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The Consequences of Employment Protection Legislation for the Youth Labour Market

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Editorial Note:

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Abstract

Understanding the causes of unemployment and job insecurity among young people remains a central concern for social scientists and policy makers. This study focuses on one potential institutional cause of high youth unemployment, employment protection legislation (EPL). While many are sceptical of a link between EPL and high aggregate unemployment rates, a consensus has emerged linking EPL to high youth unemployment in particular. The review of theoretical and empirical research conducted here challenges this consensus. Search and matching theoretic explanations have difficulty making unambiguous predictions about the effects of EPL on youth unemployment and empirical research has undertaken little effort to uncover its causal effects. The empirical analysis tests for the existence of aggregate employment effects of EPL across youth labour markets using aggregate data from affluent OECD countries (1985–2007) and individual data from labour force surveys for 15 Western European countries and the U.S. (1992–2007). It conducts conventional regression analyses and also implements a Difference-in-Difference design. Neither conventional nor Difference-in-Difference analyses yield any robust evidence whatsoever linking either dimension of EPL to inferior youth labour market performance, for any of the education groups or institutional conditions tested. Altogether, this study rejects the view that strict EPL is or has been the cause of high youth unemployment rates or low youth employment rates, at least for the sample of countries tested here.

Contents

Introduction[1]

Young people suffer disproportionately from unemployment and job insecurity across advanced Western countries (Blanchflower and Freeman 2000; Blossfeld et al. 2005). Compared to the United States, some European countries have experienced dramatic increases of youth unemployment over the past decades, in particular those bordering the Mediterranean (Figure 1). Youth unemployment recovered after reaching a maximum in the early 1990s, but returned to similar heights in the course of the financial and economic crisis of 2008–9. In times of declining and in times of rising youth unemployment, we also observe rapid growth of temporary employment that is, with the exception of Spain, concentrated among young people.

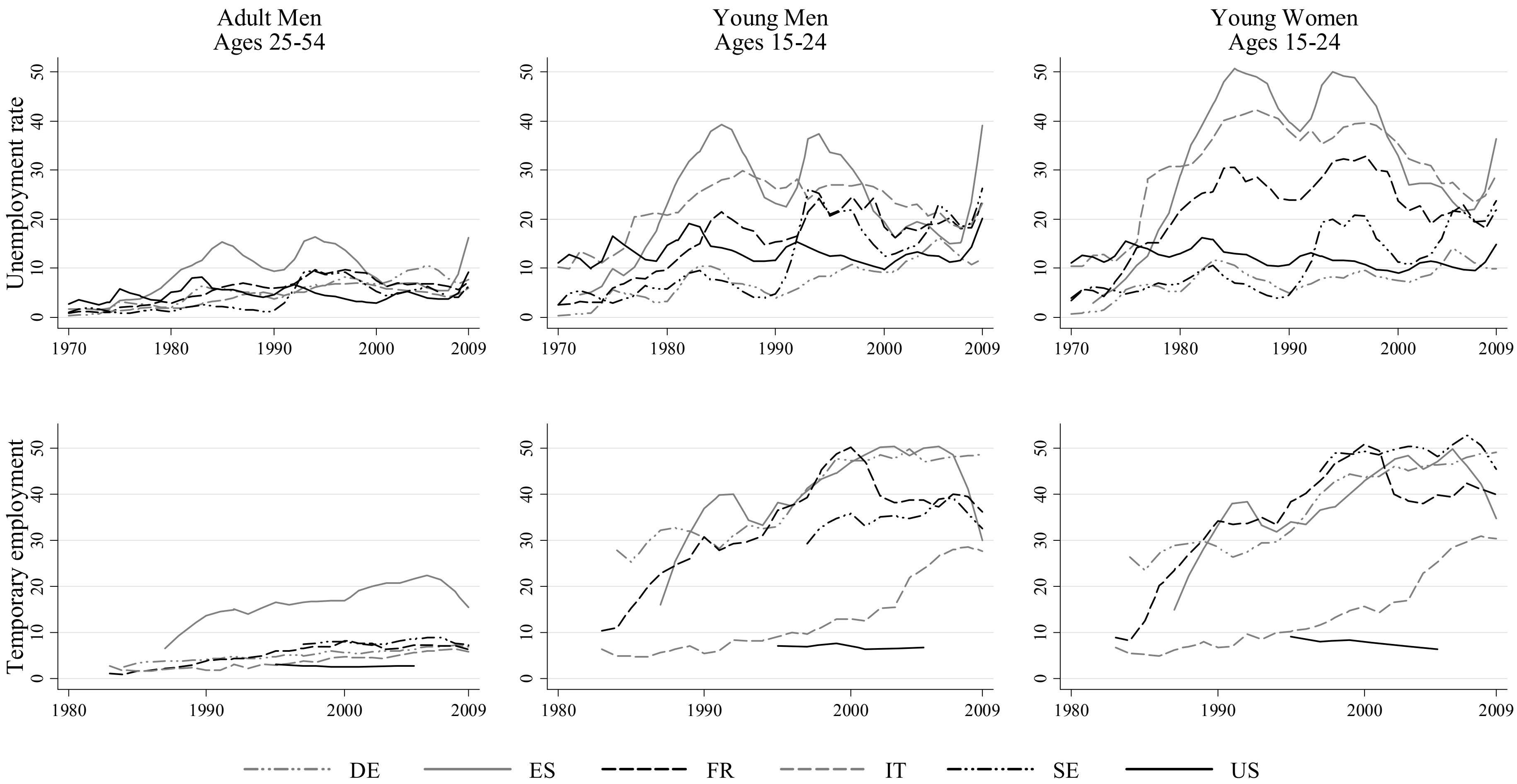
Among the institutional factors causing high job insecurity in particular among young people, employment protection legislation (EPL) is frequently mentioned. EPL restricts the extent to which employers can resort to workforce reductions in order to adjust to market changes and forces them to resort to alternative strategies of adjustment. EPL includes job security provisions that make dismissals of permanent employees costly as well as restriction on the usage of temporary employments. Many have argued that these regulations impose costs on society. By regulating employers' hiring and firing choices, they violate the fundamental principle of market flexibility, which economic theory has intricately linked to healthy economic performance and which has shaped the debate over labour market reform in recent decades.

Early analyses pointed to an ambiguous relationship between employment protection and unemployment (Lazear 1990; Bertola 1990). In an intellectual climate influenced by scepticism towards market regulations and a pressing need for solutions to persistent labour market problems, social scientists and public commentary converged on a consensus that employment protection is in part to blame, in particular because it diminishes the labour market chances of young people (OECD 1994; Scarpetta 1996; Siebert 1997). Reviews by international organizations (OECD 1994, 2004, 2006; European Commission 2006; World Bank 2007) have pointed to a causal link between EPL and high youth unemployment, and many studies and reviews in the fields of economics and sociology have provided support for this view (Esping-Andersen 2000; Heckman et al. 2000; Bertola et al. 2002, Kahn 2007; Addison and Texeira 2003; Botero et al. 2004; Breen 2005; Allard and Lindert 2006; Autor et al. 2006; Cahuc and Zylberberg 2004; Kahn 2007).

This study challenges the consensus view. The existence of a consensus on the adverse effects of EPL is in itself surprising, given theoretical ambiguity about its effects and given that very little is

[1] Earlier versions have been presented at the 2010 EQUALSOC Final Conference in Amsterdam, the 2010 World Congress of Sociology in Gothenburg, at the Center of European Studies and the Economic Sociology Colloquium at Harvard University, and at the Mannheim Centre of European Social Research. I am grateful for comments received on earlier drafts from Jason Beckfield and Markus Gangl, as well as to the two anonymous reviewers of the MZES working paper series. I gratefully acknowledge financial support from the Volkswagen Foundation and by the Mannheim Centre for European Social Research.

Figure 1 Unemployment (% unemployed in labour force) and temporary employment (% temporary employed in labour force), by age and gender



Source: OECD Labour Force Statistics. For temporary employment, values for Germany (2002, 2003, and 2004) and France (2002) calculated from augmented EULFS data.

known empirically about the causal effects of EPL. The study focuses on the effects of two dimensions of EPL, job security provisions and regulations on the usage of temporary employment, as well as their interaction with each other. The review of theoretical research indicates that both dimensions of EPL have mutually offsetting effects on youth unemployment rates, making unambiguous predictions about their effect difficult. The review of empirical research indicates that the case for deregulation is much weaker than proponents of reform argue.

The empirical analysis tests for the existence of aggregate employment effects of EPL, focusing on youth unemployment and other aggregate outcomes. Extant research based on individual data has difficulty answering this question because of the likely existence of general equilibrium effects, which do not permit inferences from micro to macro level effects. To assess aggregate employment effects, the analysis relies on aggregate data from affluent OECD countries (1985–2007) and labour force survey data for 16 Western countries (1992–2007). It relies on regression analyses similar to those supporting the consensus view, but also tries to improve on past empirical work by taking more seriously the problem of causal inference in the context of quantitative comparative research (Gangl 2010; Morgan and Winship 2007). To this end, a Differences-in-Differences design is implemented, and we also test within this framework for heterogeneous effects of EPL reforms across institutional contexts and education groups.

Employment protection legislation and youth unemployment

Job security provisions impose costs on employers when dismissing workers on permanent employment contracts (for an overview, see OECD 2004). These time-unlimited employment contracts still constitute the most abundant form of employment, especially among adult workers, across advanced OECD countries. Job security provisions include mandatory severance payments as well as procedural barriers, like advance notice periods, notification of external bodies and the involvement of courts if dismissals are legally contested. In contrast to permanent contracts, temporary contracts have a set date of expiration. They end automatically, often after 12 months or less, without imposing further obligations on employers. If dismissal of permanent employees is costly, temporary contracts become attractive as instruments for short-run adjustment of staffing levels.

Stricter job security provisions and stricter regulations on temporary contracts both affect aggregate unemployment rates through similar mechanisms (Bertola 1999; Blanchard and Landier 2002; Cahuc and Zylberberg 2004). By lowering rates of dismissal or job loss, they lower the exit rate from employment to unemployment. By lowering hiring rates, they lower the exit rate from unemployment to employment. Because these mechanisms have opposing effects on the aggregate unemployment rate, their total effect this outcome variable is ambiguous. Nevertheless, many have argued that by lowering turnover, EPL constrains employment opportunities for young people, leading to a prolonged absorption of first job seekers into first employment and therefore higher youth unemployment.

In the following, we will describe the underlying theoretical arguments in more detail, focusing on youth unemployment as the dependent variable, although we will also consider other outcomes in the empirical analysis. We emphasize the fundamentally ambiguous effects of both types of regulations. Because EPL diminishes both rates of transition into and rates of transition out of first employment, it is difficult to arrive at clear-cut predictions about their effect on youth unemployment rates. Moreover, extant research has mainly focused on the effects of EPL on turnover, but ignored other mechanisms through which it may impact youth labour market outcomes. Most importantly, by prolonging first job spells, EPL increases incentives for human capital formation, which should improve young people's labour market chances. Theoretical research has paid relatively little attention to determinants of EPL, while empirical research as often ignored potential confounders. Most importantly, working class representatives (trade unions, left parties) influence both the strictness of EPL and the distribution of unemployment risks. Insufficient controls for the power of organized labour may therefore lead to spurious associations between EPL and youth unemployment.

Job security provisions and the youth labour market

Job security provisions lower the threshold until which the productivity of an employer-employee match may decline for the employee to be dismissed (Bertola 1999; Cahuc and Zylberberg 2004). The cause of dismissals is usually modelled as a temporary or permanent decline of worker (or employer-employee match) productivity, which force employers to trade off the loss resulting from such shocks against the costs resulting from dismissals. Productivity shocks may result from worker behaviour or external factors, such as a decline in demand on product markets, and cannot be fully foreseen by employers. Employees are dismissed, if the costs due to shocks exceed the costs of dismissals. By making dismissals costly, dismissal costs therefore lower dismissal rates.

At the same time, job security provisions create hiring risks. When hiring new workers, employers have to anticipate paying dismissal costs with a certain probability, for example if a new worker turns out less productive than initially expected. Dismissal costs therefore lower the expected utility of hiring an additional worker; and by lowering the utility of hiring, dismissal costs also lower hiring rates. Dismissal costs should therefore reduce (job/labour) turnover and lengthen employment and unemployment spells. However, because they diminish rates of exit into and rates of exit out of unemployment, the impact of job security provisions on the aggregate unemployment rate is ambiguous and can only be resolved empirically (Lazear 1990; Blanchard and Katz 1997; Bertola 1999; OECD 2004).[2]

In spite of this fundamental ambiguity, many have argued that job security provisions structure the distribution of unemployment risks and in particular shift unemployment risks towards young people (Lazear 1990; OECD 1994, 2004, 2006; Bertola 1999; Esping-Andersen 2000; Heckman et al. 2000; Allard and Lindert 2006; Cahuc and Zylberberg 2004; Breen 2005; Autor et al. 2006; Kahn 2007). Job security provisions reduce the number of vacancies that appear at any given time and thereby reduce potential employment opportunities for the unemployed. Unemployment is therefore concentrated

2 Some authors still adhere to versions of the early critique of EPL (OECD 1994; Siebert 1997) and blame it for poor aggregate labour market outcomes (Scharpf 2000; Kenworthy 2004; Pontusson 2005).

among certain demographic groups, who are more at risk because of certain life course transitions they undergo, like leaving school and entering the labour market. If job security provisions reduce the occurrence of vacancies and thereby restrict employment opportunities, each cohort of young people leaving school and arriving on the labour market takes longer to make the transition into first employment. Stricter job security provisions should therefore increase youth unemployment rates.[3]

However, job security provisions also exert countervailing forces on youth unemployment. While dismissal costs prolong the transition to first employment, they also increase the duration of first employment spells (see Wolbers 2007, for empirical evidence). The first effect increases and the second effect lowers youth unemployment rates. Moreover, longer first employment spells also create incentives for human capital formation, which should further diminish unemployment risks among young people (see below).

Given their opposing effects on rates of transitions into and out of first employment, it is difficult to resolve the effect of job security provisions (and also regulations on temporary contracts, see below) using search and matching theory. Model predictions can be dependent on quite subtle assumptions, for example about functional form of model parameters (Bertola 1999; Ljungquist 2002). Moreover, credible identification and estimation of full structural models remains challenging (Chetty 2009). The empirical analysis therefore carries a strong burden given this theoretical indeterminacy. Analysis of aggregate employment outcomes can provide an answer about how these underlying mechanisms add up to impact youth unemployment rates, an outcome many social scientists and policy makers are concerned about, without the need of fully specifying and identifying the parameters that describe the underlying processes.

Deregulating temporary employment contracts

In the absence of dismissal costs on permanent contracts, permanent employees can be dismissed instantaneously and employers would not benefit from using temporary contracts. If dismissal of permanent employees is costly (due to job security provisions or other factors), temporary contracts become an alternative employment type that does not impose dismissal costs and provides flexibility to adjust the size of the workforce. The usage of temporary contracts has traditionally been restricted,[4] but in many countries these regulations have been gradually lifted.

What are the consequences of deregulating temporary contracts? On the one hand, hiring rates should increase, as dismissal costs (hiring risks) are effectively lowered. On the other hand, temporary jobs end automatically after a relatively short period, which shortens employment spells and increases

[3] Some authors focus on relative youth unemployment, for example the ratio or difference of youth and adult unemployment rates, as dependent variables (Bertola et al. 2002; Breen 2005). Analyzing ratios leaves open whether it is adult or youth unemployment rates (or both) which are affected by EPL, which is, however, of interest substantively and for policy purposes. We focus the theoretical discussion on the effect of EPL on (absolute) youth unemployment rates.

[4] Traditionally, temporary contracts were often only allowed for inherently temporary work, for example seasonal work in agriculture or apprenticeship contracts. Furthermore, the maximum cumulative duration and the maximum number of individual temporary contracts an individual may hold with the same employer are regulated.

job loss rates. More jobs begin and end at any given moment, and more transitions between employment and unemployment occur. Just like a reduction in job security provisions, deregulation should affect labour market flows, but leave aggregate employment or unemployment rates unchanged (Blanchard and Landier 2002; Kahn 2010).

Nevertheless, two views have emerged, stressing either the virtuous ('integration') or adverse ('entrapment') consequences of deregulating temporary contracts (see also Giesecke and Groß 2003). Following the 'integration scenario', deregulating temporary contracts may undo the negative effects of job security provisions (assuming they exist) on young people. By increasing turnover in entry level positions, temporary jobs create more vacancies for young people, speed up the transition into first employment, and therefore lower youth unemployment (OECD 1994). An alternative effect of deregulation may be a stimulation of labour demand by reducing temporary workers wages. Empirical studies consistently find considerable wage penalties for temporary compared to permanent employees (for example Booth et al. 2002; Mertens et al. 2007), especially among young temporary employees (Gebel 2009). If young people in entry-level temporary jobs dread job loss and unemployment, they may accept (or be unable to resist) being underpaid for their work in exchange for the opportunity of having their temporary jobs converted into permanent ones (Polavieja 2003). Deregulation thus provides not only numerical but also wage flexibility, and if not the former, it may be the latter (probably unintended) effect that is actually employment-enhancing.

Critics, however, argue that deregulating temporary contracts simply induces employers to destroy permanent jobs and replace them with temporary ones (Blanchard and Landier 2002; Cahuc and Postel-Vinay 2002; Kahn 2010).[5] This 'entrapment scenario' should prevail if dismissal of permanent employees is very costly, for example if permanent contracts are subject to strict job security provisions. If temporary contracts are deregulated while dismissal of permanent employees remains costly ('partial deregulation', Blanchard and Landier 2002), employers substitute permanent with temporary jobs and now have a lowered incentive to convert temporary into permanent jobs. Contract conversion is costly, because it increases employee bargaining power, which can be used to extract higher wages after conversion. To offset this increase in bargaining power, employers raise the productivity threshold for temporary jobs to be converted into permanent ones. In consequence, employers' destroy even relatively productive matches to be able to try their luck with a new employee.

As temporary jobs are not renewed or converted into permanent jobs, young people cycle through repeated spells of temporary employment and unemployment with lowered chances of obtaining a permanent job. Turnover in entry-level jobs may even increase excessively, resulting in higher, not lower youth unemployment (Blanchard and Landier 2002; Cahuc and Postel-Vinay 2002). The positive effect

[5] This substitution should lower young workers' welfare. Compared to permanent workers, temporary employees report lower job satisfaction (Booth et al. 2002), experience higher job insecurity (Giesecke and Groß 2003; DiPrete et al. 2006) and suffer from wage penalties (Booth et al. 2002; Gebel 2009). Kalleberg et al. (2000) show for the United States that temporary work is associated with considerably inferior job quality along different dimensions.

on unemployment is subtle, however, reflecting the ambiguous effects of both types of regulations on the exit rate from and the entry rate into unemployment.[6]

We are faced with two competing scenarios about the effect of deregulating temporary contracts (Giesecke and Groß 2003), which again do not permit a straightforward prediction regarding the impact of deregulation on youth unemployment (see also Gebel and Giesecke, forthcoming). Empirical analysis will have to provide clues whether either of these scenarios dominates. However, the preceding section suggests that we should particularly look for interactive effects of deregulation. In the 'integration scenario', deregulating temporary contracts undoes whatever problems are created by job security provisions on permanent jobs. The unemployment-reducing effect of deregulation should therefore be stronger if job security provisions are particularly strict. The 'entrapment scenario' predicts the opposite, i.e. if job security provisions are particularly strict, deregulation of temporary contracts might even increase youth unemployment. Unlike prior research that has aggregated both dimensions of EPL (e.g. Botero et al. 2004; OECD 2004), it is important to keep them separate and model them interactively (see also Gebel and Giesecke, forthcoming).

Employment protection legislation and on-the-job training

Extant research has largely ignored potential benefits of EPL. However, some have pointed to its potential role in human capital formation (Acemoglu and Pischke 1998; Estevez-Abe et al. 2001; Fella 2005). By increasing the duration of employment spells, both young people and employers have greater incentives to invest into general and specific skills. Because human capital investments increase productivity of job matches, employers are compensated for potential costs resulting from dismissal. As young peoples' skills improve, their unemployment risks diminish. From this perspective, deregulation (lower job security provisions, less restriction on temporary contracts) destroys incentives for skill formation and damages young people labour market career prospects.[7]

Class struggle versus employment law

Theoretical research has paid relatively little attention to the determinants of EPL, which is reflected by lack of concern for the endogeneity of EPL in empirical studies (see below). In Western Europe, job security provisions were extended between the 1960s, when tight labour markets increased worker bargaining power, and the 1970s, when still strong labour tried to shield workers from the growing risk of unemployment. Left parties and trade unions have played an important role in the extension and

[6] In the Blanchard-Landier model, the effect of partial deregulation is non-linear in the distribution of threshold productivities (above which employers keep temporary workers in permanent jobs), such that in some parts of the distribution, partial deregulation increases unemployment, while it decreases unemployment in other parts (Blanchard and Landier 2002: 226-7). In the Cahuc-Postel-Vinay model, firing costs on permanent contracts lower unemployment, but this effect diminishes if the labour market share of temporary contracts increases. Only at large values for firing costs does the spread of temporary contracts actually have a (small) positive effects on the unemployment rate (Cahuc and Postel-Vinay 2002: 83).

[7] Different studies find that compared to permanent employees, temporary employees are indeed less likely to receive training (Arulampalam and Booth 1998; Booth et al. 2002; Dolado et al. 2002; Arulampalam et al. 2004) and also point to inefficiently low levels of training provision on the deregulated British or American youth labour markets (Layard et al. 1994).

maintenance of job security provisions (Berger and Piore 1980; Allard and Lindert 2006; Botero et al. 2004; Rueda 2005). Working class representatives may also have played a central role in the deregulation of temporary employment. If their core constituents, which we assume to be mainly adult wage workers, demand job security in times of high unemployment, extending job security provisions or deregulating temporary contracts represent two alternative strategies to accomplish this goal: Because it is easier for employers to dismiss (or not renew the contracts of) temporary employed, cyclical employment adjustment disproportionately affects those on temporary contracts and permanent employees become even more protected from job loss (Bentolila and Dolado 1994; Polavieja 2003). Therefore, if working class representatives (trade unions, left parties) are influential determinants of both EPL and the distribution of labour market outcomes (see Bertola et al. 2007, for evidence on the latter), the effect of EPL on youth labour market outcomes may be spurious.

Summary and hypotheses

Even though EPL exerts countervailing effects on aggregate unemployment rates, a consensus has emerged linking strict job security provisions and regulations on temporary contracts to high youth unemployment. The goal of the subsequent analysis is to test this proposition. We will test whether deregulation on either dimension lowers youth unemployment. Furthermore, we will test whether the unemployment-reducing effect of deregulation on one dimension is stronger, if regulations on the other dimension are particularly strict. And we will test, whether these effects vary across education groups.

Our theoretical discussion, however, has also suggested that deregulation may make young people worse off. First, deregulation should lower training incentives and productivity of young people, which should increase their unemployment risks. Second, deregulating temporary contracts in the presence of strict job security provisions may generate excess turnover in entry-level positions and increase youth unemployment. Whatever beneficial effect the consensus view expects from deregulation may be offset by these countervailing mechanisms, and may even increase youth unemployment. Given the multiple countervailing forces at work, we should also not be surprised if they cancel each other out and yield small and statistically insignificant effects.

The power of organized labour is an important confounder that could be related to both high youth unemployment and strict EPL. If there is an effect of EPL on youth labour market outcomes, this effect should weaken or disappear once measures of the power of organized labour are accounted for.

Review of empirical studies

While one may be sceptical about the existence of a clear relationship between EPL and youth labour market outcomes, extant research often comes to a different conclusion: “[T]here is ample evidence that stringent EPL tends to worsen the employment prospects of those groups that are most subject to problems of entry in the labour market, such as young people...” (European Commission 2006: 83). The European Commission’s Employment in Europe Report arrives at a similar assessment as other international organizations (OECD 1994, 2004, 2006; World Bank 2007).

What is this “ample evidence”? OECD (2006) quotes Bertola et al. (2002), Jimeno and Rodriguez-Palenzuela (2002) and OECD (2004) in support of the consensus view. The Employment in Europe Report cites OECD (2004) and OECD (2006). Turning to these sources, however, we find partial support, at best. For example, using cross-national aggregate panel data, the OECD (2004) finds that the negative effect of EPL on youth employment rates is washed away entirely once country fixed effects (FE) are controlled for. Since one should approach results based on cross-sectional (cross-country) inference with healthy scepticism, this result should alert us to endogeneity biases driving the results.

Indeed, the strongest and frequently cited support of the consensus view comes from studies that are most vulnerable to endogeneity problems, namely those using cross-sectional country comparisons and (mostly) aggregate data, including Scarpetta (1996), Esping-Andersen (2000), Botero et al. (2004), OECD (2004), Breen (2005), and Kahn (2007). Disconfirming evidence from the cross-section also exists (OECD 1999; Neumark and Wascher 2004), but is frequently disregarded. Some studies also support the consensus view using aggregate country panel data and fixed effects (FE) estimation (Heckman et al. 2000; Bassanini and Duval 2006), but other studies using FE methods find no support (Lazear 1990; OECD 1999; Kahn 2010).

Few of the preceding studies address explicitly or try to remedy potential endogeneities of employment protection legislation and its reforms. Moreover, few studies address the problem of serial correlation in cases where units of observation (usually countries) are observed repeatedly (Bertrand et al. 2004). The data used in these analyses often involve variables that change only slowly over time (especially EPL itself), resulting in serial correlation of residuals that can strongly inflate t-statistics. We should therefore be concerned that significant effects of EPL in published research are just due to residual correlation that is not or improperly accounted for. Explicitly addressing these issues, Autor et al. (2006) provide evidence of negative effects of job security provisions on youth employment rates in the United States, but these effects are theoretically unexpected given the type of reform analyzed and dissipate over time.

Since it is difficult to generate unambiguous predictions from theory, the empirical analysis carries almost all of the weight in the assessment of the effects of EPL on aggregate youth labour market outcomes. A careful reading of extant research must raise serious doubts about the robustness of the empirical link between employment protection legislation and youth labour market outcomes. And it should also raise concerns about a rather selective reading of the evidence practiced by some. Bivariate or cross-sectional correlations are most consistently supportive of the consensus view, but certainly insufficient to either indicate a causal relationship or motivate reforms.

Empirical design, data and methods

The empirical analysis uses aggregate and micro-data from advanced OECD countries and adopts three different approaches to provide a thorough test of the effects of EPL on youth labour market outcomes. We begin with analyses that are similar in design to the studies cited above. First, we use cross-sectional aggregate data and OLS regression analysis to assess whether an association between EPL and youth unemployment is present in the cross-section, and whether it is sensitive to controlling for indicators measuring the power of organized labour. Second, we perform more extensive specification checks using aggregate panel data to assess how robust the correlations we find in the cross-section are.

To improve upon these conventional designs and take seriously the problem of causal inference (Gangl 2010; Morgan and Winship 2007), we propose a Differences-in-Differences approach (DD). We use adults' outcomes to non-parametrically adjust for time-varying unobserved confounders and restrict the analysis to countries that actually reformed, by exploiting variation in the direction and extent of reforms. Intuitively, we test in this framework whether the evolution of aggregate outcomes changes around the implementation of a reform in reform countries, and whether size and direction of this change are associated with the size and direction of change of EPL (measured by a continuous indicator). The DD analysis will also assess whether the impact of EPL reforms differs across education groups and institutional contexts.

A clear advantage of using aggregate data is the potential existence of general equilibrium effects that lead to violation of the stable unit treatment value assumption (SUTVA). For example, deregulation of temporary employment may induce employers to substitute permanent with temporary jobs, may alter how young people compete for jobs, and may affect macroeconomic variables such as labour demand (Blanchard and Landier 2002; Cahuc and Postel-Vinay 2002). Because of such macro-level effects, all young people entering the labour market, not just those who obtain a temporary job, are affected by deregulation. Consequently, the effect of holding a temporary employment contract on subsequent employment outcomes estimated in studies using individual data (for example Giesecke and Groß 2003; DiPrete et al. 2006) is no longer informative about the aggregate impact of temporary contracts (Hagen 2003). In contrast, our aggregate level estimates already reflect the presence of general equilibrium effects that are part of the causal effect of EPL reforms.

Data

The analyses rely on aggregate panel data from 21 affluent OECD countries (1985–2007) as well as micro data from 15 Western European countries and the U.S. (1992–2007). This sample captures a period of substantial variation in youth labour market outcomes and substantial reform activity, containing all Western European EU member states (plus Norway and Switzerland, minus Luxembourg) where most EPL reforms observed in OECD countries occurred. Adding observations from different world regions or earlier time periods would increase our concerns about potential biases, as units of observation become increasingly less comparable.

We rely on two sources for labour market outcome data: OECD Labour Force Statistics data (21 countries, 1985–2007) as well as individual data mainly drawn from the European Union Labour Force Survey (EULFS). The EULFS provides cross-sectional individual data on various aspects of employment. It consists of nationally representative labour force surveys that are standardized and harmonized following standard international classification systems by EUROSTAT (2005). Augmenting the EULFS data with data from the Current Population Survey for the U.S. (IPUMS-CPS, King et al. 2010) and the German Microcensus, we obtain micro-data on 15 Western European countries (Austria, Belgium, Denmark, Germany, Spain, Finland, France, Greece, Ireland, Italy, Netherlands, Norway, Portugal, Sweden, and the United Kingdom) and the U.S. for the period from 1992 to 2007.

Table 1 displays the countries, country acronyms used and years covered, as well as the sample sizes. For a few countries, we lack data from the first half of the 1980s and early 1990s. Using EULFS data, we can analyze 14 reforms of job security provisions in Western European countries between 1994 and 2004. Using the OECD dataset, this number increases to 23. Table 1 charts the reforms as the positive/negative change on the corresponding index across years. The EULFS data are well suited for analyzing the consequences of reforming temporary contracts: We observe 14 changes in regulation in Western European countries between 1994 and 2006 (16 in the OECD dataset).

The key independent variables are indicators measuring job security provisions on permanent (regular) employment contracts and regulations on the usage of temporary contracts available on an annual basis for OECD countries from 1985 onwards (Venn 2009). Both indicators were published first by the OECD (1999) and periodically revised since then. They are based on individual items measuring different aspects of regulation, which are coded by national experts into numerical scores varying between 0 and 6. Experts consider employment law as well as judicial rulings that define the de-facto situation. The resulting scores are weighted and aggregated. The resulting index varies from 0 (min) to 6 (max) and represent the most comprehensive measure of EPL available (see Venn 2009, for further discussions).

The indicator for job security provisions is based on eight individual items: four items on procedural inconveniences employers are facing upon starting the dismissal process (notification procedures, delay involved before notice can start, length of notice period), the amount of severance pay, definitions of justified or unfair dismissal, length of trial period that is exempt from employment protection provisions, and two items on repercussions for employers in case of unfair dismissals (compensation, right to reinstatement). The items for length of advance notice period and severance pay further differentiate by length of tenure of employees to be dismissed (9 months, 4 years and 20 years). We also use an alternate measure of job security provisions gathered by Botero et al. (2004), averaging their indicators for “cost of firing workers” and “dismissal procedures”, which measures the legal situation over the period 1991–2000. The OECD indicator measuring restrictions on the usage of temporary contracts is based on three items: valid reasons for using a temporary (rather than a permanent) employment contract, maximum number of successive temporary contracts with the same employer, and the maximum cumulated duration of temporary contracts with the same employer.

Table 1 Reforms and data availability

Countries / Acronyms		Reforms of Job Security Provisions			Reforms of regulations on temporary contracts			Data availability		Sample Size
		t=0	Δ	Aver. RTC	t=0	Δ	Aver. JSP	OECD	Augmented EULFS	Augm. EULFS
Austria	AT	2003	-0.55	1.75				1994–2007	1995–2007	172,874
Australia	AU	1996	0.50	1.25				1985–2007		
Belgium	BE	2000	0.05	1.50	1997	-3.75	1.70	1985–2007	1992–2007	170,461
Canada	CA							1985–2007		
Germany	DE	1993	0.10	3.05	1997	-2.25	2.68	1991–2007	1996–2007	655,390
		2004	0.32	0.75	2004	-0.50	3.00			
Denmark	DK	1995	-0.05	2.25				1985–2007	1992–2007, missing: 2006	62,857
Spain	ES	1994	-1.11	2.50	1994	0.50	2.71	1985–2007	1992–2007	480,859
		1997	-0.16	2.60	2001	0.50	2.52			
		2003	-0.15	3.00						
Finland	FI	1991	-0.34	3.25				1985–2007	1995–2007	103,143
		1996	-0.14	3.25						
		2001	-0.14	3.25						
France	FR	1986	-0.17	3.60	1990	0.50	2.34	1985–2007	1993–2007	383,762
		2001	0.13	4.00						
Greece	GR	1990	-0.13	4.00	2003	0.25	2.33	1985–2007	1997–2007	363,762
		2003	0.08	4.25						
Ireland	IE				2003	0.75	1.60	1985–2007	1992–2007, missing: 1998	336,885
Italy	IT				1997	-1.25	1.77	1985–2007	1997–2007	644,265
					2001	-2.00	1.77			
Japan	JP							1985–2007		
Netherlands	NL	1999	-0.03	0.75	1999	-0.75	3.05	1985–2007	1996–2007	187,766
New Zealand	NZ	2000	0.35	1.50	2000	1.25	1.70	1989–2007		
Norway	NO				2006	0.25	2.25	1985–2007	1996–2007	61,182
Portugal	PT	1989	-0.17	2.25				1985–2007	1992–2007	164,861
		1991	-0.50	2.25						
		2004	-0.16	1.75	2004	-0.50	4.17			
Sweden	SE	1993	-0.04	2.49	1997	-0.92	2.86	1985–2007	1995–2007	186,500
Switzerland	CH	2000	0.17	0.15				1991–2007		
United Kingdom	UK				2002	0.25	1.12	1985–2007	1992–2007, missing: 1998	291,366
United States	US							1985–2007	1992–2007	520,336

Note: For reforms on each dimension of employment protection legislation, Table 1 shows the year in which reforms were implemented (year t=0), the change on the corresponding index (Δ), and the average value on the other (non-reform) dimension of EPL from year 0 to four years after implementation. RTC – Regulations on temporary contracts, JSP – Job security provisions.

Finally, some analyses use control variables. We use three indicators measuring the power of organized labour: trade union density (OECD Labour Force Statistics),[8] collective bargaining coverage[9] and an index of the cabinet power of left parties (Armingeon et al. 2009).[10] We use three indicators for potential determinants of both EPL and youth labour market outcomes: variation of the business cycle, measured by the output gap,[11] an index of economic globalization (Dreher 2006)[12] and an index of product market regulations (Conway and Nicoletti 2006; see Blanchard and Giavazzi 2003 regarding substantive motivation).[13] Finally, we use as an indicator for the size of the apprenticeship system, i.e. the percentage of students enrolled in upper and post-secondary (ISCED 3-4) programs that combine school-based vocational education with workplace-based training, which is only available from 1996 onwards.[14]

Differences-in-Differences estimation

In a standard Differences-in-Differences setup, we would compare changes in outcomes of countries experiencing EPL reform to changes in outcomes in countries not experiencing EPL reform. Non-reform countries are used to approximate and adjust for whatever changes in outcomes would have occurred for reform countries had they not experienced reform. Many of the quantitative comparative studies cited above follow this approach, which entails assuming in this analysis that, for example, the U.S. and Japanese youth labour markets trends are good counterfactuals for what would have happened on the Spanish and Italian youth labour market in the absence of reform. In cross-national research, we should generally expect such an assumption to be violated. Even in a relatively homogenous country sample, youth labour markets differ substantially across countries, because young people differ in their characteristics, and because institutional and economic environments differ considerably. And, these differences change in country-specific ways over time. Moreover, countries differ in their environments that determine the political feasibility as well as the implementation and effectiveness of reforms. Two main sources of bias result, first due to unobserved time-varying confounders, and second due to unobserved heterogeneity in the effect of reforms.

8 Ratio of wage and salary earners that are trade union members, divided by the total number of wage and salary earners, in percentages.

9 Employees covered by wage bargaining agreements as a proportion of all wage and salary earners in employment with the right to bargaining, in percentages, Visser (2009). Data from Ireland is fully missing, imputed a value of 70 for entire period (Ochel 2001). For Greece and Portugal, only 6 data points are available between 1985 and 2006. Missing data linearly interpolated. 2007 data missing for Austria, Spain, Greece, Japan, Portugal, Sweden and Norway, 2006 value imputed.

10 The index varies from 1 to 5, where 1 equals hegemony of right-wing (and centre) parties and 5 equals hegemony of social-democratic and other left parties. For Italy 1995 (missing), a value of 2.5 was imputed.

11 The output gap is measured as the difference between actual and potential gross domestic product (GDP) in per cent of potential GDP (OECD 2009).

12 The indicator is based on the following components: trade (percent of GDP), foreign direct investment (flows, percent of GDP), foreign direct investment (stocks, percent of GDP), portfolio investment (percent of GDP), income payments to foreign nationals (percent of GDP), hidden import barriers, mean tariff rate, taxes on international trade (percent of current revenue), and capital account restrictions.

13 Index for regulatory conditions in seven non-manufacturing sectors: airlines, telecommunication, electricity, gas, post, rail, and road freight, varies from 0 (min) to 6 (max).

14 Codes n (negligible amount) and a (not applicable) were assigned a value of 0. 1997 (missing) imputed as the average of adjacent years for each country. Missing data for Ireland 1998-2004, the Netherlands 1999 and 2001, and Norway 1998-2004 are imputed by linear interpolation. Source: OECD Education at a Glance (various issues).

To address bias resulting from time-varying unobserved heterogeneity, rather than using control observations from other countries to model counterfactual trends in outcomes, we use older cohorts within the same country (Kahn 2007) to (non-parametrically) control for annual country- and education group-specific labour market shocks. Changes on the adult labour market within the same country, assuming they are not affected by EPL reform, should better capture and control for changes on the youth labour market unrelated to reform than whatever information can be provided about these counterfactual trends from non-reform countries. We have to assume though that adult outcomes are not affected by EPL reforms,[15] which we can easily test empirically.

To address bias resulting from selection on the expected effectiveness of reforms, we restrict the analysis only to countries that undergo reform and exploit variation in the intensity and direction (upregulation/downregulation) of EPL reforms. Reform countries are likely to differ from non-reform countries in terms of (partly unobserved) factors which impact both youth labour market performance as well as the implementation of reforms. For example, reforms are probably enacted mainly in countries and time periods, in which they are politically feasible and policy makers actually expect reforms to have an impact. To the extent that the factors determining feasibility and expected effectiveness of reforms are unobserved, the effect of reforms may be easier to identify, if we restrict the sample to reform countries, which presumably are more similar in terms of these unobservables. Moreover, also for substantive reasons, it is most interesting to estimate the effect of reforms for countries that actually reformed.

Using the EULFS data, we define young labour market entrants corresponding to the level of educational attainment and typical graduation ages, so that the resulting groups are similar in terms of potential labour force experience. For ISCED 0-2 graduates, we focus on 15–24 year olds, for ISCED 3-4 graduates 20–29 year olds, and for ISCED 5-6 25–34 year olds. Setting age bands according to educational attainment is more plausible than imposing a common age range across education groups, which would be our only alternative given the scarce information in the data. We then select cohorts on average 20 years older to serve as control groups: For young people with an ISCED 0-2 degree, we select 35–44 year olds with the same degree. For young people with ISCED 3-4 degree, we select 40–49 year olds with the same degree; and for young ISCED 5-6 graduates, we select 45–54 year olds with the same degree. Our analysis sample only contains individuals belonging to any of these groups.

EPL reforms are not passed simultaneously across countries and few countries repeatedly implement reforms, which poses some difficulty in implementing a standard DD design. We follow Bertrand et al. (2004: 267 f.) to deal with this issue, and adapt their aggregation technique, which also addresses the problem of serial correlation. We proceed as follows. First, using adult labour market outcomes, we

[15] If older cohorts' outcomes are changed by EPL reforms, we risk controlling away part of the effect we are trying to estimate. Extant research suggests that the effect of reforms on adult unemployment rates is ambiguous in theory and empirical research indicates that it is non-existent or very small in practice (Bertola 1999; Esping-Andersen 2000; OECD 2004). Alternatively, we can think of this design as assessing the distributional effects of reforms, i.e. an analysis of effects on relative outcomes or the distribution of outcomes between young people and adults (for example Breen 2005; Kahn 2007; Bertola et al. 2007).

adjust youth labour market outcomes for annual country- and education group-specific shocks; these adjusted youth labour market outcomes become the dependent variable in the remaining analysis. Second, and optionally, we de-trend the adjusted youth labour market outcomes. Third, we only retain observations starting three years prior to four years after reform. Fourth, we calculate before- and after-reform average adjusted outcomes, take their difference and regress this difference on the difference in the EPL index associated with a particular reform. The following paragraphs explain this approach in more detail.

I. We estimate the following equation using OLS regression separately for each country (first-step regressions),

$$Y_{it} = \alpha_{1t}YOUTH_t + \beta_{1t}ISCED02_t + \beta_{2t}ISCED34_t + \beta_{3t}ISCED56_t + \varepsilon_{it} \quad (1)$$

Y_{it} is the binary outcome variable for individual i in year t. ISCED02, ISCED 34 and ISCED 56 are year-specific dummy variables indicating respondents' educational degree. The β coefficients estimate year- and education-group-specific unobserved shocks that affect individuals with the same educational degree but different ages equally. YOUTH is a dummy variable coded 1 for young people (aged 15–24 with ISCED 0-2 degree, 20–29 with ISCED 3-4 degree, and 25–34 with ISCED 5-6 degree) and zero for older cohorts (ages 35–44 with ISCED 0-2 degree, 40–49 with ISCED 3-4 degree, and 45–54 with ISCED 5-6 degree). The α_{1t} coefficients estimate the t average year-specific outcomes of young people net of annual education-group-specific shocks. We thereby adjust young people's labour market outcomes non-parametrically for country by year by education group specific shocks that are common across age groups. To obtain adjusted education group specific outcomes, we interact the YOUTH dummy variables with dummy variables for respondents' educational attainment.

II. We now have a country by year panel where units of observation are the α coefficients from the first step regressions. We can either continue with the analysis and follow rest of the procedure outlined in the following paragraphs, or further adjust the data. Inspection of the data revealed that some countries show persistent trends in adjusted youth labour market outcomes. These are removed by running an OLS regression on country fixed effects and country-specific linear trends. We thereby remove any country-specific forces that specifically impact youth labour market outcomes and can be captured by a linear trend variable, for example recovery from an economic crisis. We obtain the residuals from the corresponding regressions and use them as dependent variables in the subsequent analysis.

III. We only retain observations from countries experiencing reform and we further retain only observations starting three years prior (from t=-3 to t=-1) to four years after reform (from t=0 to t=+4, reforms are implemented at t=0).[16] We thereby force the effect of reforms to appear within the first five years following implementation (see Autor et al. 2006, for a similar approach). Prolonging the post-reform observation window raises the risk of spurious causal inference, as one becomes increasingly likely to

[16] For few reforms, we do not have full before-after information. Also, in two cases, we have to duplicate observations, since countries reform twice, or in one case, three times, causing post-reform observations of an earlier reform to overlap with pre-reform observations from a later reform.

attribute whatever unobserved shocks impact the outcome variable to prior changes in regulation. We vary the length of the post-reform observation period to assess variation in the impact of reform over time.

IV. We calculate the pre- and post-reform averages for the dependent variable (α coefficients). We now have a two period panel, one pre- and one post-reform observation per reform. We then estimate an OLS regression of the before-after difference in average adjusted youth labour market outcomes on the before-after difference in the respective EPL index (second step regression). Having non-parametrically controlled for unobserved country-specific time-varying shocks in the first step, in this step we also eliminate unobserved time constant (macroeconomic, institutional) factors that cause youth labour market outcomes to differ across countries (over the eight year period of observation).[17] We use the change on the index rather than separate variables for de- and up-regulation to increase efficiency, but have to assume that reforms can be mapped into movements on the unidimensional continuous indicator. To assess potential sensitivity of results to influential outliers, we use DFBETA statistics, added variable plots and also re-estimate each regression dropping one observation or country at a time.

Results

Do job security provisions make young people worse off?

Figure 2 displays the cross-sectional association between job security provisions and aggregate youth (ages 15–24) unemployment. We use four different indicators of job security provisions. In the upper left scatter plot, we use the aggregate OECD indicator, which we also use in the remainder of the analysis. Since hiring decisions may be particularly affected by the costs of dismissing a newly hired worker, we recalculate the OECD indicator so that it only refers to dismissal costs at 9 months tenure (for notification procedures and severance pay), and drop the items relating to dismissal after 4 or 20 years of tenure (lower left plot). For the lower right plot, we recalculate the job security provisions index such that it only comprises the direct monetary costs that accrue with certainty in the event of dismissing a recently hired worker with 9 months tenure (notification procedures, delay involved before notice can start, length of advance notice periods, and severance pay).[18] For the upper right plot, we use an index calculated from data provided by Botero et al. (2004).

There is no association between job security provisions and youth unemployment. This is counter to many studies that have found such an association in the cross-section (Esping-Andersen 2000;

[17] Importantly, with an eight year observation period we should be less concerned about the spurious regression problem plaguing time-series analyses. Moreover, we control for whatever is unobserved time-constant in the eight years around reform, which should eliminate bias more effectively, than if we were to control for whatever is time-constant over a 15 or 20 year observation period.

[18] When recalculating the aggregate indicator we adjust the weights so that the overall weighting scheme remains intact.

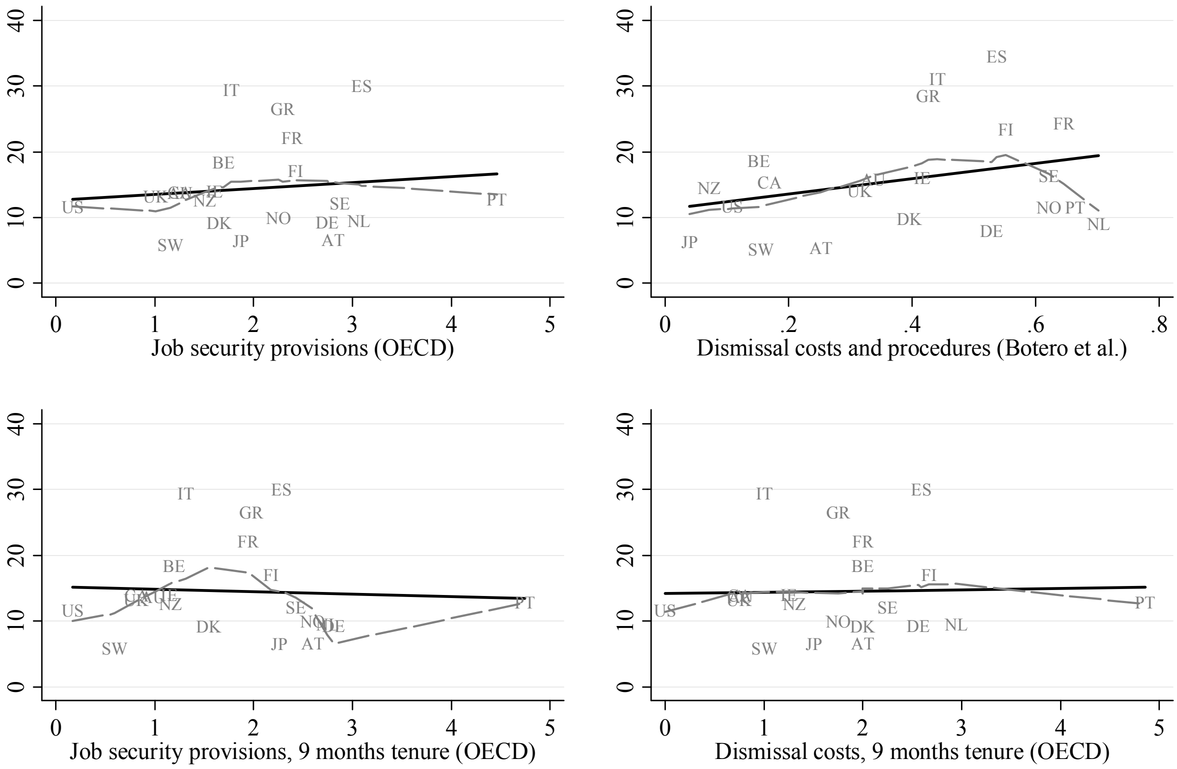
Heckman et al. 2000; OECD 1999, 2004; Botero et al. 2004; Breen 2005).[19] In Table 2, we explore these results further using aggregate data for 21 OECD countries (1996–2007 averages). Once we control for the size of the apprenticeship system, which may keep youth unemployment low in some countries with high job security provisions, the effect of job security provisions on youth unemployment strengthens a bit. However, controlling independently for indicators of labour power, it diminishes again and even changes sign. These results illustrate the endogeneity of job security provisions to the power of organized labour, in particular measured by collective bargaining coverage and left party power. Using youth employment rates (employment-to-population ratios) as the dependent variable, we observe very similar patterns (results available on request).

To further explore the robustness of the association between job security provisions and youth (ages 15–24) labour market outcomes, we report the results of pooled OLS and fixed effects regression using aggregate cross-national panel data for maximally 21 OECD countries (1985–2007). Such estimates have been the central piece of evidence documenting adverse effects of job security provisions. We conduct the analysis separately by gender and also separately analyze less educated young people, since the effects of job security provisions possibly differ across these groups (Esping-Andersen 2000). Moreover, we vary the sample in terms of time periods and countries covered, contrasting the full sample (21 OECD countries) to a restricted sample of EU member states. The latter group of countries is more similar in institutional and economic characteristics, but still countries differ consid erably in terms of EPL. Finally, we use different estimation techniques (pooled OLS, OLS with country fixed and year fixed effects, and OLS with country fixed, year fixed effects and country-specific linear trends). The dependent variable is measured in natural log percentages. Each regression is run twice, once including only the indicator, and once adding a carefully selected and flexibly specified set of control variables.[20]

[19] The published results of Botero et al. (2004) and Breen (2005) could both be replicated. However, Botero et al. (2004) use a very heterogeneous sample of 54 developed and developing countries. Constraining the sample to the 21 advanced countries analyzed here (i.e. comparing more comparable countries), the effect of employment regulations breaks down. In Breen's (2005) analysis, job security provisions only have a significant effect on youth unemployment in countries without apprenticeship systems. This result is driven by the Greek, Italian and South Korean observations. If we replace the OECD (1999) indicator used in the original analysis with the revised and updated indicator of job security provisions (Venn 2009) used in the remainder of the analysis, this conditional effect of job security provisions is no longer statistically significant. This is due to a downward correction of the indicator for the three countries driving the original relationship, in particular South Korea (-.3) and Italy (-1).

[20] Control variables were selected that are plausibly exogenous to the outcome variables and and/or important predictors of employment protection legislation. The following controls are used: collective bargaining coverage, trade union density, left party power in government, the output gap, economic globalization, and product market regulations. We include their main effects as well as three non-linear terms that significantly predict the job security provisions index in either a pooled OLS or a fixed effects regression analysis using the full sample of 21 OECD countries (1985-2007). The non-linear terms were determined using an algorithm (details available on request). The goal is to flexibly control for observed predictors of the EPL variable. The algorithm found 3 quadratic or multiplicative interaction terms that significantly (cluster robust t-statistic>=1.96) predict job security provisions: output gap * trade union density, collective bargaining coverage * left party power, and collective bargaining coverage squared.

Figure 2 Associations between different measures of job security provisions and average youth (ages 15–24) unemployment rates



Note: Average values for the period 1985–2007 (Botero indicator: 1991–2000), fitted OLS regression line, and non-parametric lowess smoother (dashed line).

Source: Dependent variables from OECD Labour Force Statistics, for other variables, see text.

Since fixed effects soak as much as 96% of the variation in job security provision in the sample of 21 countries (1985–2007), we worry about attenuation bias when using the FE estimator and therefore begin by considering evidence from pooled OLS analysis (upper half of Table 3). Confirming the pattern in Table 1, adjusting for control variables, all estimates have negative signs that in some cases are large and statistically significant. Fixed effects (FE) estimates are usually not significant. The trend-adjusted FE analyses mainly yield negative coefficient estimates, some of which reach statistical significance. Altogether, there is no evidence whatsoever that job security provisions increase youth unemployment, neither in the cross-sectional nor in the longitudinal analyses. Correlations of job security provisions with youth unemployment are vulnerable to the inclusion of relevant control variables, country fixed effects and country-specific linear trends.

Table 2 OLS regression estimates (standard errors) of job security provisions on youth (ages 15–24) unemployment rates (ln)

	(1)	(2)	(3)	(4)	(5)	(6)
Job security provisions	0.060 (0.118)	0.074 (0.098)	0.004 (0.103)	-0.089 (0.105)	0.070 (0.101)	-0.179 (0.107)
Size of apprenticeship system		-0.014*** (0.005)	-0.014*** (0.004)	-0.016*** (0.004)	-0.014*** (0.005)	-0.015*** (0.004)
Left party power			0.135 (0.080)			0.141* (0.071)
Collective bargaining coverage				0.009** (0.004)		0.011** (0.004)
Trade union density					0.002 (0.004)	-0.005 (0.004)
N	21	21	21	21	21	21
Adjusted R2	-0.04	0.28	0.35	0.46	0.25	0.53

Note: *** p<0.01, ** p<0.05, * p<0.1. All variables averaged across 1996–2007.

Source: Dependent variables from OECD Labour Force Statistics, for other variables, see text.

Table 3 OLS Regression estimates ("cluster robust" standard errors) of the association between job security provisions on permanent employment contracts and youth (ages 15–24) unemployment rates (ln), OECD and EULFS data

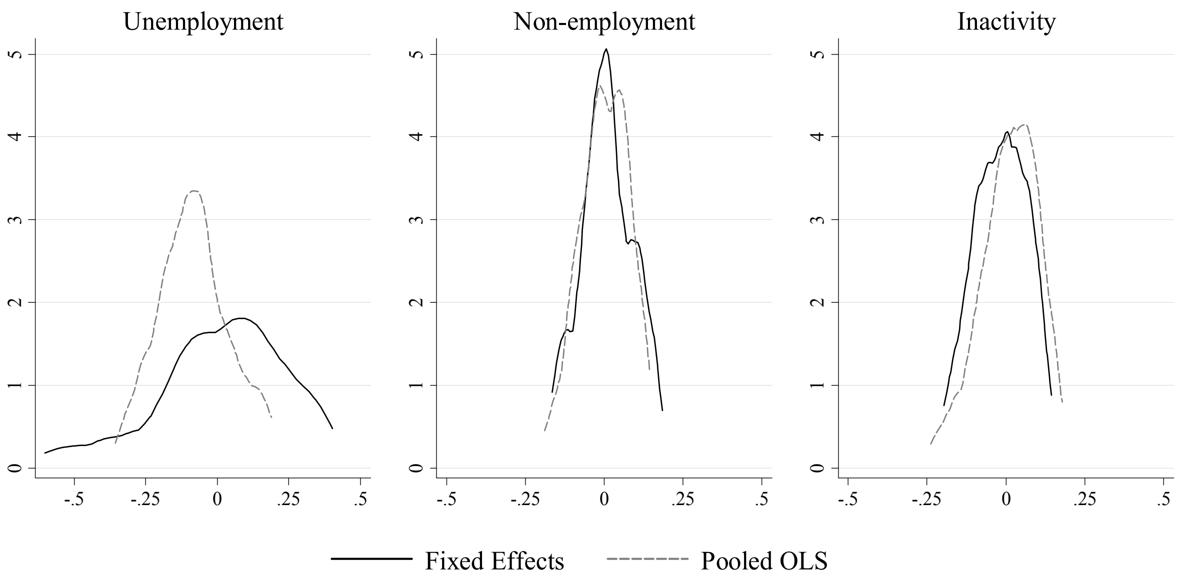
		Young men		Young women		ISCED 0-2	
		Bivariate	Control	Bivariate	Control	Bivariate	Control
OLS	Full Sample	-0.020 (0.073)	-0.116* (0.060)	0.122 (0.083)	-0.066 (0.083)	-0.124 (0.087)	-0.260*** (0.079)
	EU14	-0.108 (0.091)	-0.232*** (0.061)	0.009 (0.120)	-0.186* (0.095)	-0.191* (0.105)	-0.261** (0.104)
	1985–1992	-0.034 (0.092)	-0.133 (0.101)	0.114 (0.098)	-0.091 (0.093)		
	1993–2000	-0.025 (0.099)	-0.110 (0.101)	0.115 (0.106)	-0.065 (0.114)	-0.128 (0.103)	-0.308** (0.122)
	2001–2007	0.013 (0.068)	-0.048 (0.069)	0.141* (0.079)	-0.013 (0.108)	-0.126 (0.085)	-0.179** (0.078)
Fixed effects	Full Sample	0.057 (0.199)	0.224 (0.247)	-0.007 (0.219)	0.229 (0.249)	0.183 (0.231)	0.301 (0.365)
	EU14	0.064 (0.237)	0.244 (0.229)	-0.010 (0.255)	0.271 (0.275)	0.203 (0.229)	0.228 (0.357)
Fixed effects, linear trends	Full Sample	-0.100 (0.207)	0.040 (0.109)	-0.049 (0.223)	0.040 (0.111)	-0.237* (0.127)	-0.500*** (0.150)
	EU14	-0.122 (0.224)	-0.051 (0.139)	-0.037 (0.247)	-0.011 (0.147)	-0.205 (0.126)	-0.447** (0.163)

Note: *** p<0.01, ** p<0.05, * p<0.1. OLS – Ordinary least squares regression; Fixed effects – OLS with country fixed effects and year fixed effects; Fixed effects, linear trends – OLS with country fixed effects, year fixed effects and country-specific linear trends. Regarding control variables, see footnote 20. The control variable specifications additionally contain a dummy variable for the Spanish observations 1992 and 1993, which prove highly influential in the fixed effects analyses.

Sources: Data on dependent variables for young men and women from OECD Labour Force Statistics, 21 OECD countries, 1985–2007, full sample: N=450, EU14: N=330. Data on dependent variables for ISCED 0-2 graduates from augmented EULFS, full sample: N=231, EU14: N=215.

This conclusion is further reinforced by Figure 3, where we display the estimated probability distribution of the 52 regression coefficients shown in Table 2. We also graph the distributions of regression coefficients from identical analyses of two other outcomes: inactivity and non-employment (detailed results available on request).[21] For the latter outcomes, pooled OLS and fixed effects estimates cluster around zero. There is no systematic relationship, but high vulnerability to specification changes, which may sometimes generate a result that crosses the 5% significance level simply by chance. On average, however, the results indicate that job security provisions are not associated with inferior aggregate youth labour market outcomes.

Figure 3 Distribution of regression coefficients estimating the effect of job security provisions on aggregate youth labour market outcomes



Note: Kernel (epanechikov) density estimates of the distribution of OLS estimates of the effect of job security provisions on youth labour market outcomes; detailed results for unemployment in Table 2.

Finally, we evaluate the effects of reforms of job security provisions on youth unemployment using Differences-in-Differences (DD), and test whether the impact of reforms depends on the strictness of regulations on temporary contracts. We begin by analyzing the impact of reforms on adult unemployment rates to ensure that we do not control away the effect of reforms by differencing out adult outcomes in the first-step regressions.[22] The Spanish 1994 reform represents a very influential outlier.[23]

[21] Non-employment rates equal 100 minus the employment rate. Inactivity rates are defined as the ratio of the number of inactive (not employed or actively searching for employment) divided by the population.

[22] To analyze adult outcomes, we modify the DD procedure slightly. Omitting step I, we take adult (ages 35-44 with ISCED 0-2 degree, 40-49 with ISCED 3-4 degree, and 45-54 with ISCED 5-6 degree) unemployment rates as dependent variables. In step II, we regress adult unemployment rates on either (a) country-specific linear trends, or (b) country-specific cyclical shocks, i.e. interactions between country dummies and the output gap. Each regression also controls for country fixed effects. We obtain the residuals and then follow the rest of the procedure as outlined in steps III and IV.

[23] The average absolute value of reforms is 0.22, which the Spanish 1994 reform (change of 1.1) exceeds by 5 standard deviations.

Accounting separately for this observation, we observe no statistically significant effects of job security provisions on adult unemployment rates.

Tables 4 reports the results of the second step, first-difference regression of aggregate adjusted youth unemployment rates on the change in the job security provisions indicator (education group-specific results available on request). The results in column 1 again indicate a negative effect of job security provisions on youth unemployment. Once we control for cyclical variation (column 2), the effect turns positive. Like the dependent variable, control variables take the value of the difference of average before-reform (t=-3 to t=-1) and average after-reform (t=0 to t=4) values. Further adjusting for indicators of the power of organized labour again weakens the effect and triggers a sign change (column 4). If we de-trend the dependent variable, stricter regulations have a strong negative effect (column 6), which is halved and no longer statistically significant, if we account for the influential Spanish 1994 reform (column 7). Columns 8–10 indicate that if regulations on temporary contracts are strict,[24] tightening job security provisions would diminish youth unemployment.[25] With the exception of the outlier-driven result in column 6, none of the estimates of the effects of job security provisions is even remotely significant and we again observe sign changes and inconsistent results across specifications.

Analyzing the data separately by education groups, repeating the analysis with the larger OECD data-set, and repeating these analyses with youth employment rates (employment-to-population ratios) as the dependent variable provides no evidence of adverse effects of job security provisions (all results available on request). We also tested interactions of job security provisions with the indicators measuring the power of organized labour, but did not obtain statistically significant effects. Neither did we find evidence that job security provisions work differently in countries bordering the Mediterranean (Spain, France, Italy, Greece, plus Portugal). We found no robust evidence whatsoever supporting the view that job security provisions cause inferior youth labour market performance.

[24] We take the average value of regulations on temporary contracts in the five-year post-reform period (see Table 1). They are time-constant by construction, and hence their main effects are omitted from the analysis.

[25] If we shorten the post-observation window from 5 to 3 years, the interaction term in column 10 actually becomes significant (results available on request) and robust to outliers.

Table 4 First-Difference OLS regression estimates (OLS standard errors) of the effects of reforms of job security provisions on youth unemployment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Job security provisions (JSP)	-0.950	0.338	0.150	-0.084	0.305	-3.284**	-1.637	1.256	3.377	3.924
	(1.914)	(1.444)	(1.484)	(1.431)	(1.498)	(1.352)	(2.334)	(7.415)	(5.393)	(4.752)
Reg. temp. contracts * JSP								-0.999	-1.372	-3.265
								(3.235)	(2.339)	(2.073)
Output Gap		-0.963***	-0.894**	-0.761**	-0.965***				-0.971***	
		(0.282)	(0.299)	(0.312)	(0.293)				(0.292)	
Left party power			0.217							
			(0.264)							
Coll. bargaining coverage				-0.297						
				(0.222)						
Trade union density					-0.239					
					(0.481)					
Spain 1994							-2.396			
							(2.752)			
Constant	-1.438*	-0.740	-0.727	-0.727	-1.122	-0.274	-0.259	-1.502*	-0.821	-0.481
	(0.688)	(0.541)	(0.550)	(0.523)	(0.952)	(0.486)	(0.491)	(0.744)	(0.575)	(0.477)
Linear trends	No	No	No	No	No	Yes	Yes	No	No	Yes
N	14	14	14	14	14	14	14	14	14	14
Adjusted R^2	-0.06	0.44	0.42	0.47	0.40	0.27	0.26	-0.15	0.40	0.35

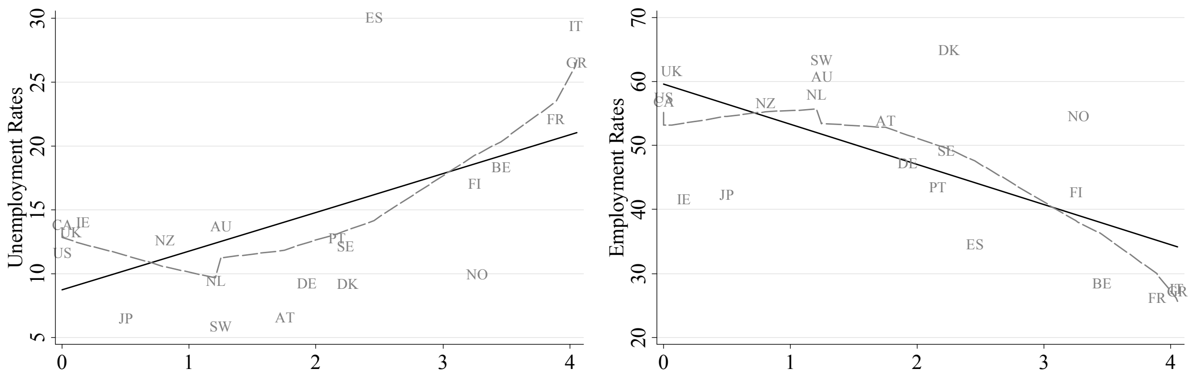
Note: *** p<0.01, ** p<0.05, * p<0.1. Result of OLS regressions of the before-after difference in average adjusted youth unemployment rates on the before-after difference in the respective EPL index (second step regression). For details on estimation, see pages 15 and 16, esp. step IV.

Source: European Union Labour Force Survey, German Microcensus, IPUMS-CPS; own calculations.

The consequences of deregulating temporary contracts

Figure 4 displays the association between the OECD indicator for regulations on the usage of temporary employment contracts and youth (ages 15–24) unemployment and employment rates. As in the case of job security provisions, Mediterranean countries and Finland display strict regulations and high youth unemployment, but we no longer observe countries with high regulations and low youth unemployment as we did in Figure 3.

Figure 4 Associations between Regulations on the usage of temporary contracts and average youth unemployment and employment rates



Note: Average values for the period 1985–2007, fitted OLS regression line, and non-parametric lowess smoother.
Source: Dependent variables from OECD Labour Force Statistics, for other variables, see text.

Table 5 reports results from multivariate regressions for 21 OECD countries (1996–2007 averages). The association between regulations on temporary contracts and (the natural log of) youth unemployment is strong, statistically significant, and robust to inclusion of control variables. A one-unit decrease on the indicator is associated with a more than 20% reduction of youth unemployment rates. Using youth employment rates as dependent variables, the effect of regulations is weaker and less robust to inclusion of control variables, but maintains statistical significance across the different specifications (results available on request).

How robust are these large effects from the cross-section? Table 6 repeats the specification checks we performed for job security provisions in the preceding section (see Table 3). We start again with the results from the cross-sectional pooled OLS analysis.[26] Interestingly using a few more and more flexibly specified control variables, the robustly significant effect of regulations that we found in Table 5 does frequently not survive. In particular, controlling for collective bargaining coverage and the extent of economic globalization causes the association between regulations on temporary contracts (male/female) youth unemployment to weaken considerably. If we control for fixed effects, *all* esti-

[26] Regulations on temporary contracts show considerably more variation over time than job security provisions. Controlling for country fixed effects eliminates “only” 83% percent of the variation on this indicator.

mates turn negative, some even statistically significant. The trend-adjusted models suggest that if anything tighter regulations lower youth unemployment, particularly in EU14 countries.

Table 5 OLS regression estimates (standard errors) of regulations on temporary contracts on youth (ages 15–24) unemployment rates (ln)

	(1)	(2)	(3)	(4)	(5)	(6)
Regulations on temporary contracts	0.215*** (0.065)	0.221*** (0.044)	0.204*** (0.047)	0.221*** (0.064)	0.224*** (0.047)	0.204*** (0.067)
Size of apprenticeship system		-0.014*** (0.003)	-0.014*** (0.003)	-0.014*** (0.003)	-0.014*** (0.003)	-0.014*** (0.003)
Left party power			0.055 (0.054)			0.062 (0.059)
Collective bargaining coverage				0.000 (0.003)		0.000 (0.003)
Trade union density					-0.001 (0.003)	-0.002 (0.003)
N	21	21	21	21	21	21
Adjusted R2	0.33	0.69	0.69	0.67	0.67	0.66

Note: *** p<0.01, ** p<0.05, * p<0.1. All variables averaged across 1996–2007.

Source: Dependent variables from OECD Labour Force Statistics, for other variables, see text.

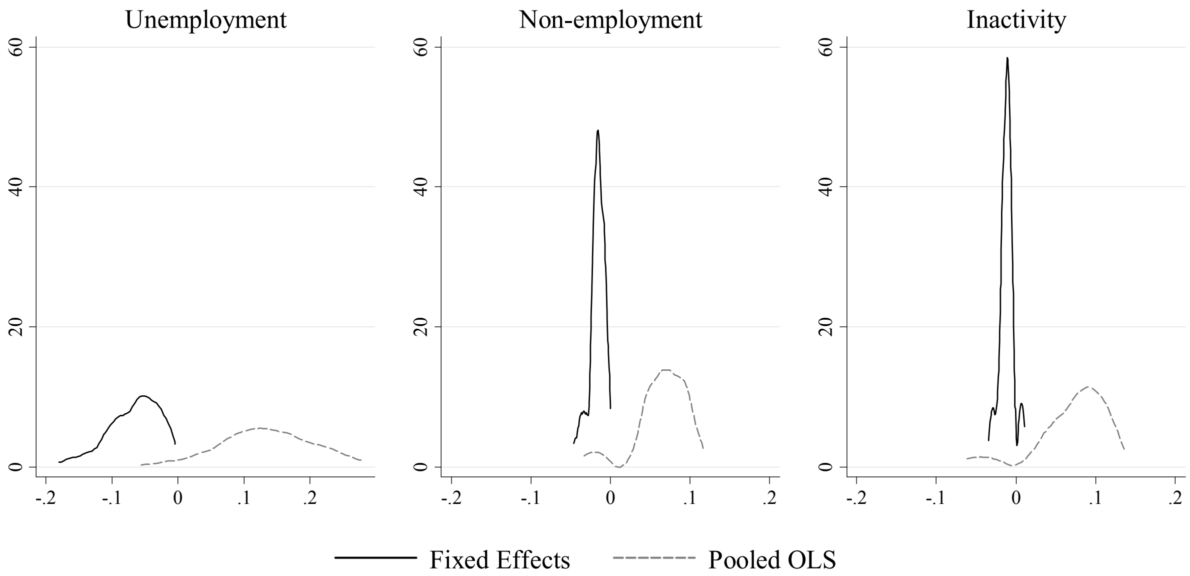
Table 6 OLS Regression estimates ("cluster robust" standard errors) of the association between regulations on temporary contracts and youth (ages 15–24) unemployment rates (ln), OECD and EULFS data

		Young men		Young women		ISCED 0-2	
		Bivariate	Control	Bivariate	Control	Bivariate	Control
OLS	Full Sample	0.079* (0.045)	0.036 (0.078)	0.204*** (0.050)	0.128 (0.081)	0.113** (0.048)	0.156 (0.107)
	EU14	0.088 (0.050)	0.076 (0.093)	0.215*** (0.048)	0.183* (0.091)	0.139** (0.059)	0.175 (0.127)
	1985–1992	0.024 (0.065)	-0.025 (0.115)	0.148** (0.062)	0.069 (0.105)		
	1993–2000	0.119** (0.047)	0.137 (0.109)	0.232*** (0.052)	0.197* (0.111)	0.120** (0.047)	0.247 (0.141)
	2001–2007	0.111** (0.041)	0.082 (0.091)	0.251*** (0.060)	0.185 (0.125)	0.126* (0.063)	0.126 (0.121)
Fixed effects	Full Sample	-0.086 (0.063)	-0.108* (0.059)	-0.041 (0.056)	-0.078 (0.055)	-0.083* (0.041)	-0.090** (0.032)
	EU14	-0.112 (0.067)	-0.163** (0.061)	-0.061 (0.060)	-0.138** (0.053)	-0.081* (0.044)	-0.094** (0.035)
Fixed effects, linear trends	Full Sample	-0.043 (0.034)	-0.022 (0.022)	-0.040 (0.030)	-0.033 (0.023)	-0.032 (0.033)	-0.028 (0.024)
	EU14	-0.076*** (0.024)	-0.033 (0.021)	-0.064** (0.024)	-0.044* (0.023)	-0.035 (0.034)	-0.032 (0.025)

Note: *** p<0.01, ** p<0.05, * p<0.1. See Note to Table 3. The following control variables are used: collective bargaining coverage, trade union density, left party power in government, the output gap, economic globalization, product market regulations, output gap * trade union density, collective bargaining coverage * left party power, and collective bargaining coverage squared.

Figure 5 displays the estimated probability distribution of the regression coefficients in Table 6, as well as the regression coefficients of identical analyses using two other outcome variables. For non-employment and inactivity, country fixed effects wash away completely whatever effects regulations have in the cross-section. These results point to strong omitted variable bias in the cross-sectional analyses, which therefore should not be trusted.

Figure 5 Distribution of regression coefficients estimating the effect of regulations on temporary employment on aggregate youth labour market outcomes



Note: Kernel (epanechikov) density estimates of the distribution of OLS estimates of the effect of job security provisions on youth labour market outcomes; detailed results for unemployment in Table 6.

Table 7 reports the results from the DD analysis, separately for each education group.[27] As the FE results lead us to expect, we find no support whatsoever for the view that regulations on temporary contracts have an impact on youth unemployment. For the least educated (ISCED 0-2), stricter rules seem to go together with lower youth unemployment (columns 1–4, 5–7), but adjusting for linear trends causes signs to switch. The change in the output gap captures cyclical variation unaccounted for in the first-step regression (column 2, 3 and 6). Column 3 also attests to a strong effect negative effect (robust to outliers) of unionization on youth unemployment.[28] For more educated groups, we observe positive effects of regulations on unemployment (columns 1–4, 7), but negative effects if regulations on temporary contracts are strict (columns 5–6). We no longer observe a residual impact of the business cycle, reflecting less sensitivity to macro-economic fluctuation among more skilled.

27 In Table A3, we check whether reforms impact adult unemployment rates, which is not the case.

28 Given that trade union density has been declining in most countries during the period of observation, this result is consistent with our theoretical expectation: As trade union come under pressure (by losing members), youth unemployment increases. The other indicators of labour power do not reach statistical significance and do not affect the results in a meaningful way.

We also tested whether the effect of deregulation interacts with collective bargaining coverage, trade union density, left party power, and geographic region (bordering Mediterranean, plus Portugal), but found no significant results. Finally, repeating these analyses with adjusted employment rates as dependent variables, we also find no significant effects (all results available on request). Even though temporary employment is now much more common among young people (see Figure 1), deregulation does not seem to have lead to overall employment growth. Instead the evidence is more consistent with a substitution of permanent with temporary jobs on the youth labour market (see also Kahn 2010).

Summary and discussion

In their review of European unemployment trends, Nickell and Layard (1999) conclude that compared to the role played by unions and social security systems "time spent worrying about strict labour market regulations, employment protection and minimum wages is probably time largely wasted (1999: 3030)." The argument that EPL causes high unemployment (OECD 1994) has been challenged on theoretical (Lazear 1990; Blanchard and Katz 1997; Bertola 1999; Pissarides 2001; Cahuc and Zylberberg 2004) and empirical grounds (OECD 1999; Howell et al. 2007; Baccaro and Rei 2007). However, the original critique of EPL has subsequently been reformulated. Across academic disciplines and partisan divides a consensus has emerged that if EPL is not bad for everyone, it is bad for young people in particular.

The review of theoretical and empirical research conducted here raises serious doubts about the validity of this consensus. The promise of deregulation was that by freeing market forces workers would be compensated for the loss of permanent employment opportunities and lessened protection in permanent jobs. The evidence presented here suggests that this promise has been broken, in particular for young people (for similar results, see OECD 1999; Kahn 2010). The empirical analysis finds no robust evidence whatsoever linking either dimension of EPL to inferior youth labour market performance. Making it easier to fire incumbent workers by reducing dismissal costs neither lowers young peoples' unemployment risks nor raises their employment chances. Not even the positive bivariate correlation between job security provisions and youth unemployment often reported by prior research can be reproduced. A strong positive correlation between regulations on temporary contracts and youth unemployment is found in the cross-section, but this correlation is wiped out completely by country fixed effects. Temporary employment has become a lot more common among young people (see Figure 1), but deregulating this type of employment has done nothing to reduce youth unemployment or increase youth employment rates. However, partial deregulation has at least not increased youth unemployment, as some critics have argued. The theoretical discussion already indicated multiple countervailing mechanisms through which EPL affects youth labour market outcomes. Possibly, these mechanisms are offsetting each others' effects with more or less equal strength, yielding small effects not significantly different from zero.

Table 7 First-Difference OLS regression estimates (OLS standard errors) of the effects of reforms of regulations on temporary contracts on youth unemployment

ISCED 0-2	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Reg. temp. contracts (RTC)	-0.301 (0.641)	-0.753 (0.547)	-0.074 (0.457)	0.250 (0.571)	1.853 (2.376)	0.737 (2.006)	-0.399 (2.191)
Job security prov. * RTC					-1.119 (1.188)	-0.764 (0.988)	0.337 (1.096)
Output Gap		-1.664** (0.619)	-2.030*** (0.472)			-1.593** (0.637)	
Trade union density			-1.844*** (0.580)				
Constant	-0.756 (0.904)	0.042 (0.791)	-2.528** (0.998)	-0.155 (0.805)	-1.019 (0.950)	-0.171 (0.852)	-0.075 (0.876)
Linear trends	No	No	No	Yes	No	No	Yes
N	14	14	14	14	14	14	14
Adjusted R^2	-0.06	0.30	0.62	-0.07	-0.07	0.27	-0.15
ISCED 3-4	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Reg. temp. contracts (RTC)	0.423 (0.594)	0.250 (0.630)	0.685 (0.686)	0.869 (0.511)	2.753 (2.169)	2.379 (2.272)	0.409 (1.966)
Job security prov. * RTC					-1.211 (1.085)	-1.092 (1.120)	0.239 (0.983)
Output Gap		-0.635 (0.713)	-0.869 (0.709)			-0.533 (0.722)	
Trade union density			-1.181 (0.870)				
Constant	-1.338 (0.838)	-1.033 (0.912)	-2.680 (1.498)	0.263 (0.721)	-1.623* (0.867)	-1.339 (0.966)	0.320 (0.786)
Linear trends	No	No	No	Yes	No	No	Yes
N	14	14	14	14	14	14	14
Adjusted R^2	-0.04	-0.06	0.02	0.13	-0.02	-0.06	0.05
ISCED 5-6	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Reg. temp. contracts (RTC)	0.401 (0.546)	0.206 (0.568)	0.235 (0.674)	0.585 (0.502)	2.254 (2.023)	1.804 (2.081)	0.482 (1.935)
Job security prov. * RTC					-0.963 (1.012)	-0.819 (1.025)	0.053 (0.968)
Output Gap		-0.718 (0.644)	-0.734 (0.696)			-0.642 (0.661)	
Trade union density			-0.080 (0.855)				
Constant	-0.395 (0.770)	-0.050 (0.823)	-0.161 (1.471)	0.380 (0.708)	-0.621 (0.809)	-0.279 (0.884)	0.392 (0.774)
Linear trends	No	No	No	Yes	No	No	Yes
N	14	14	14	14	14	14	14
Adjusted R^2	-0.04	-0.02	-0.12	0.03	-0.05	-0.05	-0.06

Note: *** p<0.01, ** p<0.05, * p<0.1. Result of OLS regressions of the before-after difference in average adjusted youth unemployment rates on the before-after difference in the respective EPL index (second step regression). For details on estimation, see pages 15 and 16, esp. step IV.

Source: European Union Labour Force Survey, German Microcensus, IPUMS-CPS; own calculations.

Alternatively, the effect of EPL may simply be spurious. EPL is one of different institutional factors that potentially cause variation in labour market dynamics; and it is itself endogenous to broader differences in institutional environments, which themselves structure labour market dynamics. Sociological research has long pointed to the role of education systems and credentialing practices in structuring labour market dynamics (DiPrete 2002; Gangl 2004b) and the role of tertiary education to provide an alternative role for young people in times of slack labour markets (Barnhouse Walters 1984; Bozick 2009). Active and passive labour market policies are likely to have crucial influences as well (DiPrete et al. 2001; Gangl 2006), as do trade unions and collective bargaining regimes (Western 1998; Bertola et al. 2007) and cyclical factors (Blanchflower and Freeman 2000). Without convincing attempts to rule out these alternate factors, whatever empirical effects ascribed to EPL may be spurious.

In any case, under which conditions more dynamic labour markets lead to better aggregate and individual labour market outcomes is still an open question. Job loss can be very costly for individuals (Gangl 2004a, 2006; Brand 2006). It does not only cause skill and earnings losses, but also poses serious health risks (Sullivan and von Wachter 2009).[29] Neither is job mobility generally welfare enhancing (Fuller 2008; Gladden and Taber 2007). Viewed from this perspective, EPL may indeed have social benefits. It diminishes the existential threat of job loss by constraining to what extent individual workers are treated like “variable costs” (Krugman 2010). It may enhance productivity and welfare by providing incentives for human capital formation, innovation[30] as well as economic security. In short, future research should consider both costs *and* benefits of EPL.

Finally, the socio-economic and political implications of the increasing concentration of job insecurity among young people remains a highly relevant topic for research in class analysis and political economy. For example, it is remarkable that young people in Western European countries have largely kept quiet in the face of a pronounced erosion of their labour market position (Blanchflower and Freeman 2000). The outbursts of violence in Paris, London or Athens can be interpreted as a sign of young people’s grievances, but as of yet, there is no evidence that these grievances are effectively mobilized by working class representatives (Bonoli 2005). Trade unions and left parties appear to have been unable or unwilling to resist deregulation at the margin, trading-off job security of their core constituents against the fortunes of marginal groups that have less influence on the political process and on bargaining tables. In an effort to curb class inequalities, working class representatives may thus have contributed to the emergence of demographic inequalities, concentrating unemployment and atypical employment among politically weaker groups (see Bertola et al. 2007, for empirical evidence).

[29] Using administrative data from Pennsylvania, Sullivan and von Wachter (2009) find post-displacement increases in mortality hazards of 50-100% in the years immediately following job loss.

[30] Job security provisions may provide the necessary incentive for high skill workers to engage in innovative, but risky projects that in case of failure would lead to dismissal in the absence of job security provisions (Acharya et al. 2010).

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Appendix

Table A1 First-Difference OLS regression estimates (OLS standard errors) of the effects of reforms of job security provisions on adult unemployment

	(1)	(2)	(3)	(4)	(5)	(6)
Job security provisions (JSP)	-0.258 (1.490)	-2.893** (1.089)	-0.803 (1.780)	1.189 (5.780)	2.257 (3.917)	1.493 (4.120)
Reg. temp. contracts * JSP				-0.656 (2.522)	-2.333 (1.709)	-1.351 (2.171)
Spain 1994			-3.041 (2.100)			-2.042 (2.692)
Constant	-0.828 (0.536)	-0.267 (0.391)	-0.248 (0.375)	-0.869 (0.580)	-0.415 (0.393)	-0.340 (0.413)
Country spec. shocks	Yes	No	No	Yes	No	No
Country spec. trends	No	Yes	Yes	No	Yes	Yes
N	14	14	14	14	14	14
Adjusted R^2	-0.08	0.32	0.38	-0.17	0.36	0.34

Note: *** p<0.01, ** p<0.05, * p<0.1. Result of OLS regressions of the before-after difference in average adjusted adult unemployment rates on the before-after difference in the respective EPL index (second step regression). For details on estimation, see pages 15 and 16, esp. step IV, as well as footnote 22.

Source: European Union Labour Force Survey, German Microcensus, IPUMS-CPS; own calculations.

Table A2 First-Difference OLS regression estimates (OLS standard errors) of the effects of reforms of regulations on temporary contracts on adult unemployment

	(1)	(2)	(3)	(4)
Reg. temp. contracts (RTC)	-0.295 (0.348)	0.565 (0.349)	1.093 (1.269)	0.601 (1.344)
Job security prov. * RTC			-0.721 (0.634)	-0.019 (0.672)
Constant	-0.691 (0.491)	0.826 (0.492)	-0.861 (0.507)	0.822 (0.537)
Country spec. shocks	Yes	No	Yes	No
Country spec. trends	No	Yes	No	Yes
N	14	14	14	14
Adjusted R^2	-0.02	0.11	0.00	0.03

Note: *** p<0.01, ** p<0.05, * p<0.1. see Note to Table A1.

Source: European Union Labour Force Survey, German Microcensus, IPUMS-CPS; own calculations.