EMPLOYMENT PROTECTION AND JOB INSECURITY

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Country Acronyms

AT	Austria
AU	Australia
BE	Belgium
CA	Canada
DE	Germany
DK	Denmark
ES	Spain
FI	Finland
FR	France
GR	Greece
IE	Ireland
IT	Italy
JP	Japan
NL	Netherlands
NO	Norway
NZ	New Zealand
PT	Portugal
SE	Sweden
SW	Switzerland
UK	United Kingdom
US	United States

Abbreviations

DiD	Differences-in-Differences
EPL	Employment protection legislation
EPR	Employment protection regular contracts
EULFS	European Union Labour Force Survey
LSDV	Least squares dummy variable
OECD	Organisation for Economic Co-operation and Development
OLS	Ordinary least squares
RTC	Regulations on the usage of temporary contracts

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Introduction and Overview

Youth labor market outcomes have declined over the last four decades across advanced economies. In absolute terms or relative to adults, young people now earn less, are more likely to be unemployed, less likely to be employed in full-time, permanent jobs, and more likely to be employed on a temporary or part-time basis (Blanchflower and Freeman, 2000; Ryan, 2001; Blossfeld et al., 2005; DeFreitas, 2009). Policy makers have had a long-standing interest in facilitating the transition from school to work, in particular for non-college-bound youth, to avert the far-reaching negative individual and societal consequences that may result from failed integration into working life. That we have witnessed deterioration nevertheless, and despite steadily shrinking youth cohorts and ever-rising levels of educational attainment, should therefore be a cause of concern. Squandering youth potential at the transition from school to work cannot be economically sustainable in countries where fewer and fewer economically active have to support an increasing number of pensioners. These concerns have gained new relevance in the recent economic and financial crisis, during which young people have again been among those affected the most (Bell and Blanchflower, 2010).

While some factors that have contributed to the decline of youth labor market outcomes are difficult to manipulate, especially in the short run, much research has emphasized the role of policies and institutions. When surveying the literature, employment protection legislation frequently tops the list of labor market institutions affecting not only unemployment but also temporary employment among young people (Scarpetta, 1996; Esping-Andersen, 2000; Heckman and Pages, 2004; Russell and O'Connell, 2001; van der Velden and Wolbers, 2003; Breen, 2005; Allard and Lindert, 2006; Bertola et al., 2007; OECD 1994, 2004, 2006; Kahn, 2007). To many, employment protection seems to perfectly exemplify the idea of "labor market rigidity", which neoclassical economics sees at the root of European unemployment (OECD, 1994; Siebert, 1997). Hence, employment protection continues to spark debates in politics and academia (Kalleberg, 2009). As markets become more integrated, individuals become more vulnerable to economic decisions taken far removed from their sphere of influence. Across European countries workers have been granted degree of job protection against market forces beyond their control; and employers have accommodated these demands. However, granting workers rights to a job seems fundamentally at odds with an apparent consensus that has emerged across academic disciplines and political divides: Economies, employers, and individuals alike need to be always ready to respond to changes in markets that have become irrevocably more turbulent.

While it has been increasingly acknowledged that the impact of employment protection on the aggregate unemployment rate is indeterminate, a new consensus has emerged that stresses the distributional effects of employment protection legislation. Employment protection may not affect some aggregate outcomes, but its effects are detrimental for specific demographic groups, in particular young people. On this basis, economists continue to recommend deregulation of employment protection to improve youth labor market outcomes, in particular to lower youth unemployment and regulate the growth of temporary employment (OECD, 1994, 2004, 2006).

I began working on this issue, because a number of influential empirical studies that are cited in support of the consensus rely on cross-sectional data, comparing employment protection legislation and youth labor market outcomes across countries (Esping-Andersen, 2000; Breen, 2005; Polavieja, 2006; Kahn, 2007; Wolbers, 2007). A key goal of this study therefore is to perform a longitudinal assessment, estimating the effects of reforms of employment protection on youth labor market outcomes within countries over time, which may result in credible estimates of the effects of employment protection. Second, hardly any evidence exists on whether distinct dimensions of employment protection, i.e. job security provisions on regular employment contracts and regulations on the usage of temporary contracts, have different effects or interact with each other. Third, indicators of employment protection legislation have been developed, revised and extended several times over the past twenty years, which has led to more accurate measurement of employment protection legislation. For this study, we can draw on the most recent and arguably highest quality indicator of employment protection, released by the Organization of Economic Cooperation and Development (OECD) in 2009 (see Venn, 2009). Fourth, while many studies supporting the consensus have used aggregate data on youth unemployment or employment, this study estimates the effects of employment protection legislation at a more disaggregate level, focusing on demographic groups defined by gender and education.

While initial results indeed supported the consensual view, subsequent work showed that these results were not robust to inclusion of control variables, to changes in the country composition, and changes in model specification. Indeed, I could not even reproduce the simple bivariate relationship that youth unemployment is significantly higher in countries with strict job security provisions on regular employment contracts. The persistent lack of empirical support for the economic and sociological consensus linking employment protection to youth unemployment led me to reassess its theoretical underpinnings. Against common interpretations (Lazear, 1990; OECD, 2004; Breen, 2005; Cahuc and Zylberberg, 2007), my reading of theoretical research suggests that the effect of employment protection on (relative) youth unemployment is indeterminate and can only be resolved empirically.

Given this theoretical indeterminacy, an even larger burden is imposed on the empirical analysis. However, empirical research on this issue has tried to simultaneously tackle various explanations of youth (un)employment, where typically a number of predictors of youth unemployment are entered into a regression model and each estimated coefficient is implicitly or explicitly given a causal interpretation. Even though we cannot eliminate all obstacles to causal inference, our premise here is that focusing our theoretical and empirical analysis on just one factor, we may be more successful in identifying the true, causal effect of employment protection legislation. We devote more space to a detailed discussion of the complex underlying mechanisms, which in this case leads to some qualifications of the consensual view about the adverse effects of employment protection on young people. Empirically, we can make a broader assessment of the effects of employment protection on different dependent variables, use different analytical designs and perform more extensive specification checks to probe the robustness of our findings.

Using differences-in-differences to estimate the effect of employment protection reforms (changes in job security provisions on regular employment contracts, changes in regulations on the usage of temporary contracts), we obtain the following results, focusing on sixteen Western countries for the period from 1992 to 2006: We find no effects of job security provisions on regular employment contracts on overall employment, temporary employment or unemployment among young people or adult men. Neither could we find evidence that the effect of job security provisions differs significantly across education groups nor depends on the extent of regulations on temporary contracts. However, strict job security provisions may constrain labor market opportunities of young people, which causes them to drop out of the labor market. We could not detect aggregate effects of the deregulation of temporary contracts on employment, unemployment and labor force participation. We also could not find any evidence that the effect of deregulation depends on the strictness of job security provisions on regular contracts. Considering different demographic groups, in particular tertiary educated youth seem to benefit from deregulation, while less educated adult men seem to suffer. Deregulation of temporary contracts therefore seems to have increased inequalities in job insecurity, but has not led to employment gains among the less educated.

The rest of this study proceeds as follows. Chapter 2 surveys recent trends on the youth labor market. We document a gradual erosion of job security on the youth labor market over the past four decades, which manifests itself in terms of rising unemployment, temporary employment and part time employment. Job insecurity grew in absolute terms and especially relative to adults and continued to grow in the 1990s and early 2000s, despite more favorable macroeconomic conditions compared to earlier periods and despite favorable supply side shifts. We offer several explanations that focus on the role of demand shifts and institutions in accounting for this trend.

Chapter 3 presents a theoretical discussion of the relationship between employment protection and job insecurity, and assesses the theoretical foundations of the claim that employment protection makes young people worse off on the labor market, in relative or absolute terms. We distinguish two dimensions of employment protection legislation, job security provisions on regular/permanent contracts and regulations on the usage of temporary contracts, as well as two main dimension of job insecurity, unemployment and temporary employment. While we can derive unambiguous hypotheses about the effect of employment protection on temporary employment, we argue that contrary to the prevailing consensus the effect of employment protection on youth unemployment is theoretically indeterminate.

Chapter 4 outlines the analytical strategies and data sources that the empirical analyses rely on. We begin with a description and discussion of the data sources, the countries and years covered. We describe the advantages and drawbacks of analytical designs that rely on cross-sectional or longitudinal data from the perspective of casual inference. In particular, we explain how we adapted the differences-in-differences approach to the question and data at hand, as well as how we estimated standard errors. The Data Appendix to the chapter provides references for all data sources used in the analyses as well as variable definitions.

Chapter 5 and 6 summarize the results of the empirical analysis. We use two different data sources for labor market outcomes and three main analytical steps. We begin with a cross-sectional analysis, where we rely on both variation within and across countries, since this type of design is commonly used in published research. In the next step, we adjust for unit-specific (country-specific) fixed effects, eliminating all variation between units (countries), and conduct a conventional fixed effects analysis. The main point is to assess the robustness of the cross-sectional results and relate our findings to published research using similar designs. In the third, decisive step, we use differences-in-differences to estimate the effect of reforms of both dimensions of employment protection legislation on different outcomes. We assess whether there have been interactions between both dimensions of employment protection (job security provisions, regulations on temporary contracts) and whether the impact of reform differed for individuals with different genders or levels of education.

In Chapter 7, we restate the consensus about the distributional effects of employment protection legislation, focusing on the role of the Organization of Economic Co-operation and

Development (OECD) in its making and maintenance, and we contrast this consensus with the findings from this study. We summarize and discuss our results in light of their policy relevance and also address some methodological concerns. We conclude by discussing potential shortcomings of this study as well as alternative explanations for the decline in job security on the youth labor market.

2

The Changing Labor Market

As the post-war economic boom came to an end in the early 1970s, labor markets in advanced economies entered a period of crisis and restructuring. Across European countries, individuals have been increasingly exposed to unemployment and have had to cope with jobs that are limited in duration, in hours, and in terms of benefits. At the same time, the United States experienced a dramatic polarization of the wage and employment structure (Acemoglu, 2004; Autor et al., 2006; Lemieux 2008). Since the late 1970s, the distribution of wages has become more polarized, as high earners have recorded robust gains, and middle and bottom earners have lost. The polarization of the wage structure is mirrored by a polarization of employment, with shrinking middle, and expanding upper and lower classes. Similar tendencies, although less extreme, have been observed in European countries as well (Goos and Manning, 2007; Goos et al., 2009; Spitz-Oener, 2006; Dustmann et al., 2009).

In 1994, Paul Krugman provided a succinct summary of the mainstream economic consensus, hypothesizing that both rising unemployment and rising wage inequality are "two sides of the same coin" (Krugman 1994: 37; Blau and Kahn, 2002). The basic argument behind this “unified theory” is that Europe and the U.S. have reacted to similar changes in different ways because of different wage setting institutions. Universal changes have swept across advanced economies, most important of which has been a skill-biased demand shift, i.e. a shift in labor demand away from less skilled to more skilled workers. If wages are determined flexibly by labor market participants, a skill-biased demand shift leads to higher wage inequality in the U.S. (and other liberal economies), as employers pay lower wages to unskilled and higher wages to skilled workers. In Europe, however, institutions (minimum wages, unemployment insurance, collective wage bargaining, trade unions) constrain wage flexibility. If institutions prevent employers from lowering relative wage of less skilled workers, their only response can be to adjust employment (Bertola and Rogerson, 1997). Consequently, employment among the less skilled declines and unemployment rises.

The "unified theory" has been appealing for its simplicity and analytical ambition: a common explanation of two of the most important trends witnessed on the labor markets of advanced Western countries in recent decades. It has also been appealing to some for condensing the neoclassical critique of European unemployment: a portrayal of "European institutions" as rigid, as being in the way of markets, at the root of unemployment in Europe (OECD, 1994; Siebert, 1997). Economists have attacked the Krugman hypothesis, mainly on empirical grounds (Nickell and Bell, 1996; Card et al., 1999). Other scholars have voiced more fundamental concerns, emphasizing that analysis ought to be more sensitive to the diversity in employment outcomes across European countries and to the variation in institutional conditions across European countries and over time (Ebbinghaus and Kittel, 2005; Maurin and Postel-Vinay, 2005; DiPrete et al., 2006; Nickell, 1997; Nickell et al., 2005).

In this chapter, focusing on trends on the youth labor market over the past four decades, we want to offer some extensions and modifications to the current consensus. Much of the literature on growing skills gaps has focused on inequalities between education groups. However, when considering the growth and distribution of atypical employment, inequalities between demographic groups defined by age, gender and ethnicity become crucial. The empirical analysis illustrates this point by comparing the changing labor market outcomes of young people and adult men. We show in the following that job security on European youth labor markets has eroded (see also Ryan, 2001; Blanchflower and Freeman, 2000; Blossfeld et al., 2005; DeFreitas, 2009). Across Europe, young people have had to come to terms with a decline in full-time permanent employment, a growth in unemployment, temporary and part-time employment in both absolute terms and relative to adult men. These trends suggest that young people have shouldered a disproportionate share of the burden of adjustment to structural change.

The key point we want to illustrate and offer explanations for is that the growth of labor market inequalities in Europe, the rise of unemployment, job insecurity or precarious employment, should not just be understood from a class- or education perspective, but from a demographic perspective. It is well known that young people, women and migrants have a weaker attachment to the labor force and are hence more responsive to structural and cyclical changes, but most research has almost exclusively focused on inequalities between education groups. However, research on the labor market disadvantages experienced by women, youth and migrants has shown that their relative disadvantages can often not be fully accounted for by productivity, education and class position alone. Comparative research suggests that their relative disadvantages vary substantially across countries and time, which points to the importance of social and cultural factors. [1] From a sociological perspective, a way of approaching this issue is to focus on the role of institutions in structuring and generating inequalities at particular life course transitions (DiPrete, 2003). In this chapter, we illustrate this perspective focusing on the youth labor market, but it clearly applies to other demographic inequalities as well.

The overall goal of this study is to analyze whether employment protection legislation affects the labor market position of young people, and whether employment protection reforms are generally effective policy tools to improve the labor market conditions of young people. Apart from employment protection legislation, which we discuss in the following chapters, we identify three other theoretical accounts linking institutions to the growth of demographic inequalities, in particular age-based inequalities, across advanced economies. We survey these explanation in the following section and then proceed with an analysis of youth labor market trends.

2.1. Explaining the decline in job security on the youth labor market

From a labor demand perspective, changes on the youth labor market are consequence of changes in labor demand, i.e. adverse, cyclical demand shocks or skill-biased demand shifts. Young people disproportionately suffer in this context because they lack skills, both general and specific skills. Institutions condition the impact of demand shocks, and may also change in response to these shocks. In particular, institutions determine which outcomes are affected, whether young people's wages or job security declines.

[1] Experimental studies repeatedly show evidence of ethnic and gender discrimination on the labor market (Rouse and Goldin, 2000; Pager, 2003; Bertrand and Mullainathan, 2004). Comparative research also documents persistent gender gaps in income and unemployment (Azmat et al., 2006; Gangl and Ziefle, 2009) and age gaps (Ryan, 2001; Breen, 2005) in employment outcomes, which appear not reducible to skills alone and which appear to be responsive to institutional or cultural factors.

From a bargaining perspective, young people may lose out in the distributional conflict over quality jobs since they lack bargaining power relative to labor market insiders, in particular adult men. The institutional and possibly cultural context is decisive in how this conflict is resolved. Following Goldthorpe (1984), we identify a tendency towards dualization, a demographically selective retrenchment of job security, protecting the rents of the core workforce, while exposing marginal labor market groups to higher risks, or a corporatist response, where collective bargaining has managed to partly contain dualization tendencies. Finally, it may not be so much be the presence of strict job protection legislation, but the absence of generous unemployment benefits (in countries that have strict job security provisions), which causes job insecurity to shift onto young people. In the following, we survey each of these explanations, and then continue with a survey of key trends on the youth labor market.

2.1.1. Labor demand shifts: extending the unified theory

Economists have clearly recognized a deterioration of outcomes among young people over the past four decades, starting in the early 1970s (Blanchflower and Freeman, 2000; Ryan, 2001; DeFreitas, 2009). Within the framework of the "unified theory", this decline is found to manifest itself either in terms of a decline in relative wages in liberal countries like the U.S. and Great Britain or a decline in employment in regulated European countries like France or Spain. In a third group of countries, including Japan and Germany, institutional factors seem to have prevented major declines on either dimension. This deterioration occurred despite changes on the supply side that should have improved labor market outcomes of young people, including shrinking cohort sizes and rising educational attainment. Hence, the problem must lie on the demand side.

Blanchflower and Freeman (2000) argue that the youth labor market merely reflects, in an amplified way, general economic tendencies, including weak aggregate labor demand. However, cyclical factors alone cannot explain the persistent erosion of job security among young people, characterized by permanent losses in relative wages and increases in unemployment, temporary and part-time employment. To account for the persistent decline, Ryan (2001, 2009, see also Christopolou, 2009) argues that a "doubly skill-biased" demand shift has not only led to widening differentials between education groups, but also to widening differentials between individuals with different levels of labor force experience.

Conventional approaches assume that skill-biased demand shift have led to increasing skill gaps in terms of employment/unemployment, where skill is defined by educational attainment (Krugman, 1994; OECD, 1994). However, this perspective has run into empirical difficulty as it does agree with simple trends observed across advanced countries (Nickell and Bell, 1995, 1996; Card et al., 1999).[2] Operating along-side a skill-biased demand shift raising the returns to educational degrees, an experienced biased-demand shift would explain why we have observed a deterioration of youth labor market outcomes despite favorable developments on the supply side. It would explain the relative stability in outcomes among labor market insiders, in particular adult men, and the concurrent decline in the position of young people. Thus, apart from rising returns to education, we have witnessed rising returns to experience, which implies growing inequality in outcomes between youth and adults. Rigid pay setting institutions then determine whether rising

[2] A basic problem facing the Krugman hypothesis is that it does not seem to agree with basic evidence. Nickell and Bell (1995, 1996) identify a cluster of countries including the U.S., Japan, Norway and Sweden, in which aggregate unemployment grew little from the mid 1970s to the late 1980s, but most of the increase in aggregate unemployment was driven by increases in unemployment among the unskilled. Widely diverging institutional setting seem to have produced similar employment responses. In contrast, unemployment grew strongly in Germany, the Netherlands, Spain, the UK and Canada, but both skilled and unskilled unemployment contributed to this rise, which Nickell and Bell (1996) interpret as the consequence of a skill-neutral shock.

returns to experience translate into a deterioration of young people's relative wages or relative employment/unemployment.

Recent critiques of the "unified theory" have argued that focusing on the contrast between employment/unemployment vs. wages as labor market outcomes ignores the growth of atypical jobs, in particularly temporary employment. Moreover, the assumption that rigid, time-invariant pay setting institutions are the key institutional factor accounting for cross-country variation in outcomes has been modified (Maurin and Postel-Vinay, 2005; DiPrete et al., 2006). European labor market institutions have been responsive to market changes that have lowered relative demand for unskilled workers. In particular, the deregulation and growth of temporary employment represents a market-driven institutional adjustment to increase employer flexibility, which results in higher job insecurity in particular among the unskilled. "European adjustment strategies have thereby produced rising inequality, but rising inequality in the job security rather than the wage component of the employment relationship" (DiPrete et al., 2006: 217).

Taking stock of the deregulation and growth of temporary employment as well as moving beyond the wage-unemployment contrast is a crucial extension to the unified theory that better accounts for the trends we have observed on European labor markets. However, while these contributions emphasize growing skill-gaps in job security, measured by education or occupation, we think that this perspective ignores that temporary employment grew particularly on the youth labor market in a number of countries.[3] Similar to Ryan's critique of the "unified theory", we should think of skills or human capital not just in terms of educational attainment, but also in terms of job-specific skills or in terms of labor force experience.

[3] For example, Giesecke and Gebel's (2009a) analyses for Germany suggest that growth in age-gaps in temporary employment were clearly more pronounced than changes in education- or class-gaps. Moreover, they show that temporary jobs are particularly prevalent among university graduates, which seems to contradict expectations that educated are generally less likely to be affected by insecure employment contracts (see also, McGinnity et al., 2005).

Combining both economic and sociological critiques, we offer the following "doubly-augmented unified theory" to link changes in labor demand to the growth of job insecurity on European (youth) labor markets. Following Ryan (2001), skill-biased demand shifts have led to a widening of skill-differentials in labor market outcomes, where skills have to be interpreted both in terms of general skills (measured by educational attainment) as well as specific skills (measured by labor force experience). This should lead to relative declines in labor market outcomes among less educated (compared to more educated) as well as young people (compared to adults), suggesting that uneducated young people have suffered the most. Second, European labor markets have responded to these demand shifts by granting employers more flexibility. By deregulating the usage of temporary contracts, employers now have at their disposal employment contracts with low or zero dismissal costs that effectively lower the costs of hiring less skilled, including young workers. Thus, to the extent that they cannot adjust wages, deregulation of temporary contracts as well as rising returns to education *and* experience have led to an erosion of job security, a growth of unemployment and temporary employment, among less educated and among young people. In countries with flexible pay setting regimes, however, employers have used wages to accommodate demand shocks, leading to a decline in earnings among young people. In either case, less educated youth should have suffered the most.

2.1.2. Political economy: dualism vs. corporatism

From an institutional perspective, the labor demand perspective has at least two shortcomings. First, it interprets age differences in labor market outcomes solely as the result of skill differences. Young people also differ from the adult workforce in other characteristics, in particular in terms of bargaining power. They have less political influence in determining the distribution of labor market outcomes through institutionalized channels, in particular collective bargaining arrangements. Second, the labor demand perspective has difficulty in accounting for relatively favorable youth labor markets observed in some countries, despite their "rigid" institutions. While the youth labor markets in Japan, Germany or the Netherlands have shown strain, they still seem to perform better than some of their European or Anglo-Saxon counterparts. The following perspective addresses both critiques, by analyzing the role of collective bargaining regimes in distributing employment across demographic groups. Following Goldthorpe (1984), we conceptualize a 'dualist' and a 'corporatist' response to labor market crisis in European countries.

Dualism in this context refers to the selective retrenchment of social rights, such that particular groups are no longer covered by certain rights that were won during the post-war boom years, like job security. As full-time ("full-benefit"), permanent jobs become increasingly scarce, labor representatives, i.e. trade unions and left parties, are faced with a zero-sum distributional conflict. In this situation, they have strong incentives to protect the labor market position of their core constituents. Political considerations (vote maximization) lead them to protect in particular the employed, core workforce, at the expense of the unemployed, precariously employed or inactive, by supporting policies safeguarding or extending rents of the employed (Saint-Paul, 2000; Rueda 2005, 2007).[4] Cultural factors privileging a male-breadwinner-model may further add a gender bias, as labor representative try in particular to protect the position of male breadwinners. Thus, groups with a weak labor force attachment (or with a weak political relevance in the eyes of labor representatives) find themselves at the losing end.[5] Depending on

[4] These models bear some resemblance to the insider-outsider models developed in economics (see Saint-Paul, 2000, for an updated account).

[5] To the extent that employment chances are merely redistributed within households, from wives, sons and daughters to male household heads, net changes in the flow of economic resources from employers to households do not change much. This in turn would calm outsider political resistance in the case of women and in the case of young people, to the extent that they still live with their parents.

the organization of wage bargaining, trade union influence may be particularly detrimental, if unions pursue sectional interests, and extract substantial rents for specific occupations or industries, which are won at the expense of outsiders' employment opportunities. Thus, particularly if wage bargaining occurs at decentralized or intermediate levels (industry or occupation), we would expect particularly adverse employment outcomes among outsiders in times of crisis (Calmfors and Driffill, 1988). Altogether, the dualist response to labor market crisis possibly characterizes best the experience of Southern European countries (including France), combining traditional family values, aggressive labor movements and influential left parties.

An alternative response has occurred in countries with corporatist industrial relations regimes, where employer and worker representatives, with variable involvement of the state, bargain over wages, employment and employment conditions for a large portion of the workforce. It is commonly argued that by coordinating their wage demands across the economy, trade unions are more likely to internalize the consequences of excessive wage demands, since resulting reductions in employment are likely to affect their members (Kenworthy, 2002). By bringing about (the expectation of) real wage restraint, governments may also be more likely to spend more on policies to reduce unemployment, having to worry less about inflation. Moreover, in some corporatist countries, employers and the state have maintained large scale vocational education systems that facilitate smooth transitions from school to work (Rosenbaum et al., 1990; Müller and Shavit, 1998; Ryan, 2001; Breen, 2005).

While active labor market policies have been particularly prevalent in the Scandinavian countries, extensive vocational education programs exist in Austria, Denmark, Germany, the Netherlands, and Switzerland. Corporatist arrangements may explain why some countries, in particular the latter Continental European countries could maintain relatively low rates or quickly restore low rates of youth unemployment, and they may also account for the exceptionally low unemployment rates found in Nordic countries before the financial crisis of the early 1990s. However, a number of studies also show that corporatist arrangements have come under pressure, by deregulatory reform, budgetary problems, gradual erosion of coordinating mechanisms and waning employer participation in vocational education (Thelen and Kume, 2006; Culpepper and Thelen, 2009; Thelen, 2009; Barbieri, 2009). The consequence of this erosion may be understood as a growing dualization, as certain demographic groups in the workforce, including young people, no longer enjoy the rights and benefits of older cohorts, like access to quality apprenticeship places or permanent, full-time employment.

2.1.3. Unemployment insurance

Apart from employment protection and collective bargaining regimes, unemployment insurance may be a factor contributing to the distribution of unemployment risks. Employed workers as well as labor representatives have a greater incentive to bargain for job protection, if institutionalized support for the unemployed is lacking. If employed workers face drastic income losses upon unemployment, we should expect more resistance to dismissal and more litigation to enforce existing employment protection legislation. If countries predominantly rely on either strategy, job security provisions or unemployment insurance, to reduce outflow from employment or to mitigate the consequences of unemployment, it may be less the prevalence of strict job security provisions, but the absence of generous unemployment support, which led to higher insider protection and diminishing employment chances for young people.

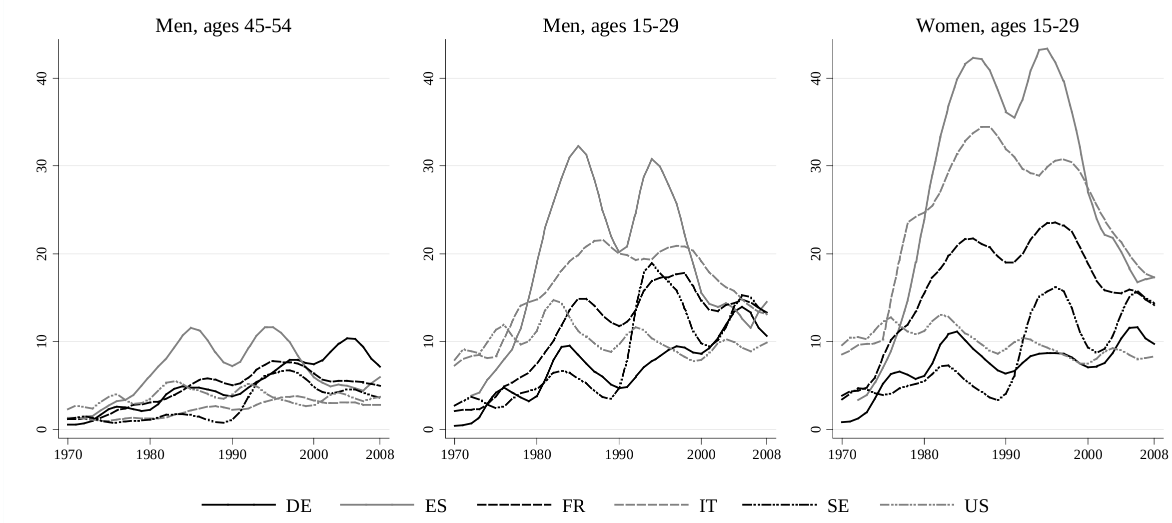
Cultural norms, favoring a male-breadwinner-model, may exacerbate this tendency (Algan and Cahuc, 2006; Azmat et al., 2006). In effect, employers and incumbent (male) employees act to protect male breadwinners in times of crisis, which at the societal level implies that at least one family member remains in stable employment and guarantees economic sustenance. In the absence of unemployment benefits, it would be too costly for society to risk job loss among sole family breadwinners, which were mainly men, especially in Southern Europe and in times of acute crisis throughout the 1970s and 1980s. Thus, weak unemployment insurance provisions for adult workers may therefore independently or in interaction with job security provisions led to adverse relative employment outcomes for labor market outsiders.

2.2. Youth unemployment

The following section charts the erosion of job security on the youth labor market. We start with the evolution of unemployment for different demographic groups in the United States and selected European countries, which were particularly affected by rising unemployment (Figure 2.1 and Figure 2.2). In the following, we usually define the youth labor market in terms of the age of participants, where youth or young people are 15-29 year olds.[6] We contrast their experience with the group that is normally most strongly attached to the labor market, adult (prime-age) men aged 45-54. Figure 2.1 shows that young people were disproportionately affected by rising unemployment. While the U.S. had comparatively high unemployment rates in the 1970s, some Western European countries witnessed substantial increases in unemployment. Youth unemployment rates virtually exploded in some countries, particularly among female youth, whose unemployment rates climbed well beyond 20-30% in a number of countries during the 1980s and again in the early 1990s. Compared to the magnitude of changes among youth, we hardly observe changes among adult men.

[6] In most aggregate data analysis, the youth labor market is defined in terms of age. While individuals aged 15-24 are considered to be on the youth labor market, we extend the upper limit to 29 whenever the available data permits this, reflecting that increasing participation in higher education has postponed the age at which young people enter the labor market for the first time.

Figure 2.1 Unemployment 1970-2008, % unemployed in labor force unemployed, by age and gender



Note: Data smoothed using one year moving average[7]
Source: OECD Labor Force Statistics

Comparing the experience of Italy to the experience of the U.S. allows for a very drastic numeric portrayal of the age-gap in unemployment: In 1981, 1.2% of Italian adult men aged 45-54 active on the labor market were unemployed, a *decrease* of 0.1 percentage points compared to 1970. Adult men's unemployment rate would reach a high of 3.8% in 1998 and recover to 2.8% by 2008. Over the entire period of observation, 1970 and 2008, it averaged around 1.4 percentage points below the rate of the corresponding demographic group in the U.S. In the meantime, unemployment among young Italians skyrocketed. In 1981, 25.5% of active young women aged 15-29 were unemployed. Female youth unemployment was roughly 21 times as high as the corresponding rate for adult men. Female youth unemployment would continue to rise, rarely dipping beneath 30% in the period from 1984 to 1999. Male youth unemployment followed a similar path, but averaged 8 percentage points below the corresponding female rate over the

[7] Each value in this and the following figures in this chapter is calculated as $y_{i,t} = (y_{i,t-1} + y_{i,t} + y_{i,t+1})/3$ in order to smooth out some cyclical variation and enhance readability.

period from 1970 to 2008. Over that same period, young Italian women's unemployment exceeded the adult men's unemployment rate on average by a factor of 12 (8 in case of young Italian men). In contrast, unemployment among young Americans of both genders averaged at around 10% exceeding the adult rate roughly by a factor of 3.

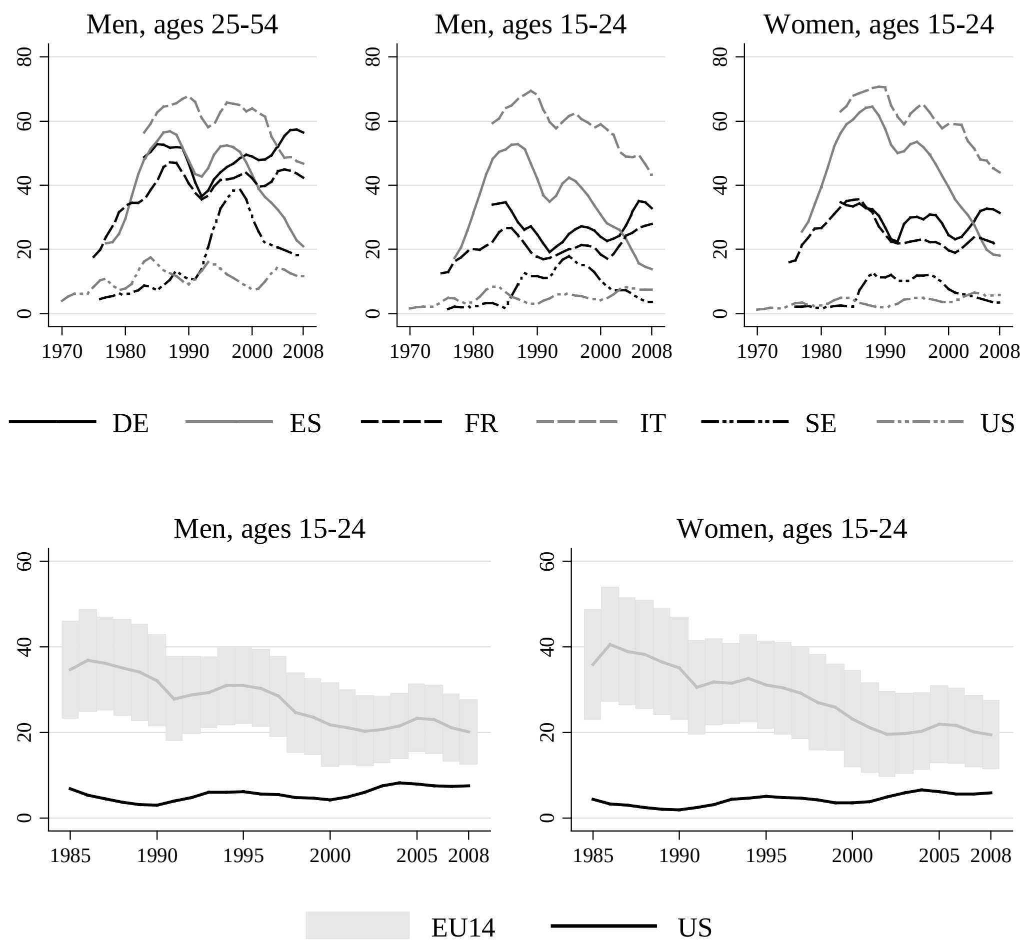
While the Italian case is maybe the most dramatic from a distributional perspective, a number of European countries witnessed rapid increases in youth unemployment rates from the 1970s until the mid-1990s. European youth unemployment peaked in the mid-1980s and the mid-1990s. Thereafter, we have observed some recovery. Average unemployment rates for Western European countries declined after 1995, for youth and adults, men and women alike. Although European youth unemployment is lower in 2008 compared to the mid-1980s or mid-1990s, we see substantial heterogeneity in the underlying country trends. In Italy and Spain, unemployment declined steadily from very high levels since the mid-1990s, but youth unemployment remains stubbornly high in Greece and France. Starting at very low levels in the 1980s, German and Swedish youth unemployment has shown a long-term upward trends. In contrast, Irish, British, but also Dutch and Danish youth unemployment rates have diminished substantially from high levels in the 1980s.

In 2008, we still observe the long shadows of the preceding decades (Table 2.2, Appendix, p. 26). Considering a broad sample of 28 OECD countries, Spain still has the highest unemployment rate among young men, in absolute terms and relative to adult men. Male youth unemployment is similarly high in Turkey and Italy. Sweden has the fourth highest unemployment rate among male youth and, in relative terms it ranks third among 28 OECD countries, surpassing Greece by a narrow margin. Averaging across countries, female youth unemployment is 0.7 percentage points above the male rate, but there exist considerable gender gaps in a number of countries, where we observe higher relative male unemployment in some

Anglo-Saxon countries and higher relative female unemployment in Mediterranean countries. In absolute terms, female youth unemployment is highest in the Mediterranean countries, with Greece taking the lead in terms of both relative and absolute female youth unemployment. Spain, Turkey, Italy and Portugal follow. At the other end of the distribution, among Western European countries the Netherlands is the top performer among both men and women. Youth unemployment is also low in Denmark and Switzerland. Extraordinarily low youth unemployment appears to be a thing of the past in Germany, although in relative to adult men, German youth unemployment is still among the lowest in Western Europe.

In Figure 2.2, we consider the duration of unemployment spells, comparing the percentage of unemployed youth who are out of a job for one year or longer over countries and years. The trends in the share of long-term unemployed reflect the trends in the aggregate youth unemployment rate, but there is evidence of a persistent decline since 1985. While average European youth unemployment rates have approached levels similar to those in the U.S., the incidence of long-term youth unemployment in Europe is still substantially higher compared to the U.S. Long-term unemployment among young people is also remarkably low in Sweden. Further analyses (data not shown) indicate that in 2006, the modal group of unemployment spells is six months or less in the countries considered here, except for Italy and Greece, where youth unemployment is predominantly long-term, i.e. exceeding 12 months. While still having predominantly short-term unemployment spells, we observe a rise in long-term spells among Austrian and German youth as well as among French and Greek male youth throughout the 1990s and early 2000s.

Figure 2.2 Long-term unemployment 1985-2008, % unemployed one year or longer among unemployed, by age and gender



Note: The EU14 comprise the pre-2004 EU member states minus Luxembourg, plus Norway. Bars represent 90% robust confidence intervals around the unweighted EU14 mean for a given year.
Source: OECD Labor Force Statistics

2.3. Precarious employment on the youth labor market

European countries implemented a number of reforms to improve labor market outcomes and bring down unemployment, including a shift from passive to active labor market policies, reforms of public employment services and deregulation of temporary and part-time employment. These reforms occurred in the context of broader structural and macroeconomic changes, including the phasing out of anti-inflationary fiscal and monetary policy of the early 1980s, intensifying European and global integration, persistent technological change, as well as declining youth cohort sizes and rising participation in tertiary education. While these factors have probably contributed to the decline in youth unemployment, the structure of employment that has emerged after crisis and reform has changed fundamentally. Across Europe, precarious forms of employment have grown, and it is particularly young people who have been affected by this trend.

In Figure 2.3 and Figure 2.4, we chart the growth of temporary jobs for selected European countries and the U.S. Temporary jobs expire after a contractually specified period and usually impose no further obligations on the employer after expiration. While subject to some regulation, they grant employers considerable flexibility in adjusting employment, especially since in a number of European countries regular employment contracts are subject to rather strict dismissal regulations. Temporary employment is more likely to be involuntary than part-time employment, and usually implies contract durations of less than 12 months (see Table 2.3 in Appendix to this chapter).[8] We define temporary employment here as the number of individuals holding a temporary job in the active labor force.[9]

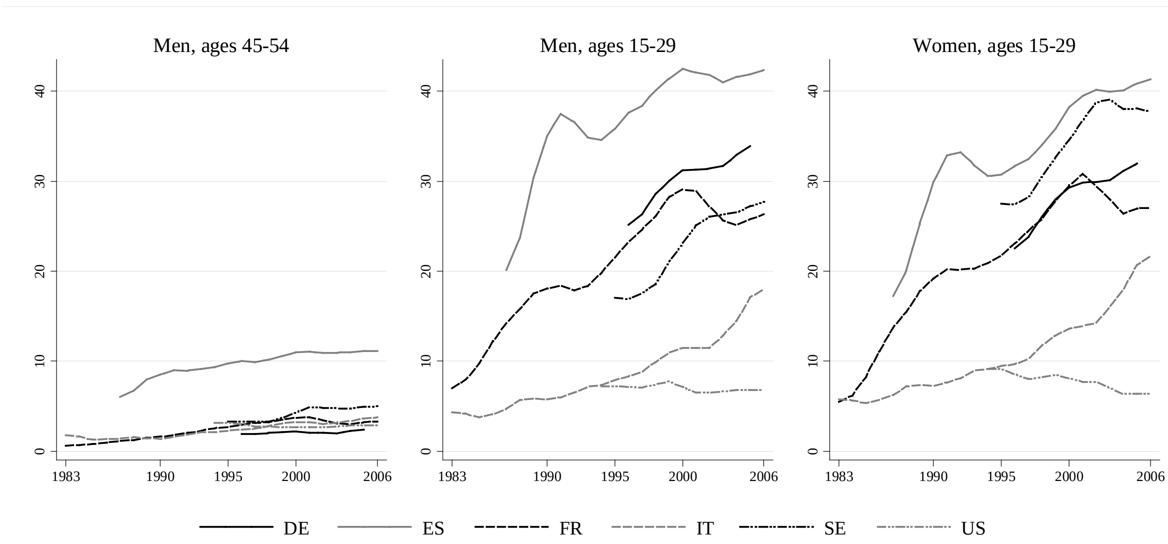
Temporary employment has grown explosively in Spain, but we also observe persistent growth elsewhere in Europe, with the exceptions of Denmark, Ireland, and the UK, as well as,

[8] In the case of part-time employment, where part-time employment appears to be more prevalent, it also tends to be more often voluntary. In the Netherlands, where the incidence of part-time employment is high, only 6% of young people holding a part-time contract in 2006 report doing so involuntarily. We should therefore bear in mind that part-time and temporary employment need to be distinguished as different forms of labor market "precarity", where part-time employment is more often entered voluntary in the presence of a second full-time earner in the household. In contrast, in the case of temporary employment appears to be more often involuntary and of short duration (less than 12 months) particularly where its incidence is high. It thus represents a clear source of job insecurity, which is confirmed by many micro data studies (for example, Giesecke and Gross, 2003; DiPrete et al., 2006).

[9] In the following, we mainly rely on the augmented EULFS dataset, which like the available OECD data has some missing information (see Data Appendix, p.103).

Greece. These results correspond to Booth et al. (2002) assessment of aggregate trends in temporary employment. However, it is less frequently noted that, across Europe, a lot of the increase in temporary employment has occurred among young people. If we average over all countries (which is problematic given the unbalanced availability of data), we observe that 11% young people's jobs are under a temporary contract in 1983. The corresponding number in 1996 is 29%, and 36% in 2006.[10] At the same time, among 45-54 year old men, we observe only a slight increase, from 2.5 to 5.5% between 1983 to 2006.

Figure 2.3 Temporary employment, % in temporary employment in labor force, by age and gender



Note: Data for U.S. on 15-24 year-olds (young people) and 25-54 year-olds (adult men).
Source: EULFS, own calculations. OECD Labor Force Statistics for data on U.S.

The imbalance in the distribution of temporary employment across age groups is even more pronounced than in the case of unemployment (Table 2.1). In 2006, temporary employment among young people (aged 15-29) exceeds temporary employment among adult men (aged 45-

[10] Between comparison between 1996 and 2006 is unaffected by changes in country composition, while we lack data for certain countries before 1995.

54) roughly by a factor of 7. Compared to adult men, average temporary employment among young people is 22 percentage point higher among young women and 19 percentage points higher among young men. Temporary employment is also slightly more common among young women compared to young men.

Table 2.1 Temporary employment in 2006, % in temporary employment in labor force, by age and gender

	Men, ages 45-54	Men, Ages 15-29	Women, Ages 15-29	(2) - (1)	(3) - (1)
Austria	2.04	25.41	21.24	23.37	19.20
Belgium	2.01	15.48	18.46	13.46	16.45
Denmark	4.23	21.93	21.69	17.70	17.46
Finland	3.73	25.51	36.77	21.78	33.04
France	3.67	27.07	27.98	23.40	24.31
Germany	2.59	34.76	33.15	32.17	30.56
Greece	2.95	10.93	12.69	7.98	9.74
Ireland	1.06	5.96	9.60	4.90	8.54
Italy	3.86	19.22	22.83	15.36	18.97
Netherlands	3.45	29.48	30.12	26.03	26.67
Norway	1.96	19.85	27.73	17.89	25.78
Portugal	6.88	31.68	34.51	24.80	27.63
Spain	11.19	42.16	42.41	30.97	31.22
Sweden	5.04	28.87	40.16	23.83	35.12
UK	2.12	7.53	9.08	5.40	6.96
Average	3.79	23.06	25.90	19.27	22.11

Source: EULFS, own calculations

In 2006, temporary employment is now highest (in absolute terms and relative to adult men) in Spain, Sweden and Finland. The majority of Finish, Spanish and Swedish youth who hold a temporary contract, do so involuntarily, and around 80% of these contracts (70% in Finland in 2006) are shorter than twelve months (Table 2.3 in Appendix to this chapter). We also note relatively high temporary employment in Germany and the Netherlands, which is to a certain extent accounted for by the extensive apprenticeship system that relies on temporary apprenticeship contracts. In the Swedish and Finnish case, active labor market policies account for a certain share of temporary contracts.

In Figure 2.4, we compare the evolution of temporary employment and unemployment on the youth labor market across Western Europe. In Austria, Germany, the Netherlands, Portugal, Belgium, France, and Sweden, we observe pronounced rises in temporary employment that exceed corresponding decreases in unemployment. In Austria and Germany, we observe simultaneous increases in youth unemployment and temporary employment. The Anglo-Saxon countries as well as Denmark, Norway and Greece show a divergent pattern of stable or declining youth unemployment and no (persistent) growth of temporary employment.

That growing temporary employment does not generally translate into lower unemployment becomes more apparent, when we sum these two rates. In Figure 2.5, we chart the evolution of job insecurity, which we define as the percentage of individuals in the active labor force that are either unemployed or employed on a temporary basis. We observe persistent growth of job insecurity among young people in the majority of European countries. Exceptions are the Anglo-Saxon countries, where we observe no trends or even recoveries, driven by declining youth unemployment rates and absent growth in temporary employment. Greece, Denmark, Finland[11] and Norway also show no clear trend in the period observed. Figure 2.5 also shows that young people have been more affected by the increase in job insecurity compared to adult men. Compared to the magnitude of change on the youth labor market, adult men were barely affected.

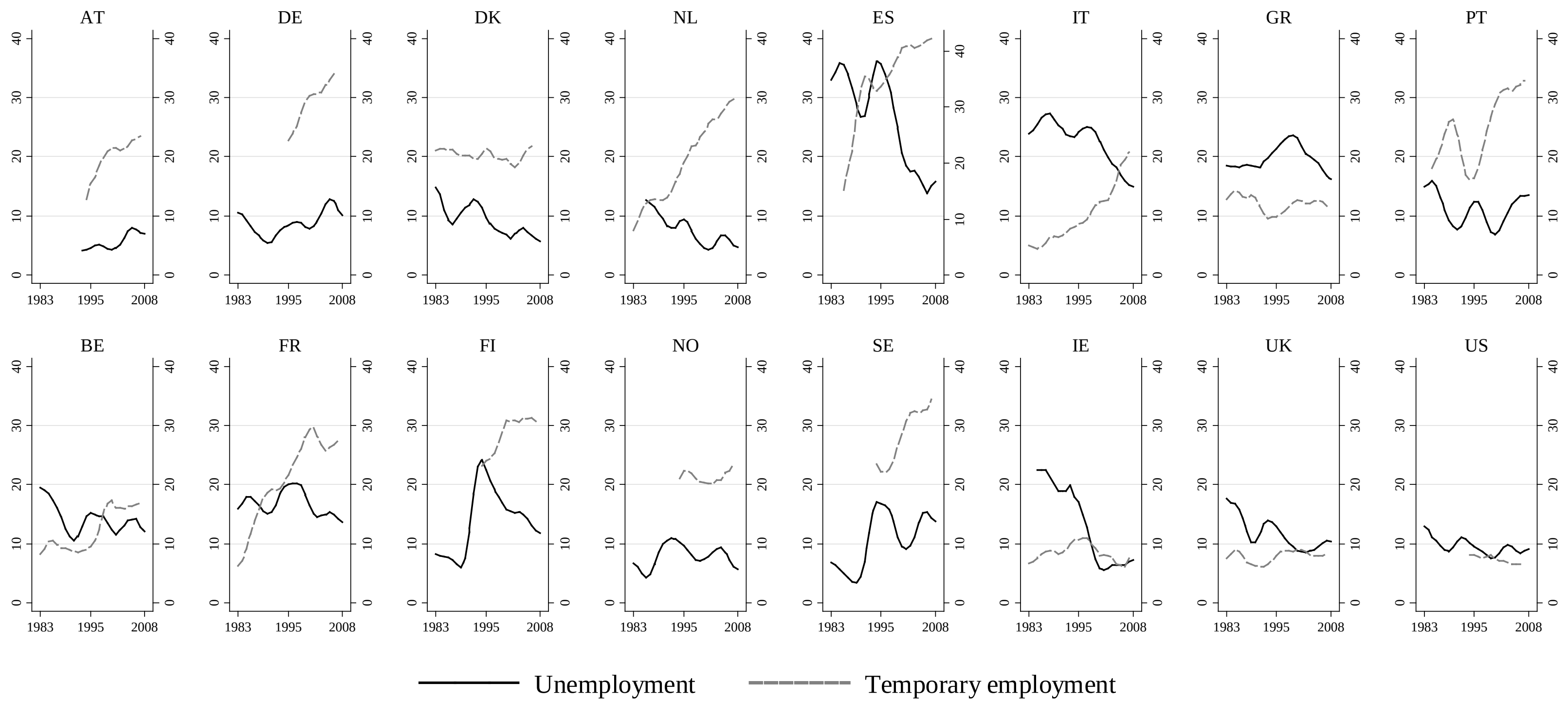
In Figure 2.6, we add another dimension of youth labor market outcomes, distinguishing between part-time and full-time jobs. Labor market precarity is defined here as being either unemployed or in part-time/temporary employment. Conversely, we define job security as being

[11] If we would include data from before 1995, which are unfortunately not available in the EULFS, we would, however, also observe an increase in job insecurity, triggered by the economic and financial crisis in the early 1990s.

in a full-time, permanent job. We express the number of individuals in each state as a percentage of the active labor force, so that both add up to unity. As a reference, we also plot labor force participation rates. Despite rising levels of educational attainment and declining cohort sizes, young people are less likely to hold full-time, permanent jobs and more likely to be found in a precarious labor market position in the majority of European countries. Much of these trends have persisted throughout the 1990s, during a period of declining youth unemployment. Even in the liberal UK, we see a growth in precarious labor market outcomes, while Ireland shows a strong performance mainly due to economic boom during the 1990s.

In Figure 2.7, we focus on the same outcomes as in Figure 2.6, but we subtract the outcomes for men aged 45-54 from the corresponding youth outcomes. The resulting trend lines indicate to what extent both groups were equally affected by rising job precarity. If both were equally affected, the resulting trend lines would be flat around zero. What we observe, however, is that young people were disproportionately affected by rising precarity and declining job security. The decline in relative job security is nearly universal, as is the growth in relative job precarity. These underlying trends continued unabatedly in a number of countries in the late 1990s and early 2000s, which suggests an ongoing trend that the current economic and financial crisis may even exacerbate. The most dismal trends in the 1990s and early 2000s are visible in Germany, Italy, Portugal, France and Belgium, where youth labor force participation has been declining faster than among adult men. Even though part of these declines are due to increased educational participation, young people in these countries seem to be unable to convert their educational titles into secure jobs. We now also observe growing age-gaps in Greece, which we did not observe focusing on absolute outcomes.

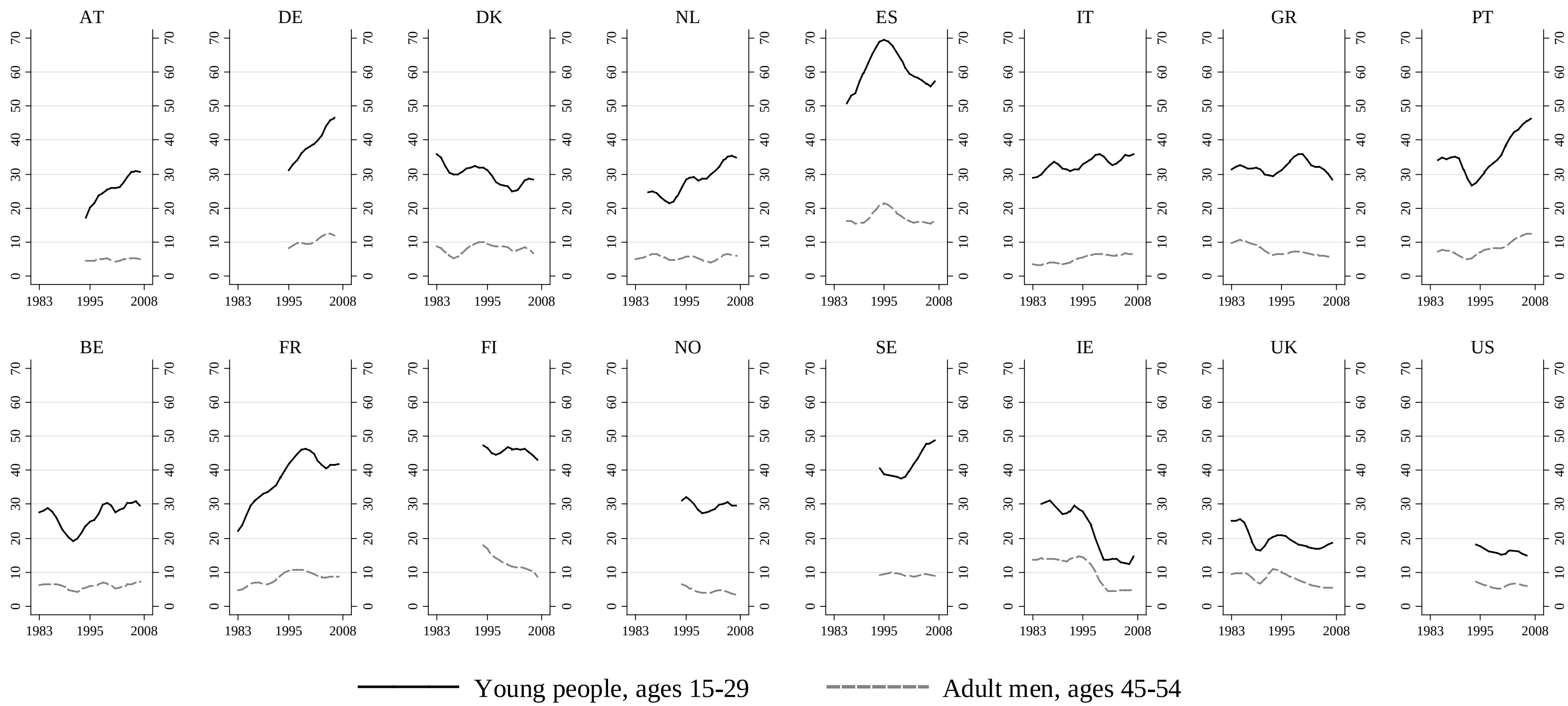
Figure 2.4 Unemployment (% unemployed in labor force) and temporary employment (% in temporary employment in labor force), young people aged 15-29



Note: Data for U.S. on 15-24 year-olds

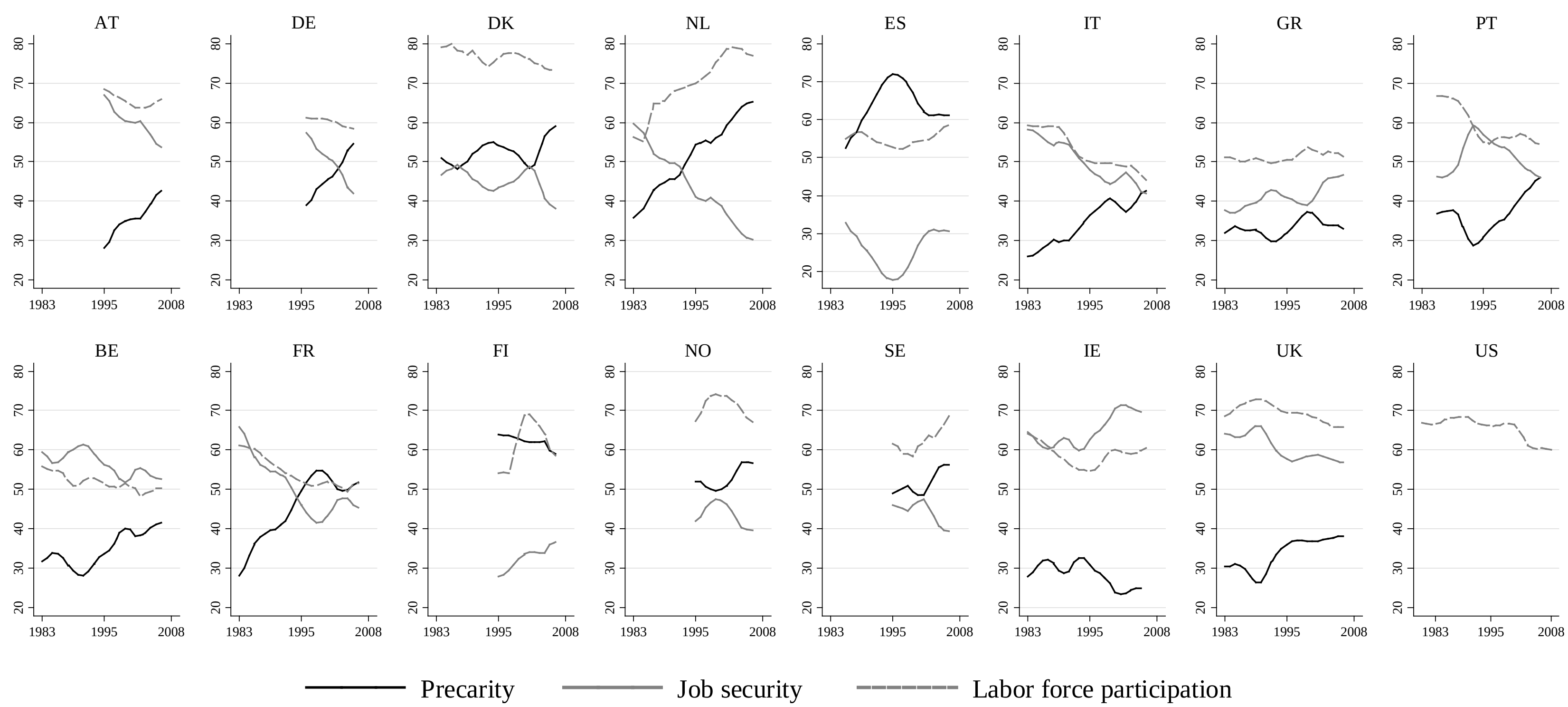
Source: OECD Labor Force Statistics for unemployment and U.S. data for temporary employment; EULFS, own calculations for temporary employment

Figure 2.5 Job insecurity (% unemployed or in temporary employment in labor force), young people aged 15-29 and adult men aged 45-54



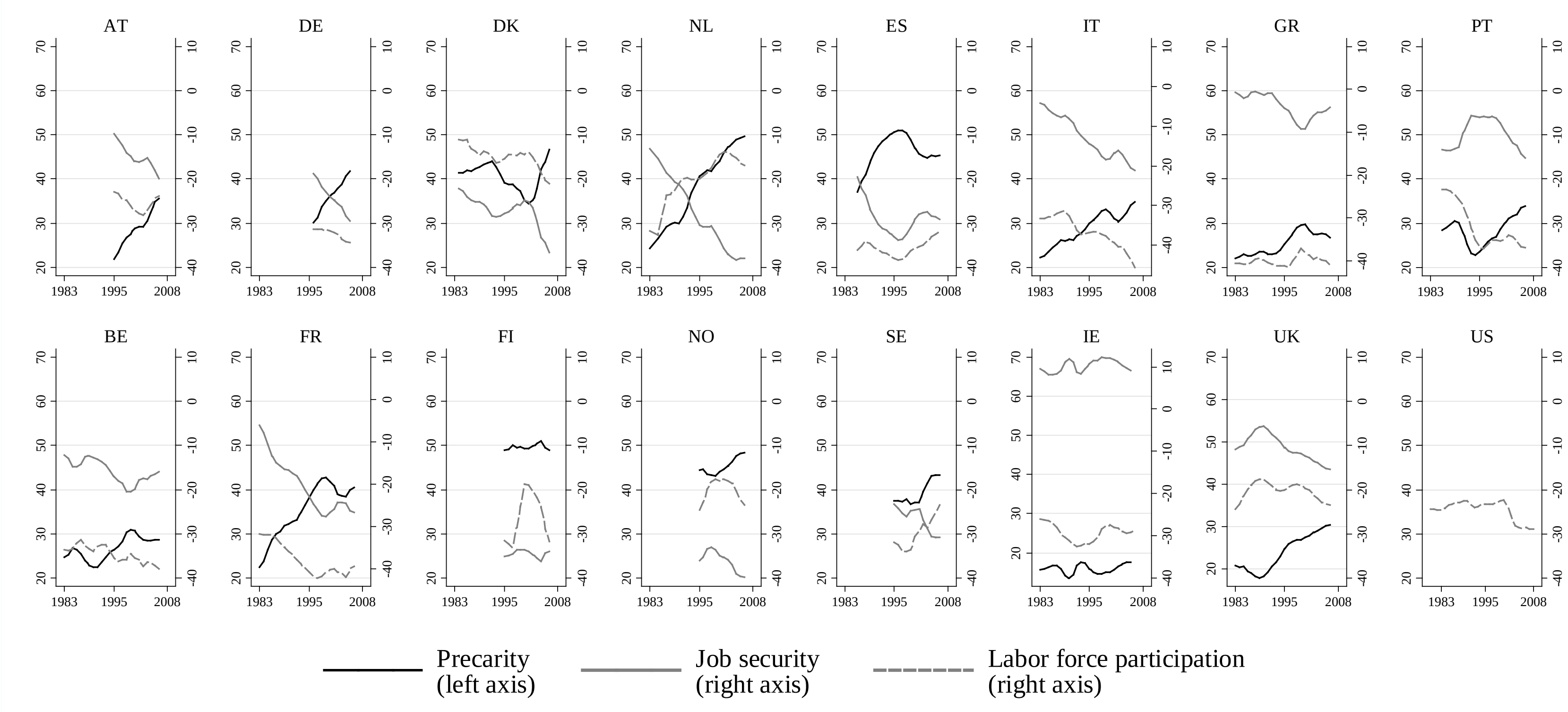
Note: Data for U.S. on 15-24 year-olds (young people) and 25-54 year-olds (adult men)
Source: EULFS, own calculations; U.S. data from OECD Labor Force Statistics

Figure 2.6 Precarity (% in labor force unemployed, in temporary and/or part-time employment), job security (% in labor force in permanent, full-time employment) and labor force participation rates (% in labor force in population), young people aged 15-29



Note: Data for U.S. on 15-24 year-olds (young people)
Source: EULFS, own calculations. OECD for U.S. data.

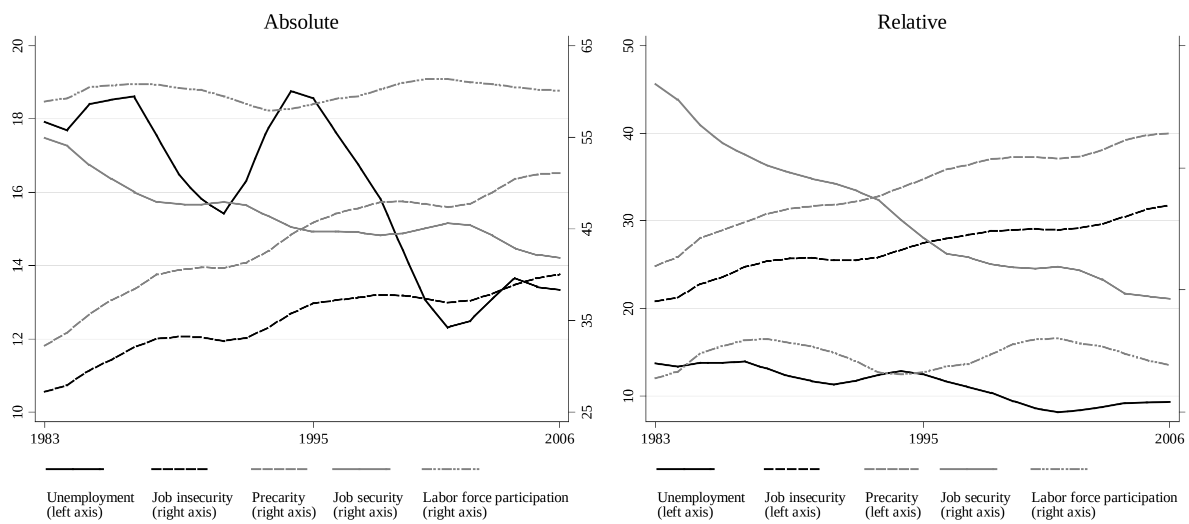
Figure 2.7 Relative precarity (% in labor force unemployed, in temporary and/or part-time employment), relative job security (% in labor force in permanent, full-time employment) and relative labor force participation rates (% in labor force in population), young people aged 15-29



Note: Outcomes for young people (ages 15-29) minus outcome for adult men (ages 45-54); data for U.S. on 15-24 year-olds (young people) and 25-54 year-olds (adult men)
Source: EULFS, own calculations. OECD for U.S. data.

To summarize these trends, we averaged the data depicted in Figure 2.6 and Figure 2.7 for the "regulated" Continental and Southern European as well as Scandinavian countries, omitting the liberal countries (U.S., UK and Ireland).[12] In a period of declining relative and absolute youth unemployment, we observe a decline of secure employment, a growth of insecure jobs and precarious labor market positions among young people. The decline in full-time permanent employment relative to adult men is particularly salient.

Figure 2.8 Absolute and relative trends in precarity (% in labor force unemployed, in temporary and/or part-time employment), job security (% in labor force in permanent, full-time employment) and labor force participation rates (% in labor force in population), young people aged 15-29



Note: Unweighted averages for Austria, Belgium, Denmark, Finland, France, Germany, Greece, Italy, Netherlands, Norway, Portugal, Spain, Sweden; Relative outcomes are the outcomes for young people (ages 15-29) minus outcomes for adult men (ages 45-54)
Source: EULFS, own calculations

Between 1983 and 2006, the decline in youth unemployment was outpaced by the growth of temporary employment, as job insecurity among young people grew on average by 0.55

[12] We have to exercise some caution in interpretation because a number of countries lack information before 1995.

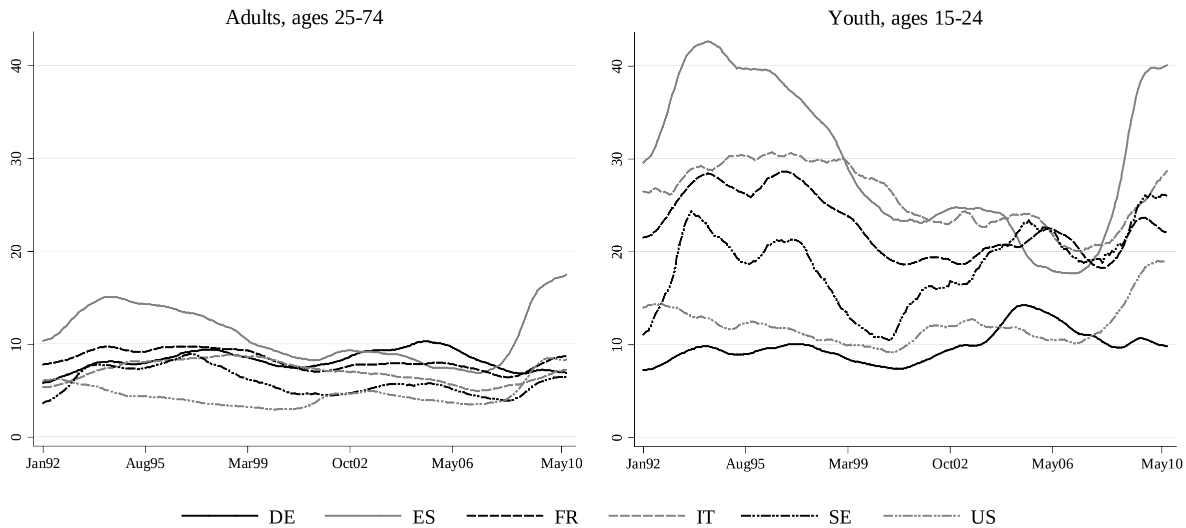
percentage points each year, while precarious labor market positions grew on average by 0.82 percentage points. Relative to adult men, full-time, permanent employment was declining at a rate of 0.76 percentage points. Further analysis (data not shown), for the period from 1995 to 2006 show that declines in job security were mainly concentrated among the least educated, while tertiary educated held or improved their position. The late 1990s and early 2000s, despite an overall favorable macroeconomic environment in a number of countries, were therefore still marked by persistent growth in labor market inequalities.

2.4. The impact of the recent financial and economic crisis

Finally, we want to briefly survey the impact of the recent financial and economic crisis on the youth labor market. We rely on monthly unemployment rate data from Eurostat and resort to conventional definitions, where youth are individuals aged 15-24 and adults are aged 25-74. Figure 2.9 traces the evolution from the early 1990s, just before the second peak of European youth unemployment to May 2010 for a selected set of European countries and the U.S. In Figure 2.10, we take the unweighted annual average over 15 Western European countries and the U.S. of the youth and the adult unemployment rate.

While recent shocks to economic output were more severe than the crises in the early 1990s, we may judge the unemployment response as temperate. Nevertheless, the financial crisis and recession has virtually wiped out all declines in youth unemployment that were won since youth unemployment last peaked in the early 1990s. We are nearly back to the highest youth unemployment levels seen in the past 40 years (see also Bell and Blanchflower, 2010). However, we also see that at present unemployment seems to have reached a peak, as signs of economic recovery appear in a number of countries. Again, we observe a much more pronounced response to crisis on the youth labor market. With the exception of Spain, adults seem little affected.

Figure 2.9 Unemployment January 1992 to May 2010, % unemployed in labor force unemployed, by age

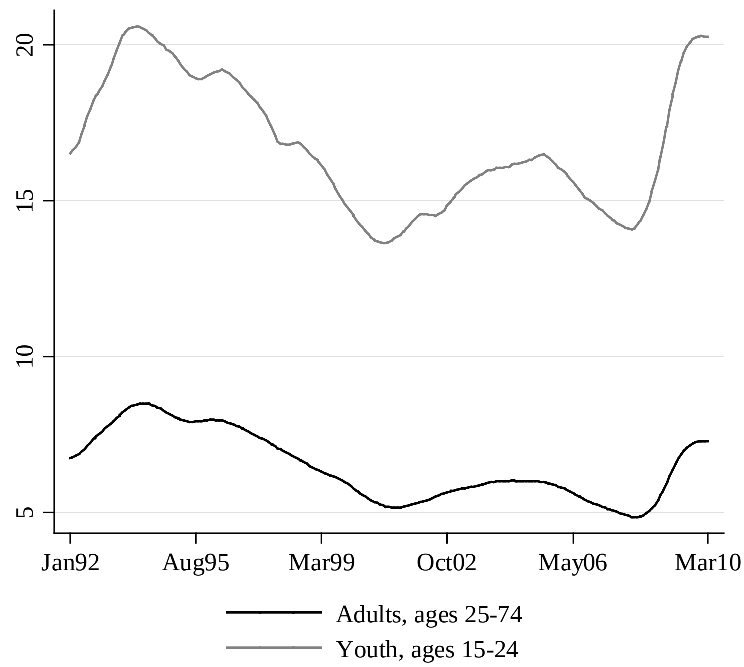


Source: EUROSTAT

We should bear in mind that in 2010 we have moved further along in terms of favorable supply side shifts, i.e. youth cohorts are still shrinking and becoming ever more educated. In the future, we are less and less able to rely on these supply shifts to mitigate the situation on the youth labor market. Moreover, we have already experienced a persistent erosion of job security on the youth labor market. In the aftermath of the unemployment crisis in the 1980s and early 1990s, job insecurity grew to unprecedented levels without a sign of trend reversal, and now we again face nearly record youth unemployment levels.

A crucial question will be whether governments can further rely on “passive stabilizers”, such as youth dropping out of the labor market to obtain further education, thereby temporarily taking the pressure from the youth labor market. Tertiary enrolments have risen persistently in a number of European countries throughout the 1990s. Overinvesting into tertiary education carries immediate costs for individuals and tax-payers, and it brings down the returns on investment (Dolado et al., 2000). Another crucial issue concerns demands for fiscal austerity, which are spreading across Europe. A sudden decrease of state spending and interest rate increases are likely to weaken aggregate demand, which directly impacts youth labor market chances, and would prolong recovery.

Figure 2.10 Unemployment January 1992 to March 2010, Western countries, by age



Note: Unweighted averages for Austria, Belgium, Denmark, Finland, France, Germany, Greece, Italy, Ireland, Netherlands, Norway, Portugal, Spain, Sweden, United Kingdom, United States
Source: Eurostat

Unless Europeans have become a lot more capable in managing crisis on the youth labor market, we are bound to see the worst youth labor market conditions in recent history for the years to come. If the Swedish/Finnish response to crisis in the early 1990s is any guide (see Figure 3), we should expect to see further rises in temporary employment on European youth labor markets. As investors and employers lack confidence in macroeconomic recovery, hiring employees on a temporary basis becomes more attractive, and lacking better offers, young people have no other choice but to accept (Holmlund and Storrie, 2002).

2.5. Summary and Discussion

Before the onset of the recent financial crisis, youth unemployment across Western Europe has been on a path of recovery. However, the emergent structure of employment on the youth labor market is fundamentally different from the situation in early 1970s. Despite favorable trends on the supply side, young people are increasingly less likely to hold a full-time, permanent job, and more likely to be employed on a temporary or part-time basis. At risk of over-generalizing, the basic trends fits with a doubly modified version of the original Krugman hypothesis. Liberal Anglo-Saxon countries (esp. UK and the U.S.) have witnessed rising earnings inequality, where income gaps have grown both between education and experience groups (Gottschalk, 1997; Blanchflower and Freeman, 2000; Ryan, 2001). Wages have been somewhat more stable in Continental and Southern Europe, as well as Scandinavia. While we did not observe the predicted uniform rise in unemployment rates among the less educated, we have seen a clear decline in job security, which occurred partly among the less educated (Maurin and Postel-Vinay, 2005), but as we have shown here, we have seen substantial growth in job insecurity among young people, in absolute terms and relative to adult men. Thus, young people and less educated youth in particular have borne a disproportionate share of the burden of adjustment to structural change.

While strong age-differentials have been a well-known feature of Southern European labor markets, the data presented here also indicates signs of strain in some of the Continental and Northern corporatist countries. The Swedish youth labor market seems to have never quite recovered from the crisis in the early 1990s. The 1990s and early 2000s, were also marked by a simultaneous rise in unemployment and temporary employment in Germany and Austria. We thus observe some tendencies towards dualization in these corporatist countries. Shifting labor market risks onto young people appears to have been a solution that both left and right governments in these countries could agree too, as it meets demands from the right for more flexibility, while not cutting into the rents of what left parties seem to perceive as their politically relevant constituents.

The recent crisis has created an even more pressing need to address the labor market situation of young people, but will governments on strained budgets be able to manage the crisis? The still ruling economic consensus by default advocates deregulation/privatization as low cost solutions that supposedly pay for themselves in the long run. Given that financial crisis seems to have precipitated a crisis of confidence into market-based solutions to social problems, it will be interesting to see if and to what extent governments resist deregulatory pressures. In Spain, for example, the left government has recently implemented a reform that lowers dismissal costs on permanent employment contracts, while imposing tougher constraints on the usage of temporary contracts.[13] However, we should ask at this point, again, do these reforms really work for young people?

The record of deregulatory reform over the past decades is mixed at best: Before the recent financial crisis, youth unemployment rates had recovered in Western Europe, but the decline in unemployment has been outpaced by the growth in atypical jobs, which are limited in terms of duration, hours and benefits. It is not clear whether deregulation of employment protection has actually contributed to the recovery in unemployment. Youth labor markets in some European countries bordering the Mediterranean still appear dysfunctional, while the traditional corporatist top performers also show increasing signs of strain. Fueled by the recent crisis, the search for

[13] The Spanish parliament approved the reform bill on June 22, 2010.

viable answers to young people's labor market problems will certainly remain on the scientific agenda, but it is less certain whether policy makers are able or willing to deliver solutions. While we have witnessed violent outbursts of youth discontent in France and Greece, the shift of job insecurity onto young people has not been mobilized in the political arena.

2.6. Appendix

Table 2.2 Unemployment rates by age and gender, 2008

	Men, ages 45-54	Men, Ages 15-29	Women, Ages 15-29	(2) - (1)	(3) - (1)
Australia	2.55	7.14	7.17	4.59	4.62
Austria	2.95	6.37	6.91	3.42	3.96
Belgium	4.75	10.48	10.13	5.73	5.38
Canada	5.03	10.40	8.21	5.37	3.18
Czech Rep.	3.07	5.99	6.96	2.93	3.89
Denmark	1.39	4.76	6.46	3.36	5.06
Finland	4.14	11.21	12.25	7.06	8.11
France	4.84	13.06	13.55	8.22	8.71
Germany	6.64	9.86	9.07	3.22	2.42
Greece	2.35	11.48	20.69	9.13	18.34
Hungary	6.33	12.39	13.55	6.06	7.22
Ireland	4.41	10.05	5.76	5.65	1.36
Italy	3.09	13.51	17.70	10.42	14.61
Japan	3.05	6.98	6.07	3.93	3.01
Mexico	2.06	5.43	7.24	3.37	5.19
New Zealand	2.05	8.82	8.54	6.76	6.48
Netherlands	2.19	4.34	4.44	2.16	2.25
Norway	1.57	6.42	5.31	4.85	3.74
Poland	5.16	10.66	13.54	5.50	8.38
Portugal	5.25	10.34	16.50	5.09	11.25
Slovakia	7.31	13.05	15.54	5.75	8.23
South Korea	2.22	8.63	5.85	6.41	3.63
Spain	7.23	18.02	18.88	10.80	11.65
Sweden	3.70	13.31	14.36	9.61	10.66
Switzerland	2.06	5.38	5.99	3.32	3.93
Turkey	8.08	16.31	18.64	8.24	10.56
UK	3.51	11.60	8.95	8.09	5.44
USA	4.24	11.03	8.93	6.79	4.69
Average	*3.97*	*9.89*	*10.61*	*5.92*	*6.64*

Source: OECD Labor Force Statistics

Table 2.3 Characteristics of atypical jobs, young people aged 15-29

	% of temporary employment that is involuntary		% of temporary contracts shorter than 12 months		% of temporary contracts longer than 12 months		% of part-term employment that is involuntary		
	2000	2006	2000	2006	2000	2006	1983	1995	2006
Austria	7.35	6.72	22.05	30.04	72.40	69.96		11.73	15.08
Belgium	74.79	79.37	68.85	79.60	31.15	20.40	50.94	50.06	33.05
Denmark	35.51	42.81	54.53	44.44	43.87	50.65	14.68	17.04	14.23
Finland	41.13	50.16	53.69	69.18	25.41	22.56		37.66	18.80
France		52.11		71.08		28.92		58.86	39.52
Germany	3.57	7.41	22.54	31.02	73.08	67.38		10.65	21.30
Greece	67.21	63.58	68.76	62.67	31.24	37.33	35.10	53.80	52.72
Ireland	31.11	15.80	63.84	25.33	26.14	72.85	47.97	45.26	13.91
Italy		42.48		58.36		33.25	46.21	50.55	51.74
Netherlands	39.30		76.17		17.43			13.09	5.36
Portugal	39.56	78.27	82.73	52.58	17.27	47.42	43.31	33.47	50.87
Spain	70.15	58.28	89.46		10.54		46.85	26.55	36.62
Sweden	51.32	50.50	78.99	41.60	17.75	44.60		37.06	43.34
UK	28.47	24.49	36.88	29.47	21.26	15.23	17.43	16.76	12.41
Average	*40.79*	*44.00*	*59.87*	*49.61*	*32.30*	*42.55*	*37.81*	*33.04*	*29.21*

Note: Involuntary temporary (part-time) employed are those claiming to hold a temporary (part-time) job, because they could not find a permanent (full-time) job. Figures regarding part-time employment for the UK unreliable because of missing data
Source: EULFS, own calculations

3

Employment Protection and Job Insecurity

In this chapter, we survey key theoretical arguments about the relationship between employment protection legislation and labor market outcomes, in particular unemployment and temporary employment, two key dimensions of job insecurity. While earlier research has linked employment protection legislation to the unemployment crisis on European labor markets (OECD, 1994; Siebert, 1997), deriving unambiguous predictions from theory and detecting robust effects in empirical research has been difficult. Scientists have responded by shifting the focus of the critique from aggregate to distributional consequences of employment protection (Esping-Andersen, 2000; OECD, 2004, 2006; Breen, 2005; Bertola et al. 2007; Kahn, 2007; Cahuc and Zylberberg, 2007). Young people entering the labor market, women returning from child care breaks, individuals who lost their jobs, or migrants, may suffer if their employment prospects are restricted by job security provisions, while incumbent workers, adult men in particular, are shielded from job loss.

While job security provisions on regular (permanent) employment contracts have been the target of criticism, actual reforms instead often reduced barriers to the use of temporary contracts as an alternative way to grant employers more flexibility. We have seen in Chapter 2 that temporary employment has grown across European countries and in particular among young people. A number of authors have drawn a causal connection between strict dismissal regulations on regular contracts and the incidence of temporary employment (Polavieja, 2006; Kahn, 2007). Strict regulations on dismissing permanent employees may induce employers to substitute permanent with temporary work to obtain greater flexibility. They may also raise the bargaining power of incumbent employees, which facilitates their extraction of rents, including job security: In particular, incumbent employees have an incentive to raise the temporary workforce, to a certain extent, to act as a buffer to market shocks.

By now, the idea that employment protection is both behind high unemployment and temporary employment among young workers represents a consensus spanning disciplines, despite occasional dissent (e.g. Nickell, 1997). It is based on the notion that advanced economies require flexibility to survive against global competition and accommodate new technologies and that employment protection fundamentally interferes with this process. Implicitly or explicitly, it advocates deregulation of employment protection, a shift of economic risks from employers (back) to workers. In this chapter, we want to analyze the theoretical arguments linking employment protection to the distribution of unemployment and temporary employment. Our theoretical survey suggests that strict job security provisions and regulations on using temporary contracts should be linked to the incidence of temporary employment among young people. However, contrary to the consensus, the effect of both types of regulation on youth unemployment is indeterminate. The commonly accepted view that the effect of employment protection on aggregate unemployment is indeterminate should also hold for the youth unemployment rate.

3.1. Employment protection: origins and overview

At the level of individual actors, job security provisions impose costs on employers when dismissing workers, usually in the form of severance payments and/or procedural inconveniencies.[14] Firing costs are normally imposed on regular (permanent) employment contracts, while temporary contracts upon expiration do not impose dismissal costs on employers. In a standard matching theoretic framework (Cahuc and Zylberberg, 2007), firing costs lower the productivity threshold until which the productivity of an employer-employee match may decline for the employee to be dismissed. The cause for dismissal is usually modeled as a possibly temporary decline in the productivity of a match. Productivity shocks may result from worker behavior or external factors, such as a decline in demand on product markets, and cannot be fully foreseen by employers. If a productivity shock occurs, employers are forced to trade off the loss resulting from that shock against the firing costs resulting from dismissal. Productivity shocks only lead to dismissal if the loss due to the productivity shock exceeds the firing costs. Thereby, firing costs constrain employers in their flexibility to adapt if a match is hit by a productivity shock and forces them to keep unproductive workers in employment (Blanchard and Katz, 1997). Lower firing rates in turn imply a lower need for new hiring. By reducing both (un)employment inflow and outflow, firing costs smooth employment over the

[14] Employers may be able to "contract around" firing costs and transfer all costs through wage cuts to employees, if employees accept such wage cuts in return for protection against job loss (Lazear, 1990; Autor et al., 2006). While this may be more plausible in the U.S. case, we assume in keeping with much of the literature on European labor markets that firing costs represent sunk costs to employers which cannot be fully transferred to employees (for example, Blanchard and Landier, 2002).

business cycle, as employers fire fewer workers during downturns and hire fewer workers during upturns (Bertola, 1999).

Employment protection legislation can be interpreted as a concession by employers to workers demands for job security (see Saint-Paul, 2000, for a discussion). It requires employers to give up flexibility in the short run and forces them to internalize some of the costs of dismissal, while workers obtain some protection against market fluctuations. This insulation against market forces at the same time strengthens incumbent workers bargaining power, which they may in turn use to obtain wage increases in excess of productivity increases or to shift layoff risks to less protected groups, like low seniority, part-time or temporary workers (Blanchard and Katz, 1997, Bentolila and Dolado, 1994; Polavieja, 2003).

Employers are more likely to concede employment protection to employees if incumbent workers enjoy high bargaining power. Many of the current job security rights have been won in times of high economic growth, where flexibility was less of a concern for employers (Blanchard 2006) and tight labor markets increased worker bargaining power. Strong or militant labor movements have also been a key factor in the extension of employment protection legislation (Berger and Piore, 1980), as have been left wing parties seeking to protect the rights of incumbent workers (Botero et al. 2004; Rueda, 2005). Botero et al. (2004) also point to the role of legal tradition, contrasting civil vs. common law origins, in explaining the emergence of statutory employment protection legislation in some countries. Algan and Cahuc (2006) develop a cultural explanation, linking religion and traditional family values to job security provisions that protect male breadwinners, for example in Southern European countries.

With the end the postwar boom and the profound macroeconomic and institutional changes that followed, past bargains over workers status rights (Streeck, 1986) or employment rents (Sorensen, 2002) have become renegotiated. A consensus has emerged stating that after the peak of industrial capitalism and with the end of postwar super-growth, flexibility has become an economic imperative. Economies need to be flexible to constantly adjust to an environment that has become permanently more turbulent. They need to constantly adapt in order to incorporate technological innovations, react to sometimes unforeseen changes in the market and survive against global competition (Blanchard, 2006, Blanchard and Katz, 1997). On this presumption, different authors have argued that job security provisions violate the objective of flexibility, because they constrain employers "too much" in their ability to hire and fire their employees at will. Employers are forced to keep low productivity workers, which lowers average productivity. Employment protection also lowers turnover to unhealthy, sclerotic levels, delaying necessary adjustments to a constantly changing environment, which further constricts productivity growth. European employment protection regimes therefore have to become more flexible to speed up the reallocation of labor from declining to expanding employers and industries, which should lead to economic growth and stimulate demand for labor. Job security provisions are therefore to blame for weak job creation and high and persistent unemployment (OECD 1994; Siebert 1997, Blanchard and Katz, 1997; Pontusson, 2005).

While the idea of flexibility (and the related concept of 'flexicurity') and the critique of employment protection have become extremely influential in public policy over the past decades, their scientific basis always has been and still remains shaky, both theoretically and empirically. First, early theoretical work has shown that the effect of employment protection and unemployment is indeterminate (Lazear, 1990; Bertola, 1990), and by now different authors agree that the accumulated empirical evidence suggests that there is no association between employment protection and aggregate unemployment or employment rates (Cahuc and Zylberberg, 2007; Howell et al., 2007; Baccaro and Rei, 2007). Also the empirical link between employment protection and job turnover appears fragile. Rates of job creation and job destruction appear remarkably similar across countries (Bertola and Rogerson 1997; Cahuc and Zylberberg, 2007) irrespective of the extent of employment protection. Third, the relationship between employment protection and productivity appears to be more complex, since employment protection also provides incentives for employers and individuals alike to invest into specific skills (Estevez-Abe et al., 2001).

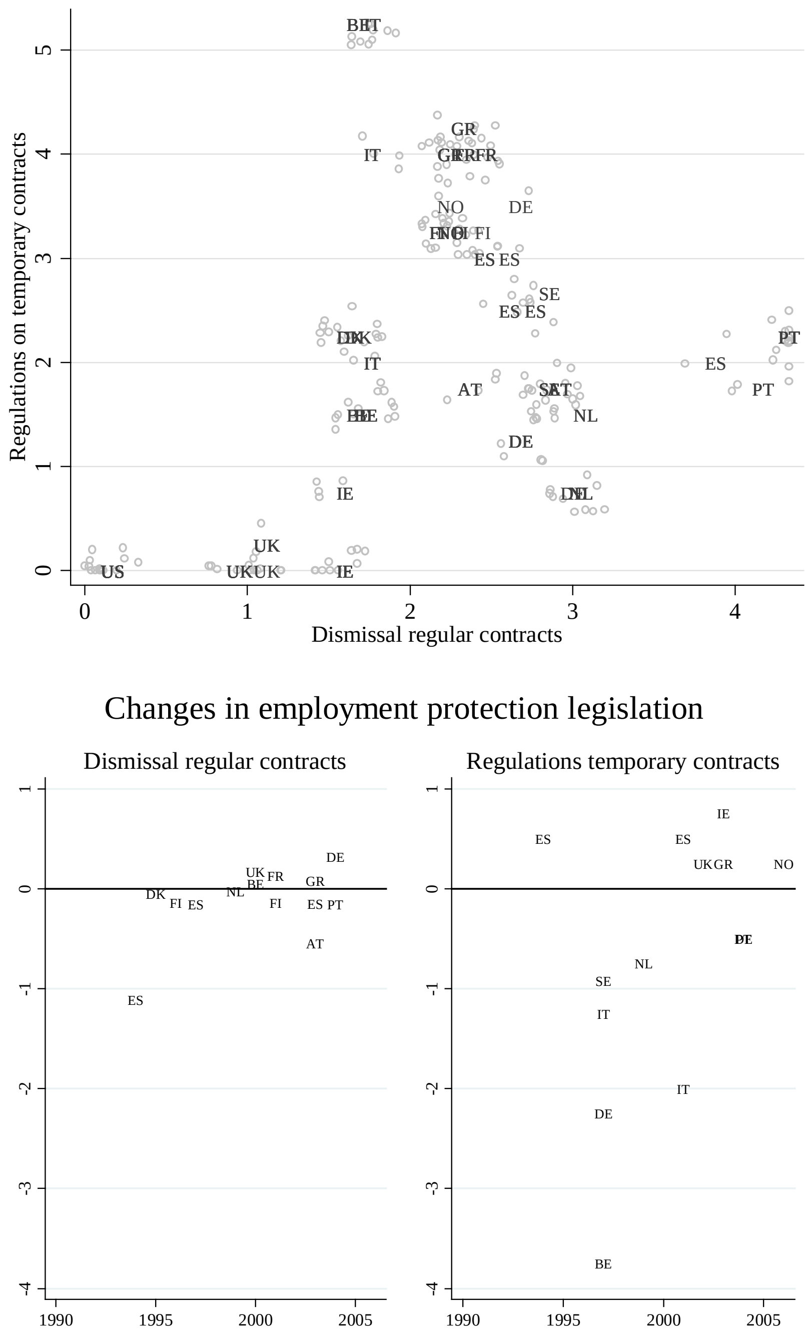
Conceding that the effect of employment protection on aggregate labor market outcomes is more complex and that robust evidence of its effects on aggregate outcomes are hard to come by, the critique of employment protection has shifted its focus. A new consensus appears to have emerged arguing that employment protection does not affect the level, but the distribution of aggregate labor market outcomes (Esping-Andersen, 2000; OECD 2004; Breen, 2005; Addison and Texeira, 2003; Cahuc and Zylberberg, 2007). By protecting tenured insiders against dismissal and constraining job opportunities for those entering the labor market, employment protection legislation may systematically work against certain demographic groups less strongly attached to the labor force and more likely to be unemployed, in particular women, young people, less educated and migrants.

Although much of the critique of employment protection focused on dismissal regulations on regular contracts, reforms often left these provisions unaltered. Instead, flexibility has been obtained by introducing or deregulating the usage of temporary contracts (OECD, 2004), which have become significantly more common in a number of European countries, in particular among young people. In contrast to permanent employment contracts, temporary contracts by definition end after a pre-specified period, and impose no firing costs on employers upon expiration. A recent concern has been that this form of partial deregulation may have in fact made things worse by providing employers with incentives to destroy permanent and replace them with temporary jobs, increasing turnover and potentially increasing unemployment (Blanchard and Landier, 2002; Cahuc and Postel-Vinay, 2002).

Firing costs, which we also refer to as dismissal regulations or job security provisions on regular contracts as well as regulations on the usage of temporary contracts are two sides of employment protection in European countries that we need to consider separately. We will first discuss the impact of firing cost and then the effect of regulation on temporary contracts on both absolute and relative youth labor market outcomes. A key point we wish to make is that a both types of regulation affect absolute and relative outcomes through different channels, making their overall impact on either absolute or relative outcomes ambiguous. At last, we will consider potential interactions or complementarities between both types of regulations.

Figure 3.1 gives an overview of employment protection and its reforms, using the corresponding OECD indicators of employment protection legislation (see Venn, 2009). Both indicators vary on a scale from 0 to 6. The OECD indicator on job security provisions aggregates information on firing costs accruing to employers when dismissing an employee on a regular employment contracts. These include procedural inconveniencies, advance notice periods, severance payments and other factors affecting the difficulty and potential costs of dismissal. The indicator for regulations on the usage of temporary contracts aggregates information on the economic context, in which temporary contracts are permitted, as well as the maximum cumulated duration and the number of successive contracts an individual can have with the same employer (see also Section 4.1.3, p. 81f. and Section 4.7.1, p. 106f.). Other dimensions of employment protection include regulations on temporary agency work and collective dismissals, which we do not consider in this study.

Figure 3.1 Employment protection regimes in Western countries, 1992-2006



Source: see Data Appendix in Chapter 4, Section 4.6 (p. 106f.)

We have restricted the data to 15 Western European countries and the U.S. for the years 1992 to 2006, which will be the focus of the multivariate analysis. In the upper panel, we observe that both indicators vary quite independently from each other, which analyses aggregating both dimensions would miss. The right panel graphs the 28 individual reforms, 14 on each dimension, falling within our window of observation. Were we to extend the window of observation to the 1980s, we would still see that the bulk of reforms occurred in the second half of the 1990s, which was a period of recovery, at least in terms of declining unemployment, in many European countries.[15] We also observe a number of up-regulations on both indicators, but the average change across countries is downwards (only barely so, in case of job security provisions on regular contracts). We observe larger reform steps in case of deregulations of temporary contracts compared to job security provisions for regular employees.[16] In absolute terms, the median reform of job security provisions was a meager 0.15 movement on a scale from 0 to 6. This is important for the empirical analysis, where we usually interpret unit effects of reforms, but we have to bear in mind that actual reforms were of much smaller magnitude. For regulations on temporary contracts, the median reform was a 0.63 unit movement on the corresponding indicator.

At last, we briefly want to consider the correlates of employment protection reform. On the one hand, this analysis provides an insight into the institutional and structural factors that are associated with employment protection reform. On the other hand, accounting for the factors that predict reform is a crucial step for ruling out endogeneities when estimating the effects of

[15] It is maybe no coincidence that the bulk of reforms is observed post-1994, when the Organization of Economic Cooperation and Development (OECD) issued an influential call for deregulation across advanced economies to reign in labor market crisis (OECD, 1994).

[16] We observe a number of outlying observations, which will become crucial for the multivariate analysis (Spain for regulations on permanent contracts; Belgium, Germany and Italy for regulations on temporary contracts).

reforms on labor market outcomes (see Section 4.3, for more discussion). Table 3.1 reports the results from fixed effects analysis using two samples that differ in countries and years covered.[17] Starting with job security provisions on regular contracts (EPR), we see that left parties when in government appear to protect incumbent workers, as suggested by Botero et al. (2004) and Rueda (2005). This effect is only present in the restricted sample. We find no government partisanship effect on the regulation of temporary contracts. Higher levels of economic globalization also seem associated with less strict job security provisions.

Regarding regulations on temporary contracts, we observe that stricter job security provisions on regular contracts, generous unemployment benefits, and intermediate/sectoral level wage bargaining institutions are associated with lower regulations on temporary contracts. Moreover, product market regulations are influential factors as well. The analysis thus suggests that globalization indeed seems to erode, while left parties in government protect the position of incumbent workers. There is no partisanship effect on the regulation of temporary contracts, since presumably both left and right governments can agree to this type of deregulation: Right parties obtain more flexible legislation, while left parties consent to ostensibly sensible economic policy while protecting their core constituents at the same time. Maintaining high levels of unemployment benefits (for which particularly workers with a longer job history qualify) and insider protection, while deregulating temporary contracts, can be interpreted as an attempt to redistribute labor market risks towards specific demographic groups perceived to be politically less relevant.

[17] The restricted dataset corresponds to the data used to estimate the causal effects of employment protection reforms using differences-in-differences.

Table 3.1 OLS fixed effects regression estimates ("cluster robust" standard errors) of the determinants of employment protection legislation

	21 OECD countries, 1985-2007				16 Western countries, 1992-2006			
	EPR	EPR	RTC	RTC	EPR	EPR	RTC	RTC
RTC		-0.08				-0.07		
		(0.05)				(0.05)		
EPR				-0.86**				-1.18***
				(0.38)				(0.38)
Left	0.02	0.02	-0.03	-0.01	0.03*	0.03**	-0.02	0.02
	(0.02)	(0.02)	(0.05)	(0.04)	(0.02)	(0.01)	(0.04)	(0.03)
GenRR	-0.00	-0.00	-0.04*	-0.04**	-0.00	-0.00	-0.04*	-0.05*
	(0.01)	(0.00)	(0.02)	(0.02)	(0.00)	(0.00)	(0.03)	(0.02)
TradeU	-0.02	-0.02	-0.02	-0.04	-0.00	-0.00	-0.02	-0.02
	(0.01)	(0.01)	(0.03)	(0.03)	(0.01)	(0.01)	(0.05)	(0.05)
Wage1	-0.03	-0.04	-0.04	-0.07	-0.08	-0.08*	-0.00	-0.10
	(0.04)	(0.04)	(0.08)	(0.07)	(0.05)	(0.05)	(0.16)	(0.12)
Wage2	0.01	0.00	-0.10	-0.09	-0.01	-0.04	-0.41**	-0.43**
	(0.05)	(0.05)	(0.10)	(0.09)	(0.03)	(0.04)	(0.19)	(0.18)
ALMP	0.02	0.03	0.08	0.10	-0.09	-0.12	-0.52	-0.63
	(0.11)	(0.11)	(0.23)	(0.23)	(0.11)	(0.11)	(0.49)	(0.48)
PMR	-0.00	0.04	0.56**	0.55**	-0.01	0.02	0.56*	0.54*
	(0.03)	(0.05)	(0.23)	(0.22)	(0.04)	(0.06)	(0.30)	(0.30)
Glob	-0.02***	-0.01***	0.03	0.01	-0.01*	-0.01	0.05	0.04
	(0.01)	(0.00)	(0.03)	(0.03)	(0.01)	(0.00)	(0.05)	(0.04)
TTS	0.00	0.00	-0.01	-0.00	0.00	0.00	-0.00	0.00
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)
CohS	0.00	0.00	0.01	0.01	-0.02	-0.01	0.01	-0.01
	(0.01)	(0.01)	(0.06)	(0.06)	(0.02)	(0.02)	(0.08)	(0.08)
OutG	-0.00	-0.00	0.00	0.00	0.02	0.02	0.06	0.08
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.05)	(0.05)
R-sq	0.29	0.34	0.28	0.33	0.25	0.31	0.35	0.40
N	448	448	448	448	212	212	212	212

Note: *** $p<0.01$, *** $p<0.05$, * $p<0.1$. EPR - Employment protection regular contracts, RTC - Regulations on the usage of temporary contracts. Main independent variables: indicators for the generosity of unemployment benefits (GenRR), trade union density (TradeU), two indicators for the coordination of wage bargaining (Wage1, Wage2), indicator for the cabinet power of left parties (Left), expenditure on active labor market policies (ALMP), product market regulations (PMR), economic globalization (Glob.), terms-of-trade shocks (TTS), output gap (OutG), the youth cohort size (CohS), country and year fixed effects.
Source: see Data Appendix in Chapter 4, Section 4.6 (p. 106f.)

3.2. Firing costs and unemployment

Economic theory has had difficulty deriving unambiguous effects of firing costs on aggregate unemployment rates. By reducing both the ins and outs of unemployment, they prolong employment and unemployment spells, but their effect on aggregate unemployment rates is ambiguous, at least without further assumptions (see Ljungqvist, 2002, for a discussion of the different modeling approaches in economics and their underlying assumptions). While unambiguous effects on aggregate unemployment rates are difficult to derive, a number of authors have concluded that the distributional effects are unambiguous (Esping-Andersen, 2000; Addison and Texeira, 2003; OECD 1999, 2004; Breen, 2005; Kahn, 2007; Bertola et al., 2007; Cahuc and Zylberberg, 2007): In particular, employment protection shifts unemployment risks from adult to young workers, raising unemployment rates among young workers in absolute terms and/or relative to adults. Drawing on Gangl (2006: p. 990), we distinguish two related theoretical perspectives to conceptualize the link between firing costs and age-inequalities in unemployment risks. First, firing costs affect the structure of the labor market, by lowering employee turnover and vacancy rates ("structural effects"). Second, firing costs may alter the behavior of labor market participants, in particular employers, in more subtle ways ("behavioral effects"). In the following section, we will show that within neither perspective can we deduce unambiguous effects of firing costs on the distribution of unemployment.

3.2.1. Structural effects

Starting with the structural perspective, by lowering turnover, firing costs limit the number of vacancies and therefore diminish job opportunities for labor market entrants. In particular, firing costs should lower the transition rate from unemployment to the first job for labor market entrants, and thereby raise youth unemployment (Breen, 2005). At the same time, employed, tenured workers are protected against job loss and face lower risks of dismissal. Firing costs should therefore shift the distribution of unemployment risks towards young people, and we should expect that unemployment rates decline with age, and this decline should be more pronounced in countries with high firing costs.

However, this prediction is difficult to reconcile with the countervailing effects of firing costs on both hiring and firing rates. For the aggregate labor market, many authors now accept that the joint effect of firing costs, via hiring and firing rates, on the aggregate unemployment rate is indeterminate (see discussion in Cahuc and Zylberberg, 2007: p. 748f.). If we consistently apply this result to the youth labor market, firing costs make it more difficult for young people to find a job, but once they find a job, they are also less likely to lose it. While the first effect raises youth unemployment, the second effect lowers youth unemployment, which is consistent with the available empirical evidence that has considered both effects (see Wolbers, 2007, for a direct test). [18] Unless we are willing to make further assumptions, the effect of firing costs on individual flows should be homogenous across age groups. Sometimes, unambiguous prediction have been obtained by disregarding the effect of firing costs on the outflow rate from first employment and only focusing on their effect on the transition rate into first employment (Russell and O'Connell, 2001; Breen, 2005). We maintain here that the theoretical result, which is largely accepted for the adult labor market, also holds for the youth labor market: Firing costs affect labor market dynamics, but not unemployment levels. Higher firing costs lower turnover on the youth just as on the aggregate labor market, and given their joint effect on hiring and firing rates, their overall

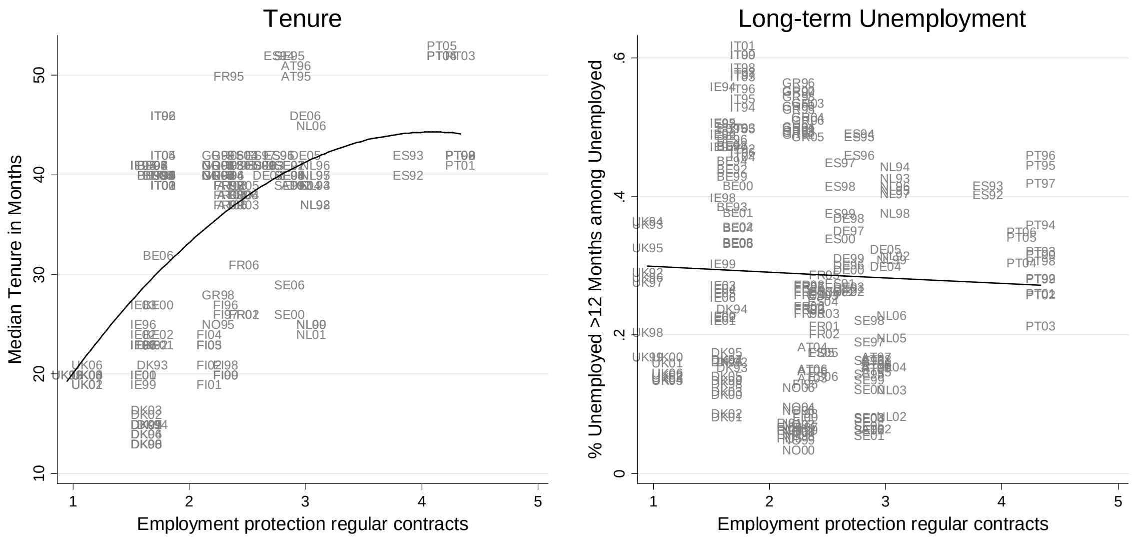
[18] Using longitudinal micro-data from 11 European countries, Wolbers (2007) shows that strict employment protection legislation lowers both the rate of the transition into the first job, but also the risk of becoming unemployed again, once a first job has been found. Comparing the U.S. to Portugal, Blanchard and Portugal (2001) show that unemployment duration is three times longer in Portugal, but worker flows into unemployment are also three times lower, leading to similar aggregate unemployment rates.

impact on the youth unemployment rate is indeterminate and can only be resolved by empirical analysis.

Figure 3.2 and Figure 3.3 illustrate this argument with some descriptive evidence. Figure 3.2 plots the relationship between job security provisions on regular contracts, which we take as a measure of firing costs, and labor market outcomes for young people aged 15-29 in 15 Western European countries. Data on employment protection is taken from the OECD, and the labor market outcomes are calculated from the microdata of the European Union Labor Force Survey (see Section 4.1.3, p. 81f., for more details). The left panel of Figure 3.2 graphs the median number of months that young people employed on a permanent contract have spent with their current employer. We clearly see a strong and positive relationship that grows weaker at very high levels of employment protection, which we take as an indication that high firing costs on permanent contracts indeed prolong employment spells in permanent contracts. This is partial support for the argument that turnover on the youth labor market is lower if firing costs are high.

Contrary to our expectations, we detect a weak negative association between firing costs and long-term youth unemployment (measured as the percentage of unemployed who have been unemployed for more than 12 months). We should be cautious, however, since in the LFS data, short unemployment spells are under-sampled. Moreover, young people may avoid prolonged job search by dropping out of the active labor force or taking temporary jobs instead of permanent jobs. Facing prolonged unemployment spells, they have the option to avoid prolonged job search, especially by entering higher education to update their skills and improve their job-market chances. Young people may also stay enrolled in the education system or in active labor market policies until they find a matching job, hence diminishing the risks of (long-term) unemployment. Or, they may drop into inactivity. Such substitution of labor market states in response to expected difficulties in finding a job are alternate explanations that may account for the absence of an effect of employment protection on youth unemployment, an issue we will take up in the empirical analysis.

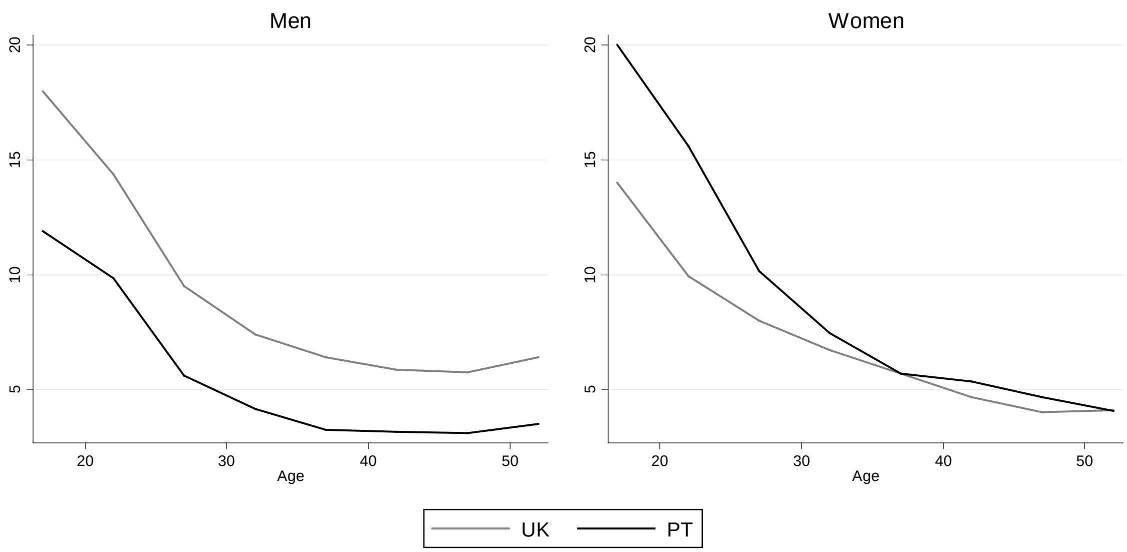
Figure 3.2 Median tenure and long-term unemployment on the youth labor market



Source: European Union Labour Force Survey, own calculations.

If firing costs lower both the transition rate into as well as out of first jobs, it becomes more difficult to sustain the prediction that firing costs raise the youth relative to the adult unemployment rate. To further illustrate this point, Figure 3.3 compares two European countries with arguably the most liberal and the most restrictive protection of incumbent employees, the United Kingdom and Portugal. The data represent aggregates of age-group specific unemployment rates for both countries for the period from 1986 to 2006. While the dynamics generating the aggregate outcomes differ fundamentally (Blanchard and Portugal, 2001), age-specific unemployment rates are rather similar. Men at all ages, and the youngest age group in particular, are even better off, while women, younger women in particular, seem to suffer disadvantages in the highly regulated Portuguese labor market. This illustrates our argument that firing costs – by lowering both outflow from unemployment to first employment and outflow from first employment to unemployment – are unrelated to age-based inequalities in unemployment risks, at least among men.

Figure 3.3 Age-unemployment profiles, United Kingdom and Portugal, 1986-2006



Source: European Union Labour Force Survey, own calculations.

3.2.2. Behavioral effects

A number of recent theoretical contributions have pointed at a more complex linkage between firing costs and economic outcomes. Firing costs may provide incentives for on-the-job training which benefits young workers and lowers their dismissal rates. At the same time, firing costs may also lead to hiring discrimination, both against experienced workers who lost their job, as well as against young workers, which are more risky to hire. However, again we face the difficulty of making a clear prediction regarding age-based inequalities, since the mechanisms work in opposing directions.

From a human capital perspective, by stabilizing employment relationships, firing costs provide an incentive to invest into skills, both for employers and employees. By lowering turnover, firing costs increase the likelihood that both employers and employees reap the benefits of skills investments (Becker, 1964; Estevez-Abe et al., 2001). Employers may even invest into their employee's general skills, if they can be more certain of the continuation of the employment relationship (Fella 2005; Acemoglu and Pischke 1998).[19] A higher training intensity makes young employees more valuable to employers, which further stabilizes the employment relationship. Thus, firing costs may lead to more investment into both general and specific skills, which should lower the risk for young people to be dismissed. Moreover, by smoothing employment over the business cycle, employment protection also facilitates the maintenance of specific skills. While in the short run, employers may reduce staffing levels in times of crisis, this may be inefficient in the long run because of the human capital that is lost. Firing costs therefore not only protect individual workers, but also the skills of the workforce.

From a screening perspective, the uncertainty associated with hiring a young worker may cause risk-averse employers to prefer hiring experienced, older workers rather than younger inexperienced workers. It is more difficult to forecast the expected profit from hiring a young person, since productivity can be observed less reliably. Especially in the case of young people, who just left school, employers lack reliable indicators of performance like job history and previous employer recommendations (Rosenbaum et al., 1990; Breen, 2005). Hence, compared to hiring a more experienced worker, employers face greater uncertainty about the profit outlook of a new match when hiring a young worker. In the presence of firing costs, hiring becomes risky since employers incur a loss if they select somebody less productive than expected. At the

[19] Acemoglu and Pischke (1998) develop a model of employer behavior that puts the information gains from screening at the heart of the explanation of employer provided apprenticeship training in some European countries.

margin, risk-averse employers therefore hire individuals for whom they can make a more precise assessment of future productivity. Hence, they are more likely to hire (employed or unemployed) workers with previous job experience, or fill a vacancy from among the already employed (Kugler and Saint-Paul, 2004). As a result, firing costs lower relative hiring rates for young people without job experience.

Furthermore, firing costs may raise the stigma associated with dismissals (Canziani and Petrongolo, 2000). Because firing costs lower the dismissal threshold, the higher the firing costs, the lower the productivity of those actually dismissed. Hence, if dismissal occurs in the presence of high firing costs, it signals particularly low productivity. Firing costs thereby amplify the stigma attached to dismissal, and may make dismissed older workers worse off compared to young people fresh out of school. Furthermore, experienced workers who are dismissed risk losing their skills in prolonged unemployment spells, in particular if employment protection restricts their re-employment opportunities, which should further reduce their advantage relative to inexperienced younger workers.

Finally, another employer response that may offset the negative effects of firing costs is an intensification of screening (Marinescu, 2009). If hiring becomes more risky in the presence of firing costs, employers may commit more resources on screening prospective employees, which raises costs, but also improves match quality and diminishes the risks and potential costs associated with hiring. While it is sometimes argued that firing costs, by diminishing mobility opportunities, may keep young people in jobs that they do not like, intensified screening as a response to firing costs may diminish not only employer but also individual costs from mismatch. More intense pre-employment screening thus can potentially improve the allocative efficiency of the school to work transition, compared to a situation of trial-and-error job matching, where information problems can result in excess turnover (Gladden and Taber, 2009). Acemoglu and

Pischke (1998) even argue for the German case that employers are willing to shoulder the costs of general skill provision in the context of apprenticeship training because of the screening opportunities and information gains they acquire.

In sum, we can think of structural as well as behavioral implications of firing costs for the youth labor market (see Gangl, 2006: 990). First, firing costs lower turnover and vacancy rates. Because of the countervailing effects of firing costs on the flows into and out of unemployment, firing costs have an ambiguous effect on aggregate unemployment rates. This conclusion also applies to the youth labor market, and therefore the relationship between firing costs and the distribution of unemployment risks is theoretically ambiguous without further assumptions. Second, firing costs may alter the behavior of labor market participants, in particular employers, in more complex ways. They create incentives for on-the-job training, which can benefit young people, an intensification of screening and improvement of initial match quality, but they may also lead to hiring discrimination, which may work for or against young people. Since these screening and training mechanisms again have opposing effects on young people's labor market position, they may mutually offset each other as determinants of relative youth unemployment rates. Empirical testing has to show whether the underlying mechanisms add up in favor or against young people, or whether they cancel each other out, yielding no effects on the distribution of unemployment.

3.3. Firing costs and the deregulation of temporary employment

Traditionally, the employed workforce across advanced economies has held contracts of indefinite duration, which we have referred to as regular employment contracts. In some European countries, employment protection legislation imposes substantial firing costs on employers who intend to dismiss a worker on a regular contract. Temporary contracts, which last for a contractually specified duration, are usually exempt from such regulations. Upon expiration, the employment relationship can be dissolved without further costs, or employers may convert the temporary into a regular (permanent) contract. To prevent the erosion of job security provisions for workers on regular contracts, regulations on temporary contracts limit the economic contexts within which temporary workers may be used, the number of temporary contracts an individual may hold with the same employer and the total duration of temporary employment with an employer. [20] While much of the detrimental effects of employment protection had been ascribed to high firing costs for regular employees, reform attempts often left this type of regulation unaltered. Instead regulations on temporary contracts were eased to raise employer flexibility, foster job creation and reduce unemployment.

3.3.1. The incidence of temporary employment

Different institutions in European countries are commonly assumed to constrain employers in their ability to shift adjustment costs in product markets onto their employees. Collective bargaining arrangements, powerful trade unions and generous unemployment benefits are assumed to constrain wage flexibility, while employment protection legislation acts as constraint on hiring and firing. In particular, these institutional rigidities raise employment costs for less skilled workers compared to liberal institutional environments found in Anglo-Saxon countries, in particular the United States (Krugman 1994; Blau and Kahn, 2002). However, following DiPrete et al. (2006), temporary contracts lower employment costs, and have allowed European countries to create jobs in particular for less skilled workers, who would be otherwise priced out of the labor market. Temporary contracts reduce or eliminate potential firing costs and therefore

[20] For example, before the deregulation of temporary employment in Spain, this type of contract was only permitted in certain, inherently seasonal economic activities, such as agriculture and tourism (Bentolila and Dolado, 1994; Dolado et al., 2002).

make hiring less costly. Polavieja (2003) also points to the cost-effect of temporary contracts: Since temporary workers face a heightened unemployment risk compared to permanent workers, they accept lower wages in return for potentially being rewarded with a permanent employment contracts (see also Guell, 2000).

From a human capital perspective, temporary contracts are more likely to be used in unskilled jobs. Whenever investments into specific skills are made, the incentive to convert a temporary into a permanent contract increases. From this perspective, growing skill demand, triggered by technological change or globalization, may have even increased the conversion rate from temporary to permanent employment among those employees who receive training and work in more skill-intensive jobs. Thus, while more skilled (educated) workers may hold temporary contracts, particularly in the beginning of their career, temporary employment should normally not be a terminal destination as employer require long term employment relationships with their skilled workforce.

In contrast, temporary contracts are attractive in unskilled jobs where workers are more easily replaceable. Thus, it is commonly argued that temporary contracts are a source of labor market flexibility in the low skill labor market (DiPrete et al., 2006; Maurin and Postel-Vinay, 2005), where macroeconomic changes have led to a decline in demand for low-skilled workers. Given constraints on wage flexibility, this decline in demand is translated into a deterioration of job security, i.e. unskilled workers are hired into temporary rather than a permanent employment contract. Commonly skill gaps in job security are understood as inequalities between education or occupation groups (e.g. Maurin and Postel-Vinay, 2005), where skill biased demand shifts result in relative gains among highly educated (in skill-intensive jobs) and relative losses among less educated (unskilled jobs). However, as we have seen in Chapter 2, much of the growth in temporary employment has occurred on the youth labor market, and sizeable inequalities exists in the distribution of temporary employment between youth and adult workers. If we think of age as proxy for experience or specific skills, we can interpret this age-gap as a skill-gap, and we can also interpret the growth in job insecurity among young people (in absolute terms and relative to adults) as the consequence of skill-biased (experience-biased) demand shifts (Ryan, 2001, 2009).

While the human capital perspective stresses the role of skills in determining job security, the concentration of temporary contracts on the youth labor market suggest a second plausible interpretation. As we argued above, hiring into a highly protected permanent employment position is risky, which makes employers rely on temporary contracts as screening devices in entry-level positions. Temporary contracts allow employers to observe potential candidates for permanent employment on the job and arrive at a more reliable productivity estimate, which minimizes the risk of hiring an unqualified candidate. Because they diminish or eliminate the hiring risks associated with filling a highly protected permanent job, we can interpret flexible temporary employment in entry level jobs as a complement to protected, skilled permanent employment.

Finally, from a bargaining perspective, youth and adults differ in terms of the power they wield within firms and in worker representation and collective bargaining procedures. Incumbent adult workers may therefore exploit their bargaining power to lobby for the use of temporary contracts as a buffer against employment fluctuations (Polavieja, 2003; Salvatori, 2009). Extensive job security provisions may further increase the bargaining power of incumbent employees, and their ability to use temporary (young) workers as a buffer. Polavieja (2003) and Bentolila and Dolado (1994) argue that this effect may be non-linear: As temporary workers become more numerous, the bargaining power of insiders starts to erode and employers may use less expensive temporary workers to undercut incumbent workers' bargaining position (Salvatori, 2009).

In sum, we would expect that the deregulation of temporary contracts has led to a growth of temporary employment in Continental and Southern Europe as well as Scandinavia, where employers gain flexibility by using temporary contracts. Temporary employment should be higher particularly in the presence of strict regulations on dimsissal from regular contracts. However, job security provisions are only one of the potential constraints on flexibility; wage setting institutions and strong trade unions may be other relevant factors increasing demand for temporary employment. The effects of deregulating temporary contracts on the growth of temporary employment should be minor if regular contracts already grant sufficient flexibility.

3.3.2. Temporary contracts and unemployment

Employers usually face no dismissal costs when temporary job contracts expire. They may renew the contract again on a temporary basis[21], hire the employee on a permanent basis, chose to close the position or fill the job with an outside applicant. Thus, temporary contracts imply more flows into and out of employment, which implies higher turnover and shorter employment and unemployment spells (Dolado et al. 2002). Just like a reduction in firing costs on permanent contracts, deregulating the usage of temporary contracts should have no effect on aggregate unemployment. And, extending the conclusion from the preceding discussion on firing costs, if deregulating temporary contracts increases both the ins and outs from unemployment, the distribution of unemployment should also be unaffected.

Nevertheless, two types of scenarios have emerged that stress the potentially virtuous or detrimental consequences of temporary employment. On the virtuous side, flexible temporary contracts may be seen as complements to secure permanent employment. Without destroying job

[21] A number of European countries have enacted regulations on the number of successive temporary contracts an individual may hold with the same employer.

security for the core workforce, temporary contracts are used to screen entrants to the workforce and represent stepping stones to permanent employment (McGinnity et al., 2005). Since hiring into permanent employment is risky if firing costs are high, temporary contracts allow employers to screen prospective employees before they hire them permanently, diminishing hiring risks and turnover costs. By increasing flexibility and low cost screening opportunities, temporary contract may add jobs where there was (high) unemployment before, especially among the low skilled and young people. Rather than engaging into prolonged, potentially discouraging job search, temporary work allows individuals to gather job experience, which employers may value as more reliable indicators of productivity than educational degrees, and which should therefore improve their subsequent employability.

However, deregulating temporary contracts may induce employers not to offer more jobs, but to destroy existing permanent jobs and replace them with flexible temporary jobs. This incentive is particularly strong where permanent contracts are subject to severe dismissal regulations. As employers substitute permanent for temporary jobs to obtain more flexibility, turnover increases but no new jobs are added. Workers lose some of their job security rents, employers gain flexibility, but unemployment or employment remains unchanged. However, given that incentives to provide training for temporary employees are lower (Arulampalam et al., 2004), a rise in temporary employment may depress worker productivity growth (Dolado et al., 2002), which may be detrimental to employment in the long run.

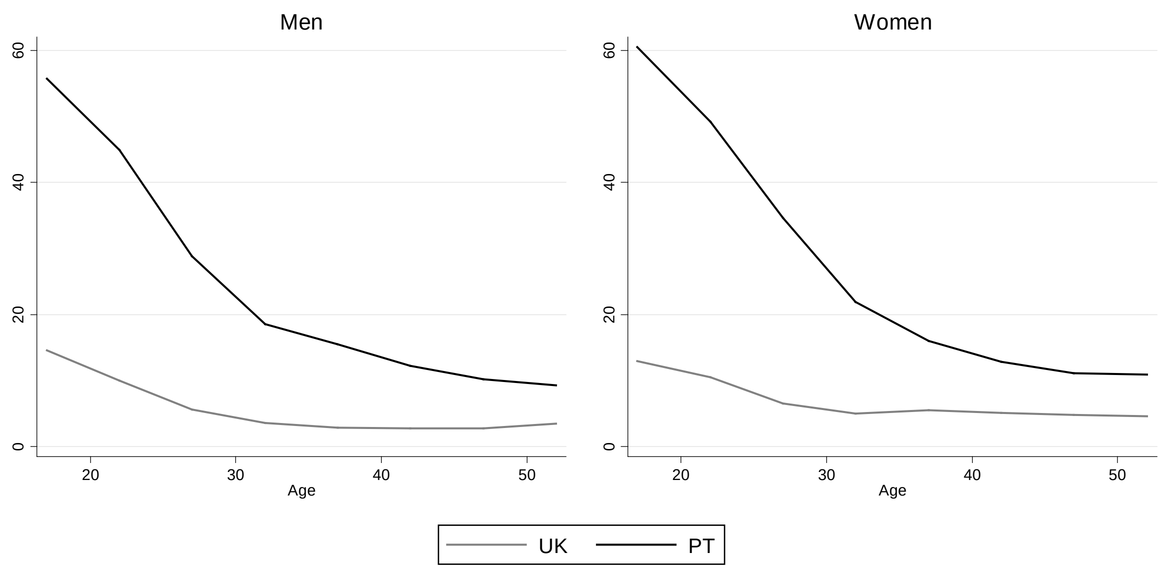
Moreover, if firing costs on permanent contracts are high, deregulation of temporary contracts may even raise unemployment, particularly among young people who are more likely to hold that type of contract. Firing costs raise the productivity threshold for a temporary job to be converted into a permanent one, since expected dismissal costs on permanent contracts need to be compensated for by a higher level of productivity. Moreover, if firing costs are high, employers lose more bargaining power when promoting employees from temporary to permanent status, which should also lower the conversion rate (Blanchard and Landier, 2002; Cahuc and Postel-Vinay, 2002). Thus, deregulation of temporary contracts while keeping permanent contracts highly protected leads to a segmentation of the labor market, where particular demographic groups cycle in and out of temporary jobs and unemployment with low probabilities of escaping into a permanent job. Under some circumstances, the growth of temporary jobs may therefore not lower but rather raise unemployment.

However, the link between segmentation, heightened turnover and unemployment is subtle. The calibration exercises for the theoretical models presented by Blanchard and Landier (2002) and Cahuc and Postel-Vinay (2002) only give limited support to the implication that partial deregulation would raise unemployment rates. In the Blanchard-Landier model, the effect of partial deregulation (a decrease in firing costs for temporary contracts) is ambiguous.[22] Similarly, in the Cahuc-Postel-Vinay model, firing costs on permanent contracts lower unemployment, but this effect diminishes if the labor market share of temporary contracts increases. However, 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: 83). Deregulating temporary contracts when firing cost on permanent contracts are "average" or low, should therefore not change anything. Altogether, this suggests that the "perverse effects" of partial deregulation, if

[22] The effect of deregulating temporary contracts while keeping firing costs in permanent contracts high is non-linear in the distribution of the match productivities in regular jobs, such that in some parts of the distribution, partial deregulation increases unemployment, while it decreases unemployment in other parts (F226-7). While the authors leave open, how to apply this result to concrete labor market situations, they suggest that the French experience shows the dark side of deregulation in particular for young people. As they acknowledge, however, their empirical analysis leaves many questions open. For example, France experienced very rapid educational expansion, which is one of the most pronounced trends that Blanchard and Landier observe (see their Figure 7). As more and more young people enter higher education, the age-group Blanchard and Landier study, 20-24 year olds, has quite likely undergone compositional changes, which may partly account for some of the declines in outcomes observed. At the same time, higher enrolment in education may be a result of poor labor market prospects, which points to the importance of assessing substitution effects.

present, are likely to manifest themselves in those few countries where firing costs on permanent employment contracts are very high. Moreover, partial deregulation should affect in particular the less educated workers (DiPrete et al., 2009; Gebel and Giesecke, 2009b), as well as young people to the extent that they are overrepresented in unskilled positions.

Figure 3.4 Age-temporary-employment (% of total employed) profiles, United Kingdom and Portugal, 2003-2006



Source: European Union Labour Force Survey, own calculations.

Figure 3.4 (see above) illustrates the role of temporary contracts in the UK and Portugal using EULFS data from 2003-6, graphing the percentage of employed on temporary contracts across age groups. In both countries, the incidence of temporary contracts is higher among young people. Comparing both countries, the percentage employed on temporary contracts in Portugal is more than four times higher than in the UK among younger people. This is consistent with the expectation that if permanent contracts are highly regulated, employers are more likely to use temporary contracts, particularly in entry level positions. We may interpret the British and

Portuguese approaches as two alternative flexibility strategies, where Britain relies on deregulated permanent contracts, and Portugal relies on temporary contracts at entry levels and regular, protected contracts elsewhere. The key difference, however, is that older workers enjoy much higher job security in Portugal than in Great Britain. Furthermore, we see that the high incidence of temporary employment in Portugal is not associated with higher unemployment, or higher age-based inequalities in unemployment.

In sum, we again face some difficulty in deriving clear predictions. If deregulation effectively reduced employment costs, employers may have responded by expanding employment particularly in the low skill sector. Moreover, temporary jobs may reduce hiring risks and lead to increased hiring across the skill distribution. These mechanisms may have contributed to net job creation and a decline in (youth) unemployment. At the same time, employers may have simply substituted permanent for temporary employment, which implies that deregulation did not reduce unemployment. Even worse, excessive turnover generated by "partial deregulation" may have even raised youth unemployment, in particular among the less skilled.

3.4. Discussion and Summary

Researchers have had difficulty in squaring the often cited need for flexibility with the constraints that employment protection imposes. The impact on public policy notwithstanding, this issue is not resolved. Employment protection regimes seem to affect worker flows and labor market dynamics, but their link to job flows is less clear. It is also less clear whether or to what extent reduced worker flows are necessarily inefficient and diminish productivity and economic growth. They do not seem to affect aggregate unemployment and employment growth, two crucial indicators of economic performance. Moreover, it seems that empirically (Bertola and Rogerson, 1997; Bauer et al., 2007, Hunt, 2000) employment protection is less of a constraint on employment adjustment and job turnover than theorists assume (Blanchard and Katz, 1997), and its effects on productivity are also difficult to detect (Autor et al. 2007). The argument that lack of flexibility carries efficiency costs, which then translate into inferior employment performance, requires further elaboration and testing.

A forceful defense of employment protection legislation stresses its role in productivity and skill formation, in particular in the formation of specific skills which sustain global competitiveness in manufacturing industries (Estevez-Abe et al., 2001). Employment protection may therefore be part of the comparative advantage that allows some countries to maintain a highly skilled manufacturing workforce, which may to some extent pre-empt the need to generate low quality service sector jobs. Employment protection may facilitate investment into both general and specific skills (Acemoglu and Pischke, 1998; Estevez-Abe et al., 2001), while high worker turnover is associated with underinvestment in skills, particularly among non-college-bound youth (Stevens, 1996; Layard et al., 1994). In the absence of employment protection, excess turnover is associated with substantial losses of specific skills (Estevez-Abe et al., 2001; Gangl, 2004). The potential adverse effects of deregulation may therefore be a reduction in skills investment, which would exacerbate already existent scarcities of skilled labor. Given that temporary workers receive less training, the deregulation of temporary employment may have exacerbated skill scarcities and diminished productivity growth (Dolado et al., 2002), which may have serious consequences for economic performance and labor demand.

Up to now, few studies have attempted to identify the causal effects behind employment protection, training and labor productivity (but see MacLeod and Nakachavara, 2007; Bassanini et al., 2005). Neither do we have a clear idea just how much turnover is efficient (for an empirical assessment, see Gladden and Taber, 2009). Furthermore, we may miss important institutional factors, when focusing on the impact of firing costs only. Turnover may depend on a number of institutional factors that may be complements to or interact with job security provisions on regular contracts. The previous discussion has pointed to the role of temporary contracts as potential complements to dismissal regulations on permanent jobs. Moreover, institutions such as the German dual system may be viewed as complements to highly protected jobs by providing a low-cost screening opportunity for employers (Breen, 2005). Active labor market policies may perform a similar function, too. Bertola and Rogerson (1997) have pointed to the consequences of wage compression for raising job turnover in the presence of employment protection. Moreover, corporatist bargaining structures may be capable of undoing detrimental effects of insider bargaining for aggregate employment outcomes. Comparative research can clearly be helpful in resolving this issue empirically, especially in terms of potential interactions and complementarities between institution that vary only across countries. However, given the limited number of comparable advanced countries where employment protection is an issue, degrees of freedom problems remain.

While institutional complementarities and interactions are crucial for our understanding of employment protection and for devising effective and efficient reforms, the goal of this study is more modest. We are mainly concerned with assessing the average effects of employment protection regimes across countries, and assessing their impact across education groups. The only institutional interaction we test relates to the interaction between both dimensions of employment protection. Nevertheless, reducing our focus in this way hopefully sharpens the analysis and delivers more credible estimates of the average effects of employment protection. In the empirical chapters, we will test the following hypotheses:

Deregulating temporary contracts should have raised temporary employment among young workers, in particular if job security provisions on regular contracts constrain employer flexibility. Strict job security provisions should by themselves be associated with higher temporary employment. Regarding unemployment or employment, we cannot arrive at clear cut predictions. We argued that the overall effect of job security provisions on youth unemployment is indeterminate, and this conclusion also extends to the effect of deregulating the usage of temporary contracts. To the extent that job security provisions or insecure temporary contracts are perceived to reduce youth labor market chances, this may translate into higher rates of non-participation, which may partly account for the absence of an effect on unemployment/employment.

"Partial deregulation", i.e. removing restrictions on the usage of temporary contracts while maintaining extensive job security provisions on regular contracts, may have effectively lowered employment costs in the low skill sector. This may have resulted in job growth and lower youth unemployment; but it may have also led to substitution of permanent with temporary jobs, which may have even raised youth unemployment. The adverse effects of partial deregulation are most likely to present themselves among the least educated, who normally work in jobs requiring few specific skills. In contrast, the positive effects of partial deregulation may manifest themselves particularly among the highly skilled (the most educated), for whom temporary contracts represent a screening period, a stepping stone to permanent employment.

4

Data and Methods

In this chapter, we outline the analytical design we have chosen to evaluate the effects of employment protection on the youth labor market. Like many analyses in this field, we have to rely on observational data, and we face the known pitfalls of observational data analysis (see Morgan and Winship, 2007; Angrist and Pischke, 2009; Imbens and Wooldridge, 2009). However, we try to improve on past empirical work in several respects to make more confident statements about causal relationships. In the following, we outline in some detail how we arrive at the tables that contain the core results in the next chapters. The Data Appendix at the end of this chapter contains additional details on the data sources and control variable definitions.

In this study, we pool data on group-specific labor market outcomes and employment protection legislation data for many countries and years. We rely on two principal data sources for labor market outcomes. First, we use data on aggregate labor market outcomes, broken down by gender and age group, for 21 OECD countries for the period from 1985 to 2007. Second, we draw on microdata from labor force surveys for 15 Western European countries and the United states from 1992-2006. The latter data set covers key European countries, which have been at the center of debate about employment protection legislation and experienced several reforms in the period of observation. Using microdata allows us to calculate outcomes for specific groups at a more disaggregate level, using data on respondents' age, gender and education, which yields further insight into distributional effects of employment protection.

We adopt two analytical designs: As a starting point, we use variation both across countries and across time to estimate the association between employment protection and labor market outcomes. We refer to the first type of analyses as "cross-sectional analyses" to emphasize that the bulk of the variation stems from variation across countries. Second, we use only variation within countries across time, disregarding all cross-sectional variation. We implement this approach using both conventional fixed effects estimation as well as differences-in-differences (DiD) estimation. Since our goal is to draw causal inference about the effects of employment protection legislation, the latter design has clear conceptual advantages which we discuss below.

A number of studies have emerged that look at specific reforms of employment protection in different countries (Kugler et al., 2005; Autor et al., 2006; Kugler and Pica, 2008; Bauer et al., 2007) using a DiD approach. While these studies are informative about the impact of specific reforms, it is not clear whether their results can be generalized, in particular if the impact of changes in employment protection is conditional on the macro-economic or institutional environment. By studying reforms in many countries and over extended time periods jointly[23], we hope to be in a better situation to assess the existence and size of reform effects and the conditions, under which reforms are effective (although space limitations prevent us from

[23] In the period from 1992 to 2006, we study the effect of 14 changes in regulations on the usage of temporary contracts and 14 changes in job security provisions on regular contracts.

addressing this issue extensively here)[24]. We also gain more degrees of freedom to adjust for time-varying confounders that are a threat to causal inference in single-country studies.

When performing both cross-sectional and DiD analyses, we hope to improve upon prior research in several ways. First, prior studies have sometimes attempted to identify the effects of employment protection in the cross-section, and these studies are frequently cited as support for the consensus about the distributional effects of employment protection. Given the political relevance and the theoretical indeterminacy, we should probe the robustness of cross-sectional results and attempt a more rigorous design for causal inference. Second, we use the most authoritative data on employment protection to date, drawing on the latest revision of the OECD indicator (Venn, 2009). Third, many past studies have estimated linear regression models with many parameters, and implicitly or explicitly assigned a causal interpretation to the various parameters estimated (Nickell et al., 2005; Bassanini and Duval, 2006; Blanchard and Wolfers, 2000; Bertola et al., 2007). Here, we focus on estimating the effects of two variables, job security provisions on regular contracts and regulations on using temporary contracts. Limiting our focus in this way allows us to take greater care in the analysis, by discussing the underlying mechanisms more thoroughly, and by doing extensive specification checks to probe the robustness of our findings and achieve more credible, causal estimates. Finally, we follow the clear recommendation in the literature and adjust our standard errors for arbitrary correlation within observations from the same country.

To make identification of causal effects more plausible, we try to use control variables more effectively. In both cross-sectional and DiD design, we have to assume that we have accounted for all observed and unobserved factors that determine both outcome variable and employment

[24] Future work will further assess interactions between different institutions. In this study, we only assess the interaction between the both dimensions of employment protection legislation.

protection regimes. To make this assumption more plausible, we propose several conditioning strategies. Based on recent critiques of regression analyses (see Morgan and Winship, 2007), we propose a conditioning strategy that uses a more flexible specification of the control variables, and thereby relax the restrictive and often implausible linearity assumptions that are frequently used in the analysis of aggregate data: Following the logic of matching analysis, we try to detect quadratic or interaction terms generated from the control variables that model treatment assignments (i.e. the choice of employment protection regime) and attempt to balance the data in this way. In addition to the main effects of the control variables, we add these "powerful" quadratic/interaction terms generated from the control variables that predict treatment assignment. Using this more flexible control variable specification, we try to more effectively adjust for confounders that conventional linear specification fail to account for.

Finally, we often arrive in situations where we need to make decisions that may be very consequential for the results. We try to defend here the multiplicity of consequential choices we have to make even before we specify estimation models. In particular, when performing regression analyses, we have considerable freedom in deciding which model we eventually present for publication. Young (2009) has recently pointed to the enormous asymmetry between researchers and reviewers in terms of the results that different model specifications produce and, consequently, the need for reporting more extensive, sensible specification checks: “Results that depend on an exact specification, and unravel with sensible model changes, are not reliable findings” (Young, 2009: p. 394). For this reason, we try to pin down at least some objective rules that we commit ourselves to throughout the analysis to reduce opportunities for "partisan model selection".

4.1. Data

We rely on two data sources on labor market outcomes: the OECD Labor Force Statistics database (online) as well as individual data from the European Union Labour Force Survey (EULFS). The separation between aggregate and individual data is somewhat artificial here. The OECD labor force statistics database allows for calculation of “aggregate” labor market outcomes broken down by gender and five-year age groups. The only additional grouping characteristic that the EULFS adds is the level of education. The latter distinction is crucial, however, because educational inequalities are at the heart of the academic debate. As such, the EULFS it is the best data source we currently have to assess changes in the demographic distribution of employment outcomes, taking account of educational differences.

In general, this analysis is concerned with the experience of advanced OECD countries starting in the early 1970s up until the present. We focus on a period where advanced countries were experiencing episodes of severe economic distress, manifesting itself in rising inflation and unemployment, as well as economic and financial crisis in Scandinavia. The multivariate analysis, however, only covers the period from 1985 onwards and much of the empirical analysis focuses on Western European countries in the early 1990s until the mid-2000s. We therefore study the consequences of institutional change in a period of (adjustment to) crisis, in periods where aggregate productivity and labor demand has fallen beneath levels prevalent in the 1960s. In short, the macro-economic environment cannot be considered to be in equilibrium, but we have to assess the consequences of employment protection reform in a period of economic adjustment. This complicates the analysis by introducing a lot of cyclical and structural variation. Our results may therefore not generalize to economically more stable periods.

4.1.1. The OECD dataset on labor market outcomes

From the OECD data, we calculate aggregate outcomes separately for each gender. We usually focus on outcomes for 15-29 year olds. The past convention has been to study 15-24 year olds, but given rising participation in tertiary education, an increasing number of young people make their first, main transition into the labor market after the age of 24. We therefore extend the upper age limit to 29 year olds, but also conduct robustness checks where we look at five-year age groups separately. The outcomes we study are unemployment, employment and labor force participation rates, as well as temporary employment. We restrict the sample to 21 OECD countries for the period from 1985 to 2007 and refer to this dataset as the “OECD dataset” (as opposed to the “EULFS dataset”, see below). While some of the data series go back into the 1960s for some countries, we do not use observations before 1985 for the statistical analyses, since we only have data on employment protection legislation that differentiates between regulations on permanent and regulations on temporary contracts from 1985 onwards. The 21 countries included are: Austria, Belgium, Germany, Denmark, Finland, France, Greece, Ireland, Italy, Japan, the Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, the United Kingdom, the United States, Canada, Australia and New Zealand. Since we lacked information on key control variables, we did not analyze the Eastern European countries (the Czech and Slovak Republics, Hungary and Poland) as well as Mexico, Turkey and South Korea. After dropping all observations with missing information on dependent and independent variables, we are left with an unbalanced panel data set for 21 OECD countries from 1985 to 2007. The time series are complete for most countries (see Table 4.1 in the Data appendix, Section 4.7).

Our data and analyses therefore do not cover the initial increases in unemployment starting in the early 1970s, which has been the focus of many preceding analyses (Blanchard and Wolfers, 2000; Nickell et al., 2005). Instead a substantial part of the longitudinal variation is generated by the decline in unemployment rates observed in a number of countries since the early 1990s. We may thus learn less about why unemployment or temporary employment increased, but more about why it persisted or declined. Furthermore, the data set is selective in terms of the countries covered. Our results may therefore not be generalizable beyond Western European countries.

At the same time, the critique of employment protections legislation has developed mainly in response to the experience of Western European countries. Whichever non-Western-European countries we add to our sample, it would be essential to check that our findings also hold up, if we focus on Western European countries only. From the perspective of causal inference, we need to exercise control over the heterogeneity in the sample. Given the unique history of Eastern European countries, and the developmental and cultural differences between Western European countries and Mexico, Turkey or South-East Asian OECD countries, we may do more harm than good in adding these cases to our analysis. While gaining more cases and variation, the experience of these countries is quite different in terms of partly unobserved economic and institutional characteristics, making the resulting sample unnecessarily heterogeneous in terms of baseline outcomes and treatment effects.

4.1.2. The augmented EULFS dataset on labor market outcomes

The second source of data on labor market outcomes is the European Union Labor Force Survey (EULFS). The EULFS provides cross-sectional information on individual labor force participation and various other aspects of employment. It consists of nationally representative labor force surveys that are standardized and harmonized by EUROSTAT. We use data from 1992-2006 for the following countries: Austria, Belgium, Denmark, Spain, Finland, France, Greece, Ireland, Italy, Netherlands, Norway, Portugal, Sweden, and UK. Employment status is harmonized based on ILO guidelines (1990). The EULFS allows for distinction of ISCED 0-2 (lower secondary education or less), ISCED3-4 (upper and post-secondary education) and ISCED5-6 (tertiary) education degrees (cf. UNESCO 1997, for the International Standard Classification of Education (ISCED) 1997 version). Information on respondents' age is only available in aggregate form for five year age groups. Among the labor market outcomes considered are employment, unemployment and labor force participation, as well as part-time employment, and temporary employment.

While the EULFS contains relatively detailed measures of individual employment outcomes (but no information on earnings or income in the publicly released data), it only contains sparse information on other individual characteristics, especially if we want to use data for a longer time series, which is our goal here. At the same time, the EULFS is the only micro-data source available that provides harmonized data and allows for calculation of diverse labor market outcomes for specific demographic groups, defined by age, gender and education. Sample sizes are larger than for many other available surveys, which allows for the reliable calculation of labor market statistics for distinct demographic groups. These features make the EULFS the best data source we currently have to assess changes in the demographic distribution of employment outcomes.

Table 4.1 (Data appendix, Section 4.7) details the countries and years we use for multivariate statistical analyses. While micro data is available for some countries from 1983 onwards, we only have information on individual education (three levels) from 1992 on. Moreover, we lack data for some years for a number of countries. We enriched the dataset with data from the German Microcensus (1996-2005), as well as the American Current Population Survey (IPUMS-CPS, King et al. 2010). We thus obtain a homogenous sample of 16 Western countries, with data for maximally 15 years of observation per country. If all countries were fully observed, this would provide for 240 country-year observations, but our actual sample comprises of 207 country-year observations. For analyses of temporary employment, we have to drop the U.S. because of missing data and focus solely on Western Europe.

For the statistical analyses, we calculate labor market outcomes for specific demographic groups. When analyzing the youth labor market, we define six groups by education (three levels) and gender. We set the age group corresponding to the level of educational attainment and typical graduation ages, so that the groups are similar in terms of potential labor force experience. For ISCED 0-2 graduates, we take 15-24 year olds, for ISCED 3-4 graduates 20-29 year olds, and for ISCED 5-6 25-34 year olds. This imposes some distortions, but setting age bands according to educational attainment is more plausible than imposing a common age range, which would be our only alternative given the scarce information in the data.

The dataset for the analysis of youth labor market outcomes therefore comprises of six observations per year and country for which we have data. Given that we have 207 country-years at the aggregate level, this yields a sample of 6*207=1242 observations. We view the resulting dataset as pooling time-series of labor market outcomes for different groups, nested within countries and years. A key problem resulting from this data structure relates to calculation of standard errors, which we discuss in Section 4.2.2.

We disregard the information about the number of observations used to calculate the group specific outcomes and assume that these aggregates are estimated without error. If we were to correct for uncertainty in the estimates, this would imply penalizing countries for drawing small samples for their labor force survey and make the results very sensitive to the inclusion of countries drawing large samples/populations, like Germany or the U.S. There is no substantive reason why the results should be more sensitive to the relationships found in certain countries. We also disregard information about the relative size of the different groups within a country, since we again do not see a substantive motivation for such a correction. Our goal is to estimate whether certain broadly defined demographic groups have been more affected by employment protection reform, irrespective of the particular size of each group.

4.1.3. Independent variables

Our independent variables vary at the country and year level. The Data Appendix (Section 4.7, p. 106) list in detail sources and definition of our control variables as well the key variables of interest, i.e. the measures of employment protection legislation. The control variables are standard institutional and macro-economic factors used in analysis of aggregate employment or unemployment rates (Blanchard and Wolfers, 2000; Nickell et al. 2005; Baccaro and Rei, 2007): indicators for the generosity of unemployment benefits, trade union density, two indicators for the coordination of wage bargaining, indicators for the cabinet power of left parties, expenditure on active labor market policies, product market regulation, economic globalization, terms-of-trade shocks, long-term interest rates, the inflation rate, output gap, and youth cohort size. We include a "hump" shaped wage bargaining indicator (Calmfors and Driffill, 1988) and left party power in government to capture factors that have been linked to determinants of employment protection or the incidence of temporary employment (Rueda, 2005; Polavieja, 2006). The "hump-shaped" wage bargaining indicator is derived from the indicator measuring the centralization of wage bargaining to capture the potentially detrimental effects of wage bargaining at intermediate (sectoral, industrial) levels, as opposed to company-level or economy-wide wage bargaining. Unfortunately, we could not obtain a suitable aggregate indicator for the duration of unemployment benefit payment. Our indicator only focuses on the generosity of unemployment benefits.

The key independent variables of interest are the OECD indicators of employment protection legislation. We rely on two indicators, one for regulations of dismissal of employees on regular employment contracts, and one for regulations on the usage of temporary contracts. While the OECD has published previously this indicator for certain periods, the latest revision comprises of nearly complete, annual time-series data for most OECD member countries from 1985 to 2008 (Venn, 2009). Both indicators are based on individual items measuring different aspects of regulation.

The indicator for regulations governing dismissal of workers on regular (permanent) contracts 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 indicator on regulations on the usage of temporary contracts is based on three items: valid reasons for using a temporary (rather than a regular) employment contract, maximum number of successive temporary contracts with the same employer, and the maximum cumulated duration of temporary contracts with the same employer. All items are coded by national experts into numerical scores varying between 0 and 6. In assigning the scores, experts do not just consider employment law, but also 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).

Venn (2009) shows that the aggregate index (combining all dimensions of employment protection) is fairly insensitive to alternate weighting strategies used when aggregating the items (for further discussion, see Venn, 2009: 11f.). However, when translating legal text and practices into numerical codes, some information is certainly lost. Severance pay could be measured on a cardinal scale, instead of aggregating the variation into six groups. Also, severance pay is probably highly contingent on individual work situations, especially tenure[25], and taking one aggregate measurement almost invariably leads to a loss of information. Both Lazear (1990) and Heckman and Pages (2004) rely on severance pay measured on a cardinal scale as the aggregate measure of employment protection.

However, severance pay is not the only mechanism through which employment protection legislation makes firing costly. The OECD indicator is clearly more comprehensive in accounting for dismissal costs besides severance pay, by explicitly including administrative and judicial costs. The latter distinction is also theoretically important: While severance pay may be partly transferable to employees (“limited bonding”, Lazear, 1990), this becomes more difficult with judicial and administrative costs, which are more likely to be sunk costs to employers (Blanchard and Landier, 2002: 216). Venn (2009) shows that there is a high and statistically significant positive correlation between country rankings derived from the OECD indicators and alternative measures of employment protection, which are more limited in focus. However, the OECD indicator is preferable to the Heckman and Pages (2004) or Lazear (1990) indicator, since it represents a more comprehensive attempt to measure dismissal costs.

To my knowledge, the OECD indices represent the most comprehensive and transparent attempt to measure employment protection legislation. Also, I am not aware of another indicator measuring regulations on the usage of temporary contracts besides the OECD indicator. The OECD indices have received a high level publicity, are often used in social science research and have been subject to at least a decade of review by the scientific community. This has resulted in three major revisions and updates in 1999, 2004 and 2009 (OECD, 1999, 2004; Venn, 2009). Of course, the construction of indicators involves decisions that are to some extent arbitrary, but the

[25] The OECD index does consider the relevance of tenure for notification periods and severance pay. The corresponding items take the average for the corresponding regulations in case of 9 months, 4 years and 20 years of tenure.

current indicator is more comprehensive and captures more variation than those used by past research (Lazear, 1990; Heckman and Pages 2004).

The OECD indicator most likely also improves on the index published by Blanchard and Wolfers (2000), which is based on data from Lazear (1990) and the OECD (1999) and also uses the OECD coding methodology. The indicator is heavily interpolated and the authors have to make a number of other idiosyncratic choices to fill up gaps in the data. This indicator was later adopted by Bertola et al. (2007), in a rigorous, longitudinal study that supports the conventional view of distributional effects of employment protection legislation. However, the question remains whether these results hold up using better measurement of employment protection.

4.2. Preliminaries: Dependent variables and standard errors

4.2.1. Dependent variables

A basic question to answer at the outset concerns the nature of our dependent variables. Since we are interested in the distributional effects of employment protection legislation. Using relative labor market outcomes, such as youth-adult outcome ratios, as outcomes (or the difference of outcomes) is an intuitive solution and has been adopted in a number of studies (see, for example, Bertola et al., 2002; Jimeno and Rodriguez-Palenzuela, 2002; Esping-Andersen, 2000). Thus, we can model relative youth outcomes which provides a direct answer to the question whether employment protection makes youth relatively worse off, as in equation (1), where U_{jt}^{Y} denotes the logged youth unemployment rate and U_{jt}^{A} denotes the logged adult unemployment rate in country j at time t. δ is the coefficient of interest and D_{jt} a variable measuring employment protection. For ease of exposition, we omit other variables, except for a constant γ_0 and an idiosyncratic error term ε_{jt}.

(1) $U_{jt}^{Y} - U_{jt}^{A} = \gamma_0 + \delta D_{jt} + \varepsilon_{jt}$

This formulation has been motivated by the argument that the sharpest prediction we can theoretically derive about the effect of employment protection is on the distribution of (un)employment or relative outcomes (Bertola et al., 2002; Esping-Andersen, 2000). In Chapter 3, we raised concerns whether this theoretical argument is valid in case of youth unemployment. However, we nevertheless follow these authors here, in an attempt to relate our findings to published research. We can think of equation (1) simply as a model for relative youth unemployment. Looking more closely at the left hand-side, we can think of the adult unemployment rate in this model as a proxy variable for time-constant and time-varying factors that have an identical impact on both youth and adult unemployment (Bertola et al., 2007: 846). However, if employment protection affects both youth and adult unemployment, the adult unemployment rate may actually control away some of the effect we try to estimate. Moreover, to the extent that adult unemployment is correlated with both treatment and outcome variable, it may constitute a "bad control" (see discussion below), introducing rather than solving endogeneity problems. Equation (2) represents a more flexible specification of the same model:

(2) $U_{jt}^{Y} = \gamma_0 + \delta D_{jt} + \gamma_1 U_{jt}^{A} + \varepsilon_{jt}$

Now, we empirically estimate the relationship between the youth and the adult outcomes, instead of constraining this effect to be equal to unity, which we implicitly do in equation (1). In either case, we can interpret δ as the effect of employment protection on youth unemployment, net of whatever cyclical variation is captured by the adult unemployment rate. In this formulation, we can also think about replacing U_{jt}^{A} with other measures of cyclical variation, which we discuss below. While empirically more flexible, this formulation also has the inherent substantive advantage that it is a model of youth unemployment. Equation (1) leaves open, whether the effect of employment protection is generated by changing outcomes for young people or adults or both. While relative formulations (as in equation 1) have been frequently used in the literature (Esping-Andersen, 2000; Bertola et al., 2002; Kahn, 2007), they avoid a fundamental question that only a direct analysis of youth or adult outcomes can answer. In all analyses, we take the natural log of the dependent variable.

4.2.2. Standard errors

Another consequential decision we need to make concerns the estimation of uncertainty of our coefficient estimates. The key problem that results from our data structure, which has observations nested within countries and years, is that observations within the same country tend to be very similar on both dependent and independent variables. This within-country correlation of observations creates problems for unbiased and consistent estimation of standard errors. If unaccounted for, it leads to a strong downward bias in standard errors (e.g. Bertrand et al. 2004, Kezdi, 2003; Angrist and Pischke, 2009).

Different solutions have been used in applied research. The error structure is sometimes modeled with additional parameters that are supposed to capture clustering or serial correlation. For example, mixed or multilevel models are used to account for clustering of observations, or (unit-specific) auto-correlation parameters are estimated from the data to allow for serial correlations following a specific correlation structure. Alternatively, variance-covariance estimators that allow for arbitrary correlation of the error term within panel units (countries, in our case) have been proposed as a solution (see Bertrand et al., 2004 and Angrist and Pischke, 2009, for more discussion and suggestions).

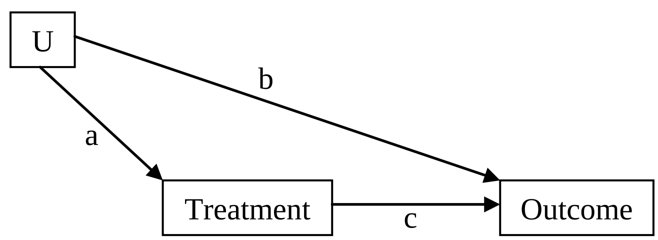
Bertrand et al. (2004) show for empirical situations similar to the one encountered here that parametric models for autocorrelation or serial correlation in the error term perform poorly and yield too small standard errors. They argue that parametric corrections should be viewed with caution unless the parametric model imposed on the data is close to the truth (see also Angrist and Pischke: Chapter 8). Without knowing the data generating process, the chosen parametric correction and the extra parameters we have to estimate are likely to be biased. Bertrand et al. (2004) show that a popular correction, especially the first-order autocorrelation model (AR1), generally does not succeed in remedying the serial correlation problem. This is true for the common as well as the cluster- or unit-specific AR1 correction (Bertrand et al., 2004: 264). Given that the AR1 correction is used frequently in analysis of aggregate data (see Bertola et al., 2007, for example), this casts doubts over the (statistically) significant effects that the authors obtain, in this case about the alleged positive effect of employment protection on youth unemployment.

Bertrand et al. (2004; see also Kezdi, 2003; Cameron and Trivedi, 2005) instead advocate the use of “cluster robust” standard errors as one alternative, i.e. variance-covariance estimators that allow for arbitrary correlation of the error term within panel units. Presence of correlation within panel units, which is very strong in our application, drastically increases estimated standard errors compared to conventional OLS standard errors. Unfortunately, the advantages of the cluster robust estimator also decline when the number of clusters (or countries in our case) diminishes, but the “cluster robust” estimator still performs better than the AR1 correction even with as little as ten clusters. Given that the number of clusters in our study varies between 15 and 21, Bertrand et al.’s (2004) results suggest that this correction still works well (see their Table IV, lines 4 and 6). Moreover, two factors may mitigate the serial correlation problem in our application, in particular in the differences-in-differences analysis using EULFS data. First, we use relatively short time series. Second, the control variables, in particular the linear time trends, may capture some of the within-unit correlation across time.

4.3. Conditioning and causal inference

From the perspective of causal analysis, our goal is to estimate the causal effect of an intervention in the labor market, in this case a reform of employment protection, on an outcome (see Morgan and Winship, 2007; Angrist and Pischke, 2009; Wooldridge and Imbens, 2009). Following conventional terminology, we will also refer to this intervention as a treatment, and to units (countries) exposed to treatment as treated units and units not exposed to treatment as control units. Our goal is to arrive at a statistical model, in which the association between exposure to treatment and outcome identifies the causal effect of the treatment on the outcome. Following Morgan and Winship's exposition, Figure 4.1 schematizes this issue.

Figure 4.1 Causal Diagram



We want to estimate the effect of a treatment (a change in employment regulation) on an outcome (for example, youth unemployment) in the presence of confounder U. Because U causes both treatment (via path a) and outcome (via path b), the effect of the treatment on the outcome variable (path c) is not identified unless we account for the influence of the factor U, which may be observed or unobserved. Causal inference is possible, however, if we can appropriately condition for control variables so that the effect of U (or many U's) on either the outcome or the treatment is "controlled away". If we can "block" (Morgan and Winship, 2007) either path a or path b, the treatment effect (path c) is identified, i.e. we can estimate the treatment effect in a simple regression framework and assign a causal interpretation to the corresponding regression coefficient.

Randomly assigning treatment status is the benchmark approach to achieve identification of causal effects, since randomization ensures that treated and control units do not systematically differ in their observed and unobserved U's. However, since employment protection reforms are not implemented in this fashion, we need to worry about the presence of unobserved or observed U's. In this context, we can think of two strategies to rule out the influence of U. We either use a set of control variables W that model treatment assignment and block path a. This approach underlies statistical matching analysis. The goal is to model assignment to the treatment status using control variables in order to "balance" the U's between treated and control groups, leaving no systematic differences between both groups.[26] Alternatively, we could use a set of control variables X that account for the direct influence of U on the outcome variables, and hence block path b. In the following, we propose a conditioning strategy using the control variables that essentially tries to accomplish both, adjusting for factors that determine treatment assignment as well as adjusting for causes of the outcome.

For both cross-sectional an longitudinal analysis, we employ an extensive set of control variables, which were selected both because they predict the outcome variables as well as treatment assignment. While only few quantitative studies exist that discuss the causal factors behind the assignment of employment protection regimes (for example Rueda, 2005, 2007; Botero et al., 2004), there has been much research on the determinants of aggregate employment outcomes (for example, Blanchard and Wolfers, 2000; Baccaro and Rei, 2007; Bertola et al.,

[26] Instrumental variable analysis, by relying on exogenous variation in treatment assignment, can be similarly interpreted as an attempt to rule out an effect of U via path a. While instrumental variable specifications have been suggested to estimate the effect of employment protection (Allard and Lindert, 2006), we could not find credible instruments in this application. For an interesting approach to identify institutional effects in the cross-section using instrumental variables, see Acemoglu et al. (2002).

2007). From this research, we distilled a list of control variables (see Data Appendix at the end of this chapter).

While representing a fairly comprehensive set of control variables typically used in this field of research, they may not fully capture the unobserved confounders. Published research has often entered them as linear effects into regression models and often implicitly or explicitly claimed to achieve identification of causal effects in this way. However, we can use the information contained in the control variables more effectively. Following the logic of matching analysis, we can try to detect the set of W control variables that model treatment assignments and attempt to balance the data in this way. In particular, in addition to the main effects of the control variables, we add "powerful" quadratic/interaction terms generated from the control variables that predict treatment assignment (the choice of employment protection regime). We are not interested in interpreting these interactions/non-linear terms, because their sole purpose is to balance the data.

To select the quadratic/interaction terms, we use a data driven approach. Based on Imbens' and Rubin's (2008) suggestions for specifying a propensity score model, we propose an algorithm that selects all "powerful" quadratic and interaction terms generated from the control variables that predict treatment assignment. When estimating the effect of employment protection legislation, we then control for these quadratic/interaction terms to explicitly capture some additional factors unaccounted for by the conventional linear specifications. If these non-linear/interaction terms matter, omitting them from the specification would constitute a specification error. How exactly the algorithms were designed is discussed in the following section.

Implicitly, we try to accomplish the same goal as in propensity score matching analysis, but without estimating and adjusting for a propensity score. Instead, we use the variables that predict treatment status (or treatment intensity, since our treatment variable is continuous) and include them directly in the outcome equation. Angrist and Pischke (2009: 86) point to the similarity between first estimating than adjusting for the propensity score and regression model that uses a flexible specification of the variables used to model at the propensity score. Instead of estimating a balancing score and then conditioning on it, we directly control for the observed covariates used to predict the treatment variables and therefore attempt to block path a in Figure 4.1 above.

We also apply the same reasoning, when modeling the U's that affect the outcome (path b in Figure 4.1). Again, published research usually constrains the effects of control variables to be linear, which is not plausible. For example, institutions modify the impact of macroeconomic shocks, but institutions may also interact with each other. Indeed, a number of contributions have focused on precisely these questions (Blanchard and Wolfers, 2000; Bassanini and Duval, 2006). Unfortunately, published research is a poor guide in detailing the functional form of control variables and which interactions to include, particularly when studying the youth labor market (or other specific demographic group). Instead one strategy has been to use nonlinear least squares, where sets of institutions are allowed to interact with many observed macroeconomic variables (e.g. Blanchard and Wolfers, 2000; Jimeno and Rodriguez-Palenzuela, 2002), leading to the estimation of a large number of parameters while avoiding any statement about just which interactions/nonlinearities matter.

Similar to modeling treatment assignment using additional non-linear terms, we therefore also use a data-driven approach to detect non-linear terms that predict the respective outcome variable (path b in Figure 4.1). If these non-linearities are present and we omit them from the model, we commit a specification error. The risk of this approach is that we include an irrelevant term in the model that is just significant by chance. Essentially, we are caught between two potential sources of specification error. An advantage of the data-driven approach is that it reduces the opportunities for "partisan model selection" (see Ho and Imai, 2007, and Young, 2009, for a discussion of the pitfalls of model dependence), and replaces the subjectively preferred specification with a specification that is determined on the basis of objective criteria. Because we let an algorithm decide about the functional for of the control variables, this precludes specification searches on the basis of the treatment effect. "Partisan model selection" is a particular concern in this setting, because we deal with relatively small sample sizes, in which robustness to specification changes is a considerable problem and gives a lot of room that researchers may exploit to arrive at a preferred result. By delegating the decision about how the functional form of the control variables to an algorithm, we effectively preclude some model selection on the basis of estimated treatment effects.

Finally, while Imbens and Rubin (2008) recommend a partly data-driven approach to balance the vis-à-vis the treatment variable, applying the same procedure vis-à-vis the outcome variable may seem questionable. However, repeating the analysis where we just include the non-linear terms that predict the treatment variable (and omit the non-linear terms that predict the outcome variable) yielded virtually identical results. In the following analyses, we generally use both sets of non-linear terms.

4.4. Cross-sectional analysis

For the cross-sectional analysis, we estimate multivariate OLS regression models of the following form:

(3) $U_{jt}^{Y} - U_{jt}^{A} = \gamma_0 + \delta_1 epr_{jt} + \delta_2 rtc_{jt} + X_{jt}'\beta + \varepsilon_{jt}$,

where U_{jt}^{Y} represents the (natural log) youth and U_{jt}^{A} is the (natural log) adult unemployment rate in country j and year t. γ_0 is a (common) intercept, $X_{jt}'\beta$ is a vector of control variables and ε_{jt} is an i.i.d. error term. By differencing the left hand side in equation (3), we implicitly control for country- and year-specific shocks that affect youth and adults symmetrically (Bertola et al., 2007: 846), i.e. that they change youth and adult unemployment by the same amount. The coefficients of interest are δ_1 and δ_2, which measure the association between (relative) youth unemployment and the measures of employment protection, i.e. dismissal regulations on permanent contracts (epr_{jt}) and regulations on the usage of temporary contracts (rtc_{jt}). For example, a one unit change in the *rtc* index would be associated by a change in the difference between youth and adult unemployment rates by δ_2 percent. Given the sensitivity of results to specification changes, we report the results of several control variable specifications.

4.4.1. Conditioning strategies for cross-sectional analysis

We propose three different specifications to control for the influence of potential confounders in the cross-sectional analysis. Apart from the measures of employment protection legislation, the "basic specification" contains the k control variables (listed above) as linear terms as well as year fixed effects. The "basic specification" is a point of reference, since it represents the workhorse of much published research in this area. The "full specification" adds to the "basic specification" all interaction/quadratic terms that predict significantly the outcome variable or either measure of regulation. The "restricted specification" only adds the most powerful interaction/quadratic terms from the "full specification". In addition, we report a bivariate specification that contains only the measures of regulation as regressors. While much published research relies on variants of the "basic specification", i.e. specifications that are linear in the control variables, we prefer the "restricted specification" that permits nonlinearities in the control variables.

The intuition behind the algorithm used to select the nonlinear/interaction terms is derived from Imbens and Rubin (2008: Chapter 11) who propose a data-driven algorithm to estimate the propensity score for statistical matching analysis. Their goal is to achieve in-sample balance of the treated and control groups with respect to the pre-treatment control variables using a flexible propensity score specification that was obtained using an algorithm similar to the one described below. They caution against relying on automatic procedures only, and encourage the use of substantive information to arrive at propensity score specifications. We follow their advice in selecting control variables on this basis, considering explicitly those that matter for treatment assignment.

To "balance" the data on the observed control variables, we use the following algorithm for each of the regulation measures and the outcome variable (adapted from Imbens and Rubin 2008, Chapter 11): Using k control variables, we generated all possible k*k interaction/quadratic terms. The algorithm begins by running k*k OLS regressions of the outcome variable on the k control variables and year fixed effects (starting specification), adding just one of the k*k quadratic/interaction terms at a time and recording their respective t-statistic. The term with the highest absolute t-value is then added to the starting specification of k control variables and year fixed effects. The algorithm continues by running k*k-1 regressions, testing, one at a time, the k*k-1 remaining quadratic or interaction terms. The one with the largest t-value is again added to the base specification. The algorithm continues until, after running k*k-m regressions, the term with the highest absolute t-value fails to exceed an a priori set critical value, which we set corresponding to the 5% significance level (two-tailed test). We thus obtain a set of m interaction and quadratic terms. We run this algorithm for each of the measures of regulation, as well as for the respective outcome variable, obtaining three (potentially overlapping) sets of interaction and quadratic terms. After removing duplicates, we enter all three sets as control variables into the "full specification", containing p interaction/quadratic terms. The full specification then includes the (main) linear effects of the k control variable, the p interaction/quadratic terms, year fixed effects, and the two measures of regulation.[27]

Based on the "full specification", we also propose a "restricted specification" that only uses a subset of the interaction/quadratic terms used in the "full specification". We again use an algorithm that selects the strongest interaction/quadratic terms from the set of interaction/quadratic terms arrived at in the "full specification". The algorithm begins by running p OLS regressions of the outcome variable on a starting specification which contains both measures of employment protection legislation, the k control variables, year fixed effects, adding one of the p interaction/quadratic terms that were determined according to the procedure used to arrive at the "full specification". We record the t-statistic of each interaction/quadratic term; the interaction or quadratic term with the strongest t-statistic enters the starting specification, and we repeat the exercise with the remaining p-1 interaction/quadratic terms. We keep on adding interaction/quadratic terms until the "strongest" remaining interaction/quadratic term fails to exceed an a priori specified value, set at the 5% significance level (two-tailed test).

The goal of the extended specifications here is to maximally use the information contained in the control variables to balance the data vis-à-vis both treatment and outcome, and hence to block both paths a and b in Figure 4.1. However, we may not succeed, if we lack an important control variables, if the control variables are poorly measured or if they induce rather than solve endogeneity issues (for a discussion of "bad" control variables, see Angrist and Pischke, 2009: 64; Morgan and Winship, 2007: Chapter 3).[28]

[27] When running the algorithm for the outcome variable, we include the measures of regulation among the controls, but we do not include or test their interactions and quadratic terms, since such "effect heterogeneities" should be part of the substantive analysis.

[28] We excluded productivity growth as a control from the outset, since here the endogeneity/simultaneity is relatively obvious: Young workers are inherently less productive than older workers, and should employment

Moreover, some may be suspicious that at the aggregate level many of the control variables become associated in a process of mutual causation. Moreover, the measures may not capture well the actual processes at the level of individual actors. Taking this stance to its extreme, we may be generally suspicious about the conditioning strategy based on observed control variables in an aggregate data analysis. In the absence of a source of exogenous variation in employment protection (which we do not have), this stance would make causal inference from aggregate data a lost cause from the outset, a position we would consider too extreme.

A central pitfall of cross-sectional analysis is that we may be left with remaining unobserved U's that our measured covariates fail to account for. Given that we dispose of longitudinal data, we can account for all unobserved time-constant factors that are unique to a particular unit or groups, which is accomplished in fixed effects analysis (Wooldridge, 2002). Identification then hinges on the assumption that there exist no unobserved, time-varying confounders. In the context of differences-in-differences (DiD), this assumption is often stated as "parallel counterfactual trends": We have to assume that the trends in the dependent variable for both treated and control group would have been the same in the absence of treatment. We discuss this method, as well as available strategies for conditioning on control variables in the following sections.

4.4.2. Cross-sectional versus longitudinal analysis

Even though cross-sectional analyses are principally problematic from the perspective of causal inference, they have several merits.[29] First, cross-sectional analyses allow us to test simple

protection lowers youth relative to adult employment it would naturally raise aggregate productivity (Gordon and Dew-Becker, 2008).

[29] We use the term “cross-sectional analysis” for analyses that do not control for country or group fixed effects, i.e. analysis that rely on variation both across countries and across time to estimate the association between employment protection and labor market outcomes. “Longitudinal analyses” control for country or

descriptive hypotheses that are frequently found in the literature, such as that strict dismissal regulations on regular employment contracts are associated with higher youth unemployment. We can test whether such associations are at all present in the data and robust to the inclusion of different sets of control variables. Second, cross-sectional analysis uses more information, both longitudinal and cross-sectional, in estimating the associations. When focusing only on longitudinal data, we sacrifice a lot of variation. Using the OECD dataset, country fixed effects soak up 95% percent of the variation in the regulation of permanent contracts and 80% of the variation in the regulation of temporary contracts. This information loss is drastic, and one may question the merits of fixed effects analysis in this case (see discussion in Angrist and Pischke, 2009). However, at the very least, adjusting for fixed effects represents an important specification check if we have longitudinal data available. If we obtain a clear and highly significant association in the cross-section, and controlling for fixed effects yields a similarly signed, but weaker effect that falls just beneath conventional thresholds for statistical significance, this may be judged as inconclusive. We may just lack enough longitudinal variation, or whatever longitudinal variation we have may be too noisy, to identify the effect of interest. However, if the associations remain statistically significant but switch sign, for example, when they turn from positive significant to negative significant, we interpret this as evidence that the cross-sectional results do not hold.

Finally, a number of influential studies on this issue (Esping-Andersen, 2000; OECD, 2004; Breen, 2005; Polavieja, 2006; Kahn, 2007) fully rely on inferences from cross-sectional data. We therefore begin our analyses by using a more recent and extensive database to repeat some of the published cross-sectional analyses. This allows us to assess whether past results are robust to

group fixed effects and therefore only consider longitudinal variation in the data, disregarding all cross-sectional variation across units.

changes in data sources, control variables, unit-specific fixed effects (i.e. country fixed effects). Ultimately, it gives us an idea, whether we should trust cross-sectional analyses of the effects of employment protection legislation on the distribution of labor market outcomes.

4.5. Differences-in-Differences (DiD)

While cross-sectional data is sometimes the only data available, this situation is no longer true for the specific questions we study here. We therefore consider it important to adopt a rigorous longitudinal approach, using differences-in-differences, to obtain more credible estimates of the causal effects of employment protection legislation. An important gain from using DiD is conceptual in nature, because we phrase our analytical question in terms of a timed intervention, a treatment, and observations exposed to treatment and control observations. By tying in with recent literature on counterfactual causal inference, this approach improves upon standard fixed effects regression analysis (e.g. Bertola et al., 2007), where removing fixed effects and adding linear control variables is considered sufficient to achieve identification.

A reform in employment protection legislation usually occurs as a discrete, economy-wide change in the legal environment. A growing number of studies have used such discrete policy changes to assess the consequences of reform for the labor market (Autor et al., 2006; Kugler et al., 2005). In this setting it is natural to assess the effect of reform by comparing outcomes between "treated" and "control" observations before and after the reform. More precisely, one compares the before-after difference in the treatment group to the before-after difference in the control group. By subtracting these two differences, we obtain the differences-in-differences (DiD) estimator of the effect of reform.

Because it compares changes over time within the same units, DiD is as immune to bias as fixed effects estimators (Angrist and Pischke, 2009). By including unit-specific fixed effects, we can effectively rule out all time-invariant unobserved factors that may otherwise lead to omitted variable bias. In addition, we also eliminate biases that are due to trends in the outcome variable that are *common* to both groups.[30] One crucial assumption we still need to make is that the trends in the outcome variable would have been the same for both groups in the absence of treatment. This assumption is implausible if treatment and control group differ with respect to unobserved variables that generate trends in the dependent variable (Ashenfelter and Card, 1985). We try to address this issue by using different control variable specifications (see below).

In this application, we use data from the European Union Labor Force Survey, where we group individuals by country, year, gender and education group, restricting the sample to young people, and calculate the average of the outcome variable for each group. We thereby obtain six observations per country and year (two genders times three education groups). With data on groups g nested in j countries and t time periods, we can obtain the differences-in-differences estimate of the effect of a binary policy change on the (log) outcome variable Y_{gjt} with the following linear model, which can be estimated with Ordinary Least Squares (OLS):

(4) $Y_{gjt} = \mu_g + \lambda_t + \delta D_{jt} + \varepsilon_{gjt}$,

where μ_g represent group fixed effects (country fixed effects would suffice), λ_t are time fixed effects (year dummy variables) and D_{jt} represents a dummy variable equal to 1 in countries and years affected by a policy change and zero otherwise. δ is the DiD estimate of the reform.

Following Autor et al.'s (2006) study, we enrich this specification in several ways. The key challenge we face given our data structure is that units experience treatment in different years and some units experience treatment repeatedly. We therefore do not use all observations before and all observations after a change in regulation to estimate the effect of a reform. Instead, we focus

[30] The same can be achieved by including common time effects in a standard fixed effects regression model.

on the change around the year in which the reform is implemented, i.e. two years before (t-2) to three years after (t+3) the change in legislation occurred.[31] We will test different specifications for this window of observation, which allows us to assess the dynamics of the impact of the reform. At the same time, this specification implies that a country experiencing reform may contribute both treatment and control observations. To allow for a post-treatment period before a treated country can contribute "regular" control observations, we add a post-treatment dummy variable. Third, we exploit our knowledge about the intensity of reform, as measured by the respective change in the OECD index, and pool positive and negative changes in regulation into one indicator of treatment intensity. We thus arrive at the following specification.

(5) $Y_{gjt} = \mu_g + \lambda_t + \gamma_{1j} Treat_{jt} + \delta Dif_{jt} + \gamma_2 Post_{jt} + \varepsilon_{gjt}$,

where μ_g and λ_{t} are group and year specific fixed effects. $\gamma_{1j} Treat_{jt}$ models baseline outcomes for treated countries, where $Treat_{jt}$ switches from 0 to 1 for each reform for the duration of the specified window of observation (the default being t-2 to t+3, reforms are implemented at t=0). We estimate this baseline outcome separately for each reform. Dif_{jt} is the change in the index of employment protection legislation for a specific reform. Dif_{jt} is the difference in the corresponding OECD index I_{jt} calculated as $Dif_{jt} = I_{j,t=0} - I_{j,t=-1}$. Dif_{jt} takes on this value for the period from t=0 to t=3 following reform, and is equal to zero otherwise. The coefficient of interest δ estimates the deviation of the outcome variable from the country and reform specific baseline outcome over the period t=0 to t=3, as a function of the direction (negative/de-regulation or positive/up-regulation) and magnitude of change. We will test for heterogeneity of this coefficient across gender, education groups and institutional conditions.

[31] This should also diminish the problem of serial correlation (see below).

$Post_{jt}$ is a dummy variable that takes the value 1 for countries that experienced reform for the period from t=4 to t=5 in the default specification.

In contrast to our design, most DiD studies identify the DiD effect with a dummy variable as in equation (4). Similar to equation (4), we assume that the DiD effect is linear additive, but we also model observed heterogeneity in the treatment effect (Card, 1992). We model this heterogeneity by using the information contained in the OECD indicators of employment protection. In particular, we assume that all reforms can be mapped into movements in either direction of a uni-dimensional scale ranging from 0 (highly liberal) to 6 (highly regulated).

4.5.1. Conditioning strategies for differences-in-differences analysis

Whether we can assign a causal interpretation to δ depends, among other things, on the assumption that trends in the outcome variable for treated and non-treated units would have been the same in the absence of treatment. We use two conditioning strategies to make this assumption more credible. First, we use the "restricted specification" as discussed, which adds a linear set of control variables, including the non-treatment measure of employment protection[32], as well as a set of interactions generated from the control variables that are significant predictors of outcome and treatment variable. This specification attempts to achieve balance across units affected and not affected by employment protection reform in terms of observed time-varying covariates.

As an alternative, instead of using covariates, we simply add country-specific linear trends. This specification is an intuitive way of accounting for unit-specific, non-parallel trends, but risks controlling away part of the treatment effect if the treatment generates the underlying trend. Furthermore, we add the respective adult outcome variable to the country-specific linear trend

[32] For example, if we assess the effect of a reform in the regulations on temporary contracts, we use the indicator of employment protection on regular contracts as a control variable.

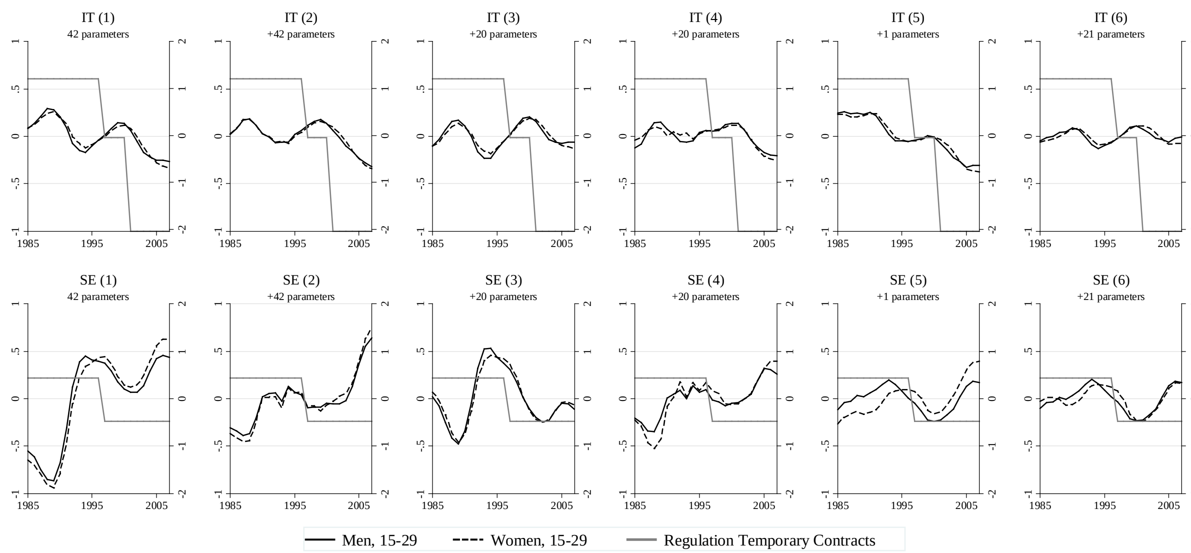
specification (Angrist and Pischke 2009: 233). The adult outcomes variable may succeed in capturing short run cyclical variation, while the linear trend captures longer running trends in the dependent variable, which may have already been present in the data, before the treatment occurred.

To illustrate the relative success or failure of different conditioning strategies, Figure 4.2 graphs the smoothed residual from a regression of log youth (ages 15-29) unemployment using different control variable specifications (21 OECD countries, n=436). We graph the results for Italy and Sweden, which both experienced deregulations of the usage of temporary contracts. Specification (1) includes country and year fixed effects only. In (2), we add country-specific macroeconomic shocks.[33] In (3), we add to (1) country-specific linear trends. (4) is the "restricted specification" adding to (1) control variables and some interaction/quadratic terms. In (5), we add to (1) the (logged) unemployment rate for adult men, aged 45-54. In (6), we add to (1) country-specific linear trends and the (logged) unemployment rate for adult men, aged 45-54.

After eliminating country and year fixed effects (specification 1), the residuals still show both cyclical variation and a trend. In Italy we observe a recovery from very high values in the early 1980s, while in Sweden we observe the consequences of the economic and financial crisis in the early 1990s. In either case, the 42 parameters (year and country fixed effects) included in model 1 unfortunately leave considerable cyclical variation and country-specific trends unaccounted for. If we stopped here, fixed effects regression analysis would suggest that deregulation has made young people better off in Italy and worse off in Sweden, while clearly the results seem to be driven by strong underlying trends in the dependent variable.

[33] We fit for each country two interaction terms between the country fixed effects and two measures of macroeconomic changes, the output gap and the inflation rate.

Figure 4.2 The effect of using different conditioning strategies, youth unemployment



Source: OECD Labor Force Statistics (own calculations)

Adding country-specific shocks in specification (2), we fit a model with altogether 92 parameters, but do not seem to accomplish very much, as considerable cyclical variation and trends remain. Specification (3) succeeds in removing the trend from the data, which is encouraging. Specification (5), which only adds the logged adult unemployment rate to specification (1), seems to capture some cyclical variation, but a trend remains in the Italian data. Using both country-specific trends and the logged adult unemployment rate, we succeed in both removing trend and some cyclical variation, but some cyclical variation also remains. Compared to this, specification (4), which is the "restricted specification", seems to yield comparable results using a similar number of extra parameters.

Generally, we would expect that the linear trend specification is more credible for shorter time series, as in the EULFS data set, which we rely on in the differences-in-differences analysis, comprising maximally 15 years per country. Longer time series may require non-linear trend specifications, which demand a lot more parameters to be fit to the data. Nonetheless, we should be wary about the remaining cyclical variation in the data. In either specification, if we were to only consider the two years before and two years after the Swedish reform, we may erroneously attribute an effect to this reform, which is really due to unaccounted macroeconomic factors. An important specification check will therefore be not to consider the immediate before and after observations, but extend the window of observation to include more pre- and post-reform observations.

4.5.2. Which results do we present?

At last, we want to briefly discuss how we decide which results to present in the DiD analysis. Quantitative research of this kind suffers from a huge information asymmetry between the amount of regressions (or other statistical) models that researchers estimate, the results of these models and what is eventually selected for publication (Young, 2009). Researchers can never show all specifications, but should conduct sensible specification checks and try to specify clear rules according to which they have selected the final models for publication.

Following the specification labels in the previous section, we routinely estimate the basic DiD specification, as well as three specifications using control variables, one containing country-specific linear trends, one containing country-specific linear trends and the adult outcome, and one specification containing linear control variables as well as non-linear terms generated from the control variables ("restricted specification"). Because of space limitations, we will only report the "restricted specification" based on control variables and the specification including country-specific linear trends and the adult outcome variable. If we obtain conflicting results in the more basic specifications, this is mentioned in the text. In any case, if we find an effect in the basic specification, the crucial question is whether this effect survives adjustment for confounding factors using either conditioning strategy. For example, if adjusting for country-specific linear trends would destroy the effect found to be present in the basic DiD specification, we would have to conclude that we cannot rule out that the results are generated by non-parallel trends in the treated and control countries (Angrist and Pischke, 2009). While both the "restricted" and the trend specifications are plausible, each has its drawbacks and we would hope for consistent results across the specifications.[34]

The DiD setup used here requires us to chose a window of observation around the reform, within which we estimate the effect of the reform. The default window chosen starts two years before and ends three years after reform (as in Autor et al., 2006), but we also routinely re-estimate all analysis with a larger window, starting three years before and ending five years after reform. This allows us to assess the sensitivity of the results, but also indicates whether effects grow stronger or weaker if we consider a longer time period. We do not report the results, but refer to them in the text in case they differ from the findings of the default observation window specification.

Another specification check we routinely conduct is to drop one country at a time, and re-run the analysis. We perform this check on both the trend and the control-variable based specification (“restricted specification”). As we have shown in Section 3.1 (p. 44f.), some countries implemented reforms that according to the corresponding OECD indicator were much more extensive than reforms in other countries. Also, some countries have witnessed more than one reform. In particular, Spain (on the indicator for job security provisions on regular contracts) as well as Belgium and Italy (on the indicator for regulations on temporary contracts) may be

[34] We refrain from using lagged dependent variable specifications (see discussion in Angrist and Pischke, 2009). Using unit fixed effects is a substantial advantage in this design, and adding lagged dependent variables, while principally possible, requires instrumental variables strategies and further assumptions about the error term for identification. In particular, the presence of serial correlation in the error, which clearly is an issue in the type of data we use here, then threatens identification.

outliers. In any case, we would not want our results to depend solely on the presence of one country. If dropping any particular country destroys an effect, we should conclude that there is no general reform effect. There may be an effect in the outlying country, but not a general effect, the detection of which is the goal of our analysis.

4.6. Data appendix

4.6.1. Dependent variables

We relied on two data source for labor market outcomes. The OECD data comes from the online OECD Labor Force Statistics database, which provided data on unemployment, employment, labor force participation, and populations size broken down by gender and age groups, as well as data on temporary employment for youth (15-24 year olds) and adults (25-54), also broken down by gender. Data on tertiary enrolment is taken from the World Bank (World Development Indicator Database, 2010). Tertiary enrolment rates are gross enrolment rates, i.e. the number of students enrolled in tertiary institutions divided by the population at the typical age for tertiary students, which is defined according to the country-specific situation. The key database for the differences-in-differences analysis is the European Union Labor Force Survey (EULFS, Eurostat 2005), which we augment with IPUMS-CPS data for the U.S. (1992-2006) and data from the German Microcensus (1996-2005).

Table 4.1 displays the countries and years covered for each of the data sources considering unemployment as an outcomes. For temporary employment, the OECD data has more gaps. For the augmented EULFS dataset, we also recorded the total number of observations that went into the calculation of the youth labor market outcomes for multivariate analysis. The information on the year coverage refers to the situation after dropping individual years because of missing information on any of the control variables. In particular, we lose observations from Greece before 1997, because of a lack of data on interest rates. In the differences-in-differences analysis, some specifications do not use control variables, for which we gain back these observations.

Table 4.1 Data coverage OECD and EULFS

	OECD, available years	EULFS, available years	EULFS, sample size
Australia	1985-2007		
Austria	1994-2007	1995-2006	139,472
Belgium	1985-2007	1992-2006	150,525
Canada	1985-2007		
Denmark	1985-2007	1992-2005	46,314
Finland	1985-2007	1995-2006	89,998
France	1985-2007	1993-2006	370,116
Germany	1991-2007	1996-2005	260,198
Greece	1997-2007	1997-2006	184,231
Ireland	1986, 1991, 1994-2007	1994-2006, no data 1998	320,800
Italy	1985-2007	1992-2006	551,300
Japan	1985-2007		
Netherlands	1987-2007	1996-2006	169,568
New Zealand	1990-2007		
Norway	1985-2007	1996-2006	56,083
Portugal	1985-2007	1992-2006	139,706
Spain	1985-2007	1992-2006	464,571
Sweden	1985-2007	1995-2006	136,834
Switzerland	1991-2007		
United Kingdom	1985-2007	1992-2006, no data 1998	276,028
United States	1985-2007	1992-2006	480,520

Notes: OECD and EULFS data coverage for unemployment rates, no data on temporary employment for U.S. in augmented EULFS dataset.

4.6.2. Independent variables

To obtain a dataset that is as complete, comprehensive and up-to-date as possible, we had to impute and extrapolate some information, in particular for observations in the 1980s and in the year 2007. This particularly affects the analyses relying on the OECD data, but the analyses based on the EULFS data are, with two exceptions, not affected at all. In the following, we list information on definition and calculation of all independent variables used in the analyses, as well as their sources.

Employment protection regular contracts (EPR)

OECD index of regulations governing dismissal off individual workers with regular contracts. The index used here is based on eight individual items: procedural inconveniences employers are facing upon starting the dismissal process (notification procedures, delay involved before notice can start, length of notice period), severance pay, definitions of justified or unfair dismissal, length of trial period that is exempt from employment protection provisions, and repercussions for employers in case of unfair dismissals (compensation, right to reinstatement). All items are coded into numerical scores varying between 0 and 6, weighted and aggregated. The resulting index varies from 0 (min) to 6 (max).

Source: OECD Indicators of Employment Protection, downloaded October 3, 2009 from www.oecd.org/employment/protection

RTC - Regulations on the usage of temporary (fixed-term) contracts

Subindex of the OECD index on regulation. The index is based on three items: valid reasons for using a temporary (rather than a regular) employment contract, maximum number of successive temporary contracts with the same employer, and the maximum cumulated duration of temporary contracts with the same employer. The items are coded into numerical scores varying between 0 and 6, weighted and aggregated. The resulting index varies from 0 (min) to 6 (max).

Source: OECD Indicators of Employment Protection, downloaded October 3, 2009 from www.oecd.org/employment/protection

Generosity of unemployment benefits (GenRR),

Definition: OECD summary measure of generosity of unemployment benefit entitlements (gross replacement rates), average of the gross unemployment benefit replacement rates for two earnings levels, three family situations and three durations of unemployment.

Note: Data available only for uneven years. Data for missing years imputed as the average of the two adjacent years.

Source: OECD Tax-Benefit models, downloaded October 20, 2009 from http://www.oecd.org/els/social/workincentives

Expenditure on active labor market policies (ALMP)

Definition: Ratio of total public expenditures on active labor market policies divided by the gross domestic product. Data expressed in percentages.

Missing: Greece 2006/7, imputed the value from 2005; Denmark 1985, imputed the value from 1986; Italy 1985-9, imputed the average value for the period 1990-4; Japan 1985-9, imputed the average value for the period 1990-4.

Source: OECD.Stat Online Database ("Social and Welfare Statistics - Social Protection - Social Expenditure - Aggregated data"), downloaded October 28, 2009 from http://stats.oecd.org/

Trade union density (TUD)

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

Note: Value for Portugal 2006 taken from Visser (2009).

Missing: 2007 data for Austria, Spain, Portugal, New Zealand and Switzerland. 2006 for Greece, 2005 for Portugal. Missing values imputed by linear extrapolation.

Source: OECD.Stat Online Database ("Labour - Trade Union"), downloaded July 1, 2009 from http://stats.oecd.org/

Coordination of wage bargaining (Wage1)

Index, varying from 1 (no coordination) to 5 (economy-wide bargaining). 5 = economy-wide bargaining, based on a) enforceable agreements between the central organizations of unions and employers affecting the entire economy or entire private sector, or on b) government imposition of a wage schedule, freeze, or ceiling; 4 = mixed industry and economy-wide bargaining: a) central organizations negotiate non-enforceable central agreements (guidelines) and/or b) key unions and employers associations set pattern for the entire economy; 3 = industry bargaining with no or irregular pattern setting, limited involvement of central organizations and limited freedoms for company bargaining; 2 = mixed industry- and firm level bargaining, with weak enforceability of industry agreements; 1 = none of the above, fragmented bargaining, mostly at company level.

Name of variable in source data: WCoord

Source: Visser (2009), ICTWSS Database, Version 2

"Hump-shaped" wage bargaining indicator (Wage 2)

Recoded from Wage1 (Coordination of wage bargaining) so that the resulting indicator increases as the level of bargaining moves towards the industry level. The resulting index varies from 0 (economy-wide / company-level bargaining) to 2 (industry-level bargaining). 0 = 1 or 5 on Wage1 (economy-wide or company-level bargaining), 1 = 2 or 4 on Wage1, 2 = 3 on Wage1 (industry-level bargaining).

Cabinet power of left parties (Left)

Definition: Schmidt-Index of cabinet composition. The index varies from 1 to 5, where 1 = hegemony of right-wing (and centre) parties, 2 = dominance of right-wing (and centre) parties, 3 = balance of power between left and right, 4 = dominance of social-democratic and other left parties, 5 = hegemony of social-democratic and other left parties.

Missing: Italy 1995 (caretaker government), imputed a value of 1, since no left/social democratic parties were represented.

Name of variable in source data: govparty

Source: Armingeon et al. (2006), Comparative Political Data Set 1960-2007

Product market regulations (PMR)

Index for regulatory conditions in seven non-manufacturing sectors: airlines, telecommunication, electricity, gas, post, rail, and road freight. The indicator used here is also referred to as ETCR (energy, transport and communication) indicator. The ETCR indicator has a pyramidal structure, where individual items measuring aspects of anti-competitive regulations (for example, barriers to entry, public ownership and price controls) in different sectors are normalized over a scale ranging from 0 to 6, weighted and aggregated. The resulting indicator varies between 0 (minimum) and 6 (maximum).

Note: Some of the sub-items contained missing data for specific countries, which were imputed using linear interpolation.

Source: Conway and Nicoletti (2006)

Dual System

Definition: % of students enrolled in upper and post-secondary (ISCED 3-4) programs that combine school-based vocational education with workplace-based training.

Note: Only available from 1996 onwards. Codes n (negligible amount) and a (not applicable) were assigned a value of 0. Data for Norway 1996 taken from OECD (1998).

Missing: no data for 1997, imputed the average of adjacent years for each country. Missing data for Ireland 1998-2004, the Netherlands 1999 and 2001, Norway 1998-2004. Imputed by linear interpolation.

Source: OECD Education at Glance, various issues.

Economic globalization (Glob)

Summary indicator 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), capital account restrictions

Source: Dreher (2006)

Real interest rate

Definition: Nominal returns on long-term government bonds minus the inflation rate for the following year

Missing: Greece did not issue comparable bonds before 1997.

Source: OECD Economic Outlook December 1998 Dataset, OECD Economic Outlook December 2009 Dataset, downloaded March 31, 2010 from http://stats.oecd.org/

Change in inflation rate

Yearly changes in the consumer price index (CPI), calculated for each country as $CPI_t - CPI_{t-1}$

Source: OECD Reference Series, downloaded March 31, 2010 from http://stats.oecd.org/

Terms-of-trade shocks

The log-difference of the terms of trade multiplied by trade openness. Trade openness is defined as the ratio between import plus exports divide by GDP (at constant prices).

Source: Imports/exports from OECD International Trade (MEI) Dataset, trade openness from OECD Macro Trade Indicators Dataset, downloaded March 31, 2010 from http://stats.oecd.org/

Youth Cohort Size

Flexibly defined as ratio of young people divided by the number of adult men, aged 45-54. The denominator corresponds to the group, whose labor market outcomes are being analyzed and is calculated from the corresponding data source on labor market outcomes.

Output Gap

Definition: the difference between actual and potential gross domestic product (GDP) as a per cent of potential GDP

Source: OECD Economic Outlook December 2009, downloaded March 31, 2010 from http://stats.oecd.org/

5

Employment Protection and Youth Unemployment

In Chapter 3, we argued that the effect of employment protection legislation on youth unemployment is theoretically indeterminate. Job security provisions on regular employment contracts diminish both inflows and outflows from youth unemployment. It is an empirical question, which of these mechanisms dominate, or whether they cancel each other out to yield no effect at all. Apart from the effect on unemployment, we will also assess the effect of job security provisions on youth labor force participation and employment in this chapter. To the extent that job security provisions make the transition from school to work more difficult, young people may anticipate such difficulty and delay their entry into the labor force. Compared to adult workers, young people may have lower economic opportunity costs to staying out of the workforce, especially if they spend the time out acquiring higher levels of education in an attempt to improve their future career chances. Regarding employment effects, it may be the case that by hindering reallocation of labor and delaying necessary adjustments, job security provisions depress productivity, which should translate into lower labour demand (Blanchard and Katz, 1997; Bassanini and Duval, 2006). This effect may manifest itself particularly in terms of lower employment levels.

For each outcome, we will also test whether the effect varies by levels of education and gender, although it is again difficult to make clear cut predictions. Less skilled in particular may be priced out of the labor market by high dismissal costs, but compared to more educated young people, they also have relatively low opportunity costs for acquiring further education and are therefore more likely to drop out of the labor force. Highly educated may suffer, since it is particularly risky to hire a high skilled worker, if dismissal costs are high (Wolbers, 2007). Thus, to the extent that job security provisions lead to a more difficulty transition, we may expect that particularly less educated are more likely to withdraw from the labor force.

Constraints on using of temporary contracts were lifted particularly in countries with strict regulations on dismissal from permanent jobs. However, the relationship between unemployment and regulations on or the incidence of temporary employment is theoretically ambiguous. "Partial deregulation", i.e. removing restrictions on the usage of temporary contracts while maintaining extensive job security provisions on regular contracts, may have effectively lowered employment costs in the low skill sector. This may have resulted in job growth and lower youth unemployment, but it may have also led to substitution of permanent with temporary jobs, which may have even raised youth unemployment. The adverse effects of partial deregulation are most likely to present themselves among the least educated, who normally work in jobs requiring few specific skills. In contrast, the positive effects of partial deregulation may manifest themselves particularly among the highly skilled (the most educated), for whom temporary employment represent a screening period, a stepping stone to permanent jobs.

We build our answer to these questions in three steps. First, we begin with a cross-sectional analysis, as cross-sectional designs have been frequently used to support the argument of adverse effects of employment protection on youth labor market outcomes (Esping-Andersen, 2000; Breen, 2005; Kahn, 2007). The cross-sectional analysis allows us to test simple descriptive questions: Are young people relatively worse off when permanent jobs are highly protected? And, are young people relatively worse off, when temporary contracts are deregulated? In this section, we also take some analytical choices that are similar to the ones taken in these studies, for example using OECD data for youth labor market outcomes and studying youth-adult unemployment rate ratios as dependent variables. We also assess the robustness of findings from the cross-section to inclusion of control variables, and discuss why our results may differ from published research.

In the second step, we assess the robustness of findings to inclusion of country fixed effects. This analysis provides a stricter test of whether the association between employment protection legislation and youth labor market outcomes is causal or not. If we are to believe our cross-sectional estimates, the effects should survive the inclusion of fixed effects. If fixed effects completely wipe out or reverse the sign of the effect estimated in the cross-section, we should interpret this as strong evidence for omitted variable bias and that findings from the cross-section should not be interpreted as causal relationships. The key purpose of the cross-sectional and fixed effects analysis is to relate our findings to published research and use similar approaches and data to get a feeling how strong of a case we can build for or against employment protection legislation in these frameworks.

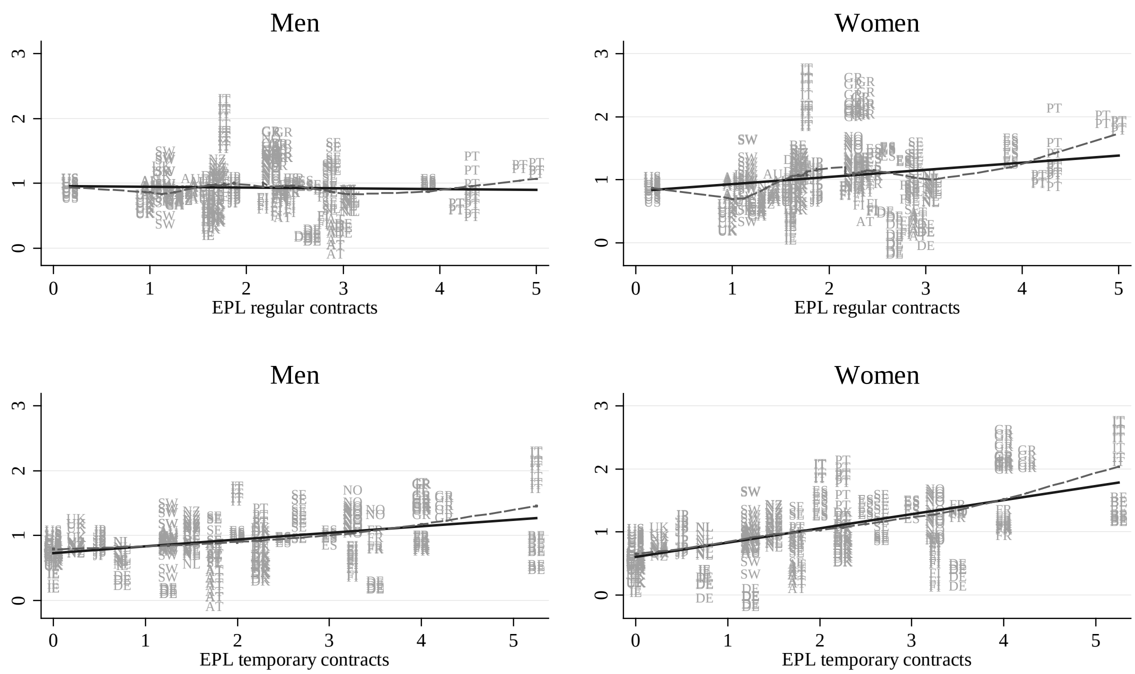
The third step of our analysis, however, is the decisive one: Here, we perform a comprehensive assessment of the distributional effects of employment protection reform on key youth labor market outcomes: unemployment, employment and labor force participation. We assess whether there is an interaction between both types of employment regulations, and we assess whether their reform has had unequal impacts on different groups on the youth labor market defined by education and gender.

5.1. Results from cross-sectional analysis

As a starting point, Figure 5.1 plots the difference of the logged youth minus logged adult unemployment rates against the OECD measures of employment regulation. Youth are defined as age groups 15-29, adults are 45-54 year old men. We use OECD data for all the available countries and years. We also plot the linear regression line and the lowess smoother as a non-parametric measure of association. The clearest positive relationship emerges between the regulation of temporary contracts and the relative unemployment rates of young women. Young women seem to be relatively better off, if temporary contracts are deregulated. The other relationships appear relatively weak. In particular, there is no indication that stricter regulation of permanent employment contracts makes young men relatively worse off.

To test the robustness of the associations in the data, we run OLS regressions of the difference of the logged unemployment rates on the two measures of employment protection legislation as well as control variables. We report the results of four different specifications. Specification (1) only contains the two measures of regulation as regressors. Specification (2) contains the control variables as linear terms and year fixed effects. Specification (3), which we referred to as the "full specification" in Section 4.4.1 (p. 93f.), adds to specification (2) any interaction/quadratic terms that predict significantly the outcome variable or either measure of regulation. Of these interaction/quadratic terms, specification (4), referred to as "restricted specification", only retains the strongest ones. We generally prefer the "restricted specification" that permits nonlinearities in the control variables. Information on data sources and control variable definitions is available in Section 4.6 (p. 106f.)

Figure 5.1 Employment protection and relative youth (ages 15-29) unemployment rates, 21 OECD countries, 1985-2007



Notes: The dependent variables is the difference of the natural log youth (ages 15-29) and natural log adult (men, ages 45-54) unemployment rates.
Source: OECD Labor Force Statistics

Table 5.1 reports the results for young men and young women separately. Consistent with the impression gained in Figure 5.1, we find that tighter regulation of temporary contracts is associated with higher relative unemployment rates among young people.[35] The estimated coefficient for women is around twice as large as that for men. Moreover, we find that if employment protection for regular contracts has an effect, it tends to lower, not raise, relative

[35] Both measures of employment protection are not strongly correlated in the cross-section (ρ=0.38), which should diffuse concerns about multicollinearity.

youth unemployment rates. Regarding the regulation of temporary contracts, the results are consistent across the different model specifications used. The negative effect of the regulation of permanent contracts only becomes significant, once we add control variables. Table 5.20 in the Appendix repeats the analysis in Table 5.1, separately analyzing 15-19, 20-24 and 25-29 year olds. The results are very similar, except that associations between measures of regulation and relative unemployment are somewhat weaker among 25-29 year olds.

Table 5.1 OLS regression estimates ("cluster robust" standard errors) of the association between employment protection legislation and relative youth (ages 15-29) unemployment rates, 21 OECD countries, 1985-2007

	A. Men				B. Women			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
EPR	-0.08	-0.11*	-0.15*	-0.10	-0.02	-0.09	-0.18**	-0.14**
	(0.07)	(0.06)	(0.08)	(0.06)	(0.09)	(0.08)	(0.08)	(0.06)
RTC	0.11**	0.10**	0.09**	0.09**	0.21***	0.18***	0.16***	0.16***
	(0.06)	(0.05)	(0.04)	(0.04)	(0.07)	(0.04)	(0.03)	(0.03)
R-sq	0.16	0.42	0.62	0.56	0.33	0.57	0.82	0.80
N	436	436	436	436	436	436	436	436

Notes: *** p<0.01, ** p<0.05, * p<0.1. EPR - Employment protection regular contracts, RTC - Regulations on the usage of temporary contracts. Control variables used in specifications (2), (3) and (4): indicators for the generosity of unemployment benefits, trade union density, two indicators for the coordination of wage bargaining, indicators for the cabinet power of left parties, expenditure on active labor market policies, product market regulation, economic globalization, terms-of-trade shocks, long-term interest rates, the inflation rate, output gap, the youth cohort size, and year fixed effects.
Source: OECD Labor Force Statistics, see Data Appendix in Chapter 4, Section 4.6 (p. 106f.)

The analyses reported in Table 5.2 focus on the effect of employment regulations on relative employment and labor force participation rates, where the dependent variables are the difference between the logged youth and the logged adult outcomes (adult men aged 45-54). We report only the results from the fourth, "restricted" model specification. The results suggest that if temporary contracts are highly regulated, both relative youth employment and labor force participation rates are lower, where the relationships between employment protection and the labor force participation rate tend to be somewhat stronger. The regulation of permanent contract is only associated with relative unemployment rates, not with the other two outcomes.

Table 5.2 OLS regression estimates ("cluster robust" standard errors) of the association between employment protection legislation and relative youth (ages 15-29) unemployment (U), employment (E) and labor force participation (L) rates, 21 OECD countries, 1985-2007, results from model specification (4)

	A. Men			B. Women		
	(U)	(E)	(L)	(U)	(E)	(L)
EPR	-0.10	-0.01	-0.02	-0.14**	-0.01	-0.01
	(0.06)	(0.02)	(0.01)	(0.06)	(0.03)	(0.02)
RTC	0.09**	-0.04***	-0.03***	0.16***	-0.07***	-0.03***
	(0.04)	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)
R-sq	0.56	0.74	0.77	0.80	0.76	0.76
N	436	436	436	436	436	436

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. EPR - Employment protection regular contracts, RTC - Regulations on the usage of temporary contracts, see notes to Table 5.1

To see how exactly the associations between employment protection legislation and age inequalities in the distribution of unemployment are generated, we directly estimate the association between EPL and logged youth and adult unemployment rates (Table 5.3). There is no evidence of any association between either measure of regulation and the adult male unemployment rate. However, we observe a positive association between the regulation of temporary contracts and youth unemployment, which is roughly twice as large among young women, and a weak and insignificant association between dismissal regulations on permanent contracts and youth unemployment. Thus, it appears that observed associations between employment protection and age-inequalities in the distribution of unemployment are driven by the association between employment protection and youth unemployment. Table 5.21 (Appendix) reports the full results for the relative and absolute unemployment rate regression analyses, using specification (2), which is linear in the control variables (year fixed effects not shown). Except for regulations on the usage of temporary contracts, there is little evidence for strong institutional effects, while the macroeconomic variables have strong effects. However, more strongly centralized wage bargaining seems to be associated with lower unemployment among youth and adults.[36]

Table 5.3 OLS regression estimates ("cluster robust" standard errors) of the association between employment protection legislation and youth and adult men's unemployment rates, 21 OECD countries, 1985-2007

	A. Men, ages 15-29		B. Women, ages 15-29		C. Men, ages 45-54	
	(1)	(4)	(1)	(4)	(1)	(4)
EPR	-0.07	-0.08	-0.00	-0.05	0.01	0.05
	(0.08)	(0.07)	(0.09)	(0.07)	(0.09)	(0.04)
RTC	0.09*	0.11***	0.18***	0.20***	-0.02	0.04
	(0.05)	(0.04)	(0.05)	(0.04)	(0.06)	(0.03)
R-sq	0.06	0.70	0.23	0.70	0.00	0.72
N	436	436	436	436	436	436

Notes: *** p<0.01, ** p<0.05, * p<0.1. EPR - Employment protection regular contracts, RTC - Regulations on the usage of temporary contracts, see notes to Table 5.1

5.1.1. Robustness checks

We conduct several robustness checks of the results reported in Table 5.2. We repeated the analysis for a subsample of Western European countries only (EU15 minus Luxembourg, plus Switzerland and Norway). By restricting the sample to Western Europe only, our units of observation should become more similar on unobservables. The results regarding unemployment rates appear to be sensitive to the country composition. The negative association between dismissal regulations on regular employment contracts becomes even stronger if we focus on Western European countries only. At the same time, the association between regulations on temporary contracts weaken in the restricted sample.

[36] Moreover, we obtain somewhat unexpected results that should caution us against causal interpretation of the findings. First, expenditure on active labor market policies is positively correlated with unemployment, presumably since in times of high unemployment, more is spent on such policies. Second, we also observe that more generous unemployment benefits are associated with lower unemployment among young men. It may be that in countries experiencing economic crisis, the absence of unemployment benefits motivates drastic exclusionary behavior on the side of incumbent employees, leading to reductions in outflows from employment and reductions of worker turnover (see Section 2.1.3, p.18).

Table 5.4 OLS regression estimates ("cluster robust" standard errors) of the association between employment protection legislation and relative youth (ages 15-29) unemployment (U), employment (E) and labor force participation (L) rates, 16 Western European countries, 1985-2007, results from model specification (4)

	A. Men			B. Women		
	(U)	(E)	(L)	(U)	(E)	(L)
EPR	-0.25**	0.04*	-0.00	-0.30***	0.05	0.02
	(0.09)	(0.02)	(0.01)	(0.09)	(0.03)	(0.02)
RTC	0.06*	-0.04***	-0.04***	0.09**	-0.08***	-0.03***
	(0.03)	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)
R-sq	0.68	0.79	0.77	0.84	0.80	0.78
N	326	326	326	326	326	326

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. EPR - Employment protection regular contracts, RTC - Regulations on the usage of temporary contracts, see notes to Table 5.1

We conduct two further robustness checks (results not shown). First, we aggregate the yearly data into period averages, where each periods is four years long (three years, in case of the last period). We thereby smooth out some cyclical year-to-year variation and may better reveal medium to long-run associations. The analysis are otherwise identical to those reported in Table 5.2 and the results are nearly identical as well. This may not be very surprising since, by differencing the dependent variable, we already remove considerable cyclical variation. Second, we repeated the analysis in Table 5.2 within two sub-periods: 1985-1995 and 1996-2007. For the more recent period, we additionally control for the share of young people enrolled in upper and post-secondary vocational education programs that combine school-based education with employer-provided training; the existence of such 'dual system' arrangement is consistently associated with low absolute and relative youth unemployment and a rapid integration of young people into working life (Müller and Shavit, 1998; Breen, 2005; Wolbers, 2007). Including this control variable does not change our conclusion about the effect of the regulation of temporary contracts on relative youth unemployment rates. Supplementary analyses (not reported) show that the estimated coefficients of employment protection legislation become even stronger (larger estimated coefficients, smaller standard errors) once we add this control.

Finally, we analyze the relationship between relative youth unemployment and employment protection legislation using the augmented European Union Labor Force dataset for 16 Western countries from 1992-2006. Using this dataset, we can assess the association between employment protection and relative youth unemployment at a more disaggregate level, distinguishing by respondents' levels of education. To make the youth samples more homogenous in terms of potential labor force experience, we select different age bands according to levels of education: ages 15-24 for those who completed only lower secondary education or less (ISCED 0-2), ages 20-29 for those with upper secondary / post-secondary degrees (ISCED 3-4), and ages 25-34 for those with tertiary degrees (ISCED 5-6). Results are reported in Table 5.5.

Table 5.5 OLS regression estimates ("cluster robust" standard errors) of the association between employment protection and relative youth unemployment rates by education (ISCED-97) levels, 15 Western European countries and the U.S., 1992-2006, results from model specification (4)

	A. Men				B. Women			
	0-2	3-4	5-6	aggr.	0-2	3-4	5-6	aggr.
EPR	-0.23***	-0.37***	0.04	-0.19***	-0.22**	-0.43***	-0.21	-0.16*
	(0.06)	(0.08)	(0.12)	(0.04)	(0.09)	(0.08)	(0.18)	(0.09)
RTC	0.14***	0.08**	0.15***	0.11***	0.25***	0.26***	0.14*	0.17***
	(0.04)	(0.03)	(0.03)	(0.02)	(0.05)	(0.03)	(0.07)	(0.05)
R-sq	0.81	0.82	0.77	0.89	0.74	0.92	0.75	0.87
N	207	207	207	207	207	207	207	207

Notes: *** p<0.01, ** p<0.05, * p<0.1. Dependent variables are the differences of the (logged) unemployment rates for youth and adults (men, ages 45-54), calculated separately by levels of education. See notes to Table 5.1
Source: EULFS data (own calculations), see Data Appendix in Chapter 4, Section 4.6 (p. 106f.)

The results are broadly consistent with what we observed in the macro-data (the fourth column presents the corresponding analyses aggregating over education groups). However, there is some variation in associations across education groups. The effects of employment protection legislation appear stronger at lower levels of education. Among those with less than tertiary education, we observe strong, negative effects of employment protection legislation on relative unemployment rates. For tertiary educated, stricter regulation of temporary contracts seems to be associated with higher unemployment risks.

5.1.2. Summary, discussion and further analyses

The results in the previous section contradict findings obtained from cross-sectional analyses by Esping-Andersen (2000: 85f.), the OECD (2004: Table 2.4), Breen (2005) and Kahn (2007). While Kahn (2007) uses micro data from the International Adult Literacy Survey (IALS), the other three studies use OECD macro data; all studies use different versions of the OECD indicators of employment protection.[37] These (empirical) studies are often cited in support of claims for the alleged compositional effects of employment protection on the composition of unemployment. Here, we have shown their results to be non-robust.

In fact, the results point to nearly opposite conclusions. While dismissal regulations on permanent contracts are expected (and have been shown by the aforementioned studies) to lead to higher relative and absolute youth unemployment, we find the opposite effect in bivariate analyses, as well as in analyses using different specifications for control variables, country samples and data sources. Moreover, while a number of authors have tended to emphasize the potential dark sides of deregulation of temporary contracts (OECD, 2004; Blanchard and Landier, 2002) we find that youth unemployment is in fact lower in absolute and relative terms wherever temporary contracts are less regulated.[38] We therefore conclude that, first, youth unemployment in both absolute and relative terms is not higher in countries with stricter

[37] None of the studies focuses on both types of regulations simultaneously. Breen (2005) and Kahn (2007) use only the indicator for regulations on regular contracts.

[38] A proper test of the potential dark side of deregulating temporary contracts would specify an interaction between both types of regulation, which we do in subsequent analyses.

employment protection. The evidence rather suggests it to be lower in particular if we focus on Western European countries only. Second, stricter regulation of temporary contracts is associated with higher youth unemployment, as well as lower relative youth employment and labor force participation rates. These associations tend to be stronger for women and less educated.

* * *

Why do our findings differ from other published research? To shed some light on this question, we replicate the analysis published in Breen (2005). Column 1 in Table 5.6 are the results published by Breen (2005), column 2 is a replication using current OECD data for the same years and countries as well as identical specifications of dependent and independent variables. While the data sources of Breen's and this analysis are principally identical, we use most recently published data, especially the recently updated annual OECD indicators of employment protection legislation.

The results in Table 5.6 indicate that the OLS estimate of the association between the regulation of permanent contracts (EPR) and relative youth unemployment weakens and becomes more uncertain when using more recent data. Column (3) shows that the effect is of EPR is negative and statistically insignificant, while educational institutions (VOC) and the regulation of temporary contracts (RTC) matter. Column (4) shows that when controlling for RTC, the interaction EPR_VOC is also no longer statistically significant. The latter results suggest that the results are also sensitive to the inclusion of additional, relevant variables, pointing to omitted variable bias.

Table 5.6 Replication of Breen (2005), Table 4. OLS regression estimates ("cluster robust" standard errors) of the association between employment protection legislation and relative youth unemployment, 23 OECD countries, 1995-1999

	Breen 2005	Replication	(3)	(4)
EPR	0.111**	0.085	-0.014	0.016
	(0.047)	0.057)	(0.055)	(0.066)
EPR_VOC	-0.003***	-0.004**		-0.001
	(0.001)	0.001)		(0.003)
UA	0.950***	0.922***	0.859***	0.866***
	(0.082)	0.091)	(0.101)	(0.100)
RTC			0.067**	0.064*
			(0.031)	(0.034)
VOC			-0.007*	-0.004
			(0.003)	(0.007)
Constant	0.787***	0.903***	1.084***	1.019***
	(0.178)	0.178)	(0.201)	(0.191)
R-sq	0.84	0.83	0.85	0.85
N	113	113	113	113

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Youth are here defined as age groups 15-24. UA is the logged aggregate unemployment rate (ages 25-54). EPR - Employment protection regular contracts, RTC - Regulations on the usage of temporary contracts. VOC - Share of students at ISCED level 3-4 enrolled in vocational programs combining school-based vocational education with workplace based training. EPR_APP - multiplicative interaction between EPR and APP.
Source: See notes to Table 5.1, column 1 reproduced from Breen (2005: Table 4)

Closer inspection of the underlying data reveals that changes in the dependent and independent variables between OECD data releases are key factors behind the changes in results. In Table 5.22 (Appendix to this chapter), we tabulated the youth/adult unemployment rate ratios as calculated by Breen from OECD Employment Outlook data and the OECD Index for employment protection for regular contracts (OECD 1999), and compare it to more recent OECD data releases for both employment protection legislation and unemployment rates. We see discrepancies on both dependent and independent variables. In a number of cases, we see that relative unemployment "diminished" between data releases in more regulated countries (e.g. Sweden) or "increased" in less regulated countries (e.g. Australia). Moreover, on the index of employment protection, there have also been some changes. For example, Italy, a country with high youth unemployment, has been downgraded by a full unit, making it similar to Ireland or Denmark, where it previously had the same value as Sweden or Germany. OECD (2004: 107f.) details some of the changes that were made to the employment protection indices published by the OECD in 1999 (OECD 1999, see also Venn, 2009, for the most recent update). If we assume that the OECD indicator has become more accurate with subsequent releases, this would imply that past indicators were mismeasured, which may have resulted in biased inferences.

In Table 5.7 (see below), we report the results of bivariate regression analyses, using both old and new data as tabulated in Table 5.22. Changes in both the dependent and independent variables contribute to the weakening of the association between relative youth unemployment and the regulation of permanent contracts. The change in the EPR index is decisive, destroying the association fully. Even though the old and new measure tabulated in Table 5.22 are highly correlated ($\rho = 0.93$), the small changes on the index suffice to destroy whatever association is present, or even change the sign of the association.

One would have imagined that two highly correlated indices yield the same result, but this is not the case. Even though we are working with cross-sectional data, which, as we saw above, are quite robust to changes in specification, we are still very vulnerable to measurement error. This may be an issue other studies in sociology and economics that have used the OECD (1999); but it also is a general reminder that the multilevel designs can very vulnerable to measurement error in the higher level, contextual variables. While here we use observations on 23 countries and our indicators are highly correlated, many multilevel analyses (using individual level data from a cross-section of countries) rely on fewer country observations and use proxy indicators generated from the same micro data source, which should make the problem of measurement error even more severe.

Table 5.7 Bivariate associations between old and new data on relative unemployment rates and dismissal regulations on regular contracts. OLS regression estimates and standard errors.

	U^Y "old"	U^Y "old"	U^Y "new"	U^Y "new"
EPR "old"	0.044		0.027	
	0.052		0.050	
EPR "new"		0.005		-0.010
		0.051		0.048
R-sq	0.02	0.00	0.01	0.00
N	27	27	27	27

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Data from columns 1, 2, 4, and 5 in Table 5.22. Dependent variables: U^Y "old" - logged youth unemployment ratio (column 1), U^Y "new" - logged youth unemployment ratio (column 2). Independent variables: EPR "old" (column 4) and EPR "new" (column 5).
Source: Table 5.22

The replication analysis also alerted us to the sensitivity of the results to the inclusion of different control variables. We also saw that the (unexpected) negative association between regulations on permanent contracts is more likely to be statistically significant, if we include control variables into the analysis. While much comparative research uses cross-sectional data, we are frequently reminded that this type of design is inherently vulnerable to omitted variable bias. In some situations, cross-sectional data is all that is available for comparative research and in this situation, cross-sectional analysis can still generate useful insights; but often, we can and should try to adopt a better design if the goal is – as it usually is – causal inference.

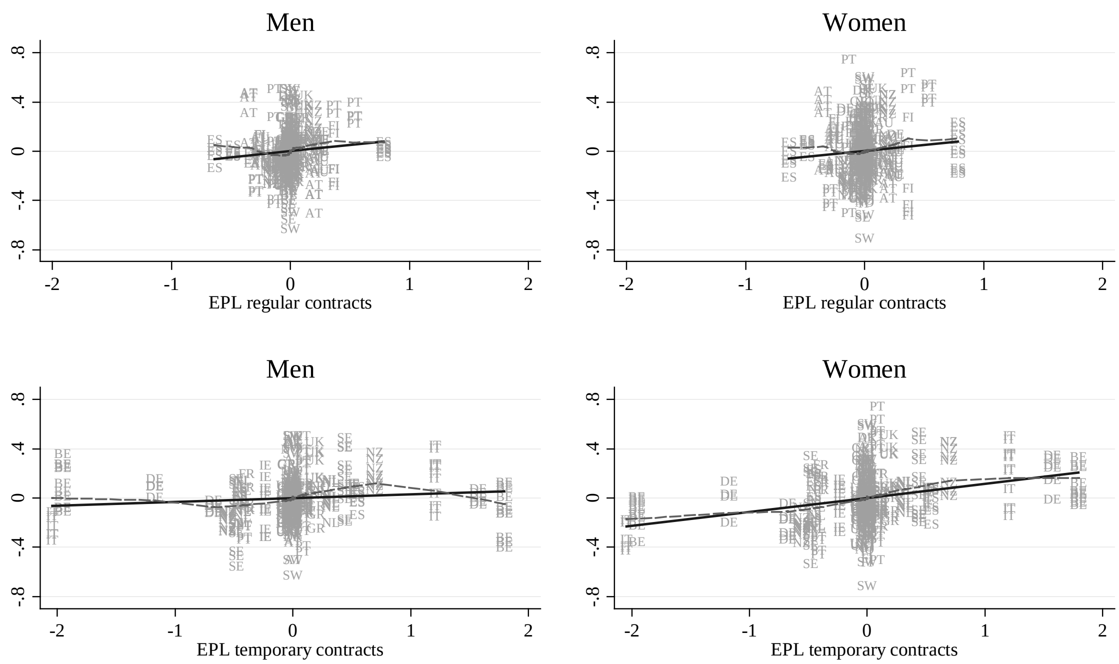
5.2. Results from fixed effects analysis

The following section repeats some of the preceding analyses, adjusting for country specific, time-invariant ("fixed") effects. The main purpose of this section is “procedural” to show the vulnerability of cross-sectional inference to this type of unobserved heterogeneity and to relate our work to other published studies using similar methods. As we will show, in this application, almost none of the results from cross-sectional analyses are robust to the inclusion of country fixed effects. Moreover, a number of studies have been published using fixed effects models such as the ones used here. We summarize their results and discuss some of their shortcomings. The main analysis is done using difference-in-differences in the subsequent section.

Adjusting for fixed effects comes at a steep price: Using the full OECD dataset from 1985 to 2007, country fixed effects soak up 78% of the variation in relative unemployment rates for young men (85% in case of young women), and 96% of the variation in the index for the regulation of permanent contracts (85% for the index of regulations on temporary contracts). While some of the eliminated between country variation is due to unmeasured factors that could bias the results, we also eliminate some useful variation. This is the often encountered trade-off between bias vs. efficiency. Furthermore, after eliminating all between country variation, we are left with only the within-country variation, i.e. the year-to-year changes in the variables, and we become more vulnerable to measurement error. Thus, both effect size and significance are likely to diminish. Given that we have already been alerted to potential measurement error, we should exercise caution when interpreting the fixed effects results.

To illustrate whether and how the inclusion of fixed effects altered the results from cross-sectional analysis, we have repeated the analyses reported in Section 5.1. Analogous to Figure 5.1, Figure 5.2 graphs the association between employment protection and relative youth unemployment after country fixed effects have been removed from the data. We notice that the variation in the data has diminished considerably as most point cluster in the center of the graphs. Little variation is left on the indicator for EPL on regular contracts, and Spain strikes us as an outlier. We observe considerably more within variation in terms of the regulation of temporary contracts; Belgium and Italy in particular show substantial variation, raising concerns for potential outlier status. The remaining associations look similar to those reported above. We again observe the strongest association between the regulation of temporary contracts and relative youth unemployment for young women, while no such association appears to be present for young men. The association between EPL on regular contracts and relative youth unemployment seems to be positive for both genders.

Figure 5.2 Employment protection and relative youth (ages 15-29) unemployment rates, 21 OECD countries, 1985-2007, country-demeaned data



Notes: The dependent variables is the difference of the natural log youth (ages 15-29) and natural log adult (men, ages 45-54) unemployment rates. Dependent and independent variables are country-demeaned, i.e. we subtracted from each observation on each variable the country mean of the corresponding variable.
Source: OECD Labor Force Statistics

In the following, we assess whether these association are robust to the inclusion of control variables. We adopt the same model specification strategies as above, but include country and year fixed effects into all specifications.[39] Compared to the cross-sectional analysis reported in Table 5.1, the absolute size of the estimates diminish considerably, probably not least due to

[39] We reran the algorithms for specifications (3) and (4) also controlling for year and country fixed effects. Thus, the nonlinear terms differ between the cross-sectional and the fixed effects specification.

attenuation bias (see Table 5.8). Nevertheless, we still observe a significant negative effect of stricter regulations on the usage of temporary contracts on *relative* unemployment among young women. Young men seem to be affected by neither regulation.[40] There is no indication that regulations on permanent contracts matter for either gender.

Table 5.8 OLS fixed effects (LSDV) regression estimates ("cluster robust" standard errors) of the association between employment protection legislation and relative youth (ages 15-29) unemployment rates, 21 OECD countries, 1985-2007

	A. Men				B. Women			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
EPR	0.11	-0.07	0.01	-0.01	0.07	-0.07	-0.03	-0.08
	(0.11)	(0.10)	(0.12)	(0.11)	(0.14)	(0.12)	(0.13)	(0.11)
RTC	0.04	0.03	0.00	-0.00	0.12**	0.09***	0.07***	0.07***
	(0.06)	(0.04)	(0.03)	(0.02)	(0.04)	(0.03)	(0.02)	(0.01)
R-sq	0.80	0.86	0.90	0.90	0.87	0.91	0.94	0.94
N	436	436	436	436	436	436	436	436

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. EPR - Employment protection regular contracts, RTC - Regulations on the usage of temporary contracts, see notes to Table 5.1

Considering relative employment and labor force participation rates, Table 5.9 reports the results of analysis similar to those in Table 5.2. Adjusting for country fixed effects, stricter regulation of temporary contracts is now positively associated with relative employment and labor force participation rates among young women men and women. For both genders and dependent variables, the sign of the association has switched, being significant in both cross-section and fixed effects analysis. Graphical analysis using added variable plots suggests that these results are not driven by outliers. Moreover, we now observe a significant negative effect of stricter employment protection on regular contracts on relative youth labor force participation.

[40] Looking at different age groups separately, we observe a significant negative effect of regulations on temporary contracts for young men aged 15-19.

Table 5.9 OLS fixed effects (LSDV) regression estimates ("cluster robust" standard errors) of the association between employment protection legislation and relative youth (ages 15-29) unemployment (U), employment (E) and labor force participation (L) rates, 21 OECD countries, 1985-2007, results from model specification (4)

	A. Men			B. Women		
	(U)	(E)	(L)	(U)	(E)	(L)
EPR	-0.01	-0.02	0.01	-0.08	-0.04	-0.03**
	(0.11)	(0.02)	(0.02)	(0.11)	(0.03)	(0.02)
RTC	-0.00	0.02***	0.02***	0.07***	0.02***	0.02***
	(0.02)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)
R-sq	0.90	0.96	0.97	0.94	0.98	0.97
N	436	436	436	436	436	436

Notes: *** p<0.01, ** p<0.05, * p<0.1. EPR - Employment protection regular contracts, RTC - Regulations on the usage of temporary contracts, see notes to Table 5.1

Table 5.10 reports fixed effects results, where we directly estimate the association between EPL and logged youth and adult unemployment rates. While in the cross-section, regulation of temporary contracts (RTC) was not associated with the unemployment rate among prime-age men, we now obtain a significant negative effect. The relationship between RTC and relative youth unemployment changed, too. While we observed significant positive effects for young women in the cross-section, this effect has now disappeared. In the cross-section, the positive association between RTC and relative unemployment among young women was driven by the effect of RTC on the unemployment rate of young women, while there was no effect for adult men. Now, the situation is exactly the reverse. Furthermore, while there was no effect for young men in the cross-section, stricter regulation of temporary contracts is now associated with lower unemployment among young men.

Table 5.10 OLS fixed effects (LSDV) regression estimates ("cluster robust" standard errors) of the association between employment protection legislation and youth and adult men's unemployment rates, 21 OECD countries, 1985-2007

	Men, ages 15-29		Women, ages 15-29		Men, ages 45-54	
	(1)	(4)	(1)	(4)	(1)	(4)
EPR	0.02	0.06	-0.03	-0.04	-0.10	0.13
	(0.20)	(0.16)	(0.23)	(0.13)	(0.22)	(0.11)
RTC	-0.10	-0.10***	-0.03	-0.02	-0.15*	-0.10**
	(0.06)	(0.02)	(0.05)	(0.03)	(0.08)	(0.04)
R-sq	0.67	0.89	0.78	0.93	0.64	0.89
N	436	436	436	436	436	436

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. EPR - Employment protection regular contracts, RTC - Regulations on the usage of temporary contracts, see notes to Table 5.1

5.2.1. Discussion

The cross-sectional analysis, using the most recent and comprehensive data available and conducting extensive specification checks, yielded robust associations which are contrary to influential studies that relied on similar cross-sectional analyses. These robust associations, however, do not survive once we control for country-specific fixed effects. We interpret this result as powerful evidence for the presence of unobserved heterogeneity, which we were unable to capture in the cross-sectional analysis using any of the rich control variable specifications. This shortcoming is highly consequential for the results. While issues with measurement error have made us wary of attenuation bias, we observed in several instances that the estimates actually moved away from rather than towards zero.

The cross-sectional associations still provided important insights. In countries with stricter employment protection on regular contracts, youth unemployment is not higher as many argue, but lower. More consistent with popular belief, but inconsistent with some critiques, tighter regulation on temporary contracts are associated with worse youth labor market outcomes. These are highly robust associations in the data, and they provide no support whatsoever for the argument that youth unemployment in Southern Europe or some other European countries is so high because of high firing costs on regular contracts. None of the models presented hitherto supports this conclusion. Nevertheless, we should not assign a causal interpretation to the associations found in the cross-section. Instead, the fixed effects analysis suggests that these correlations are spurious. There is clear evidence of omitted variable bias that an analysis aimed to go beyond statements of association has to remedy. This is the goal of the following section.

* * *

Before proceeding, however, we should relate our results to those of published research, which has often relied on fixed effects models similar to the ones estimated here. Controlling for country fixed effects, Bassanini and Duval (2006: 48f.) find negative effects of the OECD summary indicator of employment protection on youth employment rates. Bertola et al. (2002, 2007) estimate fixed effects GLS models (using a parametric correction for serial correlation in the error term) of (relative) youth employment and unemployment rates similar to those reported above. They rely on data initially published by Blanchard and Wolfers (2000).

Using two different specifications, Bertola et al. (2002, *NBER Working Paper*) consistently obtain negative effects of employment protection on relative youth employment. Regarding relative youth unemployment, they find no significant or inconsistent effects.[41] Bertola, Blau and Kahn (2007, *Journal of Population Economics*) is based on the preceding working paper, but use absolute not relative youth employment and unemployment rates as outcomes. The result now show that employment protection raises youth unemployment, but does not affect youth employment.

[41] The effect of employment protection on relative unemployment of young women, is significant in one and insignificant in the other specification. There is no effect of employment protection on relative unemployment of young men.

Jimeno and Rodriguez-Palenzuela (2002) use the same data source, but adopt a different modeling strategy that focuses on interactions of institutions with observable and unobservable shocks as in Blanchard and Wolfers (2002). They provide some evidence that (absolute) youth unemployment rises in response to shocks, if employment protection is stricter, particularly among young women. However, the results regarding relative youth unemployment appear non-robust, which leads them to conclude that employment protection (on regular and temporary contracts) does not raise relative youth unemployment rates.

The results should alert us that findings regarding relative outcomes (where the dependent variable is a log ratio of youth divided by adult outcomes) and absolute outcomes (where the dependent variable is logged youth outcomes) may differ. On the basis of published research, we would thus expect that stricter regulation entails higher youth unemployment (Bertola et al., 2007; Rodriquez-Palenzuela and Jimeno, 2002) and lower youth employment (Bassanini and Duval, 2006), consistent with the common wisdom of negative effects of regulation on youth labor market outcomes.

In contrast to this, our fixed effects analyses could hardly find any effects of either type of regulation on youth unemployment. If anything, tighter regulations on the usage of temporary contracts lower, not raise unemployment among young men, which would be more consistent with a critical reading of the effects of deregulation (Blanchard and Landier, 2002; Cahuc and Postel-Vinay, 2002). Our results agree with the OECD (2004) study suggesting that effects of employment protection on youth employment present in the cross-section disappear after fixed effects are controlled for, although we differ from the OECD in interpreting this result (see Section 7.1, p. 177f.).

We cannot directly compare our results with those published, however, since none of the studies differentiate between the regulation on permanent contracts and temporary contracts, although such a differentiation is meaningful. The underlying mechanisms differ, and both types of regulations may interact with each other. Moreover, few if any specification checks are reported regarding the effects of employment protection, and we are therefore left to wonder how robust the results are to changes in specification and sample composition. Moreover, we are skeptical about the quality of the measurement of employment protection in the Blanchard and Wolfers dataset (see Section 4.1.3, p. 81f.).

Finally, if this issue is addressed at all, the analyses use parametric corrections for autocorrelation, which is likely to underestimate the uncertainty of the parameter estimates, if the parametric autocorrelation model is incorrectly specified (Bertrand et al., 2004: 263f.). Unless we know the data generating process, we face the risk of such specification errors. Following Bertrand et al.'s recommendation (see also Cameron and Trivedi, 2005; Kezdi, 2003), we have used variance-covariance estimators ("cluster robust" standard errors) that allow for an arbitrary correlation of the error term within panel units, instead of imposing a panel-specific, first-order autocorrelation structure on the error term as Bertola et al. (2007) do. In Table 5.11, we repeated the analysis in Table 5.10 using Bertola et al.'s estimation strategy, i.e. generalized least squares with panel specific (i.e. country-specific) first-order autocorrelated error terms. As Bertrand et al.'s results suggest, the parametric error correction yields larger t-statistics / smaller p-values compared to the "cluster robust" standard errors. If we consistently applied this estimation strategy, we would be more likely to detect significant effects of employment protection legislation that are really due to serial correlation in the data. However, in this particular instance, our qualitative conclusions are not affected.

Table 5.11 Comparison of OLS fixed effects ("cluster robust" standard errors) and GLS fixed effects (standard errors adjusted for panel-specific, first-order autocorrelation), 21 OECD countries, 1985-2007

		Men, ages 15-29		Women, ages 15-29		Men, ages 45-54	
		(1)	(4)	(1)	(4)	(1)	(4)
OLS, cluster robust s.e.	EPR	0.02	0.06	-0.03	-0.04	-0.10	0.13
		(0.20)	(0.16)	(0.23)	(0.13)	(0.22)	(0.11)
	RTC	-0.10	-0.10***	-0.03	-0.02	-0.15*	-0.10**
		(0.06)	(0.02)	(0.05)	(0.03)	(0.08)	(0.04)
GLS, p.s. AR(1) s. e.	EPR	-0.06	0.03	-0.10	0.00	-0.03	0.07
		(0.08)	(0.06)	(0.07)	(0.06)	(0.10)	(0.07)
	RTC	-0.06***	-0.07***	-0.02	-0.03*	-0.07**	-0.06***
		(0.02)	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. EPR - Employment protection regular contracts, RTC - Regulations on the usage of temporary contracts, see notes to Table 5.1

5.3. Results from Differences-in-Differences Analysis

In this section, we try to arrive at a conclusive empirical answer about the distributional effects of employment protection legislation. We rely on the augmented EULFS data set, which allows for a more detailed distributional assessment of the effects of employment protection reforms and focuses on a core set of Western European countries for the period from 1992 to 2006, during which we observe altogether 28 reforms. We implement a differences-in-differences estimator, as outlined in Section 4.5 (p. 98f.). We estimate equations of the following form, where Y_{gjt} is the log youth labor market outcome of group g in country j and year t.

(1) $Y_{gjt} = \mu_g + \lambda_t + \gamma_{1j} Treat_{jt} + \delta Dif_{jt} + \gamma_2 Post_{jt} + \varepsilon_{gjt}$,

where μ_{g} and λ_{t} are group and year specific fixed effects. $\gamma_{1j} Treat_{jt}$ models baseline outcomes for each reform over the specified window of observation (t-2 to t+3). Dif_{jt} is the change in the index of employment protection legislation calculated as $Dif_{jt} = I_{j,t=-1} - I_{j,t=0}$, where the new regulation is implemented in t=0. Dif_{jt} takes on this value for the period from t=0 to t=3 following reform, and is equal to zero otherwise. The coefficient of interest δ estimates the deviation of the outcome variable from the country and reform specific baseline outcome over the period t=0 to t=3, as a linear function of the direction (negative/de-regulation or positive/up-regulation) and magnitude of change. $Post_{jt}$ is a dummy variable that takes the value 1 for countries that experienced reform for the period two years after the observation window, i.e. from t=4 to t=5 in the default specification.

5.3.1. Results for young people

As outlined in Section 4.5.1 (p. 101f.), we adopt two specifications for control variables, which we add to equation (1). The control variable based specification (C) uses a list of control control variables, including the non-treatment indicator of employment protection legislation, as well as interactions/quadratic terms generated from the control variables that significantly predict either outcome or the treatment variable.[42] The trend specification (T) contains country-specific linear trend variables as well as the corresponding outcome for men, aged 45-54 (measured at the education-group specific level). Before studying more detailed distributional effects and interactions between both types of regulation, we begin with an assessment of average effects of employment protection reform. We also routinely estimated the basis DiD specification (equation 1 above), as well as the country-specific linear trend specification without the adult outcome variable. We do not report the results here because of space limitations. However, the results usually did not differ from what we obtained in the specifications reported here.

Comparing both specifications for each outcome, we observe no or inconsistent effects of the regulation of temporary contracts. There is also no evidence that employment protection on regular contracts has affected youth employment or unemployment rates. However, we observe a

[42] We reran all analyses using just interaction/quadratic terms that significantly predict the treatment (dropping interaction/quadratic terms that significantly predict the outcome). The results did not differ noticeably.

strong effect of job security provisions on youth labor force participation. This effect is nearly identical using both specifications, and similar in size to the coefficient reported in the fixed effects model for women (Table 5.9), using the OECD data for 21 countries from 1985-2007. Rerunning this analysis using the OECD dataset, constraining the sample to the countries and years included in the EULFS dataset, yields results that are nearly identical to those in Table 5.12 results and show no evidence of effect heterogeneity between genders (results not shown).

Table 5.12 Differences-in-differences estimates ("cluster robust" standard errors) of the effect of reforms of employment protection on youth unemployment, employment and labor force (LF) participation rates, 16 Western countries, 1992-2006

I. Effect of employment protection on regular contracts (epr)						
	Unemployment		Employment		LF Participation	
	(C)	(T)	(C)	(T)	(C)	(T)
δ_{epr}	-0.018	-0.003	0.004	-0.010	-0.036***	-0.036***
	(0.042)	(0.048)	(0.007)	(0.010)	(0.004)	(0.006)
R-sq	0.60	0.54	0.48	0.45	0.32	0.31
N	1242	1242	1242	1242	1242	1242
II. Effect of regulations on the usage of temporary contracts (rtc)						
	Unemployment		Employment		LF Participation	
	(C)	(T)	(C)	(T)	(C)	(T)
δ_{rtc}	-0.025	0.019	-0.002	-0.007	-0.004	-0.002
	(0.013)	(0.020)	(0.003)	(0.004)	(0.003)	(0.004)
R-sq	0.59	0.57	0.46	0.42	0.30	0.25
N	1242	1242	1242	1242	1242	1242

Notes: *** p<0.01, ** p<0.05, * p<0.1. epr - Employment protection regular contracts, rtc - Regulations on the usage of temporary contracts, the R-squared statistic is the "within R-squared", see notes to Table 5.5

We routinely conduct two specification checks assessing the sensitivity of the effect of job security provisions on labor force participation to the country composition and the observation window around the reform. We rerun the analyses reported in Table 5.12 dropping one country at a time and record the δ coefficient. Furthermore, we vary the window of observation by extending either the periods prior to change in legislation from t-2 to t-3 or by extending the periods after the change in legislation from t+3 to t+5. These checks suggest that the effects reported in Table 5.12 are robust in sign and significance. There are some indications that particularly over the longer run, more generous job security provisions for regular employment lower both youth employment and unemployment, but these results are not robust to changes in country composition. Regarding regulations on temporary contracts, there is no robust evidence of any average effect on any of the outcomes.

In Table 5.13, we report representative specification checks which we performed for all results reported in this section, in this case for the effect of job security provisions on regular contracts on youth labor force participation rates. The first row records the estimated effects and standard errors for the full country sample. The second row (Max) records the maximum beta coefficient that we obtained after dropping any of the countries, as well as the corresponding country. Conversely, the third row (Min) records the minimal beta coefficient we obtained after dropping any of the countries. We see that using shorter post-reform periods the results are sensitive to country composition. In particular, the Spanish case proves influential. If we consider longer post-reform periods, this sensitivity disappears and effect sizes increase.

Table 5.13 Differences-in-differences estimates ("cluster robust" standard errors) of the effect of reforms of employment protection on regular contracts on youth labor force participation, 16 Western countries, 1992-2006, specification checks

	A. Control (C) Specification				B. Trend (T) Specification			
	t-2 to t+3	t-2 to t+5	t-3 to t+3	t-3 to t+5	t-2 to t+3	t-2 to t+5	t-3 to t+3	t-3 to t+5
δ_{epr}	-0.036***	-0.059***	-0.036***	-0.060***	-0.036***	-0.031***	-0.038***	-0.052***
	(0.004)	(0.008)	(0.007)	(0.010)	(0.006)	(0.009)	(0.010)	(0.008)
Max	-0.039***	-0.063***	-0.040***	-0.066***	-0.046*	-0.050***	-0.046	-0.055***
	DE	US	SE	SE	ES	ES	ES	DK
Min	0.021	0.048***	-0.025	-0.054***	-0.032***	-0.023***	0.032***	-0.048***
	ES	ES	ES	FI	SE	AT	SE	NO

Notes: *** p<0.01, ** p<0.05, * p<0.1. epr - Employment protection regular contracts, rtc - Regulations on the usage of temporary contracts, see notes to Table 5.5

We thus conclude that youth labor supply is sensitive to reforms in the rigidity of job security provisions, particularly over the longer run: A one unit decrease in the corresponding index would raise youth labor force participation by around 4%, which is a sizeable effect. At the same time, we do not observe consistent effects on either unemployment or employment rates. How do we interpret these findings? Assuming that there are adverse effects of job security provisions, we may not observe these effects because young people reduce their labor supply, which may lower unemployment and employment rates by amounts that are too small to reach statistical significance. In support of this, we did observe long-run negative (positive) effects of stricter job security provisions on youth employment (unemployment), which were, however, non-robust to changes in country composition.

The labor supply response is plausible for young people compared to adults. Young people are often not entitled to unemployment benefits and still receive support from their parents, which makes inactivity more attractive relative to unemployment. They also have a lower opportunity cost for returning to the education system. In countries, where higher education is publicly financed, staying enrolled in education and prolonging educational careers is a rational and socially legitimate response to an anticipated tough transition into employment. Young people thereby upgrade their skills and hope to improve their chances. Alternatively, enrolment in active labor market policy measures may be a similarly attractive, alternative labor market state as a response to unemployment risks, which also lowers labor force participation.

To sustain this speculation, more analyses would be required. In the following, we offer one test that assesses whether job security provisions are associated with tertiary enrolments. Unfortunately, we cannot distinguish different inactivity statuses in the EULFS data, and had to rely on aggregate tertiary enrolment data published by the World Bank. We run fixed effects regressions of aggregate tertiary enrolment rates (number of tertiary students divided by the number in the corresponding age group, as defined by the World Bank) on the OECD indices of employment protection, the aggregate male unemployment rate and country-specific linear trends. Given the limited number of observations, we could not run the full DiD specification. We relied on the linear trends specification, which appears suitable given the relatively short time series and the strongly and smoothly trending dependent variable.

Table 5.14 OLS fixed effects (LSDV) regression estimates ("cluster robust" standard errors) of the association between (log) enrolment rates in tertiary education institutions and employment protection, 15 Western Countries (no data on Germany), 1998-2007

	(1)	(2)	(3)	(4)
EPR	0.121	0.160***	0.177***	0.183***
	(0.138)	(0.033)	(0.044)	(0.054)
FTC			-0.008	-0.001
			(0.013)	(0.008)
UAM			0.045	0.021
			(0.038)	(0.030)
Trends	No	Yes	Yes	Yes
Year FE	No	No	No	Yes
R-sq	0.69	0.97	0.97	0.98
N	141	141	141	141

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. UAM is the logged unemployment rate for adult men aged 45-54. EPR - Employment protection regular contracts, RTC - Regulations on the usage of temporary contracts. Trends are country-specific, linear trends, year FE are year fixed effects. Source: Data on tertiary enrolment (number of students / relevant age group) from World Bank, see Data Appendix in Chapter 4, Section 4.6 (p. 106f.)

The results support our speculation. A 1 unit change in the index of employment protection, which corresponds to the observed average size of a reform, is associated with a 18% change in enrolment rates, which is a very large effect. We performed extensive specification checks to tests for the presence of influential outliers, which suggest that Austria contributes influential observations, but they do not alone generate the strong association we observe.[43] We repeated this analysis using the log of expenditures on active labor market policy measures as a percentage of GDP as a dependent variable. We found stricter job security legislation to significantly increase expenditure on such policies, while regulation on temporary contracts had the opposite effect. The results proved sensitive to country composition, though, and require further exploration.

* * *

Returning to the labor market analysis, we now assess whether reforms had different effects on different demographic groups. In Table 5.15, we report results of analyses of the effects of reform of job security provisions on regular contracts, where we estimate separate treatment effects by levels of education and gender. The effects of reform on labor force participation are nearly identical across education groups. Robustness checks suggest a greater sensitivity of these results (in terms of statistical significance) to the presence of Spain in the sample, particularly the effect on the least educated.[44] Moreover, we observe an effect on unemployment among women, which, however, does not prove to be robust to dropping Spain from the sample. Increasing the post-reform observation period to five years after the reform, we observe stronger negative effects on employment in particular among the less educated, but the effects lose statistical significance once Spain is dropped from the sample. Regarding labor force participation, increasing the post-reform observation period to five years after the reform yields insignificant effects for tertiary educated, while the effects for less educated gain in strength and significance.

[43] Specification (3) turned out to be robust, while specification (4) proved sensitive to the presence of Austria. But dropping Austria made the Netherlands and influential observation, and dropping both Netherlands and Austria, we again obtain a statistically significant result, but smaller beta coefficients.

[44] For young people with either ISCED 3-4 or ISCED 5-6 degrees, we observe the results to be sensitive to the exlusion of Spain for either the trend or the control, but not both specifications.

Table 5.15 Differences-in-differences estimates ("cluster robust" standard errors) of the effect of reforms of job security provisions on regular employment contracts on youth unemployment, employment and labor force participation rates, 16 Western countries, 1992-2006, heterogeneous reform effects

	Unemployment		Employment		LF Participation	
δ_{epr}	(C)	(T)	(C)	(T)	(C)	(T)
ISCED 0-2	-0.037	-0.027	0.014	-0.005	-0.033***	-0.032***
	(0.056)	(0.069)	(0.014)	(0.012)	(0.012)	(0.012)
ISCED 3-4	-0.019	-0.022	-0.002	-0.012	-0.035***	-0.035***
	(0.069)	(0.064)	(0.016)	(0.016)	(0.011)	(0.013)
ISCED 5-6	-0.087	-0.047	-0.012	-0.023	-0.037***	-0.037***
	(0.041)	(0.057)	(0.013)	(0.015)	(0.011)	(0.013)
Women	0.059***	0.058***	0.007	0.007	-0.003	-0.003
	(0.018)	(0.017)	(0.008)	(0.008)	(0.010)	(0.010)
R-sq	0.60	0.54	0.48	0.45	0.32	0.31
N	1242	1242	1242	1242	1242	1242

Notes: *** p<0.01, ** p<0.05, * p<0.1. epr - Employment protection regular contracts, rtc - Regulations on the usage of temporary contracts, see notes to Table 5.5

While we could not detect average effects of reforms on the usage of temporary contracts, we discover some effect heterogeneities "beneath the surface" (see Table 5.16). We see that particularly more educated groups may benefit from employment enhancing effects of deregulation. In contrast, there is some evidence that women and the least educated are worse off after deregulation. Deregulation of temporary contracts may also raise youth labor force participation among more educated groups. However, varying the window of observation and the country composition, we find that most of the results are sensitive to the country composition, particularly the presence of Italy and Belgium, which both experienced extensive reforms. Some of the employment effects of deregulation are robust, however: Tertiary graduates benefit from a deregulation of temporary contracts, at least over the short run, considering a period up to three years after reform. Women are worse off in particular over the long run, considering a period up until five years after reform.

Table 5.16 Differences-in-differences estimates ("cluster robust" standard errors) of the effect of reforms of regulations on the usage of temporary contracts on youth unemployment, employment and labor force participation rates, 16 Western countries, 1992-2006, heterogeneous reform effects

	Unemployment		Employment		LF Participation	
δ_{ftc}	(C)	(T)	(C)	(T)	(C)	(T)
ISCED 0-2	-0.025	0.021	0.014*	0.006	0.007	0.008
	(0.015)	(0.018)	(0.006)	(0.006)	(0.006)	(0.007)
ISCED 3-4	-0.030*	0.013	-0.011**	-0.013***	-0.009***	-0.007***
	(0.016)	(0.018)	(0.005)	(0.003)	(0.003)	(0.002)
ISCED 5-6	0.013	0.059**	-0.020***	-0.023***	-0.011**	-0.010**
	(0.017)	(0.024)	(0.005)	(0.005)	(0.005)	(0.005)
Women	-0.023	-0.023	0.007*	0.007*	0.002	0.002
	(0.014)	(0.013)	(0.003)	(0.003)	(0.002)	(0.002)
R-sq	0.59	0.58	0.47	0.43	0.31	0.26
N	1242	1242	1242	1242	1242	1242

Notes: *** p<0.01, ** p<0.05, * p<0.1. epr - Employment protection regular contracts, rtc - Regulations on the usage of temporary contracts, see notes to Table 5.5

Next, we assess interactions between both types of legislation as suggested by Blanchard and Landier (2002) and Cahuc and Postel-Vinay (2002). We begin by running regressions similar to those reported in Table 5.12, assessing whether the effects of changes in the regulation of temporary contracts depend on the strictness of job security provisions on regular contracts. We thus estimate an interaction between the treatment effect δ_{ftc} and the strictness of job security provisions on regular contracts. The results are reported in Table 5.17. The "control" specifications support the hypothesized interaction: If regular jobs are highly protected, tightening regulations on the usage of temporary contracts raises youth employment and labor force participation and diminishes youth unemployment (not significant). Conversely, deregulating temporary contracts if regular contracts are highly protected makes young people worse off on all three outcomes.

Table 5.17 Differences-in-differences estimates ("cluster robust" standard errors) of the effect of reforms of regulations on the usage of temporary contracts on youth unemployment, employment and labor force participation rates, 16 Western countries, 1992-2006, consequence of "partial deregulation”

	Unemployment		Employment		LF Participation	
	(C)	(T)	(C)	(T)	(C)	(T)
δ_{ftc}	-0.028	0.020	0.002	-0.004	-0.001	0.002
	(0.014)	(0.029)	(0.005)	(0.005)	(0.003)	(0.006)
epr * δ_{ftc}	-0.016	0.004	0.023**	0.008	0.014**	0.014
	(0.024)	(0.046)	(0.009)	(0.009)	(0.006)	(0.012)
R-sq	0.59	0.57	0.44	0.42	0.31	0.26
N	1242	1242	1242	1242	1242	1242

Notes: *** p<0.01, ** p<0.05, * p<0.1. epr - Employment protection regular contracts, rtc - Regulations on the usage of temporary contracts, see notes to Table 5.5

However, these effects disappear completely (or lose statistical significance) once we control for linear trends and log adult outcomes.[45] It appears that partial deregulation may be an ineffective policy that was adopted in countries experiencing a deterioration of the youth labor market. We prefer this explanation, particularly since we do not observe significant effects on unemployment, where we would expect the potentially negative effects of partial deregulation to manifest themselves first. Moreover, the effect on employment rates is contrary to our expectation of a substitution of permanent with temporary jobs, which should not lead to a net change in aggregate employment. That we observe a negative effect of partial deregulation should therefore rather be taken as evidence of a general downward trend on the youth labor market, which may have prompted partial deregulation. Partial deregulation in itself then did not lead to a divergence from the underlying negative trend on the youth labor market.

We checked further whether the interaction plays out differently for different demographic groups, but found no significant effects. We also checked for sensitivity to country composition and varied the window of observation, which did not change the results. Finally, we also assessed

[45] Adjusting for linear trends only suffices to destroy the effects.

the "reverse interaction", i.e. whether the effects of reforms of security provisions on regular contracts depend on strictness in the regulations on temporary contracts, but found no evidence in support.

5.3.2. Results for adult men

In the final step, to further clarify the distributional effects of employment protection, we assess its effect on the adult labor market. We focus in particular on the outcomes of men aged 45-54. Consistent with theoretical expectations, we find no evidence that employment protection on regular contracts affects the labor market outcomes of adult men (Table 5.18). This result is similar to what we obtained using a different data set and fixed effects analysis in the preceding section (Table 5.10).

Table 5.18 Differences-in-differences estimates ("cluster robust" standard errors) of the effect of reforms of employment protection on adult men's (ages 45-54) unemployment, employment and labor force participation rates, 16 Western countries, 1992-2006

I. Effect of employment protection on regular contracts (epr)

	Unemployment		Employment		LF Participation	
	(C)	(T)	(C)	(T)	(C)	(T)
δ_{epr}	0.086	0.053	-0.001	0.001	0.003	0.001
	(0.052)	(0.049)	(0.006)	(0.005)	(0.004)	(0.004)
R-sq	0.50	0.47	0.38	0.40	0.40	0.39
N	621	621	621	621	621	621

II. Effect of regulations on the usage of temporary contracts (rtc)

	Unemployment		Employment		LF Participation	
	(C)	(T)	(C)	(T)	(C)	(T)
δ_{rtc}	-0.130***	-0.083***	0.007***	0.005***	0.001	0.002*
	(0.018)	(0.022)	(0.002)	(0.002)	(0.001)	(0.001)
R-sq	0.53	0.47	0.42	0.38	0.43	0.37
N	621	621	621	621	621	621

Notes: *** p<0.01, ** p<0.05, * p<0.1. epr - Employment protection regular contracts, rtc - Regulations on the usage of temporary contracts, see notes to Table 5.5

Examining the distribution of the effects across education groups suggests no significant effect heterogeneity. Robustness checks (including five post-reform years, dropping individual countries from the sample) also do not reveal any significant or sizable effects.

However, the deregulation of temporary contracts appears to have raised unemployment among adult men (confirming the results of the fixed effects analysis in Table 5.10), and lowered their employment rates, although the latter effect is relatively small. These effects are robust to changes in the country composition, but weaken somewhat if we consider longer post-reform periods. Thus, we find support for the alleged adverse effects of deregulation for a demographic group, where we initially did not expect it to be most prevalent. To further clarify these results, we estimate the effects across education groups. We would generally expect that less educated would be more vulnerable to the adverse effects of a deregulation of temporary contracts. Results are reported in Table 5.19.

Table 5.19 Differences-in-differences estimates ("cluster robust" standard errors) of the effect of reforms of regulations on the usage of temporary contracts on adult men's (ages 45-54) unemployment, employment and labor force participation rates, 16 Western countries, 1992-2006, heterogeneous reform effects

	Unemployment		Employment		LF Participation	
δ_{ftc}	(C)	(T)	(C)	(T)	(C)	(T)
ISCED 0-2	-0.135***	-0.082**	0.011***	0.009***	0.002	0.003**
	(0.019)	(0.028)	(0.003)	(0.003)	(0.001)	(0.001)
ISCED 3-4	-0.119***	-0.063**	0.004**	0.003	0.000	0.001
	(0.023)	(0.027)	(0.002)	(0.002)	(0.001)	(0.002)
ISCED 5-6	-0.135***	-0.105**	0.004***	0.004**	0.002*	0.003*
	(0.020)	(0.016)	(0.001)	(0.001)	(0.001)	(0.001)
R-sq	0.53	0.47	0.43	0.38	0.43	0.37
N	621	621	621	621	621	621

Notes: *** p<0.01, ** p<0.05, * p<0.1. epr - Employment protection regular contracts, rtc - Regulations on the usage of temporary contracts, see notes to Table 5.5

Regarding unemployment we do not observe a clear ranking of effects across education groups, at least over the short-run. However, when evaluating the effect over the long run (i.e.

five post-reform years), the effect on tertiary educated disappears. Regarding employment, the adverse effects of deregulation appear to be concentrated among the least educated. The weak effects we observe over the short-run among the more educated groups (ISCED 3-4, 5-6) dissipate over the long run. Thus, the adverse effects of deregulation seem to be concentrated among the less educated, leading to higher inequality in job security among adult men.[46]

5.4. Summary

Consistent with theoretical expectations, the difference-in-difference analysis indicates no effect of reforms of job security provisions on unemployment or employment among either young people or adults. This suggests that reductions of job security provisions are not generally effective policy levers if the goal is to lower youth unemployment or raise employment rates. However, we found some evidence that stricter job security provisions lead to lower labor force participation in particular among the less educated youth, and higher enrolment rates in tertiary education.

This result is plausible if there are indeed negative effects of job security provisions on youth labor market chances, which young people anticipate and to which they respond by improving their competitive position on the labor market through enrolling in or staying enrolled in higher education. Job security provisions may push young people into higher education, who would otherwise not enter higher education. Thus, job security provisions impose costs on individuals, and given that higher education is to a large extent publicly financed in a European context, job security provisions are costly to tax payers as well. Moreover, we would expect that young people respond differently in anticipation of a tough transition from school to work. Especially

[46] We could not find evidence of an interaction effect between job security provisions on regular contracts and the regulation of temporary contracts.

young people with more privileged backgrounds are more likely to go to university under such conditions, while those with less privileged backgrounds find themselves in a tough transition from (lower/incomplete) secondary school to work. Finally, an overinvestment into tertiary education, particularly if young people with low expected returns are pushed into higher education, should lead to a decline in the returns on tertiary degrees, even for those who had high expected returns when deciding to enter higher education (Dolado et al., 2000).

Regarding regulations on temporary contracts, our results differed substantially across education groups. This strong effect heterogeneity probably reflects the different functions that temporary contracts perform on different segments of the labor market. Our results showed no aggregate effects on young people, but we could detect robust, positive effects of deregulation on employment chances of tertiary educated youth, at least over the short run. For those with less than tertiary education, we did not obtain robust results. We found young women to be worse off after deregulation, in particular over the long-run. Moreover, we found that deregulation to be associated with lower employment and higher unemployment among adult men, in particular among less educated. Thus, both women and less educated young men seem to suffer from the negative consequences of deregulation, a destruction of stable employment and an increase in turnover. Highly skilled youth in contrast benefit in terms of higher employment rates, which is consistent with a screening interpretation.

Before drawing further conclusions, we will analyze the effects of employment protection reform on the incidence of temporary employment, which is the goal of the following chapter. However, the results so far do not suggest that lowering job security provisions on regular contracts is a particularly effect tool to improve the labor market outcomes of young people. Deregulation of temporary contracts seems to have improved the outcomes of tertiary educated youth only, and thereby increased inequalities on the labor market suggested by Goux and

Maurin (2005) as well as DiPrete et al. (2006). In particular, employment protection reform is unlikely to improve the outcomes of less educated youth.

5.5. Appendix

Table 5.20 OLS regression estimates ("cluster robust" standard errors) of the association between employment protection legislation and relative youth unemployment rates, different gender and age-groups

I. Ages 15-19

	A. Men				B. Women			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
EPR	-0.12	-0.08	-0.22**	-0.22***	-0.02	-0.03	-0.11	-0.13
	(0.09)	(0.09)	(0.08)	(0.07)	(0.10)	(0.09)	(0.09)	(0.08)
RTC	0.13*	0.15**	0.09*	0.11***	0.22***	0.23***	0.18***	0.21***
	(0.06)	(0.06)	(0.04)	(0.03)	(0.06)	(0.05)	(0.04)	(0.04)
R-sq	0.11	0.36	0.73	0.72	0.28	0.52	0.78	0.77
N	436	436	436	436	436	436	436	436

II. Ages 20-24

	A. Men				B. Women			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
EPR	-0.07	-0.11	-0.17*	-0.11	-0.01	-0.10	-0.13	-0.07
	(0.06)	(0.07)	(0.09)	(0.06)	(0.10)	(0.10)	(0.11)	(0.07)
RTC	0.14**	0.13***	0.09**	0.12***	0.25***	0.22***	0.23***	0.23***
	(0.06)	(0.04)	(0.04)	(0.04)	(0.07)	(0.04)	(0.04)	(0.04)
R-sq	0.25	0.45	0.59	0.53	0.40	0.60	0.74	0.71
N	436	436	436	436	436	436	436	436

III. Ages 25-29

	A. Men				B. Women			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
EPR	-0.05	-0.10	-0.15*	-0.11*	-0.01	-0.10	-0.24**	-0.18**
	(0.05)	(0.06)	(0.07)	(0.06)	(0.09)	(0.09)	(0.09)	(0.07)
RTC	0.11*	0.09*	0.07	0.07*	0.20***	0.17***	0.19***	0.18***
	(0.06)	(0.04)	(0.04)	(0.04)	(0.07)	(0.05)	(0.02)	(0.02)
R-sq	0.19	0.40	0.52	0.46	0.31	0.54	0.77	0.75
N	436	436	436	436	436	436	436	436

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. EPR - Employment protection regular contracts, RTC - Regulations on the usage of temporary contracts, see notes to Table 5.1

Table 5.21 Regression estimates ("cluster robust" standard errors) of the association between employment protection, relative youth (ages 15-29) unemployment rates (U^Y / U^A), absolute youth unemployment rates (U^Y) and adult men's (ages 45-54) unemployment rate (U^A), 21 OECD countries, 1985-2007

	A. Men, aged 15-29			B. Women, age 15-29		
	U^Y/U^A	U^Y	U^A	U^Y/U^A	U^Y	U^A
EPR	-0.11*	-0.04	0.06	-0.09	-0.02	0.06
	(0.06)	(0.07)	(0.09)	(0.08)	(0.08)	(0.09)
RTC	0.10**	0.13***	0.03	0.18***	0.20***	0.03
	(0.05)	(0.04)	(0.05)	(0.04)	(0.04)	(0.05)
GenRR	-0.01	-0.01***	0.00	-0.01	-0.01	0.00
	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)
TradeU	-0.00	0.00	0.00	-0.00	-0.00	0.00
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)
Wage1	0.01	-0.10***	-0.12*	0.03	-0.08*	-0.12*
	(0.04)	(0.04)	(0.06)	(0.06)	(0.04)	(0.06)
Wage2	0.03	-0.04	-0.08	0.08	0.01	-0.08
	(0.05)	(0.08)	(0.11)	(0.07)	(0.11)	(0.11)
Left	0.02*	0.04*	0.02	0.02	0.04	0.02
	(0.01)	(0.03)	(0.03)	(0.01)	(0.02)	(0.03)
ALMP	-0.06	0.30**	0.36	-0.16	0.19	0.35
	(0.19)	(0.14)	(0.26)	(0.22)	(0.16)	(0.26)
PMR	0.04	-0.02	-0.07	0.10	0.05	-0.06
	(0.07)	(0.07)	(0.11)	(0.09)	(0.09)	(0.11)
Glob.	0.00	-0.00	-0.00	0.00	-0.00	-0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
TTS	-0.00	-0.00	0.00	-0.01**	-0.01*	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
LTIR	0.05***	0.02	-0.03	0.06**	0.03*	-0.02
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Infl.	0.08**	-0.02	-0.10**	0.09***	-0.00	-0.10**
	(0.03)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)
OutG	0.00	-0.12***	-0.12***	0.01	-0.10***	-0.12***
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)
CohS	-0.17	0.90***	1.07***	-0.20	0.80***	1.03***
	(0.17)	(0.20)	(0.31)	(0.24)	(0.24)	(0.30)
R-sq	0.42			0.57		
N	436	436		436	436	

Notes: *** p<0.01, ** p<0.05, * p<0.1. The "U^Y/U^A" models were estimated using OLS. The "U^Y" and "U^A" models for each gender are estimated jointly using Seemingly Unrelated Regression (Wooldridge, 2002). All dependent variables in natural logs. EPR - Employment protection regular contracts, RTC - Regulations on the usage of temporary contracts. Control variables: indicators for the generosity of unemployment benefits (GenRR), trade union density (TradeU), two indicators for the coordination of wage bargaining (Wage1, Wage2), indicators for the cabinet power of left parties (Left), expenditure on active labor market policies (ALMP), product market regulations (PMR), economic globalization (Glob.), terms-of-trade shocks (TTS), long-term interest rates (LTIR), the inflation rate (Infl.), output gap (OutG), the youth cohort size (CohS), and year fixed effects; see notes to Table 5.1

Table 5.22 Discrepancies between previous and most recent OECD data releases on youth unemployment and dismissal regulations on regular contracts for the period 1995-1999

	Youth unemployment rate ratios			Employment protection, regular contracts		
	Breen 2005, Table 3	see Section 4.6 (p. 106f.)	(2) - (1)	OECD 1999, Table 2.2	see Section 4.6 (p. 106f.)	(4) - (3)
Australia	2.30	2.39	0.09	1.0	1.4	0.4
Austria	1.46	1.56	0.10	2.6	2.9	0.3
Belgium	2.63	2.64	0.01	1.5	1.7	0.2
Canada	2.00	1.99	-0.01	0.9	1.3	0.4
Czech Rep.	2.25	2.25	0.00	2.8	3.3	0.5
Denmark	1.68	1.79	0.11	1.6	1.6	0.0
Finland	2.23	2.21	-0.02	2.1	2.3	0.2
France	2.46	2.45	-0.01	2.3	2.3	0.0
Germany	1.14	1.12	-0.02	2.8	2.7	-0.1
Greece	3.80	3.80	0.00	2.4	2.3	-0.2
Hungary	2.05	2.08	0.03	2.1	1.9	-0.2
Ireland	1.69	1.65	-0.04	1.6	1.6	0.0
Italy	3.62	3.50	-0.12	2.8	1.8	-1.0
Japan	2.35	2.36	0.01	2.7	1.9	-0.8
Korea	3.31	3.23	-0.08	3.2	2.9	-0.3
Mexico	2.25	2.22	-0.03	2.3	2.3	0.0
Netherlands	2.05	2.18	0.13	3.1	3.1	0.0
N. Zealand	2.40	2.43	0.03	1.7	1.4	-0.4
Norway	3.21	3.49	0.28	2.4	2.3	-0.2
Poland	2.55	2.56	0.01	2.2	2.1	-0.1
Portugal	2.42	2.39	-0.03	4.3	4.3	0.0
Spain	2.04	2.04	0.00	2.6	2.7	0.1
Sweden	2.55	2.37	-0.18	2.8	2.9	0.1
Switzerland	1.52	1.66	0.14	1.2	1.2	0.0
Turkey	2.91	2.90	-0.01	2.6	2.6	0.0
UK	2.15	2.29	0.14	0.8	1.0	0.2
USA	2.79	2.90	0.11	0.2	0.2	0.0
Average	*2.36*	*2.39*	*0.02*	*2.17*	*2.13*	*-0.04*

Notes: Values in column 5 are rounded to the first decimal. Youth unemployment rate ratios are the ratios of the youth (ages 15-24) divided by the aggregate (ages 25-54) unemployment rate.

6

Employment Protection and Temporary Employment

In this chapter, we analyze empirically the relationship between employment protection legislation and temporary employment on the youth labor market. While we argued in Chapter 3 that the relationship between employment protection and youth unemployment is theoretically indeterminate, we can derive clear expectations about the effect of employment protection on the incidence of temporary employment on the youth labor market. We generally expect that stricter job security provisions on regular contracts should raise the incidence of temporary contracts (Polavieja, 2006; Kahn, 2007, 2010). If employees on regular employment contracts enjoy some degree of job protection, which is the case in all Western European countries analyzed here, employers stand to gain from using temporary contracts. Thus, we expect that deregulating constraints on the usage of temporary contracts should lead to a higher incidence of temporary employment. This effect should be particularly pronounced in countries with strict job security provisions on regular contracts, where employers should gain the most from using temporary contracts.

From a human capital perspective, employers should rely on temporary contracts for unskilled jobs, where individuals are most easily replaceable (DiPrete et al., 2006). Since employers have no economic interest in long-term employment relationships, we would expect a substitution of permanent with temporary jobs, to the extent that regulations permit. In contrast, in the high skill labor market, employers partly finance training of newly hired and therefore have incentives to maintain a long-term employment relationships. Temporary contracts are still attractive in order to screen employees, before hiring them on a permanent basis, in particular if strict dismissal regulations make hiring risky. Thus, we would argue that deregulation of temporary contracts should lead to growth in temporary employment across the skill distribution, although the function of temporary contracts in the different skill segments is very different. Temporary jobs are likely to substitute for permanent jobs in the low skill segment, which may raise turnover and unemployment risks among less educated. In the high skill segment, they represent screening devices and stepping stones to permanent employment, which may lower unemployment and raise employment rates among tertiary educated.

As in the preceding chapter, we build our analysis in three steps, starting with a cross-sectional analysis of the association between temporary employment and the two dimensions of employment protection legislation as well as their interaction. Most comparative analyses published on this question rely on cross-sectional data (Booth et al., 2002; Polavieja, 2006; Kahn, 2007; Baranowska and Gebel, 2010; but see Kahn, 2010, for a longitudinal analysis). We use different definitions of the dependent variable, different control variable specifications and both the OECD and EULFS dataset to test the robustness of the associations. In the second step, we assess whether the associations found in the cross-section hold up if we control for country fixed effects. This step is mainly to illustrate the sensitivity of cross-sectional estimates as well as the sensitivity of fixed effects estimates to different control variable specifications. The third step of our analysis uses differences-in-differences estimation to assess the effects of reforms of employment protection legislation on temporary employment on the youth labor market. This is the decisive step of our analysis. We assess whether there have been aggregate reform effects as well as whether demographic groups have been differentially affected, and whether reform in one dimension of employment protection depended on the extent of regulation in the other dimension.

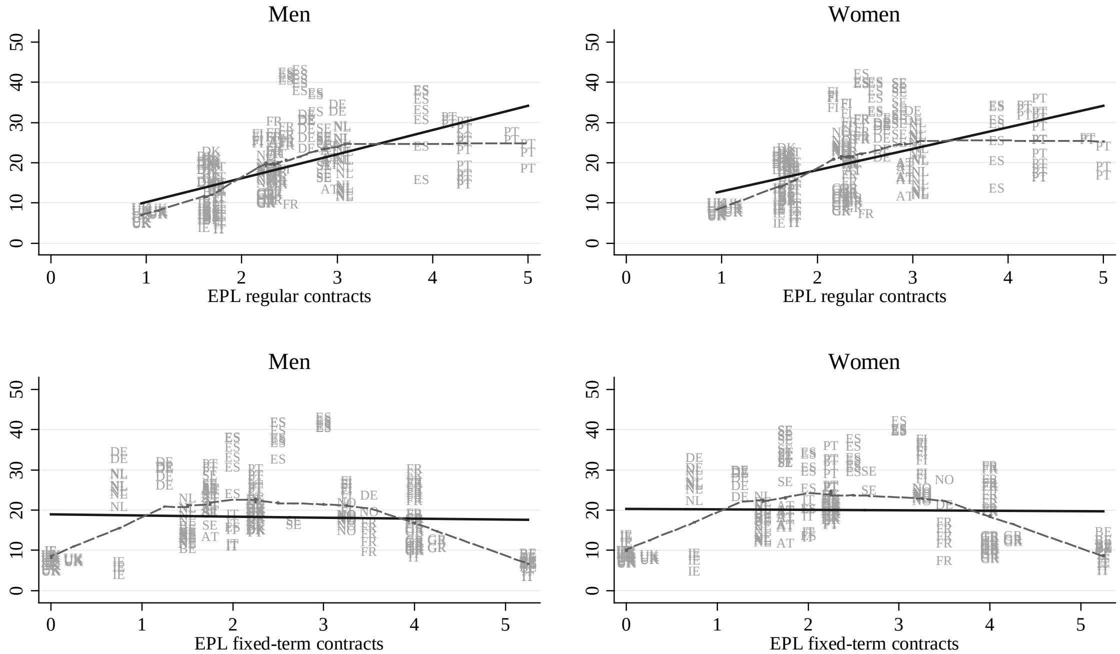
6.1. Results from cross-sectional analysis

While conventions exist on how to define unemployment and inactivity, we lack similar conventions for temporary employment. Past comparative research has tended to focus on temporary employment as a proportion of the employed (e.g. Dolado et al., 2002), which also corresponds to frequently published OECD statistics as used by Booth et al. (2002) and Polavieja (2006). In the following, we will use different definitions that vary the reference quantity, i.e. the percentage of individuals holding a temporary contract among the employed, in the active labor force and in the population. Focusing on the employed or active population already introduces sample selectivity, restricting the sample to individuals who chose to become active or succeeded to obtain a job, which both may be influenced by job security provisions. Indeed, Chapter 5 has provided robust evidence that participation is influenced by job security provisions. It therefore appears preferable to define temporary employment as the number of individuals holding a temporary contract in the population, which we refer to as the “temporary employment rate”. Particularly, if we perform longitudinal analysis, using the population as a reference quantity appears preferable, because it is less vulnerable to unobserved, time-varying factors that are causal antecedents to both temporary employment and employment protection, particularly in the short and medium run. However, we nevertheless consider the alternative definitions, both as robustness checks and because they are commonly used in the literature. For all statistical analyses, we take the natural log of each dependent variable. Information on data sources and control variable definitions is available in Section 4.6 (p. 106f.)

As a first look at the evidence, we plot the incidence of temporary employment on the youth labor market (young people aged 15-29). We use data from 15 Western European countries for the period from 1985 to 2006. We define the incidence here as the number of young people in temporary employment among 15-29 year olds. The results are very similar if we use different definitions of the dependent variable, or if we look at the ratio of youth to adult temporary employment. We also plot the linear regression line and the lowess smoother as a non-parametric measure of association.

We observe that temporary employment is indeed more prevalent on the youth labor market, if dismissal from regular contracts is highly regulated, although this relationship becomes weaker at higher levels of regulation. These results concur with Booth et al. (2002) and Polavieja (2006) who analyze the aggregate incidence of temporary employment on Western European labor markets. Similar to Booth et al. (2002), we also find no linear association between regulations on the usage of temporary contracts and their incidence. Instead the effect appears to be non-linear, although we have no reason to expect a non-linearity: Stricter regulations are associated with higher incidence of temporary contracts for low values of regulation, while at high value of regulation, we observe the expected negative association. In the following section, we will assess the robustness of these associations.

Figure 6.1 Employment protection and temporary employment (as % of the population) among 15-29 year old men and women



Source: EULFS 1985-2006, own calculations

We use OLS regression and adopt different conditioning strategies to assess the robustness of the associations depicted in Figure 6.1. Specification (1) just contains the two regulation indicators. Specification (2) adds a set of control variables as well as year fixed effects. Specification (3) adds all interaction/quadratic terms generated from these control variables that significantly predict either the outcome or the regulation indicators. Specification (4) retains only significant interaction/quadratic terms (see Section 4.4.1, p.93f.).

Table 6.1 presents the results using an unbalanced panel dataset for 20 OECD countries from 1985-2007. The dependent variable here is the log percentage of 15-24 year old dependent employees, who are on a temporary contract. We see that the effect of job security provisions on permanent contracts diminishes upon inclusion of control variables, but it remains sizeable and statistically significant. There is no linear association between the regulations on the usage of temporary contracts and their incidence. Given the absence of a substantive reason, we generally do not include a quadratic term for regulations on temporary contracts, but will test for non-linearities below.

Table 6.1 OLS regression estimates ("cluster robust" standard errors) of the association between employment protection and temporary employment (% on temporary contract among dependent employees), young people aged 15-24, 20 OECD countries, 1985-2007

	A. Men				B. Women			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
EPR	0.31**	0.17*	0.15***	0.15***	0.30**	0.19**	0.19***	0.19***
	(0.13)	(0.09)	(0.05)	(0.05)	(0.10)	(0.09)	(0.06)	(0.06)
RTC	0.03	-0.02	-0.02	-0.02	0.05	0.03	0.01	0.01
	(0.07)	(0.07)	(0.03)	(0.03)	(0.06)	(0.06)	(0.04)	(0.04)
R-sq	0.25	0.58	0.81	0.81	0.28	0.57	0.77	0.77
N	323	323	323	323	323	323	323	323

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. EPR - Employment protection regular contracts, RTC - Regulations on the usage of temporary contracts. Control variables used in specifications (2), (3) and (4): indicators for the generosity of unemployment benefits, trade union density, two indicators for the coordination of wage bargaining, indicators for the cabinet power of left parties, expenditure on active labor market policies, product market regulation, economic globalization, terms-of-trade shocks, long-term interest rates, the inflation rate, output gap, the youth cohort size, and year fixed effects.
Source: OECD Labor Force Statistics, see Section 4.6 (p. 106f.)

Table 6.2 OLS regression estimates ("cluster robust" standard errors) of the association between employment protection and temporary employment, young people aged 15-29, 15 Western European countries, 1992-2006

	% on temporary contracts among employed		% on temporary contracts in labor force		% on temporary contracts in population	
	(1)	(4)	(1)	(4)	(1)	(4)
EPR	0.37***	-0.02	0.39***	0.06	0.41***	0.24***
	(0.11)	(0.03)	(0.12)	(0.03)	(0.14)	(0.04)
RTC	0.11*	0.05**	0.05	0.04*	-0.00	-0.02
	(0.06)	(0.02)	(0.06)	(0.02)	(0.07)	(0.02)
R-sq	0.24	0.51	0.25	0.54	0.23	0.61
N	1152	1152	1152	1152	1152	1152

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. EPR - Employment protection regular contracts, RTC - Regulations on the usage of temporary contracts, see notes to Table 6.1
Source: EULFS data (own calculations), see Section 4.6 (p. 106f.)

In Table 6.2 (see above), we repeated the same analysis using the augmented EULFS dataset for 15 Western European countries (1992-2006), which we also use in the differences-in-differences analysis. The effect of job security provisions on regular contracts is less robust than in the OECD data. At least for the log temporary employment rate (% of individuals on temporary contracts in population), we observe a robust effect that survives inclusion of quadratic/interaction terms generated from the control variables.

In Table 6.3, we have restricted the sample to the years 1996-2006 and added an additional control variable to capture variation in the size of apprenticeship systems (% of ISCED 3-4 students enrolled in programs combining school-based vocational education with on-the-job training), which account for a sizeable share of temporary employment in a number of European countries. In the upper panel, we see that the effect of job security provisions is now highly robust to the inclusion of control variables. The middle panel tests for the existence of a non-linear relationship between the regulation of temporary contracts and their incidence, which is clearly present in the data as suggested by Figure 6.1. It is unclear, however, what this means, since we had no theoretical expectation for this effect to be non-linear. It seems implausible that tighter regulations should be associated with a higher incidence, unless there may be a policy endogeneity such that politicians may unsuccessfully try to restrict the usage of temporary contracts to limit their excessive growth. Evidence for such failed reforms exists (see Dolado et al. 2002, for the Spanish case), and indeed dropping the Spanish case considerably weakens the non-linear effect. The non-linearity may thus be rather due to Spanish exceptionalism in terms of temporary employment (see Polavieja, 2006). The lower panel tests for the presence of an interaction between both types of regulations. While we expect regulations on the usage of temporary contracts to have an effect especially when regular employment is highly protected, there is no evidence whatsoever for this interaction.

Table 6.3 OLS regression estimates ("cluster robust" standard errors) of the association between employment protection and temporary employment, young people aged 15-29, 15 Western European countries, 1996-2006

	% on temporary contracts among employed		% on temporary contracts in labor force		% on temporary contracts in population	
	(1)	(4)	(1)	(4)	(1)	(4)
EPR	0.37***	0.08**	0.41***	0.20***	0.45***	0.28***
	(0.09)	(0.03)	(0.10)	(0.02)	(0.14)	(0.04)
RTC	0.16**	0.06***	0.09	0.07***	0.04	0.05***
	(0.06)	(0.02)	(0.07)	(0.01)	(0.08)	(0.02)
R-sq	0.27	0.49	0.26	0.51	0.25	0.60
N	960	960	960	960	960	960
	(1)	(4)	(1)	(4)	(1)	(4)
EPR	0.26***	0.06***	0.29***	0.19***	0.32**	0.27***
	(0.07)	(0.02)	(0.08)	(0.02)	(0.13)	(0.04)
RTC	0.19***	0.08***	0.12**	0.07***	0.06	0.07***
	(0.04)	(0.01)	(0.05)	(0.02)	(0.07)	(0.02)
RTC^2	-0.11***	-0.04***	-0.12***	-0.02*	-0.12***	-0.02*
	(0.03)	(0.01)	(0.03)	(0.01)	(0.04)	(0.01)
R-sq	0.32	0.49	0.34	0.51	0.33	0.60
N	960	960	960	960	960	960
	(1)	(4)	(1)	(4)	(1)	(4)
EPR	0.31***	0.09**	0.35***	0.21***	0.35**	0.29***
	(0.08)	(0.04)	(0.10)	(0.03)	(0.15)	(0.05)
RTC	0.14**	0.07***	0.07	0.07***	0.01	0.06**
	(0.06)	(0.02)	(0.07)	(0.02)	(0.08)	(0.02)
EPR_RTC	-0.11	0.04	-0.12	0.01	-0.17	0.01
	(0.07)	(0.04)	(0.09)	(0.03)	(0.11)	(0.04)
R-sq	0.27	0.49	0.27	0.52	0.27	0.60
N	960	960	960	960	960	960

Notes: *** p<0.01, ** p<0.05, * p<0.1. EPR - Employment protection regular contracts, RTC - Regulations on the usage of temporary contracts, see notes to Table 6.1
Source: EULFS data (own calculations), see Section 4.6 (p. 106f.)

Finally, we repeated the analysis reported in Table 6.2 for different education groups (results in Table 6.10, Appendix). Focusing on the temporary employment rate (% of temporary contracts in population), we observe the strongest effect of job security provisions on the least educated and on the most educated, particularly in those models that account for control variables.

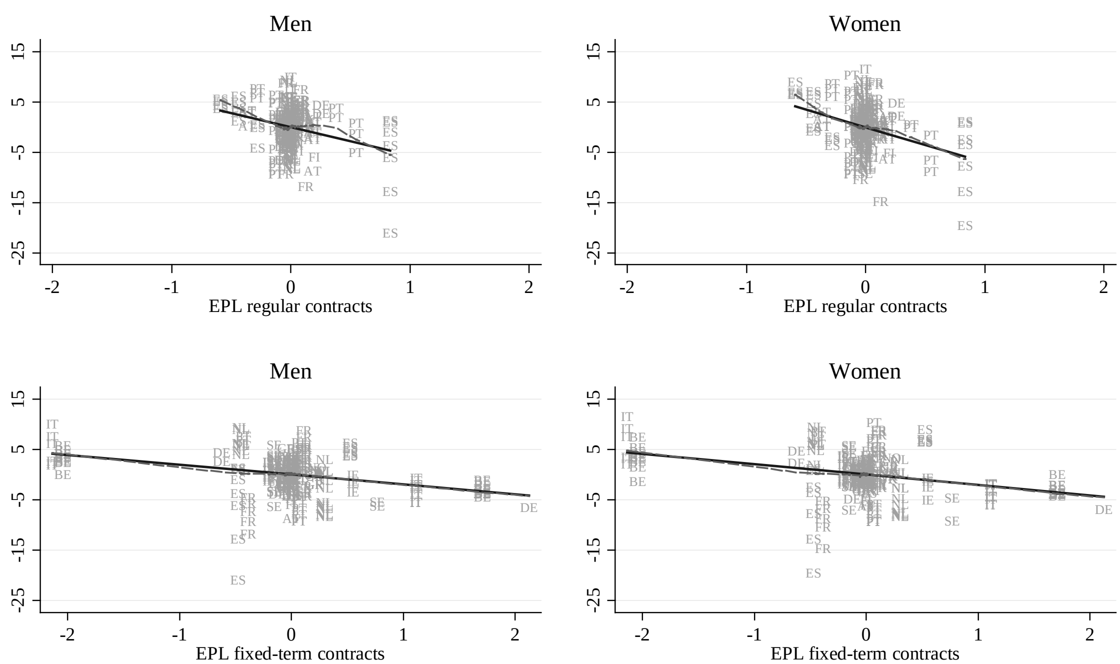
Our findings thus far concur with research published on Western European countries about cross-sectional variation in the aggregate incidence of temporary employment across Western countries. Both Polavieja (2006) and Booth et al. (2002) find job security provisions on permanent contracts to be associated with a higher incidence of temporary employment. This suggests that employers rely on temporary employment as a source of flexibility, if dismissal conditions for regular employees are strict. Like Booth et al (2002), we also find no linear effect of regulations on temporary contracts and their incidence. Kahn (2007) also finds a positive effect of job security provisions on the relative incidence of temporary employment among young people. Compared to these studies, we have used a greater number of observations at the country-year level and used more demanding control variable specifications, but the results proved to be quite robust. In the following, we want to assess how robust they are once we include country fixed effects.

6.2. Results from fixed effects analysis

Among the studies that have used more rigorous identification strategies, Kahn (2010) uses longitudinal data and a fixed effects approach to identify the effect of employment protection reforms on overall and temporary employment. The author finds robust evidence that deregulating the usage of temporary contracts raises their incidence, but does not lead to overall employment gains, which is interpreted as evidence for a substitution of permanent with temporary contracts. An earlier study by the OECD (1999: 80), using cross-national data from two periods and random effects estimators, finds that stricter regulations on permanent contracts are associated with lower temporary employment, which the study dismisses as theoretically implausible.

As a first graphical illustration, we have country-demeaned the same data used to generate Figure 6.1 and now only focus on the within-country variation of the variables. The observed pattern changes considerably (see Figure 5.2). We now observe that increasing jobs security provisions on regular contracts is associated with lower incidence of temporary employment, which defies theoretical expectations. Moreover, we now observe as one might expect that stricter regulations on the usage of temporary employment are associated with a lower incidence of temporary employment. For both indicators of employment protection, we observe the presence of outlying observations, which in part seem responsible for the negative associations. For this reason, it will be an important part of the subsequent analyses to probe the robustness of results to changes in country composition.

Figure 6.2 Employment protection and temporary employment (as % of the population) among 15-29 year old men and women, country-demeaned data



Source: EULFS 1985-2006, own calculations

In the following, we probe the robustness of these associations to inclusion of different control variables using the EULFS and OECD datasets. Table 6.4 reports the results. Specification (1) just includes the two measures of replication and reproduces the results of the cross-sectional analyses in the preceding section. Specification (2) adds country fixed effects, which causes the effect of job security provisions on regular contracts to turn from positive significant ($p<0.05$) to negative significant ($p<0.01$). While we could not observe a linear effect of regulations on the usage of temporary contracts in the cross-section, this effect now appears as expected.

Table 6.4 OLS fixed effects regression estimates ("cluster robust" standard errors) of the association between employment protection and temporary employment, EULFS data (young people, 15 Western European countries, 1992-2006) and OECD data (young people aged 15-24, 20 OECD countries, 1985-2007)

	EULFS, % on temporary contracts in population				OECD, % on temporary contracts among dependent employees			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
EPR	0.41**	-0.26***	-0.20**	0.06	0.30**	-0.18***	0.07	0.21
	(0.14)	(0.04)	(0.08)	(0.11)	(0.12)	(0.05)	(0.10)	(0.17)
RTC	-0.00	-0.14***	-0.13***	-0.03	0.04	-0.19***	-0.11***	-0.08**
	(0.07)	(0.01)	(0.03)	(0.03)	(0.07)	(0.04)	(0.04)	(0.04)
CFE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
YFE	No	No	Yes	Yes	No	No	Yes	Yes
Contr.	No	No	Yes	No	No	No	Yes	No
Trends	No	No	No	Yes	No	No	No	Yes
R-sq	0.23	0.19	0.36	0.45	0.27	0.32	0.58	0.77
N	1152	1152	1152	1152	323	323	323	323

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. EPR - Employment protection regular contracts, RTC - Regulations on the usage of temporary contracts, CFE – country fixed effects, YFE – year fixed effects, Contr. – control variables (see notes to Table 5.1), Trends – country-specific linear trends.
Source: EULFS data (own calculations) and OECD Labor Force Statistics, see Section 4.6 (p. 106f.)

Specification (3) adds year fixed effects and control variables as main effects (no interaction/quadratic terms), while specification (4) adds year fixed effects and country-specific linear trends. Adding country-specific linear trends eliminates the effect of job security provisions on regular contracts in both datasets, as does adding control variables and year fixed effects in the OECD dataset. Furthermore, in the EULFS dataset country-specific linear trends also wipe out the effect of regulations on the usage of temporary contracts. This suggests that in the period from 1992 to 2006, Western European countries traveled along specific temporary employment growth paths, and after adjusting for these trends, there appears to be no change in the growth rate of temporary employment around a reform. Adjusting for trends also destroys the effect of job security provisions (EPR) on temporary employment, a result that is unintelligible in our theoretical framework, but has also been reported by the OECD (1999: p. 88).

In Table 6.5 we repeat the analysis using EULFS data for the three different outcomes analogous to the analyses reported in Table 6.3, but now control for country fixed effects. The theoretically implausible negative effect of job security provisions on temporary employment that appears in the specification without any controls (1) disappears once we add control variables (4) and turns positive and significant in one instance. The effect of regulations on temporary contracts is in the expected theoretical direction and retains a considerable size and significance across specifications. The results suggest that tighter regulations on the usage of temporary contracts lower temporary employment, as reported by Kahn (2010).

Nevertheless, the previous analysis reminds us that countries may experience different growth trends that our control variables fail to fully capture. If temporary employment is growing for reasons unaccounted for by the model, and a number of countries implement deregulatory reforms, we would obtain an association in the fixed effects models even if there was no causal effect. Hence, we consider the control variable specification that accounts for linear trends an important specification to test for this possibility. Finally, we test for an interaction between both types of regulation, expecting that a deregulation of temporary contracts leads to growth of temporary employment especially in the presence of strict dismissal regulations on permanent contracts. The estimated interaction effect, however, rather attests to the – theoretically implausible – opposite at least in the specification where it reaches statistical significance.

Table 6.5 OLS regression estimates ("cluster robust" standard errors) of the association between employment protection and temporary employment, young people aged 15-29, 15 Western European countries, 1996-2006

	% on temporary contracts among employed		% on temporary contracts in labor force		% on temporary contracts in population	
	(1)	(4)	(1)	(4)	(1)	(4)
EPR	-0.14**	0.14**	-0.24***	-0.00	-0.26***	-0.05
	(0.05)	(0.05)	(0.03)	(0.05)	(0.04)	(0.04)
RTC	-0.15***	-0.08***	-0.16***	-0.10***	-0.14***	-0.11***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
R-sq	0.24	0.57	0.25	0.53	0.19	0.50
N	1152	1152	1152	1152	1152	1152
	(1)	(4)	(1)	(4)	(1)	(4)
EPR	-0.16**	0.13**	-0.26***	0.02	-0.29***	-0.02
	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)	(0.03)
RTC	-0.17***	-0.08***	-0.17***	-0.08***	-0.16***	-0.10***
	(0.03)	(0.01)	(0.03)	(0.02)	(0.02)	(0.02)
EPR_RTC	-0.03	-0.00	-0.03	0.04*	-0.06	0.05***
	(0.05)	(0.02)	(0.05)	(0.02)	(0.04)	(0.02)
R-sq	0.25	0.57	0.25	0.53	0.20	0.49
N	1152	1152	1152	1152	1152	1152

Notes: *** p<0.01, ** p<0.05, * p<0.1. EPR - Employment protection regular contracts, RTC - Regulations on the usage of temporary contracts, see notes to Table 6.1
Source: EULFS data (own calculations), see Section 4.6 (p. 106f.)

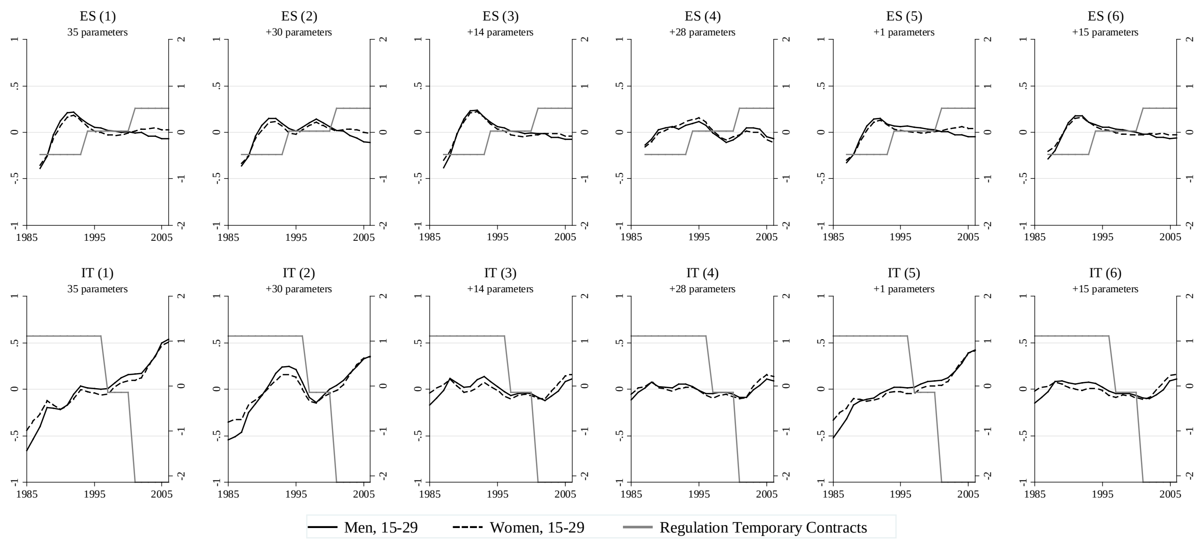
The previous analyses, as in Chapter 5, reminded us again of the sensitivity of cross-sectional inference to unobserved confounders. While the cross-sectional analysis suggested that stricter job security provisions are associated with higher temporary employment, adjusting for fixed effects reversed this association or rendered it insignificant. Moreover, the results regarding regulations of temporary contracts also changed substantively. The cross-sectional analysis attested to a theoretically unexpected non-linearity, where stricter regulations are associated with higher temporary employment for low values of regulation, but we found no evidence for the expected interaction between job security provisions and regulations on temporary contracts. In the fixed effects analysis, the non-linear main effect of regulations disappears (results not shown), and we obtain more theoretically plausible effects similar to those reported by Kahn (2010). However, the results do not survive inclusion of country-specific linear trends. Moreover, we now have to deal with a theoretically unexpected result regarding the interaction between both types of employment protection legislation.

We thus conclude that the results from cross-sectional analysis cannot be interpreted as causal relationships. However, we also view the results of the fixed effects analysis as tentative, because of the sensitivity to inclusion of country-specific trends. We also adopted a rather restrictive control variable specification that does not allow for non-linearities. To illustrate the relative success or failure of different conditioning strategies, Figure 6.3 graphs the smoothed residual from a regression of log youth (ages 15-29) temporary employment using different control variable specifications (15 Western European countries, 1985-2006, n=265). We graph the results for Italy and Spain, which experienced deregulations and reregulation of temporary contracts, respectively.[47] Adjusting for country and year fixed effects (specification 1), we observe still persistent trends in the dependent variable that match the trends observed in the unadjusted data quite closely. In the Spanish case, we observe rapid growth in the 1980s and a reversal in the early 1990s roughly following macroeconomic trends. In Italy, we observe persistent growth, with some cyclical variation. From the Italian case, we would obtain a strong

[47] Specification (1) includes country and year fixed effects only. In (2), we add we add country-specific macroeconomic shocks, i.e. we fit for each country two interaction terms between the country fixed effects and two measures of macroeconomic changes, the output gap and the inflation rate. In (3), we add to (1) country-specific linear trends. (4) is the "restricted specification" adding to (1) control variables and some interaction/quadratic terms. In (5), we add to (1) the (logged) temporary employment rate for adult men, aged 45-54. In (6), we add to (1) country-specific linear trends and the (logged) temporary employment rate for adult men.

effect of deregulation on temporary employment growth in specification (1), but the data clearly show that this growth already commenced more than ten years before deregulation occurred.

Figure 6.3 The effect of using different conditioning strategies, youth temporary employment



Source: EULFS data (own calculations)

Adjusting for country-specific trends is thus a crucial specification check, and may change our inference significantly. The Spanish case illustrates the need to adjust for macroeconomic factors that are strongly associated with the growth of temporary employment (Dolado et al., 2002; Holmlund and Storrie, 2002). The linear trends alone do not succeed in capturing non-linear cyclical fluctuation, as specification (3) shows. In specification (4), we succeed in removing some of the cyclical variation in temporary employment using control variables, including cyclical variables and interactions among these with institutions. However, in this application, we need many more degrees of freedom to achieve a cyclical correction. As in the case of unemployment (Section 4.5.1, p. 101f.), we therefore consider it reasonable to focus on both trend on control variable based specifications, i.e. specifications (4) and (6). Effects should be consistent across specifications.

6.3. Results from differences-in-differences analysis

We proceed analogous to the differences-in-differences (DiD) analysis in the preceding chapter, exclusively relying on the augmented EULFS dataset. We generally perform the same specification checks, varying the window of observation as well as dropping one country at a time and rerunning the analysis. Table 6.6 reports results for average effects of employment protection reforms on temporary employment on the youth labor market.

Table 6.6 Differences-in-differences estimates ("cluster robust" standard errors) of the effect of reforms of employment protection on temporary employment among young people, 15 Western countries, 1992-2006

I. Effect of employment protection on regular contracts (epr)

	% on temporary contracts among employed		% on temporary contracts in labor force		% on temporary contracts in population	
	(C)	(T)	(C)	(T)	(C)	(T)
δ_{epr}	0.035 (0.018)	-0.006 (0.026)	0.033 (0.019)	0.024 (0.028)	0.010 (0.014)	-0.012 (0.029)
R-sq	0.58	0.55	0.56	0.53	0.53	0.49
N	1152	1152	1152	1152	1152	1152

II. Effect of regulations on the usage of temporary contracts (rtc)

	% on temporary contracts among employed		% on temporary contracts in labor force		% on temporary contracts in population	
	(C)	(T)	(C)	(T)	(C)	(T)
δ_{rtc}	-0.035 (0.020)	-0.014 (0.032)	-0.023 (0.024)	-0.015 (0.030)	-0.029 (0.024)	-0.017 (0.030)
R-sq	0.58	0.53	0.54	0.51	0.50	0.46
N	1152	1152	1152	1152	1152	1152

Notes: *** p<0.01, ** p<0.05, * p<0.1. epr - Employment protection regular contracts, rtc - Regulations on the usage of temporary contracts, the R-squared statistic is the "within R-squared", see notes to Table 2.1Table 6.2

We observe that there is no effect of job security provisions on regular contracts on the incidence of temporary employment. Given the widely varying results that we obtained in the previous section, this result may not be surprising. However, regarding the effect of regulations on temporary contracts, we obtain no statistically significant effect either, although the estimated effects are consistently negative, which we expected from theory.[48] Changing the window of observations does not affect these conclusions.

The effect of regulations on temporary contracts is sensitive to the country composition. Dropping Italy turns the effect significant for all three dependent variables. However, after dropping Italy, all significant results are driven by the presence of Belgium. Without Belgium and Italy, none of the results are significant, reproducing the results of the full sample. Since Belgium and Italy are outliers in terms of having experienced comparatively large reforms, and since each country influences the results in opposing directions, we conclude that there is no average effect of reforms of regulations on temporary contracts on the incidence of temporary employment on the youth labor market.

Next, we test whether the effect of reforms is heterogeneous across demographic groups. In Table 6.7, we report the effect of reforms of job security provisions on regular contracts. As in the preceding analysis, reform effects differ across control variable specifications and dependent variable definitions. There is no evidence of reform effects for any of the education groups. The significant effect for women in the middle column disappears if we drop Spain or prolong the window of observation to include five post-reform years.

[48] The control variable specifications differ from the ones adopted in the fixed effects analysis in the preceding section. The control variable based specification in Table 6.4 only contains linear effects, while here we also add nonlinear terms. The trend specification in Table 6.4 does not include the adult outcome variable as a control.

Table 6.7 Differences-in-differences estimates ("cluster robust" standard errors) of the effect of reforms of job security provisions on regular employment contracts on temporary employment among young people, 15 Western countries, 1992-2006, heterogeneous reform effects

	% on temporary contracts among employed		% on temporary contracts in labor force		% on temporary contracts in population	
δ_{epr}	(C)	(T)	(C)	(T)	(C)	(T)
ISCED 0-2	0.014	-0.026	0.017	0.008	-0.003	-0.025
	(0.027)	(0.047)	(0.036)	(0.044)	(0.029)	(0.046)
ISCED 3-4	0.033	-0.011	0.021	0.006	-0.001	-0.030
	(0.033)	(0.045)	(0.036)	(0.046)	(0.038)	(0.054)
ISCED 5-6	-0.003	-0.043	-0.013	-0.016	-0.036	-0.052
	(0.019)	(0.030)	(0.018)	(0.029)	(0.017)	(0.034)
Women	0.041	0.041	0.049**	0.049**	0.046	0.046
	(0.025)	(0.024)	(0.020)	(0.020)	(0.029)	(0.029)
R-sq	0.58	0.55	0.57	0.53	0.53	0.49
N	1152	1152	1152	1152	1152	1152

Notes: *** p<0.01, ** p<0.05, * p<0.1. epr - Employment protection regular contracts, see notes to Table 2.1Table 6.2

Regarding regulations on the usage of temporary contracts, the results suggest that reforms were more effective in regulating temporary employment among tertiary educated, while we observe no significant effects among those with less than tertiary education (Table 6.8). However, also the effects for tertiary educated are sensitive to the choice of control variables, and usually do not survive the inclusion of country-specific linear trends. We also again have to deal with sensitivity to sample composition: Dropping Italy yields statistically significant estimates across education groups, but only in the control variable based specification. Dropping both Italy and Belgium leaves no significant effects whatsoever.

Table 6.8 Differences-in-differences estimates ("cluster robust" standard errors) of the effect of reforms of regulations on the usage of temporary contracts on temporary employment among young people, 15 Western countries, 1992-2006, heterogeneous reform effects

	% on temporary contracts among employed		% on temporary contracts in labor force		% on temporary contracts in population	
δ_{ftc}	(C)	(T)	(C)	(T)	(C)	(T)
ISCED 0-2	-0.034	-0.013	-0.013	-0.004	-0.008	0.004
	(0.029)	(0.042)	(0.036)	(0.041)	(0.039)	(0.045)
ISCED 3-4	-0.032	-0.011	-0.025	-0.017	-0.037	-0.025
	(0.022)	(0.033)	(0.027)	(0.032)	(0.027)	(0.032)
ISCED 5-6	-0.043*	-0.022	-0.046*	-0.037	-0.059***	-0.047*
	(0.020)	(0.029)	(0.022)	(0.026)	(0.019)	(0.024)
Women	0.002	0.002	0.009	0.009	0.011	0.011
	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)
R-sq	0.58	0.53	0.54	0.52	0.50	0.47
N	1152	1152	1152	1152	1152	1152

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. rtc - Regulations on the usage of temporary contracts, see notes to Table 2.1Table 6.2

Finally, we assess whether the deregulation of temporary contracts has led to stronger growth in temporary employment in countries where job security provisions on permanent contracts are particularly strict. The results are reported in Table 6.9. In the basic DiD specification, without controls, the interaction effects between reforms of temporary contracts and the level of employment protection of permanent employment contracts are generally insignificant. Adding control variables, the effect becomes significant in one instance only. However, the effect has the opposite sign of what we expected theoretically, suggesting that stricter regulations on temporary contracts *raise* temporary employment especially when regulations on permanent employment are strict, a result we already obtained in the fixed effects analysis in the preceding section. We further assessed the reverse interaction, i.e. whether reforms of job security provisions on regular contracts depend on the level of regulation on temporary contracts, but found no significant results.

Table 6.9 Differences-in-differences estimates ("cluster robust" standard errors) of the effect of reforms of regulations on the usage of temporary contracts on temporary employment among young people, 15 Western countries, 1992-2006, consequences of "partial deregulation”

	% on temporary contracts among employed		% on temporary contracts in labor force		% on temporary contracts in population	
	(C)	(T)	(C)	(T)	(C)	(T)
δ_{ftc}	-0.014	0.001	-0.034*	-0.001	-0.018	0.001
	(0.020)	(0.028)	(0.019)	(0.027)	(0.021)	(0.027)
epr * δ_{ftc}	-0.000	0.057	0.004	0.053	0.060*	0.068
	(0.032)	(0.036)	(0.026)	(0.037)	(0.032)	(0.040)
R-sq	0.58	0.53	0.55	0.52	0.50	0.47
N	1152	1152	1152	1152	1152	1152

Notes: *** p<0.01, ** p<0.05, * p<0.1. epr - Employment protection regular contracts, rtc - Regulations on the usage of temporary contracts, see notes to Table 2.1Table 6.2

6.4. Summary and discussion

The preceding chapter has shown that there are hardly any robust associations between employment protection legislation and temporary employment on the youth labor market. In particular, the differences-in-differences analysis has shown that neither reforming dismissal restrictions on regular contracts nor reforming regulations on the usage of temporary contracts has affected the incidence of temporary employment. We could reproduce the published result that job security provisions are associated with higher temporary employment in the cross-section (Polavieja, 2006; Kahn, 2007), but this effect became insignificant or changed sign in the longitudinal analysis.

Effects of regulations on temporary contracts proved to be sensitive to control variable specifications and sample composition. While the fixed effects analysis indicated that regulations indeed affect the incidence of temporary employment, we could not rule out that the reform effect is spurious and may be caused by unaccounted growth trends in temporary employment. The cross-sectional and fixed-effects analysis yielded theoretically implausible effects of non-linear effects. Results from the difference-in-difference analysis were not robust to inclusion of country-specific trends and suffered from sensitivity to country composition. At best, there was some evidence that temporary employment among the tertiary educated has been responsive to changes in regulation, although results were not really robust.

The results thus far suggest that reforms in regulations on temporary contracts were ineffective in regulating the growth of temporary employment throughout the 1990s. Taken at face value, this suggests that regulating or reversing the growth of temporary employment is difficult, since reforms have been ineffective. This result is neither implausible nor unprecedented. Kugler et al. (2006), for example, point to a failed reform in Spain in 1994 that sought to restrict the usage of temporary contracts to seasonal work only, but employers were able to circumvent this legislation and use temporary contracts in all types of work, not just seasonal jobs. Employers have clear gains from using temporary contracts that they are unwilling to give up even in the face of tightening regulation.

Apart from employer agency, other factors may contribute to the persistence of temporary employment. For example, Boeri and Jimeno (2005) as well as Autor et al. (2006) point to variable enforcement of employment regulations. Certain regulations on the usage of temporary contracts may be difficult to enforce, particularly if the group affected does not resort to litigation. If temporary workers have or perceive themselves to have weak political or judicial efficacy, they may not challenge employer practice, particularly if high rates of joblessness weaken their bargaining power. Moreover, if labor representatives may have a vested interest in maintaining temporary employees as a buffer workforce for their core constituents, they have no incentive to enforce stricter regulations on the usage of temporary contracts (Bentolila and Dolado, 1994; Polavieja, 2003).

6.5. Appendix

Table 6.10 OLS regression estimates ("cluster robust" standard errors) of the association between employment protection and temporary employment, young people aged 15-29, 15 Western European countries, 1992-2006

	% on temporary contracts among employed		% on temporary contracts in labor force		% on temporary contracts in population	
			ISCED 0-2			
	(1)	(4)	(1)	(4)	(1)	(4)
EPR	0.38	0.12	0.42*	0.22**	0.54*	0.49***
	(0.22)	(0.07)	(0.22)	(0.09)	(0.25)	(0.11)
RTC	0.05	0.02	-0.03	0.00	-0.15	-0.07
	(0.08)	(0.03)	(0.09)	(0.03)	(0.10)	(0.05)
R-sq	0.30	0.77	0.29	0.72	0.29	0.76
N	384	384	384	384	384	384
			ISCED 3-4			
	(1)	(4)	(1)	(4)	(1)	(4)
EPR	0.39***	-0.09	0.40***	-0.03	0.33***	0.12*
	(0.09)	(0.10)	(0.09)	(0.09)	(0.13)	(0.06)
RTC	0.15*	0.10**	0.09	0.06**	0.06	0.07*
	(0.07)	(0.04)	(0.07)	(0.03)	(0.08)	(0.04)
R-sq	0.36	0.80	0.34	0.81	0.24	0.82
N	384	384	384	384	384	384
			ISCED 5-6			
	(1)	(4)	(1)	(4)	(1)	(4)
EPR	0.34***	0.00	0.36***	0.13*	0.37***	0.30***
	(0.08)	(0.05)	(0.08)	(0.06)	(0.08)	(0.07)
RTC	0.14**	0.09**	0.10	0.08***	0.09	0.13***
	(0.06)	(0.03)	(0.06)	(0.02)	(0.06)	(0.03)
R-sq	0.35	0.78	0.34	0.73	0.36	0.71
N	384	384	384	384	384	384

Notes: *** p<0.01, ** p<0.05, * p<0.1. EPR - Employment protection regular contracts, RTC - Regulations on the usage of temporary contracts, see notes to Table 6.1
Source: EULFS data (own calculations), see Section 4.6 (p. 106f.)

7

Revising the Consensus

The goal of this study has been to evaluate distributional effects of employment protection on job insecurity. Across economics and sociology, a consensus has emerged arguing that employment protection legislation (EPL) redistributes secure, permanent jobs towards adult (male) workers, while shifting labor market risks, like unemployment and temporary employment onto young people. We focused on two main dimensions of job insecurity, unemployment and temporary employment, and tested the claim that employment protection legislation raises their incidence on the youth labor market. We used different analytical designs, different data sets, and conditioning strategies to assess effects of regulations on the usage of temporary contracts and job security provisions on regular contracts on different labor market outcomes. We assessed interactions between both dimensions of regulation as well as heterogeneous reform effects across different groups on the youth labor market defined by education and gender. We also considered other outcomes, such as total employment, labor force participation and tertiary enrolment rates, to gain a broader perspective of the effects of employment protection. Our results indicate no robust relationship between employment protection and either temporary employment or unemployment on the youth labor market, which leads us to challenge the prevailing consensus about the distributional effects of employment protection legislation.

Our study does not rule out that deregulating employment protection under specific conditions may prove effective, a point we will pick up again below. However, our findings do suggest that (partially) deregulating employment protection legislation has proven generally ineffective in reducing youth unemployment or regulating the incidence of temporary employment across Western European countries during the period from 1992 to 2006. We therefore do not see a scientific basis for advocating the deregulation of employment protection as a *general* reform strategy to alter the outcomes we have investigated here. This is not the first study to call into question the deregulation of employment protection as an effective reform strategy. Our review of the literature suggested that past empirical research on the effects of employment protection has been inconclusive in several instances, and we also showed that even theoretically the argument that job security provisions raise youth unemployment is not very compelling (see Chapter 3). Therefore, before summarizing and discussing the findings of the present study, we want to briefly survey the making and maintenance of the consensus about the distributional effects of employment protection.

In this regard, we focus particularly on the work of the Organization of Economic Co-operation and Development (OECD), a Paris-based research institution and public policy think tank. The OECD regularly publishes analyses and policy reviews. Its recommendations for reform tend to reflect a scientific consensus that is widely shared among academics, economists in particular. By translating scientific research and opinion into salient policy recommendations, the OECD wields considerable influence in the public and academic debate.

7.1. The making and maintenance of the EPL consensus

From the influential OECD Jobs Study in 1994 to its reassessment in 2006, loosening job security provision and deregulating temporary contracts has, with little adjustments, remained an integral part of the OECD's policy recommendations to ailing economies. The 1994 Jobs Study was a highly publicized analysis of unemployment in advanced economies and proposed several reforms, including the deregulation of employment protection legislation. However, even before the original OECD Jobs study was published, existing theoretical work (Lazear, 1990; Bertola, 1990) pointed to the ambiguity of the relationship between employment protection and unemployment. Subsequent theoretical and empirical work has not settled this question (see Chapter 3). While it has been accepted that the effect of employment protection on aggregate unemployment is indeterminate, the consensus has emerged and been advanced by the OECD that employment protection works against groups with weaker labor force attachment, in particular young people.

OECD Jobs Study 1994, p. 50

"Establish, via legislation or central-level collective agreements, employment security provisions which sanction unfair or discriminatory dismissals, yet allow dismissals which are required on economic grounds. More explicit long-term commitments overriding the latter provision should be negotiated at decentralised level, rather than imposed on all firms.

Loosen mandatory restrictions on dismissals in countries where current provisions appear to seriously hinder economic restructuring and the hiring chances of new labour force entrants.

Permit fixed-term contracts. In order to limit incentive for their use from becoming excessively widespread, mandatory protection for regular contracts could be kept relatively "light", especially at the beginning of the contract. Further, policies should avoid any subsidy to fixed-term employment through the unemployment insurance system. Ways to achieve this could include increased rates of employer contribution; or a requirement that the employer pay a special bonus at the end of a fixed-term contract."

The influential Lazear study (1990) was among the first to hypothesize that employment protection should particularly affect young people: "To the extent that [firing costs] affect the various groups, one expects that new entrants are more likely to be hurt than existing workers, who may benefit from the law. Young workers are disproportionately new entrants so if [firing costs] have effects, they should be more pronounced on young workers" (p. 723). Lazear neither offers a formal argument nor do his analyses support this claim, but later studies did find support (for example, Scarpetta, 1996). Reviewing past work in the field, Esping-Andersen (2000: p. 67) expresses an emergent consensus across economics and sociology that "a nation's strictness of employment regulation has very little to do with its *levels* of unemployment, but probably something to do with the *composition* of unemployment" (emphasis in original). His own analysis support the view that it is particularly youth and less skilled who suffer from employment protection.

A major revision of prior research, however, the 1999 OECD Employment Outlook was somewhat at odds with this consensus. We quote from this publication more extensively, as some results are similar to our findings here: "Simple bivariate associations suggest that stricter EPL raises employment for prime-age men but lowers employment for youths and women, with the overall effect being a net reduction. Similarly, youths and perhaps women appear to bear a larger share of the burden of unemployment. However, these associations tend to be weaker or entirely absent when multivariate techniques are used to control for other factors that influence employment and unemployment levels" (OECD, 1999: 48). Moreover, the "regressions accord with the bivariate analysis in detecting no significant effect of overall EPL strictness on the overall share of temporary employment and, at best, a weak effect for youths. When three separate indicators of EPL strictness are included in the model for total temporary employment, many of the estimated coefficients are large but the signs are uniformly inconsistent with the theoretical predictions that regulation of regular employment increases the incidence of temporary employment while regulation of temporary employment reduces it. Thus, the analysis does not confirm earlier research that found an important role for EPL in encouraging the growth of temporary employment" (OECD 1999: 79f.). The findings suggest that the consensus is not supported by the analysis, as soon as it moves beyond tests of bivariate association.

In 2004, however, the OECD affirms the official consensus: "While EPL is generally shown to have little or no effects on the employment rates of prime-age men, several studies suggest that stringent employment protection tends to decrease the employment rates of both youth and women" (OECD 2004: p. 81). At the same, their own analytical results "vary in significance" (OECD 2004: p. 85), i.e. the negative effect of employment protection on youth employment is washed away completely, once country fixed effects are controlled for, echoing the result of the 1999 study. Nevertheless, this is not interpreted as evidence against the consensus, and under the heading "main findings" the OECD concludes from their literature review and analysis that “it is possible to detect a link between EPL and employment rates for specific groups. Some studies, as well as the analysis presented in this chapter, suggest the possibility of a negative link between strict EPL and the employment rates of youth and prime-age women, while there may be positive links to the employment rates of other groups. [...] Indeed youth and prime-age women are more likely to be subject to entry problems in the labour market than is the case with other groups, and they are therefore likely to be disproportionately affected by the effects of EPL on firms’ hiring decisions” (OECD 2004: 63). While the conclusion is guarded with subjunctives, it only partly reflects the results of the analysis, which are sensitive to sensible specification changes.

There have certainly been dissenting voices and findings over the past decades (Lazear, 1990; OECD 1999, 2004, see also Nickell, 1997; Gangl et al., 2003: p. 287; Baccaro and Rei, 2007; Howell et al., 2007; Baranowska and Gebel, 2010), but other scholars offered cross-sectional evidence in support for the alleged adverse effects of employment protection on youth (un)employment (Esping-Andersen, 2000; Russell and O'Connell, 2001; van der Velden and Wolbers, 2003; Breen, 2005; Kahn, 2007) and temporary employment (Booth et al., 2002; van der Velden and Wolbers, 2003; Polavieja, 2006; Kahn, 2007). [49] Two studies published by the OECD use longitudinal data and panel estimators to support the view that employment protection lowers youth employment (Scarpetta, 1996; Bassanini and Duval, 2006), while Bertola et al. (2007) find mixed support. However, we should ask whether these results would be affected by more rigorous specification checks, by adjusting the standard errors for clustering of observations within countries, or using the updated OECD indicator of employment protection legislation.

Re-assessing the 1994 Jobs Study, the OECD concludes in 2006 that “[t]here is evidence that too-strict legislation will hamper labour mobility, reduce the dynamic efficiency of the economy and restrain job creation. This may worsen job prospects of certain groups, like young people, women and the long-term unemployed” (OECD, 2006: 12). The re-assessment then concludes that job security provisions and temporary contracts should be further deregulated in some countries. Absorbing the critique of the adverse effects of "partial deregulation" (Blanchard and Landier, 2002; Cahuc and Postel-Vinay, 2002), they recommend a more balanced approach between both types of regulation.

It should be pointed out that the OECD's own analyses (OECD 1999, 2004) on the effects of employment protection on aggregate youth labor market outcomes, at least those attempting somewhat more rigorous tests, fail to support their policy recommendations. While presenting a balanced theoretical discussion and guarding empirical findings with the necessary subjunctives and footnotes, the policy recommendations default on the rather market friendly, deregulatory

[49] Most of these studies relied on the 1999 indicator of employment protection (OECD, 1999), see discussion in Section 4.1.3 (p. 82f.).

consensus, which belies the inconsistent results of their own as well as published empirical work. Given its role as a consultative body, there are incentives to present solutions even when empirical is ambiguous. And given this ambiguity, there may be a tendency to fall back on the presently operative consensus.

> *OECD Employment Outlook 2006* **-** *Re-assessing the Jobs Study, p. 22*
>
> "Make sure that employment protection legislation helps labour-market dynamism and provides security to workers
>
> C7. Employment protection legislation should be reformed in countries where it is overly strict, by sanctioning unfair dismissal (for example by prohibiting dismissal on the basis of discrimination with respect to gender, age and ethnicity), but reducing constraints on dismissals for economic reasons; severance costs and administrative procedures should be made more predictable so as to reduce judicial uncertainty; reasonable dismissal notice periods should be provided so as to help laid-off workers find new jobs.
>
> C8. Regulations on fixed-term and temporary contracts may need to be relaxed in some countries; in order not to aggravate labour market duality and thus undermine labour market performance in the long term, a more balanced treatment between temporary and permanent contracts should be pursued, with one option being that dismissal protection rights grow in line with seniority"

7.2. Has deregulation made young people better off?

In Chapter 2, we documented a gradual erosion of job security on the youth labor market over the past four decades, despite ever increasing educational attainment and shrinking cohort sizes. The pattern observed fits with a modified version of the "unified theory". Liberal Anglo-Saxon countries have witnessed rising earnings inequality, where income gaps have grown both between education and age groups. In Continental and Southern Europe, as well as Scandinavia, we observed growing gaps in job security between education and age groups. Young people and less educated youth in particular have borne a disproportionate share of the burden of adjustment to adverse macroeconomic trends. Even corporatist model countries like Sweden or Germany could no longer contain exclusionary tendencies and witnessed, not unlike countries bordering the Mediterranean, a gradual retrenchment of secure, full-time jobs among younger cohorts. Apart from few outbursts of violence among discontent youth in France or Greece, these tendencies have been hardly politicized, or were greeted as the intended, positive results of deregulation/flexibilization; and they are likely to be amplified by the recent economic crisis

In Chapter 3, we discussed theoretically the relationship between employment protection and job insecurity. Employment protection legislation appears to be a profound violation of the often cited need for more flexibility. Failing to document links to aggregate unemployment in particular, economists have refocused the critique of employment protection in terms of its distributional effects, stating that it makes young people worse off in relative or absolute terms. Against this consensus, we argued that the effect of employment protection on youth unemployment is theoretically indeterminate. However, we can draw clear links between employment protection and the growth of temporary employment.

7.2.1. The effects of job security provisions on regular employment contracts

Starting with the effects of job security provisions on regular employment contracts, we observe no effect of reforms on either employment or unemployment among either young people or adult men. For Western European countries, this suggests that, contrary to OECD (2004, 2006) recommendations, lowering job security provisions on regular employment contracts is unlikely to raise employment or lower unemployment among young people or adult men. Reforms will therefore also not redistribute unemployment risks between these groups. Maybe more controversially, our findings also show that reforms of job security provisions are unlikely to affect the incidence of temporary employment among young people. Using longitudinal data and adjusting for country fixed effect was sufficient not only to wipe out the positive effect of job security provisions, but even to reverse its sign. Neither for employment, unemployment nor temporary employment could we find any evidence that the effect of job security provisions differ significantly across education groups or depends on the extent of regulations on temporary contracts. However, we did find some indication that strict job security provisions constrain labor market opportunities of young people, to which they respond by dropping out of or staying out of the labor market. Some may re-enter or prolong their educational career in higher education institutions. However, while raising tertiary enrolment and improving the skills of young people is an important policy goal in itself, this "solution" may neither be economically viable nor socially justifiable.

Sociological research on tertiary enrolment attests to strong social selectivity in terms of eligibility for and enrolment in higher education (Arum et al., 2007). Conditional on individual ability, young people from more privileged social backgrounds are more likely to respond to perceived labor market difficulties with investing into more education. Young people from less privileged backgrounds are less likely to be eligible for or enroll in higher education and more likely face a tough transition to employment. Thus, young people from privileged backgrounds are relatively more likely to be pushed into higher education by adverse labor market conditions. Moreover, if young people from privileged backgrounds are thus pushed into higher education, although they would not enter higher education if labor market conditions were normal, this probably also increases adverse selection among tertiary students.

Moreover, apart from raising educational inequalities and adverse selection into higher education, producing an excess number of graduates beyond market demands is in itself likely to bring down the returns to higher education, even for those who had high expected returns when they decided to enter higher education. As a result, tertiary educated youth, after spending 15 or more years in the education system, may have to settle for jobs that require increasingly lower qualification levels. Over-educated youth then displace less educated youth from their skill-adequate employment, which may lead to a process of crowding out. This description seems to fit the Spanish experience (Dolado et al., 2000) well and may also characterize other European labor markets (Gangl, 2002).

Potential adverse effects of job security provisions on unemployment may be masked by young people's labor supply response, particularly in countries with public higher education systems. However, we do not observe a correlation between job security provisions and labor force participation in the cross-section. Moreover, it is somewhat puzzling, that we do not observe a corresponding negative effect of job security provisions particularly on employment rates. The results of the fixed effects analysis point in this direction (Table 5.1, p. 117) but the difference-in-difference analysis finds no support (Table 5.15, p. 142).[50] Possibly, the effects are too weak to reach statistical significance in this study. Otherwise, I am not aware of any other study that has looked at the effects of job security provisions on youth labor force participation, since studies usually focus on either employment or unemployment. It would be a worthwhile task for future research to examine this issue more closely and look at the individual transitions between education, inactivity, unemployment and employment in relation to macroeconomic and institutional conditions (see for example, Azmat et al., 2006).

7.2.2. The effects of regulations on the usage of temporary contracts

Governments responded to the call for deregulation primarily by lowering constraints on the usage of temporary contracts (see Chapter 3). We could not detect aggregate effects of the deregulation of temporary contracts on employment, unemployment and labor force

[50] Some published research using fixed effects analysis (Bassanini and Duval, 2006; Bertola et al., 2002) do find negative effects, while Bertola et al., (2007) do not find negative effects.

participation. We also could not find any evidence that the effect of deregulation depends on the strictness of job security provisions on regular contracts, thus finding no support for the critics of partial deregulation (Blanchard and Landier, 2002; Cahuc and Postel-Vinay, 2002). However, beneath the aggregate level we found some evidence that lower regulations on temporary contracts increase overall employment among tertiary educated, while less educated adult men seemed to have suffered from deregulation in term of higher unemployment and lower employment. These results agree with Goux and Maurin (2005) and DiPrete et al. (2006) who suggest that skill-biased market shifts have led to a growth in skill-gaps in job security, facilitated by the deregulation of temporary contracts. The distributional effects of temporary contracts seem to fit the different functions that temporary contracts play in the different segments of the labor market, i.e. “stepping-stones” to permanent employment among highly educated and substitutes for permanent contracts among the less educated. In the countries and periods observed, deregulation of temporary employment has neither increased (temporary) employment nor diminished unemployment among the less educated.

These effects do not seem to depend on the strictness of job security provisions on regular employment contracts. None of our results suggest that deregulating temporary contracts while maintaining job security provisions on permanent contracts makes young people (or adult men) worse off. Blanchard and Landier (2002) and Cahuc and Postel-Vinay (2002) sought to expose the unintended consequences of partial labor market reform. While this view has been absorbed by the OECD, who now recommends a balanced deregulatory approach (see Table 1 above) to avoid the potentially negative effects of partial labor market reform (Blanchard and Landier, 2002; Cahuc and Postel-Vinay, 2002), there has been to date no single published rigorous assessment of this claim (but see Giesecke and Gebel, 2009b), apart from Blanchard and Landier’s (2002) illustrative trend charts for France. Moreover, as we argued in Chapter 3, neither theoretical analysis allows for strong predictions. In Blanchard and Landier's analysis, the effect of partial deregulation on unemployment is in fact ambiguous (see Section 3.3.2) and in Cahuc and Postel-Vinay's analysis, we obtain rather small effects at rather extreme values. On the basis of our analysis, we would reject the claim that partial deregulation has had an effect on the outcomes we studied.

7.3. Methodological issues

Particularly in our analysis of youth unemployment, where we could compare our results to a number of published studies, we noted the lack of robustness of even cross-sectional results. While past cross-sectional studies found a positive effect of job security provisions on youth unemployment, we found none or even a negative effect. Adjusting for fixed effects, the association disappeared fully. While we observed an extremely robust positive effect of regulations on temporary contracts on unemployment in the cross-section, this effect disappeared completely once we adjusted for fixed effects. Regarding temporary employment, we found the positive effect of job security provisions that other cross-sectional studies had also found, but the effect switched from positive significant to negative significant when adjusting for fixed effects, before disappearing after adding control variables.[51]

Stopping at any step along the way would allow us to tell quite different stories. One lesson in this application has been that cross-sectional results were not robust and should not be considered a good basis for policy recommendations, especially if longitudinal data sources are available. Single country studies, where we assess the effect of a single reform over time, are

[51] While we remain wary of attenuation bias in the fixed effects analysis and the lack of variation on our reform indicator, in particular for job security provisions, this cannot by itself account for the lack of robustness we observe.

likely to yield more convincing, causal estimates (e.g. Kugler et al., 2005; Autor et al., 2006; Bauer et al., 2007). Observations from different, adjacent years in the same country tend to be more similar in terms of unobservables than observations from different countries in the same year. The drawback of single country studies is that they are informative about one particular reform in one particular country, where the reform that is analyzed often occurred at least ten years before the date of publication. Therefore, it is not clear whether the results still apply to the current situation in the same or other countries.[52] Differences-in-differences analyses of pooled longitudinal data on reforms from different countries represents another possibility where we lose some internal validity, but obtain potentially more generalizable findings.

Another important concern is the estimation of standard errors. Methodological research has made considerable progress in specifying the conditions and developing estimators for causal inference (Imbens and Wooldridge, 2009). However, less seems to be known about how to obtain unbiased standard errors if residuals violate classical assumptions (Bertrand et al., 2004; Angrist and Pischke, 2009). In this application, we relied on the recommendation that has emerged in the literature and used a generalized Huber-White estimator, which allows for arbitrary patterns of correlation among the residuals within panel units, although our number of panel units falls short of what one would consider sufficient. The simulations by Bertrand et al (2004) suggest that the method chosen here is superior to the commonly used panel-specific AR1 correction that similar analyses to this one have used (Bertola et al., 2007; Nickell et al., 2005). The AR1 correction

[52] Interestingly, recent studies that use differences-in-differences to evaluate individual employment protection reforms in one country do not generally find significant effects. An Italian study finds the expected effects on turnover (Kugler and Pica, 2008), but some studies on Germany do not find effects (Hunt, 2000; Bauer et al., 2007), while others do (Boockmann et al., 2008). Kugler et al. (2005) find strong distributional/segmenting effects of a Spanish reform, but it is less clear whether these effects are produced by the reduction of dismissal restrictions or the simultaneous, age-selective cut in payroll taxes. Autor et al. (2006) find modest employment effects of the passage of the implied contract exception in the United States, particularly among young people, which however seem to dissipate over time.

tends to produce standard errors that are biased downwards, which would lead us to falsely conclude that employment protection has an effect, which is really just due to serial correlation in the error term.

Apart from specification searches, standard error estimation also adds to the asymmetry in producing quantitative results for publication that exists between researchers and reviewers, in particular if reaching conventional significance levels is assigned an important role in the selection of results for publication.[53] Regarding the effects of employment protection legislation on the youth labor market, incorrectly estimated standard errors may be part of the explanation behind the persistence of the deregulatory consensus. Properly calculating standard errors is therefore decisive for sound policy advice, but more research and guidance on the properties of different standard error estimators would be helpful.

7.4. Why has job insecurity increased on the youth labor market?

This study challenges the common view that employment protection legislation is to blame for high temporary employment or unemployment on the youth labor market. We have shown here using some of the most recent data available that deregulation of employment protection has neither lowered youth unemployment nor affected the growth of temporary employment in Western European countries between 1992 and 2006. In the following, we want to discuss potential ways, in which employment protection may have had effects that we could not or did not capture here; and we want to conclude with a short discussion of alternative explanations for the growth and persistence of job insecurity on the youth labor market.

[53] In a review of leading sociology journals, there is an usual clustering of results just below the 0.05 p-value, which Gerber and Malhotra (2008) argue would have occurred by chance with a probability of 1 in 10 million. They interpret this result as strong evidence for publication bias.

First, employment protection may lead to deterioration on the youth labor market only under specific macroeconomic conditions, which we did not specify here or that were not present in our period of observation. Focusing on the period from 1970 to 1996, Jimeno and Rodriguez-Palenzuela (2002) directly address this issue, but do not find robust effects of interactions between employment protection and observed or unobserved macroeconomic shocks. [54] Similarly, initial deregulations on the usage of temporary contracts that occurred before our period of observation quite plausibly set off growth trends in temporary employment. However, our findings suggest that after temporary contracts have become established means of flexibilization, regulating their incidence seems to be difficult.

Second, employment protection may only be consequential when other institutional conditions are present. For example, economic approaches assume that for firing costs to produce an employment response, employers must constrained in their ability to transfer these firing costs onto workers in the form of wage cuts (Lazear, 1990; Cahuc and Zylberberg, 2007; Kahn, 2007). If wages are collectively bargained over, employers may not be able to turn firing costs into wage cuts and are instead forced to adjust their hiring behavior. Hence, we should expect stronger effects of employment protection when wage flexibility is constrained. Other institutional factors may similarly act as constraints on wage flexibility, especially at the lower end of the wage distribution, for example minimum wages or unemployment benefits. Moreover, other institutions may act as complements to employment protection legislation, mitigating its potentially adverse effects, for example vocational education systems (Breen, 2005) or active labor market policies.

[54] A practical concern here is that there does not seem to be agreement on which macroeconomic conditions are relevant (inflation, terms-of-trade shocks, shocks to labor productivity, shocks to economic output, interest rates etc.) and how to best model these interactions.

Relatedly, under some circumstances, union behavior may lead to inequalities in the distribution of employment and selective non-participation in the labor force. In particular, if trade unions manage to extract wage increases beyond productivity increases, employers are forced to adjust employment levels. Unions may accept these employment effects to the extent that they obtain or safeguard rents for their core constituents (Bertola et al., 2007). If labor supply of those who suffer from disemployment effects is more elastic, there may be fewer incentives for unions to take their concerns into account. Compared to adult men, diminished labor market opportunities may be less costly for young people, because of the existence of alternative labor market states, like continuing or re-entering higher education. However, the impact of unions on wage setting and employment likely depends on the coordination of collective bargaining (Goldthorpe, 1984; Kenworthy, 2002).

There are also a number of plausible alternative explanations for the growth and persistence of temporary employment. In general, young people, if they cannot find permanent jobs, should be more likely to accept temporary jobs. High rates of temporary employment then reflect more general problems on the (youth) labor market, like weak aggregate demand. However, specific institutional factors may also affect their incidence. First, it is possible that labor representatives have acquiesced to the growth of temporary employment. They may have agreed to redistributing risks onto groups that are perceived to be less electorally relevant, while defending or extending rights of their core constituents (Rueda, 2005, 2007). Second, employment protection regulations may not be properly enforced, because particularly those affected by irregular (illegal) temporary employment do not litigate. Particularly in the case of irregular temporary employment, young people may not see themselves in a position to successfully litigate, if they lack the resources, if jobs are scarce, and if temporary employment is considered a norm that is implicitly backed by labor representatives. Forcing employers to convert a temporary into a permanent contract in court may not be appealing, if young people are then faced with negative consequence for their further career with that employer.

In conclusion, we clearly require further efforts to better understand how job insecurity and employment outcomes are distributed across advanced economies, and in particular why some societies impose a disproportionate share of the burden of adjustment onto young people. Rising wage inequality has received a lot of attention, but we know much less about rising inequality in the distribution of job security as well as quality of employment. In this analysis, we have shown drastic inequalities between demographic groups in the incidence of temporary employment and unemployment. These inequalities should principally be amenable to policy intervention to improve the outcomes of groups at risk. Improving young people's outcomes in particular should remain a central concern, and social science should continue to strive to find politically viable solutions.

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