)	1.116* (2.29)	1.006 (0.13)
WF daily	0.968 (-0.56)	0.949 (-0.84)	0.95 (-0.86)	0.902 (-1.40)	0.861* (-1.96)	0.802** (-2.75)
Freelancer	1.596*** (6.54)	1.603*** (4.92)	1.747*** (8.2)	1.666*** (7.32)	1.503*** (4.43)	1.835*** (8.21)
TAW	1.822*** (5.94)	2.020*** (6.12)	2.238*** (7.04)			
FTC				1.855*** (5.57)	2.011*** (6.02)	2.290*** (7.02)
Works council	1.333*** (3.31)	1.563*** (3.32)	2.589*** (10.7)	1.281** (2.67)	1.504*** (4.05)	1.961*** (7.95)
Number of workers up	1.208*** (4.2)	1.233*** (4.51)	1.398*** (7.42)	1.151*** (3.6)	1.165*** (3.99)	1.353*** (8.489)
Number of workers down	0.973 (-0.47)	1.028 (0.45)	1.011 (0.19)	0.985 (-0.27)	0.951 (-0.81)	1.186** (3.02)
Absent	1.346*** (5.02)	1.454*** (5.66)	1.613*** (7.21)	1.252*** (3.32)	1.288** (2.79)	1.447*** (5.58)
Gender share	1.007*** (5.64)	1.007*** (6.15)	1.011*** (7.72)	0.996* (-2.54)	0.995** (-2.99)	0.991** (-3.20)
High-skilled share	1.001 (0.4)	1 (0.13)	1.004* (2.34)	1.002 (1.15)	1.002 (1.16)	0.999 (-0.35)
Flexible working time	1.210*** (5.15)	1.259*** (4.22)	1.211*** (4.73)	1.103 (1.6)	1.128* (1.98)	1.173* (2.11)
Cross-level interactions						
EPLP*WF annual	1.087	1.181	1.112	1.027	1.072	1.043

*Continued on next page*

Table 3.5 – *Continued from previous page*

Dependent variable	If any FTC			If any TAW		
	(4)	(5)	(6)	(7)	(8)	(9)
	(1.16)	(1.41)	(1.59)	(0.37)	(1.24)	(0.54)
EPL FTC*WF annual	0.984	0.989	1.019	1.031	1.022	1.034
	(-0.49)	(-0.23)	(0.58)	(0.6)	(0.42)	(0.65)
EPL TAW*WF annual	0.999	0.945	0.968	1.069	1.091*	1.062
	(-0.04)	(-1.26)	(-1.10)	(1.73)	(2.47)	(1.75)
Bargaining*WF annual	0.997	0.994*	0.997	0.999	0.997	0.999
	(-1.95)	(-2.14)	(-1.56)	(-0.54)	(-1.74)	(-0.42)
Country controls						
EPLP		1.515			2.16	
		(1.1)			(1.76)	
Bargaining coverage		0.999			1.030***	
		(-0.13)			(6.24)	
EPLP*bargaining		1.038**			0.942***	
		(2.72)			(-3.69)	
Unemployment rate		0.862			1.02	
		(-1.49)			(0.25)	
EPL FTC		0.976			0.770*	
		(-0.15)			(-2.27)	
EPL TAW		1.097			0.705**	
		(0.59)			(-2.74)	
EPLP*EPL FTC		0.589			4.441***	
		(-1.38)			(3.6)	
EPLP*EPL TAW		0.775			1.268	
		(-1.23)			(1.45)	
EPL FTC*bargaining		1.007			0.997	
		(1.57)			(-0.87)	
EPL TAW*bargaining		0.996			1.007*	
		(-1.01)			(2.29)	
Establishment size	yes	yes	no	yes	yes	no
Sectors	yes	yes	no	yes	yes	no
Country dummies	yes	no	yes	yes	no	yes
Establishments	18407	18407	18407	18407	18407	18407
Countries	20	20	20	20	20	20

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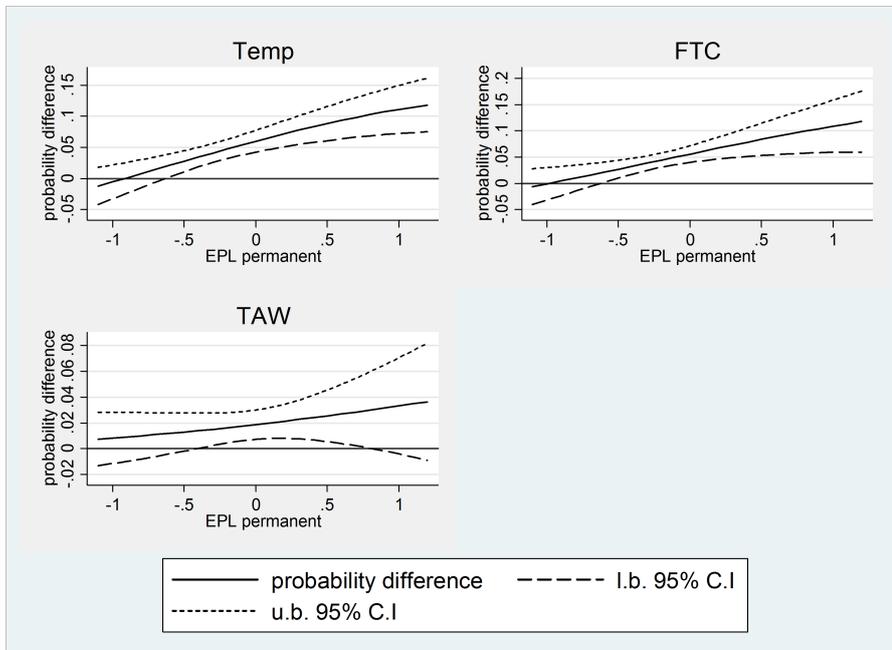
Table 3.5 – *Continued from previous page*

Dependent variable	If any FTC		If any TAW			
	(4)	(5)	(6)	(7)	(8)	(9)
LL	-9060	-9601	-9703	-8340	-8623	-8880

Note: \*\*\* significant at 0.1 per cent, \*\* significant at 1 per cent, \* significant at 5 per cent. Coefficients are reported as odds ratios and are from logistic regression models with clustered standard errors, z-values in parentheses. Continuous variables are centered. WF stands for workload fluctuation; EPLP is employment protection legislation for permanent workers; EPL TAW is employment protection for temporary agency workers; EPL FTC is employment protection for fixed-term contract workers; no. workers up means that the number of workers increased; no. workers down means that the number of workers decreased; absent means that the level of absenteeism and/or sickness causes human resource problems; high-skilled share means the proportion of employees working in high-skilled jobs which usually require an academic degree, flexible working time. Establishment-level variables described in Table 3.1. Country-level variables described in Table 3.2.

### 3.7.2 Robustness: employer weights

Figure 3.15: Does the positive relation of fluctuation with the probability of employing Temps (or FTCs, TAWs) differ significantly with EPLP in 2009 using employer weights?



Note: Difference in the predicted probabilities of employing at least one temp (FTC or TAW) of establishments with annual fluctuation versus establishments without fluctuation, Model (2), (5) and (8) with employers weight, calculated at zero for all institutions (bargaining coverage rate, EPL for temporary workers (Temps) (Model (2)), EPL for fixed-term contract (FTC) workers, EPL for temporary agency workers (TAW)), except EPL permanent. 95% confidence interval (C.I.) presented with the lower bound (l.b.) and upper bound (u.b.).

### 3.7.3 Description of the original sample and the estimation sample

Comparing the original 2009 ECS with the selected sample, we drop a major share of observations. Therefore, we provide a full description of data availability for the 30 initial countries in the 2009 ECS. Establishment data are presented in for the original sample in Table 3.6 and for the estimation sample in Table 3.7. We explain in detail why we lost observations. The initial number of establishments is 27,160 out of 30 countries which are: Austria, Belgium, Bulgaria, Croatia, Cyprus, the Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Italy, Ireland, Latvia, Lithuania, Luxembourg, Malta, the Netherlands, Poland, Portugal, Romania, Slovakia, Slovenia, Spain, Sweden, Turkey, the United Kingdom and the Former Yugoslav Republic of Macedonia.

Out of 27,160 establishments, there are 73 and 64 establishments with missing values in FTC and TAW and 142 (210, 201) establishments with missing values in yearly fluctuation (weekly, daily). This leaves us with 26,649 establishments. EPL TAW and EPL FTC is not available in 2008 for Bulgaria, Cyprus, Lithuania, Malta, Romania, Croatia and the Former Yugoslav Republic of Macedonia. This leaves us with 22,802 observations. Furthermore, the variables on temporary employment are not comparable for Spain and Italy (Eurofound, 2010b), which are 1,509 and 1,502 establishments, respectively. Hence, 20 countries and 19,791 establishments are left. Excluding the public owned establishments makes 19,711 observations. Excluding missing values in the other micro-variables further reduces the sample to 18,407 observations. The majority of missing values (approximately 1,000) relate to the variables on the gender and high-skilled share.

This leaves us with only one-third of the original ECS countries. One might be worried that the resulting variation of EPL remains sufficient to identify the coefficients. Although, we are left with only 20 countries, we fortunately do not suffer in terms of variation in EPL. The maximum and the minimum of the EPL indicators do not change when the ten countries are dropped (see Tables 3.8 and 3.9). Standard deviation even increases for EPL for permanent workers, EPL for temporary workers and EPL for fixed-term workers.

Table 3.6: Summary statistics for establishment-level variables (2009): original sample

Variable	Mean	SD	Min	Max	N
If any temp	0.65	0.48	0	1	27160
If any TAW	0.27	0.44	0	1	27096
If any FTC	0.58	0.49	0	1	27087
Share of FTC	8.39	18.81	0	100	26169
If any WF daily	0.31	0.46	0	1	26950
If any WF weekly	0.42	0.49	0	1	26959
If any WF annual	0.63	0.48	0	1	27018
If any freelancer	0.23	0.42	0	1	27031
If any works council	0.50	0.50	0	1	27160
No. of workers increased between 2006 and 2009	0.34	0.47	0	1	27160
No. of workers decreased between 2006 and 2009	0.27	0.44	0	1	27160
If high absenteeism and/or sickness rates (absent)	0.16	0.37	0	1	27035
Gender share (centered)	41.41	30.09	0	100	26347
High-skilled share (centered)	24.67	28.81	0	100	26126
If flexible working time schemes	0.55	0.50	0	1	26986
Establishment size (1-10)	3.43	2.78	1	10	27160
NACE C-E	0.31	0.46	0	1	27160
NACE F	0.10	0.30	0	1	27160
NACE G	0.15	0.35	0	1	27160
NACE H	0.04	0.18	0	1	27160
NACE I	0.05	0.21	0	1	27160
NACE J	0.02	0.14	0	1	27160
NACE K	0.09	0.29	0	1	27160
NACE L	0.06	0.24	0	1	27160
NACE N	0.07	0.25	0	1	27160
NACE O	0.04	0.20	0	1	27160

Note: Source is ECS 2009 (Eurofound, 2010b). Descriptive statistics with employer weights. Temporary workers (temp), temporary agency worker (TAW), fixed-term contract worker (FTC), workload fluctuation (WF), number (no.). High absenteeism means that an establishment encounters a human resource problem due to absenteeism and/or sickness. The share of high-skilled means the proportion of employees working in high-skilled jobs which usually require an academic degree. NACE Rev. 1.1: C-E Manufacturing and energy; F Construction; G Wholesale and retail trade, repair of goods; H Hotels and restaurants; I Transport and communication; J Financial intermediation; K Real estate and business activities; L Public administration; M Education; N Health and social work; O Other community, social and personal services.

Table 3.7: Summary statistics for establishment-level variables (2009): estimation sample

Variable	Mean	SD	Min	Max	N
If any temp	0.67	0.47	0	1	18407
If any TAW	0.30	0.46	0	1	18407
If any FTC	0.60	0.49	0	1	18407
Share of FTC	8.77	18.97	0	100	17995
If any WF daily	0.30	0.46	0	1	18407
If any WF weekly	0.42	0.49	0	1	18407
If any WF annual	0.63	0.48	0	1	18407
If any freelancer	0.22	0.42	0	1	18407
If any works council	0.49	0.50	0	1	18407
No. of workers increased between 2006 and 2009	0.36	0.48	0	1	18407
No. of workers decreased between 2006 and 2009	0.27	0.44	0	1	18407
If high absenteeism and/or sickness rates (absent)	0.17	0.38	0	1	18407
Gender share (centered)	40.53	30.12	0	100	18407
High-skilled share (centered)	23.73	28.30	0	100	18407
If flexible working time schemes	0.57	0.49	0	1	18407
Establishment size (1-10)	3.42	2.81	1	10	18407
NACE C-E	0.32	0.47	0	1	18407
NACE F	0.10	0.30	0	1	18407
NACE G	0.14	0.35	0	1	18407
NACE H	0.03	0.18	0	1	18407
NACE I	0.05	0.21	0	1	18407
NACE J	0.02	0.14	0	1	18407
NACE K	0.10	0.30	0	1	18407
NACE L	0.06	0.23	0	1	18407
NACE N	0.08	0.26	0	1	18407
NACE O	0.04	0.20	0	1	18407

Note: Source is ECS 2009 (Eurofound, 2010b). Descriptive statistics with employer weights. Temporary workers (temp), temporary agency worker (TAW), fixed-term contract worker (FTC), workload fluctuation (WF), number (no.). High absenteeism means that an establishment encounters a human resource problem due to absenteeism and/or sickness. The share of high-skilled means the proportion of employees working in high-skilled jobs which usually require an academic degree. NACE Rev. 1.1: C-E Manufacturing and energy; F Construction; G Wholesale and retail trade, repair of goods; H Hotels and restaurants; I Transport and communication; J Financial intermediation; K Real estate and business activities; L Public administration; M Education; N Health and social work; O Other community, social and personal services.

Table 3.8: Summary statistics for country-level variables (2009): original sample

Variable	Mean	SD	Min	Max	N
EPLP	2.309	0.545	1.170	3.510	24786
EPL temp	2.374	1.153	0.290	4.880	24786
EPL FTC	2.018	1.312	0.250	4.250	23215
EPL TAW	2.805	1.359	0.333	5.500	23215
Bargaining coverage rate	63.831	28.646	10.000	100.000	26291
Unemployment rate	6.837	2.747	2.500	14.600	26640

Note: Temporary workers (temp), temporary agency worker (TAW), fixed-term contract worker (FTC), EPL for permanent workers (EPLP), EPL for temporary workers (EPL temp), EPL for temporary agency workers (EPL TAW), EPL for fixed-term workers (EPL FTC). Data sources: EPL 2008 from OECD (2012), national unemployment rate in the first quarter of 2009 Eurostat (2012), bargaining coverage rate Hayter and Stoevska (2011) and Eurofound (2007a).

Table 3.9: Summary statistics for country-level variables (2009): estimation sample

Variable	Mean	SD	Min	Max	N
EPLP	2.344	0.565	1.170	3.510	18407
EPL temp	2.290	1.165	0.290	4.880	18407
EPL FTC	1.925	1.367	0.250	4.250	18407
EPL TAW	2.653	1.336	0.333	5.500	18407
Bargaining coverage rate	63.754	28.780	11.300	100.000	18407
Unemployment rate	6.299	1.948	2.500	10.100	18407

Note: Temporary workers (temp), temporary agency worker (TAW), fixed-term contract worker (FTC), EPL for permanent workers (EPLP), EPL for temporary workers (EPL temp), EPL for temporary agency workers (EPL TAW), EPL for fixed-term workers (EPL FTC). Data sources: EPL 2008 from OECD (2012), national unemployment rate in the first quarter of 2009 Eurostat (2012), bargaining coverage rate Hayter and Stoevska (2011) and Eurofound (2007a).

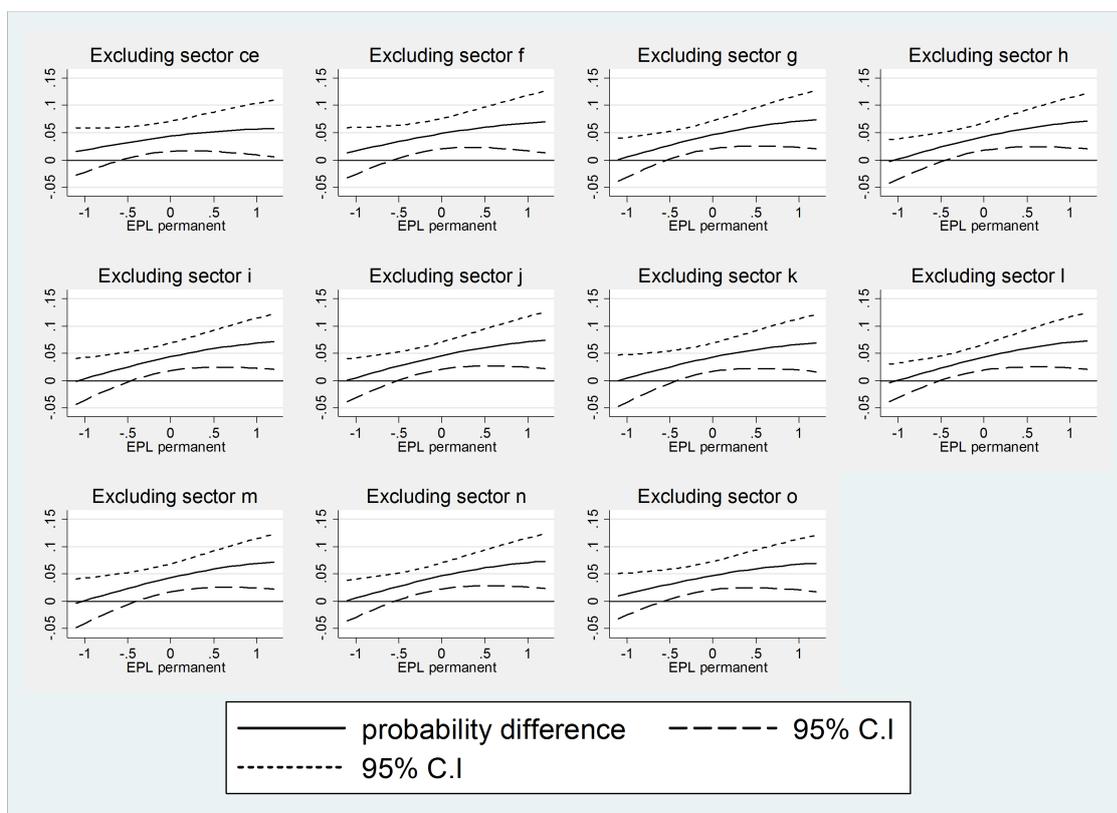
### 3.7.4 Description of the governance indicators

Three governance indicators of the World Bank (Kaufmann et al., 2004) were chosen to capture the degree of enforcement of EPL for permanent workers due to differences in governance: government effectiveness, rule of law and control of corruption. They are aggregated indicators on perceptions of governance. Government effectiveness as an aggregated indicator includes the quality of public service provision, as well as the independence of civil services from political pressure and the trustworthiness of the government's commitment to rules. Rule of law is an aggregated measure of the confidence in rules; for instance, the enforceability of contracts is included. All three indicators are normally distributed and range from around -2.5 to 2.5.

### 3.7.5 Robustness: sectors

We check whether our results are robust with reference to different sectors. We do not find that the results are driven by one specific sector (Figure 3.16). Sector-specific estimates are less robust, although these estimates partly suffer from a small number of observations (around 300).<sup>34</sup>

Figure 3.16: Does the positive relation of fluctuation with the probability of employing Temps differ significantly with EPLP in 2009?



Note: Difference (diff.) in the average of predicted probabilities of employing at least one temp of establishments with annual fluctuation against establishments without fluctuation, Model (2), calculated at zero for all institutions (bargaining coverage rate, EPL for temporary workers (Temps)), except EPL permanent. 95% confidence interval (C.I.) presented with the lower bound (l.b.) and upper bound (u.b.). Samples exclude firms of the sector mentioned in the title. C-E Manufacturing and energy; F Construction; G Wholesale and retail trade, repair of goods; H Hotels and restaurants; I Transport and communication; J Financial intermediation; K Real estate and business activities; L Public administration; M Education; N Health and social work; O Other community, social and personal services.

<sup>34</sup>Results are available upon request.

### 3.7.6 Comparison of strategies to deal with clustering

Table 3.10: Comparison of strategies to deal with clustering based on Cameron and Miller (2015)

Dependent variable	If any temporary worker		
	Logit	FGLS	ML RS
WF annual	1.316*** (3.95)	1.329*** (6.24)	1.335*** (5.17)
WF weekly	1.198* (2.26)	1.150* (2.09)	1.148*** (2.87)
WF daily	0.857* (-2.16)	0.894* (-1.96)	0.895* (-2.16)
EPLP*WF annual	1.283* (2.35)	1.147* (2.16)	1.161 (1.43)
EPLP	1.349 (0.90)	1.294 (0.68)	1.277 (0.62)
Establishment variables	yes	yes	yes
Establishment size fixed effect	yes	yes	no
Sectors fixed effect	yes	yes	no
Cross-level interactions	yes	yes	yes
Country fixed effect	yes	no	yes
Country variables	no	yes	no
Establishments	18407	18407	18407
Countries	20	20	20
LL	-9213	-8662	-8655

Note: Logistic regression models with clustered standard errors in Logit and FGLS, FGLS indicates feasible GLS estimation, ML RS indicates a Multilevel model with random slopes. \*\*\* significant at 0.1 per cent, \*\* significant at 1 per cent, \* significant at 5 per cent. Coefficients are reported as odds ratios, z-values in parentheses. Continuous variables are centered. Temps is temporary workers. WF stands for workload fluctuation; EPLP is employment protection legislation for permanent workers. Country variables: EPL for temporary workers, bargaining coverage, EPLP\*bargaining coverage, EPL for temporary workers\*bargaining coverage, EPLP\*EPL for temporary workers, unemployment rate. Establishment variables: freelancer, works council, number of workers increased, number of workers decreased, high absenteeism and/or sickness rates (i.e. absenteeism and/or sickness causes human resource problems), gender share, share of high-skilled workers (i.e. the proportion of employees working in high-skilled jobs which usually require an academic degree), flexible working time. Interaction between country and establishment variables: WF annual\*EPL for temporary workers, WF annual\*bargaining coverage. Establishment-level variables described in Table 3.1. Country-level variables described in Table 3.2.

## Chapter 4

# Employment protection reform effects on well-being

### 4.1 Introduction

Employment protection legislation for permanent contracts (EPLP) is a potential source of a high incidence of temporary employment and of youth unemployment (Kahn, 2007). In order to decrease adverse effects, liberalizing reforms in EPLP were proposed in the public debate in the aftermath of the 2007 financial crisis. Following this discussion, policy makers liberalized EPLP between 2008 and 2013, e.g., in Portugal, Spain, Italy, and Greece (OECD, 2013b). Such reforms are considered to be politically harmful because powerful permanent workers would suffer, while less powerful temporary workers would benefit (e.g. Rueda, 2005).

Previous research on the effects of EPLP, however, suggests that the effect of EPLP on well-being is not as clear. Search and matching models predict that job destruction and construction of permanent jobs increases when EPLP decreases. Marinescu (2009) and Boockmann et al. (2008) show that job stability might decrease. In moral hazard situations, a decrease in EPLP could decrease monitoring, as dismissals can be applied as disciplinary advices, and thereby, decreases stress (Lepage-Saucier and Wasmer, 2012). Hence, for permanent workers less job stability must be weighed against the reduced stress. Due to increased job construction, temporary workers could benefit from a more likely access into permanent jobs

(Centeno and Novo, 2012). Booth et al. (2002) showed that temporary work is associated with lower training, lower wages and less job satisfaction compared to permanent work. Workers, however, who remain in a temporary job after a reduction in EPLP might suffer due to the comparison to colleagues who transitioned into a permanent job.

In order to improve our understanding on effects of EPLP on well-being, I evaluate the effect of an increase and a decrease in German EPLP on life satisfaction as a proxy for well-being. The identification strategy relies on German EPLP reforms which changed EPLP for small firms only. Due to this subgroup and time variation, I am able to employ the reforms as quasi-experiments in a difference-in-difference approach (DID). As a major share of permanent workers was almost not affected by the reforms in EPLP, the focus is on temporary workers. Using the longitudinal German Socio-Economic Panel (GSOEP), I account for individual fixed effects.

A major drawback of the GSOEP, however, is that the treatment group can not be measured precisely, and hence, incorporates measurement errors. It is, therefore, likely that the estimates of the effect of EPLP on well-being are biased towards zero. This has to be kept in mind when interpreting the results. Importantly, in order to address potential violation of the common trend assumption and worker selection, I control for both observables and time-invariant unobservables and conduct placebo tests as well as pre-treatment trend tests. If the reforms induce selection, I capture for this, if the process can be explained by observables or time-invariant unobservables. When interpreting the results, this has to be kept in mind.

This paper contributes to the growing literature which employs evaluation techniques to study effects of labor market institutions and policies on well-being (e.g. Hamermesh, Kawaguchi and Lee, 2014; Dorsett and Oswald, 2014). Within this literature and to the best of my knowledge, this paper is the first that combines standard evaluation techniques for the effect of reforms in employment protection on objective outcomes<sup>1</sup> with the literature on determinants of life satisfaction.<sup>2</sup>

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<sup>1</sup>For instance, Leonardi and Pica (2013), Scoppa (2010), Martins (2009), Kugler and Pica (2008), Boockmann et al. (2008), and Bauer et al. (2007).

<sup>2</sup>For instance, Frey and Stutzer (2012), Clark and Senik (2010), Kassenboehmer and Haisken-DeNew (2009), and Clark et al. (2008).

Boarini et al. (2013) and Oxsen and Welsch (2012) analyze the relation between employment protection and life satisfaction based on within-country variation of employment protection and pooled cross-sectional data but do not investigate within-country subgroup variation.<sup>3</sup> Thereby, they cannot easily rule out concerns about unobserved confounding and reversed causality.<sup>4</sup> Busk et al. (2015) (DID with propensity score matching), Lepage-Saucier and Wasmer (2012) (DID) and Kuroki (2012) (DID) are exceptions in the literature of employment protection and well-being. They examine the effect of employment protection on stress (Lepage-Saucier and Wasmer, 2012) and job satisfaction (Busk et al., 2015; Kuroki, 2012) but not on life satisfaction.

Furthermore, this paper is the first to study the effect of employment protection for permanent workers on life satisfaction. Boarini et al. (2013) and Oxsen and Welsch (2012) do not differentiate between protection for permanent versus temporary contracts. Other studies on employment protection and well-being which account for this difference investigate job satisfaction, perceived job security and stress but not life satisfaction (Lepage-Saucier and Wasmer, 2012; Salvatori, 2010; Clark and Postel-Vinay, 2009; Kuroki, 2012). Lastly, this paper investigates effect heterogeneity and discusses potential mechanisms for the effect of employment protection legislation on life satisfaction.

The main finding is that the decrease in EPLP in 1996 decreased life satisfaction of temporary workers by 6% of the mean in life satisfaction. An explanation for this is that temporary workers who remain in a temporary job suffer from the comparison to colleagues who successfully transitioned into a permanent job after the decrease in EPLP. This interpretation is in line with the finding of Centeno and Novo (2012) that EPLP adversely affects transition probabilities from temporary to permanent work and is in line with the literature on social comparison (e.g. Clark and Senik, 2010). Selection of workers is accounted for as long as this is due to unobserved time-invariant heterogeneity or observed heterogeneity. Unobserved time-variant heterogeneity might remain an issue. Pre-reform trend

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<sup>3</sup>Salvatori (2010) and Clark and Postel-Vinay (2009) investigate employment protection and job satisfaction or job security but do not investigate within-country subgroup variation, too.

<sup>4</sup>Reversed causality is a potential crucial issue as, for instance, workers who are worried about job security demand, as a consequence, higher EPLP from political actors (Clark and Postel-Vinay, 2009).

tests, however, do not reject that control and treatment group follow a common trend controlling for above mentioned heterogeneity. Furthermore, I find that less employable workers are specifically strong affected by a decrease in EPLP. The negative effect returns back to zero after one year which might be explained by adaptation (e.g. Clark et al., 2008). I find no effect of the increase in EPLP (1999) on temporary workers which could be explained by the notion that losses are valued stronger than gains (e.g. Boyce, Wood, Banks, Clark and Brown, 2013). As the majority of permanent workers were not strongly affected by the EPLP reform, I do not expect effects on their well-being. Indeed, I do not find any. Due to the measurement error in the treatment status, however, all effects should be considered as lower bounds.

The paper is organized as follows: The next section develops hypotheses on the effect of employment protection on well-being. Following that, I present the institutional background in Section 4.3. The fourth Section introduces the identification strategy and data. Section 4.5 presents the results of the empirical analyses, and in the final section, I conclude.

## 4.2 Related literature

Employment protection regulations regulate the hiring and firing of workers with temporary contracts and/or with permanent contracts. A temporary contract finishes after a specified period of time, while a permanent contract is open-ended in its duration. These employment protection regulations are based on formal legislation, collective bargaining, and court interpretation of legislation. In this paper, I focus on formal employment protection legislation for permanent contracts (EPLP), regulating issues like the period of notice for termination, specific forms of dismissal, or severance payments. Thereby, stronger EPLP increases adjustment costs of the workforce at the firm level.

In labor economics, firm-level adjustment costs are often modeled in dynamic labor demand models (e.g. Boeri and Garibaldi, 2007; Nunziata and Staffolani, 2007) and in search and matching models (e.g. Cahuc et al., 2012; Boeri and van Ours, 2013; Boeri, 2011; Cahuc and Zylberberg, 2004). This literature suggests that employment protection has an effect on job destruction and creation, flows

into and out of employment, but an ambiguous effect on employment levels.<sup>5</sup>

Relying on labor economics and the empirical literature on well-being, I derive hypotheses on the effect of EPLP on well-being.<sup>6</sup> Well-being is considered to be a function of current income, expected income, and relative social status. The expected well-being from a permanent job is assumed to be higher than from a temporary job as workers in a temporary job exhibit a higher probability of becoming unemployed in the future.

### 4.2.1 Employment flows

Search and matching models incorporate employment protection via firing costs, which alter the profit function of firms. Boeri (2011) models the effects of an increase in firing costs for permanent workers in a labor market which allows the existence of temporary and permanent contracts in his model. An increase in firing costs yields a decrease in job destruction of permanent workers and a decrease in the conversion of temporary jobs to permanent jobs. Employing micro-level data and reforms which increased EPLP, Kugler and Pica (2008) find that separation from and access to permanent work decreases, Centeno and Novo (2012) show that transition probabilities decreased, Boockmann et al. (2008) show that job stability increased, and Marinescu (2009) finds a decrease in the firing hazard.

Based on these findings, a change in EPLP could affect well-being in several ways. Some temporary workers might benefit from a decrease in EPLP by actually transitioning from a temporary into a permanent job where expected income is higher.<sup>7</sup> Workers who remain in a temporary job might benefit from higher probability of access into a permanent job in terms of employment prospects which may then increase the expected income of temporary jobs. I refer to this positive effect of a decrease in EPLP on well-being as the *transition hypothesis*. Temporary

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<sup>5</sup>For an overview of the literature, see Boeri and van Ours (2013), OECD (2013b), Cahuc and Koeniger (2007), and Cahuc and Zylberberg (2004). Furthermore, a reduction in EPLP might have an effect on productivity (Cappellari et al., 2012) and might either increase (Lazaer, 1990) or decrease (Lindbeck and Snower, 2001) the wages of permanent workers.

<sup>6</sup>See Appendix 4.7.7 for channels via job security and job satisfaction. Due to less convincing common trend assumptions (Appendix 4.7.2), results are not part of the main paper and no implications are derived from the analyses.

<sup>7</sup>Booth et al. (2002) show that temporary compared to permanent jobs are associated with lower wages, job satisfaction and training opportunities.

workers also, however, might suffer from a decrease in EPLP because the protection of their future job decreases (Salvatori, 2010): *anticipation hypothesis*. Due to the decrease in EPLP, permanent workers might perceive an increase in both the probability of separation from their jobs and the probability of transitioning into unemployment or into a temporary job so that the expected income of permanent jobs would decrease.<sup>8</sup> I refer to this negative effect on permanent workers' well-being of a decrease in EPLP as the *insecurity hypothesis*.

Relative social status might change as well. The relative status, e.g. in terms of relative income, is a crucial determinant of well-being, which was shown in the empirical literature on well-being and in behavioral economics (e.g. Karacuka and Zaman, 2012; Clark et al., 2008; Luttmer, 2005; Falk and Knell, 2004). A milestone in the literature on well-being and relative positions was the seminal article of Easterlin (1974). Despite substantial increases in wealth and the finding that income is positively related to well-being across countries and across individuals within countries, he finds no substantial increase in happiness within countries.<sup>9</sup> In order to explain this "paradox", social comparison and adaptation are discussed as potential explanations. Concerning social comparison, Clark and Senik (2010) show that income comparison is highly relevant for well-being and that people often compare themselves with their colleagues. When EPLP decreases, an increased amount of temporary colleagues might improve their status by moving into a permanent job. Hence, temporary workers who remain in a temporary job after the reform are worse off than former temporary colleagues who transitioned into a permanent job. Thus, a decrease in EPLP could decrease the temporary workers' well-being who remained in a temporary job through the mechanism of comparison. In the following, I refer to this argument as the *comparison hypothesis*.

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<sup>8</sup>Perceived job security might decrease, too. For the positive relation between perceived job security and life satisfaction in economics, see: Praag, Frijters and Ferrer-i-Carbonell (2003), Geiskecker (2012), Green (2011), Campbell, Carruth, Dickerson and Green (2007). For the relation between job security and life satisfaction in psychology, see: Cheng and Chan (2008), De Witte (2005) and Sverke, Hellgren and Näswall (2002).

<sup>9</sup>For recent controversial discussion on this "paradox", see Easterlin, McVey, Switek, Sawangfa and Zweig (2010) and Stevenson and Wolfers (2008).

### 4.2.2 Moral hazard and monitoring

EPLP might also change monitoring of permanent workers. In a moral hazard situation between permanent workers and employers, dismissals can serve as disciplinary devices. Higher employment protection makes these devices more costly, and employers dismiss less often. Thereby, the value of jobs for shirkers increases in efficiency wage models. In this situation, the employer might raise monitoring in order to avoid shirking. A decrease in EPLP, therefore, might decrease monitoring, and hence, stress.<sup>10</sup> Indeed, Lepage-Saucier and Wasmer (2012) show that EPLP is positively related with stress. Hence, permanent workers might benefit in terms of well-being: *monitoring hypothesis*.

### 4.2.3 Employability as a loss multiplier?

Previous research shows that perceived employability is an important mediator of the effect of unemployment and perceived job insecurity on well-being (Green, 2011). Individuals who perceive themselves as less employable - measured as low expectations to find a good job - suffer more from unemployment and perceived job insecurity. Psychologists explain this by the degree of perceived dependency on the current job, which is higher when perceived employability is low.

In this study, I explore whether changes in EPLP affect workers differently depending on their perceived employability. For instance, temporary workers who perceive their employability as low might have much stronger preferences for a permanent job than others because they expect to face major difficulties in finding a new job. Hence, when they do not manage to transition, even though the propensity to do so increased, they could suffer even stronger when comparing to colleagues who transitioned. The same applies to permanent workers.

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<sup>10</sup>Furthermore, less monitoring (personal control) is positively related to job satisfaction (Warr, 2003). For the relation between job satisfaction and life satisfaction in economics, see (Praag et al., 2003). For the effect of job satisfaction on life satisfaction in psychology, see Warr (2003), Iverson and Maguire (2000) and Judge and Locke (1993).

## 4.3 Institutional background

### 4.3.1 Employment protection in Germany

In international comparison, Germany's dismissal protection for permanent contracts ranked among the top five of OECD countries in 2013 (Venn, 2009; OECD, 2015). Hence, it ranks similarly to Portugal and France, but much higher compared to United Kingdom and United States. German EPLP is regulated in the Protection Against Dismissal Act, in the Civil Code, and in laws for specific groups such as disabled workers. The latter two regulations apply to all firms and define minimum criteria for a fair dismissal (e.g. written form, specific period of notification, and application of good faith, basic rights). In the case of an unfair dismissal, the court decides over severance payments.

The Protection Against Dismissal Act, in contrast, only applies to firms that pass a threshold in terms of the number of employees and defines stricter rules which have to be met for a fair dismissal. Dismissals are only considered fair under EPLP regulations if: 1) the cause lies in the worker (e.g. long-term incapacity), 2) the worker's behavior is deemed damaging or unacceptable (e.g. theft), or 3) it is an economic necessity. A dismissed worker has the right to bring the case to court but only if s/he did not forgo this right by accepting severance payments. In case of an unfair dismissal, the worker has the right to return to the firm or to claim severance payments. Hence, the Protection Against Dismissal Act increases adjustment costs in terms of transfers, e.g. severance payments, and taxes, e.g. procedural costs of dismissals, only for firms above a specific threshold. In the following, I refer to this German legislation as EPLP.

### 4.3.2 Reforms in employment protection

This paper investigates variation in EPLP across firm size (threshold regulation) and variation across time (reforms in 1996 and 1999). Figure 4.1 shows which firms depending on firm size [measured in full time equivalent employees (FTE)] are required to meet regulations of the EPLP between 1995 and 2000. Before the reform in 1996, all workers in firms with more than 5 FTE were covered by the EPLP. The Christian Democrat/Liberal government decided on reforms in

order to increase the flexibility of the labor market. On the 1st of October in 1996, the minimal number of FTE was increased from 5 to 10 FTE for newly hired permanent workers, i.e. contracts were signed after the 30th of September in 1996.<sup>11</sup> This means that small firms with less than 10 FTE did not have to apply the EPLP for newly hired permanent workers anymore. From this date on, newly hired permanent workers in small firms could be dismissed much more easily. Incumbents who signed the permanent contract before the reform took place (1st of October in 1996), however, were exempted from the reform until September 1999. For these workers only a decrease in future EPLP (September in 1999) became effective on the 1st of October in 1996. Thereby, incumbent permanent workers faced no direct change in EPLP on the reform date.

The second reform took place on the 1st of January in 1999. In this reform, the Social Democrat/Green government re-regulated the law and returned to the old threshold.<sup>12</sup> Thereby, newly hired permanent workers, i.e. whose contracts were signed after the 30th of September 1996, who were employed in firms with 6 to 10 FTE faced an increase in employment protection on the 1st of January in 1999. Incumbent workers, i.e. whose contracts were signed before the 1st October in 1996, the reform in 1996 did not change EPLP. Only incumbents who signed the contract after September in 1996 faced an increase in EPLP. According to OECD (2015), the share of workers with a job tenure more than three years in total employment was 27.9 percent in 1999. Including temporary and permanent workers, however, this figure indicates that only a small share of permanent workers were affected by the increase in EPLP. Therefore, the policy effects for permanent workers should be understood as a lower bound estimates. The 1999 reform took place after elections in September 1998, in which this reform was already strongly discussed (Bauer et al., 2007). Therefore, I discuss anticipation of the reform in the empirical analyses (Section 4.5).

Finally, for identification issues, it is important whether parallel reforms took place. On the 1st of October in 1996, the regulation of fixed-term work was liberalized (increasing the maximum duration from 18 to 24 months and allowing

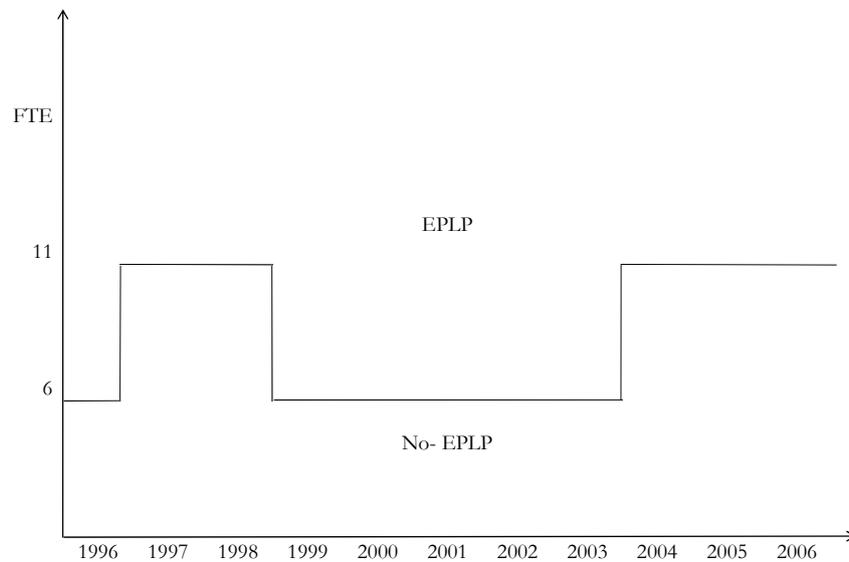
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<sup>11</sup>Social selection criteria in the case of economic redundancies were loosened. The FTE calculation changed.

<sup>12</sup>The FTE calculation changed slightly and the selection criteria were strengthened.

renewals up to three times).<sup>13</sup> These reforms, however, apply to all firms and workers.

Figure 4.1: EPLP reforms in Germany from 1996 to 2005



Note: Own presentation; FTE: full-time equivalent workers; EPLP: employment protection legislation for permanent workers.

## 4.4 Empirical strategy

### 4.4.1 Identification strategy

The effect of EPLP on well-being is identified by exploiting variable enforcement across firm-size and within-country time variation of EPLP (Boeri and Jimeno, 2005). Employing these kinds of variations in a difference-in-difference approach

<sup>13</sup>Furthermore, in 1997 temporary agency work was liberalized and in 2001 fixed-term work as well as temporary agency work was liberalized.

became a standard tool for causality analyzes of the effect of EPL reforms on objective outcomes (e.g. Leonardi and Pica, 2013; Martins, 2009; Kugler and Pica, 2008; Bauer et al., 2007; Boeri and Jimeno, 2005).

The threshold regulation reforms in 1996 and 1999 in Germany serve as quasi-experiments. These reforms in German EPLP were already evaluated in their effects on, for instance, employment dynamics (Bauer et al., 2007), job duration (Boockmann et al., 2008), temporary employment (Boockmann and Hagen, 2001). Figure 4.1 shows that the reforms generated a subgroup of firms which faced a change in EPLP and a subgroup of firms, for whom EPLP did not change. Workers who are employed in firms with 6-10 FTE are defined to be treated (the treatment group), while workers in firms above 10 FTE serve as controls (the control group). I compare the change in well-being for the treatment group to that of the control group. The difference-in-difference estimator is the effect of EPLP on well-being if the identifying assumption of a common trend is true.

The effect of EPLP is estimated by the following empirical specification:

$$Y_{it} = \gamma_1 TG_i + \gamma_2 TG_i R_t + \gamma_3 R_t + \beta' \mathbf{X}_{it} + \epsilon_{it} \quad (4.1)$$

$$R_t = 1[\text{year} \geq \text{reform year}_t] \quad (4.2)$$

$$\epsilon_{it} = u_{it} + a_i \quad (4.3)$$

$Y_{it}$  is the dependent variable which is measured at the level of individual  $i$  in time  $t$ ,  $TG_i$  is the dummy for being in the treatment group or not,  $R_t$  is the reform dummy,  $\mathbf{X}_{it}$  represents a vector of covariates and  $\epsilon_{it}$  is the error term.  $\mathbf{X}_{it}$  contains determinants which are important for well-being equations with well-being proxied by life satisfaction. In the baseline model,  $TG_i$  is time-invariant. It equals one if an individual works in a small firm at the time of the reform and zero if an individual works in a large firm ( $TG_i$ ).<sup>14</sup>  $TG_i$  captures group specific time-invariant differences between the treatment and the control group which are not linked to the reform. The coefficient of the interaction between the reform dummy ( $R_t$ ) and the treatment group dummy ( $TG_i$ ) is the main measure of interest: the

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<sup>14</sup>In other specifications,  $TG_i$  is time-variant and equals one if an employee works in a small firm in the year of observation but zero otherwise ( $TG_{it}$ ).

policy effect ( $\gamma_2$ ).

In order to check whether there are pre-treatment trend differences and whether the policy effect fades or grows, I add pre-reform and post-reform policy effects similar to Autor (2003). The additional included reform dummies are coded as follows

$$R_{t-1} = 1[\text{year} \geq \text{reform year}_{t-1}] \quad (4.4)$$

$$R_{t+1} = 1[\text{year} \geq \text{reform year}_{t+1}] \quad (4.5)$$

The error term  $\epsilon_{it}$  contains a time-invariant individual fixed effect  $a_i$  and an idiosyncratic component  $u_{it}$ . Individual fixed effects are very important for well-being equations since time-invariant personality traits have a large effect on well-being (Ferrer-i-Carbonell and Frijters, 2004). It is assumed that  $Y_{it}$  is cardinal. According to Ferrer-i-Carbonell and Frijters (2004), the cardinality versus ordinality assumption is relatively unimportant for well-being measured as life satisfaction on a 0 to 10 scale.<sup>15</sup> I estimate a variance-covariance matrix, which accounts for the possible correlation of the errors at the individual-level as well as for heteroscedasticity.

The identifying assumption for DID analyzes is the common trend assumption. The treatment and control group are allowed to differ in terms of the outcome, but this difference is not allowed to change over time. The assumption fails if the composition of treatment and control group change, if groups differed in their time varying covariates, or if a constantly different composition induced diverging dynamics in the outcome. The policy effect  $\gamma_2$  would therefore be biased. By including the covariates  $\mathbf{X}_{it}$  and allowing for unobserved time-invariant heterogeneity, I generalize the common trend assumption: Conditional on mentioned controls the treatment and control group are assumed to have the same trend in the dependent variable. To assess the plausibility of the assumption, I run placebo reform tests, placebo group tests, and pre-reform trend tests.<sup>16</sup>

Concerning endogenous selection, for example, workers with children might prefer highly protected jobs. After a decrease in protection in small firms, workers

<sup>15</sup>The authors compare fixed effect ordered logit models, ordered logit and fixed effect OLS.

<sup>16</sup>For life satisfaction, the tests support the common trend. See Section 4.5 and Appendix 4.7.2.

with children would endogenously sort into bigger firms and bias the policy effect. In order to tackle these issues, observable differences and time-invariant unobservable differences are controlled for by including  $\mathbf{X}_{it}$  and estimating fixed-effects. I cannot rule out any concerns related to unobservable time-variant heterogeneity but I discuss this issue by referring to pre-treatment trend tests.

#### 4.4.2 Data

The data source is the German Socio-Economic Panel (GSOEP). The GSOEP is a representative survey of currently more than 11,000 private households and 20,000 individuals in Germany. The first wave was conducted in 1984 and has been repeated annually since then. Haisken-DeNew and Frick (2005) present technical details. The major advantage of the GSOEP for well-being equations is its longitudinal structure. The major disadvantage of the GSOEP for this study is that the treatment group variable is associated with measurement error (discussed below). This biases the effect towards zero.

#### Variables

The dependent variable of interest is well-being ( $Y_{it}$ ). Well-being is proxied by overall life satisfaction, which is a retrospective evaluation of life (Kahneman and Krueger, 2006).<sup>17</sup> The GSOEP contains the standard single-item life satisfaction question (Kahneman and Krueger, 2006), "How satisfied are you with your life, all things considered? Completely dissatisfied (0) - completely satisfied (10)." With regard to the statistical quality, this single-item life satisfaction question is

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<sup>17</sup>In the economic literature, well-being proxied as life satisfaction is usually linked to the concept of utility (Kahneman and Krueger, 2006). It is distinguished between expected (decision) utility (Clark et al., 2008; Benjamin, Hefetz, Kimball and Rees-Jones, 2012), experienced utility (Kahneman and Krueger, 2006; OECD, 2013b), and remembered utility (Kahneman and Krueger, 2006). The latter is a weighted average of experienced utility. Life satisfaction is considered as remembered utility (Kahneman and Krueger, 2006). With respect to the relation between life satisfaction and expected utility, expected utility is not necessarily equal to remembered utility as individuals make systematic computational mistakes, e.g. by neglecting adaptation (Clark et al., 2008). Even if individuals would not make computational mistakes with regard to the consequences of their choices for utility, they would not solely maximize life satisfaction (remembered utility) but would consider other aspects (Benjamin et al., 2012; Clark et al., 2008).

considered to be a reliable and valid measure in several studies. One of the most recent reviews of this literature is given in OECD (2013b) and in Clark et al. (2008).<sup>18</sup> Of course, limitations have to be taken into account (OECD, 2013b), e.g. occasion-specific events, and placement in the survey.

Concerning the contract type of a worker, I define permanent workers as workers holding an unlimited contract, while temporary workers are defined as workers holding a limited contract. The temporary workers are either temporary agency workers - workers who signed a contract with a private employment agency and were hired by third party firms - or workers with a fixed-term contract who were directly hired by the firm. Before 1995, the GSOEP contains only insufficient information on contract types.

In accordance to Green (2011), I proxy employability by the perceived easiness of finding a new job.<sup>19</sup> Workers are defined to perceive their employability as low if they answered that it would be difficult or practically impossible to find a comparable new job. This variable is available for 1997 and 1999.

I also include several control variables ( $\mathbf{X}_{it}$ ) which are important for well-being equations and usually included in such estimations (e.g. Kassenboehmer and Haisken-DeNew, 2009; Clark and Senik, 2010): household net income, working hours, age, education, female dummy, whether children live in the household, year (linear trend), state dummies (regional labor market effects), and year fixed effects (year specific macro effects).

## Treatment Group Dummy

The treatment group is defined via the number of FTEs. Figure 4.1 shows that workers in firms with 6 to 10 FTEs versus workers in firms above 10 FTEs should

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<sup>18</sup>OECD (2013b) and Clark et al. (2008) present literature on the facts that life satisfaction is correlated with real phenomena such as brain activity and smiling, that third party evaluation correlates the respondent's own report, that satisfaction measures have objective consequences (Oswald, Proto and Sgroi, 2013; Krause, 2013), and that life satisfaction has robust relationships with, e.g. health, income, and unemployment (e.g. Kassenboehmer and Haisken-DeNew, 2009; Luttmer, 2005).

<sup>19</sup>The GSOEP question is: "If you were to lose your job, would it be easy, difficult, or practically impossible for you to find a comparable job?". Outcomes are: "easy (1), difficult (2) or practically impossible (3)".

be identified.<sup>20</sup> In the EPLP, FTEs are measured by subtracting the number of workers in training from the overall number of employees and weighting the part-time workers by a specific key.

A major drawback of the GSOEP is that FTEs cannot be measured precisely. The GSOEP asks, "Approximately how many people does the company employ as a whole?" [less than 5, 5-19, 20-199 (99, 100-199 after the year 1998), 200-1999, at least 2000 workers, self-employed without coworkers]. Workers who answered that they were in firms with 5-19 (20-199) workers are defined as the treatment (control) group. Thereby, the treatment group probably includes workers who were not treated. At the same time, the control group might include workers who were actually treated. Hence, the policy effect is likely to be biased towards zero, and the true effects of EPLP are stronger.

I define two different treatment group dummies. First, for the time-invariant treatment group dummy ( $TG_i$ ) the treatment status is measured at the time of the last interview before the reform takes place. The dummy is defined to be one, if the worker is employed in a firm with 5-19 employees at the time of the reform, and defined to be zero, if s/he works in a firm with 20-199 employees. The treatment status does not change over time, even if the number of workers in the firm changes. Second, the time-variant treatment group dummy ( $TG_{it}$ ) is measured at the year of observation. It is one, if the worker is employed in a firm with 5-19 employees in the year of observation, and zero, if s/he works in a firm with 20-199 employees in the year of observation. The treatment status is allowed to change. In the latter case, workers might enter or exit the sample due to changes in the number of workers in the firm. The advantage of the time-variant treatment group dummy is that the number of observations are higher, which is important in order to study effect heterogeneity.

### 4.4.3 Sample selection and descriptive statistics

I construct separate samples for temporary and for permanent workers as well as for both reforms. The samples are restricted to employees who are employed in

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<sup>20</sup>I choose larger firms as the control group, because Bauer et al. (2007) show that smaller firms face different dynamics with regard to insolvencies.

private firms, between 15 and 65 years old, without missing values in questions on job security as well as on job satisfaction.<sup>21</sup> Further, I exclude employees who are in the upper/lower 1st income percentile. Concerning the reform periods, I start observing individuals around two years before and two years after the reform. Therefore, for the 1996 reform, the sample period begins in 1995 and ends in 1998.<sup>22</sup> For the 1999 reform, I start in 1997 and end in 2001. It is not possible to start earlier because of the 1996 reform. Finally, the samples are restricted to workers for whom the treatment group dummy is defined.

Concerning permanent workers, the sample includes newly hired permanent workers who face an actual change in EPLP and incumbent workers who face only the announcement of a change in EPLP. This biases the effect of a direct change in EPLP towards zero. As the sample becomes very small, however, when it is restricted to newly hired workers, the main analyzes are conducted for the full sample of permanent workers.<sup>23</sup> Furthermore, the sample for permanent workers excludes workers in the probationary period as here EPLP does not apply.

Samples for each contract type (permanent workers, temporary workers) and for each reform period (1995 until 1998, 1998 until 2001) are generated:

1. **Sample A - workers irrespective of their employment status after the reform:** The effect of a change in EPLP on workers who were in a temporary or in a permanent contract at the time of the reform is analyzed. For this purpose, I construct a sample of temporary/permanent workers who were, at the time of their last interview prior to the reform, in a temporary/permanent contract as well as employed in private firms with 5-199 workers. The treatment group is time-invariant ( $TG_i$ ). For instance, if person A is in a temporary job in a firm with 20-199 employees in 1996, prior to the reform, but in a permanent job in 1997, the person is included in the sample of temporary workers but not in the sample of permanent workers. I allow the panel to be unbalanced.

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<sup>21</sup>Results for life satisfaction as the dependent variable are relatively robust to this restriction. See Appendix 4.7.5 for Sample B. Sample is restricted due to models in Appendix 4.7.7.

<sup>22</sup>Results for Sample B are robust to this restriction and are robust to the ending month in 1998. See Section 4.5.3.

<sup>23</sup>For results when I focus on newly hired workers, see Appendix 4.7.4.

2. **Sample B - workers remain in the employment status after the reform:** I investigate the effect of a change in EPLP on workers when they remain in their contract type (e.g. remain temporary workers) after the reform. For this purpose, I construct a sample which includes only persons who are observed in the specific year in a temporary/permanent contract and stay either in the control or treatment group (stayers).<sup>24</sup> I allow the treatment group to vary over time ( $TG_{it}$ ).<sup>25</sup> For instance, if person A holds a temporary contract in a firm with 5-19 employees in 1995, 1996, and 1997 but not in 1998, I keep three observations (1995-1997) in the sample of temporary workers. I allow the panel to be unbalanced.

Tables 4.1 and 4.2 present descriptive statistics of the Sample A for temporary and permanent workers.<sup>26</sup> Temporary workers are usually younger than permanent workers because temporary contracts are often used to screen the productivity of younger workers or to train youth in dual apprenticeships. This is also the case in the estimation samples. On average, temporary workers are around 30 years old, whereas permanent workers are, on average, around 40 years old. In terms of monthly household net income, permanent workers are better off compared to temporary workers. Life satisfaction and job satisfaction is around 7 on a scale of 0 to 10 for temporary and permanent workers.

## 4.5 Empirical results

The main result is that, on average, temporary workers suffered in terms of life satisfaction from the decrease in EPLP in 1996.<sup>27</sup> The negative effect of the decrease in EPLP on life satisfaction of temporary workers is specifically strong for temporary workers who remain in a temporary job after the reform. This

<sup>24</sup>For robustness checks for samples including movers, i.e. workers who are allowed to switch between treatment and control group, see Section 4.5.3.

<sup>25</sup>Alternatively, I could estimate the effect based on a subsample of Sample A. As I run out of observations in that case, I stick to Sample B.

<sup>26</sup>For descriptive statistics of Sample B, see Appendix 4.7.1.

<sup>27</sup>Results with longitudinal weights change in the sense that the signs of the policy effects remain similar but that standard errors become larger. Among others, however, this is due to the use of the fixed-effect dummy estimator and not the mean difference estimator, which is used for the presented results.

Table 4.1: Descriptive statistics: temporary workers (at the date of the reform)

<b>Variable</b>	<b>Mean</b>	<b>Std. Dev.</b>	<b>Min.</b>	<b>Max.</b>	<b>No.</b>
EPL - (1996)					
Pre-reform period					
life satisfaction	7.078	1.657	1	10	486
job satisfaction	7.097	2.218	0	10	486
job security	2.041	0.793	1	3	486
monthly HH net income (€)	2,022.34	802.718	511	4,857	486
age	28.492	11.569	17	58	486
female	0.506	0.5	0	1	486
Post-reform period					
life satisfaction	6.865	1.762	0	10	406
job satisfaction	7.012	2.041	0	10	406
job security	2.032	0.748	1	3	406
monthly HH net income (€)	2,116.406	854.61	511	5,113	406
age	29.877	11.181	18	60	406
female	0.446	0.498	0	1	406
EPL + (1999)					
Pre-reform period					
life satisfaction	7.114	1.668	0	10	590
job satisfaction	7.105	2.085	0	10	590
job security	1.992	0.778	1	3	590
monthly HH net income (€)	2,174.832	903.256	562	5,624	590
age	27.561	10.638	17	60	590
female	0.434	0.496	0	1	590
Post-reform period					
life satisfaction	7.172	1.492	1	10	786
job satisfaction	7.093	2.043	0	10	786
job security	2.14	0.728	1	3	786
monthly HH net income (€)	2,328.34	939.654	614	5,624	786
age	30.053	10.976	18	63	786
female	0.472	0.5	0	1	786

Note: Reform 1996: pre-reform period 01.01.1995-31.09.1996, post-reform period 01.10.1996-31.12.1998; Reform 1999: pre-reform period 01.01.1997-31.12.1998, post-reform period 01.01.1999-31.12.2001; Sample A: restricted to workers who were in a temporary job and in firms with 5-199 employees when the reform took place. Source: Own calculation based on GSOEP.

Table 4.2: Descriptive statistics: permanent workers (at the date of the reform)

Variable	Mean	Std. Dev.	Min.	Max.	No.
EPL - (1996)					
Pre-reform period					
life satisfaction	7.039	1.583	0	10	3,634
job satisfaction	7.025	1.995	0	10	3,634
job security	2.325	0.696	1	3	3,634
monthly HH net income (€)	2,202.242	819.236	767	5,778	3,634
age	39.547	10.57	17	65	3634
female	0.424	0.494	0	1	3634
Post-reform period					
life satisfaction	6.953	1.58	0	10	3,325
job satisfaction	6.916	1.943	0	10	3,325
job security	2.194	0.707	1	3	3,325
monthly HH net income (€)	2,277.961	835.237	767	5,783	3,325
age	40.879	10.194	18	65	3,325
female	0.428	0.495	0	1	3,325
EPL + (1999)					
Pre-reform period					
life satisfaction	7.018	1.558	0	10	3,818
job satisfaction	6.976	1.924	0	10	3,818
job security	2.191	0.714	1	3	3818
monthly HH net income (€)	2,260.053	825.903	818	5,624	3,818
age	40.018	10.186	18	65	3,818
female	0.429	0.495	0	1	3,818
Post-reform period					
life satisfaction	7.068	1.553	0	10	5,191
job satisfaction	6.902	1.913	0	10	5,191
job security	2.263	0.688	1	3	5,191
monthly HH net income (€)	2,380.785	836.193	818	5,624	5,191
age	41.872	9.773	20	65	5,191
female	0.432	0.495	0	1	5,191

Note: Reform 1996: pre-reform period 01.01.1995-31.09.1996, post-reform period 01.10.1996-31.12.1998; Reform 1999: pre-reform period 01.01.1997-31.12.1998, post-reform period 01.01.1999-31.12.2001; Sample A: restricted to workers who were in a permanent job and in firms with 5-199 employees when the reform took place. Source: Own calculation based on GSOEP.

could be explained by social comparison. I cannot fully rule out bias due to time-variant unobserved heterogeneity but pre-reform trend tests show that there is no difference in the pre-reform trend between control and treatment group. This finding might at least reduce concerns about the relevance of time-variant unobserved heterogeneity. Furthermore, less employable workers are specifically strong affected. The decrease in EPLP had no significant effect on well-being, which would be in line with loss aversion. As the EPLP reforms affected newly hired workers but almost not incumbents, a large proportion of permanent workers were not affected from the decrease and increase in EPLP. Hence, as I expected, I did not find any effects of the reforms on their well-being. Importantly, however, all results should be considered as lower bound estimates due to a non-negligible measurement error in the treatment status. The true effects might be stronger.

### **Common trend assumption**

Before I present the results, the common trend assumption is discussed. The common trend assumption is the identifying assumption for the unbiasedness of the policy effect in a DID approach. Although no formal test exists in order to assess the validity of this, pre-treatment trend and placebo tests help to assess whether the assumption is critical. I summarize the main findings here, before I present the results.

Pre-treatment tests did not result in any significant pre-treatment differences for life satisfaction as the dependent variable.<sup>28</sup> In Tables 4.3, 4.4, and 4.5, I test whether a common pre-policy trend for the treatment and the control group is rejected -  $TG \times Reform(t-1)$ . None of the coefficients, however, is significant.<sup>29</sup> Hence, in the pre-treatment period, I capture all the relevant heterogeneity which might induce different trends between treatment and control group in terms of life satisfaction. Although I do not technically control for unobserved time-invariant heterogeneity, the tests show that in the pre-reform period unobserved time-invariant

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<sup>28</sup>I also tested pre-treatment trend differences for perceived job security and job satisfaction. As I observe pre-treatment differences, which I cannot explain by anticipation or by group-specific linear trends, I do not consider them as dependent variables in the main paper. See Appendix 4.7.2 and 4.7.7.

<sup>29</sup>Additional checks for Sample B support this (Appendix 4.7.2).

heterogeneity does not yield a different trend between treatment and control group when it comes to life satisfaction. This finding reduces concerns about the relevance of unobserved time-variant heterogeneity for the policy effects. Finally, in the case of an anticipation effect, one would expect that the common pre-treatment trend is not met. This is not the case here.

In addition, I conduct placebo tests for Sample B.<sup>30</sup> They show that life satisfaction did not change for workers in medium sized (non-treated) versus large firms (non-treated) at the time of the reforms (placebo group test); and it did not change for workers in the treatment group (small sized firms) versus workers in the control group (medium sized firms) in 1998 (placebo reform). Overall, I do not find evidence against the hypothesis that treatment and control group follow a common trend for life satisfaction.

### 4.5.1 Effect of EPLP on life satisfaction

This section tests for the effect of EPLP reforms on life satisfaction of workers who were either temporarily or permanently employed at the time of the reform (Sample A). These workers are allowed to change their employment status after the reform, e.g. from a temporary to a permanent job.

#### Temporary workers

The theoretically expected effect of a decrease in EPLP on the well-being of workers who were in a temporary job at the time of the reform is ambiguous. The transition hypothesis expects a positive effect, while the comparison and anticipation hypothesis suggest a negative effect on well-being.

The DID results for the 1996 reform with life satisfaction as the dependent variable is presented in the upper left part of Table 4.3. The main result is that workers who were in temporary job at the time of the reform suffered by around 0.5 units in life satisfaction (TGxReform) from the decrease in EPLP in 1996 - see columns (1) to (3). Thus, the transition hypothesis is outweighed by the comparison and anticipation hypotheses. As already mentioned I account for selection due

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<sup>30</sup>See Appendix 4.7.2.

to observables and time-invariant unobservables. Selection due to time-invariant unobservables is not ruled out but the common pre-reform trend might reduce concerns about the relevance of time-invariant unobservable heterogeneity in my case.

The preferred model with unobservable and observable heterogeneity is presented in column (3). In this specification, temporary workers loose 0.407 units of life satisfaction due to a decrease in EPLP which is 5.8% of the mean. When I exclude the socio-demographic control variables, the policy effect becomes larger in its magnitude (-0.548). This could be due to observables, which capture different dynamics between control and treatment group. The results for the 1999 reform are presented in the right part of Table 4.3 - see columns (4) to (6). The increase in EPLP had no significant effect on life satisfaction. It is possible that this is due to effect heterogeneity, which is investigated in Section 4.5.2.

## Permanent workers

Theoretical expectations for the effect of a decrease in EPLP on the life satisfaction of permanent workers are ambiguous, too. While the insecurity hypothesis suggests a negative effect, the monitoring hypothesis expects a positive one. Due to the reform design, however, I do not expect strong effects of the reforms on life satisfaction of permanent workers.

The lower part of Table 4.3 shows the results for the reform in 1996 (decreasing EPLP) - see columns (1) to (3) - and for the reform in 1999 (increasing EPLP) - see columns (4) to (6). The policy effects (TGxReform) are not different from zero for both reforms, which is in line that a large proportion of permanent workers were not directly affected by the reforms. Effect heterogeneity, however, might explain the zero effects, too, and is elaborated in Section 4.5.2.

### 4.5.2 Effect heterogeneity

Based on the comparison, transition, and insecurity hypotheses, workers who remain in their contract type after the reform might exhibit a different reform effect, on average, compared to workers who transition into another employment status. In order to investigate this heterogeneity, I construct samples with workers who

Table 4.3: Dependent variable: life satisfaction

	EPL - (1996)			EPL + (1999)		
	(1)	(2)	(3)	(4)	(5)	(6)
Temporary workers (at the date of the reform)						
	FE	FE	FE	FE	FE	FE
TGxReform(t-1)		-0.154 (0.232)	-0.185 (0.244)		0.140 (0.210)	0.0882 (0.220)
TGxReform	-0.395* (0.202)	-0.548** (0.221)	-0.407* (0.236)	-0.00717 (0.160)	-0.0686 (0.189)	-0.0179 (0.192)
TGxReform(t+1)		0.463* (0.259)	0.415 (0.259)		0.0292 (0.173)	0.167 (0.180)
Socio-demo. controls	no	no	yes	no	no	yes
<i>N</i>	892	892	892	1,376	1,376	1,376
<i>R</i> <sup>2</sup>	0.023	0.029	0.091	0.011	0.011	0.050
Permanent workers (at the date of the reform)						
	FE	FE	FE	FE	FE	FE
TGxReform(t-1)		0.0336 (0.074)	0.0499 (0.074)		0.0397 (0.071)	0.0683 (0.073)
TGxReform	-0.0370 (0.061)	-0.0988 (0.075)	-0.0622 (0.078)	0.0322 (0.052)	-0.00483 (0.069)	0.00165 (0.069)
TGxReform(t+1)		0.100 (0.076)	0.0917 (0.075)		0.0324 (0.067)	0.0183 (0.071)
Socio-demo. controls	no	no	yes	no	no	yes
<i>N</i>	6,959	6,959	6,959	9,009	9,009	9,009
<i>R</i> <sup>2</sup>	0.007	0.007	0.025	0.002	0.002	0.014

Note: Fixed effects (FE) estimations, clustered standard errors in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ ; TG = 1 if 5-19 and TG = 0 if 20-199; Reform = [1 if year  $\geq$  reform year]; Reform(t-1) = [1 if year  $\geq$  one year before the reform year]; Reform(t+1) = [1 if year  $\geq$  one year after the reform year]; Sample A: sample of workers who were in a permanent/temporary job at the time of the reform and employed in firms with 5-199 employees at the time of the reform; Controls: reform dummies, TG, year fixed effects; Socio-demographic (socio-demo.) controls: firm size dummies, log of monthly HH income, working hours, working hours<sup>2</sup>, age, age<sup>2</sup>, education, female, married, child dummies as well state fixed effects.

remained in their contract type after the reform (Sample B)<sup>31, 32</sup>. This is obviously related to selection due to the EPLP reform. Workers who remain temporarily employed in the treatment group might differ from those in the control group. I discuss this issue in the preceding analyses. Furthermore, I investigate heterogeneity due to differences in the employability of workers.

### Temporary workers

If the comparison hypothesis explains the negative effect of EPLP in 1996 on life satisfaction of temporary workers (column (3) in Table 4.3), I expect that the negative effect is specifically strong for temporary workers who remain in a temporary job in a treated firm after the reform. This is because workers who benefited from potentially increased transition probabilities into permanent work by actually moving into a permanent job are excluded. Thereby, the transition hypothesis becomes less relevant, whereas the comparison hypothesis becomes more relevant, while the relevance of the anticipation hypothesis remains similar.

The main result is that temporary workers who remained in a temporary job suffered significantly in economical and statistical terms (TGxReform) in life satisfaction from the decrease in EPLP in 1996 (Table 4.4). The negative effect of the 1996 reform holds independently of controlling for observed heterogeneity or not and for excluding pre- and post-policy effects - see columns (1) to (3). In the preferred specification, temporary workers suffered by 0.588 units in life satisfaction - see column (3) - which is 8% of the mean, and thereby, higher as the effect on temporary workers who are allowed to move into a permanent job after the reform (5.8% of the mean, see column (3) in Table 4.3). Hence, given that Centeno and Novo (2012) show that EPLP is negatively related to transition probabilities from temporary work to permanent work, and based on the findings here, the comparison hypotheses remains a plausible explanation for the negative effect of EPLP on life satisfaction of temporary workers.<sup>33</sup>

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<sup>31</sup>For summary statistics, see Appendix 4.7.1.

<sup>32</sup>Unfortunately, I run out of observations in the case of subsamples of Sample A.

<sup>33</sup>Keeping in mind the measurement error, I test the effect on the transition probabilities, too. I find expected signs but the magnitude and the statistical significance are quite sensitive. See Appendix 4.7.6. A deeper investigation of this issue remains open for future research.

If the access into permanent employment in the treatment group became easier in the treatment group compared to the control group, the negative reform effect could also be due to selection. One could argue that temporary workers in the treatment group who remain in a temporary job even though transition became easier are an adverse selection of temporary workers. They might be generally less satisfied compared to those who remain in temporary employment in the control group. The policy effect might capture this difference. I can rule out this argument, if the difference is due to time-invariant difference in life satisfaction because I control for this. If the difference is due to a different trend in life satisfaction, I do not capture this. Pre-reform trend tests, however, show that control and treatment group do not differ in their life satisfaction trend in the pre-reform period. This might at least reduce concerns about the relevance of unobserved time-variant heterogeneity for the negative policy effect.

After one year - see  $TGxReform(t+1)$ , life satisfaction significantly increases again which is shown in column (3). This is in line with the adaptation to life events. Concerning the increase in EPLP in 1999, I do not find that the unconditional or conditional policy effects ( $TGxReform$ ) on life satisfaction are different from zero. One might expect that life satisfaction would increase from this reform, however, the results are in line with loss aversion: Workers value a loss stronger compared to a gain.

Finally, the effect of a change in EPLP might also differ with the perceived employability of the workers (Green, 2011). Specifically, highly employable workers might not mind if protection decreases, but less employable workers might be much more concerned. Column (4) of Table 4.4 presents the results of column (3) for the subsample of workers who gave a valid answer to the question on perceived chances of finding a new job in 1997 (1996 reform) or in 1999 (1999 reform).<sup>34</sup> Column (5) presents the subsample of less-employable workers, i.e. workers who perceive it to be difficult or practically impossible to find a new comparable job.<sup>35</sup>

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<sup>34</sup>Effect heterogeneity can also be investigated by interaction effects estimations (Leonardi and Pica, 2013) rather than subsample estimations (Centeno and Novo, 2014; Bauer et al., 2007; Autor, Donohue and Schwab, 2006; Autor, 2003). I chose subsample estimation as it allows for a high level of heterogeneity in the life satisfaction equation.

<sup>35</sup>Unfortunately, there are too few workers who feel employable in order to estimate the effects on this subgroup.

Concerning the reform in 1996, the coefficients and standard errors remain quite similar when it is restricted to workers who answered the question on perceived chances to find a new similar job - see columns (3) and (4). Comparing columns (4) and (5), the main result is that the negative effect of the 1996 reform becomes more significant in economical and statistical terms when the sample is restricted to persons who feel less employable. They lost 0.703 units in life satisfaction, but this loss is only temporarily. The results for 1999 are presented in the lower part. The policy effect, however, is again statistically not different from zero.

### **Permanent workers**

The effect of a decrease in EPLP on permanent workers in sample of workers who remained in a permanent job after the reform is expected to be less negative compared to a sample of workers who might transition into a temporary job or into unemployment. This is because permanent workers who suffered from lower EPLP by being dismissed are excluded, and thereby the insecurity hypothesis becomes less relevant. Overall, due to the reform design, however, I do not expect strong effects of both EPLP reforms on the life satisfaction of permanent workers because only newly hired permanent workers faced lower or higher EPLP but a minority of incumbents.<sup>36</sup> Keeping in mind the bias towards zero due to the measurement error in the treatment status, the upper and lower parts of Table 4.5 show that the policy effects (TGxReform) are not different from zero - neither for the decrease nor for the increase in EPLP - see columns (1) to (3). The zero effects can also not be explained by effect heterogeneity due to employability - see columns (3) versus (4).

### **4.5.3 Robustness**

#### **Movers and stayers**

Results for Sample B (workers who remain temporary/permanent employed) are restricted to stayers. Stayers are not allowed to switch between small-sized and

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<sup>36</sup>In Appendix 4.7.4, I present results for a sample which is restricted to entries.

Table 4.4: Dependent variable: life satisfaction (temporary workers who remain temporary employees)

	(1)	(2)	(3)	(4)	(5)
	FE	FE	FE	FE (No MV)	FE (Less Empl)
EPL - (1996)					
TGxReform(t-1)		-0.327 (0.283)	-0.253 (0.283)	-0.239 (0.332)	-0.211 (0.417)
TGxReform	-0.543* (0.311)	-0.595* (0.342)	-0.588* (0.342)	-0.574 (0.348)	-0.703* (0.417)
TGxReform(t+1)		0.534* (0.288)	0.553* (0.292)	0.593** (0.292)	0.674** (0.341)
Socio-demo. controls	no	no	yes	yes	yes
<i>N</i>	624	624	624	540	404
<i>R</i> <sup>2</sup>	0.040	0.053	0.110	0.133	0.172
EPL + (1999)					
TGxReform(t-1)		0.106 (0.264)	0.148 (0.268)	0.0281 (0.313)	0.0862 (0.426)
TGxReform	-0.167 (0.238)	-0.207 (0.263)	-0.140 (0.271)	-0.172 (0.273)	-0.135 (0.326)
TGxReform(t+1)		0.0419 (0.220)	0.0802 (0.217)	0.0503 (0.220)	-0.0628 (0.250)
Socio-demo. controls	no	no	yes	yes	yes
<i>N</i>	1,155	1,155	1,155	757	587
<i>R</i> <sup>2</sup>	0.008	0.008	0.047	0.063	0.069

Note: Fixed effects model (FE), clustered standard errors in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ ; TG = 1 if 5-19 and TG = 0 if 20-199; Reform = [1 if year  $\geq$  reform year]; Reform(t-1) = [1 if year  $\geq$  one year before the reform year]; Reform(t+1) = [1 if year  $\geq$  one year after the reform year]; Sample B: remain in the contract form (workers who remain in a temporary/permanent job over the sample period), stayers (workers who remain in TG or CG over the sample period); No missing values (No MV): sample of workers with a valid answer to the perception of finding a new job; less employable (less empl): sample of workers who perceive it to be practically impossible or difficult to find a new job; Controls: reform dummies, TG, year fixed effects; Socio-demographic (socio-demo.) controls: log of monthly HH income, working hours, working hours<sup>2</sup>, age, age<sup>2</sup>, education, female, married, child dummies as well state fixed effects.

Table 4.5: Dependent variable: life satisfaction (permanent workers who remain permanent employees)

	(1)	(2)	(3)	(4)	(5)
	FE	FE	FE	FE (No MV)	FE (Less Empl)
EPL - (1996)					
TGxReform(t-1)		0.0487 (0.081)	0.0696 (0.080)	0.103 (0.087)	0.142 (0.097)
TGxReform	-0.0426 (0.068)	-0.0943 (0.085)	-0.0809 (0.085)	-0.0909 (0.087)	-0.0995 (0.095)
TGxReform(t+1)		0.0643 (0.079)	0.0555 (0.079)	0.0645 (0.079)	0.0512 (0.086)
Socio-demo. controls	no	no	yes	yes	yes
<i>N</i>	5,917	5,917	5,917	5,485	4,633
<i>R</i> <sup>2</sup>	0.007	0.008	0.026	0.031	0.034
EPL + (1999)					
TGxReform(t-1)		0.0229 (0.082)	0.0261 (0.081)	0.0189 (0.086)	-0.0857 (0.094)
TGxReform	-0.0121 (0.061)	-0.0325 (0.075)	-0.0191 (0.075)	-0.0357 (0.075)	-0.000727 (0.083)
TGxReform(t+1)		0.0215 (0.079)	0.0439 (0.079)	0.0468 (0.078)	0.0338 (0.087)
Socio-demo. controls	no	no	yes	yes	yes
<i>N</i>	9,255	9,255	9,255	7,220	5,916
<i>R</i> <sup>2</sup>	0.001	0.001	0.011	0.016	0.016

Note: Fixed effects model (FE), clustered standard errors in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ ; TG = 1 if 5-19 and TG = 0 if 20-199; Reform = [1 if year  $\geq$  reform year]; Reform(t-1) = [1 if year  $\geq$  one year before the reform year]; Reform(t+1) = [1 if year  $\geq$  one year after the reform year]; Sample B: remain in the contract form (workers who remain in a temporary/permanent job over the sample period), stayers (workers who remain in TG or CG over the sample period); No missing value (No MV): sample of workers with a valid answer to the perception of finding a new job; less employable (less empl): sample of workers who perceive it to be practically impossible or difficult to find a new job; Controls: reform dummies, TG, year fixed effects; Socio-demographic (socio-demo.) controls: log of monthly HH income, working hours, working hours<sup>2</sup>, age, age<sup>2</sup>, education, female, married, child dummies as well state fixed effects.

medium-sized firms, while movers are. Enlarging Sample B of temporary workers to movers, the 1996 policy coefficient in Table 4.4 changes slightly towards zero.<sup>37</sup>

There are two explanations. First, it would be plausible that stayers compare stronger to their temporarily employed colleagues than movers, and therefore, they are affected stronger. Second, the policy effect (TGxReform) is not only identified via a change in legislation but also via job switches. Job switchers, who consciously switch their jobs, face a "honeymoon" and then a "hangover" in terms of satisfaction (Chadi and Hetschko, 2014). If workers are aware of higher transition probabilities in small firms, they might consciously switch. Hence, counteracting the dynamics of the 1996 reform. The finding that from 1995 to the next period of observation a smaller share of workers (2.2% of workers) switched from a large into a small firm compared to 1996 to the next period (7.79%) is in line with this argument (Table 4.6).

Table 4.6: Share of less employable temporary workers moving into CG or TG firms

	<b>Into TG from CG</b>	<b>Stayer</b>	<b>Into CG from TG</b>
1995	2.20	83.52	14.29
1996	7.79	83.12	9.09
1997	4.00	93.00	3.00

Note: Treatment group (TG), control group (CG); TG = 1 if 5-19 and TG = 0 if 20-199; "Into TG from CG" means that the worker moves in the next observed period from TG into CG; Sample B: remain temporary(temp)/permanent(perm) (workers who remain in a temporary/permanent job over the sample period), stayers (workers who remain in TG or CG over the sample period).

## Sample period

Finally, I test whether the policy effect changes, when I choose different sample periods. Policy effects are estimated for sample periods ending in January and December. For instance, the decrease in life satisfaction by 0.703 units of less-employable temporary workers due to a decrease in EPLP in 1996 (Table 4.4) is robust to changes to the ending month (January 0.702 and December 0.703).<sup>38</sup>

<sup>37</sup>See Appendix 4.7.3, columns (1) to (3) versus (4) to (6) in Table 4.13.

<sup>38</sup>See Appendix 4.7.3, columns (4) to (6) in Table 4.13.

With regard to the 1999 reform, the policy effect is robust to different sample periods, too.

## 4.6 Conclusion and discussion

This study investigates the impact of two almost perfectly symmetric reforms (1996, 1999) in German employment protection legislation for permanent contracts on well-being. EPLP reforms vary by firm size and allow for a difference-in-difference approach. Thus, I combine standard evaluation tools in the literature on the effects of employment protection on objective outcomes with the literature on determinants of life satisfaction for the first time. To identify the effects, I use longitudinal data of the GSOEP allowing me to control for individual fixed effects. In order to address the potential violation of the common trend assumption required for the DID approach and worker selection, I account for observables as well as for time-invariant unobservables. Also, I conduct placebo-tests, and pre-treatment trend tests. A major drawback is that the GSOEP allows me to measure firm size only imprecisely, which is likely to bias the policy effect estimator towards zero.

Following the literature, I distinguish between effects on temporary and permanent workers at points of the reform. The main result is that temporary workers suffered in terms of life satisfaction, on average, from a decrease in EPLP in 1996. A plausible explanation for this finding would be social comparison. Centeno and Novo (2012) found that EPLP is negatively related with transition probabilities from temporary to permanent work. Hence, temporary workers who remain temporarily employed might suffer due to comparison with colleagues who transitioned successfully in a permanent job after the reform. I account for selection due to observables and time-invariant unobservables and discuss the relevance of time-invariant unobservables. Unfortunately, I cannot fully rule out remaining concerns regarding the latter. Common pre-reform trends, however, show that treatment and control group do not differ in their pre-reform trend. This might at least reduce concerns about the relevance of unobserved time-variant heterogeneity for the policy effect. The increase in EPLP had no significant effect on well-being which would be, however, in line with the literature on loss aversion. As the EPLP reforms affected newly hired workers but less incumbents, a large proportion of

permanent workers were not affected from the decrease and increase in EPLP. Hence, I did not expect strong effects of the reforms on their well-being, which is confirmed.

In the aftermath of the 2007 financial crisis, decreasing EPLP was often discussed and liberalizing reforms took place, e.g. in Spain. Policy makers should account for potential negative well-being effects of a decrease in EPLP on temporary workers when designing such reforms. Based on that, a deeper investigation of the mechanisms behind the negative effect which I discuss in the paper (e.g. comparison and anticipation hypotheses) would be interesting to investigate. As I cannot mitigate any remaining concerns about the relevance of time-invariant unobservables for the policy effect, future research which investigates other sources of variation in EPLP would be beneficial in order to investigate the relevance of the remaining concerns. In general, combining standard evaluation techniques to study the effect of labor market institutions and policies with research on determinants of well-being proxied by life satisfaction is a fruitful task for future research. Research in this area is still rare with important exceptions: Hamermesh et al. (2014), Dorsett and Oswald (2014), D'Addio, Chapple, Hoherz and Landeghem (2014), Montizaan and Vendrik (2014), Kuroki (2012), and Lepage-Saucier and Wasmer (2012). This is surprising given that well-being is frequently applied in economic research (e.g. Hetschko, Knabe and Schöb, 2014; Frey and Stutzer, 2012; Clark and Senik, 2010) as well as in public policy (e.g. OECD, 2013b; OECD, 2011; Oswald, 2010).

## 4.7 Appendix

### 4.7.1 Descriptive statistics

Table 4.7: Descriptive statistics: temporary workers who remain temporary employees

Variable	Mean	Std. Dev.	Min.	Max.	No.
EPL - (1996)					
Pre-reform period					
life satisfaction	7.078	1.706	1	10	319
job satisfaction	7.097	2.07	0	10	319
job security	1.997	0.799	1	3	319
monthly HH net income (€)	2,049.887	803.545	511	4,704	319
age	26.665	11.12	17	58	319
female	0.498	0.501	0	1	319
Post-reform period					
life satisfaction	6.977	1.796	0	10	305
job satisfaction	7.075	2.168	0	10	305
job security	1.98	0.761	1	3	305
monthly HH net income (€)	2,159.207	904.051	460	5,266	305
age	25.603	10.349	17	59	305
female	0.439	0.497	0	1	305
EPL + (1999)					
Pre-reform period					
life satisfaction	7.064	1.753	0	10	358
job satisfaction	7.148	2.052	0	10	358
job security	1.98	0.765	1	3	358
monthly HH net income (€)	2,178.564	905.288	557	5,624	358
age	25.249	9.968	17	58	358
female	0.439	0.497	0	1	358
Post-reform period					
life satisfaction	7.105	1.694	1	10	797
job satisfaction	7.118	2.083	0	10	797
job security	2.049	0.741	1	3	797

*Continued on next page*

Table 4.7 – *Continued from previous page*

<b>Variable</b>	<b>Mean</b>	<b>Std. Dev.</b>	<b>Min.</b>	<b>Max.</b>	<b>No.</b>
monthly HH net income (€)	2,282.748	903.451	511	5,624	797
age	26.955	10.921	17	61	797
female	0.484	0.5	0	1	797

Note: Reform 1996: pre-reform period 01.01.1995-31.09.1996, post-reform period 01.10.1996-31.12.1998; Reform 1999: pre-reform period 01.01.1997-31.12.1998, post-reform period 01.01.1999-31.12.2001; Sample B: stayers (workers who remain in TG or CG), workers in a temporary job and in firms with 5-199 employees at the date of observation. Source: Own calculation based on GSOEP.

Table 4.8: Descriptive statistics: permanent workers who remain permanent employees

<b>Variable</b>	<b>Mean</b>	<b>Std. Dev.</b>	<b>Min.</b>	<b>Max.</b>	<b>No.</b>
EPL - (1996)					
Pre-reform period					
life satisfaction	7.069	1.569	0	10	3,091
job satisfaction	7.039	1.98	0	10	3,091
job security	2.354	0.685	1	3	3,091
monthly HH net income (€)	2,200.121	809.201	767	5,670	3,091
age	39.839	10.526	17	65	3,091
female	0.424	0.494	0	1	3,091
Post-reform period					
life satisfaction	6.989	1.565	0	10	2,826
job satisfaction	6.961	1.925	0	10	2,826
job security	2.202	0.699	1	3	2,826
monthly HH net income (€)	2,261.674	814.505	767	5,624	2,826
age	40.782	10.233	21	65	2,826
female	0.424	0.494	0	1	2,826
EPL + (1999)					
Pre-reform period					
life satisfaction	7.027	1.564	0	10	3,084
job satisfaction	7.021	1.896	0	10	3,084
job security	2.207	0.700	1	3	3,084

*Continued on next page*

Table 4.8 – *Continued from previous page*

<b>Variable</b>	<b>Mean</b>	<b>Std. Dev.</b>	<b>Min.</b>	<b>Max.</b>	<b>No.</b>
monthly HH net income (€)	2,284.295	833.399	818	5,697	3,084
age	40.425	10.099	19	65	3,084
female	0.431	0.495	0	1	3,084
Post-reform period					
life satisfaction	7.188	1.536	0	10	6,171
job satisfaction	7.077	1.903	0	10	6,171
job security	2.285	0.689	1	3	6,171
monthly HH net income (€)	2,408.497	851.719	818	5,880	6,171
age	41.791	9.854	19	65	6,171
female	0.439	0.496	0	1	6,171

Note: Restricted to permanent workers in firms with 5-199 employees (at least 12 months in their job); Reform 1996: pre-reform period 01.01.1995-31.09.1996, post-reform period 01.10.1996-31.12.1998; Reform 1999: pre-reform period 01.01.1997-31.12.1998, post-reform period 01.01.1999-31.12.2001; Sample B: stayers (workers who remain in TG or CG), workers in a permanent job and in firms with 5-199 employees at the date of observation. Source: Own calculation based on GSOEP.

### 4.7.2 Common trend assumption

This section provides additional in depth analyses of the common trend assumption for Sample B. I proceed as follows: First, I provide placebo tests which are typically conducted in the literature on EPLP evaluation (e.g. Leonardi and Pica, 2013); Second, I proceed by a detailed analyses of a potential pre-treatment trend difference between control and treatment group.

#### Placebo tests

In this part, I provide placebo tests for Sample B and for the subsample of Sample B of less-employable workers. Overall, placebo tests support the common trend assumption for life satisfaction as the outcome variable. In order to conduct a placebo group tests, I define workers to be in the treatment group, when they work in firms with 20-199 workers, and to be in the control group, when they are employed in firms larger than 199 workers. Hence, both groups of workers did not face any changes. If the policy effect is different from zero, then the general dynamic between small and large firms differ. The policy effects are, however, not significant - neither for the 1996 nor for the 1999 reform (Table 4.9). In order to conduct a placebo reform test, I define a placebo reform dummy for 1998 and choose a sample period from 1996 to 1999. I do not find that there is a general different dynamic in life satisfaction for workers in 5-19 versus 20-199 sized firms (Table 4.10).

#### Pre-treatment trend

In this part, I investigate in detail the pre-treatment trend of control and treatment group for Sample B and for the subsample of Sample B of less-employable workers. I do this for life satisfaction, job security and job satisfaction as dependent variables. This analyses was conducted as a pre-analyses in order to decide whether the common trend assumption is at least met in the pre-treatment period. If this was not the case, I did not include the respective model in the main paper.

I proceed as follows: First, I test whether control and treatment group differ in their pre-treatment trend; Second, if I find a difference in the pre-treatment trend

Table 4.9: Dependent variable: life satisfaction (pseudo group)

	(1)	(2)	(3)	(4)
	FE	FE (Less Empl)	FE	FE (Less Empl)
	Temporary workers (remain)		Permanent workers (remain)	
EPL - (1996)				
TGxReform	-0.0626	-0.187	0.0518	0.0798
	(0.302)	(0.339)	(0.059)	(0.065)
<i>N</i>	824	584	10,836	8,819
EPL + (1999)				
TGxReform	0.263	0.404	0.0361	0.0521
	(0.239)	(0.261)	(0.056)	(0.062)
<i>N</i>	1,464	786	16,459	10,778

Note: Fixed effects model (FE), clustered standard errors in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ ; TG = 1 if 20-199 and TG = 0 if 200-1999; Reform = [1 if year  $\geq$  reform year]; Reform(t-1) = [1 if year  $\geq$  one year before the reform year]; Reform(t+1) = [1 if year  $\geq$  one year after the reform year]; Sample B: Permanent/temporary workers (remain) are workers who remain in a temporary/permanent job over the sample period and who remain in TG or CG over the sample period); less employable (less empl): sample of workers who perceive it to be practically impossible or difficult to find a new job; Controls: reform dummies, TG, year fixed effects, TGxReform(t+1), TGxReform(t-1); Socio-demographic (socio-demo.) controls: log of monthly HH income, working hours, working hours<sup>2</sup>, age, age<sup>2</sup>, education, female, married, child dummies as well state fixed effects.

Table 4.10: Dependent variable: life satisfaction (placebo reform 1998)

	(1)	(2)
	FE	FE
	Temporary workers (remain)	Permanent workers (remain)
TGxReform	0.437	0.0474
	(0.284)	(0.077)
<i>N</i>	662	6,030
<i>R</i> <sup>2</sup>	0.064	0.022

Note: Fixed effects model (FE), clustered standard errors in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ ; TG = 1 if 5-19 and TG = 0 if 20-199; Reform = [1 if year  $\geq$  1998 year]; Reform(t-1) = [1 if year  $\geq$  1997]; Reform(t+1) = [1 if year  $\geq$  1999]; Sample B: Permanent/temporary workers (remain) are workers who remain in a temporary/permanent job over the sample period and who remain in TG or CG over the sample period); Controls: reform dummies, TG, year fixed effects, TGxReform(t+1), TGxReform(t-1); Socio-demographic (socio-demo.) controls: log of monthly HH income, working hours, working hours<sup>2</sup>, age, age<sup>2</sup>, education, female, married, child dummies as well state fixed effects.

between control and treatment group in a specific regression, I test whether anticipation explains this; Third, if I cannot explain the pre-treatment trend difference by anticipation, I investigate for this specific model whether a group-specific trend captures the differences between treatment and control group; if this would be the case, one could control for it in the DID analyses; Fourth, if this is not the case, I do not consider this specific model in the main paper.

Overall, I conclude from the analyses, that the life satisfaction equations are not problematic in terms of pre-treatment trend differences between control and treatment group. I cannot, however, confirm this for job security and job satisfaction equations. Therefore, I focus in the paper on life satisfaction as the outcome and do not extend the main paper to job security and job satisfaction.<sup>39</sup> This would be interesting, however, in order to investigate potential channels for the effect of EPLP on life satisfaction.

### **Pre-treatment trend**

Concerning life satisfaction, the models in Tables 4.3, 4.4, and 4.5 test, whether there is a trend difference between control and treatment group in the period before the reform takes place. For life satisfaction equations, there are no pre-treatment trend differences. Concerning the job security and the job satisfaction equation, I find the pre-policy effect for the decrease in EPLP in 1996 of temporary workers to be significant, which is specifically the case for less-employable workers - see Table 4.16, columns (5) and (6).

### **Anticipation**

In Table 4.11, I test whether the aforementioned pre-reform differences for the 1996 reform in job security of less employable temporary workers - see Table 4.16 in column (5), or job satisfaction of less employable temporary workers - see 4.16 in columns (6) - are due to an anticipation of the reform. If the pre-policy effect is due to anticipation, exclusion of the time period, in which the reforms were already discussed, can abolish the pre-policy effect. The discussion for the 1996

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<sup>39</sup>Appendix 4.7.7 presents some results for job satisfaction and perceived job security. Importantly, no implications are derived from them due to the aforementioned reasons.

reform intensified in May 1996.<sup>40</sup> Hence, I restrict the sample for the 1996 reform to a period from January 1995 to April 1996, and from October 1996 to December 1998. Table 4.11 presents results for the two models. The restricted samples are in the lower part. The pre-policy effects decrease, but I interpret this as not substantial. In order to mitigate any concerns, I continue by investigating whether group-specific trends explain this difference.

### Group-specific trends

Similar to Besley and Burgess (2004), I investigate group-specific linear trends  $TG_i * year$  for job security and job satisfaction equations of less employable temporary workers (1996) in columns (1) to (4). If the pre-policy effects for less-employable temporary workers is due to a group-specific linear trend, one could account for this in the DID analyzes. For this purpose, I define the reform dummies being one only in the respective year and zero otherwise. I estimate models with the three re-defined reform dummies and models with two reform dummies plus a group-specific linear trend.

The effect of a decrease in EPLP on job satisfaction turns from positive (non-significant) to negative (non-significant) and the pre-reform effect fades - in Table 4.12 in columns (3) and (4). Hence, the group-specific trend could pick up the pre-policy effect. In the case of perceived job security, results do not change considerably, and the pre-policy effect remains positive significant - see columns (1) and (2). As the pre-policy effect could not be captured by a group specific trend, the common trend assumption seems to be critical in the case of job security. In order to mitigate any concerns, I do not consider job satisfaction and perceived job security in the main paper as dependent variables.

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<sup>40</sup>Based on research in the online archive of the newspaper DIE ZEIT.

Table 4.11: Is anticipation relevant?

	(1)	(2)
	LPM FE(less empl)	FE(less empl)
	Temporary workers (remain)	
Dependent Var.	JoSec	JobSat
Sample Period	1995-1998	1995-1998
TGxReform(t-1)	0.283** (0.122)	0.963* (0.540)
TGxReform	-0.238** (0.115)	-0.220 (0.463)
TGxReform(t+1)	0.0556 (0.097)	0.869** (0.430)
<i>N</i>	404	404
<i>R</i> <sup>2</sup>	0.100	0.151
Sample Period	95-96, 10.96-98	95-4.96, 10.96-98
TGxReform(t-1)	0.219* (0.124)	0.905 (0.551)
TGxReform	-0.122 (0.120)	-0.160 (0.500)
TGxReform(t+1)	0.0723 (0.098)	0.872** (0.427)
<i>N</i>	382	382
<i>R</i> <sup>2</sup>	0.089	0.164

Note: Linear probability model (LPM), fixed effects model (FE), clustered standard errors in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ ; TG = 1 if 5-19 and TG = 0 if 20-199; Reform = [1 if year  $\geq$  reform year]; Reform(t-1) = [1 if year  $\geq$  one year before the reform year]; Reform(t+1) = [1 if year  $\geq$  one year after the reform year]; JobSat: job satisfaction; JobSec: perceived job security (0,1); Sample B: temporary workers (remain) are workers who remain in a temporary job over the sample period and who remain in TG or CG over the sample period; less employable (less empl): sample of workers who perceive it to be practically impossible or difficult to find a new job; Controls: reform dummies, TG, year fixed effects; Socio-demographic (socio-demo.) controls: log of monthly HH income, working hours, working hours<sup>2</sup>, age, age<sup>2</sup>, education, female, married, child dummies as well state fixed effects.

Table 4.12: Group specific time trends and life satisfaction for the reform 1996 (less employable temporary workers who remain temporary employees)

Dependent Var.	(1)	(2)	(3)	(4)
	LPM FE	LPM FE	FE	FE
	Perceived job security		Job satisfaction	
TGxReform(t-1)	0.283** (0.122)	0.250** (0.102)	0.963* (0.540)	0.426 (0.443)
TGxReform	0.0457 (0.129)	-0.0218 (0.083)	0.743 (0.594)	-0.332 (0.386)
TGxReform(t+1)	0.101 (0.150)		1.612** (0.644)	
TG*year		0.0338 (0.050)		0.537** (0.215)
<i>N</i>	404	404	404	404
<i>R</i> <sup>2</sup>	0.100	0.100	0.151	0.151

Note: Linear probability model (LPM), fixed effects model (FE), clustered standard errors in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ ; TG = 1 if 5-19 and TG = 0 if 20-199; Reform = [1 if year = reform year]; Reform(t-1) = [1 if year = one year before the reform year]; Reform(t+1) = [1 if year = one year after the reform year]; Perceived job security is 0/1 for low/high perceived job security; Sample B: remain temporary(temp)/permanent(perm) (workers who remain in a temporary/permanent job over the sample period), stayers (workers who remain in TG or CG over the sample period); less employable (less empl): sample of workers who perceive it to be practically impossible or difficult to find a new job; Controls: reform dummies, TG, year fixed effects; Socio-demographic (socio-demo.) controls: log of monthly HH income, working hours, working hours<sup>2</sup>, age, age<sup>2</sup>, education, female, married, child dummies as well state fixed effects.

## 4.7.3 Robustness: sample period, movers and stayers

Table 4.13: Dependent variable: life satisfaction of less empl. workers (EPL- (1996))

	Movers			Stayers		
	(1) FE(Jan)	(2) FE(May)	(3) FE(Dec)	(4) FE(Jan)	(5) FE(May)	(6) FE(Dec)
Temporary workers (remain temporary)						
TGxReform(t-1)	-0.281 (0.392)	-0.350 (0.388)	-0.359 (0.388)	-0.113 (0.422)	-0.190 (0.417)	-0.211 (0.417)
TGxReform	-0.612 (0.444)	-0.579 (0.405)	-0.584 (0.403)	-0.702 (0.452)	-0.699* (0.416)	-0.703* (0.417)
TGxReform(t+1)	0.675 (0.628)	0.545 (0.340)	0.575* (0.324)	1.290* (0.671)	0.654* (0.359)	0.674** (0.341)
<i>N</i>	394	475	483	325	398	404
<i>R</i> <sup>2</sup>	0.140	0.126	0.125	0.192	0.175	0.172
Permanent workers (remain permanent)						
TGxReform(t-1)	0.0940 (0.090)	0.107 (0.089)	0.103 (0.089)	0.121 (0.098)	0.144 (0.097)	0.142 (0.097)
TGxReform	-0.0415 (0.092)	-0.0301 (0.090)	-0.0321 (0.090)	-0.100 (0.097)	-0.0984 (0.095)	-0.0995 (0.095)
TGxReform(t+1)	0.0320 (0.160)	0.0120 (0.089)	0.00588 (0.085)	0.130 (0.166)	0.0588 (0.090)	0.0512 (0.086)
<i>N</i>	4,390	5,338	5,442	3,745	4,553	4,633
<i>R</i> <sup>2</sup>	0.030	0.024	0.026	0.036	0.031	0.034

Note: Fixed effects (FE) estimations, clustered standard errors in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ ; TG = 1 if 5-19 and TG = 0 if 20-199; Reform = [1 if year  $\geq$  reform year]; Reform(t-1) = [1 if year  $\geq$  one year before the reform year]; Reform(t+1) = [1 if year  $\geq$  one year after the reform year]; Sample B: remain temporary(temp)/permanent(perm) (workers who remain in a temporary/permanent job over the sample period), stayers (workers who remain in TG or CG over the sample period) or movers (workers who are allowed to switch between TG or CG over the sample period); less employable (less empl): sample of workers who perceive it to be practically impossible or difficult to find a new job; Sample period ends in January(Jan), May or December (Dec); Controls: reform dummies, TG, year fixed effects; Socio-demographic (socio-demo.) controls: log of monthly HH income, working hours, working hours<sup>2</sup>, age, age<sup>2</sup>, education, female, married, child dummies as well state fixed effects.

#### 4.7.4 Newly hired permanent workers

The EPLP reforms changed EPLP for new hires with a permanent contract but not (less) for incumbents in a permanent work relation. In particular, the 1996 abolishment of EPLP for small firms was only applied to workers, who signed their contracts after September 1996; for incumbents, only a future reduction (after September 1999) in EPLP became effective on the 1st October 1996. The 1999 reform increased EPLP for new hires and workers who signed the permanent contract after September 1996 while for incumbents only an increase in future EPLP became effective.

Therefore, I restrict the samples to newly hired workers. For the 1996 reform, I only include permanent workers who signed a new contract between October 1996 - 1998 or between May 1994 - September 1996. For the 1999 reform, I only include those, who signed the contract between October 1996-1998 or 1999 - March 2001. Table 4.14 (lower part) presents the results in columns (3) and (6). Importantly, the number of observations becomes considerably low, specifically for the 1996 reform (371 observations). Therefore, these samples are not employed for the main analyses in the paper. Table 4.14 shows that the policy effect (TGxReform) is negative for the decrease in EPLP [column (3)], while it is positive for the increase in EPLP [column (6)]. Both effects, however, are not significant in statistical terms.

#### 4.7.5 Non-response in job satisfaction and perceived job security

In this section, I present the robustness of the negative EPLP effect on life satisfaction on temporary workers by accounting for non-responses in job satisfaction as well as in perceived job security. For example, the sample size is reduced by around 7% in the case of temporary workers for the 1996 reform (Sample B).<sup>41</sup> When I compare the results for samples excluding those observations, the policy effect for the 1996 reform becomes smaller in absolute terms but remains negative - Table 4.14, column (1) versus (4), column (3) versus (6).

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<sup>41</sup>Results for Sample A are available upon request.

Table 4.14: Dependent variable: life satisfaction and sample restrictions (workers who remain in contract)

	EPL -			EPL +		
	(1)	(2)	(3)	(4)	(5)	(6)
Temporary workers	FE	FE (MV)	FE (15y)	FE	FE (MV)	FE (15y)
TGxReform(t-1)	-0.253 (0.283)	-0.216 (0.264)	-0.245 (0.291)	0.148 (0.268)	0.0570 (0.256)	0.159 (0.268)
TGxReform	-0.588* (0.342)	-0.499 (0.331)	-0.603* (0.345)	-0.140 (0.271)	-0.0927 (0.255)	-0.137 (0.271)
TGxReform(t+1)	0.553* (0.292)	0.566** (0.276)	0.617** (0.296)	0.0802 (0.217)	0.0443 (0.205)	0.0868 (0.217)
<i>N</i>	624	665	606	1,155	1,321	1,147
<i>R</i> <sup>2</sup>	0.110	0.102	0.108	0.047	0.044	0.048
Permanent workers	FE	FE (MV)	FE (new)	FE	FE (MV)	FE (new)
TGxReform(t-1)	0.0696 (0.080)	0.0795 (0.079)	0.224 (0.281)	0.0261 (0.081)	0.00619 (0.081)	0.277 (0.327)
TGxReform	-0.0809 (0.085)	-0.0713 (0.084)	-1.061 (0.705)	-0.0191 (0.075)	0.00221 (0.073)	0.608 (0.570)
TGxReform(t+1)	0.0555 (0.079)	0.0377 (0.079)	-0.0998 (0.314)	0.0439 (0.079)	0.00857 (0.076)	-0.107 (0.418)
<i>N</i>	5,917	6,077	371	9,255	9,836	891
<i>R</i> <sup>2</sup>	0.026	0.026	0.246	0.011	0.011	0.083

Note: Fixed effects (FE) estimations, clustered standard errors in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ ; TG = 1 if 5-19 and TG = 0 if 20-199; Reform = [1 if year  $\geq$  reform year]; Reform(t-1) = [1 if year  $\geq$  one year before the reform year]; Reform(t+1) = [1 if year  $\geq$  one year after the reform year]; Sample B: remain in contract (workers who remain either in a temporary/permanent job over the sample period), stayers (workers who remain in TG or CG over the sample period); MV: sample includes observations with a missing value either in job satisfaction or perceived job security; new: only newly hired permanent workers; 15y: only temporary workers with less than 15 years in one firm; Controls: reform dummies, TG, year fixed effects; Socio-demographic (socio-demo.) controls: log of monthly HH income, working hours, working hours<sup>2</sup>, age, age<sup>2</sup>, education, female, married, child dummies as well state fixed effects.

### 4.7.6 Probability to transition from a temporary into a permanent job

Table 4.15: Dependent variable: Transition from temporary into permanent work

	EPL - (1995-1998)		EPL + (1997-2001)	
	(1)	(2)	(3)	(4)
	LPM(FE)	LPM(FE): 3	LPM(FE)	LPM(FE): 3
TGxReform(t-1)	-0.108 (0.093)	-0.122 (0.122)	0.116 (0.092)	0.0223 (0.126)
TGxReform	0.141 (0.125)	0.275* (0.147)	-0.0488 (0.081)	-0.122 (0.105)
TGxReform(t+1)	0.144 (0.164)	0.0258 (0.218)	0.0304 (0.116)	0.0108 (0.142)
Socio-demo. controls	yes	yes	yes	yes
<i>Mean y<sub>it</sub></i>	0.28	0.25	0.30	0.27
<i>N</i>	557	388	836	568
<i>R</i> <sup>2</sup>	0.427	0.514	0.408	0.452

Note: Dependent variable: 1 if temporary worker transitions in the next period into a permanent job, 0 if not; linear probability model (LPM), fixed effects model (FE), clustered standard errors in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ ; TG = 1 if 5-19 and TG = 0 if 20-199; Reform = [1 if year  $\geq$  reform year]; Reform(t-1) = [1 if year  $\geq$  one year before the reform year]; Reform(t+1) = [1 if year  $\geq$  one year after the reform year]; Sample: sample of workers who worked in firms with 5-199 employees at the time of the reform, either fixed-term worker or permanent worker, employable age; Sample "3" means that the sample is restricted to workers who stayed with the firm 3 years at maximum; Controls: reform dummies, TG, year fixed effects; Socio-demographic (socio-demo.) controls: firm size dummies, log of monthly HH income, working hours, working hours<sup>2</sup>, age, age<sup>2</sup>, education, female, married, child dummies as well state fixed effects.

### 4.7.7 Channels: Perceived job security and job satisfaction

Importantly, as already mentioned in Appendix 4.7.2, perceived job security and job satisfaction as dependent variables are less convincing with regard to the common trend assumption. This section presents some results for job satisfaction and job security, which are interpreted carefully and are not included in the main paper. No major implications are drawn.

The effect of EPLP on life satisfaction might be transmitted via perceived job security and job satisfaction. Perceived job security is positively related to life satisfaction.<sup>42</sup> Temporary workers might benefit in terms of perceived job security as transition probabilities are expected to increase. They might suffer, however, in terms of perceived job security as they could anticipate that the next permanent job is less secure compared to before of the reform. Permanent workers who were hired after the reform might suffer in terms of job security due to higher likelihood of becoming unemployed. Concerning job satisfaction, job satisfaction is conceptually and empirically positively related to life satisfaction (Praag et al., 2003).<sup>43</sup> Temporary workers could suffer in terms of job satisfaction when they remain in the temporary job, while others transition into a permanent position. Finally, permanent workers might benefit in terms of job satisfaction. Monitoring by employers might decrease, and thereby, job related stress decreases (Lepage-Saucier and Wasmer, 2012) while the opportunity for personal control increases. Those, in turn, are negatively and positively related to job satisfaction (Warr, 2003).

In order to explore these ideas, I first estimate the DID regression with job satisfaction and perceived job security as dependent variables. Second, I include job satisfaction and job security as mediators in the life satisfaction equation (mediation analysis). Analyzes are conducted for the 1996 reform for Sample B for less employable workers.

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<sup>42</sup>See Section 4.2 for literature. For the effect of perceived job security on objective outcomes in economics: Stephens (2004), Campbell et al. (2007) and Böckerman, Ilmakunnas and Johansson (2011).

<sup>43</sup>For the effect of job satisfaction on life satisfaction in psychology, see Section 4.2.

Table 4.16: Mechanisms for EPL - (less employable temporary workers who remain temporary employees)

	(1)	(2)	(3)	(4)	(5)	(6)
	FE	FE	FE	FE	LPM FE	FE
Dependent Var.		Life Satisfaction			JobSec	JobSat
TGxReform(t-1)	-0.211 (0.417)	-0.366 (0.419)	-0.371 (0.415)	-0.501 (0.417)	0.283** (0.122)	0.963* (0.540)
TGxReform	-0.703* (0.417)	-0.579 (0.419)	-0.667* (0.403)	-0.558 (0.405)	-0.238** (0.115)	-0.220 (0.463)
TGxReform(t+1)	0.674** (0.341)	0.626* (0.333)	0.529 (0.342)	0.492 (0.335)	0.0556 (0.097)	0.869** (0.430)
JobSec (low)						
middle		0.468** (0.225)		0.406* (0.212)		
high		0.657** (0.294)		0.599** (0.282)		
JobSat			0.167** (0.064)	0.158** (0.064)		
<i>N</i>	404	404	404	404	404	404
<i>R</i> <sup>2</sup>	0.172	0.192	0.208	0.224	0.100	0.151

Note: Linear probability model (LPM), fixed effects model (FE), clustered standard errors in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ ; TG = 1 if 5-19 and TG = 0 if 20-199; Reform = [1 if year  $\geq$  reform year]; Reform(t-1) = [1 if year  $\geq$  one year before the reform year]; Reform(t+1) = [1 if year  $\geq$  one year after the reform year]; JobSat: job satisfaction; JobSec: perceived job security (0,1); Sample B: remain temporary (workers who remain in a temporary job over the sample period), stayers (workers who remain in TG or CG over the sample period); less employable (less empl): sample of workers who perceive it to be practically impossible or difficult to find a new job; Controls: reform dummies, TG, year fixed effects; Socio-demographic (socio-demo.) controls: log of monthly HH income, working hours, working hours<sup>2</sup>, age, age<sup>2</sup>, education, female, married, child dummies as well state fixed effects.

Table 4.17: Mechanisms for EPL - (less employable permanent workers who remain permanent employees)

	(1)	(2)	(3)	(4)	(5)	(6)
	FE	FE	FE	FE	LPM FE	FE
Dependent Var.		Life Satisfaction			JobSec	JobSat
TGxReform(t-1)	0.142 (0.097)	0.134 (0.097)	0.153 (0.095)	0.147 (0.095)	0.0211 (0.025)	-0.0701 (0.121)
TGxReform	-0.0995 (0.095)	-0.100 (0.095)	-0.105 (0.094)	-0.105 (0.094)	0.0154 (0.024)	0.0325 (0.119)
TGxReform(t+1)	0.0512 (0.086)	0.0616 (0.086)	0.0718 (0.085)	0.0782 (0.085)	-0.0425 (0.027)	-0.129 (0.125)
JobSec (low)						
middle		0.241*** (0.076)		0.165** (0.073)		
high		0.372*** (0.089)		0.270*** (0.087)		
JobSat			0.159*** (0.018)	0.154*** (0.017)		
<i>N</i>	4,633	4,633	4,633	4,633	4,633	4,633
<i>R</i> <sup>2</sup>	0.034	0.041	0.075	0.079	0.024	0.019

Note: Linear probability model (LPM), fixed effects model (FE), clustered standard errors in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ ; TG = 1 if 5-19 and TG = 0 if 20-199; Reform = [1 if year  $\geq$  reform year]; Reform(t-1) = [1 if year  $\geq$  one year before the reform year]; Reform(t+1) = [1 if year  $\geq$  one year after the reform year]; JobSat: job satisfaction; JobSec: perceived job security (0,1); Sample B: remain permanent(perm) (workers who remain in a permanent job over the sample period), stayers (workers who remain in TG or CG over the sample period); less employable (less empl): sample of workers who perceive it to be practically impossible or difficult to find a new job; Controls: reform dummies, TG, year fixed effects; Socio-demographic (socio-demo.) controls: log of monthly HH income, working hours, working hours<sup>2</sup>, age, age<sup>2</sup>, education, female, married, child dummies as well state fixed effects.

## Data

The GSOEP contains appropriate variables. The following dependent variables ( $Y_{it}$ ) are considered: First, perceived job security [very concerned (1) - not concerned at all (3)]<sup>44</sup> is coded as a dummy variable, which is zero for workers who are very concerned, and one for workers who are not concerned about their job security;<sup>45</sup> Second, job satisfaction [totally unhappy (0) - totally happy (10)]<sup>46</sup>.

## Temporary workers

The negative effect of a decrease in EPLP on life satisfaction might hide different channels via perceived job security and job satisfaction. Workers might have suffered in terms of job satisfaction (comparison hypothesis) and perceived job security (comparison and anticipation hypothesis) or workers might have benefited in terms of perceived job security (anticipation).

First, concerning the decrease in EPLP, job security and job satisfaction of less employable temporary workers seems to be affected negatively [Table 4.16, columns (5) and (6)] in comparison to the previous year. Less employable workers in small firms suffered in terms of job satisfaction but not significantly.<sup>47</sup> The pre-policy difference and the post-policy difference, however, are positive and significant. In Appendix 4.7.2, I test for pre-treatment differences, anticipation and allow for group-specific trends. I conclude that there might be a positive group-specific trend, and hence, that job satisfaction (TGxReform) could be affected negatively. Concerning perceived job security, the coefficient is negative and significant [Table 4.16, column (5)]. After the reform they were 0.238 percentage points less likely to be "not concerned" about their job security (TGxReform). In this case, however, neither anticipation nor group-specific trends could explain the

<sup>44</sup>GSOEP question: "What is your attitude towards the following areas - are you concerned about them?...Your job security."

<sup>45</sup>Multinomial models for temporary workers showed that a decrease in EPLP leads to relative odds of being very concerned/not concerned at all rather than somewhat concerned that are not significantly different/significantly higher. Therefore, I merged the categories of being somewhat concerned with not concerned at all. I proceeded in the same way for permanent workers.

<sup>46</sup>GSOEP question: "How satisfied are you with your job?"

<sup>47</sup>I estimate a linear probability model with individual fixed-effects. A random effect ordered logit model with three categories for the response variable is robust.

pre-reform differences.<sup>48</sup> Therefore, I do not make any conclusions from perceived job security as the dependent variable.

Second, I include perceived job security and job satisfaction, in the life satisfaction equation. When I include job satisfaction in column (3) of Table 4.16, the negative policy effect and the post-policy effect become smaller in absolute terms in comparison to the model without job satisfaction [column (1)]. Including job security yields the policy effect to fade [column (2)]. Finally, when I include job satisfaction and perceived job security in the model, the negative effect decreases from 0.703 [column (1)] (significant) to 0.558 fade [column (4)] (non-significant), and the post-policy effect decreases from 0.674 [column (1)] (significant) to 0.492 [column (4)] (non-significant).

To summarize, job satisfaction might contribute to explain a part of the pattern I find for the effect of a decrease in EPLP on life satisfaction of temporary workers. Overall, however, I do not derive any strong conclusions because of the critical common trend assumption which is specifically the case for job security.

## Permanent workers

Another possible explanation next to effect heterogeneity for the zero effect for permanent workers (Sample B) when EPLP decreases is that possible channels job security and job satisfaction cancel each other out. On the one hand, the security hypothesis predicts that permanent workers suffer in terms of perceived job security because permanent contracts become more instable. On the other hand, the monitoring hypothesis predicts that workers might be more satisfied with their job. Due to the reform design, however, for the majority of permanent workers only future EPLP changes. Therefore, the expected effect is not very strong.

First, I find that less employable permanent workers did not benefit in terms of job satisfaction from a decrease in EPLP [Table 4.17 column (6)]. They also did not suffer in terms of perceived job security [column (5)].<sup>49</sup> Second, including these

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<sup>48</sup>See Appendix 4.7.2.

<sup>49</sup>I estimate a linear probability model with individual fixed effects. A random effect ordered logit model with three outcomes for the response variable also identified no significant reform effects.

variables into the life satisfaction equation does not change the policy effect nor the standard errors considerably. Neither effect heterogeneity nor opposing mechanisms of perceived job security and job satisfaction could explain why permanent workers were not affected by a decrease in EPLP. The most plausible explanation is the reform design.

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# Curriculum Vitae

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## Personal Information

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50931 Köln, Germany  
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Research Interests: Applied Microeconomics, Labour Economics, Economics of Happiness

## Academic Education

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2008 - present **Ph.D. Student**, Cologne Graduate School (CGS), University of Cologne  
Supervisors: Prof. Ph.D. Jaeger (since 2013), Prof. Dr. André Kaiser (since 2010)

2008 – 2011 **Ph.D. Graduateprogramme, CGS**, University of Cologne

2003 – 2008 **Diplom-Volkswirtin (M.A. Economics)**, University of Cologne (1.4)  
Supervisor: Prof. Dr. Clemens Fuest (1.0)

2006 – 2006 **Graduate Studies in Economics and Social Policy**, University of Edinburgh

## Professional Career

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2011 – 2014 IZA - **Resident Research Affiliate**: Projection of Labor Shortages

2011 – 2011 University of Cologne – **Teaching**: “Comparative Political Economy” (appr. 40 students)

2009 – 2009 IZA - **Research Assistant**: Structural Estimation of Reform Effects in the Tax and Transfer System (Dr. Andreas Peichl, PD Dr. Hilmar Schneider)

2007 – 2008 FiFo Institute for Public Economics - **Student Research Assistant**: Comparison of European Tax and Transfer Systems (Prof. Dr. Clemens Fuest)

2005 – 2006 Max-Planck-Institute for the Study of Societies - **Student Research Assistant**: The Economy as a Topic in Sociological Research (Prof. Dr. Jens Beckett)

## Publications

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Dräger, V. and Marx, P. (2012): Do Firms Demand Temporary Workers When They Face Workload Fluctuation? IZA DP 6894 (revised and resubmitted (2014) at Industrial Labor Relations Review (ILRRReview)).

Dräger, V. (2014): Zukünftige Fachkräfteengpässe in Deutschland? IZA DP 8434.

## Scholarships

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Z008 – 2011 **Ph.D. Scholarship by the CGS**, University of Cologne, **German Academic Exchange Service** (ECPR Summer School)

2006, 2007 **German Academic Exchange Service** (Essex Summer School, Erasmus)

## Selected Conferences

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2014 Spring Meeting of Young Economists 2014

2013 European Association of Labour Economists (EALE); XXVIII AIEL Conference of Labour Economics; Public Happiness Conference (Heirs Association)

- 2012 1st Potsdam PhD Workshop in Empirical Economics; Workshop on Atypical Employment and Skill Shortages (Federal Ministry of Statistics)
- 2011 SASE 23<sup>rd</sup> Annual Meeting of the Society for the Advancement of Socio-Economics, Universidad Autónoma de Madrid

### Invited Seminars

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- 2015 Upcoming in June: Joint Research Center (European Commission), Italy

### Referee

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Journal of Comparative Economics, Journal of Labour Market Research

### Selected Summer Schools

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- 2012 Topics in Econometrics and Statistics: Microeconomic Analysis (Frank Vella, IZA); 7th Winter School on Inequality and Social Welfare Theory
- 2011 Strategies in comparative analysis: Multi-level, Multi-Group and Dummy Approaches (Universidad Pompeu Fabra), Spain
- 2008 Multi-County Tax-Benefit Microsimulation Modelling with EUROMOD (University of Essex), Labour Supply Microsimulation (University of Cologne)
- 2007 Essex Summer School – Maximum Likelihood and Limited Dependent Variable Models (University of Essex); Qualitative response variables (University of Cologne)

### Language Skills

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German: Native, English: Fluent, French: Basic

### Data and Software

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Stata, Latex, Scientific Workplace

Linked-Employer-Employee Data IAB, IAB-Establishment Panel, Socio-Economic Panel (SOEP), Microcensus, European Company Survey, EUROMOD (tax-benefit microsimulation for the European Union), IZAΨMOD (IZA Policy Simulation MODel)

### Non-Academic Education and Work Experience

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- 2005 – 2005 Capacity Building International (InWEnt gGmbH) – **Internship** in Development and Training
- 2002 – 2003 **Internships** in Journalism
- 2000 - 2002 M+W Zander Gebäudetechnik GmbH – Dual Apprenticeship (Chamber of Commerce)

Cologne, May 13, 2015