The Impact of Employment Protection Mandates on Demographic Temporary Employment Patterns: International Microeconomic Evidence
Author(s): Lawrence M. Kahn
Source: *The Economic Journal*, Vol. 117, No. 521, Features (Jun., 2007), pp. F333-F356
Published by: Wiley on behalf of the Royal Economic Society
Stable URL: http://www.jstor.org/stable/4625527
Accessed: 15-05-2015 09:29 UTC

The Economic Journal, **117** (*June*), F333–F356. © The Author(s). Journal compilation © Royal Economic Society 2007. Published by Blackwell Publishing, 9600 Garsington Road, Oxford OX4 2DQ, UK and 350 Main Street, Malden, MA 02148, USA.

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

Lawrence M. Kahn

This article uses 1994–8 International Adult Literacy Survey microdata for Canada, Finland, Italy, the Netherlands, Switzerland, the UK and the US to study the impact of employment protection laws (EPL) on joblessness and temporary employment by demographic group. More stringent EPL raises relative non-employment rates for youth, immigrants, and, possibly, women, controlling for demographic variables and country dummies. For wage and salary workers, EPL raises the relative incidence of temporary employment for the low skilled, youth, native women, and especially immigrant women. These effects are often stronger in countries with higher levels of collective bargaining coverage.

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. Among the suggested policy remedies for reducing joblessness are temporary employment contracts without mandated protection (or considerably less protection than exists on permanent jobs). These have been used in some countries as an attempt to generate jobs that would not have been created.[1] 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 agree 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. This outcome is important since temporary jobs tend to be 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 if the unemployment rate does not fall (Blanchard and Landier, 2002; Cahuc and Postel-Vinay, 2002).

* The author thanks Giuseppe Bertola, Daniel Hamermesh, Barry Hirsch, Christine Jolls, Adriana Kugler, Stephen Machin, Øivind Anti Nilsen, two anonymous referees and participants at the IZA Conference on Employment Protection and Labour Markets, Bonn, Germany, December 2–3, 2005, the Society of Labor Economists Meetings, Cambridge, Mass., May 5–6, 2006, and the American Economic Association Meetings, Chicago, Illinois, January 5–7, 2007 for helpful comments and suggestions. Portions of this article were written while the author was a Visiting Fellow in the Economics Department of Princeton University, supported by the Industrial Relations Section. He is very grateful for this support.

[1] A notable example is Spain, which in the 1980s and 1990s had extremely high unemployment rates and liberalised the use of temporary contracts in an attempt to generate jobs. See Dolado *et al.* (2002).

The reasoning in such theoretical models suggests that the incidence of temporary jobs will not be randomly distributed across the labour 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 article studies the impact of employment protection mandates on demographic patterns of temporary employment as well as non-employment. More extensive employment protection mandates are expected to raise the overall employment gap between new and experienced workers as well as the experience gap in the incidence of permanent work among those with jobs. In addition, the presence of wage floors is expected is exacerbate these differences between low wage workers such as the young, women and immigrants and prime age native men. To test this reasoning, I use the 1994–8 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 protection by skill level.[2] The countries for which the IALS contains data that allowed me to analyse these effects include Canada, Finland, Italy, the Netherlands, Switzerland, the UK and the US.

I find that across these countries, all else equal, the strength of employment protection mandates (EPL – as measured by the OECD) is positively associated with the relative incidence of joblessness among the young, immigrants, and, possibly, women, controlling for demographic factors and country dummy variables. Moreover, I find that among wage and salary workers, stronger EPL raises the relative incidence of temporary employment for young workers, native women, and especially immigrant women, as well as those with low cognitive ability. These effects on non-employment and temporary employment are stronger in many instances the higher a country's level of collective bargaining coverage. The findings imply that labour market institutions disproportionately protect the jobs of prime age males, effects that are complementary to existing research which finds that more extensive coverage by centralised collective bargaining raises the relative employment of prime age men (Bertola *et al.*, forthcoming).

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

Early theories of the impact of employment protection mandates emphasised that making it difficult or expensive to fire workers reduced firms' incentives to lay off workers and to create new jobs. Of course, if wages are flexible, then firing costs can be capitalised in lower initial wages, leaving firms' incentives to offer new jobs unchanged, as long as firing costs take the form of actual severance payments to workers (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

[2] For further description of the IALS data, see OECD (1998).

lead to a greater disincentive to create jobs. Moreover, to the extent that employment protection mandates take the form of cumbersome regulations rather than payments to workers, there will again be a less than fully compensating decline in wages. Under these circumstances, the net impact on the unemployment rate will be theoretically indeterminate, since firing costs will lower both layoffs and job creation (Bertola, 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 *et al.* (forthcoming) 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 *et al.* (2006) find similar short-run results for states in the US that have granted workers the right to sue for wrongful discharge.[3]

More recent theories about employment protection recognise 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.[4] An additional motivation for offering temporary jobs is to create a buffer stock of labour, and under this view of temporary employment's function, easing restrictions on such jobs is again predicted to raise their incidence (Boeri and Garibaldi, 2007).

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. When the authors included both permanent and temporary protection mandate indexes as explanatory variables at the same time, they continued to find a significantly positive effect on temporary employment of permanent employment protection laws but no effect of temporary employment protection. The authors then suggest that regulations on temporary employment protection do not play a role in influencing the incidence of temporary jobs. It is also possible that our measures of the extent of temporary employment regulation are less precise than those for permanent jobs. Within the US,

[3] Autor *et al.* (2006) find that in the long run, there are stronger disemployment effects for older and more educated workers, who are the most likely to litigate in the US's system with its threat of jury-imposed punitive damages. In contrast, in many European systems, there is mandated dismissal pay.

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

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, although workers in this sector may still be in permanent jobs, employed by a contractor that supplies temporary labour.

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. The authors interpreted this result as suggesting that firms had raised their standards for making a job permanent in response to the reduced firing costs from temporary jobs. Of course, the overall labour market was deteriorating in France at the same time, making a conclusion about the impact of the reforms tentative.[5] Further, Boeri and Garibaldi (2007) find that Italy's reforms in the 1990s, which eased restrictions on temporary contracts, led to higher employment and lower productivity over the 1995–2000 period. While this was a period of expansion, the authors' focus on individual firms allowed them to make cross-sectional comparisons and thus control for the state of the economy. In this article, I also focus on within-country differences in temporary employment at a point in time, thus also implicitly controlling for the overall state of the economy.[6]

Unlike earlier work, I study the impact of employment protection mandates on the relative incidence of temporary employment across demographic groups. Moreover, I use microdata from several countries with varying degrees of employment protection strictness. This allows me to control for country-specific effects, and unlike research using aggregate data, I am able to control for observable heterogeneity across individuals. And, as discussed below, I am also able to consider the issue of unmeasured heterogeneity.

2. 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 and joblessness among recent labour market entrants and experienced workers. In their model, all entry level jobs start with the same productivity. Then after a period of unspecified duration, the firm receives an observation on the worker's productivity. The firm then has the option of turning the job into a permanent one or dismissing 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 placed in a permanent job and below which the worker's employment is

[5] 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.

[6] While not formally estimating the impact of firing costs on the relative incidence of temporary employment, the OECD (2002) and Petrongolo (2004) present some descriptive results on temporary jobs that are related to the present work. Specifically, the OECD (2002, p. 138) notes that among workers, temporary employment tends to be concentrated among the young, women and the less educated. And Petrongolo (2004) finds that women tend be overrepresented in temporary work.

terminated. The authors' model predicts, intuitively, that lower firing costs from permanent jobs (c_p) and higher firing costs from temporary jobs (c_t) both lower y^*, thus raising the probability that a temporary job is turned into a permanent job.

In Kahn (2006), I extend this basic framework to study the difference between the probability of having a permanent job after only one period in the labour market and after, say, $N > 1$ periods in the labour market. I find that for large enough N, higher c_p and lower c_t both raise the gap between experienced and inexperienced workers in the probability of being in a permanent job. This result makes 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. Of course, if the N required to reach this conclusion is large relative to the length of one's work life, then higher c_p or lower c_t may not raise the gap in permanent employment between experienced and inexperienced workers. Thus, whether this result holds in reality is an empirical question, although the limit argument just outlined shows that it will hold asymptotically.

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 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 (for example, the lower permanent job firing costs are).[7]

The basic model assumes for simplicity that one is employed each period. In the likely event that it takes time to find a job when one enters the labour market or has left a job, then we expect permanent employment protection mandates to lower the relative propensity of inexperienced individuals to be employed, since the exit rate from temporary jobs is likely to be higher than the exit rate from permanent jobs. Finally, all of the predictions mentioned above become weaker the more downwardly-flexible starting wages are, using reasoning discussed earlier. Thus, the effects of EPL on joblessness and temporary employment are expected to become stronger in countries with more downward wage rigidity.

3. Institutional Setting and Data

As noted earlier, I use 1994–8 IALS data for Canada, Finland, Italy, the Netherlands, Switzerland, the UK and the US 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

[7] A caveat to these conclusions concerns the possibility 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. 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 get a permanent job is perhaps more likely. In such a scenario, higher firing costs could still raise the experience gap in permanent employment.

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
United Kingdom	0.0	0.5	2.4	8.0	2.8	1.0	0.8
United States	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
United Kingdom	No limit	No limit	4.0	0.3
United States	No limit	No limit	4.0	0.3

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

1990s. For example, Table 1 shows that Italy had much higher mandated severance pay 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.

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. Each country is represented by a single random cross-section taken in one year, with the exception of Switzerland, for which the German and French-speaking subsamples were suveryed in 1994, while the Italian-speaking subsample was surveyed in 1998. The other countries' survey years were as follows: Canada, the Netherlands, and the US – 1994; the UK – 1996; and Finland and Italy – 1998. Because the design is a single cross-section, I am unable to analyse the frequency and duration of temporary employment. Rather, I in effect study the stock of temporary jobs, an analysis conceptually similar to studying the stock of unemployed workers. It should be noted that any national differences in the overall duration or frequency of temporary employment will be controlled for by the inclusion of country dummies in some models. 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 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 prose, document and quantitative literacy skills and are described in more detail in the Appendix. 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 0.9. 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.

A distinctive feature of the IALS is that it allows one to isolate a sample of low skill workers (for whom I expect stronger EPL effects), where skill is comparably defined across countries. Other microdata bases do not allow such comparability, implying that the IALS can potentially yield more valid conclusions about the determinants of labour market outcomes for unskilled workers in different countries. In addition, the data contain a rich body of labour force information about these workers, and the IALS includes countries with very different labour market institutions, such as Italy, the Netherlands and Finland on the one hand and the US, Britain and Canada on the other.

Table A1 provides some descriptive information on the employment outcomes analysed here. Table A1 shows that in the population, the incidence of permanent work among men ranges from a low of 57% in Finland to 77% in the US, while for women the range is even greater – from 32% in Italy to 61% in the US. These figures of course combine both the incidence of work and the incidence of permanent jobs among those with work. The IALS figures for non-employment are very highly correlated with those in the OECD (1999) for 1998: specifically, for the seven countries studied here, the IALS and OECD-based non-employment rates among men have a correlation of 0.88, those for women are correlated at 0.83, and the gender gap in joblessness is correlated at 0.90 in the two data sources.

Table A1 also shows the incidence of temporary employment among wage and salary workers in the IALS and from the OECD (2002) for 2000. The OECD definition of temporary work includes those on fixed term contracts, temporary agency workers, daily workers, trainees, people in job creation schemes, workers on contracts for a specific task, those on replacement contracts, and on-call workers (OECD, 2002, pp. 170–1). In contrast, the IALS definition is less specific and to some degree varies across countries. For example, for Britain, a temporary job is one for which 'there was some way that it was NOT permanent' (IALS British questionnaire documentation file, p. 27), while the US questionnaire asks whether the job was temporary or permanent, and the Italian questionnaire asks whether one's labour contract had a determinate

[8] 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.

length. In these three countries, the incidence of temporary work is somewhat higher in the IALS than the OECD; perhaps respondents interpreted the IALS question there as referring to *de facto* limitations on job duration rather than *de jure* limitations. Moreover, while the OECD definition explicitly includes temporary agency workers, the IALS merely asks whether the job is permanent. In any case, inclusion of country dummies in some models can at least partially account for such differences as can my empirical strategy of studying within-country demographic differences.

Overall, in both the OECD and the IALS, the US and the UK have a relatively low incidence of temporary work, and Finland has a high incidence. The figures from the two data sources are generally consistent with each other, although the correlation is less close than it is for joblessness: temporary employment for men is correlated at 0.57 in the two data sources and the correlation for women is 0.65. However, in both the OECD (2002) and IALS data, women are more likely to have temporary jobs than men do, and the gender gap in temporary work is highly correlated in the two data sources at 0.76.[9] The fact that the gender gap in temporary work is more highly correlated between the IALS and the OECD than the individual figures for men and women suggests the usefulness of studying within country demographic differences in temporary employment.

A further concern in comparing temporary employment across countries is that the economic content of this concept may differ across countries even when it is similarly defined. For example, in the US even workers with permanent jobs have limited protection, perhaps lessening the distinction between temporary and permanent jobs; in contrast, in Italy with its strong permanent job protection, workers in temporary jobs probably do have less job security than those in permanent jobs. In an extreme case, firms in the US may not call jobs temporary even if they intend to discharge workers from them. To investigate whether there is any empirical content to the permanent-temporary job distinction, recall that Booth, Francesconi and Frank (2002) found that in Britain, temporary workers were less well paid, received less training, and had lower levels of job satisfaction than those in permanent jobs. In addition, I analysed weekly earnings of wage and salary workers in the IALS for the six countries for which data were available on temporary employment, employment protection mandates (from the OECD) and earnings: Canada, Finland, Italy, the Netherlands, Switzerland and the US. Analysing earnings separately by gender and country and controlling for years of schooling, a series of age dummy variables,[10] cognitive test scores, and immigrant status, I found that the log weekly earnings was higher in each country-gender group for permanent than for temporary jobs. The effects were significantly different from 0 (at the 5% level or better on two tailed tests) in 11 of 12 cases, with the significant coefficients ranging from 0.134 (Dutch men) to 0.908 (US women), and with one insignificant effect (0.028) for Dutch women. Thus, in the IALS, the distinction

[9] Excluding Italy, which has the largest divergence in temporary employment incidence between the two data sources, temporary employment is very highly correlated across the IALS and OECD: for men, the incidence has a correlation of 0.74, for women, the correlation is 0.83, and the gender gap is correlated at 0.91. As discussed below, my basic econometric results for the incidence of temporary employment were very similar when Italy was excluded.

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

between temporary and permanent jobs appears to be real, with permanent jobs usually paying considerably more than temporary jobs, other things equal. Moreover, the fact that in each case, permanent jobs were higher-paid than temporary jobs is consistent with the analytical framework discussed above in which firms convert temporary jobs into permanent jobs only if the worker's productivity is greater than or equal to a reservation threshold.

4. Empirical Procedures

To investigate the impact of employment protection mandates, I proceed in two steps. First, I analyse the impact of protection on non-employment among all adults. Second, I study the determinants of permanent employment among wage and salary workers. To examine non-employment, I estimate a multinomial logit model with a dependent variable that takes on three possible values: 1 for non-employment, 2 for being employed in a temporary job, and 3 for being employed in a permanent job.[11] The model can be summarised as follows:

$$\begin{aligned}\text{Prob}(Perm_{ij} = 1|Z_{ij})/\text{Prob}(Nonemployed_{ij} = 1|\mathbf{Z}_{ij}) = exp(\mathbf{C}_1'\mathbf{Z}_{ij}) \equiv \exp(\mathbf{B}_1'\mathbf{X}_{ij} + a_{11}EPL_j \\ + a_{12}EPL_j \times AGE2635_{ij} + a_{13}EPL_j \times AGE3645_{ij} + a_{14}EPL_j \times AGE4655_{ij} \\ + a_{15}EPL_j \times AGE5665_{ij} + a_{16}EPL_j \times EDYRS_{ij} + a_{17}EPL_j \times LEVEL1_{ij} \\ + a_{18}EPL_j \times FEMALE_{ij} + a_{19}EPL_j \times IMMIG_{ij})\end{aligned} \tag{1}$$

$$\begin{aligned}\text{Prob}(Perm_{ij} = 1|\mathbf{Z}_{ij})/\text{Prob}(Temp_{ij} = 1|\mathbf{Z}_{ij}) = exp(\mathbf{C}_2'\mathbf{Z}_{ij}) \equiv \exp(\mathbf{B}_2'\mathbf{X}_{ij} + a_{21}EPL_j \\ + a_{22}EPL_j \times AGE2635_{ij} + a_{23}EPL_j \times AGE3645_{ij} + a_{24}EPL_j \times AGE4655_{ij} \\ + a_{25}EPL_j \times AGE5665_{ij} + a_{26}EPL_j \times EDYRS_{ij} + a_{27}EPL_j \times LEVEL1_{ij} \\ + a_{28}EPL_j \times FEMALE_{ij} + a_{29}EPL_j \times IMMIG_{ij}),\end{aligned} \tag{2}$$

where for each person i in country j between 16 and 65 years old, *Perm* is a dummy variable equalling one if one is employed in a permanent job; *Non-employed* is a dummy variable equalling one if one is not employed; *Temp* is a dummy variable for being employed in a temporary job; **X** is a vector of explanatory variables to be described; *EPL* is the country's OECD permanent employment protection indicator; *AGE*2635-*AGE*5665 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); *EDYRS* is years of schooling; *LEVEL*1 is a dummy variable for having average IALS test score in the *LEVEL* 1 (lowest) range; *FEMALE* is a female dummy variable; *IMMIG* is an immigrant dummy variable; and **Z** is a vector including all of these explanatory variables.

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

[11] While a multinomial logit model is not strictly necessary for studying non-employment (i.e., one could use simple logit with two outcomes – employed and not employed), it is more flexible than a simple logit, which would treat temporary and permanent employment the same. Results were similar, however, when I estimated the simple logit model.

as a full set of interactions between gender and the age, education, low test score and immigrant variables. Coefficients $a_{12}-a_{19}$ and $a_{22}-a_{29}$ can be used to test the hypothesis that employment protection has different effects on the indicated demographic or skill group. These interactions in effect produce a differences-in-differences analysis. Youth, women and immigrants are all likely to be more recent entrants than older native men; thus, our theoretical reasoning predicts that employment protection lowers their relative employment and, when, employed, their likelihood of being in a permanent job. Moreover, the asymptotic standard errors are corrected for clustering within countries.

In some models, I include country dummies in order to account for unmeasured, country-specific influences on the incidence of non-employment, permanent employment or temporary employment.[12] These include but are not limited to other policies and institutions such as taxes, UI, collective bargaining, disability programmes, and product market regulation, as well as the availability and quality of educational opportunities and population characteristics that influence employment outcomes. On the other hand, excluding country dummies allows EPL to affect overall levels of permanent employment and joblessness. Thus, I report results both including and excluding country dummies.

Even with country dummies, however, other institutions such as collective bargaining coverage may have indirect effects on the relative incidence of non-employment or 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 employment or out of permanent jobs. In addition, collective bargaining coverage may add to the strength of employment protection by providing workers with representation and support if they challenge a dismissal. Therefore, in some models, I allow for three-way interactions between employment protection and 1994 collective bargaining coverage from OECD (1997) and the demographic variables, implementing a differences-in-differences-in-differences analysis. I of course include lower-level interactions between collective bargaining and the controls, and when country dummies are excluded, EPL and collective bargaining main effect effects and their interaction. Since youth, women and immigrants are more likely than older men to be constrained by wage floors, we expect stronger EPL effects for these low wage groups in countries with high levels of collective bargaining coverage. If these groups are less likely to be covered by collective bargaining, then this provides an additional reason why they may be shut out of permanent jobs (and thus more likely to be jobless) in countries with high levels of unionisation and employment protection mandates.

To study the determinants of permanent employment, I restrict the sample to employed wage and salary workers and estimate a logit model that includes all of the regressors that are in (1) and (2). The theory of temporary employment outlined earlier is best applied to wage and salary workers, who have employers making decisions about whether to keep them, and self-employed workers may interpret a question about temporary employment differently from wage and salary workers. In addition, the variables in (1) and (2) are augmented in some cases by a series of occupation and

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

industry dummy variables, as well as their interactions with gender, in order to control for compositional differences across countries.[13] 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. In addition, inclusion of industry and occupation dummies can control for other motivations for offering temporary jobs such as reducing adjustment costs (Boeri and Garibaldi, 2007). On the other hand, 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 also estimate the determinants of permanent employment not controlling for sector. Finally, as described in Section 5.2, I also attempt for robustness checks several other specifications in estimating the probability of permanent employment among wage and salary workers.

5. Results

5.1 *Basic Findings*

Table 3 shows the impact of employment protection on the 'unconditional' (i.e., conditional only on the explanatory variables) probability of non-employment, which can be recovered from the multinomial logit coefficients. Table 3, Panel (*a*)'s point estimates suggest that employment protection raises the relative likelihood of employment for prime age vs. young individuals, men vs. women, and natives vs. immigrants. The results are similar whether or not I control for country dummies. The magnitudes are moderately large, particularly for age and immigrant status. Specifically, the units in Panel (*a*) are employment probability effects, and the impact of a one unit change in EPL (roughly the difference between Switzerland's and Finland's indexes) on non-employment for 36–45 year olds vs. 16–25 years, for example, ranges from −7.6 to −7.7 percentage points, and the average joblessness incidence for the sample is about 36%. The effect drops for those age 56–65, a finding consistent with the male results in Bertola *et al.* (forthcoming), suggesting that elderly workers in countries with extensive employment protection systems may be moved into retirement programmes. The effects' magnitudes appear small for education and low test score, although the signs are opposite from what one would expect, but they are somewhat larger for women and even more so for immigrants. Regarding statistical significance, the age interactions are highly significant as a group (although not individually), the EPL interactions with education and test score are insignificant to marginally significant, the EPL gender interactions are about 1.5 times their asymptotic standard errors, and EPL-immigrant interactions are highly significant.

[13] 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.

Table 3

Selected Results for the Probability of Nonemployment, All Adults

	Partial derivatives of nonemployment at means of employment outcomes			
	coef	Ase	coef	ase
(*a*) Basic specification (excludes collective bargaining coverage and interactions)				
EPL Index	0.031	0.050	—	—
EPL Index × *Age 26–35*	−0.072	0.046	−0.070	0.046
EPL Index × *Age 36–45*	−0.077	0.057	−0.076	0.057
EPL Index × *Age 46–55*	−0.050	0.060	−0.048	0.060
EPL Index × *Age 56–65*	0.007	0.054	0.008	0.054
EPL Index × *Education*	0.004	0.003	0.004	0.002
EPL Index × *Low Test Score*	−0.016	0.011	−0.019	0.010
EPL Index × *Female*	0.030	0.020	0.029	0.020
EPL Index × *Immigrant*	0.064	0.018	0.071	0.015
Country dummies?	no		yes	
Sample size	26,159		26,159	
(*b*) Basic spec. Augmented by collective bargaining coverage and its interactions				
EPL Index × *Age 26–35*CB Cov*	−0.231	0.143	−0.234	0.142
EPL Index × *Age 36–45*CB Cov*	−0.243	0.102	−0.234	0.106
EPL Index × *Age 46–55*CB Cov*	−0.235	0.119	−0.242	0.117
EPL Index × *Age 56–65*CB Cov*	−0.025	0.145	−0.045	0.150
EPL Index × *Education*CB Cov*	−0.042	0.024	−0.036	0.023
EPL Index × *Low Test Score*CB Cov*	0.085	0.062	0.136	0.071
EPL Index × *Female*CB Cov*	−0.055	0.102	−0.053	0.105
EPL Index × *Immigrant*CB Cov*	−0.043	0.092	−0.014	0.060
Country dummies?	no		yes	
Sample size	26,159		26,159	

Based on multinomial logit analyses with three outcomes: nonemployment, permanent employment and and temporary employment. EPL Index is the OECD's index of strength of employment protection mandates. Controls include age dummies, education, low test score dummy, immigrant dummy and a female dummy and female interactions with each of these variables. In Panel (*b*) *CB Cov* is collective bargaining coverage. In addition to the interactions shown, a complete set of two way interactions between *CB Cov* and the demographic variables is included. When country dummies are excluded, main effects for *EPL* and *CB Cov*, as well their interaction are included. Asymptotic standard errors are corrected for correlation within countries. Data are weighted using IALS sampling weights adjusted so that each country gets the same total weight.

Table 3, Panel (*b*) shows the effect of adding a series of three way interactions among collective bargaining coverage (*CB Cov*), employment protection and the demographic and skill variables in the model. The Table shows that the negative EPL employment effects for youth become significantly larger in magnitude in countries with high levels of collective bargaining, effects which are also large in magnitude. Again, the three way interaction falls for those age 56–65, suggesting union involvement in the downsizing of older workers and movement into retirement programs (Bertola *et al.*, forthcoming). Moreover, the *EPL*×*CB Cov* interaction effects for non-employment are negative (and marginally significant) for schooling and positive (and marginally significant controlling for country dummies) for low test score. These results imply that *EPL* has a more negative effect on the relative employment of the less educated and the low skilled in countries with higher levels of collective bargaining coverage.

Table 4 shows logit analyses of the determinants of permanent employment, with the sample restricted to employed wage and salary workers. I vary the specifications in two ways:

Table 4

Selected Logit Results for the Determinants of Permanent Employment, Wage and Salary Workers

	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	0.004	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	13,736		13,736		13,736		13,736	

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.

(*i*) inclusion or exclusion of industry and occupation dummies and their interactions with gender;
(*ii*) inclusion or exclusion of country dummies.

The Table shows the partial derivatives of the dependent variable evaluated at the sample mean incidence of permanent employment. Overall, 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 every case for gender, usually significant or marginally so for immigrant status, and marginally significant for low test score when country dummies are excluded. Moreover, the interaction effects increase algebraically in each case with rising age beyond 35, suggesting rising relative protection as workers age, conditional on employment. This effect of age could also be due to the rising entitlement to dismissal pay with tenure in some countries (see Table 1). Effects of education are never large in absolute value or statistically significant. The basic results hold up controlling for country specific effects and industry and occupation dummies, as well as industry and occupation interacted with gender.[14]

To assess the magnitude of these effects, it is useful to compare outcomes under a low level of employment protection such as the US level with those under a high degree

[14] In light of the possible differing effects of EPL by sector (Messina and Vallanti, 2007; Koeniger and Prat, 2007), I also tested a series of EPL–demographic interactions with employment in agriculture and employment in manufacturing (included in the logits simultaneously). Most of these three way interactions were insignificant; however, there was some indication that EPL effects on permanent employment were more negative for those with low test scores in manufacturing, low levels of schooling in agriculture and for young workers in manufacturing.

of protection such as that of the Netherlands. The difference in the OECD's employment protection index between these two countries is 2.9. Table 5 shows the impact of changing employment protection by this extent on age, gender, cognitive ability and nativity-based gaps in permanent employment, using the estimates with country dummies and industry and occupation controls from Table 4. In addition, Table 5 shows the actual incidence of permanent employment across these dimensions for the Netherlands and the US.

Beginning with the effect of age, Table 5 shows that among those with wage and salary jobs, only 67.6% of 16–25 year olds in the Netherlands 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: 95.7% of this group in the Netherlands 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 the Netherlands is fully 28.1 percentage points, compared to only 15.2 percentage points in the US, for a Netherlands–US difference of 12.9 percentage points. Table 4's estimates for the model with country dummies and industry–occupation controls imply that raising the employment protection mandate from the United States to the Dutch level raises the permanent employment gap between 46–55 year olds and 16–25 year olds by 12.6 percentage points, a highly significant effect. Table 5 shows that this point estimate is fully 97.5% of the actual Dutch–US difference in the permanent employment gap between these two age groups. The other logit models yield predicted changes in this gap of 8.5 to 11.6 percentage points. Using any of these parameter estimates, I find that employment protection accounts for an important portion of the higher relative incidence of temporary employment for young people in the Netherlands compared to the US.

Table 5 shows similar results for the degree to which employment protection accounts for Dutch–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 4.1 percentage points higher in the Netherlands (with rounding). Changing employment protection mandates from the US to the Dutch level raises the gender gap in permanent employment by 2.9 percentage points, again a highly significant effect. The impact accounts for 71% of the actual Dutch–US difference in the gender gap using the fully specified model in Table 4. All of the other models in Table 4 show larger effects than this. Table 5 shows that in the Netherlands, 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 4.0 percentage points higher in the Netherlands than in the US, and the employment protection effect is 126% of this, using the last model in Table 4, although in this case the effect is not statistically significant. Again, the other models imply larger effects than this, some of which are at least marginally significant. Finally, natives are 6.0 percentage points more likely in the Netherlands and 0.3 percentage points less likely in the US than immigrants to have permanent jobs, for a 6.4 percentage point Dutch–US difference in the native–immigrant gap (with rounding). Using the last model in Table 4, I conclude that protection explains 101% of this difference, an effect that is twice its asymptotic standard error. The other parameter estimates in Table 4 imply a

Table 5

Decomposition of US–Dutch Differences in Demographic and Skill Differentials in Permanent Employment, Wage and Salary Workers

Dimension	Netherlands	US	Difference: Netherlands–US Effect	Asy std err
1. Age				
46–55 Permanent employment incidence	0.957	0.962	−0.006	—
16–25 Permanent employment incidence	0.676	0.811	−0.135	—
Actual permanent employment gap (46–55 minus 16–25)	0.281	0.152	0.129	—
Effect of changing from US to Dutch protection	—	—	0.126	0.032
Percentage of US–Dutch difference explained by protection	—	—	97.5%	25.2%
2. Gender				
Male permanent employment incidence	0.899	0.944	−0.045	—
Female permanent employment incidence	0.844	0.930	−0.085	—
Actual permanent employment gap (male minus female)	0.055	0.014	0.041	—
Effect of changing from US to Dutch protection	—	—	0.029	0.003
Percentage of US–Dutch difference explained by protection	—	—	71.0%	7.7%
3. Cognitive Ability				
Permanent employment incidence for higher than Level 1 test score	0.879	0.935	−0.056	—
Permanent employment incidence for low test score (Level 1)	0.855	0.950	−0.095	—
Actual permanent employment gap (above Level 1 minus Level 1)	0.024	−0.015	0.040	—
Effect of changing from US to Dutch protection	—	—	0.050	0.043
Percentage of US–Dutch difference explained by protection	—	—	126.2%	109.9%
4. Nativity				
Native permanent employment incidence	0.881	0.936	−0.055	—
Immigrant permanent employment incidence	0.821	0.940	−0.119	—
Actual permanent employment gap (Native minus Immigrant)	0.060	−0.003	0.064	—
Effect of changing from US to Dutch protection	—	—	0.064	0.032
Percentage of US–Dutch difference explained by protection	—	—	100.8%	50.3%

Note: Based on Logit model with country dummies and occupation-industry controls (last model of Table 4).

range for this estimate of 68% to 101%. Decomposition results for the US vs. Italy, another country in my sample with stringent EPL, were very similar to those in Table 5.

5.2. *Alternative Specifications for the Determinants of Permanent Employment*

In this subsection, I explore some more detailed specifications of the basic permanent employment logit model in order to examine the roles of collective bargaining, gender, temporary employment protection, and possible sample selection bias. Moreover, I discuss results where the protection measure is disaggregated into its components (severance pay, unfair dismissal pay, mandatory notice of layoffs, and procedural delays) 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.

5.2.1. *Collective bargaining interactions*

Table 6 shows logit results of models with similar collective bargaining interactions to those used in Table 3, and I find very strong three way interaction effects for EPL and collective bargaining coverage with age and with 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 large in magnitude and highly statistically significant in all specifications. For example, using the difference between Dutch and US collective bargaining coverage of 0.63 (81% vs. 18%) and using the most fully specified model in Table 6, an increase in employment protection from the US to the Dutch level widens the age 46–55 vs. age 16–25 gap in permanent employment by 34.2 percentage points more with the higher collective bargaining level. The native-immigrant permanent employment gap is widened by 24.7 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 negative, large in magnitude and highly significant when I do not control for sector. However, controlling for sector reduces the female interactions to a very small and insignificant level. Thus, to the extent that *EPL* and *CB* cov interact to shut women out of permanent jobs, these effects occur across industries and occupations but not within them. On the other hand, if women's sectoral representation is not affected by these institutions, then the results suggest that EPL and collective bargaining do not have strong interaction effects for women. Finally, the three way interactions involving education and cognitive ability go in opposite directions, making it difficult to arrive at strong conclusions about the *EPL–CB Cov* interactions with skill. 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 would suggest; on the other hand, I also obtain positive interactions with low test scores, implying the opposite.

5.2.2. *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

Table 6

Selected Logit Results for Permanent Employment, with Protection-Collective Bargaining Coverage Interactions, Wage and Salary Workers

	Partial derivatives at mean of dependent variable							
	coef	asy se	coef	asy se	coef	asy se	coef	asy se
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	13,736		13,736		13,736		13,736	

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, and dummies for low test score, immigrant, and female as well as female interactions with each of these variables. In models excluding country dummies, *EPL*, *CB Cov* and *EPL* × *CB Cov* are included. Asymptotic standard errors are corrected for correlation within countries. Data are weighted using IALS sampling weights adjusted so that each country gets the same total weight.

workers' propensities to be in permanent jobs. 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 model in Table 4. 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 4. Only the negative interactions with female and immigrants hold up in statistical significance. And when I estimated the basic Table 4 models with the permanent employment protection terms replaced by temporary employment regulation, the results were virtually identical to those in Table 4. A further specification suggested by

Table 7

Selected Logit Results for the Effects of Regular and Temporary Employment Protection on Permanent Employment, Wage and Salary Workers

	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	13,736		13,736		13,736		13,736	

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.

the theory is to control for the difference between permanent and temporary regulation strength, a specification that is nested in the models in Table 7. However, such a specification is strongly rejected (at much better than the 1% level) in favour of Table 7's equations which allow permanent and temporary employment regulation to have unrestricted effects. Despite the OECD's (1999) discussion of the importance of relaxing temporary employment restrictions, these do not seem to be well-captured by the OECD's index. This conclusion is consistent with Booth, Dolado and Frank's (2002) finding 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.

5.2.3. *Gender interactions*

The basic model in Table 4 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 labour 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 4 shows that, overall, employment protection lowers women's relative likelihood of permanent employment. In Kahn (2006), I report 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 immigrant status. These interaction results imply that 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. Moreover, they imply 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 labour market experience. Thus it might not be surprising that employed immigrant women's incidence of permanent jobs would be especially affected by EPL.

5.2.4. *Sample selection bias*

As discussed earlier, the differing employment to population ratios across the countries in my sample raise the possibility that the basic models interacting employment protection and demographic groups may be influenced by sample selection bias. The IALS data show, for example, 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 identify such a system credibly. 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. 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 country, 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).[15] I perform a similar analysis for women, for whom IALS data show that Italy is the base country with the lowest female employment to population ratio among the non-self-employed at 0.335. This procedure yields male and female samples with the same relative likelihood of employment and imposes no

[15] To illustrate this process, consider Switzerland, in which 79.5% of the population of men who were not self-employed had jobs. From the Swiss sample of men with wage and salary 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.

a priori assumptions about the market or nonmarket productivity of nonparticipants vs. participants. It is similar in spirit to propensity score matching. In Kahn (2006) I report full results for this exercise, and overall the pattern of results is very similar to that in Table 4.[16]

5.2.5. *Disaggregating the components of the OECD protection index*

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. When I disaggregated the policy components and included them individually as the measure of employment protection, the results were very similar to those in Table 4. 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.

5.2.6. *Results excluding countries with high or low employment protection levels*

As mentioned earlier, temporary employment may mean different things in a country with strong employment protection such as Italy or the Netherlands from one with weak protection mandates such as the Anglo-Saxon countries. The basic results were similar when Italy or the Netherlands was excluded, as well as when Britain, Canada or the US was excluded from the sample. These alternative analyses excluding key countries imply that the effects I have found in this article are more general than merely country-specific effects.

5.2.7. *Results excluding those age 16–25*

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 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.[17] The results of this analysis were very similar to those for the full sample (Table 4).

[16] The implied effects of EPL on permanent employment among those with jobs when I used the multinomial logit coefficients from estimation of (1) and (2) on the entire population of adults were very similar to the results reported in Table 4. This similarity provides a further indication that selection is not driving the results in Table 4.

[17] 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.

6. Conclusions

In this article, I have estimated the impact of employment protection legislation on the incidence of non-employment and permanent employment. I argued on theoretical grounds that protection should tend to lower the relative incidence of employment and of permanent jobs for the young, women, immigrants and the less skilled. I tested these predictions using 1994–8 IALS data on Canada, Finland, Italy, the Netherlands, Switzerland, the UK and the US, countries with widely varying degrees of employment protection. I indeed found that greater protection disproportionately lowered the relative probability that youths, immigrants and, possibly, women were employed; in addition, among those with jobs, I generally found that greater employment protection also lowered the relative incidence of permanent work among these groups, as well as those with low cognitive ability. Upon closer examination, the negative immigrant effects on permanent employment were concentrated on women. Moreover, in many cases, greater coverage by collective bargaining accentuated the effects of employment protection on the incidence of employment or permanent jobs among employees.

My results are complementary with those of Boeri and Garibaldi (2007) who find for a sample of Italian firms that greater use of temporary workers was associated with lower average labour productivity, controlling for workers' education levels and firm characteristics. This finding is consistent with their model in which greater use of temporary workers raises employment fluctuations, and assuming diminishing returns to labour, will result in lower average labour productivity. I find that more stringent regular employment protection raises the relative concentration of youth, women, immigrants and those with low cognitive ability in temporary jobs. Thus, in sectors which use temporary employment to a greater extent, not only is the average productivity level of a given quality of labour lower, the composition of workers in these jobs is also on average likely to have lower average skill levels than otherwise.

Previous research finds that high levels of centralised collective bargaining lead to lower relative employment or higher relative unemployment of young people and women (Kahn, 2000; Bertola *et al.*, forthcoming). 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.

I have focused on the interactions between employment protection and demographic groups at a point in time. This may be especially appropriate for studying the effect of permanent employment protection laws, since they have not changed dramatically, perhaps due to political constraints (Brügemann, 2007); rather, most changes in protection regulation have involved temporary employment regulations (OECD, 1999; Dolado *et al.*, 2002). Future work could profitably use longitudinal data to study the impact of long term changes in protection laws.

Cornell University, CESifo and IZA

References

Autor, D.H. (2003). 'Outsourcing at will: the contribution of unjust dismissal doctrine to the growth of employment outsourcing', *Journal of Labor Economics*, vol. 21(1), (January), pp. 1–42.

Autor, D.H., Donohue, III, J.J. and Schwab, S.J. (2006). 'The costs of wrongful-discharge laws', *Review of Economics and Statistics*, vol. 88(2), (May), pp. 211–31.

Bertola, G. (1992). 'Labor turnover costs and average labor demand', *Journal of Labor Economics*, vol. 10(4), (October), pp. 389–411.

Bertola, G., Blau, F.D. and Kahn, L.M. (forthcoming). 'Labor market institutions and demographic employment patterns', *Journal of Population Economics*.

Blanchard, O. and Landier, A. (2002). 'The perverse effects of partial labour market reform: fixed-term contracts in France', ECONOMIC JOURNAL vol. 112(480), (June), pp. F214–44.

Blau, F.D. and Kahn, L.M. (1996). 'International differences in male wage inequality: institutions versus market forces', *Journal of Political Economy*, vol. 104(4), (August), pp. 791–837.

Blau, F.D. and Kahn, L.M. (2005). 'Do cognitive test scores explain higher U.S. wage inequality?', *Review of Economics and Statistics*, vol. 87(1), (February), pp. 184–93.

Boeri, T. and Garibaldi, P. (2007). 'Two tier reforms of employment protection: a honeymoon effect?', ECONOMIC JOURNAL, vol. 117, pp. F357–85.

Booth, A.L., Dolado, J.J. and Frank, J. (2002). 'Symposium on temporary work introduction', ECONOMIC JOURNAL, vol. 112(480), (June), pp. F181–8.

Booth, A.L., Francesconi, M. and Frank, J. (2002). 'Temporary jobs: stepping stones or dead ends', ECONOMIC JOURNAL, vol. 112(480), (June), pp. F189–213.

Brügemann, B. (2007). 'Employment protection: tough to scrap or tough to get?', ECONOMIC JOURNAL, vol. 117, pp. F386–F415.

Cahuc, P. and Postel-Vinay, F. (2002). 'Temporary jobs, employment protection and labor market performance', *Labour Economics*, vol. 9(1), (February), pp. 63–91.

Dolado, J.J., Garcia-Serano, C. and Jimeno, J.F. (2002). 'Drawing lessons from the boom of temporary jobs in Spain', ECONOMIC JOURNAL, vol. 112(480), (June), pp. F270–95.

Güell, M. (2003). 'Fixed-term contracts and unemployment: an efficiency wage analysis', Working paper, Universitat Pompeu Fabra, July.

Heckman, J.J. (1979). 'Sample selection bias as a specification error', *Econometrica*, vol. 47(1), (January), pp. 153–62.

Holmlund, B. and Storrie, D. (2002). 'Temporary work in turbulent times: the Swedish experience', ECONOMIC JOURNAL, vol. 112(480), (June), pp. F245–69.

Hunt, J. (2002). 'The transition in East Germany: when is a ten point fall in the gender wage gap bad news?', *Journal of Labor Economics*, vol. 20(1), (January), pp. 148–69.

Kahn, L. M. (2000). 'Wage inequality, collective bargaining and relative employment from 1985 to 1994: evidence from fifteen OECD countries', *Review of Economics and Statistics*, vol. 82(4), (November), pp. 564–79.

Kahn, L.M. (2006). 'The impact of employment protection mandates on demographic temporary employment patterns: international microeconomic evidence', Princeton University Industrial Relations Section Working Paper, No. 507, January.

Koeniger, W. and Prat, J. (2007). 'Employment protection, product market regulation and firm selection', ECONOMIC JOURNAL, vol. 117, pp. F302–32.

Lazear, E.P. (1990). 'Job security provisions and employment', *Quarterly Journal of Economics*, vol. 105(3), (August), pp. 699–726.

Messina, J. and Vallanti, G. (2007). 'Job flow dynamics and firing restrictions: evidence from Europe', ECONOMIC JOURNAL, vol. 117, pp. F279–F301.

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

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

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

OECD. (2002). *Employment Outlook: July 2002*, Paris: OECD.

Petrongolo, B. (2004). 'Gender segregation in employment contracts', *Journal of the European Economic Association*, vol. 2(2–3), (April-May), pp. 331–45.

Appendix: IALS Test Scores and the Definition of Low Test Scores (termed 'Level 1')

The IALS gave each respondent tests to measure three kinds of literacy:

(*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 chequebook, 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).

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). 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.

Table A1
Summary Statistics for Dependent Variables, by Country

	Sample includes all adults (IALS)			Sample includes only employed wage and salary workers	
	Incidence of:			Incidence of:	
	Non-employment	Temporary employment	Permanent employment	Temporary employment (IALS)	Temporary employment, 2000 (OECD)
(*a*) Men					
Canada	0.277	0.071	0.652	0.100	0.118
Finland	0.362	0.066	0.572	0.123	0.145
Italy	0.322	0.071	0.607	0.129	0.088
Netherlands	0.280	0.067	0.653	0.101	0.115
Switzerland	0.184	0.054	0.762	0.064	0.105
United Kingdom	0.264	0.072	0.664	0.092	0.059
United States	0.190	0.040	0.769	0.056	0.039
(*b*) Women					
Canada	0.445	0.072	0.483	0.127	0.133
Finland	0.404	0.113	0.484	0.205	0.209
Italy	0.603	0.077	0.320	0.215	0.122
Netherlands	0.549	0.069	0.382	0.156	0.172
Switzerland	0.405	0.042	0.553	0.076	0.128
United Kingdom	0.386	0.075	0.539	0.122	0.077
United States	0.336	0.050	0.614	0.070	0.042

Sources: IALS and OECD (2002, p. 138). IALS data are weighted using IALS sampling weights adjusted so that each country receives the same total weight.