THE IMPACT OF EMPLOYMENT PROTECTION MANDATES ON DEMOGRAPHIC TEMPORARY EMPLOYMENT PATTERNS: INTERNATIONAL MICROECONOMIC EVIDENCE

by

Lawrence M. Kahn

Cornell University, Princeton University CESifo and IZA

February 2005

Revised June 2005

For presentation at: IZA Conference on Employment Protection and Labor Markets, Bonn, Germany, December 2-3, 2005.

*Preliminary draft. Comments welcome. The author thanks Giuseppe Bertola and Daniel Hamermesh for helpful comments and suggestions.

THE IMPACT OF EMPLOYMENT PROTECTION MANDATES ON DEMOGRAPHIC TEMPORARY EMPLOYMENT PATTERNS: INTERNATIONAL MICROECONOMIC EVIDENCE

Abstract

Using 1994-98 International Adult Literacy Survey (IALS) microdata, this paper investigates the impact of employment protection laws on the incidence of temporary employment by demographic group. More stringent employment protection for regular jobs is predicted to increase the relative incidence of temporary employment for less experienced and less skilled workers. I test this reasoning using IALS data for Canada, Finland, Italy, the Netherlands, Switzerland, the United Kingdom and the United States, countries with widely differing levels of mandated employment protection. Across these countries, the strength of such mandates (as measured by the OECD) is positively associated with the relative incidence of temporary employment for young workers, native women, immigrant women and those with low cognitive ability, controlling for demographic factors and country-specific effects influencing the overall incidence of temporary jobs. These effects largely hold up when I adjust for the possible sample selection due to the fact that employment to population ratios differ across countries, when I disaggregate the effects of the OECD employment protection index into its component parts, when I exclude countries with the highest or lowest levels of employment protection mandates, and when I exclude those of school attendance age (16-25 years old). Moreover, the effects of protection on the young, women, and immigrants are stronger in countries with higher levels of collective bargaining coverage, suggesting a connection between binding wage floors and the allocative effects of employment protection mandates.

JEL Classification: J21, J23.

Keywords: employment protection, temporary jobs.

I. Introduction

A considerable volume of economic research has been devoted over the last two decades to explaining and suggesting remedies for the stubbornly high unemployment rates in a number of European countries. Many authors have focused on labor market and other institutions as an important factor playing a role in influencing unemployment.[1] These institutions include collective bargaining, employment protection mandates, restrictions on business entry, and mandated benefit programs such as unemployment insurance (UI) and disability programs, as well as the taxes levied to pay for them. Temporary employment contracts without mandated protection (or considerably less protection than exists on permanent jobs) have been used in a number of countries as an attempt to generate jobs that would not have been created and, therefore, as a policy designed to lower unemployment. It is sometimes argued that by allowing firms to create jobs with a fixed duration and with little or no termination costs, policies authorizing fixed term contracts increase the flexibility of labor markets made rigid by the institutions just mentioned.[2] On the other hand, such policies may encourage firms to substitute temporary for permanent jobs thereby increasing the overall exit rate from jobs; the resulting higher turnover may even lead to higher unemployment than before, despite the new jobs created (Blanchard and Landier 2002).

While the ability of temporary contracts to lower the overall unemployment rate is uncertain, most analysts are agreed that more extensive employment protection mandates for permanent jobs increase incentives for firms to offer temporary jobs, and empirical research has found support for this prediction.[3] This outcome is important since temporary jobs tend to be

[1] See Nickell and Layard (1999), Blanchard and Wolfers (2000), and Nickell, Nunziata and Ochel (2005).
[2] A notable example is Spain, which in the 1980s and 1990s had extremely high unemployment rates and liberalized the use of temporary contracts in an attempt to generate jobs. See Dolado, Garcia-Serrano and Jimeno (2002).
[3] See, for example, Blanchard and Landier (2002), Cahuc and Postel-Vinay (2002), and Güell (2003) for theoretical models with this prediction. On the other hand, Lazear (1990) suggests that if wages are flexible, then firing costs need not raise the overall cost of offering permanent jobs. Instead, when there are high mandated firing costs, wages will adjust downward. Of course, if there are also mandated wage floors, then this adjustment cannot

lower paying, and offer less training, other things equal, than permanent jobs; moreover, workers in temporary jobs express lower levels of job satisfaction than comparable workers in permanent jobs (Booth, Francesconi and Frank 2002). Thus, policies that lead to a substitution of temporary jobs for permanent jobs may actually worsen the welfare of the average worker, especially in the event that this policy doesn't lead to lower unemployment (Blanchard and Landier 2002; Cahuc and Postel-Vinay 2002).

The reasoning in such theoretical models suggests that the incidence of temporary jobs will not be randomly distributed across the labor force. Specifically, when there are substantial firing costs for permanent jobs, firms will be relatively reluctant to hire new entrants into such jobs. Instead, new entrants will be placed in temporary jobs where their productivity can be assessed before a permanent offer is made. New entrants disproportionately include the young, women and, possibly, immigrants.

This paper studies the impact of employment protection mandates on demographic patterns of temporary employment. As I show below, an extension of these theoretical models implies that higher firing costs for permanent jobs widen the gap between the incidence of permanent jobs for experienced workers vs. recent entrants. Moreover, suppose that wage floors constrain firms' ability to compensate for firing costs by offering lower wages. Then low wage workers such as the young, women, immigrants, and those with low cognitive skills will also be less likely to be able to obtain permanent jobs. These effects will again be larger the more expensive it is to fire someone from a permanent job. To test this reasoning, I use the 1994-98 International Adult Literacy Surveys (IALS) microdata files, which contain information on whether one was employed in a temporary or a permanent job and a variety of demographic information. In addition, the IALS contains cognitive skills data on these individuals from common tests, allowing one to make comparisons across countries in the effect of employment

---

happen. Thus, the impact of firing costs for permanent jobs on the incidence of temporary jobs is to some degree an empirical question, and Booth, Dolado and Frank (2002) obtain aggregate evidence suggesting that employment protection does indeed raise the incidence of temporary employment.

protection by skill level.[4] The countries for which the IALS contains data allowing me to analyze these effects include Canada, Finland, Italy, the Netherlands, Switzerland, the United Kingdom and the United States. As I discuss further below, these countries differ widely in the extent to which they have enacted employment protection mandates, providing a high degree of variability in this key explanatory variable.

I find that across these countries, all else equal, the strength of employment protection mandates (as measured by the OECD) is positively associated with the incidence of temporary employment. Moreover, these effects are concentrated on young workers, native women, and especially immigrant women, as predicted. And there is some evidence that protection has a disproportionate effect raising the incidence of temporary employment for those of low cognitive ability, an expected outcome to the extent that wage floors prevent wages from adjusting in response to mandated employment protection. These results are robust to inclusion of country dummy variables; these account for country-specific factors that influence the strength of employment protection laws and the incidence of temporary jobs.

The basic results largely hold up when I adjust for the possible sample selection bias induced by the fact that employment to population ratios differ across countries. They also continue to hold when I disaggregate the OECD's employment protection index into its component parts: duration of mandated severance payments; mandated compensation for unfair dismissal; length of mandatory notice in the event of layoffs; and an index of procedural inconvenience facing employers who wish to dismiss workers. The results also are robust to the exclusion of individual countries with the highest (Italy or the Netherlands) or the lowest levels of mandated employment protection (the United States) and when I exclude those of school attendance age (16-25 years old). And I further find that collective bargaining coverage has significantly negative interaction effects with employment protection on the relative incidence of permanent jobs for the young, immigrants, and women, as predicted by the wage floor reasoning mentioned above. These results provide evidence that labor market institutions

[4] The IALS data are described in more detail below.

disproportionately protect the jobs of prime age males, effects that are complementary to existing research which finds that the young and women are disproportionately disemployed or unemployed in heavily unionized societies, all else equal (Bertola, Blau and Kahn 2002).

II. Employment Protection and Temporary Employment: Current Theory and Evidence

Early theories of the impact of employment protection mandates emphasized that making it difficult or expensive to fire workers reduced firms' incentives to lay off workers and to create new jobs. Of course, as noted earlier, if wages are flexible, then firing costs can be capitalized in lower initial wages, leaving firms' incentives to offer new jobs unchanged (Lazear 1990). However, if market imperfections such as wage floors or worker liquidity constraints prevent such a wage adjustment from occurring, then higher firing costs will lead to a greater disincentive to create jobs. Under these circumstances, the net effect on the unemployment rate will be theoretically indeterminate, since firing costs will lower both layoffs and job creation (Bertola 1990, 1992). But, the negative effects on job creation are expected to be disproportionately felt by new entrants, while incumbent workers are most directly affected by the negative impact of employment protection mandates on layoffs. Bertola, Blau and Kahn (2002) in fact find that more extensive employment protection does disproportionately raise young men's and young women's unemployment rates, other things equal. And Autor, Donohue and Schwab (2004) find similar results for states in the US that have granted workers the right to sue for wrongful discharge. Specifically, the authors found that this type of wrongful discharge protection reduced state employment rates, with largest effects for women, the young, and the less educated. As shown below, this same theme will inform my analysis of the impact of employment protection mandates on temporary employment.

More recent theories about employment protection recognize that firms have some rights to create temporary jobs which have a fixed duration and which can be terminated at the end of their term at relatively low cost or no cost at all. For example, Blanchard and Landier (2002) pose a model in which workers are hired into entry level, temporary jobs, and their productivity is observed by the firm. The firm then must decide whether to keep the worker in a permanent, regular job. Temporary jobs have lower firing costs than permanent jobs. The authors focus on the impact of lowering the firing costs of temporary jobs, while keeping the firing costs of permanent jobs the same, as occurred in France's recent reforms. Lower firing costs for temporary jobs or higher firing costs for permanent jobs both reduce the likelihood that a temporary job will be converted into a permanent one.[5]

Recent empirical research has examined the impact of firing costs on the incidence of temporary employment as well as the characteristics of such jobs and the workers in them. Specifically, Booth, Dolado and Frank (2002) use aggregate data to find that across 14 OECD countries for the 1980s and the 1990s, the fraction of employment that was in temporary jobs was significantly positively correlated with the OECD's index of strictness of regular employment protection mandates, as the theory outlined above predicts. However, the authors also found that the incidence of temporary employment was significantly positively correlated with the strictness of temporary employment regulation as well, a finding that is not consistent with this theory. The resolution of this apparent paradox was found by estimating a multiple regression including both permanent and temporary protection mandate indexes on the right hand side. The results continued to show a significantly positive effect on temporary employment of permanent protection laws but no effect of temporary employment protection. The authors then suggest that regulations on temporary employment protection don't play a role in influencing the incidence of temporary jobs. Rather, the main factor is the strictness of permanent employment protection regulations. Within the US, similar results have been found for the overall impact of employment protection. Specifically, Autor (2003) concluded that a state's granting workers the right to sue over wrongful termination led to an increase in the temporary help services industry employment, all else equal.

[5] See Cahuc and Postel-Vinay (2002) and Güell (2003) for theoretical models with a similar prediction.

In contrast to Booth, Dolado and Frank's (2002) findings that temporary employment regulations have no impact, Blanchard and Landier (2002) show that in France the transition probability from temporary to permanent jobs fell in the 1980s and the 1990s as the protections for temporary jobs were being relaxed, a prediction of the theory outlined above. Of course, the overall labor market was deteriorating in France at the same time, making a conclusion about the impact of the reforms tentative. Indeed, Holmlund and Storrie (2002) find that the recession in Sweden in the 1990s was a major cause of the rise in the incidence of temporary employment there.

In this paper, I extend existing theories and evidence on the impact of employment protection to examine its relative impact on different demographic groups. As discussed below, the basic theoretical setup in Blanchard and Landier (2002) can be shown to lead to a prediction that more stringent regulation of permanent employment will lead to a higher gap in the incidence of permanent employment between recent labor market entrants and more experienced workers. Moreover, I use microdata from several countries with varying degrees of employment protection strictness, allowing me to control for country-specific effects as well as observable heterogeneity across individuals in estimating the relative effects of protection mandates on temporary employment.

III. Employment Protection and the Relative Incidence of Temporary Employment: Theoretical Considerations

One can use the logic of Blanchard and Landier's (2002) model to study the impact of employment protection on the relative incidence of temporary employment among recent labor market entrants and experienced workers. In Blanchard and Landier's (2002) model all entry level jobs start with the same productivity y_0. Then after a period of unspecified duration, the firm receives an observation y on the worker's productivity. The firm then has the option of turning the job into a permanent one or terminating the worker and replacing him/her. Blanchard and Landier (2002) show that the firm's optimal policy is to set a threshold observed productivity level y* above which the worker is kept in a permanent job and below which the worker is terminated. This is analogous to the reservation wage policy in models of job search. To analyze the impact of firing costs on the gap in the incidence of permanent work between new entrants and experienced labor market participants, let c_p be firing costs for a permanent job, c_t be firing costs for a temporary job, and let $y^*(c_p, c_t)$ be the productivity threshold the firm requires in order to convert a temporary job to a permanent one, where $\partial y^*/\partial c_p > 0$ and $\partial y^*/\partial c_t < 0$.

Under these assumptions, the probability that a current spell of temporary employment is converted into a permanent job is:

1) $\text{Prob}\ (y > y^*(c_p, c_t)) = 1 - F(y^*(c_p, c_t)) \equiv q(y^*(c_p, c_t))$,

where F(-) is the distribution function for productivity and q(-) is the probability that a temporary job is converted into a permanent job.

We may now compare the impact of firing costs for permanent jobs on the relative incidence of permanent and temporary employment of experienced workers who have been in the labor market for, say, N>1 periods, and recent entrants who have been in the labor market for only one period. For simplicity, suppose that everyone is employed in each period. Then after one period in the labor market, the probability that a worker is still in a temporary job is:

2) $\text{Prob}(\text{temporary job} \mid \text{one period of total experience}) = F(y^*)$,

suppressing the arguments of y*. Assuming for simplicity that permanent jobs never end and that in each period, a worker in a new temporary job has the same probability of meeting the productivity threshold, the probability that one is in a temporary job after N periods of employment is[6]:

3) $\text{Prob}\ (\text{temporary job} \mid N \text{ periods of total experience}) = (F(y^*))^N$.

[6] These assumptions are made for simplicity. Below, I discuss the implications of relaxing some of them.

From 1)-3), the impact of firing costs for permanent jobs on the relative incidence of temporary employment among recent entrants and those with N years of experience is:

4) $\partial[F(y^*) - (F(y^*))^N]/\partial c_p = f(y^*)\partial y^*/\partial c_p - NF(y^*)^{N-1}f(y^*)\partial y^*/\partial c_p$, where f(-) is the density function for F(-).

According to 4), a rise in c_p lowers the relative probability of recent entrants' working in a permanent job (versus more experienced workers) if and only if:

5) $0<f(y^*)\partial y^*/\partial c_p - NF(y^*)^{N-1}f(y^*)\partial y^*/\partial c_p =f(y^*)\partial y^*/\partial c_p[1- NF(y^*)^{N-1}]$.

Since higher firing costs c_p raise the threshold productivity level y*, inequality 5) holds if and only if:

6) $\ln F(y^*)<\ln(1/N)/(N-1)$.

By l'Hôpital's rule, the right hand side of 6) approaches zero (from below) as N gets large. Since $0<F(y^*)<1$ (i.e. assuming an interior solution in which the firm will set a productivity threshold above the minimum and below the maximum achievable productivity level), eventually for large enough N, 6) will hold. This result make intuitive sense, since for large N, the probability that a worker with N periods of experience will not have landed a permanent job becomes arbitrarily low. From the result that $\partial y^*/\partial c_t<0$, a fall in firing costs from temporary jobs has the same qualitative effect as a rise in firing costs from permanent jobs.

The scenario just described assumes that there is no on the job learning. Workers keep entering temporary jobs until they get a good enough productivity draw to induce their employer to convert the job into a permanent one. If workers acquire general human capital in these temporary jobs, then the conclusion that higher firing costs raise the difference in the incidence of temporary work between recent entrants and more experienced workers is reinforced. This is the case since more experienced workers who have only had temporary jobs up to now have more human capital than less experienced workers in temporary jobs. This implies that the instantaneous hazard for leaving a temporary for a permanent job rises with experience. This effect will be less important the more easily junior workers can get permanent jobs (i.e., the lower firing costs are).

The basic logic of this analysis of experience and the incidence of permanent work is that more experienced workers get more chances to land a permanent job, even if there is no on the job learning. One scenario in which this makes sense is one where the productivity draw is match-specific. If a worker doesn't get a good draw, this outcome does not prejudice future firms against the worker. However, it is also possible that future firms may take a worker's failure to secure a permanent job as a negative indicator of the worker's productivity. In an extreme case, this signal may be so strong as to eliminate the worker's future chances of getting a permanent job and thus make more experienced workers no more likely to qualify for a permanent job than less experienced workers. In this extreme case, everyone gets exactly one chance to qualify for a permanent job. Therefore, the incidence of permanent employment for those with one year experience will be the same as the incidence with any level of experience greater than one. In such a case, high firing costs would have no effect on the experience gap in the incidence of permanent jobs. The intermediate case in which past failure to secure a permanent job provides some information to future employers about the current worker's productivity but where the worker still has a chance to eventually to get a permanent job is perhaps more likely. In such a scenario, the probability of permanent employment could still approach one as experience rises and therefore higher firing costs could still raise the experience gap in permanent employment.

III. Institutional Setting and Data

As noted earlier, I use 1994-98 IALS data for Canada, Finland, Italy, the Netherlands, Switzerland, the United Kingdom and the United States to study the impact of employment protection on the relative incidence of temporary employment among demographic groups. As Tables 1 and 2 indicate, these countries had very different regulations on job security in the

1990s. For example, Table 1 shows that Italy had much higher mandated severance pay both for no-fault dismissals and compensation for unfair dismissals than the other countries. The countries also differed with respect to the amount of notice a worker must be given before he/she can be dismissed, with employers in Finland being required to give 6 months notice, and those in the US not required to give any. Procedural delays were especially common in the Netherlands. Finally, the OECD provided an overall indicator of regular employment protection strictness, with Italy (2.8) and the Netherlands (3.1) at the top of my group of seven countries, followed by Finland at 2.1, with Switzerland, Canada and the UK in a group at 0.8-1.2, and the US with the least protection (0.2).

Table 2 shows the OECD's measures of regulation of temporary employment. In Canada, the UK and the US, there is no limit on the maximum number of fixed term contracts a firm is allowed to offer a worker. Italy is the only country in the group with a limit on the accumulated duration of fixed term contracts or any significant barriers to employment by temporary work agencies. Across countries, the overall temporary employment protection index and that for permanent employment have a correlation of 0.74, which is significant at the 5.7% level, despite the presence of only seven observations. The similarity of the countries' rankings for their regulation of permanent and temporary employment will make it difficult to distinguish the effects of these two types of regulation.

I use the IALS microdata to study the effects of employment protection mandates on permanent employment. The IALS is the result of an international cooperative effort, conducted over the 1994-8 period, to devise an instrument to compare the cognitive skills of adults across a number of countries.[7] The sampling frame was similar across countries, with the target population being those 16 years and older who were not in institutions or the military.[8] In

[7] For further description of the IALS, see OECD (1998) and USDOE, NCES (1998).

[8] There were some geographic exclusions in some cases, but these were 3% or less of the target population, except for Switzerland, where the exclusion of Italian and Rhaeto-Romantic regions, persons in institutions and persons without telephones accounted for 11% of the total potential sample. In all cases, the IALS supplied a set of sampling weights, which I used in all analyses, after I adjusted each country's weights so that the total weight for each country was the same. See the IALS documentation file, available from Statistics Canada.

addition to test scores, data are available on gender, immigrant status, employment status including whether one was in a temporary or a regular job, schooling, age, industry, and occupation.

Of unique interest in the IALS is its measurement of cognitive skills. This was accomplished through three tests that were administered to all respondents in their respective home languages. These tests were designed to measure:

"a) Prose literacy—the knowledge and skills needed to understand and use information from texts including editorials, news stories, poems and fiction;

b) Document literacy—the knowledge and skills required to locate and use information contained in various formats, including job applications, payroll forms, transportation schedules, maps, tables, and graphics; and

c) Quantitative literacy—the knowledge and skills required to apply arithmetic operations, either alone or sequentially, to numbers embedded in printed materials, such as balancing a checkbook, calculating a tip, completing an order form, or determining the amount of interest on a loan from an advertisement" (*IALS Guide CD-ROM*, page 9).

Proficiency in each of the three test areas was scored on a scale of 0-500, after the tests were read by several graders from the respondent's own country. The IALS provides five alternative estimates of proficiency for each test, which were computed from the raw test performance information using a multiple imputation procedure developed by Rubin (1987). These alternative estimates are in fact highly correlated. Within each of the three types of test, the five estimates of the score were correlated at roughly .9. Further, to ensure comparability of grading across countries, an average of 9.4% of the tests for each country were regraded by personnel from another country; inter-rater agreement with respect to these regrades was 94-99%.

Although, in principle, interpreting prose or documents, and using mathematics may each require different skills, these skills, as measured by the IALS, are in fact highly correlated. Forming a score for each of the three tests (i.e., quantitative, prose, and document literacy) based on the average of the five available estimates, I found that these scores were correlated at roughly .9. Due to this high correlation, in the econometric work that follows, I report results based on a measure of cognitive skills which is an average of the three average test scores for each individual.

Figures 1-4 show bivariate relationships between the incidence of permanent employment and the OECD's overall indicator of regular employment protection mandates, stratified by gender, age, immigrant status, or cognitive test score level. The sample includes all individuals in the seven countries listed earlier who were employed as wage and salary workers and who didn't have any missing data for the explanatory variables (described below). In each case, a regression line is included for each subgroup. Figure 1 shows declining incidence of permanent employment for both men and women as mandated employment protection becomes stricter. Of particular note is that the relationship is stronger for women than for men, at least as indicated by the steepness of the regression line. While women and men are roughly equally likely to have permanent jobs if employment protection is minimal, the predicted gap grows to about 8 percentage points (about 10%) at the strictest employment protection levels.

Figure 2 shows the relationship between permanent employment and employment protection for 16-25 year olds and 46-55 year olds. The employed young are substantially less likely than 46-55 year olds to have a permanent job even when employment protection is minimal: the gap is roughly 10 percentage points. More importantly for the argument here, the gap grows substantially as employment protection increases. Specifically, while the incidence of permanent employment for 46-55 year olds is very high at about 95% of employment and is uncorrelated with employment protection mandates, permanent employment for the young falls sharply when employment protection becomes more stringent. The latter ranges from about 85% when there is little protection to only 60% when protection is at its sample maximum.

Figure 3 shows the permanent employment-employment protection relationship broken down by immigrant status. The incidence of permanent employment falls for both natives and immigrants, with a steeper decline for immigrants. While the incidence is about 92-93% for immigrants and natives at low levels of employment protection, permanent employment falls to 85% for natives and about 73-74% for immigrants with high levels of protection.

Finally, Figure 4 shows the permanent employment-protection relationship for those with low test scores (as defined by the IALS) and for others. The IALS distinguished five literacy levels based on where one's continuous score fell: Level 1 (0-225); Level 2 (226-275); Level 3 (276-325); Level 4 (326-375); and Level 5 (376-500). In Figure 4, low test scores are defined as Level 1. For example, on the Prose Literacy test, Level 1 questions require "the reader to locate one piece of information in the text that is identical to or synonymous with the information given in the directive" (*IALS Guide* CD, page 19). An example, given by the IALS, is to determine from an aspirin bottle label the maximum number of days one should use the product. For higher levels of Prose Literacy, respondents are required to read and interpret more and more dense selections of text and to integrate several pieces of information. On the Document Literacy Test, respondents at Level 1 must "locate a single piece of information based on a literal match" (*IALS Guide* CD, page 24). Higher Levels of Document Literacy require one to wade through distracting information and to integrate several pieces of information or to make conditional inferences. Finally, the Level 1 Quantitative Literacy questions require the reader to perform a simple calculation that is clearly laid out. Higher Levels of Quantitative Literacy require one to find information given in an example and to know which calculations to make.

Comparing those with low cognitive ability with others is a particularly relevant exercise here. This is the case, since wage floors (and therefore constraints on firms' ability to compensate for high firing costs by lowering wages) are most likely to be binding for those with low ability (as well as other low wage workers such as youth, immigrants and women). Figure 4 shows that individuals with low test scores have a slightly lower predicted incidence of permanent employment than others do at low levels of protection, with about a one percentage point gap. The difference widens with higher levels of protection to about four percentage points.

Figures 1-4 all convey a similar message: stronger employment protection mandates have a more negative relationship with the incidence of permanent employment for low skill groups or workers with less experience than for higher skill or more experienced workers. These relationships were predicted by the theoretical reasoning discussed above. However, while suggestive, none of the Figures control for other influences on permanent employment. The econometric analyses in the next section will implement such controls.

IV. Empirical Procedures and Basic Results

To investigate whether more stringent employment protection mandates widen the gap in permanent employment between experienced and inexperienced or between skilled and less-skilled workers, I estimate the following logit model:

$$\begin{aligned}
7)\ \mathrm{Prob}(\mathrm{Perm}_{ij}) = L(&B'X_{ij} + a_1{*}\mathrm{EPL}_j + a_2{*}\mathrm{EPL}_j{*}\mathrm{AGE2635}_{ij} + a_3{*}\mathrm{EPL}_j{*}\mathrm{AGE3645}_{ij} \\
&+ a_4{*}\mathrm{EPL}_j{*}\mathrm{AGE4655}_{ij} + a_5{*}\mathrm{EPL}_j{*}\mathrm{AGE5665}_{ij} + a_6{*}\mathrm{EPL}_j{*}\mathrm{EDYRS}_{ij} + \\
&a_7{*}\mathrm{EPL}_j{*}\mathrm{LEVEL1}_{ij} + a_8{*}\mathrm{EPL}_j{*}\mathrm{FEMALE}_{ij} + a_9{*}\mathrm{EPL}_j{*}\mathrm{IMMIG}_{ij}),
\end{aligned}$$

where for employed wage and salary worker i in country j between 16 and 65 years old, Perm is a dummy variable equaling one if one's job is permanent, L(-) is the logit function, X is a vector of explanatory variables to be described, EPL is the country's OECD permanent employment protection indicator, AGE2635-AGE5665 are a series of dummy variables for age in the ranges 26-35, 36-45, 46-55, and 56-65 respectively (16-25 years old is the omitted age category) [9], EDYRS is years of schooling, LEVEL1 is a dummy variable for having average test score in the LEVEL 1 (lowest) range, FEMALE is a female dummy variable, and IMMIG is an immigrant dummy variable.

[9] I adopted this age specification because the IALS age data for Canada were only available in categorical form.

The dependent variable in this analysis is a dummy variable for having a permanent job among wage and salary workers. Thus, I have abstracted from the issues of the impact of employment protection on employment, self-employment or school attendance.[10] This design has the virtue of allowing one to focus on the theories discussed earlier and in effect control for labor supply and school enrollment choices that may be confounded with employment protection mandates. However, by focusing on the employed, one may introduce sample selection biases; below, I discuss some specifications where I attempt to correct for these biases as well as models that attempt to deal with issue of excluding those enrolled in school.

The explanatory variables in X include main effects for the four age group dummies just mentioned, years of schooling, low test score, gender, and immigrant status, as well as a full set of interactions of gender and the age, education, low test score and immigrant variables. In addition, in some models, a set of eight one digit industry and occupation dummy variables and their interactions with the gender dummy variable are included.[11] Including occupation and industry can control for compositional differences across countries. If, for example, countries with stricter employment protection laws also have relatively large sectors in which temporary work is common for reasons other than mandated protection, then failure to control for sector may produce a spurious negative relationship between protection and permanent jobs. This example illustrates the value of using microdata, which allow one to control for compositional factors. However, employment protection laws may themselves lead to changes in the relative sizes of sectors if they raise costs in some industries or occupations more than in others. In this scenario, the sectoral composition is part of the impact of employment protection laws. Thus, I

---

[10] I exclude the self-employed on the grounds that the theories of temporary employment apply better to employees rather than to business owners. In addition, a self-employed respondent may interpret the question of temporary or permanent employment differently from an employee. However, when I included the self-employed, the basic results were unaffected.

[11] The industries are: 1. Agriculture, hunting, forestry and fishing; 2. Mining and quarrying; 3. Manufacturing; 4. Electricity, gas and water; 5. Construction; 6. Wholesale and retail trade; 7. Transport, storage and communication; 8. Finance, insurance, real estate and business services; and 9. Community, social and personal services. The occupations are: 1. Legislators, senior officials and managers; 2. Professionals; 3. Technicians and associate professionals; 4. Clerks; 5. Service workers and shop and market sales workers; 6. Skilled agricultural and fishery workers; 7. Craft and related trades workers; 8. Plant and machine operators and assemblers; and 9. Elementary occupations. In each case, category number 1 is the omitted category.

also present estimates with occupation and industry excluded, which allow employment protection to have its full effects.

Coefficients a_2-a_9 test the hypothesis that employment protection has different effects on the indicated demographic or skill group. In addition, a main effect a_1 is included, which gives the impact of employment protection when the age, education, gender, test score and immigrant status variables all equal zero. Moreover, equations like 7) were estimated adjusting the IALS individual sampling weights so that each country receives the same total weight. In addition, the standard errors are corrected for clustering within countries.

A challenge in doing international comparative labor market research is that many institutions occur in clusters, and it may be difficult to pinpoint the effect of one institution such as employment protection across a sample of OECD countries (Bertola, Blau and Kahn 2002). With only seven countries to work with here, it is not possible to control for the full set of other institutions that could potentially affect the incidence of permanent employment. But, since the key effects I am interested in are the interactions between protection and demographic or skill variables, it is possible to replace the protection main effect with a series of country dummies. These summarize all other unmeasured influences on the incidence of permanent employment, including other policies and institutions such as taxes, UI, collective bargaining, disability programs, and product market regulation, as well as the availability and quality of educational opportunities and population characteristics that might make temporary employment more likely. Therefore, some versions of equation 7) were estimated with country dummies.

Even with country dummies, however, other institutions such as collective bargaining coverage may have indirect effects on the relative incidence of permanent employment across demographic or skill groups. For example, if unions compress wages (Blau and Kahn 1996), then collective bargaining may accentuate the effects of employment protection in shutting younger, female, immigrant or less skilled workers out of permanent jobs. Therefore, in some models, I allow for interactions between employment protection and 1994 collective bargaining coverage and the demographic variables, as well as of course collective bargaining main effects, lower-level interactions between collective bargaining and the controls, and an interaction between collective bargaining and protection.[12] Moreover, since the availability of schooling opportunities could affect the relative incidence of temporary employment among employed youth, particularly those with high cognitive ability levels who would be the most likely to enroll, I also test the robustness of the basic results to exclusion of those age 16-25 years old. This sample is not likely to be directly greatly affected by schooling opportunities and therefore provides an additional, sharper test of the basic hypotheses outlined above.

As discussed further below, I also attempted several other specifications. First, in some models I also control for temporary employment regulation and its interactions with age, education, test score, gender and nativity status. Efforts to disentangle the effects of regular and temporary employment regulation must remain tentative, due to the previously-mentioned high correlation between permanent and temporary employment regulation. Second, I test whether the demographic effects of employment protection differ by gender. This might be expected, since women earn lower pay than men and are therefore more likely to be constrained by wage floors. Third, since the estimation sample consists of employed workers, I also address the issue of possible selection bias. For example, in countries where employment rates are relatively low, the employed workers may have particularly high work motivation or unmeasured skills (relative to the population as a whole) compared to countries with high employment rates. Workers with high levels of work motivation or unmeasured skills may be more likely than otherwise to obtain permanent employment. Since employment-population differences across countries are much larger for young people and women than for prime age males (Bertola, Blau and Kahn 2002), such selection issues may directly affect my protection-demographic group interactions. Therefore, as discussed in more detail below, in some specifications, I address this possible selection bias. Fourth, in some analyses, I disaggregate the OECD's overall protection index into its component parts, reflecting severance pay, unfair dismissal pay, mandatory notice of

[12] Collective bargaining coverage information is taken from OECD (1997).

layoffs, and procedural delays. Finally, I investigated the sensitivity of the results to exclusion of countries with very high or very low levels of employment protection.

Table 3 shows ordinary least squares (OLS) and logit analyses of the determinants of permanent employment. I vary the specifications in two ways: i) inclusion or exclusion of industry and occupation dummies and their interactions with gender; ii) inclusion or exclusion of country dummies.[13] Overall, Table 3 shows that all else equal, protection has more positive effects on permanent employment for older workers, those scoring above the lowest level on the IALS literacy tests, men and native born workers, as our earlier theoretical discussion predicted. The interaction effects are significant in almost every case for age (except for age 26-35 in the logits), in every case for gender, and usually significant or marginally so for literacy and immigrant status. Moreover, the interaction effects increase algebraically in each case with rising age beyond 35, suggesting rising relative protection as workers age. The OLS results show a significant interaction effect for age 26-35, while the logits show a small and insignificant interaction for this group (relative of course to the 16-25 year old omitted group). Effects of education are never large in absolute value or statistically significant, in contrast to the findings for test score, which has the advantage of being comparable across countries.

To assess the magnitude of these interaction effects, it is useful to compare the impact of age, cognitive ability, gender and immigrant status on permanent employment in a country with a low level of employment protection like the United States and one with a high level of protection such as Italy. The difference in the OECD's employment protection index between these two countries is 2.6. Table 4 shows the impact of changing employment protection by this extent on age, gender, cognitive ability and nativity-based gaps in permanent employment, using the logit estimates with country dummies and industry and occupation controls from Table 3. In addition, Table 4 shows the actual incidence of permanent employment across these dimensions for Italy and the United States. In order to gauge the importance of employment protection, one

[13] Inclusion of country dummies implies of course that the main effect of employment protection can no longer be included.

can compare the effect of the Italian-US difference in employment protection on these gaps in permanent employment with the actual Italian-US difference in the permanent employment gaps.

Beginning with the effect of age, Table 4 shows that among those with jobs, only 59.5% of 16-25 year olds in Italy have permanent jobs, compared to 81.1% in the US. Among the more prime age 46-55 year old group, the difference in permanent employment incidence is much smaller: 94.3% of this group in Italy have a permanent job, while 96.2% of employed 46-55 year olds in the US have one. Thus, the actual age gap in permanent employment in Italy is fully 34.8 percentage points, compared to only 15.2 percentage points in the US, for an Italy-US difference of 19.6 percentage points. Table 3's logit estimate for the model with country dummies and industry-occupation controls implies that raising the employment protection mandate from the US to the Italian level raises the permanent employment gap between 46-55 year olds and 16-25 year olds by 11.3 percentage points, a highly significant effect with an asymptotic standard error of 2.9 percentage points. Table 4 shows that this point estimate is fully 57% of the actual Italy-US difference in the permanent employment gap between these two age groups. The other logit models yield predicted changes in this gap of 7.6 to 10.4 percentage points, and the OLS results are uniformly larger than any of the logit results. Using any of these parameter estimates, one can conclude that employment protection is an important cause of the fact that young people in Italy have a much lower relative incidence of permanent employment than young people in the US.

Table 4 shows similar results for the degree to which employment protection explains Italy-US differences in the gender gap, cognitive ability gap, and immigrant-native gap in the incidence of permanent employment. Specifically, men in each country have a higher incidence of permanent employment than women do, and the gender gap is 7.2 percentage points higher in Italy. Changing employment protection mandates from the US to the Italian level raises the gender gap in permanent employment by 2.6 percentage points, again a highly significant effect that is more than nine times its asymptotic standard error. The impact accounts for 36% of the actual Italian-US difference in the gender gap using the fully specified logit model in Table 3.

All of the other models in Table 3 show larger effects than this. Table 4 shows that in Italy, those with low cognitive ability are less likely than others to have a permanent job, while in the US, they are actually slightly more likely. The skill gap in permanent employment is 5.5 percentage points higher in Italy than in the US, and the employment protection effect is 81% of this, using the last logit model in Table 3, although in this case the effect is not statistically significant. Again, the other models imply larger effects than this, some of which are statistically significant. Finally, natives are 8.8 percentage points more likely in Italy and 0.3 percentage points less likely in the US than immigrants to have permanent jobs, for a 9.2 percentage point Italy-US difference in the native-immigrant gap (with rounding). Using the last logit model in Table 3, I conclude that protection explains 63% of this difference, an effect that is twice its asymptotic standard error. The other parameter estimates in Table 3 imply a range for this estimate of 42% to 71%.

V. Alternative Specifications

The results in Tables 3 and 4 imply that employment protection of regular jobs disproportionately raises the likelihood that employed younger, female, immigrant and less skilled workers will occupy temporary jobs. In this section, I explore some more detailed specifications of the basic model in order to examine the roles of collective bargaining, gender, temporary employment protection, and possible sample selection bias. Moreover, I present results where the protection measure is disaggregated into its components as well as exploring the sensitivity of the results to exclusion of countries with very high or very low levels of employment protection or exclusion of young people.

A. Collective Bargaining Interactions

As discussed earlier, if there are wage floors, then Lazear's (1990) analysis predicts that employment protection mandates will have even larger effects than otherwise in shutting out low skill workers from permanent employment. I tested this notion by adding a series of three way interactions between collective bargaining coverage, employment protection and the demographic and skill variables in the model. In addition, I added lower level interactions between collective bargaining coverage and the demographic/skill variables as well as a main collective bargaining coverage effect and a collective bargaining coverage-employment protection interaction. Table 5 shows logit results of these tests.[14] The three way interaction effects are very strong for age and nativity status. Specifically, more stringent employment protection on regular jobs raises the age gap and the immigrant-native gap in permanent employment substantially more when collective bargaining coverage is high than when it is low, and these three way interactions are highly statistically significant in all specifications. For example, using the difference between Italian and US collective bargaining coverage of 0.64 (82% vs. 18%) and using the most fully specified model in Table 5, an increase in employment protection from the US to the Italian level widens the age 46-55 vs. age 16-25 gap in permanent employment by 31.1 percentage points more with the higher collective bargaining level. The native-immigrant permanent employment gap is widened by 22.5 percentage points more in the high collective bargaining coverage than in the low collective bargaining environment.

In addition, the three way interactions with female are all negative and significant two out of four times, suggesting that protection raises the gender gap in permanent jobs more in highly unionized than in less highly unionized countries, although the effects are much smaller than for age or nativity status. Finally, the three way interactions involving education and cognitive ability go in opposite directions. On the one hand, the positive three way interactions with education imply that protection widens the highly educated-less highly educated permanent employment gap more where there is extensive collective bargaining, as the wage floor argument

[14] OLS results for these and the other specifications were largely similar and are available upon request.

would suggest; on the other hand, I also obtain positive interactions with low test scores, implying the opposite.

Overall, then, I find that collective bargaining coverage accentuates the employment protection effects that reduce the incidence of permanent jobs for the young, immigrants, and women. These findings can be seen as complementary to earlier work that finds that higher collective bargaining coverage leads to lower employment levels for women and youth (Bertola, Blau and Kahn 2002).

B. Gender Interactions

The basic model in Table 3 assumes that employment protection has the same effect on women's relative incidence of permanent employment (i.e., versus comparable men), regardless of their age, cognitive ability, education, or nativity status. However, since women are more likely than men to be recent labor market entrants, as well as constrained by wage floors, one might expect these gender effects of employment protection to be stronger in the lower wage or lower skill groups. Indeed, Table 3 shows that, overall, employment protection lowers women's relative likelihood of permanent employment. Table 6 shows logit models where I allow the effects of employment protection by age, education, cognitive ability, and nativity to vary by gender. The three way interactions involving gender, employment protection and the other demographic or skill variables are all insignificant and small in magnitude except for a significant, negative interaction with nativity status. Looking at the effect of protection on immigrant men and women, we see in Table 6 that protection has small, positive, sometimes significant effects on the permanent employment gap for male natives vs. immigrants, but the three way interaction with female is significantly negative. Moreover, the effect of protection on the female native-immigrant permanent employment gap (i.e. adding the protection-immigrant two way interaction term and the three way protection-female-immigrant term) is large in magnitude, ranging from -0.047 to -0.059 and is always statistically significant at better than the

4.6% level. When I calculated the average effect of protection on the gap for native-born men vs. native-born women (at the mean values for the age dummies, education, and test score), I continued to find that stricter protection raises this gap; this effect was of the same magnitude as the female interaction effects in Table 3. Moreover, this difference was usually statistically significant. Thus, protection reduces the chances that both native and immigrant women will obtain permanent employment, relative to native men and immigrant men, respectively, with a larger effect for immigrants.

The findings in Table 6 suggest that employment protection reduces the incidence of permanent jobs for employed immigrant women, but does not do so for immigrant men. Perhaps immigrant women have especially low skill levels or low levels of labor market experience. In either case, it is not surprising that protection would reduce their likelihood of being able to obtain a permanent job.[15]

C. Temporary Employment Regulation

The theory outlined earlier suggests that greater protection of temporary employment should have the opposite effects of regular employment protection on employed workers' propensities to be in permanent jobs. While countries differ with respect to their regulation of temporary employment, as noted earlier the OECD's (1999) measures of such regulation are highly correlated with permanent employment protection mandates, with a correlation coefficient of 0.74. Table 7 shows what happens when I add the temporary employment index and its interactions with age, education, cognitive ability, gender, and immigrant status to the basic

[15] I also investigated whether the collective bargaining-protection interaction for immigrants shown in Table 5 was significantly different for male vs. female immigrants. In supplementary collective bargaining-interaction models, I added a three way gender-protection-immigrant, a three way gender-collective bargaining-immigrant, and a four way collective bargaining-gender-protection-immigrant interaction term. The model was therefore saturated. In all cases, both the three way collective bargaining-protection-immigrant and the four way collective bargaining-gender-protection-immigrant interaction effects were negative; however, while the three way interaction was sometimes significant, the four way interaction was never significant (the joint hypothesis that both interactions were zero was always rejected). Thus, the collective bargaining-protection interaction was not significantly different for male and female immigrants.

model in Table 3. There are rarely any significant effects of temporary employment protection. These occur only in the age 46-55 interactions for three of the four models shown in Table 7, and they go in the wrong direction of raising the relative likelihood that people in this age group will have a permanent job. Moreover, the basic regular employment protection interaction effects hold up in sign but are less statistically significant than in Table 3. Only the negative interactions with female and immigrants hold up in statistical significance. And when I estimated the basic Table 3 models with the permanent employment protection terms replaced by temporary employment regulation, the results were virtually identical to those in Table 3. These findings and those in Table 7 reinforce Booth, Dolado and Frank's (2002) conclusion that the OECD's index of temporary employment protection does not add any information beyond what is contained in its index of permanent employment protection.

D. Sample Selection Bias

As discussed earlier, the differing employment to population ratios across the countries in my sample raise the possibility that my basic models interacting employment protection and demographic groups may be influenced by sample selection bias. Appendix Table A1, for example, shows that among those who were not self-employed, employment to population ratios were highest for Switzerland among men and the US among women. One method to adjust for sample selection is to build a two equation model of employment and permanent employment along the lines suggested by Heckman (1979). However, the IALS does not contain suitable instruments to credibly identify such a system. Instead, I use a technique that is based on a method devised by Hunt (2002) and also implemented by Blau and Kahn (2005).

To understand this adjustment, consider the samples of men in Table A1. Their employment-population ratios (where the self-employed are not included in the sample) range from 0.581 in Finland to 0.795 in Switzerland. To create a sample of comparably-selected men in each county, I first estimate logits for men's probability of employment separately by country.

The explanatory variables include the age dummies, education and the low test score dummy. For each country with a higher male employment to population ratio than Finland's, among those who are employed, I then drop from the sample those with the lowest predicted probabilities of employment, leaving a sample equal to 58.1% of the population (i.e., Finland's male employment-population ratio).[16] I perform a similar analysis for women, where Table A1 shows that Italy is the base country with the lowest female employment to population ratio. This procedure yields male and female samples with the same relative likelihood of employment and imposes no a priori assumptions about the market or nonmarket productivity of nonparticipants vs. participants.

Table 8 shows the results for my basic specification, where the sample has been adjusted as described above. The results are qualitatively similar to those in Table 3. First, more stringent employment protection raises the age gap in permanent employment for 36-45 and 46-55 year olds relative to 26-35 year olds, although the interaction effects are negative for 26-35 year olds vs. 16-25 year olds and are smaller for 56-65 year olds than for those 36-55 years old. Second, protection disproportionately reduces the permanent employment of those with low cognitive ability, with consistently negative effects that are significant two of four times. Third, protection continues to disproportionately reduce the permanent employment of women, effects that are always highly statistically significant. Finally, the protection effects on immigrants are also negative relative to natives, although the coefficients are not significant. But overall, the pattern of results is very similar to those which did not correct for selection.[17]

E. Disaggregating the Components of the OECD Protection Index

[16] To illustrate this process, consider Switzerland, in which 79.5% of the men had jobs, according to Table A1. From the Swiss sample of men with jobs, I eliminate the lowest 27% (i.e. [(0.795-0.581)/(0.795)]) of individuals with respect to their estimated probability of employment. I perform an analogous adjustment for the other countries.

[17] When I included the self-employed in the sample and repeated the correction for selection, the results were very similar.

The results presented so far are based on the OECD's index of employment protection, which is treated as a continuous variable. Not only does this imply a cardinality to the index itself; it also necessarily imposes the OECD's implicit weights from the components of the index. That is, based on the components, the OECD decides on the overall index value. In this section, I present results from basic models where the components have been disaggegated. This design allows us to determine which policy (if any) is most responsible for the basic findings in Table 3. Moreover, for most of the components, the key policy variable is defined as an actual number of years of benefit or mandatory notice entitlement, allowing for a more natural interpretation of a one unit change than would a variable defined as an index.

Table A2 shows the results of this disaggregation. Specifically, I estimate a separate model for each component, in light of the correlation among the components (which prevents their simultaneous inclusion). These include i) years of mandated severance pay for a worker dismissed after 20 years; ii) years of mandated compensation in the event of unfair dismissal; iii) years of mandatory notice required for someone laid off with 20 years' seniority; and iv) the OECD's index of procedural inconvenience for firms that wish to dismiss workers. The Table shows models including country dummy variables with occupation, industry and their interactions with gender excluded (Panel A) or included (Panel B). Interaction effects between each policy and the key demographic and skill variables are shown. In each case the effects are similar to the aggregated results shown earlier. More generous severance pay or unfair dismissal compensation, longer mandatory notice, and more procedural inconvenience each have positive interaction effects with age and negative interaction effects for those with low test scores, women and immigrants in models estimating the probability of having a permanent job. While the statistical significance of these interaction effects varies, the overall pattern confirms the results based on the OECD's overall index. In particular, the interactions for prime age vs. youth, female vs. male, and immigrant vs. native are usually statistically significant.

F. Results Excluding Countries with High or Low Employment Protection Levels

Much has been written about the extensive set of employment protection regulations in Italy (see, for example, Nicoletti 2002). With my relatively small sample of countries, it is possible that the results presented so far reflect Italy-other country differences in the demographic and skill patterns of permanent employment rather than the impact of employment protection. I have therefore estimated the basic models with Italy excluded, and the results are shown in Table A3. The findings are quite similar to those in Table 3. In addition, as Table 1 shows, the Netherlands also has a high OECD overall index rating for employment protection which is actually slightly higher than Italy's. The basic results were similar when the Netherlands was excluded and when both the Netherlands and Italy were excluded from the analysis. Finally, the essential patterns remained when the United States, the country with the weakest set of employment protection mandates, was excluded. These alternative analyses excluding key countries imply that the effects I have found in this paper are more general than merely country-specific effects.

G. Results Excluding Those Age 16-25

As noted earlier, the quality and availability of schooling opportunities can affect the relative incidence of temporary employment of employed young people. For example, if one is planning to go to school, one may be much more willing than otherwise to take a temporary job. If employment protection laws are correlated with schooling opportunities, then even with country dummies, the positive interaction effects found above for employment protection-age interactions may reflect schooling opportunities. Therefore, to take account of this possibility, I have re-estimated the basic models by excluding those age 16-25. In this way, I focus on a group (those age 26-65) whose choice of permanent or temporary jobs is relatively unaffected by schooling opportunities. Table A4 shows the results of this analysis, and they are very similar to those for the full sample (Table 3). In particular, the age-employment protection effects are all positive and significant (relative to the omitted group, which is 26-35 year olds) and increase with age. And the interaction effects of protection with low test score, female, and immigrant dummy variables remain negative in every case, and are significant (at the 10% level on two tailed tests) at least half of the time. Thus, the basic results hold up for a sample which is largely beyond the school-attendance years.[18]

VI. Conclusions

In this paper, I have estimated the impact of employment protection legislation on the incidence of permanent employment. I argued on theoretical grounds that not only should protection lower the incidence of permanent jobs, but that this effect should be strongest for the young, women, immigrants, and the less skilled. I tested these predictions using 1994-98 IALS data on Canada, Finland, Italy, the Netherlands, Switzerland, the United Kingdom, and the United States, countries with widely varying degrees of employment protection. Across a variety of specifications, I indeed found that greater protection disproportionately lowered the probability that employed young, female, and immigrant workers, as well as those with low cognitive ability, had permanent jobs. Upon closer examination, the negative immigrant effects were concentrated on women. Moreover, greater coverage by collective bargaining, with its wage floors, accentuated the effects of employment protection in reducing the incidence of permanent employment for young people, women and immigrants. And the basic results held up when I adjusted for the possible sample selection bias induced by using only employed workers, when I disaggregated the OECD's employment protection index into its components, when I excluded countries with extreme levels of protection such as Italy, the Netherlands or the United States, and when I excluded those of school attendance age (16-25 years old).

[18] In particular, only about 1% of those age 26-65 in the IALS reported school attendance as their major activity, compared to 36% of 16-25 year olds.

My findings are complementary with earlier research which finds that the high wage floors associated with high levels of centralized collective bargaining lead to lower relative employment or higher relative unemployment of young people and women (Kahn 2000; Bertola, Blau and Kahn 2002). Institutions such as collective bargaining and systems of employment protection together have the effect of protecting the permanent jobs of prime age men, at the expense of a possibly large set of outsiders who spend considerable time out of work or shifting among temporary jobs.

References

Autor, David H., "Outsourcing at Will: The Contribution of Unjust Dismissal Doctrine to the Growth of Employment Outsourcing," *Journal of Labor Economics* 12, no. 1 (January 2003): 1-42.

Autor, David H., John J. Donohue III, and Stewart J. Schwab, "The Costs of Wrongful-Discharge Laws," Working Paper, September 2004.

Bertola, Giuseppe, "Job Security, Employment and Wages," *European Economic Review* 34, no. 4 (June 1990): 851-879.

Bertola, Giuseppe, "Labor Turnover Costs and Average Labor Demand," *Journal of Labor Economics* 10, no. 4 (October 1992): 389-411.

Bertola, Giuseppe, Francine D. Blau and Lawrence M. Kahn, "Labor Market Institutions and Demographic Employment Patterns," National Bureau of Economic Research Working paper 9043, July 2002.

Blanchard, Olivier and Augustin Landier, "The Perverse Effects of Partial Labour Market Reform: Fixed-Term Contracts in France," *Economic Journal* 112, no. 480 (June 2002): F214-F244.

Blanchard, Olivier J. and Justin Wolfers, "The Role of Shocks and Institutions in the Rise of European Unemployment: The Aggregate Evidence," *Economic Journal* 110, No. 462 (March 2000): C1-C33.

Blau, Francine D. and Lawrence M. Kahn, "International Differences in Male Wage Inequality: Institutions Versus Market Forces," *Journal of Political Economy* 104, No. 4 (August 1996): 791-837.

Blau, Francine D. and Lawrence M. Kahn, "Do Cognitive Test Scores Explain Higher U.S. Wage Inequality?" *The Review of Economics and Statistics* 87, no. 1 (February 2005): 184-193.

Booth, Alison L., Juan J. Dolado and Jeff Frank, "Symposium on Temporary Work Introduction," *Economic Journal* 112, no. 480 (June 2002): F181-F188.

Booth, Alison L., Marco Francesconi and Jeff Frank, "Temporary Jobs: Stepping Stones or Dead Ends," *Economic Journal* 112, no. 480 (June 2002): F189-F213.

Cahuc, Pierre and Postel-Vinay, Fabien, "Temporary Jobs, Employment Protection and Labor Market Performance," *Labour Economics* 9, no. 1 (February 2002): 63-91.

Dolado, Juan J., Carlos Garcia-Serano and Juan F. Jimeno, "Drawing Lessons from the Boom of Temporary Jobs in Spain," *Economic Journal* 112, no. 480 (June 2002): F270-F295.

Güell, Maia, "Fixed-term Contracts and Unemployment: an Efficiency Wage Analysis," Working paper, Universitat Pompeu Fabra, July 2003.

Heckman, James J., "Sample Selection Bias as a Specification Error," *Econometrica* 47, no. 1 (January 1979): 153-162.

Holmlund, Bertil and Donald Storrie, “Temporary Work in Turbulent Times: The Swedish Experience,” *Economic Journal* 112, no. 480 (June 2002): F245-F269.

Hunt, Jennifer, "The Transition in East Germany: When is a Ten Point Fall in the Gender Wage Gap Bad News?" *Journal of Labor Economics* 20, no. 1 (January 2002): 148-169.

Kahn, Lawrence M., “Wage Inequality, Collective Bargaining and Relative Employment from 1985 to 1994: Evidence from Fifteen OECD Countries,” *The Review of Economics and Statistics* 82, no. 4 (November 2000): 564-579.

Lazear, Edward P., “Job Security Provisions and Employment,” *Quarterly Journal of Economics* 105, no. 3 (August 1990): 699-726.

Nickell, Stephen and Richard Layard, “Labor Market Institutions and Economic Performance,” in O. Ashenfelter and D. Card (eds.), *Handbook of Labor Economics* Volume 3C (Amsterdam: North-Holland, 1999), pp. 3029-3084.

Nickell, Stephen, Luca Nunziata and Wolfgang Ochel, “Unemployment in the OECD Since the 1960s. What Do We Know?” *Economic Journal* 115, no. 500 (January 2005): 1-27.

Nicoletti, Giuseppe, “Institutions, Economic Structure, and Performance: Is Italy Doomed?” in Institute for Studies and Economic Analyses (ISAE), in *ISAE Annual Report on Monitoring Italy*, Chapter 3 (Rome: ISAE, 2002), pp. 1290189.

OECD, *Employment Outlook: July 1997* (Paris: OECD, 1997).

OECD, *Human Capital Investment: An International Comparison* (Paris: OECD, 1998).

OECD, *Employment Outlook: June 1999* (Paris: OECD, 1999).

Rubin Donald B., *Multiple Imputation for Nonresponse in Surveys* (New York: John Wiley and Sons, 1987).

U.S. Department of Education, National Center for Education Statistics (USDOE, NCES), *Adult Literacy in OECD Countries: Technical Report on the First International Adult Literacy Survey*, NCES 98-053 (Washington, DC: NCES, 1998). 3029-3084.

Figure 1: Incidence of Permanent Employment by Strength of Permanent Employment Protection, Men and Women

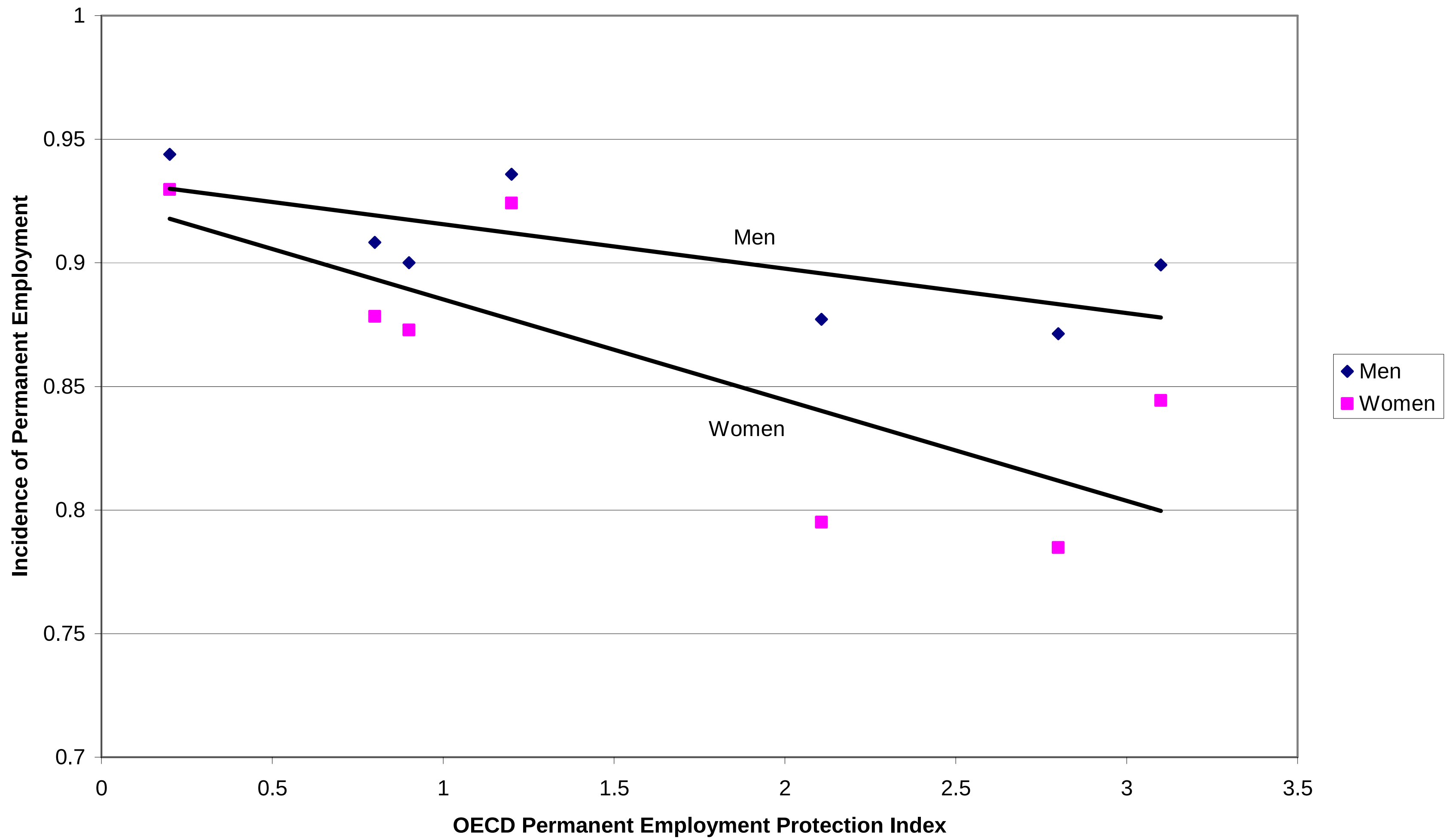

Figure 2: Incidence of Permanent Employment by Strength of Permanent Employment Protection, Age 16-25 and Age 46-55

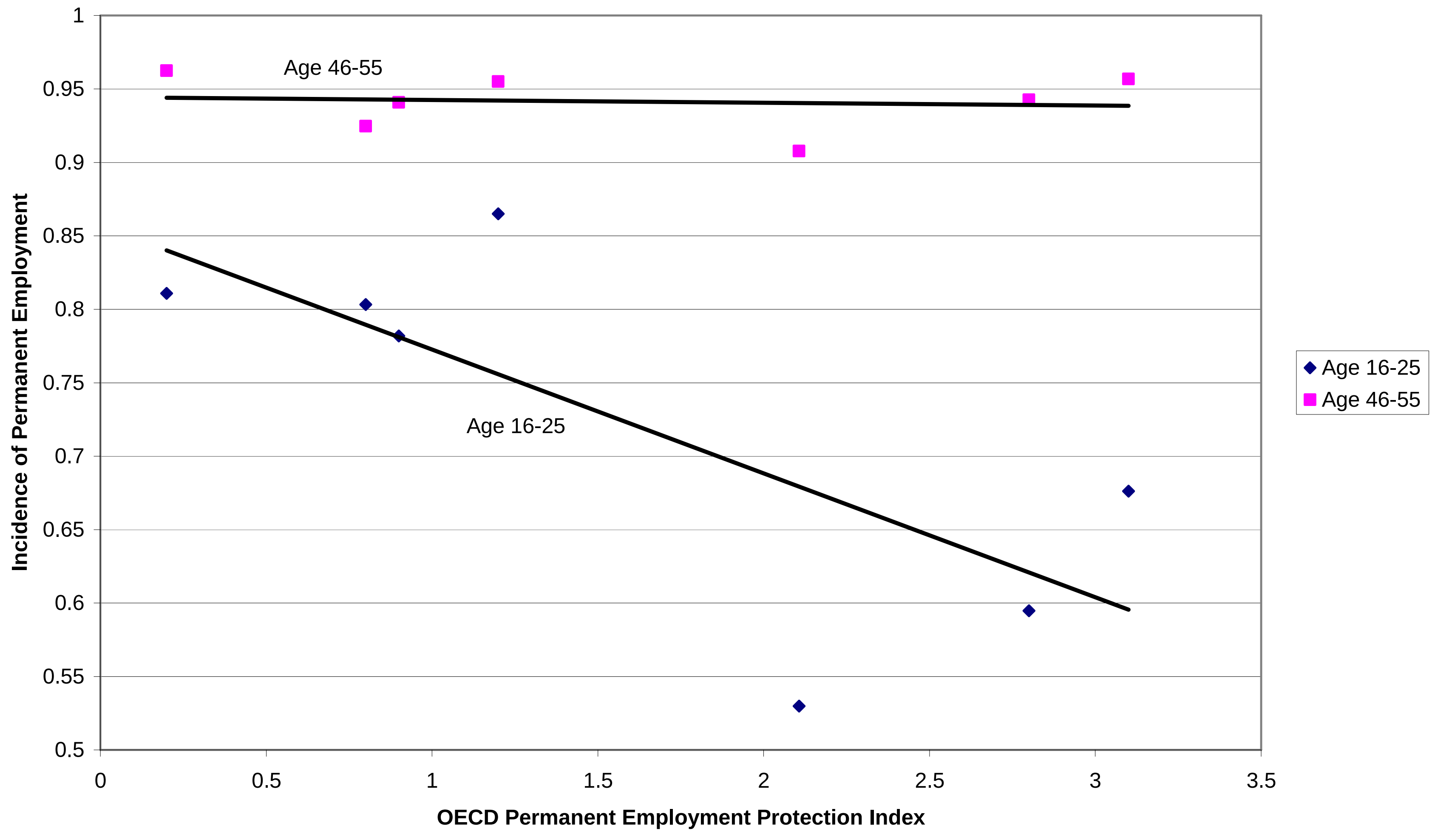

Figure 3: Incidence of Permanent Employment by Strength of Permanent Employment Protection, Natives and Immigrants

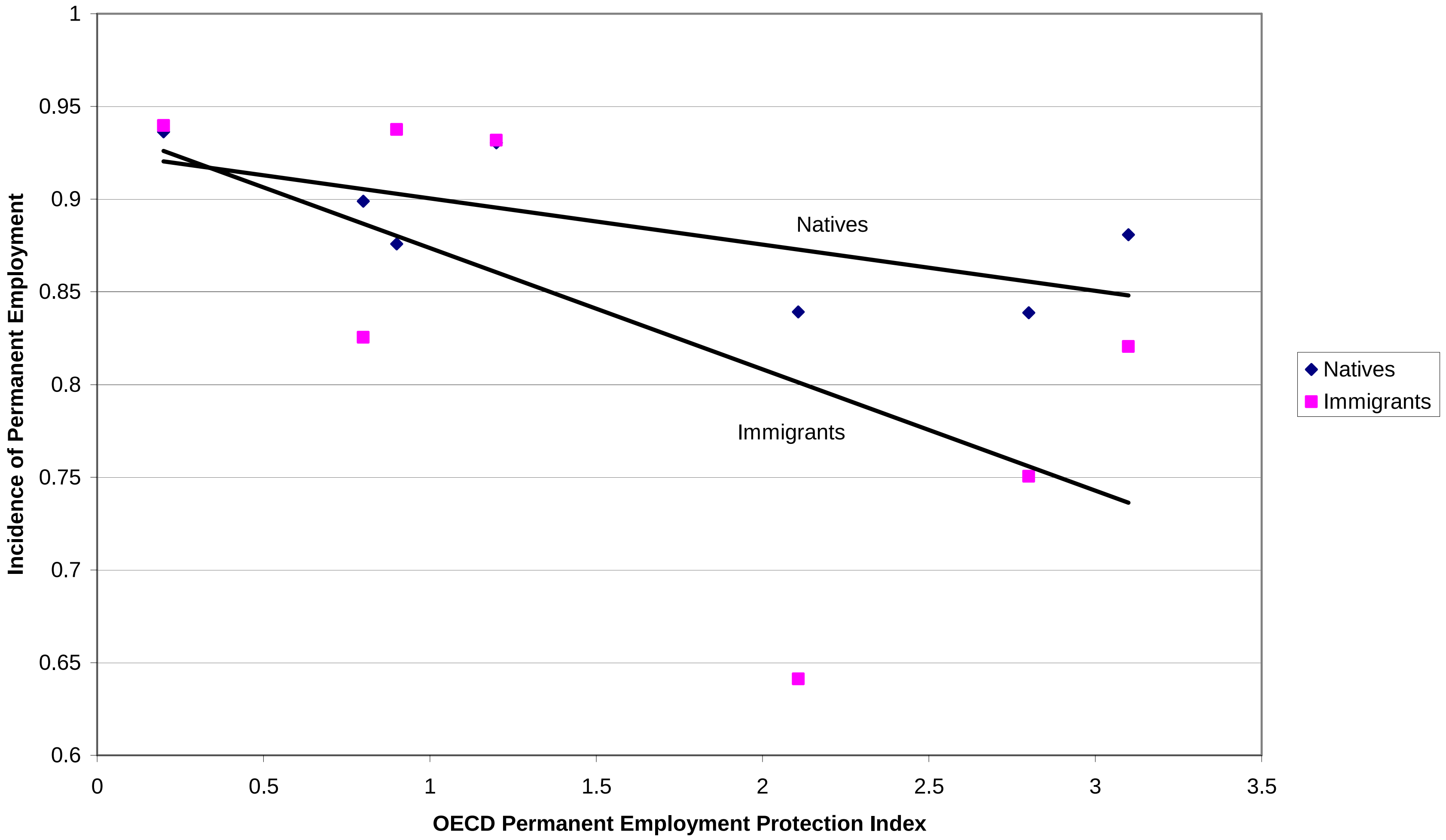

Figure 4: Incidence of Permanent Employment by Strength of Permanent Employment Protection, Individuals with Low Test Scores vs. Others

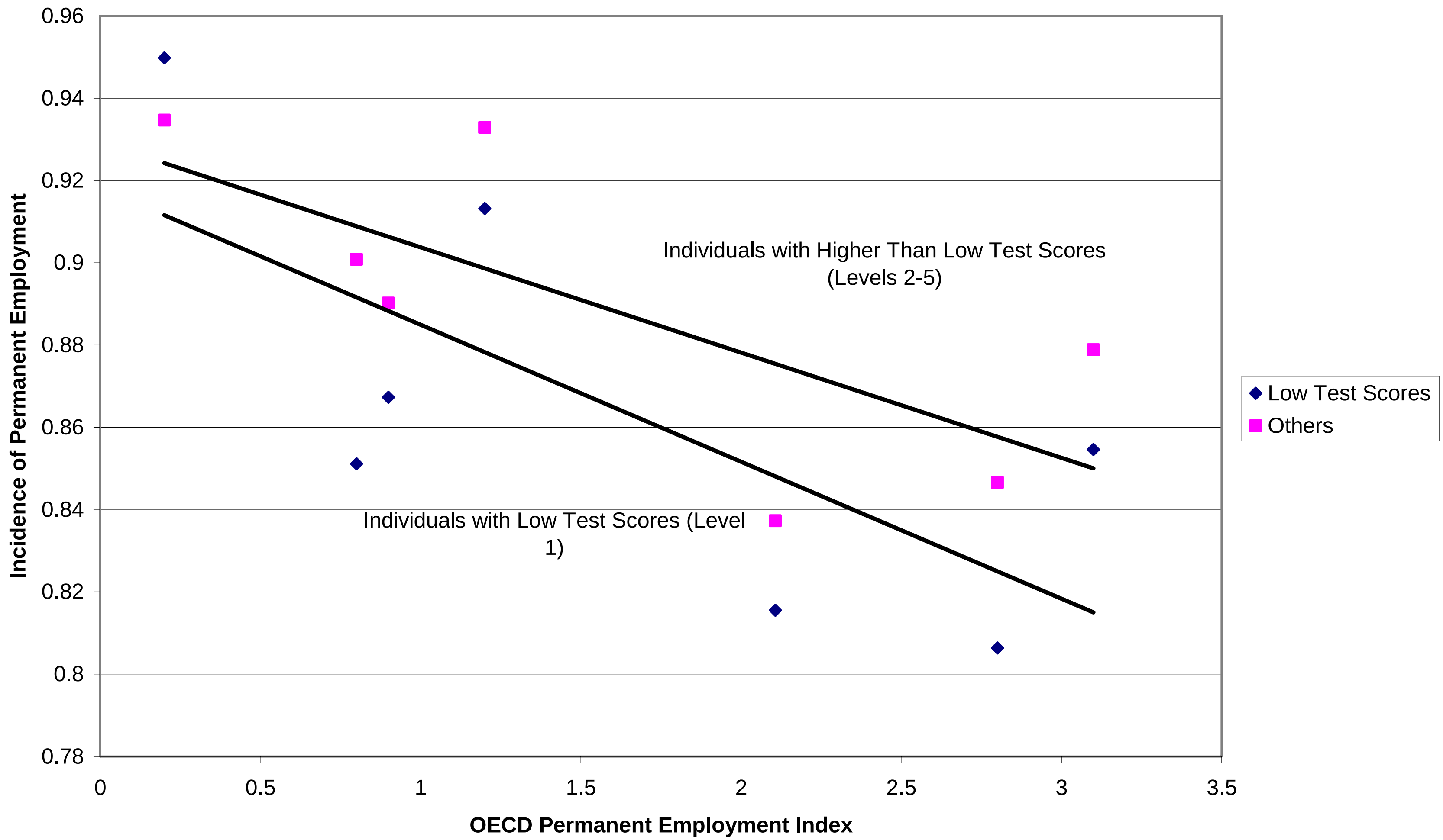

Table 1: Employment Protection Mandates for Regular Employment, Late 1990s

	Months of Severance Pay for No-Fault Dismissals by Tenure Category:			Unfair Dismissal Compensation, 20 Years Tenure (months)	Mandatory Notice for Individual Dismissals, 20 Years Tenure (months)	Index of Procedural Inconvenience (0 to 6 scale)	Overall Regular Employment Protection Score (0 to 6 scale)
	9 Months	**4 Years**	**20 Years**				
Canada	0.0	0.2	1.3	0.0	0.5	0.0	0.9
Finland	0.0	0.0	0.0	12.0	6.0	2.8	2.1
Italy	0.7	3.5	18.0	32.5	2.2	1.5	2.8
Netherlands	0.0	0.0	0.0	18.0	3.0	5.0	3.1
Switzerland	0.0	0.0	2.0	6.0	3.0	0.5	1.2
UK	0.0	0.5	2.4	8.0	2.8	1.0	0.8
USA	0.0	0.0	0.0	0.0	0.0	0.0	0.2

Source: OECD (1999), pp. 55 and 66.

Table 2: Employment Protection Mandates for Temporary Employment, Late 1990s

	Maximum Number of Fixed Term Contracts	Maximum Accumulated Duration, Fixed Term Contracts (months)	Index of Ease of Temporary Work Agency Employment (0=illegal, 4=no restrictions)	Overall Temporary Employment Protection Score (0 to 6 scale)
Canada	No limit	No limit	4.0	0.3
Finland	1.5	No limit	4.0	1.9
Italy	2.0	15.0	1.0	3.8
Netherlands	3.0	No limit	3.5	1.2
Switzerland	1.5	No limit	4.0	0.9
UK	No limit	No limit	4.0	0.3
USA	No limit	No limit	4.0	0.3

Source: OECD (1999), pp. 62 and 66.

Table 3: Selected Regression Results for the Effects of Employment Protection (EPL Index) on Permanent Employment

A. Ordinary Least Squares	coef	se	coef	se	coef	se	coef	se
EPL Index	-0.045	0.030	-0.039	0.027	---	---	---	---
EPL Index*Age 26-35	0.031	0.009	0.030	0.008	0.035	0.010	0.033	0.009
EPL Index*Age 36-45	0.058	0.017	0.058	0.017	0.063	0.018	0.063	0.018
EPL Index*Age 46-55	0.067	0.017	0.067	0.017	0.074	0.019	0.074	0.019
EPL Index*Age 56-65	0.080	0.021	0.079	0.021	0.087	0.023	0.086	0.023
EPL Index*Education	-0.001	0.002	-0.002	0.002	-0.001	0.002	-0.002	0.002
EPL Index*Low Test Score	-0.029	0.011	-0.026	0.012	-0.024	0.014	-0.022	0.014
EPL Index*Female	-0.020	0.008	-0.019	0.007	-0.016	0.007	-0.016	0.006
EPL Index*Immigrant	-0.016	0.010	-0.017	0.010	-0.025	0.009	-0.025	0.009
occup, ind ?	no		yes		no		yes	
(occup,ind)* female interactions?	no		yes		no		yes	
Country dummies?	no		no		yes		yes	
Sample size	13736		13736		13736		13736	
B. Logit (partial derivatives at mean of dependent variable)	coef	asy se	coef	asy se	coef	asy se	coef	asy se
EPL Index	-0.019	0.031	-0.014	0.026	---	---	---	---
EPL Index*Age 26-35	-0.002	0.008	-0.003	0.007	0.001	0.009	0.000	0.008
EPL Index*Age 36-45	0.021	0.015	0.023	0.013	0.029	0.017	0.032	0.016
EPL Index*Age 46-55	0.029	0.011	0.033	0.011	0.040	0.012	0.043	0.011
EPL Index*Age 56-65	0.057	0.024	0.061	0.022	0.074	0.027	0.078	0.026
EPL Index*Education	-0.001	0.002	-0.001	0.002	-0.001	0.002	-0.001	0.002
EPL Index*Low Test Score	-0.020	0.012	-0.020	0.013	-0.016	0.014	-0.017	0.015
EPL Index*Female	-0.013	0.003	-0.012	0.001	-0.011	0.002	-0.010	0.001
EPL Index*Immigrant	-0.015	0.010	-0.016	0.010	-0.022	0.012	-0.022	0.011
occup, ind ?	no		yes		no		yes	
(occup,ind)* female interactions?	no		yes		no		yes	
Country dummies?	no		no		yes		yes	
Sample size	13736		13736		13736		13736	

EPL Index is the OECD's index of strength of employment protection mandates for regular jobs.
Controls include age dummies, education, low test score dummy, immigrant dummy and a female dummy and female interactions with each of these variables. (Asymptotic) standard errors corrected for correlation within countries. Data are weighted using IALS sampling weights adjusted so that each country gets the same total weight.

Table 4: Effect of Employment Protection on US-Italian Differences in Age, Gender, Immigrant Status, and Cognitive Ability-Based Gaps in Permanent Employment Incidence

Dimension		Italy	US	Difference: Italy-US Effect	Asy std err
1. Age					
	46-55 Permanent Employment Incidence	0.943	0.962	-0.020	----
	16-25 Permanent Employment Incidence	0.595	0.811	-0.216	----
	Actual Permanent Employment Gap (46-55 minus 16-25)	0.348	0.152	0.196	----
	Effect of Changing from US to Italian Protection	----	----	0.113	0.029
	Percentage of US-Italian Difference Explained by Protection	----	----	57.4%	14.8%
2. Gender					
	Male Permanent Employment Incidence	0.871	0.944	-0.072	----
	Female Permanent Employment Incidence	0.785	0.930	-0.145	----
	Actual Permanent Employment Gap (Male minus Female)	0.087	0.014	0.072	----
	Effect of Changing from US to Italian Protection	----	----	0.026	0.003
	Percentage of US-Italian Difference Explained by Protection	----	----	35.7%	3.9%
3. Cognitive Ability					
	Permanent Employment Incidence for Higher Than Level 1 Test Score	0.847	0.935	-0.088	----
	Permanent Employment Incidence for Low Test Score (Level 1)	0.806	0.950	-0.143	----
	Actual Permanent Employment Gap (Above Level 1 minus Level 1)	0.040	-0.015	0.055	----
	Effect of Changing from US to Italian Protection	----	----	0.045	0.039
	Percentage of US-Italian Difference Explained by Protection	----	----	80.7%	70.3%
4. Nativity					
	Native Permanent Employment Incidence	0.839	0.936	-0.098	----
	Immigrant Permanent Employment Incidence	0.751	0.940	-0.189	----
	Actual Permanent Employment Gap (Native minus Immigrant)	0.088	-0.003	0.092	----
	Effect of Changing from US to Italian Protection	----	----	0.058	0.029
	Percentage of US-Italian Difference Explained by Protection	----	----	62.9%	31.4%

Note: Based on Logit model with country dummies and occupation-industry controls (last model of Table 3, Panel B).

Table 5: Selected Logit Results for the Effects of Employment Protection (EPL Index) on Permanent Employment, with Collective Bargaining Coverage (CB Cov) Interactions (partial derivatives at mean of dependent variable)

	coef	asy se	coef	asy se	coef	asy se	coef	asy se
EPL Index	0.168	0.085	0.155	0.081	---	---	---	---
CB Cov	0.731	0.145	0.674	0.142	---	---	---	---
EPL Index*CB Cov	-0.372	0.117	-0.343	0.115	---	---	---	---
EPL Index*Age 26-35	-0.096	0.018	-0.084	0.016	-0.108	0.011	-0.095	0.014
EPL Index*Age 36-45	-0.202	0.022	-0.193	0.020	-0.209	0.015	-0.199	0.015
EPL Index*Age 46-55	-0.097	0.024	-0.088	0.023	-0.103	0.018	-0.092	0.019
EPL Index*Age 56-65	-0.080	0.064	-0.067	0.059	-0.091	0.057	-0.075	0.050
EPL Index*Education	-0.005	0.003	-0.004	0.003	-0.005	0.003	-0.003	0.003
EPL Index*Low Test Score	-0.108	0.025	-0.102	0.019	-0.101	0.024	-0.098	0.020
EPL Index*Female	0.037	0.008	0.006	0.009	0.041	0.008	0.012	0.009
EPL Index*Immigrant	0.137	0.049	0.133	0.049	0.148	0.070	0.139	0.067
CB Cov*Age 26-35	-0.058	0.037	-0.060	0.032	-0.082	0.027	-0.086	0.027
CB Cov*Age 36-45	-0.134	0.047	-0.105	0.037	-0.169	0.030	-0.144	0.017
CB Cov*Age 46-55	-0.168	0.044	-0.149	0.047	-0.195	0.046	-0.180	0.053
CB Cov*Age 56-65	-0.467	0.151	-0.432	0.173	-0.506	0.115	-0.462	0.140
CB Cov*Education	-0.060	0.008	-0.056	0.007	-0.057	0.007	-0.053	0.007
CB Cov*Low Test Score	-0.338	0.049	-0.337	0.052	-0.363	0.046	-0.356	0.053
CB Cov*Female	-0.021	0.010	-0.043	0.032	-0.010	0.021	-0.032	0.038
CB Cov*Immigrant	-0.137	0.130	-0.109	0.128	-0.171	0.157	-0.140	0.147
CB Cov*EPL Index*Age 26-35	0.115	0.015	0.102	0.011	0.135	0.009	0.121	0.012
CB Cov*EPL Index*Age 36-45	0.273	0.031	0.259	0.029	0.290	0.021	0.276	0.020
CB Cov*EPL Index*Age 46-55	0.185	0.030	0.173	0.028	0.200	0.023	0.187	0.023
CB Cov*EPL Index*Age 56-65	0.302	0.060	0.285	0.063	0.326	0.063	0.302	0.062
CB Cov*EPL Index*Education	0.019	0.005	0.016	0.005	0.017	0.005	0.014	0.005
CB Cov*EPL Index*Low Test Score	0.166	0.025	0.161	0.021	0.172	0.028	0.168	0.026
CB Cov*EPL Index*Female	-0.046	0.008	-0.008	0.014	-0.051	0.010	-0.015	0.014
CB Cov*EPL Index*Immigrant	-0.128	0.048	-0.131	0.041	-0.137	0.064	-0.135	0.057
occup, ind ?	no		yes		no		yes	
(occup,ind)* female interactions?	no		yes		no		yes	
Country dummies?	no		no		yes		yes	
Sample size	13736		13736		13736		13736	

EPL Index is the OECD's index of strength of employment protection mandates for regular jobs. CB Cov is fraction covered by collective bargaining. Controls include age dummies, education, low test score dummy, immigrant dummy, and a female dummy and female interactions with each of these variables. (Asymptotic) standard errors corrected for correlation within countries. Data are weighted using IALS sampling weights adjusted so that each country gets the same total weight.

Table 6: Selected Logit Results for the Effects of Employment Protection (EPL Index) on Permanent Employment, with Female Interactions (partial derivatives at mean of dependent variable)

	coef	asy se	coef	asy se	coef	asy se	coef	asy se
EPL Index	-0.013	0.023	-0.005	0.014	---	---	---	---
EPL Index*Age 26-35	0.004	0.006	0.001	0.006	0.005	0.005	0.002	0.006
EPL Index*Age 36-45	0.026	0.022	0.031	0.020	0.033	0.025	0.038	0.022
EPL Index*Age 46-55	0.040	0.013	0.045	0.011	0.048	0.014	0.053	0.011
EPL Index*Age 56-65	0.068	0.022	0.066	0.022	0.080	0.024	0.077	0.024
EPL Index*Education	-0.002	0.002	-0.002	0.001	-0.002	0.002	-0.003	0.001
EPL Index*Low Test Score	-0.028	0.008	-0.028	0.010	-0.022	0.007	-0.023	0.009
EPL Index*Female	-0.024	0.052	-0.031	0.049	-0.028	0.056	-0.033	0.052
EPL Index*Immigrant	0.014	0.007	0.016	0.009	0.008	0.006	0.010	0.008
Female*EPL Index*Age 26-35	-0.011	0.019	-0.007	0.018	-0.008	0.021	-0.004	0.020
Female*EPL Index*Age 36-45	-0.010	0.024	-0.013	0.024	-0.007	0.028	-0.011	0.027
Female*EPL Index*Age 46-55	-0.019	0.025	-0.022	0.023	-0.013	0.029	-0.018	0.026
Female*EPL Index*Age 56-65	-0.019	0.032	-0.008	0.035	-0.010	0.040	0.002	0.043
Female*EPL Index*Education	0.002	0.003	0.002	0.003	0.002	0.003	0.002	0.003
Female*EPL Index*Low Test Score	0.014	0.032	0.014	0.037	0.011	0.034	0.011	0.039
Female*EPL Index*Immigrant	-0.061	0.026	-0.067	0.029	-0.064	0.028	-0.069	0.031
occup, ind ?	no		yes		no		yes	
(occup,ind)* female interactions?	no		yes		no		yes	
Country dummies?	no		no		yes		yes	
Sample size	13736		13736		13736		13736	

EPL Index is the OECD's index of strength of employment protection mandates for regular jobs.
Controls include age dummies, education, low test score dummy, immigrant dummy, and a female dummy and female interactions with each of these variables. (Asymptotic) standard errors corrected for correlation within countries. Data are weighted using IALS sampling weights adjusted so that each country gets the same total weight.

Table 7: Selected Logit Results for the Effects of Regular (EPL Index) and Temporary Employment Protection (Temp Index) on Permanent Employment (partial derivatives at mean of dependent variable)

	coef	asy se	coef	asy se	coef	asy se	coef	asy se
EPL Index	0.003	0.030	0.005	0.028	---	---	---	---
Temp Index	-0.021	0.021	-0.018	0.019	---	---	---	---
EPL Index*Age 26-35	0.006	0.009	0.005	0.008	0.007	0.009	0.006	0.008
EPL Index*Age 36-45	0.019	0.020	0.018	0.018	0.025	0.019	0.024	0.017
EPL Index*Age 46-55	0.021	0.015	0.021	0.014	0.028	0.012	0.027	0.011
EPL Index*Age 56-65	0.019	0.041	0.026	0.038	0.035	0.044	0.042	0.040
EPL Index*Education	-0.001	0.002	-0.001	0.002	-0.002	0.003	-0.002	0.002
EPL Index*Low Test Score	-0.025	0.022	-0.027	0.022	-0.021	0.025	-0.024	0.024
EPL Index*Female	-0.009	0.002	-0.011	0.002	-0.008	0.002	-0.009	0.002
EPL Index*Immigrant	-0.019	0.008	-0.020	0.010	-0.016	0.009	-0.017	0.010
Temp Index*Age 26-35	-0.006	0.006	-0.006	0.006	-0.006	0.005	-0.006	0.006
Temp Index*Age 36-45	0.005	0.013	0.010	0.013	0.004	0.014	0.009	0.013
Temp Index*Age 46-55	0.013	0.009	0.017	0.009	0.012	0.006	0.017	0.007
Temp Index*Age 56-65	0.050	0.035	0.048	0.036	0.044	0.033	0.042	0.034
Temp Index*Education	0.0002	0.002	-0.0003	0.002	0.001	0.002	0.0003	0.002
Temp Index*Low Test Score	0.008	0.011	0.009	0.011	0.004	0.014	0.005	0.014
Temp Index*Female	-0.004	0.003	-0.001	0.003	-0.004	0.003	-0.001	0.004
Temp Index*Immigrant	0.001	0.013	0.002	0.015	-0.008	0.017	-0.007	0.018
occup, ind ?	no		yes		no		yes	
(occup,ind)* female interactions?	no		yes		no		yes	
Country dummies?	no		no		yes		yes	
Sample size	13736		13736		13736		13736	

EPL Index is the OECD's index of strength of employment protection mandates for regular jobs.
Temp Index is the OECD's index of strength of employment protection mandates for temporary jobs.
Controls include age dummies, education, low test score dummy, immigrant dummy, and a female dummy and female interactions with each of these variables. (Asymptotic) standard errors corrected for correlation within countries. Data are weighted using IALS sampling weights adjusted so that each country gets the same total weight.

Table 8: Selected Logit Results for the Effects of Employment Protection (EPL Index) on Permanent Employment with Adjustment for Selection into Employment (partial derivatives at mean of dependent variable)

	coef	se	coef	se	coef	se	coef	se
EPL Index	-0.015	0.028	-0.015	0.028	---	---	---	---
EPL Index*Age 26-35	-0.011	0.005	-0.011	0.005	-0.007	0.006	-0.006	0.007
EPL Index*Age 36-45	0.010	0.006	0.010	0.006	0.020	0.004	0.025	0.004
EPL Index*Age 46-55	0.020	0.005	0.020	0.005	0.031	0.009	0.037	0.008
EPL Index*Age 56-65	0.005	0.032	0.005	0.032	0.020	0.030	0.011	0.030
EPL Index*Education	-0.0002	0.002	0.000	0.002	-0.0003	0.002	-0.001	0.002
EPL Index*Low Test Score	-0.017	0.008	-0.017	0.008	-0.009	0.009	-0.009	0.009
EPL Index*Female	-0.013	0.003	-0.013	0.003	-0.012	0.002	-0.012	0.004
EPL Index*Immigrant	-0.006	0.007	-0.006	0.007	-0.009	0.009	-0.008	0.009
occup, ind ?	no		yes		no		yes	
(occup,ind)* female interactions?	no		yes		no		yes	
Country dummies?	no		no		yes		yes	
Sample size	11395		11395		11395		11395	

EPL Index is the OECD's index of strength of employment protection mandates for regular jobs.

Controls include age dummies, education, low test score dummy, immigrant dummy and a female dummy and female interactions with each of these variables. (Asymptotic) standard errors corrected for correlation within countries. Data are
weighted using IALS sampling weights adjusted so that each country gets the same total weight. For description of selectivity adjustment, see text.

Table A1: Employment to Population Ratios by Gender

	Men	Women
Canada	0.696	0.531
Finland	0.581	0.560
Italy	0.593	0.335
Netherlands	0.705	0.443
Switzerland	0.795	0.569
UK	0.691	0.592
USA	0.791	0.648

Source: IALS. Sample excludes the self-employed and those
with missing values for any explanatory or dependent variable. Data are weighted
using IALS sampling weights adjusted so that each country gets the same total weight.

Table A2: Selected Logit Results for Demographic or Skill Interactions with Individual Components of the Permanent Protection Index Permanent Employment (partial derivatives at mean of dep var)

Interactions of Individual Protection Component Variable and the Indicated Personal Characteristic	**Individual Protection Component Variable**							
	Severance Pay after 20 yrs		Compensation, Unfair Dismissal		Mandatory Notice		Procedural Inconvenience Index	
	coef	ase	coef	ase	coef	ase	coef	ase
A. Occup, Industry, and their interactions with female excluded								
Age26-35	-0.020	0.010	-0.002	0.009	0.049	0.067	0.005	0.003
Age36-45	-0.005	0.024	0.025	0.020	0.185	0.107	0.022	0.009
Age 46-55	0.030	0.022	0.038	0.009	0.115	0.055	0.020	0.005
Age 56-65	0.054	0.049	0.071	0.023	0.205	0.129	0.041	0.010
Education	0.003	0.002	-0.00004	0.002	-0.033	0.004	-0.001	0.001
Low Test Score	-0.001	0.015	-0.010	0.014	-0.197	0.092	-0.008	0.010
Female	-0.008	0.006	-0.011	0.003	-0.068	0.015	-0.007	0.002
Immigrant	-0.023	0.022	-0.041	0.022	-0.271	0.105	-0.018	0.008
B. Occup, Industry, and their interactions with female included								
Age26-35	-0.017	0.009	-0.002	0.007	0.033	0.055	0.005	0.003
Age36-45	0.003	0.023	0.029	0.017	0.193	0.100	0.023	0.010
Age 46-55	0.040	0.022	0.044	0.007	0.119	0.051	0.021	0.006
Age 56-65	0.048	0.047	0.072	0.022	0.224	0.115	0.043	0.009
Education	0.002	0.002	-0.001	0.002	-0.032	0.003	-0.001	0.001
Low Test Score	-0.002	0.017	-0.012	0.015	-0.199	0.096	-0.009	0.010
Female	-0.005	0.009	-0.008	0.004	-0.053	0.017	-0.005	0.001
Immigrant	-0.023	0.021	-0.041	0.021	-0.245	0.107	-0.018	0.008
Sample size	13736		13736		13736		13736	

Entries based on a separate regression for each component indicator. Severance Pay and Unfair Dismissal pay refer to years of salary entitlement for a worker with 20 years of seniority. Mandatory Notice refers to years of notice required for someone with 20 years of seniority. Procedural Inconvenience Index is the OECD's index of procedural inconvenience, which has a range of 1-6.
Controls include age dummies, education, low test score dummy, immigrant dummy and a female dummy, female interactions with each of these variables (except the female dummy), and country dummies. Asymptotic standard errors corrected for correlation within countries.
Data are weighted using IALS sampling weights adjusted so that each country gets the same total weight.

Table A3: Selected Logit Results for the Effects of Employment Protection (EPL Index) on Permanent Employment, Italy Excluded (derivatives at mean of dependent variable)

	coef	asy se	coef	asy se	coef	asy se	coef	asy se
EPL Index	-0.0020	0.0026	-0.0018	0.0025	---	---	---	---
EPL Index*Age 26-35	0.0031	0.0009	0.0029	0.0007	0.0034	0.0011	0.0032	0.0009
EPL Index*Age 36-45	0.0050	0.0017	0.0050	0.0017	0.0056	0.0020	0.0056	0.0020
EPL Index*Age 46-55	0.0052	0.0013	0.0052	0.0012	0.0061	0.0018	0.0060	0.0018
EPL Index*Age 56-65	0.0063	0.0018	0.0064	0.0019	0.0071	0.0022	0.0071	0.0023
EPL Index*Education	-0.0002	0.0002	-0.0002	0.0002	-0.0002	0.0002	-0.0002	0.0002
EPL Index*Low Test Score	-0.0026	0.0018	-0.0024	0.0017	-0.0025	0.0019	-0.0024	0.0018
EPL Index*Female	-0.0018	0.0010	-0.0017	0.0009	-0.0014	0.0008	-0.0013	0.0006
EPL Index*Immigrant	-0.0017	0.0010	-0.0018	0.0011	-0.0023	0.0010	-0.0024	0.0010
occup, ind ?	no		yes		no		yes	
(occup,ind)* female interactions?	no		yes		no		yes	
Country dummies?	no		no		yes		yes	
Sample size	12446		12446		12446		12446	

EPL Index is the OECD's index of strength of employment protection mandates for regular jobs.
Controls include age dummies, education, low test score dummy, immigrant dummy and a female dummy and female interactions with each of these variables. (Asymptotic) standard errors corrected for correlation within countries. Data are weighted using IALS sampling weights adjusted so that each country gets the same total weight.

Table A4: Selected Logit Results for the Effects of Employment Protection (EPL Index) on Permanent Employment, Excluding Those Age 16-25 (derivatives at mean of dependent variable)

	coef	asy se	coef	asy se	coef	asy se	coef	asy se
EPL Index	-0.0101	0.0250	-0.0092	0.0207	---	---	---	---
EPL Index*Age 36-45	0.0183	0.0066	0.0209	0.0064	0.0235	0.0072	0.0260	0.0070
EPL Index*Age 46-55	0.0247	0.0085	0.0286	0.0096	0.0321	0.0073	0.0362	0.0087
EPL Index*Age 56-65	0.0471	0.0158	0.0520	0.0144	0.0638	0.0153	0.0676	0.0140
EPL Index*Education	-0.0009	0.0015	-0.0010	0.0013	-0.0007	0.0016	-0.0009	0.0014
EPL Index*Low Test Score	-0.0179	0.0079	-0.0169	0.0081	-0.0133	0.0095	-0.0134	0.0096
EPL Index*Female	-0.0129	0.0036	-0.0111	0.0047	-0.0105	0.0039	-0.0082	0.0054
EPL Index*Immigrant	-0.0074	0.0056	-0.0106	0.0062	-0.0125	0.0087	-0.0157	0.0089
occup, ind ?	no		yes		no		yes	
(occup,ind)* female interactions?	no		yes		no		yes	
Country dummies?	no		no		yes		yes	
Sample size	11614		11614		11614		11614	

EPL Index is the OECD's index of strength of employment protection mandates for regular jobs.
Controls include age dummies, education, low test score dummy, immigrant dummy and a female dummy and female interactions with each of these variables. (Asymptotic) standard errors corrected for correlation within countries. Data are weighted using IALS sampling weights adjusted so that each country gets the same total weight.