# Does Employment Protection Help Immigrants?
# Evidence from European Labor Markets

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# ABSTRACT

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High levels of employment protection reduce hiring and firing and have a theoretically ambiguous effect on the employment level. Immigrants, being new to the labor market, may be less aware of employment protection regulations and less likely to claim their rights, which may create a gap between the costs for employers of hiring a native relative to hiring an immigrant. This paper tests that hypothesis drawing on evidence for the EU and on two natural experiments for Spain and Italy. The results suggest that strict employment protection legislation (EPL) gives immigrants a comparative advantage relative to natives. Stricter EPL is found to reduce employment and reduce hiring and firing rates for natives. By contrast, stricter EPL has no effect on most immigrants and may even increase employment rates for those who have been in the country for a longer time.

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# 1 Introduction

This paper is concerned with two heavily discussed topics in the literature on European labor markets: immigrant assimilation (how well immigrants do in the labor market), and the effect of Employment Protection Legislation (EPL) on job flows and employment.

EPL includes several elements, such as notification procedures for dismissal, rules regarding the classification of dismissals as 'unfair', the amount of compensation following 'unfair' dismissals, etc. Depending on the reasons behind the firing, regulations in most European countries distinguish between redundancies (due to economic reasons, for example, poor sales, loss of competitiveness, economic recession, etc.) and disciplinary dismissals (due to misconduct, absenteeism, negligence, etc.). Redundancies are generally considered 'fair' dismissals and the worker is entitled to a severance payment. Disciplinary dismissals do not imply any severance payment, but the worker may take the case to the labor authority if he believes the dismissal was 'unfair'. In this situation, one outcome could be compensation through conciliation, where the firm and the worker meet with the labor authority in order to reach a settlement. When conciliation fails, the case goes to the labor courts. If the court decision favors the worker and the judge declares the dismissal 'unfair', then severance payments are increased.

Because EPL increases firms' firing costs, it may impact on job flows and the level of employment. There has been an extensive amount of research exploring these relations. Theoretical models, such as the flow model with endogenous job destruction of Pissarides (2000), show that an increase in employment protection reduces both job creation and job destruction and has an ambiguous effect on employment.

The purpose of this paper is to examine the differential impact of EPL on the labor market outcomes of natives and immigrants, in particular to ask whether immigrants gain a comparative advantage relative to natives in highly regulated labor markets. This could be the case if immigrants are less covered by EPL than natives. Because the analysis here focuses solely on legal immigrants, there is no difference between the legal status of natives and immigrants, so the same employment protection regulations apply to both. However, being new to the country, immigrants may be less informed about their rights and may, for example, not seek to claim compensation for 'unfair' dismissal. Therefore, while the *legal* coverage of EPL is the same for natives and immigrants, the *effective* coverage may be lower for immigrants.

One reason why immigrants may be less informed about their rights is if they are relatively less likely to join unions than natives. Figure 1 reports union membership rates by citizenship

for a number of European countries, the US and Canada, taken from the 2003 International Social Survey Programme (ISSP). Union membership varies widely across Europe, and is typically highest in northern Europe. In all countries, natives have higher union membership rates than immigrants. For the US, these numbers can be compared with data from the Current Population Survey (CPS), reported in MPI (2004). In the CPS, 12.9% natives and 10.2% immigrants were union members in 2003. The evidence on union membership supports the hypothesis that there may be an information gap between immigrants and natives regarding labor market regulations.

To test whether immigrants gain a comparative advantage in heavily regulated labor markets, this paper draws on three pieces of evidence. The first is the EU Labour Force Survey (LFS), which covers 15 European countries from the mid-1990s to 2005. The other two sources of evidence are natural experiments, based on labor market reforms in Spain and Italy. The Spanish reforms were enacted in 1997 and 2001 and introduced a new type of permanent contract with lower dismissal costs for some categories of workers, namely unemployed people between the ages of 16 and 30 and over age 45. The Italian reform was enacted in 1990 and increased dismissal costs for permanent contracts in firms with under 15 employees. The heterogeneity in the application of both these reforms allows the design of natural experiments that can be used to examine the effect of changes in dismissal costs on employment of natives and immigrants.

All these pieces of evidence tell a consistent story and confirm the hypothesis that immigrants obtain a comparative advantage in heavily regulated labor markets. The evidence from the EU LFS shows that EPL (measured by a time-varying index constructed from the OECD synthetic indices) reduces employment for natives but not for immigrants. The results suggest that EPL may even increase employment for immigrants who have been in the country for a longer time. The natural experiment for Spain shows that a reduction in dismissal costs increased permanent employment for natives but not for immigrants. Finally, the evidence for Italy shows that an increase in dismissal costs decreased both hiring and firing, with a larger effect on natives than on immigrants.

The empirical framework used in these three applications follows closely the methodology in Angrist and Kugler (2003), Kugler et al (2002), and Kugler and Pica (2005). Angrist and Kugler (2003) examine how labor and product market rigidities affect the impact of immigration on the employment of natives. Using data from the EU LFS, they find that immigration generates larger job losses for natives in countries with more restrictive regulations. Kugler et al (2002) and Kugler and Pica (2005) analyze the 1997 Spanish reform and the 1990 Italian reform using the same data sources and research design as this paper. However, they do not

distinguish between natives and immigrants. This paper extends their analysis by looking at the differential impact of EPL on these two groups of workers.

The paper is structured as follows. Section 2 presents a simple model of the effect of EPL on the labor market. Section 3 briefly reviews some key empirical studies. Section 4 presents evidence based on the EU LFS. Section 5 presents the results of a natural experiment for the 1997 and 2001 Spanish reforms. Section 6 looks at the 1990 Italian reform. Section 7 concludes.

## 2    Theoretical Framework

To analyze the effect of EPL on the labor market outcomes of natives and immigrants, a simple model is constructed, drawing on the flow model with endogenous job destruction of Pissarides (2000). The presentation here combines elements of the models in Blanchard and Portugal (2001) and Cahuc and Zylberberg (2004), but extends them to consider two types of workers: natives and immigrants. In the simplest version of the model, a particular job is either always filled by natives or always filled by immigrants, i.e., there is no substitution between the two types of workers on the labor demand side. The model can then be extended such that a particular job can be filled by either natives or immigrants over time. In this version, firms observe the worker's type and can choose freely between hiring a native or an immigrant.

### 2.1    Assumptions

There are jobs and workers in the economy. Workers are of two types: natives and immigrants. A match between a job and a worker may be destroyed due to a productivity shock. When a productivity shock occurs, firms have the option to either fire or retain the worker. Employment protection takes the form of a firing cost that the firm must pay if it decides to fire the worker. For simplicity, it assumed that natives and immigrants are in all ways identical, except in the level of the firing cost, which is lower for immigrants. This difference in firing costs has implications for wages and the hiring and firing rates of the two types of workers.

More specifically, the model has the following ingredients:

1. A match between a job and a worker yields productivity $\varepsilon$, which is a random variable with support $]-\infty, \bar{\varepsilon}]$ and cumulative distribution function $G(.)$.

2. Productivity shocks occur with probability $\lambda$. When a shock occurs, a new level of productivity is drawn from the distribution $G(.)$ and firms have the option to either

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fire or retain the worker. Productivity shocks are idiosyncratic, i.e., they affect jobs independently.

3. If the firm fires the worker following a productivity shock, it must pay a firing cost. This cost should not be interpreted as a transfer from the firm to the worker, but rather as pure waste (for example, administrative costs such as notice periods, possible legal procedures, necessary authorizations for dismissal, etc.).[1]

4. The firing cost determines the critical level of productivity, $\varepsilon_R$, below which the firm fires the worker.

5. There are two types of workers in the economy: natives and immigrants. They are in all ways identical, except in the level of the firing cost, which equals $f$ for natives and $\phi f$, where $0 \leq \phi < 1$, for immigrants. The difference in firing costs between the two groups is not due to differences in their legal coverage (since the same employment protection rules apply to both), but rather to differences in the level of awareness regarding regulations. Immigrants, being new to the labor market and less likely to join unions, are less aware of the employment protection regulations and less likely to claim compensation for dismissal. Therefore, even though *legal* coverage is the same for the two types of workers, *effective* coverage is lower for immigrants.

6. The model is characterized by 'workers waiting at the gate', so that, when a match is destroyed following a productivity shock, firms can hire another worker immediately. All new hires start with productivity $\varepsilon_s$.

7. Employed workers receive a wage $w$. Wages for each match are determined by symmetric Nash bargaining between the firm and the worker. For simplicity, it is assumed that the wage is set at the beginning of the match.

8. Unemployed workers are entitled to unemployment benefits $z$ and have a probability $h$ of finding a job. Because job creation and job destruction follow Poisson processes, the inverse of $h$ equals the duration of unemployment.

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[1]The distinction between interpreting EPL as a transfer from firms to workers or pure waste is important. Lazear (1990) shows that, if employment protection was just a transfer from firms to workers and capital markets were perfect, then the effects of EPL could be undone by a contract including a compensation from the worker to the firm at the time of hiring.

## 2.2  No Substitution on the Labor Demand Side

In this version of the model, a particular job is either always filled by natives $(N)$ or always filled by immigrants $(I)$, i.e., there is no substitution between the two types of workers on the demand side. The equilibrium for each type is characterized by the critical level of productivity $\varepsilon_R^i$, the wage $w^i$, and the job finding rate $h^i$, where $i = N, I$. The equilibrium will be derived for natives. Because immigrants are in all ways identical to natives, except for the firing cost, the equilibrium equations for them will be identical to those for natives, except that $f$ should be replaced with $\phi f$.

It is useful to start by writing the value of a filled vacancy for the firm and a native worker. A job filled with a native worker with current productivity $\varepsilon$ gives an expected profit $\Pi_e^N(\varepsilon)$, defined as:

$$r\Pi_e^N(\varepsilon) = (\varepsilon - w^N) + \lambda G(\varepsilon_R^N)[\Pi_e^N(\varepsilon_s) - \Pi_e^N(\varepsilon) - f] + \lambda \int_{\varepsilon_R}^{\bar{\varepsilon}} (\Pi_e^N(\varepsilon') - \Pi_e^N(\varepsilon))dG(\varepsilon') \quad (1)$$

In this equation $(\varepsilon - w^N)$ is the current profit from the match. The second term captures the expected profit if there is a productivity shock and the firm fires the worker, an outcome that occurs with probability $\lambda G(\varepsilon_R^N)$. In that case, the firm loses the current profit and must pay the employment protection cost $f$. The job is filled with another native worker, with productivity $\varepsilon_s$. The third term captures the expected profit if there is a productivity shock and the firm keeps the worker. In this case, a new level of productivity is drawn from the distribution $G(.)$.

The expected utility of the native worker is defined as:

$$rV_e^N = w^N + \lambda G(\varepsilon_R^N)(V_u^N - V_e^N) \quad (2)$$

The second term captures the utility if the worker is fired following a productivity shock. In that case, he becomes unemployed and receives the expected utility of unemployment, $V_u^N$, which is given by:

$$rV_u^N = z + h^N[V_e^N - V_u^N] \quad (3)$$

These three equations can be used to solve for the equilibrium, which is characterized by the critical level of productivity $\varepsilon_R^N$, the wage $w^N$, and the hiring rate $h^N$.

(i) **Critical level of productivity** $\varepsilon_R^i$

The firm fires the worker if the value of the match falls below the value of a new match net of employment protection costs. Therefore, the critical level of productivity below which a native worker is fired satisfies:

$$\Pi_e^N(\varepsilon_R^N) = \Pi_e^N(\varepsilon_s) - f$$

Using equation (1) and solving:

$$\varepsilon_R^N = \varepsilon_s - f(\lambda + r)$$

This equation describes the effect of an increase in employment protection on the firing rate and gives the first important prediction of the model: if the firing cost increases, the critical level of productivity below which the firm fires the worker decreases. Therefore, *an increase in employment protection reduces the firing rate.*

The equivalent equilibrium equation for immigrants is:

$$\varepsilon_R^I = \varepsilon_s - \phi f(\lambda + r)$$

Because $0 \leq \phi < 1$, it follows that $\varepsilon_R^I > \varepsilon_R^N$. This implies that the firing rate (given by $\lambda G(\varepsilon_R^i)$) is higher for immigrants than for natives. It also implies that *the impact of an increase in employment protection on the firing rate is lower for immigrants than for natives,* since they are effectively less covered by EPL.

(ii) **Wage**

Symmetric Nash bargaining implies that the surplus of the job for the worker equals the surplus for the firm. For jobs filled with native workers, this implies:

$$V_e^N - V_u^N = \Pi_e^N(\varepsilon_s) - (\Pi_e^N(\varepsilon_s) - f) \Rightarrow$$
$$V_e^N - V_u^N = f$$

The left hand side is the value of the job for a native worker. The alternative to being employed is to be unemployed and have expected utility $V_u^N$. The right hand side is the value of the job for the firm. If the firm fires the worker, it finds a replacement worker, but must pay the cost $f$.

This expression can be combined with equations (2) and (3) to solve for the wage paid to natives:

$$w^N = z + (r + \lambda G(\varepsilon_R^N) + h^N)f$$

The equivalent equilibrium equation for the wage paid to immigrants is:

$$w^I = z + (r + \lambda G(\varepsilon_R^I) + h^I)\phi f$$

For a given critical level of productivity $\varepsilon_R^i$ and a given job finding rate $h^i$, *an increase in employment protection increases the bargained wage.* This result is intuitive: employment protection increases the workers' bargaining power as it is more costly for the firm to replace the worker. This effect is weaker for immigrants than for natives as they are effectively less covered by EPL.

(iii) **Hiring rate** $h^i$

Employment protection increases the cost of labor in two ways: by increasing the firing cost, and by increasing the bargained wage. Firms respond to the increase in costs by hiring fewer workers. So, *an increase in employment protection reduces the hiring rate and increases the duration of unemployment.* Because the effect on firing costs and on wages is larger for natives, *an increase in EPL reduces the hiring rate by more for natives than for immigrants.*

An implication of these predictions is that *the effect of employment protection on the unemployment rate of natives and immigrants is ambiguous.* The unemployment rate equals the product of the firing rate and unemployment duration. Because employment protection reduces the firing rate and increases unemployment duration, its effect on the unemployment rate is theoretically undetermined and is ultimately an empirical question.

## 2.3 Substitution on the Labor Demand Side

In the model with no substitution on the labor demand side it is assumed that a particular job is either always filled by natives or always filled by immigrants. In practice, however, a particular job can be filled by either natives or immigrants over time. Therefore, it is important to introduce substitution between the two types of workers on the demand side.

The nature of the equilibrium is quite intuitive. Because immigrants have a lower firing cost than natives and are identical to them in all other respects, they are *a priori* more attractive to firms. Firms will only hire natives if their expected cost is the same as that of immigrants, i.e., if their relative wage decreases in order to compensate for their higher expected firing cost. If natives are to accept a lower relative wage in the bargaining process, unemployment must be less attractive to them. Therefore, equilibrium unemployment duration will be higher for natives than for immigrants, i.e., they will have a relatively lower

hiring rate.

More formally, the equilibrium can be characterized as follows:

**(i) Relative firing rates**

The firing rate of each type is determined by the condition that the value of the match equals the value of a new match net of employment protection costs. For natives, this can be written as:

$$\Pi_e(\varepsilon_R^N) = \Pi_e(\varepsilon_s) - f$$

The equivalent condition for immigrants is:

$$\Pi_e(\varepsilon_R^I) = \Pi_e(\varepsilon_s) - \phi f$$

Note that in this version of the model there is only one profit function $\Pi_e(\varepsilon)$ rather than two separate functions for each type, since now the same job may be filled by either natives or immigrants over time.

Because $0 \leq \phi < 1$ and both types have the same initial productivity $\varepsilon_s$, it follows that $\Pi_e(\varepsilon_R^N) < \Pi_e(\varepsilon_R^I)$. Given that the profit function is increasing in the level of productivity, this implies that $\varepsilon_R^N < \varepsilon_R^I$, i.e., immigrants have a relatively higher firing rate than for natives. Moreover, *an increase in employment protection reduces the firing rate of natives by a relatively larger amount*, since they are effectively more covered by EPL than immigrants.

**(ii) Relative wages**

Using equation (1) and the fact that the two types of workers have the same initial productivity $\varepsilon_s$ and the same productivity distribution $G(.)$, it follows that the condition for firms to be willing to hire natives is:

$$w^N - w^I = \lambda G(\varepsilon_R^I)\phi f - \lambda G(\varepsilon_R^N)f < 0$$

This condition says that firms are indifferent between hiring natives and immigrants if the relative wage of natives decreases as to equate the difference in expected firing costs. This implies a lower relative wage for natives than for immigrants, which is probably at odds with reality in most countries.

It is important to emphasize that this is a *ceteris paribus* result, i.e., for simplicity, the model assumes that immigrants and natives are in all ways identical, except for the level of the firing cost. If natives were assumed to have a higher starting productivity, $\varepsilon_s$, and a

productivity distribution, $G(.)$, with a higher support than immigrants, then the difference in wages would be equal to the difference in current and expected future productivities minus the difference in expected firing costs. With those assumptions, it could easily be the case that natives would have a higher relative wage than immigrants.

Regardless of what is assumed in terms of relative productivities, an increase in $f$ increases the relative firing cost of natives and makes them relatively less attractive to firms. Therefore, *an increase in employment protection reduces the equilibrium wage of natives relative to immigrants.*

### (iii) Relative hiring rates

For natives to be willing to accept a lower relative wage in the bargaining process following an increase in $f$, their labor market conditions must deteriorate. Therefore, *an increase in employment protection increases relative unemployment duration for natives, i.e., it decreases their relative hiring rate.*

To close this section it is important to note that, just as in the model with no substitution between the two types of workers, *the effect of an increase in employment protection on the relative unemployment rates of natives and immigrants is ambiguous.* On the one hand, EPL reduces the relative firing rate of natives. On the other hand, it increases relative unemployment duration. Because the unemployment rate equals the product of the firing rate and unemployment duration, its response to an increase in employment protection is ambiguous.

## 3  Results from Empirical Studies

The predictions of the flow model of unemployment have been tested in a number empirical studies, using both macro and micro data. Overall, the macro evidence is inconclusive and the results are very sensitive to the specification used. Evidence based on micro data is more conclusive.

A key reference in the macro literature is the study by Lazear (1990), which looks at the effect of employment protection on the level of employment in 22 OECD countries. Using data from 1956 to 1984, he finds that an increase in EPL reduces employment. Similar results were obtained by Scarpetta (1996) using data for 15 to 17 OECD countries over the period 1983 to 1993. But studies based on other specifications find different results. Bertola (1990) finds that employment protection does not reduce employment for a given wage and does not increase bargained wages. Nickell (1997) investigates the relation between unemployment and the OECD index of employment protection, using data for 20 OECD countries over

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the periods 1983 to 1988 and 1989 to 1994. He also does not find any significant effect of employment protection on unemployment.

Studies based on macro data suffer from two main problems. First, it is difficult to establish the direction of causality, as it is not clear whether employment protection reduces employment or whether countries with low employment adopt stricter labor market regulations. Second, there is little time-series variation in measures of employment protection, which makes it hard to identify any effect. Reflecting these concerns, a more promising avenue of research are studies with micro data, in particular those that analyze the effects of particular reforms that change the regulation on employment protection. When there is heterogeneity in the applicability of the reforms, it is possible to construct natural experiments and identify the causal effect of employment protection on employment and job flows.

Two key references in the micro literature are Kugler et al (2002) and Kugler and Pica (2005). Kugler et al (2002) construct a natural experiment to analyze the impact of the 1997 reform in Spain, which introduced a new type of permanent contract, with lower dismissal costs, applicable to certain categories of workers (namely unemployed people between the ages of 16 and 30 and over age 45). They find that, for young workers, the reform increased transitions both from unemployment to permanent employment and from temporary to permanent employment and had little effect on dismissals. As a result, permanent employment increased for young workers following the reform. The results for old workers are less significant. Kugler and Pica (2005) also use a natural experiment to analyze the effect of the 1990 reform in Italy, which increased dismissal costs for firms with under 15 employees. They find that the increase in dismissal costs decreased hiring and firing in small firms relative to large firms.

This paper extends the analysis of Kugler et al (2002) and Kugler and Pica (2005) to examine the differential impact of EPL on immigrants and natives. The study which is closest in spirit to this paper, exploring the interaction between immigration and institutional factors, is Angrist and Kugler (2003). The authors look at how labor and product market rigidities affect the impact of immigration on natives and find that immigration generates larger native job losses in countries with more restrictive institutions. This is consistent with the view that immigrants gain a comparative advantage relative to natives in highly regulated labor markets.

# 4 Evidence from the EU LFS

## 4.1 Data and Descriptive Statistics

The EU data were extracted from the Eurostat Labor Force Survey (LFS) by the German Federal Statistics Office. The dataset comprises time series of immigration and labor market variables aggregated by age, gender, education, country of birth, and, for immigrants, years since arrival. The Data Appendix contains more information about the data.

The countries and years included in the sample are listed in Table 1. Data for most countries cover the period 1995 to 2005. To ensure that the coverage of immigrants in the data is appropriate, the percentage of foreign born in the population in the LFS is compared with similar numbers from the last Census year, reported in OECD(2006). A perfect coincidence between the two series should not be expected because the LFS covers only the population above age 15, whereas the Census covers the entire population. Even so, the two series are remarkably similar, suggesting a good coverage of immigrants in the LFS data.

The definition of immigrant adopted here for the EU LFS analysis is based on nativity, i.e., country of birth. This definition is chosen because the variable years of residence, used as a control in the empirical analysis, is based on nativity. An alternative would be to focus on nationality. Angrist and Kugler (2001) briefly explore the implications of defining immigrants based on nativity or nationality and find that the groups of immigrants roughly coincide.

Some descriptive statistics are reported in Table 2. Participation rates show no clear pattern across countries, but immigrants tend to have lower participation rates than natives, especially the ones who have been in the country for a shorter period of time. Employment-to-population ratios exhibit a similar pattern. Turning to the share of total employment in permanent and temporary contracts, Spain is the country where temporary contracts are most common, accounting for over 20% of employment for natives and over 50% for recent male immigrants. France, Germany, Sweden, Finland, Switzerland, and Portugal also have large shares of workers in temporary contracts. The incidence of temporary employment is larger for women than for men and for immigrants than for natives, declining with years since immigration.

## 4.2 Measures of Employment Protection

Time series data on EPL for permanent and temporary contracts for the period 1985 to 2005 were created using OECD summary indicators of the strictness of EPL in different countries and information on the breaking points of the indicators. The Data Appendix explains how the data were constructed.

Other papers have constructed similar measures. Blanchard and Wolfers (2000) draw on data from Lazear (1990) and from the OECD indicators from the late 1980s and late 1990s to construct a 5-year period index of employment protection from 1960 to 1995. This paper follows a similar approach, but with three main differences. First, the availability of OECD indicators for 2003 allows the construction of a longer time series. Second, it looks separately at EPL on permanent and temporary contracts, rather than the aggregation of the two. Third, rather than constructing 5-year period indices, this paper explores information on the timing of reforms to adjust the OECD indicators at the exact year when reforms took place.

The evolution of the EPL indices for permanent and temporary contracts in Germany, Spain, France, the UK, and Italy is described in Figures 2 and 3. There is considerable variation in EPL across countries. The UK is the least regulated, whereas Spain and France are the most restrictive. However, there is not much time variation in the indices. Some countries, such as Italy and Germany, have introduced reforms at the margin, increasing the flexibility of temporary contracts, with the introduction and generalization of temporary work agencies, while leaving regulations on permanent contracts virtually unchanged. There are very few examples of EPL reforms for permanent contracts. Spain is an exception, having introduced reforms in 1994 and 1997. The Spanish case will be discussed in the next section. The 1990 Italian reform discussed in section 6, which increased 'unfair' dismissal costs for firms with under 15 employees, was not taken into account in the construction of the OECD series.

## 4.3   Estimates of the Effects of Employment Protection

The flow model of unemployment presented in section 2 predicts that EPL increases hiring and firing rates and has an ambiguous effect on the unemployment rate. Before turning to a more rigorous analysis of the effects of EPL, it is useful to look at some simple scatter diagrams. Figures 4, 5, and 6 plot the employment protection index for permanent jobs against unemployment duration, employment duration, and the employment rate, for natives and immigrants. The observations are averages across all countries and years in the sample. If we view job creation and job destruction as Poisson processes, unemployment duration is the inverse of the probability of being hired and employment duration is the inverse of the probability of being fired. Therefore, unemployment and employment durations provide information on the hiring and firing rates, respectively.

Figures 4 and 5 suggest that there is a positive relation between EPL and unemployment and employment durations for natives, but not for immigrants. This is consistent with

the predictions of the model: EPL reduces hiring and firing, but the effect is smaller for immigrants, since they are effectively less covered by EPL. Figure 6 suggests that there is no correlation between EPL and the employment rate, which is also consistent with the results of the model.

Although suggestive, these scatter diagrams do not explore the time series, cross-country, and cross-demographic group variation in the data. To do a more formal analysis of the effects of EPL on labor market outcomes, the following general specification is used:

$$y_{ijt} = \alpha EPL_{it} + \delta_j (Years\ residence_j * EPL_{it}) + \beta_i + \gamma_t + \mu X_{ijt} + \varepsilon_{ijt} \tag{4}$$

where $y_{ijt}$ is the outcome of interest for country $i$, year $t$, and demographic group $j$ (where $j$ denotes gender, age, education and nativity groups as defined in Appendix A.1). $EPL_{it}$ is the EPL index on permanent or temporary contracts and is measured in deviations from cross country means to make the effect interpretable as the effect 'at the mean'. $Years\ residence_j$ is a set of dummy variables, where the omitted variable equals 1 for natives and 0 for immigrants, and the remaining three dummies classify immigrants into different groups depending on years since arrival (5 years or less, 6 to 10 years, and more than 10 years). $\beta_i$ are country fixed effects and $\gamma_t$ are year fixed effects. $X_{ijt}$ is a vector of other controls, which includes dummies for gender, age, education, and nativity groups. It also includes interactions of all regressors (including country and year dummies) with the dummies $Years\ residence_j$, so that $\alpha$ can be interpreted as the effect of EPL on native workers and $\delta_j$ as the differential effect of EPL on immigrants relative to natives, by years of residence.

To explore the channel through which EPL may impact differently on natives and immigrants, $X_{ijt}$ includes interactions of the education dummies with $EPL_{it}$. The reason for including these interactions is that immigrants may be less educated than natives and may, therefore, work in less protected jobs. As a result, they would be less affected by EPL due to their lower education and not because of other differences. The inclusion of these controls allows the two effects to be separated.

In this specification, identification comes not only from the time series variation in EPL, which is very limited, but also from the cross-country and cross-demographic group variation. Standard errors are clustered on $country * year$ to control for common errors within these groups. Failure to take this into account would lead to underestimation of the standard errors and overestimation of the effect of interest, as described in Moulton (1990).

### 4.3.1 Participation Rates

Before turning to the effect of EPL on employment, equation (4) is estimated with the labor force participation rate as the dependent variable. In principle, the effect of EPL on participation is undetermined. On the one hand, EPL reduces the hiring rate, making it harder for an unemployed worker to find a job. The worker may get discouraged and leave the labor force. On the other hand, EPL also reduces the firing rate, increasing the value of having a job and encouraging participation.

The results are reported in the first two columns of Table 3. There is no evidence that EPL on permanent contracts has any effect on participation. The controls have the expected signs: immigrants have higher participation rates than natives, men have higher participation rates than women and participation increases with age and education. EPL on temporary contracts has a very small effect on participation rates.

### 4.3.2 Employment Rates

Theory does not make clear predictions regarding the effect of EPL on employment and, as discussed above, the empirical evidence on this effect based on macro studies is mixed. The last two columns of Table 3 report the results of estimating equation (4) to study the impact of EPL on the employment rate. Interestingly, higher EPL for permanent contracts unambiguously decreases the employment rate of natives. An increase in the EPL index for permanent contracts by one unit relative to the cross country mean reduces the employment rate of natives by approximately four percentage points. However, the effect is positive for immigrants who have been in the country for at least six years and increases on years since arrival. This suggests that higher EPL on permanent contracts may actually improve the job prospects of immigrants. By contrast, EPL on temporary contracts has a small positive effect for natives and no significant effect for immigrants.

To investigate whether the differential effect of EPL on immigrants and natives is due to differences in workers' characteristics, the estimation includes interactions between the education dummies and EPL. The coefficients on these interactions show that EPL has a larger negative effect on employment for more educated workers. This is not surprising since it is plausible that more educated workers are more aware of their rights under EPL. Even including these controls, the results suggest that EPL on permanent contracts reduces employment for natives and increases it for immigrants with more than six years of residence.

There may be other differences between immigrants and natives, in addition to education, which explain the differential impact of EPL. For example, it is possible that immigrants, being new to the country, are less informed about the legislation (perhaps because they

are less unionized than natives) and, therefore, less likely to seek compensation in case of dismissal. The data on unionization rates in Figure 1 support this explanation. Another piece of evidence that would be relevant to further support this hypothesis would be data on the number of complaints or the amount of severance payments received by natives and immigrants. Unfortunately, this information is not available disaggregated by nativity.

The notion that the differential effect of EPL on natives and immigrants is due to an information problem seems to be at odds with the evidence in Table 3 that the effect of EPL on employment becomes positive for immigrants who have been in the country for more than six years and increases on years since arrival. It seems plausible that immigrants who have been in the country for a longer time are more integrated, have a better knowledge of the language, and are also more aware of the labor market regulations than those who have just arrived. Therefore, one would expect that, if the comparative advantage of immigrants was based on an information problem, recent immigrants would be the ones to benefit the most.

However, an increase in years since arrival has two counteracting effects. On the one hand, recent immigrants may be less informed of the legislation and be effectively less covered by EPL. On the other hand, they may also be less attractive to employers, either because they are less productive (for example, because they have a poor knowledge of the language) or are perceived as being less productive or less reliable (since they do not have a history of employment in the country). If the second effect dominates, then it is plausible that an increase in EPL induces firms to hire more immigrants of older arrival cohorts.

### 4.3.3 Employment and Unemployment Duration

The effect of EPL on the employment rate is the result of two types of flows: flows from employment into unemployment, and flows from unemployment into employment. Both theory and the micro evidence suggest that EPL decreases these flows. On the one hand, by increasing the cost of firing, EPL reduces flows into unemployment. On the other hand, by increasing the costs for firms and the bargaining power of workers, EPL reduces flows into employment. The EU LFS collects no information on job flows, but it contains information on unemployment and employment durations. Since unemployment duration is the inverse of the hiring rate and employment duration is the inverse of the firing rate, this information is sufficient to analyze the effect of EPL on job flows.

The results of estimating equation (4) with unemployment and employment duration as dependent variables are shown in Table 4. An increase in EPL for permanent contracts increases unemployment duration, i.e., decreases the hiring rate, for natives. The effect on immigrants is much weaker, and even reverses sign for immigrants with six to ten years of

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residence. These results are consistent with the model in section 2, which predicts that an increase in EPL reduces the relative hiring rate of natives. By contrast, EPL for temporary contracts has no significant effect on the probability of being unemployed for more than one year.

Turning to the effects on employment duration, the results suggest that EPL on permanent contracts increases the probability of being employed for more than five years by approximately the same amount for natives and for immigrants. This result does not match the model in section 2, which predicts that an increase in EPL should reduce the relative firing rate of natives. When looking at the effect of EPL on temporary contracts, the results in column (2) show no significant effect on the probability of being employed for more than five years. This is not surprising since this type of EPL works on contracts of short duration. Indeed, as the last column suggests, EPL on temporary contracts does have a significant positive effect on the probability of being employed for more than one year.

The results on employment and unemployment duration are consistent with the findings for the employment rate. EPL on permanent contracts decreases hiring and firing. For natives, the reduction in hiring has a larger magnitude than the reduction in firing, resulting in a negative effect on the employment rate. For immigrants, EPL on permanent contracts has a much smaller effect on hiring and about the same effect of firing. The combination of these two effects results in a positive effect of permanent EPL on the employment rate of immigrants with more than six years of residence.

As for the employment rate, the differential effect of EPL on employment and unemployment duration across nativity groups cannot be fully explained by differences in education. Even controlling for interactions between the EPL index and the education dummies, the differential effect of EPL on natives and immigrants persists. Again, this suggests that there may be other important differences between the two groups, such as the fact that immigrants are new to the country, less aware of the legislation and less unionized than natives and, therefore, less likely to seek compensation in case of dismissal.

## 5    Evidence from the 1997 and 2001 Spanish Reforms

### 5.1    Institutional Background

In 1984 Spain liberalized the use of temporary contracts which previously could only be used for seasonal jobs. This reform generalized their use to all workers and sectors, resulting in a rapid increase in temporary employment and creating a dual labor market. Spain became the country with the highest proportion of workers with temporary contracts in the EU, as

reported in Table 2.

Subsequent governments implemented reforms to mitigate the duality in the Spanish labor market. In 1994, a reform restored the rule that temporary contracts could only be used for seasonal jobs and eliminated the most flexible type of temporary contract: the fixed term contract to promote employment. This reform also acted upon permanent contracts, relaxing their dismissal conditions. However, the 1994 reform is regarded as having been largely ineffective. In practice, firms continued to hire workers under temporary contracts for all types of jobs.

In 1997, again with the intention of reducing duality in the labor market, the newly elected conservative government of José María Aznar implemented a further reform. Instead of reducing dismissal costs for permanent contracts across the board, this reform introduced a new type of permanent contract - 'contract for promoting permanent employment' - with lower levels of compensation for 'unfair' dismissals. While the level of compensation of standard permanent contracts equals 45 days for each year of service, up to a maximum of 42 monthly payments, the new contract has a level of compensation equal to 33 days for each year of service, up to a maximum of 24 monthly payments.

The new type of permanent contract applied only to certain categories of workers, namely the young unemployed (age 16 to 30), the unemployed for more than one year, the unemployed over age 45, disabled workers, and workers previously hired under temporary contracts. In order to encourage firms to hire under the new permanent contract, the law reduced payroll taxes for newly signed contracts and for conversions from temporary to permanent employment using the new contract. The payroll tax reductions ranged from 40% to 90% and lasted for two years, except for contracts for the unemployed over age 45, where the reductions lasted for the duration of the contract.

The 1997 reform was initially valid for four years. In 2001, the Spanish government decided to adopt the new type of contract on a permanent basis and extended its scope, including new target groups, such as women hired in sectors were they are under-represented, unemployed women hired in the period of 24 months after childbirth, and people earning integration incomes. There was also another reform in 2001 which introduced dismissal costs for temporary contracts, equal to 8 days per year worked. This amount is not significant, but the purpose was again to approximate the costs of temporary and permanent contracts in order to reduce duality in the labor market.

Table 5, reproduced from Kugler et al (2002), lists the reductions in dismissal costs and in payroll taxes introduced by the 1997 and 2001 reforms for different categories of workers. The 1997 reform applied to contracts signed in 1997 and 1998. The 2001 reform extended

the reduction in dismissal costs and in payroll taxes to contracts signed in or after 1999. The table shows that the extent of the incentives varies by demographic group, with unemployed workers age 30 to 44 unaffected.

## 5.2 Identification Strategy

The variability in the application of the 1997 and 2001 reforms to different demographic groups allows the design of a natural experiment to study the effect of the reduction in dismissal costs on the Spanish labor market. There are different margins of variability that could be explored: comparing workers in different age groups, comparing disabled to non-disabled, women under-represented in their occupation to those not under-represented, the long-term unemployed to other unemployed, etc.

Following Kugler et al (2002), this paper explores the variability in age and compares labor market outcomes of workers age 30 to 44 (control group) to workers age 16 to 29 (treatment group 1) and workers age 45 to 59 (treatment group 2) before and after the reform. Workers above age 59 are excluded in order to isolate the effect of the reform of dismissal costs and avoid capturing the effect of simultaneous reforms to the pension system[2]. The advantage of defining groups based on age is that, unlike some other classifications (such as women under-represented in their occupation and the long-term unemployed) self-selection is not a problem.

To estimate the effect of the reform on the employment probability, the following logit specification is used:

$$e_{it} = \Lambda[\alpha X_{it} + \gamma_0 d_t + \gamma_1 q_t + \beta_1 treatment_{1i} + \beta_2 treatment_{2i} + \tag{5}$$
$$\delta_1(treatment_{1i} \times post_t) + \delta_2(treatment_{2i} \times post_t)] + \varepsilon_{it}$$

The dependent variable $e_{it}$ is an indicator equal to 1 if the individual is employed under a permanent or temporary contract and 0 if he is unemployed or inactive. Self-employed workers are excluded from the analysis. $X_{it}$ is a vector of controls, including an indicator for head of household, an indicator for being married, and the number of years of schooling. $d_t$ is a set of year dummies and $q_t$ is a set of quarter dummies. $treatment_{1i}$ is an indicator equal to 1 for young individuals (age 16 to 29) and $treatment_{2i}$ is an indicator equal to 1 for old individuals (age 45 to 59).

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[2]See, for example, European Commission Report (2006) for an overview of reforms to the Spanish pension system in the early 2000s.

The coefficients $\delta_1$ and $\delta_2$ are the *difference-in-differences estimates*, which capture the effect of the reduction in dismissal costs on the employment probability of the young and the old. They are the coefficients on the interactions between the treatment indicators and $post_t$, which equals 1 for the period after the 1997 reform and 0 for the period before the reform. Because the reform took place during the second quarter of 1997, this quarter is omitted. Therefore, the period before the reform extends from the first quarter of 1992 to the first quarter of 1997 and the period after the reform extends from the third quarter of 1997 to the fourth quarter of 2006. By going beyond 2001, the results capture the effects of both the 1997 reform and its extension in 2001.

Equation (5) is estimated separately for immigrants and natives and for men and women. The distinction between the effect on immigrants and natives is central to the analysis in this paper. The distinction between the effect on men and women is important given that the extension of the reform in 2001 gave special treatment to some categories of women. When the equation is estimated for the subsample of immigrants, the vector of controls $X_{it}$ also includes the number of years of residence and the region of nationality (EU 15, other Europe, Central and South America, North America, Africa, and Asia and Oceania). In all estimations, standard errors are clustered by $year * age\ group$, where $age\ group$ is 16 to 29 (young), 30 to 44 (middle-aged), and 45 to 59 (old). This controls for common errors within these age groups.

The key element of this strategy is that the outcome of the control group (individuals age 30 to 44) is taken as a counterfactual for the experiences of the treatment groups in the absence of the reforms. This strategy is valid if there are no contemporaneous shocks, other than the reforms on dismissal costs, affecting the outcomes of the control and treatment groups during the period of analysis. If the business cycle affects the control and treatment groups differently, for example, then the estimates may capture the effect of the business cycle instead of the effect of the reduction in dismissal costs, producing biased results. To account for this possibility, the specification is changed to control for age group-specific business cycle effects:

$$e_{it} = \Lambda[\alpha X_{it} + \gamma_0 d_t + \gamma_1 q_t + \beta_1 treatment_{1i} + \beta_2 treatment_{2i} + \delta_1(treatment_{1i} \times post_t) + \qquad (6)$$
$$\delta_2(treatment_{2i} \times post_t) + \lambda_1(treatment_{1i} \times expansion_t) + \lambda_2(treatment_{2i} \times expansion_t)] + \varepsilon_{it}$$

In this specification, the variable $expansion_t$ is an indicator equal to 1 for the expansionary years 1995, and 1998 to 2000, and 0 otherwise. The interactions between the indicators for

treatment and the expansion variable control for age group-specific business cycle effects.

Equations (5) and (6) capture the effect of the reform on the probability of being employed. Because a clear motivation for the reform was to reduce labor market segmentation, encouraging firms to replace temporary contracts with permanent ones, it is important to look not only at the effect on the probability of being employed, but also at the composition of employment. For this purpose, the model can be extended to a multinomial framework in which $e_{it}$ takes three values: 1 if the individual is unemployed or inactive, 2 if he works under a temporary contract, and 3 if he works under a permanent contract. The estimation of the extended model is carried out by multinomial logit.

## 5.3  Data and Descriptive Statistics

The data are drawn from the Spanish economically active population survey (EPA - Encuesta de Población Activa), which collects detailed information on individuals' demographic characteristics and labor market outcomes. The data are at quarterly frequency and cover the period from the first quarter of 1992 to the fourth quarter of 2006. Data starting in the first quarter of 1999 are available on the website of the Spanish Statistics Institute (INE - Instituto Nacional de Estadística). Data for the earlier period were provided on request by the INE.

Because the purpose of this study is to compare the labor market performance of immigrants and natives, it is important to have information on nativity or nationality. It is also important to control for years of residence in Spain. The EPA reports this information from the first quarter of 1992 onwards. The identification of immigrants and natives is based on nationality rather than nativity, since the variable years of residence in Spain is only collected for foreign nationals.

To test whether the coverage of immigrants in the EPA is appropriate, Table 6 compares the percentage of immigrants in the EPA with equivalent numbers from OECD (2006), which are taken from the registry on the number of work permits until 1999 and, from 2000 onwards, from social security data. The table shows that immigrants are over-represented in the EPA relative to the data on work permits, and slightly under-represented relative to the social security data. Overall, though, the coverage of immigrants in the EPA seems reasonably accurate relative to these other sources.

The numbers in Table 6 show a remarkable increase in immigration into Spain, especially since 2000. By the end of 2006, immigrants represented more than 8% of the Spanish labor force. Looking at the origin of immigrants, Figure 7 shows that, not surprisingly, most Spanish immigrants come from Central and South America. The second largest area of origin

is other Europe, with a remarkable increase since 2000, most likely reflecting an increase in immigration from Eastern Europe following the enlargement of the European Union.

Descriptive statistics for the Spanish EPA are reported in Table 7. The probability of being employed on a permanent job increases with age and is larger for natives than for immigrants. The employment probability increased after the reforms for all groups. There also appears to be a change in the composition of employment for native workers affected by the reforms. Natives age 30 to 44 show no increase in their probability of permanent employment, but younger and older natives do appear to have found it easier to find permanent jobs. For these age groups, there was also a simultaneous reduction in the probability of temporary employment, suggesting that firms may have used the new type of contract to convert temporary jobs into permanent positions. This suggests that the 1997 and 2001 reforms had a positive effect on the permanent employment probability of natives and reduced duality in the Spanish labor market. For immigrants, the results are not as clear. The next section explores these effects in more detail, controlling for demographic characteristics.

## 5.4 Employment Effects

The results of estimating equations (5) and (6) for the employment probability and its decomposition into temporary and permanent employment are reported in Table 8. The evidence suggests that the reforms had no significant effect on the employment probability of young native men relative to middle-aged, but reduced the employment probability of old native men by 4.8 percentage points. Relative to middle-aged native women, the employment probability of young and old native women increased by 1.4 and 1.7 percentage points, respectively. There was no significant effect on the employment probability of immigrants, except for old immigrant women, whose employment probability increased by 5.2 percentage points relative to middle-aged immigrant women. Overall, the results suggest that the reduction in dismissal costs did not increase employment across the board. While it seems to have improved employment prospects for women, it had the opposite effect for old native men.

Turning to the effect of the reforms on permanent and temporary employment, the results show a large and statistically significant reduction in the temporary employment probability and an increase in the permanent employment probability for young natives and for old native women. For old native men, however, there is a reduction in both the temporary and the permanent employment probabilities. The effect on immigrants is less clear. There is evidence that the reform reduced the temporary employment probability for young immigrants and

increased it for old immigrants, but the effects on the permanent employment probability are insignificant.

These results are consistent with the findings in Kugler et al (2002), which suggest that the 1997 reform increased permanent employment for young natives relative to the middle-aged, but had an insignificant effect on old natives. As discussed in section 2, theory makes no clear predictions regarding the effect of EPL on the level of employment, since it affects hiring and firing rates in the same direction. Using a panel version of the EPA which allows following the same individuals over time and measuring transitions, Kugler et al find that, for young natives, the 1997 reform increased transitions both from unemployment and from temporary employment to permanent employment and had little effect on dismissals. The increase in the hiring rate without a simultaneous increase in the firing rate explains the increase in permanent employment for young natives following the reform. The results in Kugler et al cannot be used to benchmark the effects on immigrants, since their study does not distinguish workers by nationality.

## 6  Evidence from the 1990 Italian Reform

### 6.1  Institutional Background

A significant reform in EPL in Italy took effect in May 1990 and updated legislation from 1970. The 1970 law introduced much stricter dismissal regulation for large firms than for small firms. While large firms (those above 15 employees) were required to pay foregone wages and rehire the worker in case of 'unfair' dismissal[3], small firms were exempt. The 1990 reform reduced this discrepancy between large and small firms, introducing dismissal costs for firms with less than 15 employees. If the dismissal is considered to be 'unfair', small firms must either rehire the worker or pay compensation. The latter varies between 2.5 and 6 monthly payments, depending on the tenure of the dismissed worker. The rules for large firms were not changed. The different treatment given to large and small firms in the 1990 reform introduces variation which can be explored to study the effect of dismissal costs on employment.

The 1970 law was at the root of much tension in the early 2000s. In October 2001, the recently elected center-right government of Silvio Berlusconi proposed a series of labor market reforms, including the reform of the 1970 regulation on 'unfair' dismissals. The proposal was to abolish compulsory reinstatement of unfairly dismissed workers in favor of financial

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[3]Aternatively, the worker may opt for a financial compensation (equal to 15 monthly payments) instead of being rehired.

compensation. This generated controversy among trade unions and left-wing movements, culminating with the murder of the mentor of the proposed reforms, Marco Biagi, in March 2002, attributed to the terrorist group Brigate Rosse. Given the turmoil, the reform of the dismissal regulations was not implemented.

While there have not been major reforms on employment protection for permanent contracts in Italy (with the exception of the 1990 reform), there has been an increase in flexibility in the use of temporary contracts. In 1987 the government liberalized the use of temporary contracts, subject to collective agreements specifying certain target groups. Flexibility at the margin was further increased in 1997 with the liberalization of temporary help agencies.

## 6.2 Identification Strategy

The differential treatment given to small and large firms in the 1990 reform is explored to estimate the impact of dismissal costs on hiring and firing rates. Because the reform increased dismissal costs for firms with less than 15 employees and did not change costs for firms with more than 15 employees, a natural experiment can be constructed. Large firms are taken as the control group and their outcomes interpreted as the counterfactual, i.e., what would have happened in small firms if dismissal costs had not increased. The effect of the increase in dismissal costs is measured by comparing the change in outcomes before and after the reform in small relative to large firms.

Since firm size may itself be affected by the reform, the classification of firms as small and large follows the strategy in Kugler and Pica (2005): small firms are those with less than 15 employees and large firms are those with more than 15 employees *in all years before the reform*. Therefore, the analysis excludes firms which crossed the 15 employees threshold before the reform.[4]

The following logit specification is used to estimate the effect of the 1990 reform on hiring and firing rates:

$$y_{ijt} = \Lambda[\alpha_0 post_t + \alpha_1 small_j + \alpha_2 immigrant_i + \alpha_3(immigrant_i \times post_t) \qquad (7)$$
$$+\alpha_4(immigrant_i \times small_j) + \alpha_5(small_j \times post_t)$$
$$+\alpha_6(immigrant_i \times smallj \times post_t) + \beta X_{ijt}] + \varepsilon_{it}$$

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[4]This correction is important as there is evidence that firms change behaviour in response to the threshold. For example, Borgarello et al (2004) find that firms just below15 employees are more reluctant to hire than those more distant from the threshold.

The dependent variable $y_{ijt}$ is an indicator equal to 1 if firm $j$ hired (fired) worker $i$ at time $t$ and 0 otherwise. $post_t$ is an indicator equal to 1 for the period post 1990 and 0 for the period pre 1990. $small_j$ is an indicator equal to 1 if firm $j$ had less than 15 employees in all years before 1990 and 0 otherwise. $immigrant_i$ is an indicator equal to 1 if worker $i$ is an immigrant. $X_{ijt}$ is a vector of controls, including worker characteristics (age, skill level, and log of wage) and firm characteristics (sector of activity, and region). All the controls are also interacted with the $immigrant_i$ indicator.

The effect of the reform on natives is given by the coefficient $\alpha_5$ and the effect on immigrants is given by the sum of $\alpha_5$ and $\alpha_6$. Therefore, $\alpha_6$ captures the differential effect of the reform on immigrants relative to natives. Because the 1990 law refers to permanent rather than temporary employment, the sample is limited to job spells under permanent contracts. In all specifications, standard errors are clustered by $post * firm\ size$.

As for the analysis of the Spanish reform, it is important to control for group-specific business cycle effects. If the business cycle affects small and large firms differently, the results will be biased and will be capturing the effect of the business cycle as well as the effect of the increase in dismissal costs. To account for this possibility, the following specification is used, where the variable $recession_t$ is an indicator equal to 1 during the recession years of 1992, 1993, and 1996:

$$
\begin{aligned}
y_{ijt} = \Lambda[&\alpha_0 post_t + \alpha_1 small_j + \alpha_2 immigrant_i + \alpha_3(immigrant_i \times post_t) \quad\quad (8)\\
&+\alpha_4(immigrant_i \times small_j) + \alpha_5(small_j \times post_t) + \alpha_6(immigrant_i \times smallj \times post_t)\\
&+\lambda_1 recession_t + \lambda_2(small_j \times recession_t) + \lambda_3(recession_t \times immigrant_i)\\
&+\lambda_4(small_j \times recession_t \times immigrant_i) + \beta X_{ijt}] + \varepsilon_{it}
\end{aligned}
$$

## 6.3   Data and Descriptive Statistics

The data are drawn from the Work Histories Italian Panel (WHIP), which contains information on individual job spells and is based on administrative records from the Italian Social Security Institute (INPS)[5]. The reference population includes Italians and foreigners who have worked in Italy at some point during their careers. A sample of 1:90 is extracted from this population, including about 740,000 observations. Workers in the public sector are not sampled.

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[5] The WHIP data were provided on request by Laboratorio Revelli - Centre for Employment Studies. http://www.laboratoriorevelli.it/whip.

The dataset is a matched employer-employee panel and contains detailed information on the characteristics of the worker (including year of birth, gender, country of birth, and skill level), the job (including the dates when the job started and ended, the annual wage, and the number of days worked in the year) and the firm (including the exact number of employees, the year when the firm was established, its region, and sector of activity). It covers the years 1986 to 1999 and the definition of immigrants is based on country of birth, i.e., nativity.

Table 9 reports descriptive statistics. Small firms have higher hiring and firing rates than large firms. Moreover, there has been a larger decline in the hiring rate in small firms than in large firms after the reform for all demographic groups. Also, while the firing rate increased in large firms after the law, it did not change much in small firms. This is a first indication that the increase in dismissal costs for small firms reduced hiring and firing rates relative to large firms. However, to confirm this result, it is important to control for worker and firm characteristics.

## 6.4    Effect on hiring and firing

The results of estimating equations (7) and (8) for the hiring and firing rates are reported in Table 10. The coefficients on $Small\ firm * Post$ capture the effect of the increase in dismissal costs on natives and the sum of the coefficients on $Small\ firm * Post$ and $Immigrant * Small\ firm * Post$ captures the effect on immigrants. The results suggest that the reform reduced hiring and firing rates for native men and women. The effects are quite large and very significant. The probability of being hired decreased by 3.4 percentage points for native men and 6.1 percentage points for native women following the reform. The probability of being fired decreased by 6.5 percentage points for native men and 5.9 percentage points for native women.

Turning to the effect on immigrants, the evidence suggest that the increase in dismissal costs also reduced their hiring and firing rates, but by a much smaller amount than for natives. The probability of being hired decreased by only 0.7 percentage points for immigrant men and 3.1 percentage points for immigrant women. The decrease in the probability of being fired was of 2.8 and 3.6 percentage points for immigrant men and women, respectively. This smaller impact on immigrants is consistent with the predictions of the model, since they are effectively less covered by EPL than natives.

# 7  Summary and Conclusions

This paper revisits the literature on the labor market effects of EPL, exploring its differential effect on natives and immigrants. Across three empirical exercises, it tells a consistent story: because immigrants are less protected by EPL than natives, they gain a comparative advantage in the labor market.

The results are summarized in Table 11. The evidence from the EU LFS suggests that stricter EPL on permanent contracts reduces employment of natives, but has a positive effect on employment of immigrants with more than six years of residence. This comes from a combination of the effects on hiring and firing rates: for natives, EPL reduces both hiring and firing, but the impact on hiring is larger, resulting in a reduction on the employment rate; for immigrants, EPL has no effect on hiring and reduces firing, resulting in an increase on the employment rate.

The analysis of the 1997 and 2001 Spanish reforms suggests that a reduction on EPL on permanent contracts increases permanent employment for natives but not for immigrants. The evidence from the 1990 Italian reform suggests that an increase in EPL reduces hiring and firing for natives, but has a much smaller effect on immigrants.

The consistency of the results from these three experiments is reassuring, since some estimations overcome the shortcomings of others. In particular, country-specific evidence has advantages over cross-country evidence as it allows disentangling the effects of EPL *per se* from the effects of the interaction of EPL with other labor market features, such as unemployment benefits, minimum wages, and early retirement schemes. Cross-country evidence does not separate these effects because of the lack of time series variation for many of these features. Country-specific evidence, however, separates these effects because different institutions interacting with EPL do not have the same cross-sectional variation as EPL.

Within the country-specific evidence, the experiment for Spain is probably superior to the one for Italy. This is because self-selection between the treatment and control groups is absent in Spain, where the treatment was applied as a function of age, but is likely to be present in Italy, where workers may move between large and small firms and employers may change their hiring and firing decisions in order to keep the size of the firm below a certain threshold.

The results point to an interesting interaction between labor market institutions and immigrant assimilation. Angrist and Kugler (2003) show that the displacement effect of immigrants on natives is smaller in countries with more flexible institutions. Our findings are supportive of that conclusion and suggest that, in countries with restrictive labor mar-

kets, immigrants gain a comparative advantage relative to the natives because they are less protected. There appears to be a silver lining to labor market rigidities after all.

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# A  Data Appendix

## A.1  *The Eurostat EU Labour Force Survey*

The Eurostat EU Labour Force Survey (LFS) covers the 25 EU member states, as well as Iceland and Norway. The data are collected by national statistical agencies, following a common procedure developed by Eurostat, ensuring comparability across countries. The data used in this paper were constructed on request by the German Federal Statistics Office and contain information on immigration and labor market variables aggregated by the following groups:

- gender: male, female.

- age: 15 to 19, 20 to 24, 25 to 29, 30 to 34, 35 to 39, 40 to 44, 45 to 49, 50 to 54, and 55 to 59.

- education: low (lower secondary), medium (upper secondary), and high (third level).

- nativity: native born, foreign born (aggregated by years of residence: 0 to 5, 6 to 10, more than 10).

Eurostat implements guidelines to ensure that the data are statistically reliable. In particular, it computes thresholds of population counts below which the data are unreliable. All unreliable observations were dropped from the dataset used in this paper.

## A.2  *Construction of EPL Time Series*

The time series for EPL on permanent and temporary contracts used in this paper cover the period 1985 to 2005 and were constructed from the OECD indicators on the strictness of EPL for the late 1980s, the late 1990s, and 2003, using information on the timing of reforms which introduced breaking points in the series. The indicators and details on how they are constructed are available in OECD (1999) and OECD (2004). The version of the indicators used in this paper is version 1, which does not include information on collective dismissals, but covers a longer period of time.

The OECD indicators aggregate information in a number of dimensions. For permanent contracts, they contain information on severance payments, notification procedures, the definition of 'fair' and 'unfair' dismissals, the amount of compensation following 'unfair' dismissals, among other measures. For temporary contracts, they contain information on the

valid cases for use of this type of contracts, the maximum number of fixed term contracts permitted, their maximum cumulative duration, as well as information on temporary work agencies.

To transform the EPL indices for the late 1980s, the late 1990s, and 2003 into a time series from 1985 to 2005, the information on the breaking points of the EPL time series given in table 2.A2.6 in OECD (2004) is used. This table lists a number of reforms for each country which had an impact on the EPL indices for permanent and temporary contracts. The starting point of the time series, 1985, takes the values of the OECD indices for the late 1980s. The other trivial point is 2003, where the series take the same values as the OECD indices for 2003. Between those dates, the series are constructed using information on the breaking points.

It is useful to give some illustrative examples. For instance, Belgium adopted a reform in 1997 which reduced restrictions on temporary contracts. This was the only reform on regulation on temporary contracts in Belgium during the period considered. Therefore, the series on EPL for temporary contracts in Belgium takes the value of the late 1980s from 1985 to 1996. In 1997 it falls to the value in the late 1990s (which equals the value in 2003 as there were no reforms after 1997).

Sometimes it is necessary to interpolate. For example, Spain had two reforms on regulation on permanent contracts: one in 1994, which relaxed the procedural requirements for dismissals for economic reasons, and another in 1997, which introduced a new type of contract, applicable to certain categories of workers, with reduced compensation for 'unfair' dismissals. Given these reforms, the series on EPL for permanent contracts in Spain takes the value of the late 1980s from 1985 to 1993. In 1997 it falls to the value in the late 1990s. Between 1994 and 1996, it is a linear interpolation of the values in the late 1980s and the late 1990s.

**Figure 1. Union Membership Rates by Citizenship**

**Union Membership (2003)**



SOURCE: International Social Survey Programme (2003)

**Table 1. Coverage of the EU LFS**

|  | LFS coverage | % of immigrants in the population | |
|---|---|---|---|
|  |  | LFS 2005 | OECD 2004 |
| Austria | 1995-2005 | 14.9 | 13 |
| Belgium | 1995-2005 | 13 | 11.4 |
| Denmark | 1995-2005 | 6.2 | 6.3 |
| Finland | 1995-2005 | 2.6 | 3.2 |
| France | 1995-2005 | 11.7 | 10 (a) |
| Germany | 1999-2005 | n.a. | 12.9 |
| Greece | 1995-2005 | 6.7 | 10.3 (b) |
| Ireland | 1999-2005 | 10.3 | 11 |
| Netherlands | 1996-2005 | 12.2 | 10.6 |
| Norway | 1996-2005 | 7.5 | 7.8 |
| Portugal | 1995-2005 | 6.1 | 6.7 |
| Spain | 1995-2005 | 10.1 | 5.3 (b) |
| Sweden | 1995-2005 | 13 | 12.2 |
| Switzerland | 2005 | 24.7 | 23.5 |
| UK | 1995-2005 | 10.4 | 9.3 |

SOURCE: EU Labor Force Survey and OECD (2006)
NOTE: Definition of immigrants is based on nativity. OECD (2006) numbers come from the last Census in or before 2004. (a) Data from 1999 Census. (b) Data from 2001 Census.

# Table 2. Characteristics of Immigrants and Natives
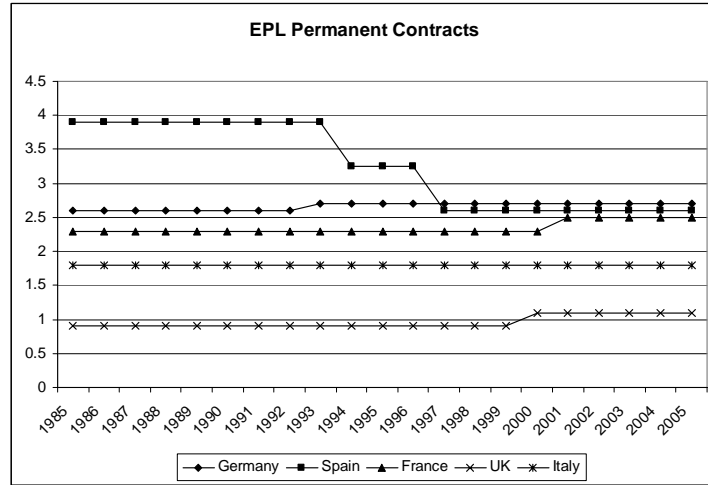
| | Men | | | | Women | | | |
|---|---|---|---|---|---|---|---|---|
| | Natives | Immigrants | | | Natives | Immigrants | | |
| | | 0-5 years residence | 6-10 years residence | >10 years residence | | 0-5 years residence | 6-10 years residence | >10 years residence |
| **A. Labor Force Participation rate** | | | | | | | | |
| Austria | 88.33 | 83.86 | 92.74 | 87.91 | 71.76 | 53.99 | 70.90 | 69.52 |
| Belgium | 85.06 | 78.10 | 82.67 | 77.94 | 66.14 | 45.08 | 48.43 | 49.52 |
| Denmark | 90.87 | 74.97 | 72.10 | 82.02 | 82.33 | 47.66 | 56.40 | 72.22 |
| Finland | 86.13 | 80.72 | 85.48 | 87.40 | 81.13 | 52.82 | 63.05 | 78.09 |
| France | 87.88 | 73.77 | 85.83 | 87.30 | 74.18 | 40.36 | 53.31 | 63.76 |
| Germany | 90.44 | 79.80 | 90.69 | 89.18 | 76.50 | 49.65 | 62.67 | 66.59 |
| Greece | 88.52 | 91.48 | 95.40 | 92.24 | 55.42 | 61.54 | 64.29 | 60.67 |
| Ireland | 87.99 | 78.19 | 86.34 | 89.55 | 60.16 | 57.06 | 58.31 | 62.55 |
| Netherlands | 91.68 | 66.26 | 80.24 | 82.73 | 72.85 | 44.64 | 51.24 | 62.03 |
| Norway | 89.26 | 83.67 | 86.31 | 84.66 | 81.41 | 59.40 | 68.86 | 75.94 |
| Portugal | 88.24 | 92.49 | 84.23 | 86.11 | 71.29 | 75.34 | 67.65 | 75.57 |
| Spain | 87.21 | 93.21 | 92.64 | 89.07 | 56.25 | 69.98 | 67.03 | 58.27 |
| Sweden | 89.05 | 69.83 | 80.96 | 82.92 | 84.92 | 54.37 | 67.25 | 76.63 |
| Switzerland | 94.34 | 94.04 | 93.77 | 89.71 | 86.44 | 69.18 | 70.61 | 76.01 |
| UK | 90.01 | 74.80 | 84.11 | 87.14 | 74.32 | 53.59 | 57.25 | 66.30 |
| | | | | | | | | |
| **B. Employment/Population** | | | | | | | | |
| Austria | 84.72 | 74.76 | 84.32 | 79.63 | 68.79 | 47.86 | 66.04 | 64.35 |
| Belgium | 80.38 | 64.38 | 67.08 | 66.39 | 60.32 | 34.67 | 37.89 | 40.68 |
| Denmark | 86.30 | 61.98 | 62.61 | 73.45 | 76.92 | 38.50 | 47.27 | 65.61 |
| Finland | 77.07 | 59.71 | 63.22 | 72.84 | 72.68 | 36.52 | 47.02 | 66.26 |
| France | 80.65 | 54.66 | 66.54 | 73.70 | 65.68 | 27.27 | 37.62 | 52.72 |
| Germany | 83.11 | 63.66 | 76.47 | 78.21 | 69.84 | 38.44 | 54.43 | 58.83 |
| Greece | 83.05 | 82.17 | 89.28 | 83.52 | 47.21 | 48.26 | 52.40 | 49.83 |
| Ireland | 80.94 | 70.25 | 78.59 | 81.61 | 56.00 | 50.82 | 53.66 | 57.79 |
| Netherlands | 89.52 | 56.33 | 70.90 | 76.57 | 70.11 | 37.55 | 45.73 | 57.76 |
| Norway | 86.54 | 72.96 | 74.14 | 79.91 | 78.92 | 53.71 | 61.56 | 72.53 |
| Portugal | 84.57 | 85.37 | 75.52 | 80.04 | 67.23 | 64.91 | 59.64 | 69.19 |
| Spain | 76.92 | 81.86 | 82.46 | 80.28 | 44.05 | 59.20 | 55.93 | 49.09 |
| Sweden | 83.36 | 53.86 | 61.70 | 73.15 | 80.44 | 41.26 | 54.42 | 69.52 |
| Switzerland | 89.45 | 86.84 | 83.33 | 83.99 | 82.22 | 58.68 | 61.21 | 69.91 |
| UK | 83.65 | 65.08 | 73.36 | 78.54 | 70.73 | 47.19 | 51.83 | 61.67 |
| | | | | | | | | |
| **C. Permanent employment/Employment** | | | | | | | | |
| Austria | 77.3 | 76.8 | 84.5 | 84.7 | 79.2 | 79.7 | 86.5 | 87.1 |
| Belgium | 77.6 | 73.4 | 73.3 | 75.2 | 77.1 | 69.1 | 72.2 | 74.2 |
| Denmark | 79.9 | 73.9 | 72.1 | 75.7 | 84 | 65.9 | 64 | 79.7 |
| Finland | 70.2 | 59.7 | 65.3 | 69.8 | 71.9 | 50.9 | 51.6 | 63.2 |
| France | 74.8 | 60.6 | 69 | 75.5 | 77.6 | 58.6 | 73.3 | 80.2 |
| Germany | 74.8 | 65.7 | 77.1 | 79.1 | 79.4 | 66.7 | 76.7 | 81.8 |
| Greece | 49.5 | 67.1 | 70.9 | 64.9 | 52.3 | 63.1 | 63.8 | 65.8 |
| Ireland | 70.9 | 80.7 | 70.4 | 70.2 | 86.7 | 81.7 | 77.7 | 80.6 |
| Netherlands | 77.1 | 61.2 | 68 | 76 | 75.6 | 55.7 | 65.8 | 75.7 |
| Norway | 80.4 | 75.4 | 73.4 | 79 | 81.8 | 68.6 | 73.1 | 78.9 |
| Portugal | 59.7 | 39.1 | 43.3 | 62.4 | 60.2 | 47.9 | 54.9 | 66.9 |
| Spain | 53.8 | 28.8 | 36.4 | 38.4 | 53.8 | 43.7 | 43 | 43.5 |
| Sweden | 74.3 | 57.2 | 62.7 | 69.7 | 78.9 | 51.2 | 58.7 | 76.7 |
| Switzerland | 73.3 | 73.5 | 78.5 | 79.9 | 72 | 73.9 | 76.2 | 82.6 |
| UK | 77.7 | 74.2 | 73.3 | 70.5 | 85.4 | 69.3 | 78.9 | 79.9 |
| | | | | | | | | |
| **D. Temporary employment/Employment** | | | | | | | | |
| Austria | 6.5 | 15 | 9.2 | 4.3 | 6.9 | 13 | 8.5 | 4.5 |
| Belgium | 4.7 | 12 | 9 | 4.3 | 9.1 | 16.8 | 10.8 | 8.7 |
| Denmark | 7.8 | 21.3 | 17 | 8.1 | 10.4 | 20.3 | 27.7 | 12 |
| Finland | 11.4 | 31.8 | 20.1 | 13.8 | 18.6 | 55.2 | 40.4 | 32 |
| France | 10.4 | 29.2 | 18.3 | 10.6 | 13.2 | 31.1 | 20.4 | 13.5 |
| Germany | 10.7 | 27.4 | 16 | 7.4 | 11.5 | 27.4 | 17.6 | 8.7 |
| Greece | 5.5 | 25.8 | 17.3 | 9.6 | 7.9 | 28.9 | 22.1 | 12.1 |
| Ireland | 2.9 | 8.1 | 3.4 | 2.2 | 5.3 | 11.8 | 5.5 | 5 |
| Netherlands | 8.8 | 31.9 | 22.7 | 11.8 | 14 | 37.7 | 25.5 | 15 |
| Norway | 7.3 | 16.1 | 15.1 | 8.3 | 11.9 | 24.3 | 18 | 13.4 |
| Portugal | 10.7 | 45.5 | 32.7 | 16.7 | 13.1 | 42.5 | 26 | 17.9 |
| Spain | 22.9 | 57 | 40.5 | 24.8 | 28.1 | 48.1 | 37.6 | 23.8 |
| Sweden | 9.8 | 29 | 19.4 | 10.8 | 14.7 | 39.5 | 32.6 | 15.6 |
| Switzerland | 18 | 17.7 | 13.1 | 6.8 | 20.6 | 18.5 | 14.9 | 8.3 |
| UK | 4.5 | 17 | 9.5 | 4.5 | 6.4 | 22.7 | 10.7 | 7 |

SOURCE: EU Labor Force Survey

NOTE: Averages over the period of data coverage. Sample restricted to population aged 20-59. Definition of immigrants is based on nativity.

**Figure 2. Evolution of EPL Index for Permanent Contracts 1985-2005**



EPL Permanent Contracts

SOURCE: See Data Appendix

**Figure 3. Evolution of EPL Index for Temporary Contracts 1985-2005**



EPL Temporary Contracts

SOURCE: See Data Appendix

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**Figure 4. Correlation between EPL and Unemployment Duration**



**Figure 5. Correlation between EPL and Employment Duration**



**Figure 6. Correlation between EPL and the Employment Rate**



SOURCE: for labor market variables, EU Labor Force Survey; for employment protection index, see Data Appendix.

NOTE: ρ denotes the correlation coefficient between the series in the two axes.

**Table 3. Effect of EPL on Participation and Employment**

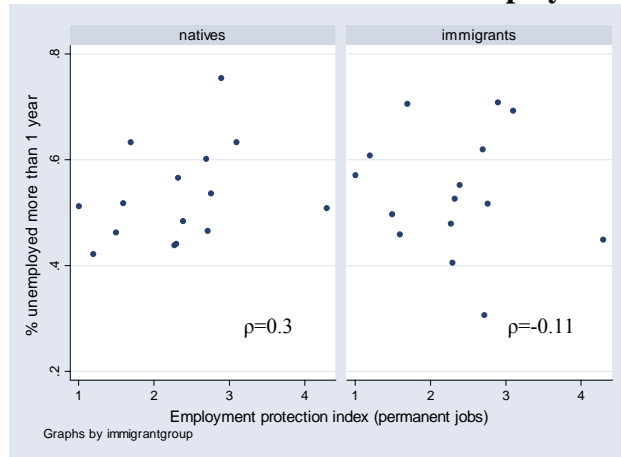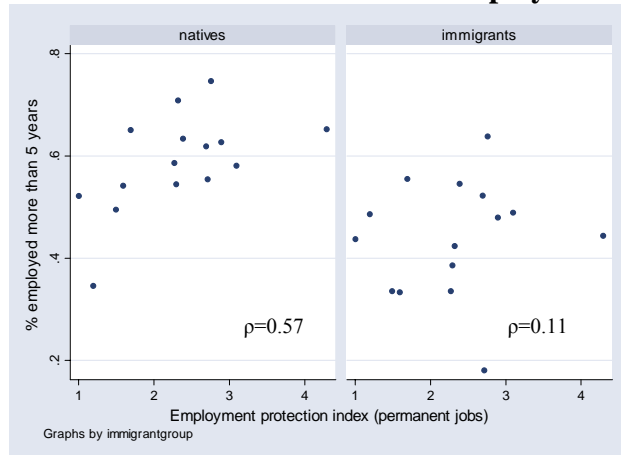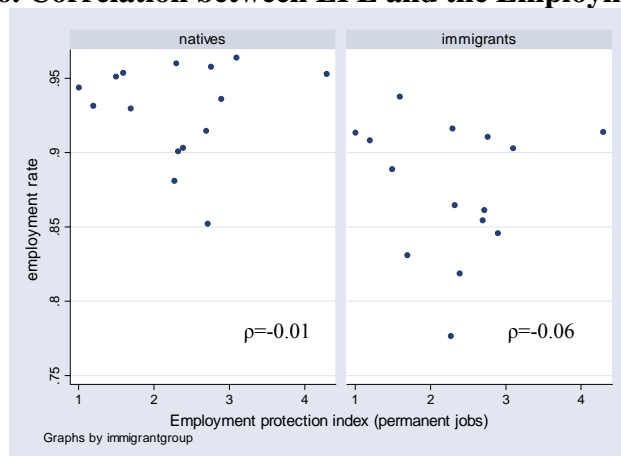| | Labor Force Participation Rate | | Employment Rate | |
|---|---|---|---|---|
| | (1) | (2) | (1) | (2) |
| EPL permanent | -0.006 | - | -0.038* | - |
| | (0.012) | | (0.023) | |
| EPL temporary | - | 0.009** | - | 0.013*** |
| | | (0.004) | | (0.004) |
| Years residence 0-5 | 0.111*** | 0.109*** | -0.019 | -0.029 |
| | (0.030) | (0.026) | (0.027) | (0.027) |
| Years residence 6-10 | 0.175*** | 0.224*** | 0.015 | 0.037* |
| | (0.033) | (0.027) | (0.023) | (0.020) |
| Years residence >10 | 0.074*** | 0.080*** | -0.093*** | -0.060*** |
| | (0.018) | (0.018) | (0.021) | (0.014) |
| Years residence 0-5*EPL permanent | 0.015 | - | 0.003 | - |
| | (0.030) | | (0.047) | |
| Years residence 6-10*EPL permanent | 0.041 | - | 0.064** | - |
| | (0.036) | | (0.028) | |
| Years residence >10*EPL permanent | 0.003 | - | 0.108*** | - |
| | (0.023) | | (0.032) | |
| Years residence 0-5*EPL temporary | - | -0.002 | - | -0.013 |
| | | (0.013) | | (0.015) |
| Years residence 6-10*EPL temporary | - | 0.023** | - | -0.013 |
| | | (0.010) | | (0.011) |
| Years residence >10*EPL temporary | - | -0.011* | - | -0.015** |
| | | (0.006) | | (0.006) |
| Medium education | 0.143*** | 0.141*** | 0.048*** | 0.047*** |
| | (0.005) | (0.005) | (0.003) | (0.003) |
| High education | 0.232*** | 0.231*** | 0.070*** | 0.069*** |
| | (0.006) | (0.006) | (0.004) | (0.004) |
| Medium education*EPL permanent | -0.040*** | - | -0.016*** | - |
| | (0.006) | | (0.003) | |
| High education*EPL permanent | -0.036*** | - | -0.025*** | - |
| | (0.007) | | (0.004) | |
| Medium education*EPL temporary | - | -0.024*** | - | -0.008*** |
| | | (0.003) | | (0.003) |
| High education*EPL temporary | - | -0.014*** | - | -0.005 |
| | | (0.004) | | (0.003) |
| Female | -0.121*** | -0.122*** | -0.024*** | -0.024*** |
| | (0.005) | (0.005) | (0.003) | (0.003) |
| Age 25-39 | 0.274*** | 0.273*** | 0.089*** | 0.089*** |
| | (0.011) | (0.011) | (0.005) | (0.005) |
| Age 40-59 | 0.213*** | 0.213*** | 0.118*** | 0.117*** |
| | (0.010) | (0.010) | (0.007) | (0.007) |

SOURCE: EU Labor Force Survey
NOTE: EPL is measured as deviation from cross country mean; N=4428; standard errors adjusted for clustering by country*year; regressions also control for country and year dummies, country*years residence, year*years residence, female*years residence, age*years residence and education*years residence. OLS estimates. * denotes significance at the 10% level, ** at the 5% level, and *** at the 1% level.

## Table 4. Effect of EPL on Employment and Unemployment Duration

| | Unemployed >1 year | | Employed > 5 years | | Employed > 1 year | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (1) | (2) | (1) | (2) |
| EPL permanent | 0.132*** | - | 0.097** | - | 0.002 | - |
| | (0.043) | | (0.045) | | (0.042) | |
| EPL temporary | - | -0.012 | - | 0.008 | - | 0.023*** |
| | | (0.018) | | (0.005) | | (0.006) |
| Years residence 0-5 | 0.038 | 0.083 | -0.078** | -0.150*** | -0.135*** | -0.092* |
| | (0.057) | (0.062) | (0.032) | (0.045) | (0.048) | (0.049) |
| Years residence 6-10 | 0.171** | 0.100 | 0.011 | 0.032 | 0.218*** | 0.190*** |
| | (0.075) | (0.074) | (0.087) | (0.082) | (0.043) | (0.042) |
| Years residence >10 | -0.032 | -0.100** | -0.050** | -0.047** | -0.060*** | -0.059*** |
| | (0.035) | (0.039) | (0.020) | (0.018) | (0.022) | (0.017) |
| Years residence 0-5*EPL permanent | 0.043 | - | -0.053 | - | 0.175*** | - |
| | (0.085) | | (0.071) | | (0.060) | |
| Years residence 6-10*EPL permanent | -0.195** | - | 0.043 | - | -0.079 | - |
| | (0.097) | | (0.086) | | (0.057) | |
| Years residence >10*EPL permanent | -0.081* | - | 0.048 | - | 0.034 | - |
| | (0.048) | | (0.036) | | (0.026) | |
| Years residence 0-5*EPL temporary | - | 0.036 | - | -0.040*** | - | -0.027* |
| | | (0.029) | | (0.015) | | (0.015) |
| Years residence 6-10*EPL temporary | - | 0.029 | - | -0.010 | - | 0.007 |
| | | (0.030) | | (0.020) | | (0.013) |
| Years residence >10*EPL temporary | - | -0.014 | - | 0.012 | - | -0.010* |
| | | (0.015) | | (0.008) | | (0.006) |
| Medium education | -0.043*** | -0.041*** | 0.018*** | 0.019*** | 0.027*** | 0.028*** |
| | (0.006) | (0.006) | (0.004) | (0.003) | (0.002) | (0.002) |
| High education | -0.084*** | -0.083*** | -0.021*** | -0.021*** | -0.001 | -0.001 |
| | (0.010) | (0.010) | (0.004) | (0.004) | (0.004) | (0.003) |
| Medium education*EPL permanent | 0.029*** | - | 0.002 | - | 0.001 | - |
| | (0.008) | | (0.004) | | (0.003) | |
| High education*EPL permanent | 0.041*** | - | 0.007* | - | -0.007** | - |
| | (0.011) | | (0.004) | | (0.003) | |
| Medium education*EPL temporary | - | 0.014*** | - | -0.010*** | - | -0.008*** |
| | | (0.005) | | (0.003) | | (0.002) |
| High education*EPL temporary | - | 0.019*** | - | -0.001 | - | -0.007*** |
| | | (0.006) | | (0.003) | | (0.003) |
| Female | 0.062*** | 0.062*** | -0.030*** | -0.030*** | -0.022*** | -0.022*** |
| | (0.005) | (0.005) | (0.002) | (0.002) | (0.002) | (0.002) |
| Age 25-39 | 0.182*** | 0.183*** | 0.429*** | 0.430*** | 0.308*** | 0.308*** |
| | (0.008) | (0.008) | (0.005) | (0.005) | (0.006) | (0.006) |
| Age 40-59 | 0.292*** | 0.292*** | 0.707*** | 0.707*** | 0.415*** | 0.414*** |
| | (0.011) | (0.011) | (0.006) | (0.006) | (0.009) | (0.009) |

SOURCE: EU Labor Force Survey
NOTE: Dependent variable: for unemployment duration, fraction of unemployed who have been in unemployment for more than one year; for employment duration, fraction of employed who have been on the current job for more than one year/more than five years. EPL is measured as deviation from cross country mean. N=4317. Standard errors adjusted for clustering by country*year. Regressions also control for country and year dummies, country*years residence, year*years residence, female*years residence, age*years residence and education*years residence. OLS estimates.

# Table 5. Labor Market Reforms in Spain after 1997

| | Dismissal costs under existing permanent contracts | Dismissal costs under new permanent contracts | Payroll tax reductions for newly hired workers under permanent contracts in 1997-1998 | Payroll tax reductions for newly hired workers under permanent contracts in 1999 |
|---|---|---|---|---|
| Unemployed aged 30-44 years | Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 45 days' wages per year of seniority with a maximum of 42 months' wages | Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 45 days' wages per year of seniority with a maximum of 42 months' wages | None | None |
| Young unemployed workers (under 30 years of age) | Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 45 days' wages per year of seniority with a maximum of 42 months' wages | Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 33 days' wages per year of seniority with a maximum of 24 months' wages | 40% of employer contributions for 24 months | 35% of employer contributions for 12 months, 25% for another 12 months |
| Unemployed workers above 45 years of age | Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 45 days' wages per year of seniority with a maximum of 42 months' wages | Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 33 days' wages per year of seniority with a maximum of 24 months' wages | 60% of employer contributions for 24 months, 50% thereafter | 45% of employer contributions for 12 months, 40% for another 12 months |
| Long-term unemployed (over 1 year of registered unemployment) | Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 45 days' wages per year of seniority with a maximum of 42 months' wages | Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 33 days' wages per year of seniority with a maximum of 24 months' wages | 40% of employer contributions for 24 months | 40% of employer contributions for 12 months, 30% for another 12 months |
| Workers employed under temporary contracts | Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 45 days' wages per year of seniority with a maximum of 42 months' wages | Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 33 days' wages per year of seniority with a maximum of 24 months' wages | 50% employer contributions for 24 months, 20% for another 12 months | None |
| Women hired under temporary contracts or long-term unemployed hired in occupations with low weight of female employment | Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 45 days' wages per year of seniority with a maximum of 42 months' wages | Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 33 days' wages per year of seniority with a maximum of 24 months' wages | 60% employer contributions for 24 months, 20% for another 12 months | 45% employer contributions for 24 months, 40% for another 12 months |
| Workers hires under training contracts | Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 45 days' wages per year of seniority with a maximum of 42 months' wages | Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 33 days' wages per year of seniority with a maximum of 24 months' wages | 50% employer contributions for 24 months, 20% for another 12 months | 25% employer contributions for 24 months |
| Workers above 45 years of age hired under temporary contracts | Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 45 days' wages per year of seniority with a maximum of 42 months' wages | Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 33 days' wages per year of seniority with a maximum of 24 months' wages | 60% employer contributions for 24 months, 20% for another 12 months | 60% employer contributions for 24 months, 20% for another 12 months |
| Disabled workers | Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 45 days' wages per year of seniority with a maximum of 42 months' wages | Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 33 days' wages per year of seniority with a maximum of 24 months' wages | 70%-90% for the whole employment spell | 70%-90% for the whole employment spell |

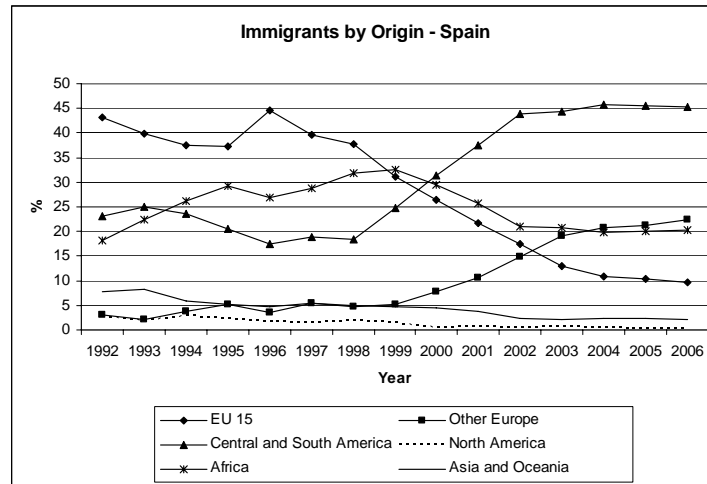SOURCE: reproduced from Kugler et al (2002)

**Table 6. Coverage of Immigrants in the Spanish EPA**

| Year | % Immigrants in Labor Force | |
|------|-----------|-------------|
| | Spain LFS | OECD (2006) |
| 1995 | 1.08 | 0.8 |
| 1996 | 1.11 | 1.0 |
| 1007 | 1.18 | 1.1 |
| 1008 | 1.38 | 1.2 |
| 1999 | 1.58 | 1.1 |
| 2000 | 1.79 | 2.5 |
| 2001 | 2.56 | 3.4 |
| 2002 | 3.52 | 4.5 |
| 2003 | 4.67 | 5.2 |
| 2004 | 5.54 | 6.3 |
| 2005 | 6.62 | n.a. |
| 2006 | 8.34 | n.a. |

SOURCE: Spanish economically active population survey (EPA) and OECD (2006)
NOTE: OECD (2006) data comes from the number of valid work permits. EU workers are not included. From 2000 on, data refers to the number of workers who are registered in the Social Security system. The source of the data is the Ministry of Labor and Social Security.

**Figure 7. Origin of Immigrants - Spain**



SOURCE: Spanish economically active population survey (EPA)

**Table 7. Descriptive Statistics for the Spanish EPA**

| | Age 16-29 | | Age 30-44 | | Age 45-59 | |
|---|---|---|---|---|---|---|
| | Pre-reform | Post-reform | Pre-reform | Post-reform | Pre-reform | Post-reform |
| | A. Native Men | | | | | |
| Employment probability | 39.25 | 49.58 | 78.09 | 84.54 | 67.1 | 74.25 |
| Permanent employment probability | 36.23 | 43.06 | 75.06 | 74.92 | 84.62 | 85.08 |
| Temporary employment probability | 63.77 | 56.94 | 24.94 | 25.08 | 15.38 | 14.92 |
| | B. Native Women | | | | | |
| Employment probability | 25.97 | 36.51 | 36.68 | 50.83 | 19.16 | 32.46 |
| Permanent employment probability | 37.33 | 42.4 | 73.11 | 70.9 | 80.02 | 82.2 |
| Temporary employment probability | 62.67 | 57.6 | 26.89 | 29.1 | 19.98 | 17.8 |
| | C. Immigrant Men | | | | | |
| Employment probability | 48.29 | 68.72 | 69.08 | 83.46 | 52.66 | 71.46 |
| Permanent employment probability | 25.4 | 26.19 | 48.63 | 40.62 | 71.33 | 50.8 |
| Temporary employment probability | 74.6 | 73.81 | 51.37 | 59.38 | 28.67 | 49.2 |
| | D. Immigrant Women | | | | | |
| Employment probability | 24.78 | 47.1 | 31.95 | 56.11 | 22.27 | 47.09 |
| Permanent employment probability | 35.4 | 35.7 | 51.8 | 47.06 | 73.25 | 56.77 |
| Temporary employment probability | 64.6 | 64.3 | 48.2 | 52.94 | 26.75 | 43.23 |

SOURCE: Spanish economically active population survey (EPA)

# Table 8. Employment Effects of the 1997 and 2001 Spanish Reforms

| | (1) | | | (2) | | |
|---|---|---|---|---|---|---|
| | P(employment) | Log[P(temporary)/ P(non-employment)] | Log[P(permanent)/ P(non-employment)] | P(employment) | Log[P(temporary)/ P(non-employment)] | Log[P(permanent)/ P(non-employment)] |
| | | | A. Native Men | | | |
| Age 16-29 | -0.197*** | -0.202*** | -1.527*** | -0.197*** | -0.209*** | -1.512*** |
| | (0.010) | (0.041) | (0.055) | (0.010) | (0.042) | (0.056) |
| Age 45-59 | -0.156*** | -1.289*** | -0.488*** | -0.160*** | -1.296*** | -0.509*** |
| | (0.010) | (0.045) | (0.046) | (0.011) | (0.046) | (0.047) |
| Age 16-29*Post | -0.007 | -0.177*** | 0.165*** | -0.007 | -0.180*** | 0.180*** |
| | (0.005) | (0.034) | (0.034) | (0.005) | (0.034) | (0.029) |
| Age 45-59*Post | -0.048*** | -0.158*** | -0.252*** | -0.051*** | -0.164*** | -0.268*** |
| | (0.008) | (0.036) | (0.046) | (0.008) | (0.036) | (0.043) |
| Age 16-29*Expansion | - | - | - | 0.001 | 0.029 | -0.094** |
| | | | | (0.005) | (0.023) | (0.038) |
| Age 45-59*Expansion | - | - | - | 0.017** | 0.035 | 0.113** |
| | | | | (0.009) | (0.029) | (0.050) |
| | | | B. Native Women | | | |
| Age 16-29 | -0.186*** | -0.091 | -1.542*** | -0.183*** | -0.095 | -1.508*** |
| | (0.016) | (0.075) | (0.094) | (0.016) | (0.076) | (0.095) |
| Age 45-59 | -0.116*** | -0.985*** | -0.375*** | -0.115*** | -00980*** | -0.374*** |
| | (0.008) | (0.048) | (0.041) | (0.009) | (0.051) | (0.043) |
| Age 16-29*Post | 0.014* | -0.128*** | 0.239*** | 0.017** | -0.128*** | 0.264*** |
| | (0.008) | (0.038) | (0.052) | (0.007) | (0.037) | (0.046) |
| Age 45-59*Post | 0.017** | -0.084* | 0.129*** | 0.017** | -0.081 | 0.130*** |
| | (0.008) | (0.048) | (0.039) | (0.008) | (0.052) | (0.036) |
| Age 16-29*Expansion | - | - | - | -0.018*** | 0.021 | -0.180*** |
| | | | | (0.006) | (0.022) | (0.050) |
| Age 45-59*Expansion | - | - | - | -0.0004 | -0.029 | -0.002 |
| | | | | (0.005) | (0.044) | (0.026) |
| | | | C. Immigrant Men | | | |
| Age 16-29 | -0.082*** | -0.171** | -0.915*** | -0.078*** | -0.176** | -0.867*** |
| | (0.018) | (0.081) | (0.118) | (0.018) | (0.079) | (0.125) |
| Age 45-59 | -0.178*** | -1.227*** | -0.833*** | -0.168*** | -1.209*** | -0.765*** |
| | (0.023) | (0.114) | (0.118) | (0.024) | (0.121) | (0.132) |
| Age 16-29*Post | -0.008 | -0.239*** | 0.216* | -0.01 | -0.231*** | 0.199* |
| | (0.016) | (0.079) | (0.111) | (0.016) | (0.075) | (0.117) |
| Age 45-59*Post | 0.006 | 0.332*** | 0.023 | 0.004 | 0.333*** | 0.008 |
| | (0.019) | (0.117) | (0.124) | (0.019) | (0.119) | (0.131) |
| Age 16-29*Expansion | - | - | - | -0.013 | 0.016 | -0.239** |
| | | | | (0.018) | (0.085) | (0.100) |
| Age 45-59*Expansion | - | - | - | -0.039** | -0.086 | -0.334*** |
| | | | | (0.019) | (0.119) | (0.092) |
| | | | D. Immigrant Women | | | |
| Age 16-29 | -0.127*** | -0.239*** | -0.907*** | -0.125*** | -0.244*** | -0.908*** |
| | (0.022) | (0.062) | (0.200) | (0.022) | (0.062) | (0.203) |
| Age 45-59 | -0.138*** | -1.093*** | -0.291*** | -0.139*** | -1.107*** | -0.278*** |
| | (0.017) | (0.093) | (0.100) | (0.018) | (0.094) | (0.103) |
| Age 16-29*Post | -0.002 | -0.117* | 0.134 | -0.002 | -0.115* | 0.134 |
| | (0.022) | (0.062) | (0.197) | (0.022) | (0.061) | (0.198) |
| Age 45-59*Post | 0.052** | 0.593*** | 0.046 | 0.052** | 0.061*** | 0.044 |
| | (0.022) | (0.108) | (0.111) | (0.022) | (0.106) | (0.110) |
| Age 16-29*Expansion | - | - | - | -0.011 | 0.023 | 0.005 |
| | | | | (0.014) | (0.052) | (0.092) |
| Age 45-59*Expansion | - | - | - | 0.005 | 0.082 | -0.081 |
| | | | | (0.016) | (0.073) | (0.073) |

SOURCE: Spanish economically active population survey (EPA)
NOTE: Logit marginal effects. Regressions include year and quarter dummies, an indicator for head of household, an indicator for being married, number of years of schooling and, for immigrants, years of residence and region of nationality (EU 15, other Europe, Central and South America, North America, Africa, and Asia and Oceania).

## Table 9. Descriptive Statistics for the Italian WHIP

| | Small firms | | Large firms | |
|---|---|---|---|---|
| | Pre-reform | Post-reform | Pre-reform | Post-reform |
| | A. Native Men | | | |
| Hiring rate | 37.86 | 11.06 | 27.17 | 9.50 |
| Firing rate | 28.98 | 28.13 | 14.59 | 21.71 |
| | B. Native Women | | | |
| Hiring rate | 35.13 | 9.68 | 28.44 | 12.07 |
| Firing rate | 26.54 | 27.00 | 16.45 | 23.82 |
| | C. Immigrant Men | | | |
| Hiring rate | 49.32 | 27.11 | 38.27 | 23.94 |
| Firing rate | 40.18 | 40.65 | 24.84 | 32.62 |
| | D. Immigrant Women | | | |
| Hiring rate | 51.59 | 26.44 | 41.10 | 23.83 |
| Firing rate | 43.65 | 40.69 | 30.47 | 33.49 |

SOURCE: Work Histories Italian Panel (WHIP)
NOTE: Sample restricted to workers with permanent contracts.

## Table 10. Effect of the 1990 Italian Reform on Hiring and Firing Rates

| | Hiring Rate | | Firing Rate | |
|---|---|---|---|---|
| | (1) | (2) | (1) | (2) |
| | A. Men | | | |
| Post | -0.141*** | -0.135*** | 0.057*** | 0.090*** |
| | (0.013) | (0.014) | (0.011) | (0.011) |
| Small firm | 0.0002 | 0.0001 | 0.104*** | 0.103*** |
| | (0.010) | (0.010) | (0.012) | (0.011) |
| Immigrant | 0.087 | 0.091 | 0.528*** | 0.537*** |
| | (0.242) | (0.241) | (0.194) | (0.185) |
| Immigrant*Post | 0.060*** | 0.064*** | -0.011** | -0.016*** |
| | (0.010) | (0.011) | (0.005) | (0.005) |
| Immigrant*Small firm | -0.005 | -0.005 | -0.032*** | -0.032*** |
| | (0.005) | (0.005) | (0.008) | (0.008) |
| Small firm*Post | -0.034*** | -0.034*** | -0.065*** | -0.073*** |
| | (0.003) | (0.003) | (0.002) | (0.002) |
| Immigrant*Small firm*Post | 0.027*** | 0.017*** | 0.037*** | 0.031*** |
| | (0.004) | (0.003) | (0.003) | (0.003) |
| Recession | - | -0.015*** | - | -0.086*** |
| | | (0.001) | | (0.001) |
| Small firm*Recession | - | 0.0003 | - | 0.043*** |
| | | (0.0005) | | (0.0004) |
| | B. Women | | | |
| Post | -0.132*** | -0.129*** | 0.043*** | 0.075*** |
| | (0.009) | (0.009) | (0.006) | (0.006) |
| Small firm | -0.013* | -0.013* | 0.080*** | 0.079*** |
| | (0.007) | (0.007) | (0.008) | (0.008) |
| Immigrant | 0.156 | 0.157 | 0.616*** | 0.611*** |
| | (0.209) | (0.212) | (0.082) | (0.084) |
| Immigrant*Post | 0.040*** | 0.044*** | -0.003 | -0.004 |
| | (0.011) | (0.011) | (0.005) | (0.006) |
| Immigrant*Small firm | -0.008*** | -0.008*** | -0.044*** | -0.044*** |
| | (0.002) | (0.002) | (0.003) | (0.003) |
| Small firm*Post | -0.061*** | -0.062*** | -0.059*** | -0.066*** |
| | (0.003) | (0.004) | (0.003) | (0.003) |
| Immigrant*Small firm*Post | 0.030*** | 0.023*** | 0.023*** | 0.028*** |
| | (0.006) | (0.006) | (0.007) | (0.006) |
| Recession | - | -0.008*** | - | -0.085*** |
| | | (0.0007) | | (0.0008) |
| Small firm*Recession | - | 0.004*** | - | 0.036*** |
| | | (0.001) | | (0.0009) |

SOURCE: Work Histories Italian Panel (WHIP)
NOTE: logit marginal effects. Regressions also control for log(wage), age, sector of activity, region where firm is located, skill level and interactions of all the covariates with the immigrant dummy.

## Table 11. Summary of results

| **Experiment:** | **EU** Increase in EPL index | | **Spain** Reduction in EPL for young and old workers | | **Italy** Increase in EPL for small firms | |
|---|---|---|---|---|---|---|
| **Effect on:** | Natives | Immigrants | Natives | Immigrants | Natives | Immigrants |
| Employment rate | - | + (a) | | | | |
| Permanent employment | | | + | insignificant | | |
| Hiring rate | -- | insignificant | | | -- | - |
| Firing rate | - | - | | | -- | - |

NOTE: (a) for immigrants with more than 6 years of residence.