Employment Protection Legislation and the Quality of Jobs: Evidence on Job Satisfaction

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Abstract

Employment Protection Legislation (EPL) imposes firing costs for permanent workers and restricts the use of temporary employment. In recent years several countries have eased EPL for temporary employees and left regulations for permanent workers unchanged. While the effects of these reforms on employment and unemployment have been studied in the literature, nothing is known empirically about their effects on workers' well-being. This paper uses longitudinal data on 250,000 workers from 13 nations to investigate the link between EPL and workers' wellbeing. The results provide some of the first evidence that both permanent and temporary employees gain from reforms easing exclusively EPL for temporary employees.

1 Introduction

Nearly every country features legislation concerning the firing of permanent workers and the use of temporary employment. This set of rules is commonly referred to in the literature as employment protection legislation (EPL). Over the last two decades, several European countries have brought about labour market reforms easing restrictions on the use of temporary employment while leaving firing costs for permanent workers substantially unaltered. In most cases such reforms have been supported by international organizations (such as the IMF, the OECD and the EU) with the aim of tackling the high and persistent European unemployment. Consequently, the economic literature has almost exclusively focused on evaluating the quantitative effects of these changes on

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employment and unemployment, while nothing is known on the link between these regulations and workers' wellbeing. We argue that the focus should be broadened consistently with the EU decision to include higher flexibility in its strategy to achieve not only "more", but also "better" jobs (European Commission [22]). In this paper, we look at the effects of EPL on the quality of jobs for permanent and temporary workers by using self-reported job satisfaction and satisfaction with security. In doing so we add to the recent literature advocating the use of subjective wellbeing measures for policy evaluation (for a recent contribution see Layard [30]).

A number of papers have modelled regulations on temporary and permanent contracts. Although only a few of them have explicitly aimed to capture the welfare effects of labour market deregulation, it is possible to take advantage of the common use of a simple expected utility function to recover the implications of this literature for the workers wellbeing.

In most of the proposed models, regulations have both direct and indirect effects (through the wage and the transition probabilities across different labour market states) on workers' expected utility. The complexity of the mechanisms at work makes theoretical predictions mostly ambiguous leaving large scope for empirical investigation. This paper takes job satisfaction as a proxy for workers' utility and proposes a first simple reduced-form test to investigate the effects of deregulation. For those who do not subscribe to the maintained hypothesis that job satisfaction is a proxy for utility, we point out that job satisfaction is a wellbeing measure of interest in itself and has been found to predict some types of behaviours (see section 3.2 for references). Albeit for temporary employees only, we also use data on satisfaction with security.

As we look separately at permanent and temporary employees, this paper joins Kahn [41] Clark and Postel-Vinay [14] in bridging two areas of the literature which have remained surprisingly separate so far: the one studying the effects of EPL and the other investigating the characteristics of temporary workers.

In our analysis, we use for the first time a time-series EPL indicator recently published by the OECD [37] for almost all EU countries. We also use microdata from the European Community Household Panel for 13 European countries and regional data from the REGIO dataset of EUROSTAT. We apply a random effects ordered probit estimating satisfaction equations separately for permanent and temporary workers.

The main conclusion of the paper is that reforms easing restricions on temporary employment have positive effects on the wellbeing of both temporary and permanent workers, particularly for women and the young. We also find that satisfaction with security of temporary workers increases with protection for permanent employees, but a simple test described in the paper does not support the hypothesis that this is the effect of fixed-term workers aniticipating future protection. The evidence indicates that the view commonly held in the political economy literature that only permanent workers support reforms focusing on temporary employment might need to be reconsidered.

Section 2 reviews related theoretical and empirical literature, while section

3 describes the data and the approach taken in the empirical analysis. Section 4 and 5 present the results for permanent and temporary workers respectively. After a preliminary investigation of the role of transition probabilities in section 6, section 7 concludes.

2 Review of related literature

2.1 Theory

Most theoretical analyses have focused on the effects of regulations on labour market performance and in the majority of the cases only restrictions to firing permanent workers have been explicitly modelled. A few recent papers, however, have also taken into account regulations on temporary employment and, while their focus has remained on the quantitative effects of regulations, they also provide some insights on the welfare effects of employment protection.

The theoretical framework commonly adopted is the matching model (Pissarides [45]) which allows to describe both direct and indirect effects of EPL on workers' utility. To gain an intuition of the mechanisms at play, consider the workers' Bellman equations in a setting with only permanent workers. If V_ε represents the maximised utility of a worker with productivity ε, following the notation of Cahuc and Zylberberg [16], we can write:

$$rV_\varepsilon = w_\varepsilon + \lambda(V_\lambda - V_\varepsilon) \quad (1)$$

$$V_\lambda = \int_{-\infty}^{\varepsilon_d} (f_e + V_u)dG(\varepsilon) + \int_{\varepsilon_d}^{\varepsilon_u} V_\varepsilon dG(\varepsilon) \quad (2)$$

where it is assumed that productivity shocks occur at rate λ and that the level of productivity ε is drawn from a random distribution with cdf $G(.)$ and support $]-\infty, +\varepsilon_u]$. ε_d is the endogenous threshold level of productivity below which the firm fires the worker. Hence equation 1 says that the asset value of a job is given by the current wage (w_ε) plus the change in value when a productivity shock hits. If a shock occurs, the worker gets expected utility V_λ since the new productivity level can be either above ε_d (in which case the worker remains employed) or below ε_d causing the worker to receive severance payment[1] f_e and an expected utility from unemployment V_u. This latter satisfies:

$$rV_u = z + p(V_0 - V_u) \quad (3)$$

where z is the instantaneous utility for an unemployed generated, for instance, by unemployment benefits; p is the probability of exiting unemployment; and V_0 is the utility from a new job. In a complete model these equations are combined with the Bellman equations for the firm, a wage setting mechanism (Nash bargaining is usually assumed) and conventional assumptions on the

[1] The other component of employment protection (administrative cost of dismissal) do not appear here because they are borne by the firm and therefore do not appear in the workers' utility function.

matching technology leading to the endogenous determination of the wage and of the threshold productivity which in turn determines the transition probabilities between employment and unemployment. Employment protection enters the model in the form of severance payment (as in equation 2) and administrative dismissal costs borne by the firm. Since EPL modifies the value functions, it affects both the wage and, through the threshold productivity level, the transition probabilities therefore producing both direct and indirect effects on workers' utility. When temporary employment is considered as well, the model becomes more complicated, but the essential mechanisms at work remain the same although the wage for temporary workers is normally taken as given.

Only few papers have modelled both temporary and permanent contracts in a matching model (Wasmer [48], Blanchard and Landier [7], Cahuc and Post-Vinay [15])[2]. While the paper by Wasmer [48] is primarily concerned with the conditions under which a firm hires both temporary and permanent workers, more relevant for our analysis are the other two papers.

In Blanchard and Landier [7] all homogeneous and risk-neutral workers are initially hired on a fixed-term contract featuring lower firing costs than a regular contract. After a productivity shock occurs, firms either keep the worker on a regular contract or dismiss him and look for another worker to hire on a fixed-term contract. In this framework, a decrease in the firing costs for temporary workers (holding those for permanent workers constant) increases the layoff rate but has ambiguous effects on the exit rate from unemployment and on the level of unemployment itself. Moreover, it is showed that reducing EPL for temporary contracts, on one hand, reduces the differential between the value of a temporary job and that of unemployment and, on the other hand, increases the differential between the value of a permanent and a temporary job, fostering a form of dualism in the labour market.

In the model proposed by Cahuc and Post-Vinay [15] a fixed-term contract entails no firing costs at all and therefore risk-neutral firms always prefer to employ temporary workers[3]. When a temporary contract expires a firm can choose whether to convert the contract into a permanent one or to dismiss the worker. Permanent contracts exist because of a legal restriction on the quota of temporary employment, which means that only an exogenous fraction of the employees is actually hired on a fixed-term contract. In this set up, it is shown that the higher the firing cost for permanent workers, the lower the conversion

[2] A different approach is taken by Dolado *et al [20]* who do not model temporary workers, but allow firing costs to different between two different groups of workers. There is only one type of job for which both types of workers compete. This is meant to capture the fact that in some countries flexible contracts are designed for specific demographic groups. In this setup, simulations show that although a number of effects of a decrease in firing costs for different groups depend on the initial labour market state, a reduction of firing costs for the low-skilled group has larger positive welfare effects (for both types of workers) than the same reduction of firing costs brought about for both groups at the same time. This effect is driven by an increase in the wage following the improvement of the value of being unemployed (because of the Nash bargaining assumption) which is determined by the higher exit rate from unemployment.

[3] The assumption of risk-neutrality along with that of efficient surplus sharing implies that the homogenous workers of this model always prefer fixed-term contracts too.

rate from fixed-term to permanent contracts which means that the destruction rate of temporary jobs increases. Given that a higher firing cost also reduces job creation and destruction for permanent jobs, the overall effect on unemployment is again ambiguous. On the other hand, less severe restrictions on the use of temporary contracts increase both job creation (because temporary jobs, not entailing firing costs, yield a higher surplus) and job destruction (because the improvement in the outside option following the increase in job creation increases the threshold productivity below which a match is ended), with the latter effect being particularly strong when firing costs are high. Theoretically, therefore, the effect on unemployment is ambiguous and so is the effect on the welfare of the employed who are faced with both an increase of the value of unemployment (because of the increase in the exit rate from unemployment) and an increase in the probability of becoming unemployed. Nevertheless, through simulations calibrated for a typical European labour market, Cahuc and Post-Vinay [15] actually obtain that allowing for more temporary contracts when firing costs are high has a negative effect on unemployment and reduces aggregate welfare.

Overall, the received literature does not deliver clear predictions on the well-being effect of employment protection but it does provide convincing indications concerning the channels through which EPL may affect wellbeing, namely the wage and the transition probabilities across different labour market status. These indications will be the base of the empirical analysis of the remainder of this paper.

2.2 Empirical literature

Two recent strands of empirical work seem of particular relevance to this study. The first group of papers has focused on evaluating the quantitative effects of employment protection regulation on aggregate labour market performance, while the second one has looked at the characteristics of temporary employment in a few cases making use of subjective wellbeing measures.

Most of the papers belonging to the first body of literature have looked exclusively at the aggregate effects of firing restrictions for permanent workers, while a few recent ones have considered regulations for temporary employment as well. Within this latter group, OECD [35] and [37] uses cross-sections or panels of countries, while Blanchard and Landier [7] and Dolado et al. [19] look at the experiences of France and Spain respectively. The general conclusion of these studies is that relaxing restrictions on temporary employment while keeping protection for permanent workers unchanged does not necessarily lead to an improvement of labour market conditions. While the reader is referred to the aforementioned papers for a thorough treatment of the subject, for our purposes it is interesting to mention that Blanchard and Landier [7] attempt to evaluate the welfare effects of labour market reforms in France. They construct a measure of the average expected present value of utility using available information on income in different states and transition probabilities across different states and conclude that there is some evidence of a negative effect of these reforms on young workers' welfare.

Research into temporary employment has been motivated by the growth of atypical employment in Europe over the last fifteen years. A few figures are enough to grasp an idea of the size of the phenomenon. In the year 2000 the temporary employment share was 13.7% in the EU-15 and above 10% in ten of the EU-15 countries. Moreover, between 1990 and 2000 in three of the four EU largest economies (France, Germany and Italy) temporary employment contributed to the growth of overall employment more than permanent employment (OECD [36]). Although, as noted by Booth *et al.* [11] for Britain, studies of temporary employees began only in recent years, some interesting facts are told by the available empirical literature concerning the characteristics of these workers and their subjective wellbeing.

The OECD [36] uses ECHP data and conclude that young and less educated workers and those employed in low-skills occupations, agriculture and small firms are more likely to hold temporary jobs. Besides, a significant wage penalty for fixed-term workers is found in all EU countries. Comparison of mean job satisfaction also reveals that temporary workers are less satisfied than permanent workers, especially when it comes to job security. Finally it is found that a considerable share of temporary workers move into permanent jobs within two years, and that more educated workers have a significantly higher probability of receiving training and of moving into permanent employment.

Booth et al. [11] use data from the first seven waves of the BHPS and conclude that British temporary workers receive less training and are less well-paid than permanent workers, but also find evidence that fixed-term contracts are mainly used as stepping-stones toward stable employment. Although they do find that temporary workers are less satisfied with job security, they do not find a negative effect on overall job satisfaction using pooled ordered probit regressions. This is consistent with the finding of Bardasi and Francesconi [3] who do not find long-lasting negative effects from being employed on an atypical contract using several wellbeing variables from ten waves of the BHPS. Quite interestingly, using a single cross-section of Australian data, Wooden and Warren [49] find a positive effect of being on a fixed-term contract on overall job satisfaction. Petrongolo [39] uses ECHP data and finds that temporary employment is more frequent among women in most European countries and that job satisfaction is lower for temporary workers in all countries, especially in the South. Similar results are obtained by D'Addio et al. [17] using the first five waves of the ECHP for Denmark.

The study of employment protection and that of temporary employment have remained separate until the very recent contributions of Clark and Postel-Vinay [14] and Kahn [41].

Clark and Postel-Vinay [14] study satisfaction with security of male respondents in the last five waves of the ECHP and use an overall measure of EPL exhibiting only cross-country variation. They use a latent class model to account for selection of workers into different types of jobs and find a negative relationship between (overall) EPL and perceived job security for temporary and permanent workers.

In the paper by Kahn [41] single cross-sections from different countries are pooled together to investigate the effects of employment protection legislation for permanent workers only on the incidence of unemployment and temporary employment for several demographic groups. The paper provides evidence that stricter employment protection is associated with higher joblessness for the young and women and with a higher incidence of temporary employment for the young, women and those with low cognitive ability (as measured in the International Adult Literacy Survey).

This paper contributes to the recent literature investigating the effects of employment protection on temporary employment by looking at the well-being effects of labour market reforms using panel data from 13 countries and a recent EPL indicator proposed by the OECD which exhibits both time-series and cross-country variation.

3 Empirical Analysis

3.1 Theoretical predictions

In section 2.1 we pointed out that EPL can be expected to have an effect on worker's satisfaction through the wage and the transition probabilities between employment and unemployment. These predictions were obtained within matching models, but are consistent with those of the dynamic labour demand reviewed in Bertola [6]. Additional insights on the effects of regulations on workers' wellbeing are provided by the *insider-outsider* literature, as employment protection is one the factors which can increase insiders' bargaining strength.

Integrating these theoretical predictions with simple intuitions suggested by the nature of the EPL indicator employed in our analysis, we can summarise the complex ways in which legal protection may affect workers' wellbeing as follow.

For permanent workers:

1. Direct effect: EPL for permanent workers encompasses severance pay and therefore enters the expected utility function of a worker directly and positively. As has been showed by Lazear [42], if wages are flexible and workers are risk-neutral, then wages will be lowered to offset the future severance payment, leaving the worker indifferent. Nevertheless, even under these circumstances, one would still expect this effect to be positive after controlling for the wage.

2. Indirect effects:

   - Firing restrictions (in the form of administrative dismissal costs) increase the surplus to share through Nash-bargaining therefore producing a positive wellbeing effect through an increase in the wage (Pissarides [45] and Cahuc and Post-Vinay [15]). Similarly, in an insider-outsider model, EPL increases insiders' strength leading to higher wage.

- EPL for permanent workers reduces the probability of losing one's job, therefore increasing the expected utility of the worker. This result is obtained both in dynamic labour demand models (Bertola [6]) and in matching models (Pissarides [45]).

- Regulations for both temporary and permanent employees reduce the probability of exiting unemployment, therefore reducing the expected utility of the worker. Bertola [6] and Pissarides [45] show that EPL for permanent workers reduces job creation, while Cahuc and Post-Vinay [15] obtain the same effect following an increase in EPL for temporary workers. As for EPL for permanent workers, we can reasonably expect this negative effect to be offset by the positive one described in the previous point if the latter is strong enough and/or workers are myopic.

- Restrictions on temporary employment reduce the possibility for the firm to use it to adjust labour to shocks, possibly increasing the probability that permanent workers might be fired instead, therefore decreasing their expected utility. Along the logic of the *insider-outsider* models, Dolado et al. [19] argue that temporary workers can act as a buffer insulating permanent insiders from labour market shocks.

- Insofar as less strict regulations on temporary employment strengthens insiders' power, they might be able to obtain from the firm non-wage benefits (we are not able to control for in our regressions) which increase their utility.

For temporary workers:

- Stricter regulations reduce job creation and therefore the probability of finding a job once the current contract has expired. A more stagnant labour market reduces the expected utility of fixed-term workers who need to find a new job at the end of their current contract.

- The wage of temporary workers is usually taken as given in matching models and therefore any effect of EPL on wage is assumed away. However, Guell builds an efficiency wage model where temporary workers are mainly motivated by the probability of having their contract converted into a permanent one and therefore may receive a lower efficiency wage. That suggests that protection for permanent workers should be associated with higher wages for temporary workers since it makes more unlikely for a temporary contract to be converted. Restrictions on temporary employment, on one hand, make it difficult to have a contract renewed pushing the efficiency wage up; on the other hand, they make it more likely that the worker is kept on a permanent contract, therefore reducing the efficiency wage.

- Since temporary employment regulations impose limits to the number of renewals, they can reduce the probability of having a contract renewed, therefore lowering the expected utility of a temporary employee. On the other hand, limits to the number of renewal might increase the probability of obtaining a permanent contract with a positive effect on workers' utility.

- Low EPL for temporary workers for a given level of EPL for permanent workers, and a high EPL for permanent workers for a given level of EPL for temporary workers might reduce the probability of moving into permanent employment, with a negative effect on workers' expected utility. This effect is obtained by Cahuc and Post-Vinay [15].

- Temporary workers might be willing to accept current insecurity in exchange of future stability in which case EPL for permanent workers enters their expected utility function positively.

Hence, the only clear expectation that we have based on available theory and intuition is that of a negative effect of restrictions on the use of temporary contracts on permanent workers' wellbeing. Moreover, under the credible assumption that workers are more worried about losing their current job than finding a new one once unemployed, we can reasonably expect that the effect of firing restrictions on their wellbeing is positive. For temporary workers, the effect of both types of regulations are *a priori* ambiguous. In what follows we propose a reduced-form test of these hypotheses using job satisfaction and satisfaction-with-security data from seven waves of the European Community Household Panel.

3.2 Job Satisfaction

In this paper we take job satisfaction as a proxy for workers' utility and propose a simple reduced-form test to investigate the wellbeing effects of labour market regulations. In the ECHP, after being asked about their level of satisfaction with specific aspects of their job (earnings, security, time of work etc...), workers are asked to rate their overall satisfaction on a scale from 1 to 6. Argyle [2] offers quite a comprehensive survey of the psychology (and some economics) literature which has found that job satisfaction is positively correlated with life satisfaction and mental health. Also Frey and Stutzer [23] and Blanchflower and Oswald [9] contain a number of references to the psychological literature which has given a positive assessment of the reliability of self-reported well-being data. Moreover, both in economics (see Clark [13] for references) and in psychology (Argyle [2]) there is clear evidence that job satisfaction is a good predictor of quits. Argyle [2] also reviews the psychological literature which has found correlation between job satisfaction and absenteeism, job performance and employees' willingness to help coworkers and their organization.

More broadly, subjective wellbeing measures are now used in many areas of economics as recently documented in Layard [30] and [31]. The book by the Harvard psycologist Gilbert [24] provides a fascinating reading on the extent to which subjective wellbeing measures are employed in modern psycology.

With reference to temporary employees only, we also look at satisfaction with security as intuition suggests that such a satisfaction measure might be particularly sensitive to changes in employment protection legislation.

3.3 The empirical strategy

The ECHP has a longitudinal structure allowing us to go beyond the simple pooling of several cross-sections while taking advantage of both cross-country and time-series variation in the data. Nevertheless, the ordinal nature of the dependent variable (job satisfaction) poses some problems with the use of linear panel data models. In a cross-section, cardinality can be allowed by employing either an ordered probit or an ordered logit (to remain within fully parametric econometrics), but their extension to panel data presents some complications. In particular, a fixed effect (FE) ordinal model would clearly be the best option as it makes no assumption on the correlation between the individual-specific effects and the regressors. As discussed by Greene [25], the implementation of nonlinear FE models have encountered both a methodological and a computational problem. The methodological problem arises from the well-known result that the FE estimator of the binary probit is inconsistent due to the incidental parameter problem (Lancaster [29]). This means that even implementing the solution to the computational issue proposed by Greene [25], the resulting estimator would still be inconsistent. On the other hand, a random effects ordered probit (or logit) can be estimated (although it involves very time-consuming computations) by maximum likelihood. As it is well-known, the advantage of identifying the coefficients on time-invariant variables comes at the cost of the maintained assumption of no correlation between the unobservable individual effects and any of the included regressors. Failure of such an assumption means again that the ML parameter estimator is inconsistent. The remaining option is to resort to linear panel data models for which both RE and FE estimation commands are readily available at the price of having to assume cardinality of job satisfaction and obtaining predictions which are inconsistent with the scaling of the satisfaction variable. Moreover, while the RE linear model would still rest on the same assumption as its nonlinear counterpart, the FE linear model would miss most of the information contained in our data, given the limited time-series variability of the EPL indicator.

In spite of the described caveats, we choose to employ a random effects ordered probit in order to (1) control for unobserved (intercept) heterogeneity; (2) to allow the ordinal nature of the dependent variable to be taken into account; (3) to make use of both the cross-section and time-series information contained in our data. There are a few applications of this model in the literature, including that of D'Addio et al. [17] to job satisfaction and that of Engellandt and Riphahn [21] to temporary worker's effort at work. To implement the model we use the gllamm programme in STATA 9 (Rabe-Hesketh *et al.* [40]).

In order to contain the problems arising from the potential correlation between unobserved heterogeneity and the EPL indicator, we include country fixed-effects as discussed in section 3.4.1. Under the assumption that national legislation is correlated with national-level rather than individual heterogeneity, this enables us to remove a potential source of correlation between the error term and the variable of main interest for our analysis.

Another potential source of bias in our estimates is given by the possible selection of workers into contract types. One could argue that workers are selcted into contract types by firms on the basis of information contained in applications which are likely to be similar to that included in our data. In such a case, provided that our specification actually include the necessary controls, the estimates would not be affected by selction problems. Clearly, if workers are selected into contract types based on unobservables which also affect their reported satisfaction, our estimates of the coefficients on the included regressors will be biased. Probably because standard two-step procedures cannot be applied to a non-linear model to account for selection, most of the literature has not attempted to tackle this problem. Two exceptions are Engellandt and Riphahn [21] and Clark and Postel-Vinay [14]. The former uses workers moving between contracts types to conclude that there are not systematic differences between workers employed on a specific contract, while the latter uses a more complicated latent class model to account for selection into contract type when investigating the effect of EPL on satisfaction with security. In this paper, we do not control for selection and therefore caution is warranted in the interpretation of results.

As the variable of main interest of this paper (EPL) is defined at the national level, ignoring the fact that values are repeated within groups (countries) may lead to biased estimates of the standard errors and consequently to wrong inference (Moulton [33]). We adopt the correction for clustering commonly employed in empirical papers, but a word of caution is in order. Although it is hard to find mention of the issue in the applied literature, recent studies have stressed that for small numbers of clusters the common cluster-corrected standard errors are downward biased (Wooldridge [50], Cameron *et al. [12]*). Hence, we use conservative significance levels.

An additional problem arises when regional controls are included in the regressions, as we now have clustering both at the national level and at the regional level. Since with the software being used it is not possible to allow for clustering at two different levels, we opt for maintaining a correction for clustering at the national level as that allows correlation between observations within the regions (although in an homogenous way for regions within a given country), whereas clustering at the regional level would assume no correlation at all across observations in different regions within the same country.

3.4 Data and specification

The ECHP is a standardized longitudinal survey which was carried out between 1994 and 2001 in all member states of the EU under the coordination of Eurostat. It includes information at the household and individual level in a wide range of topics (income, employment, health, housing etc.). The target population of the survey is composed of all private households living in the national territory of each country. All people who were 16 or older in the year before the survey were eligible for personal interview, while information about younger people living in sample households were provided by the head of the household. For Germany, UK, and Luxembourg the sample in the first three waves is actually made up of an ECHP-specific sample and a subsample of national panels. In the remaining waves, the whole subsample for these countries comes from their national panels. A detailed description of the dataset and a discussion of the methodological issues involved is in Peracchi [38].

The ECHP contains a general job satisfaction question which asks respondents to rate their satisfaction on a 1 to 6 scale with 6 being the highest level. However this information is not available for Sweden and Luxembourg which are therefore excluded from our study. We therefore use data from 13 different countries. For Germany, the job satisfaction question was only asked to the ECHP-specific sample in the first three waves. Moreover, the German sample does not have information about the region of residence, which means that we cannot use that country when including regional unemployment as a covariate. Data on regional unemployment rate have been taken from the REGIO database of EUROSTAT and matched with the regional identifier available in the ECHP[4]. Since data from the first wave do not include information on the type of contract of a worker, the empirical analysis only uses data from waves 2 to 8.

The EPL indicator employed is the one presented in OECD [35] (later revised and updated in OECD [37]). It results from the aggregation of 18 basis indicators concerning three broad areas of regulation: 1) regular contracts 2) temporary contracts and 3) specific requirements for collective dismissals. In this paper only the indicators for regular and temporary contracts are considered. It is important to note that while the indicators for regular contracts actually measure legislative impediments to firing workers and therefore some kind of protection for these workers, those measuring EPL for temporary workers capture limitations on the use of temporary employment whose role in providing protection for the workers is questionable. A detailed description of how a score is attributed to each indicator can be found in OECD [37].

The indicators are constructed for three points in time (late '80s, late '90s and 2003), and a time-series for regular and temporary workers is then obtained by allowing the indicator to vary when the main changes in legislation occurred

[4]In some cases, the level of aggregation did not coincide between the two datasets. Where the ECHP presented regions at a lower level than the REGIO datasets, each region was assigned the same unemployment rate.

For some regions, data were missing for the first year (1994), while they were available at a higher level of aggregation. In such cases the data for 1994 were reconstructed as follow: first, the average ratio between the regional unemployment and that of the larger region within which that region is contained was calculated over the available years; then the missing data were obtained by applying the same ratio to the unemployment rate of the larger region in the year for which the data were missing.

For The Nederlands and Denmark the unemployment rate is the national unemployment rate. This is also for most regions of Portugal and for Ireland excluding Dublin. For Italy, the unemployment rates for the macro regions of the ECHP were calculated as simple averages of the available data for the individual regions.

between these points in time[5]. The OECD indicator has been used in other papers before (Kahn [41] is only the latest example), but this appears to be the first study making use of the new time-series.

3.4.1 Model specifications

We focus on employees aged between 16 and 65 and adopt different specifications in an attempt to uncover the channels through which labour market regulation affect workers' satisfaction. Common to all the specifications is the inclusion of the following standard controls for job satisfaction equations: age, age squared, female, part-time, married, education, occupation and industry dummies[6], a dummy for (self-reported) health problems, log of hours worked per week, dummies for the presence of children in the household and job status dummies (supervisionary, intermediate or other). The ECHP also contains other interesting variables which would be obvious candidates as explanatory variables in a job satisfaction equation. For example, firm size (as reported by the employees) and extra-wage benefits provided by the employer are likely to be among the determinants of workers' satisfaction. However, these (and other) pieces of information are missing for a large number of individuals and particularly so for some specific countries (such as the UK and France). Since our main interest here is on a variable that does not vary within a country, the cost of including these variables appears too high.

The first specification we use does not include a control for the wage. This is meant to let the legislation indicators capture the direct and indirect effects of EPL on satisfaction as suggested by the literature. We do note that, although the wage is not controlled for, the other regressors generally appear in standard wage equations. To the extent that these covariates are correlated with the (omitted) wage, we expect the indirect effect of EPL through the wage not to be confounded with that of observable characteristics of the individuals.

In the second specification we include the log of monthly real net wage (in PPS) to look at the effect of EPL for a given wage. Thirdly, we use a specification including transition probabilities to try to isolate the direct effect of legislation on job satisfaction. The transition probabilities are estimated from a multinomial model following the procedure outlined later in this paper.

Clearly, we are faced with the common problem of controlling for other institutions to pinpoint the specific effect of EPL. In an attempt to limit these identification problems we control for country fixed effects and for time-specific effects. Since that amounts to controlling for fixed differences across countries, this means that even though we make use of a random effects estimator, the effect of legislation is actually identified through different changes over time in

[5] For a detailed description of these changes, the interested reader is referred to Oecd [37]. The time-series has not been published but has been obtained thorugh a direct contact with the OECD.

[6] Since computation time is proportional to the squared of the number of parameters, we adopted a parsimonious specification including only 2 industries dummies for Industry and Services instead of the 16 which ideally we could have included.

EPL taking place in different countries rather than through the between countries variation in EPL. Identifications problems would arise if other institutions were subject to changes correlated to the EPL changes. We are particularly concerned with Unemployment Benefits (UB) which can easily be suspected of playing a role complementary (or alternative) to EPL in insuring the worker, therefore affecting reported satisfaction. We discuss the problem and the way we try to tackle it in a later section.

The inclusion of the country dummies might also be seen as a way of tackling the potential endogeneity of EPL if the assumption holds that national legislation is correlated with unobserved (time-invarying) country-level effects rather than individual unobserved effects. Intuitively, this is likely to be the case if EPL strictness reflects cultural differences across countries.

We acknowledge that the possible interaction of institutional factors in shaping workers' wellbeing should be more carefully considered in future research.

Some of the specifications aforementioned are in some instances extended to include the regional unemployment rate. We expect it to capture features of the local labour market which go beyond those determined by EPL for two main reasons: in the first place EPL is defined at the national level (although it might be argued that its enforcement differ across regions), while countries exhibit quite large cross regional variations in unemployment; in the second place, theory predicts that the effect of EPL on unemployment is ambiguous and empirical studies do not find a clear correlation between unemployment and EPL. The inclusion of local unemployment is in line with the work on happiness and life satisfaction of Di Tella *et al.* [18] and Blanchflower and Oswald [8].

4 Results for permanent workers

Based on the theory summurised in section 3.1, when considering permanent workers we expect the overall effect of firing restrictions to be positive while that of restrictions on temporary employment negative. Column 1 of table 1 show that when the wage is omitted, the two regulation indicators do attract the expected signs, but fail to reach statistical significance. When the wage is included in the specification the results look consistent with those obatined in the literature for all employees taken together and the coefficients on the regulation variables change slightly. In particular, while protection for permanent workers remain positive but statistically insignificant, restrictions on temporary employment are now significant at the 10% level, a result that must be taken with caution given the possible downward bias of the standard errors due to the small number of clusters in the data (Cameron *et al.* [12]). Nevertheless, it is informative to look at the size of these correlations. As it is wellknown, in an ordered probit the coefficients are not the marginal effects, nor are the marginal effects themselves very interesting in the analysis of subjective wellbeing as confirmed by their scarce use in the literature. To make the size of the relevant coefficients easy to interpret we can compare them to the coefficient on the wage. Our estimates indicate that an increase in firing restrictions from the lowest level (.95 in the UK) to the median level (2.61 in Spain) has the same effect on the distribution of reported satisfaction as an impressive 58% increase in real wage. Similarly, an increase in restrictions on temporary employment from the lowest level (.25 in Ireland and UK) to the median value (3.25 in Spain) is equivalent to a 36% cut in wage.

Finally, when unemployment is controlled for (column 3), the two regulations chage only marginally and local unemployment itself shows a negative but insignificant coefficient.

To summarise, since our estimates fail to reach satisfactory levels of statistical significance, we are not able to draw strong conclusions from these results. However, we do find that in our sample permanent workers' satisfaction is positively correlated with firing restrictions and negatively correlated with restrictions to the use of temporary employment as expected from economic theory. Given the size of these effects, in Hamermesh [26]'s words, we can say that "the best estimate [of the impact of regulations on job satisfaction] is economically significant."

5 Results for temporary workers

5.1 Job Satisfaction

We begin by looking at the job satisfaction equation for fixed-term workers in table 2. We are not aware of published job satisfaction equations for fixed-term workers alone, but the results obtained seem consistent with the standard results for all employees at least in terms of signs. We can therefore focus on the results for regulations. Column one shows that the overall effect of firing restrictions on temporary workers' satisfaction is negative and statistically significant at the 1% level, while that of protection for permanent workers is positive but far from reaching statistical significance at any conventional level.

The introduction of the real wage into the specification in column 2 does not alter the results for the regulation indicators and we can again reject the null of a zero coefficient on temporary employment even at the most conservative level. The magnitude of the coefficient is slightly larger than in the previous specification and comparison with that on log of real wage reveals that the Italian reforms (the largest in Europe over the period considered) were equivalent to a 44% increase in real wage.

When the specification is extended to include local unemployment, this variable exhibits a negative and significant coefficient and the coefficients on both types of regulations increase in magnitude with that on EPL for permanent workers now reaching statistical significance at the 5% level. As previously discussed, the inclusion of the unemployment variable causes the loss of all observations for Germany, but several checks performed with a pooled ordered probit seem to indicate that the significance of the EPL variable in this specification is not driven by the different sample used[7]. The size of the coefficients

[7] These results are available from the author upon request.

imply that an increase from the minimum level of restrictions (0.95 in the UK) to the median level (2.61 in Spain) is equivalent to a 75% increase in real wage.

When the job satisfaction equation is estimated separately for males and females, the negative and significant sign on regulation for temporary contracts is only found for females, as shown in columns 1 and 2 of table 3. This result, coupled with the lower coefficient on income for women, leads to the striking result that the Italian labour market reforms were equivalent, in terms of job satisfaction, to a 95% increase in wage. Similarly, in columns 3 and 4 we find that only young temporary employees are negatively affected by restrictions to the use of temporary employment and since their satisfaction seems to be relatively little sensitive to income, the estimated coefficients imply that the Italian reforms are equivalent to a 59% increase in wage, whereas for adults the figure is 27% (but note that the coefficient on the regulation variable does not reach statistical significance).

Overall, our results provides clear evidence of a negative relationship between restrictions on temporary employment and wellbeing of temporary employees in particular for women and the young. Although this might seem at odd with the standard term of "employment protection" commonly adopted for this legislation, this finding can be explained by economic theory as the consequence of the stagnation of more heavily regulated labour markets. Clearly, fixed-term workers are more likely to be affected by a stagnant labour market as they need to find a new job at the end of their current contract. Moreover, stricter restrictions make it harder for temporary workers to have the contract renewed and to continue to work on fixed-term contracts if they wish to do so. The obvious implication is that recent reforms which have relaxed regulation on temporary employment in Europe have had a positive effect on temporary workers' wellbeing. This result is in contrast with the theoretical prediction and empirical finding of Blanchard and Landier [7] that a reduction in firing costs for temporary workers lowers their utility.

We have also obtained some evidence of a positive correlation between protection for permanent workers and temporary workers' satisfaction, which appears large in magnitude and statistically significant when unemployment is included as a regressor. This finding can be explained by the higher protection that workers will obtain once they will have been hired on a permanent contract. In other words, if temporary workers see their temporary contract as a stepping stone towards permanent employment, they might be willing to accept their current insecurity (which theory suggests is caused also by the strictness of regulations for permanent workers) in exchange for future higher protection. We attempt to further investigate this hypothesis later in the paper.

Finally, we find that unemployment exhibits a negative and significant coefficient. Di Tella *et al.* [18] and Blanchflower and Oswald [8] have found a negative correlation between other subjective wellbeing measures and unemployment, but we are not aware of any paper which distinguishes between permanent and temporary employees. While our finding is consistent with the efficiency wage argument proposed by Blanchflower and Oswald [8] (higher unemployment worsens the outside option for workers), we can also conjecture that where unemployment is higher, temporary employment might be less voluntary.

5.2 Satisfaction with Security

Table 4 shows that when the dependent variable is satisfaction with security, the coefficients on both regulation variables turn out positive but statistically insignificant, but when unemployment is included in the specification, EPL for permanent workers becomes highly significant. Because the real wage is now relatively less important in terms of satisfaction with security, the coefficient on EPL for permanent employment now implies that an increase from the minimum to the median value of the indicator is equivalent to a threefold increase in the wage. This finding is consistent with the interpretation provided above whereby currently temporary workers value the security they will enjoy once on a permanent contract but seems to be at odds with the view traditionally taken in the political economy literature whereby only insiders on permanent contracts support employment protection legislation (see for example Saint-Paul [46]). On the other hand, the failure to reach statistical or economic significance for the restrictions on the use of temporary employment seems to suggest that the effect of such regulations on general satisfaction found above does not take place through satisfaction with security. The unemployment variable is again negative and highly significant confirming that bad local labour market conditions generate a sense of insecurity in workers.

A striking result in the first two columns of table 4 is the negative and significant coefficient attracted by the dummy for those who work in the public sector on a temporary contract. A possible explanation is that temporary workers in the public sector suffer more from the insecurity of their work compared to those in the private sector because they compare themselves to the arguably most secure group of workers in the labour market, i.e., the public sector permanent employees. Another surprising result is the negative coefficient attracted by the female dummy which, by contrast, is usually found positive and significant in job satisfaction equations, including those presented in table 1. This finding is puzzling: women seem to be more satisfied with their temporary job overall, but then they are less satisfied with what is arguably the most specific aspect of it, that is, its inherent insecurity[8]. A negative coefficient in both equations could be interpreted as evidence of women being segregated into temporary contracts more than men, but the apparently contradictory signs obtained point to the hypothesis that women have a stronger taste for job security than men even after controlling for the presence of children.

When we look at satisfaction with security separately for men and women, we find that regulations on permanent contracts attract a positive and significant coefficient for both genders providing additional support to the hypothesis

[8]The negative and statistically significant coefficient was consistently found when pooled ordered probit was run with and without a control for unemployment on the same sample, which suggests that the negative sign is not simply a result of changes in the sample composition. Also, the positive sign for the female dummy in the job satisfaction equation was always confirmed after the same checks.

that temporary workers favour employment protection legislation for permanent workers. The results in table 4 also show that restrictions on temporary employment is positive for men, but negative for women and in both cases not statistically significant. Temporary employees in the public sector report lower satisfaction with security than their private sector counterpart regardless of their gender.

6 The role of transition probabilities

As discussed in section 3.1, economic theory suggests that employment protection affects workers' wellbeing directly, through the wage and through the transition probabilities across different labour market states. In the remainder of the paper we conduct a preliminary investigation of the role of transition probabilities in determining the effect of legislation on workers' reported satisfaction. To do so, we use a multinomial model which also allow us to explore two other issue: 1) the role of unemployment benefits and 2) the reasons why we find that temporary employees are positively affected by permanent workers' protection.

Using a multinomial model including the regulation variables as regressors, for each worker on a given contract at time t we predict the probability of being in one of the following states at time $t+1$: 1) employed on the same contract; 2) employed on a different contract; 3) unemployed without benefits; 4) unemployed with benefits. The details of the procedure followed and the results are discussed in the following sections.

6.1 Controlling for unemployment benefits

The absence of controls for institutions other than EPL makes it difficult to identify the specific effect of labour market regulation. The inclusion of country and time effects is of limited help in tackling this problem as it does not account for the heterogeneity of an institution within a given country. Among all the labour market institutions, both theoretical and practical arguments suggest that in this paper special attention should be given to the role of unemployment benefits (UB). From a theoretical point of view, UB's affect the value of different labour market status and provide a form of insurance for employed workers producing an effect similar to that of employment protection on workers' wellbeing. The concern that the effect of EPL may be confounded with that of UB is strengthened by the empirical observation in Boeri et al [10] that the two are negatively related in Europe.

As in Clark and Postel-Vinay [14], one could control for UB by using the country level indicator constructed by the OECD[9], but we deem this solution not satisfactory for several reasons. At a very practical level, the indicator is only available for odd-number years and we would have to average it for the

[9] See Martin [32] for a detailed description of the indicator and Clark and Postel-Vinay [14] for a paper using the indicator in satisfaction equations.

missing years adding an additional degree of approximation to the indicator. Moreover, we would be including in the equations another national variable with limited variability and probably posing more identification questions than it answers. Finally, we would be forced to use the same UB variable for both temporary and permanent workers, whereas the OECD [36] warns that (p.45):

> Even when temporary workers are subject to the same rules as permanent workers, their *de facto* entitlement to benefits may be more limited. In particular, temporary workers may fail to gain access to some or all benefits when entitlement conditions are formulated in terms of earnings thresholds and minimum duration of employment or minimum contribution periods. (...) Administrative complexity or confusion may also limit the *de facto* entitlement of temporary workers to benefits to which they are *de jure* entitled.

Therefore, in an attempt to obtain a control for UB based on the de facto rather than de jure entitlement, we turn again to our dataset.

The ECHP contains information which in principle could be used to build a measure of the expected UB for a person currently employed. In particular two items are available in the survey:

1. people who are currently unemployed are asked whether at present they receive "any unemployment benefit or assistance";

2. all interviewed persons are asked to provide the total annual amount of "unemployment related benefits" they received in the year prior to the survey.

Given the absence of information on current UB, we cannot build a prediction of the expected UB conditional on individual and past-job characteristics. Even the simple prediction of the probability of receiving UB conditional on previous job characteristics turns out to be not possible using currently unemployed people as much information on previous job is missing for a very large number of people. Likewise, using information on the amount of UB received in the previous year combined with previous year information on labour market status proved impractical due to the very limited sample size we end up with.

We therefore pursue another venue and use a multinomial logit to estimate the probability that an employee working a time t will be unemployed with or without UB at time $t+1$.

6.2 Transitions in the ECHP

Since we are interested in workers who are either on a temporary or permanent contract at time t, we restrict the sample accordingly. Based on the information available in the ECHP, we can distinguish several labour market status to which employees can move from one year to another. However,Table A.1 in the Appendix shows that some of the possible destination status are rather seldom reached. In particular, less than 3% of the observed transitions end in self-employment and similarly small fractions of employees who remain in employment end up on a contract other than a permanent or a temporary one. The limited number of observations for such outcomes is likely to result in rather imprecise estimates. Moreover, since in a multinomial setting the number of parameters to be estimated grows proportionally to the number of outcomes, the costs of including all the possible outcomes seem to outweigh the potential benefits. On the other hand, we are reluctant to drop these categories all together for that would reduce the sample size. We therefore opt to merge some of the categories making sure that this is not essential for our successive results.

Even considering all the possible outcomes listed in table A.1, one could object that whether one moves into inactivity following retirement (and therefore presumably with some sort of pension) or for other reasons could make an important difference. However, we note that less than 1.5% and less than 1% of permanent and temporary employees respectively retire in the following year. Again, such small numbers make it very difficult to propose a convincing remedy to the objection, but on the other hand they also seem to indicate that the practical relevance of this case may be limited.

As discussed in section 6.1, we are interested in estimating the probability of a worker receiving UB in the following year. Appendix A presents some figures concerning the observed transitions between consecutive years ending with the worker receiving UB. As we were expecting based on the discussion in section 6.1 we do find that the probability of receiving UB varies to a large extent between different types of contracts, but the sign of this difference is surprising. In fact, we observe that slightly less than 6% of the observed transitions out of temporary employment end up with the former worker receiving UB, while only slightly more than 1% of workers moving out of permanent employment receive UB in the following year. However, if we focus on only those transitions from employment to unemployment or to the discouraged pool, the order changes although the difference between the two groups is rather small: once unemployed, more than 36% of permanent workers and around 35% of temporary employees get some UB.

After re-grouping the observed transitions as shown in table A.3, we are left with a sample of more than 180.000 transitions spanning over 6 years and 12 countries, and with less than 2% of transitions leading to out-of-employment status with UB.

6.3 The econometric model

We use a multinomial logit model to predict transitions into different labour market states for each individual and then use these predicted probabilities as regressors in the job satisfaction ordered probit. The specification for the multinomial logit include the same regressors as the job satisfaction equation. The inclusion of the regulation indicators in the multinomial specification allows us to capture (in an admittedly rough way) the effect of regulation on transition probabilities. Because of the nonlinearity of the multinomial model, identification of the coefficients on the predicted probabilities in the ordered probit is achieved even without imposing zero restrictions on the job satisfaction equation. It follows that identification then rests on the parametric assumption that we make when adopting a multinomial logit. Clearly, there is no obvious case to favour of one parametric assumption over another, and therefore a natural extension would check the robustness of the results to different parametric assumptions (and possibly to different zero restrictions).

Once the predicted probabilities have been obtained it is obviously not possible to include all of them in the ordered probit for job satisfaction as they sum up to 1 creating a problem akin to the dummy trap. Clearly, the choice as to which categories to omit has substantial bearing on the interpretation of the obtained coefficients. In fact, given that the probabilities always have to sum up to 1, omitting the probability of a given event is equivalent to assuming that any change in the included probabilities is mirrored by an opposite change in the omitted probability (and only in that one). More formally, if β_i represents the effect of $\Pr(x_i)$ on y (with $\sum \Pr(x_i) = 1$), then when $\Pr(x_j)$ is omitted from the equation for y, we have that only $\beta_i - \beta_j$ is identified for each $\Pr(x_i)$ with $i \neq j$.

As we will explain shortly, we are mainly interested in the transition probabilities to permanent employment and to unemployment without benefits. We therefore choose to include these two predicted probabilities in the extended job satisfaction equation, implicitly assuming that any change in these probabilities is mirrored by a change in the probabilities of moving (or staying) into a temporary job or moving into unemployment without benefits. The model we estimate can be written in terms of latent job satisfaction (JS^*) as:

$$\begin{aligned} JS^*_{it} = {} & X'_{it}\beta + \delta_1 \text{EPL-Reg} + \delta_2 \text{EPL-Temp} \\ & + \gamma_1 \Pr(\text{Permanent Contract in } t+1)_{it} \\ & + \gamma_2 \Pr(\text{Unemployed with UB in } t+1)_{it} \\ & + \gamma_3 \Pr(\text{Permanent Contract in } t+1)_{it} * \text{EPL-Reg} \\ & + \eta_\iota + \varepsilon_{it} \end{aligned} \tag{4}$$

The interaction term is included to test the hypothesis that temporary workers' wellbeing is positively affected by protection for permanent workers because they anticipate future protection. The test is based on the intuition that if this is the case, then those temporary workers who are more likely to move to a permanent contract should be more affected by protection for permanent contracts.

We are also faced with an inferential problem arising from the fact that our job satisfaction equation includes generated regressors. As pointed out by Pagan [44] and Murphy and Topel [43], not accounting for the additional uncertainty stemming from the presence of the estimated regressors can lead to misleading inference as, generally, the naive standard errors will be underestimated. Recently, Hardin [27] has discussed a correction for the standard errors which would also allow to maintain the correction for clustering that we need.

We leave for future improvements of our analysis the application of such a correction, while in this paper we try to limit the inferential problems by adopting conservative significance levels.

6.4 Results

The results for two multinomial logits on temporary and permanent workers separately are reported in Appendix A . As these models are run solely to obtain predictions of transition probabilities, they are estimated without any correction for the standard errors and any inference should be drawn with great caution. The predicted probabilities obtained from these models are then used to estimate the satisfaction equations reported in table 5.

The first two columns of table 5 report the results of the job satisfaction equations including the transition probabilities. For permanent workers, firing restrictions and restrictions to the temporary employment attract again a positive and a negative coefficient respectively, as expected from economic theory. While the former remains statistically insignificant, the latter is now on the border of statistical significance at the 5% level but still warranting caution because of the generated regressor problem mentioned above. The size of the effect appears smaller than in the previous specification compared to the wage effect, as now an increase in restrictions on temporary employment from the lowest (.25 in Ireland and UK) to the median value (3.25 in Spain) is equivalent to a 24% wage cut. The coefficient on the transition probabilities to permanent employment and to unemployment with UB are both positive, a result which is not surprising given that the omitted transitions are those to temporary employment and to unemployment without UB. Neither of the probabilities reaches statistical significance at any conventional level though.

Results for temporary workers in the second column reveal that the inclusion of transition probabilities does not alter the results for the regulation variables with protection for permanent workers remaining positive and statistically non significant, and restrictions on temporary employment negative and significant. The magnitude of this latter effect is larger than previously found, with the Italian reforms now being equivalent to a 58% increase in wage (against the previous 44%). Two surprising results follow. In the first place, we find that the probability of moving into a permanent job attracts a negative (albeit not statistically significant) coefficient. In the second place, we find no evidence that temporary workers who are more likely to move into permanent employment are more positively affected by protection for permanent workers, as the interaction term actually exhibits a negative and statistically insignificant coefficient. Finally, the probability of becoming unemployed with UB is positive and significant at the 5% level.

Similar results are obtained when we look at equations for satisfaction with security (column 3 of table 5) where the regulation variables are positive and statistically insignificant as in previous specifications. The probability of moving into permanent employment exhibits again a surprising negative coefficient, and, even more surprisingly, the probability of becoming unemployed with UB has a negative (and insignificant) coefficient as well. Again, we see that the interaction of firing restrictions with the probability of transition into permanent employment is negative and insignificant.

Overall, we can conclude that these preliminary findings indicate that controlling for unemployment benefits does not lead to results different from those obtained in previous specifications. Moreover, we do not find evidence of a clear role of transition probabilities in shaping workers' wellbeing, sometimes obtaining very surprising signs, although always statistically insignificant. We find no evidence that temporary workers who are more likely to move into permanent employment are more satisfied when employment protection is higher. We have also noted that the inclusion of the transition probabilities does not alter by much the results for the regulation variables. However, these results do not seem sufficient to conclude that transition probabilities are not one of the channels through which EPL affect workers' satisfaction. In fact, further investigation of the relationship between regulation and transition probabilities is necessary, possibly considering longer time-periods to estimate the transition probabilities, lagged effects of legislation, different specifications and parametric assumptions for predicting the probabilities.

7 Concluding remarks

The purpose of this paper is to carry out an empirical evaluation of the qualitative effects of recent labour market reforms in Europe. To this end we have investigated the wellbeing effects of employment protection legislation using panel data from 13 European countries over the period 1995-2001 along with a new OECD indicator for regulations which varies both over time and across countries. After reviewing the theoretical literature pointing out the direct and indirect effects (through the wage and the transition probabilities) of regulation on workers' wellbeing, we have first looked at job satisfaction equations for temporary and permanent workers separately without including any measure of the transition probabilities.

For permanent workers, we find some evidence of a positive effect of firing restrictions and a negative effect of restrictions on temporary employment. Both effects appear large, albeit not statistically significant. Economic theory suggests that restrictions on temporary employment may have a negative effect on permanent workers' wellbeing because they reduce (permanent) insiders' strength and increase their exposure to labour market fluctuations. Moreover, stricter regulations mean a more stagnant labour market and consequently a lower expected value of unemployment.

As for temporary workers, we find that their job satisfaction decreases when restrictions on the use of temporary employment increase. Again, this may be the consequence of a more stagnant labour market caused by heavy regulation. Clearly, fixed-term workers are more likely to be affected by a stagnant labour market as they need to find a new job at the end of their current contract. Moreover, stricter restrictions make it harder for temporary workers to have the contract renewed and to continue to work on fixed-term contracts if they wish to do so. Looking at different subsamples, we find that this result only holds for the young and women, consistent with the argument that flexibility increases job opportunities for these groups.

There is also evidence that protection for permanent workers increases both job satisfaction and satisfaction with security for temporary workers. This result is surprising given the theoretical result that higher protection for permanent workers makes it more difficult for temporary workers to enter the protected segment of the labour market. This could be taken as evidence that temporary workers anticipate the higher protection they will receive once on a permanent contract. Contrary to what we would expect under this hypothesis, we find no evidence that fixed-term workers who are more likely to move into a permanent job are more affected by employment protection for permanent workers.

Moreover, in a preliminary investigation, we do not find evidence of an effect of EPL through transition probabilities in shaping worker's wellbeing, nor do we find that controlling for the probability of receiving unemployment benefits affects the estimates of the effect of regulation on satisfaction.

Finally, we have obtained two other results which, although not directly related to the main focus of the paper, appear interesting in light of the previous literature. In the first place, we find that local unemployment negatively affects job satisfaction only for temporary wokers. Previous studies have found negative correlation between unemployment and other subjective well-being measures and the efficiency wage argument proposed to explain it appears even more convincing when applied to the subsample of temporary employees who are more directly threatened by unemployment. However, it is also possible that where unemployment is higher, temporary employment might be less of a voluntary choice.

In the second place, consistent with the consolidated evidence that women tend to report higher job satisfaction, we find that women on a temporary job report higher overall satisfaction than men. However, when it comes to satisfaction with security they are less satisfied than men. This result is somehow puzzling given that insecurity is arguably the most noticeable aspect of a temporary job and hints to the the conclusion that women have a stronger taste for security even after controlling for a number of personal and household characteristics (including marriage and the presence of children).

Overall these results indicate that the wellbeing effects of reforms easing restrictions on temporary employment are positive both for permanent and temporary employees. Moreover, they seem to indicate that the generally accepted view that only insider permanent workers oppose relaxation of employment protection legislation might not be correct.

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Table 1: Random Effects Ordered Probit:Job Satisfaction Equations for Permanent Employees.

	1	**2**	**3**
	Job Satisfaction	Job Satisfaction	Job Satisfaction
	Coef./SE	Coef./SE	Coef./SE
Age	–.040***	–.054***	–.053***
	(.009)	(.007)	(.008)
Age2	.001***	.001***	.001***
	(.000)	(.000)	(.000)
Child	.014	.016	.015
	(.017)	(.018)	(.018)
Married	.049***	.038***	.041***
	(.012)	(.013)	(.013)
Female	–.004	.067**	.060**
	(.028)	(.026)	(.026)
BadHealth	–.402***	–.389***	–.375***
	(.058)	(.059)	(.057)
Lnincome		.324***	.318***
		(.075)	(.078)
Lnhrs	–.104***	–.221***	–.218***
	(.037)	(.048)	(.049)
HigherEdu	.047	–.017	–.011
	(.055)	(.049)	(.052)
SecondEdu	.027	.002	.007
	(.036)	(.036)	(.039)
Supervisor	.290***	.245***	.239***
	(.053)	(.045)	(.045)
Intermediate	.142***	.122***	.118***
	(.034)	(.031)	(.031)
Professional	.264***	.190***	.188***
	(.046)	(.034)	(.037)
Serviceworker	.133***	.111***	.108***
	(.028)	(.025)	(.026)
PubliSector	.184***	.155***	.156***
	(.042)	(.035)	(.037)
FullTime	.059	–.023	–.011
	(.046)	(.038)	(.039)
Agriculture	.020	.070	.062
	(.058)	(.054)	(.051)
Industry	–.050***	–.066***	–.069***
	(.018)	(.018)	(.019)
EPL-Perm	.155	.114	.169
	(.101)	(.098)	(.119)
EPL-Temp	–.015	–.021*	–.024*
	(.012)	(.012)	(.014)
Unempl			–.007
			(.006)
	Year and Country Dummies Included		
Observations	218765	218765	208942

Significance levels: * 10% ** 5% *** 1%
SE adjusted for country clusters
ECHP data, 1995-2001, all employees aged 16-65

Table 2: Random Effects Ordered Probit:Job Satisfaction Equations for Temporary Employees.

	1	2	3
	Job Satisfaction	Job Satisfaction	Job Satisfaction
	Coef./SE	Coef./SE	Coef./SE
Age	−.038***	−.055***	−.053***
	(.007)	(.009)	(.009)
Age2	.000***	.001***	.001***
	(.000)	(.000)	(.000)
Child	.013	.019	.018
	(.019)	(.021)	(.022)
Married	.078***	.072***	.074***
	(.016)	(.017)	(.018)
Female	.032	.090***	.078***
	(.029)	(.017)	(.023)
BadHealth	−.350***	−.322***	−.331***
	(.064)	(.064)	(.066)
Lnincome		.381***	.357***
		(.113)	(.115)
Lnhrs	−.029	−.200	−.196
	(.113)	(.127)	(.120)
HigherEdu	−.141***	−.210***	−.214***
	(.040)	(.040)	(.044)
SecondEdu	−.090*	−.117**	−.125**
	(.051)	(.052)	(.052)
Supervisor	.355***	.281***	.292***
	(.069)	(.069)	(.072)
Intermediate	.145***	.109***	.121***
	(.033)	(.030)	(.026)
Professional	.547***	.459***	.448***
	(.055)	(.049)	(.050)
Serviceworker	.207***	.192***	.191***
	(.032)	(.031)	(.031)
PubliSector	.207***	.185***	.202***
	(.021)	(.016)	(.016)
FullTime	.182***	.098***	.114***
	(.052)	(.036)	(.030)
Agriculture	−.286**	−.259**	−.227***
	(.115)	(.109)	(.076)
Industry	.018	−.018	−.016
	(.037)	(.031)	(.031)
EPL-Perm	.021	.033	.162**
	(.046)	(.044)	(.078)
EPL-Temp	−.043***	−.052***	−.070***
	(.017)	(.016)	(.022)
Unempl			−.018**
			(.008)
	Year and Country Dummies Included		
Observations	25095	25095	24172

Significance levels: * 10% ** 5% *** 1%
SE adjusted for country clusters
ECHP data, 1995-2001, all employees aged 16-65

Table 3: Random Effects Ordered Probit:Job Satisfaction Equations for Temporary Employees by Age and Gender.

	Men	**Women**	**Young**	**Adults**
	Job Satisfaction	Job Satisfaction	Job Satisfaction	Job Satisfaction
	Coef./SE	Coef./SE	Coef./SE	Coef./SE
Age	−.060***	−.051***	−.044***	−.002
	(.007)	(.012)	(.007)	(.002)
Age2	.001***	.001***		
	(.000)	(.000)		
Child	−.001	.027	.029	−.023
	(.020)	(.036)	(.024)	(.021)
Married	.051***	.094***	.108***	.062**
	(.017)	(.032)	(.026)	(.026)
Female			.066**	.104***
			(.027)	(.038)
BadHealth	−.381***	−.254***	−.362***	−.303***
	(.066)	(.095)	(.121)	(.085)
Lnincome	.436***	.313***	.384***	.418***
	(.116)	(.109)	(.126)	(.104)
Lnhrs	−.215	−.178**	−.244*	−.209*
	(.205)	(.069)	(.139)	(.114)
HigherEdu	−.159***	−.267***	−.163***	−.209***
	(.054)	(.050)	(.032)	(.059)
SecondEdu	−.093	−.156***	−.123**	−.090
	(.065)	(.049)	(.049)	(.062)
Supervisor	.318***	.225*	.260***	.302***
	(.069)	(.115)	(.099)	(.067)
Intermediate	.138***	.074*	.099***	.134***
	(.030)	(.043)	(.037)	(.037)
Professional	.347***	.639***	.576***	.348***
	(.058)	(.057)	(.035)	(.091)
Serviceworker	.035	.358***	.215***	.180***
	(.042)	(.050)	(.044)	(.058)
PubliSector	.125***	.231***	.240***	.136***
	(.031)	(.021)	(.032)	(.035)
FullTime	.124	.111	.180***	.039
	(.115)	(.073)	(.038)	(.056)
Agriculture	−.225**	−.351***	−.219**	−.275*
	(.094)	(.130)	(.097)	(.146)
Industry	−.070***	.032	.005	−.046
	(.027)	(.046)	(.032)	(.049)
EPL-Perm	.026	.077	.039	.087
	(.059)	(.087)	(.074)	(.088)
EPL-Temp	−.011	−.090***	−.069***	−.034*
	(.025)	(.029)	(.027)	(.020)
Year and Country Dummies Included				
Observations	13196	11899	12588	12507

Significance levels: * 10% ** 5% *** 1%
SE adjusted for country clusters
ECHP data, 1995-2001, all employees aged 16-65

Table 4: Random Effects Ordered Probit:Satisfaction with Security Equations for Temporary Employees.

	All	**All**	**Men**	**Women**
	Sat with Security	Sat with Security	Sat with Security	Sat with Security
	Coef./SE	Coef./SE	Coef./SE	Coef./SE
Age	–.084***	–.079***	–.085***	–.082***
	(.015)	(.014)	(.014)	(.018)
Age2	.001***	.001***	.001***	.001***
	(.000)	(.000)	(.000)	(.000)
Child	–.053**	–.047**	–.055**	–.049
	(.024)	(.024)	(.023)	(.039)
Married	.026	.034	.020	.041
	(.022)	(.023)	(.037)	(.029)
Female	–.057	–.076**		
	(.037)	(.031)		
BadHealth	–.238***	–.246***	–.275***	–.207**
	(.050)	(.049)	(.073)	(.088)
Lnincome	.225***	.189***	.243***	.145**
	(.064)	(.060)	(.060)	(.069)
Lnhrs	.042	.036	.240***	–.147***
	(.056)	(.057)	(.082)	(.054)
HigherEdu	–.228***	–.246***	–.225***	–.262***
	(.062)	(.063)	(.068)	(.074)
SecondEdu	–.116**	–.128**	–.117*	–.147***
	(.054)	(.054)	(.069)	(.045)
Supervisor	.462***	.465***	.467***	.441***
	(.103)	(.110)	(.109)	(.156)
Intermediate	.157***	.162***	.183***	.132***
	(.025)	(.026)	(.031)	(.045)
Professional	.179***	.180***	.177**	.184***
	(.041)	(.043)	(.083)	(.047)
Serviceworker	.158***	.156***	.162***	.161***
	(.033)	(.034)	(.035)	(.054)
PubliSector	–.180***	–.160***	–.142***	–.162***
	(.031)	(.032)	(.046)	(.037)
FullTime	–.075	–.046	–.080	.048
	(.069)	(.072)	(.099)	(.083)
Agriculture	–.199***	–.142***	–.163**	–.101
	(.062)	(.039)	(.072)	(.087)
Industry	–.090***	–.093***	–.097**	–.086**
	(.033)	(.035)	(.038)	(.039)
EPL-Perm	.209	.376***	.339***	.475**
	(.128)	(.126)	(.106)	(.195)
EPL-Temp	.027	–.003	.050	–.050
	(.041)	(.040)	(.038)	(.048)
Unempl		–.025***	–.029***	–.019*
		(.010)	(.008)	(.011)
Year and Country Dummies Included				
Observations	25018	24097	12711	11386

Significance levels: * 10% ** 5% *** 1%

SE adjusted for country clusters

ECHP data, 1995-2001, all employees aged 16-65

Table 5: Random Effects Ordered Probit: Satisfaction Equations with Transition Probabilities.

	Perm	Temp	
	Job Satisfaction	Job Satisfaction	Sat with Security
	Coef./SE	Coef./SE	Coef./SE
Age	–.069***	–.078***	–.077***
	(.010)	(.028)	(.029)
Age2	.001***	.001***	.001***
	(.000)	(.000)	(.000)
Child	.017	.009	–.052**
	(.018)	(.022)	(.024)
Married	.046***	.104***	.022
	(.017)	(.031)	(.038)
Female	.069**	.091*	–.063
	(.027)	(.051)	(.069)
BadHealth	–.377***	–.375***	–.230***
	(.062)	(.086)	(.071)
Lnincome	.348***	.453***	.218**
	(.097)	(.160)	(.095)
Lnhrs	–.249***	–.213	.043
	(.057)	(.149)	(.066)
HigherEdu	–.003	–.126**	–.247***
	(.051)	(.050)	(.060)
SecondEdu	.006	–.095**	–.120**
	(.036)	(.045)	(.047)
Supervisor	.253***	.382***	.446***
	(.051)	(.087)	(.134)
Intermediate	.130***	.112***	.159***
	(.036)	(.042)	(.033)
Professional	.210***	.546***	.158***
	(.045)	(.081)	(.060)
Serviceworker	.121***	.213***	.156***
	(.031)	(.032)	(.047)
PubliSector	.179***	.132*	–.175**
	(.049)	(.069)	(.080)
FullTime	–.024	.064	–.067
	(.037)	(.047)	(.064)
Agriculture	.092*	–.304***	–.198***
	(.049)	(.064)	(.061)
Industry	–.067***	–.006	–.090**
	(.018)	(.048)	(.044)
EPL-Perm	.134	.140	.189
	(.104)	(.108)	(.152)
EPL-Temp	–.028*	–.081***	.033
	(.015)	(.021)	(.043)
Pr(Perm in t+1)	.231	–.272	–.024
	(.199)	(1.389)	(1.301)
Pr(No Empl w/UB in t+1)	4.116	2.910**	–.695
	(3.341)	(1.408)	(1.174)
Pr(Perm)*EPL-Perm		–.002	–.011
		(.109)	(.181)
	Year and Country Dummies Included		
Observations	218765	25095	25018

Continued next page...

...table 5 continued

	Perm	**Temp**	
	Job Satisfaction	Job Satisfaction	Sat with Security
	Coef./SE	Coef./SE	Coef./SE

Significance levels: * 10% ** 5% *** 1%

SE adjusted for country clusters

ECHP data, 1995-2001, all employees aged 16-65

Appendix A Transition Data

Table A.1: Percentages of Transitions between two consecutive years

	Contract at time t	
Time t+1	**Perm**	**Temp**
Selfemployed	1.85	3.31
Perm	83.25	26.4
Temp	3.35	31.5
Casual work	0.95	2.4
Other contract	1.12	1.88
Unemployed	2.93	15.29
Discouraged	0.19	0.72
Inactive	6.36	18.5
Total	100	100

ECHP data
Sample restricted as in the main analysis

Table A.2: Percentages of employees in t receiving UB in t+1

	Contract at time t	
Time t+1	**Perm**	**Temp**
No UB	98.87	94.33
UB	1.13	5.67
Out of Work in t+1	**Perm**	**Temp**
No UB	86.64	82.31
UB	13.36	17.69
Unempl/Disc in t+1	**Perm**	**Temp**
No UB	63.55	64.93
UB	36.45	35.07

ECHP data
Sample restricted as in the main analysis

Table A.3: Transitions from perm/temp employment between two consecutive years

Time t+1	Obs.	Perc.
Perm+Self	143,388	78.86
Temp+Other	16,074	8.84
Work, No UB	19,062	10.48
No Work, UB	3,292	1.81
Total	181,816	100

ECHP data

Sample restricted as in the main analysis

Appendix B Multinomial estimates

Table B.1: Transition Probabilities for Temporary Employees: Coefficient Estimates from a Multinomial Logit

	Transitions to		
	Perm Empl	Out of Empl	Out of empl w/UB
	Coef./SE	Coef./SE	Coef./SE
Age	.006	–.227***	.131***
	(.014)	(.014)	(.024)
Age2	–.000	.003***	–.001***
	(.000)	(.000)	(.000)
Child	.027	.151***	.074
	(.041)	(.043)	(.074)
Married	.092**	–.201***	–.113
	(.046)	(.050)	(.079)
Female	–.122***	.278***	–.087
	(.044)	(.046)	(.081)
BadHealth	–.014	.442***	.298
	(.139)	(.135)	(.194)
Lnincome	.250***	–.412***	–.353***
	(.059)	(.059)	(.103)
Lnhrs	–.108	.005	.088
	(.115)	(.121)	(.218)
HigherEdu	.147**	.373***	–.382***
	(.064)	(.065)	(.120)
SecondEdu	.147***	.275***	–.030
	(.050)	(.053)	(.088)
Supervisor	.139	–.436***	–.702**
	(.102)	(.144)	(.277)
Intermediate	.062	–.166**	.024
	(.062)	(.072)	(.112)
Professional	–.025	.071	–.616***
	(.068)	(.071)	(.129)
Serviceworker	.193***	.098*	.005
	(.058)	(.059)	(.098)
PubliSector	–.431***	–.229***	.007
	(.050)	(.052)	(.088)
FullTime	–.050	–.161	.119
	(.100)	(.102)	(.179)
Agriculture	–.704***	–.486***	–.167
	(.110)	(.107)	(.164)
Industry	.012	–.223***	–.080
	(.053)	(.056)	(.095)
EPL-Perm	.097	–.369***	–.796***
	(.080)	(.089)	(.145)
EPL-Temp	–.069	.038	.284***
	(.049)	(.053)	(.106)

Continued next page...

...table B.1 continued

	Transitions to		
	Perm Empl	Out of Empl	Out of empl w/UB
	Coef./SE	Coef./SE	Coef./SE
Constant	–1.870***	8.187***	–.616
	(.610)	(.637)	(1.117)
	Year and Country Dummies Included		
Observations	17987		

*Significance levels:** 10% ** 5% *** 1%

ECHP data, 1995-2001, all employees aged 16-65

Table B.2: Transition Probabilities for Permanent Employees: Coefficient Estimates from a Multinomial Logit

	Transitions to		
	Temp Empl	Out of Empl	Out of Empl w/ UB
	Coef./SE	Coef./SE	Coef./SE
Age	–.114***	–.430***	.018
	(.008)	(.006)	(.017)
Age2	.001***	.005***	–.000
	(.000)	(.000)	(.000)
Child	–.076***	.027	.009
	(.025)	(.022)	(.052)
Married	–.219***	–.267***	–.325***
	(.027)	(.024)	(.054)
Female	.041	.247***	.078
	(.027)	(.023)	(.056)
BadHealth	.064	.659***	.136
	(.079)	(.049)	(.147)
Lnincome	–.816***	–.778***	–1.013***
	(.034)	(.029)	(.058)
Lnhrs	.170**	–.066	.734***
	(.079)	(.064)	(.158)
HigherEdu	.051	.379***	–.205**
	(.039)	(.032)	(.080)
SecondEdu	–.181***	.094***	–.140**
	(.031)	(.026)	(.059)
Supervisor	–.224***	–.148***	–.356***
	(.050)	(.040)	(.100)
Intermediate	–.094***	–.161***	–.283***
	(.036)	(.030)	(.074)
Professional	–.107***	.175***	–.458***
	(.039)	(.033)	(.081)
Serviceworker	–.294***	.043	–.237***
	(.034)	(.029)	(.066)
PubliSector	–.372***	–.348***	–1.045***
	(.033)	(.027)	(.077)
FullTime	–.051	–.172***	–.149

Continued next page...

…table B.2 continued

	Transitions to		
	Temp Empl Coef./SE	Out of Empl Coef./SE	Out of Empl w/ UB Coef./SE
	(.068)	(.053)	(.133)
Agriculture	.250***	−.095	−.390**
	(.073)	(.074)	(.176)
Industry	−.035	−.053**	−.016
	(.030)	(.026)	(.058)
EPL-Perm	−.356***	−.223***	−.761***
	(.045)	(.041)	(.104)
EPL-Temp	.092***	.017	.215***
	(.028)	(.022)	(.064)
Constant	6.931***	12.021***	2.412***
	(.374)	(.311)	(.753)
	Year and Country Dummies Included		
Observations	155860		

Significance levels: * 10% ** 5% *** 1%

ECHP data, 1995-2001, all employees aged 16-65