Labor Market Institutions and Demographic Employment Patterns [+]

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ABSTRACT: Using data from 17 OECD countries over the 1960-96 period and a simple theoretical framework, we investigate the impact of institutions on the relative employment of youth, women, and older individuals. Theoretically, we show that union strategies meant to improve workers' income share imply larger disemployment effects when labor supply is more elastic. Hence, demographic groups with good alternative uses of their time—youth, older individuals, and prime age women—should be relatively less employed compared to prime age males in more unionized labor markets. We regress group specific employment and unemployment outcomes on a standard set of labor market institutions, aggregate unemployment, and period and country effects. This design allows us to control for unmeasured country-specific factors that affect relative employment and unemployment. We find that more extensive involvement of unions in wage-setting decreases the employment-population ratio of young and older individuals relative to the prime-aged and of prime age women relative to prime age men. There is also evidence that unionization raises the unemployment rate of young men and prime age women compared to prime age men. The lack of evidence of union effects on unemployment for young women and older individuals suggests that disemployed individuals in these groups move predominantly into non-labor-force (education, home production or retirement) states.

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1. Introduction

The time-series and cross-sectional variability of industrialized countries' labor market performance motivates a large and influential body of research. Empirical studies have focused on labor market institutions, monetary policy and other macroeconomic shocks, and public employment as possible explanatory variables for the different evolution of aggregate unemployment rates, and an inverse relationship is also empirically apparent between within-country changes in unemployment rates and wage inequality.[1]

This paper focuses on the employment and unemployment rates of youth, women, and older individuals relative to prime-age males. The labor market position of such groups is, of course, an important issue in its own right.[2] Our approach, however, is motivated by the same broad empirical patterns and theoretical mechanisms that motivate studies of aggregate employment and unemployment. We argue that cross-country and time-series patterns of relative employment outcomes across demographic groups can be explained by the different impact across those groups of institutional differences across countries and periods, and focus in particular on the incidence of union policies on secondary labor force groups' employment. We offer a novel perspective on reasons why unionized labor markets should especially reduce employment of those groups, and provide comprehensive evidence that differences in OECD labor market outcomes are indeed concentrated on demographic groups other than prime age males.

In Section 2, we develop a model of union behavior that provides a simple and novel interpretation of wage compression and of non-prime-age-male disemployment. Theory indicates that, other things equal, wage-setting policies aimed at maximizing workers' total welfare imply larger wage increases, and therefore larger employment declines, for groups with more elastic labor supply. Intuitively, since wage increases result in some displacement of union members (compensated with the

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[1] See OECD (1994), Scarpetta (1996), Belot and van Ours (2000); Nickell, Nunziata, Ochel, and Quintini (2003); Ball (1999), Blanchard and Wolfers (2000), Bertola, Blau and Kahn (2002a); and Algan et al, 2002.

[2] Youth employment problems are prominent in Europe (Blanchflower and Freeman 2000); the labor market prospects of older workers importantly affect national pension policies and their sustainability (Disney, 1996); and women's employment outcomes are closely scrutinized in most countries and motivate equal-opportunity and parental leave policies that may or may not have actually raised female employment and labor force participation (Blau and Kahn 2000, Ruhm 1998).

proceeds of larger wage bills) employment losses are less attractive when those who lose jobs are on a steeply declining portion of their opportunity cost schedule. In reality, population groups other than prime-aged males not only tend to command lower wages in unregulated labor markets, but also have better alternatives to paid employment: schooling (youth), home production (women, under a traditional division of labor), and retirement (older individuals). Hence, our theory offers a simple and novel reason why unions should raise the relative wages and (as a result) lower the relative employment of all these secondary labor force groups, an outcome that cannot be rationalized by other theoretical mechanisms.

Empirically, our simple theory predicts that markets with stronger unions should feature larger wage increases for secondary labor force groups with better non-employment opportunities. There is abundant evidence that unionization decreases wage differentials across genders and between young and prime-age workers (see Blau and Kahn, 2003, Kahn, 2000, and references therein), and that the labor-supply elasticity differentials needed to support our proposed theoretical explanation are consistent with the mechanism we focus on (see Blundell and MaCurdy, 1999). Formal evidence of relative-employment effects is scarcer in the literature, so we proceed in Section 3 to test and quantify the main implications of our theoretical perspective with a comprehensive empirical exercise on a panel data set of 17 OECD countries over the 1960-96 period. Data on time-varying institutions enable us to control for country effects and thereby address concerns of country-specific omitted variables. Our empirical specification indexes the strength of the theoretical mechanism by indicators of union density and coverage by centralized collective bargaining institutions, as is appropriate since the theoretical mechanism supposes that union workers disemployed by higher wages fall on their non-employment options rather than obtain alternative employment. We also control for aggregate unemployment (as an indicator of macroeconomic conditions), demographic factors, and for a number of other labor market institutions. The results are consistent with the theoretical idea that more pervasive overall union activity should lead to greater relative disemployment of secondary labor force groups, and are not easily explained by spurious relationships between unionization measures and the demographic composition of the labor force.

2. A simple model of union wage-setting and relative employment effects

It may appear somewhat puzzling that, in labor markets that are more unionized, employment of secondary worker groups is relatively low. If prime-age males wield greater bargaining power, should they not use that power to boost their wages relative to other groups, and work less as a result? In this section, we show theoretically that unions should raise the relative pay (and lower the relative employment) of groups with more elastic labor supply schedules. The model is focused on the wage-employment tradeoffs faced by different groups of workers and, while abstracting from many important aspects of union-management bargaining, it offers a simple explanation both for wage compression by age and gender, and for larger disemployment effects for young, female, and older individuals. As discussed below, this combination of relative wage and employment outcomes is difficult to rationalize otherwise.

The basic insight can be illustrated in a simple log-linear analytical framework. The data we analyze below cannot distinguish between the hours and participation dimensions of labor supply: only zero-one employment and participation rates are available. Accordingly, we model group-level labor demand and participation decisions at the level of an entire labor market. To focus on the relationship between group i's employment and wages, demand or supply cross-group interaction terms are omitted in the formal model: we view this as a satisfactory approximation since, empirically, skilled prime-age workers are not close substitutes for youth, female, and older workers, while individuals within these groups are closely substitutable for each other (Disney, 1996; see Jimeno and Rodriguez-Palenzuela, 2002, for a formal model of imperfect labor-demand substitutability that would have similar implications under our assumptions regarding labor supply elasticity).

Consider the willingness-to-work function

$$w_i = s_i + \varepsilon_i(l_i - n_i), \qquad (1)$$

where l_i denotes the logarithm of the number of participating individuals and w_i the logarithm of each worker's take-home pay; s_i and n_i are labor supply shifters; and ε_i is the inverse elasticity of the group's labor supply, which depends on factors such as non-labor income, partners' wages, and non-employment uses of time. The opportunity cost of working is constant within the group if ε_i=0. Larger values of this parameter index increasingly inelastic labor supply schedules: as ε_i tends to infinity, labor market participation tends to n_i, which may vary across groups but is independent of the wage. Let labor market demand for the same group also be approximated by a log-linear schedule,

$$w_i = a_i - \eta_i l_i \tag{2}$$

where the parameter a indexes productivity, w is the log of employer labor cost, and $0<\eta_i<1$ is the elasticity of the inverse labor demand schedule facing group i.

In a *laissez faire* equilibrium where supply equals demand, the log of competitive wages and competitive employment are:

$$w_i = [\eta_i/(\varepsilon_i+\eta_i)]s_i - [\varepsilon_i\eta_i/(\varepsilon_i+\eta_i)]n_i + [\varepsilon_i/(\varepsilon_i+\eta_i)]a_i\ , \tag{3}$$

$$l_i = (a_i - s_i)/(\varepsilon_i+\eta_i) + [\varepsilon_i/(\varepsilon_i+\eta_i)]n_i\ . \tag{4}$$

Wages are quite intuitively predicted to be higher for groups with higher productivity (indexed by a), smaller size (indexed by n), better things to do out of employment (indexed by s); the *ceteris paribus* implications of different demand and supply elasticities are similarly intuitive. Note that it is possible that some workers, such as women, encounter labor market discrimination. Indeed, an extensive literature on the gender pay gap suggests that both gender differences in productivity and discrimination play a role in causing the observed differential (Blau, Ferber and Winkler 2002). This can be easily modeled by adjusting "true" productivity by the discrimination coefficient, with a representing adjusted productivity. This interpretation of a is most likely the relevant one for women, but the issue is not central to our concerns here and leaves our basic reasoning unchanged.

2.1 Unionization and the elasticity of participation

Now suppose the group of workers with labor demand schedule as in (2) and marginal opportunity costs of working as in (1) becomes unionized. We determine employment from a "right-to-manage" perspective, where firms are free to adjust the quantity of labor demanded.[3] Unions and management bargain over wages, but employers are free to set employment along their labor demand curves. Then, at union wages W (suppressing the group subscript i), firm profits are $F(L) - WL$ and the union surplus is WL

[3] Employer monopsony, or efficient bargaining over both pay and employment, would not imply that employment is lower for the groups whose wages are raised the most. See Farber (1986) and Card and Krueger (1995) for discussion of these theoretical possibilities, which we discuss below in the context of our model

– $S(L)$, where $F(\bullet)$ is the (concave) revenue function whose log marginal revenue product is expressed by equation (2), L is employment, and $S(L)$ is the aggregate opportunity cost of working for the L employees, with log marginal cost of working expressed by equation (1).[4]

Under the right-to-manage labor demand constraint $W=F'(L)$, consider an asymmetric wage bargain that chooses W to maximize

$$F(L)\text{-}WL+\beta(WL\text{-}S(L)), \tag{5}$$

where β is the relative weight of union objectives in the bargained outcome. This objective function generalizes the outcome of competitive equilibrium (where $\beta=1$ yields maximization of the total surplus $F(L)$-$S(L)$ generated by employment) to allow for different weighting of workers' and employers' surplus. If $\beta>1$, the objective weighs workers' surplus (total wages minus total opportunity cost) more heavily than employers' surplus (total value of production minus wages). This represents in stylized fashion the impact of more unionized and/or regulated labor markets. Since all incomes (from employment and non-employment) enter the objective function linearly and with equal weight, distributional concerns within the group of workers are assumed away by this specification.

The first order condition for maximization of (5) subject to $W=F'(L)$,

$$F'(L)=\beta S'(L)\text{-}[(\partial W/\partial L)L+W](\beta\text{-}1),$$

can be rearranged to read

$$S'(L)=F'(L)[1\text{-}\eta(L)(\beta\text{-}1)/\beta] \tag{6}$$

where $\eta(L)>0$ is the elasticity of the inverse labor demand curve. The $\beta=1$ case yields $S'(L_c)=W_c=F'(L_c)$, the competitive solution. At the other extreme, $S'(L_m)=F'(L_m)[1\text{-}\eta(\cdot)]$ when $\beta\to\infty$, and the employment level (L_m) preferred by a monopoly union is determined by a familiar markup term. Cases where $1<\beta<\infty$ represent intermediate labor market configurations. Quite intuitively, $\beta>1$ implies $S'(L_m)<F'(L_m)$: as long as labor demand is downward sloping, marginal productivity is less than average productivity, and a labor market allocation that privileges workers' over employers' total surplus introduces a wedge between marginal opportunity cost and marginal productivity.

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[4] As discussed below, this model assumes that workers' alternative to union employment is nonemployment. Thus, the model is most applicable to cases where a centralized union covers the entire work force.

Substituting from equations (1)-(4) and (6), we have the following expressions for the log of the ratio of union to nonunion wages and employment (again suppressing the group subscript):

$$\log(W_u/W_n) = \{\eta/(\varepsilon+\eta)\} [\log(\beta) - \log(\beta - \eta\beta + \eta)] \quad (7)$$

$$\log(L_u/L_n) = (\varepsilon+\eta)^{-1} [\log(\beta - \eta\beta + \eta) - \log(\beta)], \quad (8)$$

where *u* and *n* subscripts signify union and nonunion quantities respectively.

In equation (6), the union's markup over the opportunity cost of working evaluated at the unionized employment level depends on the elasticity of demand and on the parameter indexing the weight of workers' objectives in labor market outcomes, but is independent of supply elasticity. In equations (7) and (8), however, a more elastic group labor supply (i.e., a lower ε) implies a larger wage increase, and smaller union employment relative to nonunion employment.[5] This result is quite intuitive: since the price of monopolistic wage setting is shutting some individuals out of employment (and compensating them with the proceeds of larger wage bills), high wage markups and large employment losses are less attractive when those who lose jobs are on a steeply declining portion of their opportunity cost schedule. In this case, the optimal wage increase is relatively small and, as the disemployed move down the opportunity cost schedule, it is applied to a steeply smaller outside option.

The basic implications of out theoretical approach are easily illustrated. The left-hand diagram in **Figure 1** shows the effect of a given union markup (i.e., wedge between the demand and supply curves) on wages and employment. The right-hand diagram repeats the exercise for a similarly sloped labor demand function, but a flatter labor market participation function: the impact of the same markup on wages and employment, relative to the competitive outcome, is larger. The right-hand side diagram is drawn so as to yield a relatively low *laissez faire* wage level, which is brought closer to the higher one of the left-hand side diagram by the union mark-up. Hence, unionization implies wage convergence and employment divergence between the two groups: disemployment is more pronounced in the right-hand diagram. It may be reflected in higher open unemployment, indicated by thick horizontal lines in the

[5] Recall that the market-level participation schedule reflects the distribution of non-employment opportunities across the population of workers; hence, its functional form reflects properties of that distribution as well as of each individual's utility function.

figure, if members of the flatter-supply group keep on seeking employment at the union wage rather than dropping out of the labor force, and into their relatively appealing non-employment options.[6]

Empirically, the same groups (skilled, prime age, males) that command high wages in an unregulated labor market are also those with relatively inelastic labor supply (Blundell and MaCurdy 1999). This fact is of course theoretically unsurprising. Relative to prime-age men, women are more likely to be making choices between home production and market work (in many cases both types of work), the elderly are more likely to be choosing between employment and retirement, and youth are more likely to be choosing between work and school.[7] In the context of our model, different elasticities of labor supply imply that uniformly larger wage markups (as implied by larger values of β) should be associated with different wage and employment impacts. Thus the model implies that, other things equal, unions will compress wages by age (for youth and for older workers too if under competition they would have earned less than the prime aged) and gender. For given labor demand elasticities, wage compression results in relatively large employment losses among young, elderly, and female groups with elastic participation schedules.[8]

The model assumes that a union worker who loses his/her job has no alternative employment available. This assumption may accurately characterize an encompassing union that negotiates a contract covering a country's entire workforce, a stylized view of Scandinavian or Austrian corporatism, and a perhaps not unreasonable fit with countries like Italy or France where collective bargaining coverage is extremely high, due in part to contract extension mechanisms whereby the union negotiated wages are extended to nonunion workers. At the opposite end of the spectrum is the United States: in our data for

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[6] In Bertola, Blau and Kahn (2002b), we show that the same employment results can be obtained if workers' representatives in government enact a labor tax whose proceeds are then spent on workers. In this case, the optimal tax leads to the same wedge between the marginal product of labor and the marginal willingness to work as the optimal union wage policy derived here, and disemployment leads secondary workers to exit the labor force rather than remain unemployed.

[7] See Agell and Lommerud (1997) for a formal model where minimum wages reduce employment opportunities for young individuals and induce them to enroll in education.

[8] The results are obtained viewing each labor force as a separately unionized group, within which incomes are supposed to be perfectly transferable. Intra- or intertemporal transfers of purchasing power across groups may, however, further support the outcome. For example, from each individual's perspective it is optimal to allocate periods of non-employment to early and late stages in the life cycle, when the value of alternative uses of time are high relative to productivity in formal employment. Moreover, even if all workers are in the same bargaining unit, the union can maximize total surplus by following a wage compression strategy.

1994, unions covered roughly 18% of American workers, and a disemployed union worker may well have had nonunion jobs available. Taking the U.S. case to its logical extreme, consider a union organizing a company in an otherwise completely competitive labor market (we assume the company has some monopoly power, so the union can survive). In this case, the union workers' opportunity cost is constant at the competitive wage and is perfectly elastic. In the context of our model, then, there is no reason for wage compression or relative disemployment of secondary workers in this economy (abstracting from differences across groups in bargaining power or the elasticity of labor demand). At the other extreme, if we have a completely unionized economy with a central wage bargain then the model presented above will apply, as the union maximizes the sum of group-specific objective functions in the form of (5), and we predict higher wages and larger employment losses for groups with elastic participation schedules. This reasoning implies that higher coverage by centralized collective bargaining institutions will lead to greater wage compression and greater relative disemployment of secondary workers, making this an appropriate empirical test of our model.

2.2 Can other theories explain relative-employment union effects?

Above we have argued that, in the context of a simple union model, realistic labor supply elasticity differences across demographic groups can significantly reduce employment of individuals other than prime-age males. Before interpreting our empirical results below as evidence of such phenomena, we need to argue that other plausible differences across groups and other models of union behavior cannot explain realistic empirical patterns.

Consider first how other group-specific parameters would affect employment outcomes in the context of our simple modeling perspective. Labor-demand elasticity, denoted η above, could in general be different across demographic groups. International data on demographically-disaggregated demand elasticities (or markups) are not available, and even in theory such parameters might in general depend on complementarity and substitutability relationships between groups of workers. However, any systematic variation of η across demographic groups would imply a larger employment impact for worker groups that are less easily substituted by non-labor factors of production, and these are likely to include predominantly prime-age males (Rosen, 1970). Obviously, a larger wage markup should be optimal for unions that organize worker groups with less elastic labor demand (see, for example, Farber 1986). The low demand elasticity of prime-age male labor also reduces the negative employment effect of any given wage increase; but, steeper labor demand endows the union with more monopoly power, implies a larger gain from restricting labor supply, and (as we show formally in Appendix A) implies larger employment declines. Thus, plausible differences in labor demand elasticity across demographic groups predict higher relative wages and lower relative employment for prime-age men than for other groups, the exact opposite of what one finds. Different union bargaining power (as parameterized by β) across groups has similar, and similarly unrealistic, implications for relative wages and employment. A larger β implies higher relative wages and lower relative employment: but to the extent that union bargaining power varies across demographic groups, as in Jimeno and Rodríguez-Palenzuela's (2002) theoretical model, we would expect it to be larger for better organized prime-age males. Again, the prediction is for unions to raise wages and lower employment more for prime-age men than for other groups, counter to what we observe.

Consider next the explanatory power of other models of union behavior. It has been argued that union members may favor wage compression for the purpose of *ex post* insurance (Agell and Lommerud, 1992). Risk averse workers agree to wage equalization *ex ante*, before knowing how their *laissez faire* wage will be affected by labor demand shocks. Wage compression may also serve the purpose of enhancing union solidarity - a public good from the union's point of view - among employed members (Kahn 1993).[9] These theoretical mechanisms are applicable to unions representing homogeneous pools of *ex post* employed workers, but cannot easily rationalize the phenomena we focus on. Considerable evidence suggests that labor market institutions such as collective bargaining compress wages across as well as within age and gender groups.[10] This paper's empirical results further suggest that loss of

[9] See also Bertola's, forthcoming, analysis of EPL's motivation and wage-differential effects which invokes financial market imperfections and Acemoglu et al (2001) who suggest that unions may redistribute income across workers with different skills in a model where *ex post* wage compression offers insurance and commitment benefits.
[10] For a survey, see Blau and Kahn (2002). A recent paper by Card, Lemieux and Riddell (2003) finds that within the US, the UK and Canada, unions reduce wage inequality among men with little effect among women. We note that much of the evidence cited by Blau and Kahn (2002) compares wage inequality in highly unionized countries such those in Scandinavia with that in less unionized countries such as those studied by Card, Lemieux and Riddell (2003). Thus, the conclusion that across countries, unions compress both men's and women's wages does not necessarily conflict with the evidence found by Card, et. al (2003).

employment is the price of relatively high wages for low-productivity individuals who are *ex ante* identifiable by their gender and age. Moreover, if the price of high wages is no employment, even *ex post* wage compression in the face of less predictable product-market or health shocks may not be as attractive to (*ex post*) low-productivity workers as insurance and solidarity views would make it.

Our model assumes that firms are on their labor demand curves, although it is well known that the parties can do better by jointly setting wages and employment in an efficient bargain, which will in general be to the right of the demand curve (McDonald and Solow 1981). However, there are also well-known enforcement problems associated with such bargains, caused by management's desire to move back to the demand curve, given the negotiated wages. The right to manage model is self-enforcing, since the employer chooses the quantity of labor demanded (Farber 1986). Thus, whether we in fact have efficient contracts is an empirical question, and it is worthwhile discussing the likely wage and employment outcomes for demographic groups under efficient contracts.

As discussed by McDonald and Solow (1981), efficient bargaining models yield contract curves-- efficient combinations of wages and employment-- and the actual position one arrives at on a contract curve is determined by relative bargaining power. McDonald and Solow (1981) study a variety of efficient bargaining models and conclude that the contract curve can be vertical (in the case of risk neutral workers), upward sloping (in the case of risk averse workers), or downward sloping (if the union pays unemployed workers a benefit that is less than wages by the money value of the disutility of employment). As noted earlier, it is likely that prime age males would have higher bargaining power than the other groups. If so, then none of these three possible models can explain higher wages and lower employment among the secondary labor force groups. First, if the contract curve is upward sloping, prime age males should have larger positive union wage and union employment effects, in contrast to the facts. Second, if the contract curve is vertical, again prime age males should have the largest wage effects and there should be no employment effect, an outcome also rejected by the data. Third, in the event of a downward sloping contract curve, prime age males should have larger wage effects but more negative employment effects than the other groups, the exact opposite outcome to what we observe.

Monopsony models are also unlikely to explain the observed demographic patterns of union wage effects. It is likely that prime age males have less elastic labor supply than that of other groups, suggesting that employer monopsony power should lower prime age males' wages by more than those of other groups. Suppose that unions serve to take away monopsonists' power by imposing the competitive wage and employment outcomes. Then prime age males should receive the largest raises under trade unionism, counter to the observed outcomes.[11]

Finally, raising wages of youth, older workers and women may also be a way for prime-aged males to reduce potential competition from such low wage workers. Lazear (1983) makes an analogous point in explaining why unions flatten age-earnings profiles. The desire to reduce competition from low wage workers has also been cited as a rationale for union support for living wage laws in the United States, which place a floor under wages paid to contractors with local governments (Neumark 2001). Our model without demand-side interactions suggests a complementary union rationale for boosting the wages of these groups (their more elastic participation schedules) and also highlights the relatively high value of non-employment to them (compared to the prime-aged and males). To the extent this is the case, the negative employment effects of union policies that price out low-wage labor become more socially acceptable.

3. Empirical evidence on relative employment outcomes

A maintained hypothesis of the empirical work below is that, as postulated in our theoretical model, unions compress wage differentials across demographic groups. Ideally we would like to explicitly test this hypothesis empirically. Unfortunately, the necessary time-series cross-section wage data by demographic group are not available. However, it is reassuring that much previous work has found that gender and youth-adult differentials in wages are significantly smaller in more unionized countries,[12]

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[11] Relative employment effects of counteracting monopsony for groups with different labor supply elasticities are, however, ambiguous. Using the supply and demand equations (1) and (2) and assuming that the unconstrained monopsonist maximizes profits, the effect of monopsony (vs. competition) on log wages is $-\varepsilon \ln(\varepsilon+1)/(\varepsilon+\eta)<0$, which becomes more negative as ε rises (i.e. as the labor supply elasticity falls). But the employment effect is $-\ln(\varepsilon+1)/(\varepsilon+\eta)<0$, whose derivative with respect to ε can be positive or negative.

[12] See Blau and Kahn (2002 and 2003), Kahn (2000) and references therein.

although there is no detailed evidence, to our knowledge, on the impact of unions on wage differentials between older and prime age individuals.

Existing evidence of institutional effects on demographic employment patterns is weak relative to that of wage differential effects (Blau and Kahn 2002). There is evidence from within-country studies of negative effects on low-skill employment from union intervention.[13] Studies comparing two or three countries with different levels of unionization, however, typically find it difficult to identify the less favorable employment opportunities for low-skill workers that might be expected to follow from wage compression, [14] perhaps reflecting their lack of explicit controls for country-specific factors.[15] Their evidence is hard to extrapolate to other countries and periods, and some of the existing more readily generalizable cross-sectional studies that pool data across a number of countries with different institutional arrangements also offer mixed evidence: for example, Nickell and Bell (1995) find little evidence of more pronounced relative unemployment increases for the less-educated in countries with more rigid labor markets. However Kahn (2000), analyzing data from 15 OECD countries over the 1985-94 period, finds that collective bargaining and coordinated wage-setting are not only negatively associated with age-related and education-related wage differentials, but also with the relative employment of the young (but not the less-educated). Similarly, Blau and Kahn (1996) find for the 1980s that, among men, the employment-population ratio of low skilled relative to middle skilled workers (defined by age and

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[13] See, e.g., Edin and Topel's (1997) study of Sweden's "solidarity bargaining" period of 1968-1983, and Kahn's (1998) study of the Norwegian 1987-91 wage-compression episode. In both cases, raising floors resulted in sharp employment declines for low-skill or low-education workers (and in low wage industries, on which see also Davis and Henrekson, 1997).

[14] For example, Card, Kramarz and Lemieux (1999) found that over the 1980s, relative wages were more rigid in France than in Canada, where in turn wages were less flexible than in the U.S. Yet, relative employment across skill levels changed similarly in all the three countries. Krueger and Pischke (1998) and Blau and Kahn (2000) similarly find that the wages *and* employment of low-skill German workers both changed more favorably than those in the U.S. over the 1980s. A study by Freeman and Schettkat (2000) of the U.S. and Germany from the 1970s to the 1990s found that the relative wages of low-skill men fell in the United States compared to Germany, while their relative employment fell in Germany compared to the U.S. But these effects were too small to account for much of the rise in the overall German unemployment rate compared to the U.S.

[15] Among the many country-specific features influencing employment outcomes, availability of public sector jobs for low-skill workers may play a particularly important role. See Blau and Kahn (2000) for a discussion of the German-U.S. case, Edin and Topel (1997) and Björklund and Freeman (1997) for evidence on Sweden, Kahn (1998) for the Norwegian case, and Algan et al (2002) for theory and evidence on the impact of public jobs on aggregate employment and unemployment.

education) was higher in the U.S. and the UK than in countries (Germany, Austria, Norway) with more highly unionized labor markets and more compressed wage structures.

In a recent paper Jimeno and Rodríguez-Palenzuela (2002) offer a formal panel-data study of demographically disaggregated labor market outcomes. However, they study only youth and prime-age relative unemployment rates and (assuming fixed institutions) do not control, as we do below, for country-specific effects in estimating the impact of institutions on relative employment. Finally, Neumark and Wascher (forthcoming) use a time-series cross-section panel of OECD countries to find that minimum wages lower youth employment, other things equal. We do not control for the strength of minimum wages since in our view this institution is strongly affected by the prevalence of unions both in collective bargaining and in affecting government policy. Accordingly, our findings for the impact of unionization can be interpreted as reduced forms including possible impacts through mandated as well as negotiated minimum wage levels.

The high variability of unemployment and employment-population ratios of youth, women and older individuals compared to prime-age males provides a strong empirical rationale for our focus on their labor market outcomes. And our approach based on market-wide (rather than gender or age-specific) institutional features has important methodological advantages for the purpose of assessing their relevance. In fact, focusing on the relative employment of subgroups makes it possible to formulate and test sharper predictions of the effects of labor market institutions than is the case for aggregate labor market indicators. Consider, for example, the impact of centralization of union wage setting. More centralized wage bargaining may or may not increase overall wages and unemployment, because the greater bargaining power associated with more extensive union coverage may be offset by wage restraint resulting from the union's awareness of macro-level wage effects (Calmfors and Driffill 1988). Centralized wage setting does, however, tend to cause some compression of the distribution of wages in practice (Blau and Kahn 2002), and such compression should unambiguously decrease the *relative* employment of low-productivity worker groups regardless of whether it decreases or increases each group's employment level. In this and other instances, theory has ambiguous implications for aggregate employment and unemployment rates, but offers sharp predictions on group-relative effects of labor market institutions.

Empirical testing of predictions about group-relative effects is also simpler than in the case of aggregate outcomes. Studying relative employment reduces the potential biases in cross-sectional studies due to omitted country-specific variables to the extent that they affect the employment of different groups in a similar way. Moreover, in our empirical work, we use time-varying institutional indicators, and this makes it possible to control for country effects that affect relative outcomes by influencing the various subgroups differently.[16] Lack of suitable instruments makes it impossible to control for endogeneity of institutions along cross-sectional or time-series dimensions (for example, the possibility that increasingly generous unemployment insurance is a response to high unemployment). However, such concerns may well be less important when one is examining relative employment or unemployment than their corresponding aggregates. Thus, for example, while labor market institutions may well be endogenous, studies of relative outcomes may suffer less from endogeneity biases than studies of absolute outcomes.

3.1. Cross-country outcome and institutional patterns: the data

The cross-country time-series data set available to us builds on that constructed and analyzed by Blanchard and Wolfers (2000). We draw variables pertaining to overall unemployment and some labor market institutions from the Blanchard-Wolfers dataset. We have added data on labor force by age groups, population by age groups, and unemployment rates by age groups for male and female workers separately. To smooth out short-run fluctuations, and in light of infrequent availability of institutional information, observations are arranged in 5-year intervals (1960-64 to 1990-94) along the time dimension; the last observation refers to the shorter 1995-96 interval. The countries included are Australia, Belgium, Canada, Denmark, Finland, France, Germany, Italy, Japan, the Netherlands, Norway, New Zealand, Portugal, Spain, Sweden, the United Kingdom, and the United States.

[16] For example, Nickell (1997, p.66-67) notes that most of the apparent employment effects of EPL are accounted for by low female employment-population ratios in Southern Europe – with no effect on prime-age males – and that the evidence may thus reflect cultural difference rather than policy effects.

Figure 2 illustrates what our model aims to explain, namely, cross-country patterns of relative changes in employment rates for prime-age vs. young and prime-age vs. older individuals (separately by sex) for the set of countries with complete observations in 1970-74 and 1995-96. The relative employment incidence of the prime aged rose in virtually every case (the only exception is the Canadian comparison of prime age and young men). On average, employment gaps between the prime aged and younger and older individuals rose by more in the other countries than in the United States, and in Continental European countries (such as Italy, France, and Spain) by more than in Anglo-Saxon countries. These contrasts are stronger for the youth-prime age than for the older-prime age comparisons.

As to explanatory variables, we included variables characterizing union influence on wage-setting, as well as additional labor market institutions. Of course, limited availability of comparable information and the small number of degrees of freedom afforded even by a comprehensive OECD data set make it impossible to include all indicators that could in principle be relevant to relative-employment outcomes.[17] However, our controls for a number of important institutions—including those that are standard in the literature—allow us to place a sharper interpretation on the unionization variables. Moreover, to the extent that the omitted regulatory policies that bear on demographic employment outcomes are affected by collective bargaining, they are, in principle, subsumed in the reduced-form effects of the unionization variables. .

Table 1 reports cross-sectional and time-series data on institutional arrangements for countries for which data are available in both 1970 and 1995 (see Appendix B for definitions and sources). The institutional variables most directly relevant to our theoretical arguments pertain to the extent and character of union wage setting. Theory indicates that greater union involvement in wage setting, as indexed by the model's parameter β, should concentrate employment losses on secondary workers.[18]

[17] One example of such an omitted variable is the availability of paid parental leave, which Ruhm (1998) finds increases women's relative employment, although it is associated with reductions in their relative wages at extended durations. Christopher Ruhm kindly provided us with the data on weeks of paid parental leave that he used in Ruhm (1998). Unfortunately, however, there was too little overlap between his data and ours in countries and periods covered to allow us to allow us to control for parental leave policies.

[18] Union power may also affect demographic employment patterns more directly by influencing which group(s) bear the brunt of layoffs. For example, unions may agree to downsizing on the condition that older workers are separated first (Casey 1992), or that the most recent (and younger) employees are laid off on a last-in-first-out basis.

Empirical proxies for this parameter can be found in the form of collective bargaining coverage and degree of coordination indicators, as well as union density measures. All three variables are available on a time-varying basis. As we see in Table 1, there was considerable variation across countries in *collective bargaining coverage* trends. Coverage fell sharply in the UK, with declines centered in the 1980s under the Thatcher program, and declined more moderately in five of the remaining countries, including the U.S. Coverage increased significantly in France and Spain and was fairly stable in the Scandinavian countries. Of course, coverage was much less extensive in the U.S. than elsewhere in both years. As to *collective bargaining coordination,* between 1970 and 1995 wage setting became less coordinated in Sweden, Australia and the UK, while increases in coordination occurred in Italy and France. The other countries were stable in this regard, and of course the U.S. had the lowest level of coordination, along with Canada. This measure of coordination is not entirely satisfactory, since it does not reflect the decentralization that has taken place in the U.S. since the 1980s (Katz 1993). Changes in *union density* were even more diverse, with membership as a percent of wage and salary employment rising by 9-28 percentage points between 1970 and 1995 in Spain, Sweden and Finland and falling by 8-13 percentage points in Australia, Japan, the UK, the U.S. and France. While union density might appear to be redundant once we know what fraction of workers are actually covered by collective bargaining contracts, a higher fraction of workers who are union members may enable unions to pose a greater threat to management, all else equal.

Summary statistics on other institutional indicators are also included in Table 1. We see that *labor tax rates* (defined on an average National Income Accounts basis, and including income and consumption tax revenues) rose in each country except Japan, with especially large increases in Italy, Spain and Sweden. France, Finland, Italy and Sweden had especially high labor tax rates as of the mid-1990s. Of course, in general labor taxes need not affect employment, as they may be shifted back to net wages when they are associated with benefits valued by workers.[19] However, such wage decreases may

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However, we prefer to focus on the more general effect identified by our theoretical perspective in interpreting the data and results.

[19] See e.g. Summers (1989) for a discussion of this and related points in the context of mandated employment-related benefits.

be impossible for workers at or near binding wage floors, particularly youth and possibly adult women as well.

Institutions other than wage setting and taxes would likely also play important roles in a dynamic context. More stringent employment protection (EPL) reduces employers' propensity to hire and terminate workers, with fairly obvious implications for employment patterns across demographic groups. In high-EPL markets, young labor market entrants and women with intermittent participation spells should be over-represented among the unemployed and underrepresented among the employed, who should in turn disproportionately include mature male workers with high labor market attachment. The data summarized in Table 1 indicate that changes in *employment protection* between 1970 and 1995 were somewhat diverse in this set of countries, increasing in France, Sweden and the UK but decreasing in Finland, Italy and Spain. By and large, the increases came in the 1970s, while the decreases came in the 1980s and 1990s. Employment protection in the U.S. remained stable, and the weakest among OECD countries.

More generous UI coverage has similar expected effects, to the extent that it increases the level of outside options in unions' bargaining strategies and the latter aim at wage compression. Thus, both greater employment protection and UI generosity are expected to raise the young-prime age employment-population ratio differential. In our data, *unemployment insurance* (UI) replacement rates are measured for the first year and the fifth year of unemployment. The former is a measure of generosity for most unemployed workers, while the latter is an indicator of the duration of benefits. On this basis, UI systems were on average more generous in 1995 than 1970. Exceptions were the UK, which lowered first and fifth year replacement rates and Japan, which lowered its first year replacement rate. It was during the 1970s that many UI systems became more generous. Changes were less positive in the United States than elsewhere.

Finally, retirement-related institutions should clearly impact the relative employment of older workers, and that of other groups for whom older workers are substitutes or complements. Table 1 shows data on changing characteristics of *retirement systems*. Basic replacement rates in these programs rose everywhere between 1970 and 1995, replacement ratios for special disability and unemployment schemes for older workers also rose on average. And 10-year accrual rates, the change in the replacement rate of retirement benefits for a 55-year old male who works an additional ten years, were constant at zero in some countries but fell by varying amounts in others, a change that reduced work incentives for older workers.[20]

To summarize, on average, the institutions shown in Table 1 appear to have become more interventionist in some countries relative to others between 1970 and 1995. The United States, the United Kingdom, and other countries displaying a lesser tendency to disemploy secondary labor force groups in Figure 2 also tend to display the least tendency to increase unionization and decrease work incentives in Table 1. To move beyond this impression, below we look more systematically at the relationship between changing institutions and employment outcomes of demographic groups in a regression context that makes it possible to control for other influences and exploit all available time-series and cross-section information.

3.2. Regression specification

On the basis of the simple theoretical considerations developed above, our empirical specifications seek evidence of relative employment or unemployment effects of union wage setting. We estimate equations of the following general form separately by sex for each of three age groups: 15-24, 25-54 and 55+ years old, where the age-sex groups are indexed by g:

$$\ln(e_{gjt}) = B_g{}'X_{jt} + a_{gj} + b_{gt} + u_{gjt}, \tag{11}$$

where for country j and period t, e is the employment-to-population ratio (which we sometimes refer to as the employment-population ratio), X is a vector of explanatory variables including the overall unemployment rate, births/population 15-24 years prior to the current observation, collective bargaining coverage, coordination of wage-setting, union density, an index of employment protection mandates, the first and fifth year UI replacement rates, the retirement system indicators shown in Table 1, and the

[20] Of the explanatory variables in our analysis, the retirement variables are perhaps the most likely to suffer from reverse causation. We nonetheless present results including them in order to provide a sharper test of the impact of the collective bargaining variables, our primary focus. Results for these variables were similar when the retirement variables were excluded.

average total labor tax rate (income plus payroll plus consumption taxes), a is a country effect, b is a period effect, and u is a disturbance term. In all models, we correct for the heteroskedasticity due to correlation of errors across observations for a country and for country-specific autocorrelation using a generalized least squares procedure.

Our theory suggests an impact of unionization on the relative employment of specific age-gender groups. This effect can be recovered from the parameter vectors B_g by differencing, for example, the effects of unions on the log employment-population ratios of prime age men and young men. Measuring relative employment effects in this way—i.e., in terms of differences in the log of employment-to-population ratios—is the appropriate metric here, as in the literature on the relative wage implications of demand and supply shifts (e.g. Katz and Murphy 1992) and as implied by the first order condition in our model.[21] However, rather than estimate a model with relative employment as the dependent variable, which would implicitly constrain the impact of the explanatory variables on the two comparison groups to be equal in absolute value, our estimating equations allow each variable to have a separate effect on the employment-population ratio of each age-gender group.[22]

We are primarily interested in ascertaining whether labor market institutions affect relative employment-population ratios of particular groups, as measured by employment-to-population ratios. However, variation in the dependent variable of equations like (11) reflects the different incidence across groups not only of unemployment but also of out-of-the-labor-force status, and labor market participation decisions are both theoretically interesting and policy relevant. Hence, we also estimate models of the form of equation (11) with the group-specific unemployment rate as the dependent variable. Freeman and Schettkat (2000) argue that in comparing unemployment rates over time and across groups, raw differences (rather than, for example, log differences) are the appropriate functional form. Note also that our employment equations aggregate the nonemployment states of school attendance, retirement, and

[21] As Katz and Murphy show, simple models of labor market substitution across demographic groups posit relative demand relationships of the form: $\ln(E_i/E_j) = Z - (1/\sigma)\ln(W_i/W_j)$, where for labor force groups i and j, E is employment, W is wages, Z includes other factors affecting relative employment, and σ is the elasticity of substitution between the two groups.

[22] In Bertola, Blau and Kahn (2002b), we estimated relative employment models with very similar results to those reported below.

household production. Below, we report on some results that provide a crude control for enrollment, which although endogenous with respect to labor market institutions, provide some indication on the importance of schooling in accounting for our results.

In equation (11), we control for overall unemployment and demographic factors, as well as institutional variables, country effects and period effects. To the extent that the aggregate unemployment rate effectively controls for macroeconomic factors, this specification provides a sharp test of the relative employment hypotheses discussed earlier. Specifically, we expect overall unemployment to have a positive effect on the young-prime age employment-population ratio gap: due to downward wage rigidity, unemployment is likely to be concentrated on relatively low-productivity individuals, and the young are likely to be at the end of a queue of individuals looking for work. If we did not control for macro-level unemployment, then any observed association between institutions and relative youth employment could be due to the effects of institutions on overall unemployment rather than to the kind of union relative employment effects we have highlighted above. Moreover, the prime age-older employment gap is also likely to be positively affected by overall unemployment to the extent that retirement systems can be used to reduce the employment of older workers in a recession. Overall unemployment is less likely to raise the male-female employment gap because women are less likely to be employed in cyclically sensitive sectors than men, although they are more likely than men to be discouraged workers (Blau, Ferber and Winkler 2002).

Alternatively, it could be argued that results controlling for overall unemployment do not fully capture the effects of institutions, since institutions can also affect overall unemployment which in turn influences relative employment. Moreover, a specific mechanism whereby unions could raise aggregate unemployment is by maintaining relatively high wages for low-productivity groups in the face of adverse economic shocks (see Blanchard and Wolfers 2000; and Bertola, Blau and Kahn 2002a). Such a mechanism is quite consistent with the implications of our theoretical model. Thus, we also estimated models with the overall unemployment rate excluded, in effect estimating the total impact of institutions on relative employment or relative unemployment rates.

We include births/population 15-24 years prior to the current observation to control for the relative supply of youth (see Korenman and Neumark 2000, and Jimeno and Rodríguez-Palenzuela 2002). At a given aggregate unemployment rate, a large cohort of young people is expected to cause a deterioration in their labor market prospects and thus lower the employment-population ratio of the young relative to the prime-aged. We use prior births/population rather than current youth population share because the former is less likely to be affected by current labor market conditions through migration, and is therefore more likely to be exogenous with respect to current employment outcomes. It is not possible to control similarly for other groups' population shares because birth rate data 25-54 or 55+ years prior to the current observation are not available. Finally, we note that many of the institutional indicators are correlated with each other, potentially making it difficult to obtain significant findings. Their inclusion in the model simply serves to address possible concerns that our empirical assessment of the wage-setting effects of our theoretical model might be distorted by omission of correlated institutional features.

Note that Equation (11) analyzes employment, and we would ideally also like to study relative wages, in line with the model outlined above. Unfortunately, as noted above, lack of time-series cross-section wage data by demographic group precluded a comparable analysis for relative wages, although it is reassuring that the existing literature strongly supports the existence of such effects for youth and women (older workers have not been studied). We also note that union power may additionally affect demographic employment patterns more directly by influencing which group(s) bear the brunt of layoffs. For example, unions may agree to downsizing on the condition that older workers are separated first (Casey 1992), or that the most recent (and younger) employees are laid off on a last-in-first-out basis. However, we prefer to focus on the more general effect identified by our theoretical perspective in interpreting the data and results.

3.3 The empirical impact of unionization on relative employment outcomes

Table 2 reports basic regression results for employment as the dependent variable, and **Table 3** the results of similar regressions of unemployment.[23] Some of the coefficients on the control variables are statistically and economically significant, and deserve to be briefly discussed. We see that in each specification the effects of the overall unemployment rate on the dependent variable are larger in absolute value for youths than for adults, reflecting the greater cyclical sensitivity of youth employment. Employment protection is found to raise youth unemployment relative to that of adults. Moreover, a larger potential youth cohort (prior births/population) raises youth unemployment and lowers youth employment, although the latter effects are insignificant. The fact that the births variable has more negative effects for employment (and more positive effects for unemployment) for youths than for adults suggests cohort crowding and imperfect substitution between youth and adults (see also Korenman and Neumark 2000). And several of the retirement variables have the anticipated effects, including positive effects of retirement ages and a negative effect of the older worker UI replacement rate on older male employment.[24]

In the empirical specification, union involvement in wage setting is measured by collective bargaining coverage, coordination, and union density.[25] Inspection of Tables 2 and 3 shows that, in some

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[23] We implemented unit root tests for our panel using a method suggested by Maddala and Wu (1999). Because of our short panel, usually seven periods, we interpret these results very cautiously. To test for unit roots, we computed Dickey-Fuller statistics for each country and their associated significance levels, using the approximations in MacKinnon (1994). We then implemented the suggestion of Maddala and Wu (1999) to aggregate these individual country tests using an exact Fisher test, under which –2 times the sum of the logs of the significance levels has a chi-squared distribution with degrees of freedom equal to two times the number of countries. We accepted the null hypothesis of a unit root for most of our variables under at least one of MacKinnon's (1994) approximations. We then repeated the process on the residuals from each of the basic regression models and in each case rejected the null hypothesis of no cointegration (albeit not taking into account the fact that the residuals are themselves estimated variables due to the short panels). Thus, under these tests, we reject the hypothesis of spurious regression across our time-averaged observations.

[24] Our results for the retirement variables are partially consistent with those of Blöndal and Scarpetta (1999), who examined the labor force participation rate of men 55-64 for 15 countries for the 1971-95 period. We do not discuss their results in detail here because we have a different set of dependent variables and a more extensive set of controls for labor market institutions than in their paper, as well as a considerably different focus.

[25] As explained in Appendix B, for countries for which the first period we observe coverage is, say, t_0, we assign the t_0 value to all prior periods. Our basic results were the same when we included a dummy variable for these observations.

cases, all three union variables have effects in the same direction (e.g,. all negatively affect the employment-population ratio of older men), while in other cases, they have conflicting signs (e.g., for young men, coverage and coordination have negative effects on the employment-population ratio, while union density has a positive coefficient). It is not surprising to find some perversely signed estimates for the coefficients on the union-power indicators. The three union variables offer admittedly imprecise measures of similar aspects of the institutional environment (the correlation is 0.360 for union density and collective bargaining coverage; 0.210 for density and coordination; 0.360 for coverage and coordination).

In light of such multicollinearity, we evaluate the influence of these indicators as a group, using the regression coefficients to predict the change in employment or unemployment which would occur if all the union-related variables were simultaneously changed by one standard deviation within or between countries. Using within-country standard deviations produces a change that is in spirit similar to the regressions themselves, which include country dummies and therefore use within-country variation in the explanatory variables to test their impact. The within country standard deviations in our sample are 5.28 percentage points for collective bargaining coverage, 7.24 percentage points for union density, and 0.157 for coordination. On the other hand, using between-country standard deviations of the unionization variables tells us the impact of long-run differences across countries in wage-setting institutions. These between country differences are larger than those within nations: 23.51 percentage points for coverage, 18.51 percentage points for density, and 0.599 for coordination. Across countries, then, differences in institutions are more dramatic than are changes within countries over time.

Table 4 shows the impact of these one standard deviation changes in the unionization measures on employment-population ratios and unemployment rates. Looking first at results that control for the overall unemployment rate, we see that, with the exception of results for prime age men, unionization lowers employment-population ratios, with most of these effects being statistically significant. Effects on group specific unemployment rates are mixed, however, with positive effects obtained for prime age and older women and negative effects for all groups of men and for younger women. Estimated effects are of course larger in absolute value for the between country unionization simulation, due to the larger differences in standard deviations across (than within) countries.

For the reasons discussed above, we also present results excluding the overall unemployment rate, which allows us to observe the sum of the direct union effects (controlling for the aggregate unemployment rate) and the indirect union effects (via the union impact on the aggregate unemployment rate). The employment effects in this specification are almost always more negative, and the unemployment effects are always more positive than in the model with the overall unemployment rate included. Moreover, the estimated union effects in the models excluding the unemployment rate are always in the expected direction (negative for employment and positive for unemployment) and are statistically significant in 23 of 24 cases.

Table 5 shows the implied effects of the parameter estimates presented in Tables 2 and 3 on the key relative employment and unemployment concepts our theory emphasizes. Looking first at relative employment, the results indicate that, as our theory predicts, unionization raises the employment-population ratio gaps for each of our comparisons in every specification: prime age vs. young individuals, prime age vs. older people, and men vs. women. Moreover, the effects are statistically significant 15 out of 24 times, with especially strong effects in specifications excluding the unemployment rate. To provide an indication of the magnitudes of these estimates, **Table 6** shows the impact on relative employment of these changes in unionization divided by the within or between country standard deviations of the group differences in the relative employment measures. The magnitudes of these effects range from modest to sizable, depending on the group, specification and the size of the unionization changes at which the effects are evaluated. Specifically, larger effects are obtained for the age comparisons (i.e., youth and older individuals relative to the prime aged), the specifications excluding the unemployment rate and the evaluation of the unionization change using the between country standard deviation. So, for example, in specification II, which does not control for unemployment, an increase of one between country standard deviation in all the unionization variables raises the relative employment gaps by age by 65-123% of the relevant between country standard deviation in relative employment. In contrast, when we control for unemployment in specification I and use within country unionization changes, relative employment gaps by age rise by only 2-27% of the within country standard deviations of relative employment. The impact of unionization on male-female employment gaps is generally smaller than for the comparisons by age, ranging from 10-29% of the relevant standard deviation of the gender gap in employment.

From our theoretical perspective, more valuable alternative uses of time for nonemployed youth, older individuals and women than for prime-age men provide the rationale for the wage compression and employment displacement predictions of our model. To the extent that the nonemployed take advantage of such opportunities, and drop out of the labor force, it is not surprising to see that the results for relative unemployment are not as clear-cut as those for relative employment. The one consistent finding is that unionization significantly lowers prime age male vs. prime age female unemployment in every case, with effects ranging from 0.76 to 3.16 percentage points. These are sizable relative to the within and between country standard deviations of the male-female unemployment rate gap of 1.9 to 2.1 percentage points. An additional notable finding is that in models excluding the overall unemployment rate unionization significantly lowers the prime age male vs. the young male unemployment rate, by 1.1 to 3.5 percentage points. Again, these are sizable relative to the within country and between country standard deviations for these variables of 3.6 to 4.0 percentage points. However, the unionization effects are positive and insignificant when the overall unemployment rate is included. Unions thus appear to raise young men's relative unemployment rate mainly through their effect on the overall unemployment rate.[26]

The models discussed so far have a simple linear structure in which, for reasons of parsimony, only main effects of each variable appear. However, we might expect recessionary overall macroeconomic conditions to have more severe negative relative employment effects on youth, for example, the more rigid relative wages are with respect to economic conditions. Blanchard and Wolfers (2000) use this logic to predict that rigid labor market institutions should have more negative overall employment effects during recessions than during expansions. To some extent, the interactions between shocks and institutions emphasized by Blanchard and Wolfers (2000) in their analysis of overall unemployment are already subsumed in the unemployment rate, which serves as a control here.

[26] In Bertola, Blau and Kahn (2002b) we used a less flexible specification, constraining the impact of each institutional variable to be of equal magnitude and opposite sign. The results were similar to those reported here, except that there was little union effect on male-female employment differentials but a stronger union effect on female employment differentials for prime age vs. young individuals.

Employment institutions such as seniority-based layoffs, however, may also produce larger prime age vs. youth employment differentials when there is a recession than when there is an expansion. Hence, we ran all of our models with interactions between overall unemployment and the other variables in the equation other than the year and country dummies. These models place a considerable burden on the data, since there are 52 coefficients and only 101 observations.[27] Nonetheless, the interactions were jointly significant in each case, although there were many conflicting signs. Appendix Table A1 shows the interaction effects of unemployment and changing the unionization variables by one standard deviation. Each entry is the change in the impact of a one standard deviation increase in the unionization variables when the unemployment rate rises by one percentage point. The results are mixed. On the one hand, with between country standard deviation changes in unionization (which are larger than the within country changes), union effects on employment are usually more negative and effects on unemployment are usually more positive when the overall unemployment rate is higher, as expected. Several of these effects are statistically significant. And unionization lowers the relative employment of younger and older workers relative to the prime aged by more during recessions than during expansions, although these age comparisons are not statistically significant. On the other hand the expected relative unemployment rate effects are generally not observed. Moreover, using within country changes in unionization, only the relative employment effects for younger vs. prime age men go in the expected direction. Thus, there is some, albeit weak, evidence that unionization has more negative employment effects on younger and older individuals during periods of higher unemployment.

3.4 What else could explain the empirical impact of unionization on relative employment?

We have explored several alternative specifications that allow us to see whether potential competing hypotheses might explain our empirical results. For example, it is possible that the measured overall unemployment rate, one of our key control variables, is itself affected by the demographic composition of the population. Thus, in models not shown here, we replaced the raw unemployment rate with one that

[27] One might also speculate that cohort crowding effects would be larger with more rigid labor markets (Korenman and Neumark 2000). However, insufficient degrees of freedom prevent us from pursuing this further possible source of interaction effects.

was corrected for demographic composition. For each country-period observation, we took a weighted average of the unemployment rates for the following demographic groups: men age 15-24, men age 25-54, men age 55+, women age 15-24, women age 25-54, and women age 55+. We constructed a corrected unemployment rate by using the same weights for each demographic group for each country-period observation based on the average for the 16 country sample of 1980 observations. The results were very similar to the ones reported above.

An additional compositional issue of possible concern is that the key unionization variables which are measured at the national level may reflect different levels of coverage across demographic groups. If prime age males are more likely than other groups to have centralized wage-setting, be covered by collective bargaining, and be union members, then our findings may reflect reverse causality from the composition of employment to the institutional variables: under this scenario, when younger or older individuals or women increase their employment share, the values of the union-related variables will decrease. Since we control for country effects, the composition argument must refer to within-country changes in the institutions and in relative employment in order to be valid. However, it is highly unlikely that different levels unionization across demographic labor force groups could explain our findings.

First, in our data, the major changes in coordination and collective bargaining coverage in many cases reflect overall government decisions or union strategies regarding wage setting rather than compositional changes in the labor force. These include episodes such as the Thatcher program in the UK, the Employment Contracts Act in New Zealand, and the solidarity wage period and subsequent decentralization in Sweden. Moreover, the decline in unionization in the United States has occurred within industries and within-demographic groups (Blau and Kahn 2002; Farber and Krueger 1993).

Second, using microdata from the 1985-98 International Social Survey Programme (ISSP), we investigated changes in union density in the countries included this paper for which ISSP data were available (this resulted in an unbalanced panel containing all 17 countries except Belgium and Finland). We estimated two linear probability models of union membership among employed workers separately for each country, pooling data for all years for which ISSP data were available. The first model included only a time trend, while the second model augmented the time trend with age-gender dummy variables corresponding to the six groups studied here. We found a 98.3 percent cross-country correlation between the union density time trends not accounting for demographic composition and the time trends from equations controlling for demographic composition. In other words, trends in overall union density were virtually perfectly correlated with trends in union density controlling for the demographic composition of employment. While the data are not available to construct a demographically-adjusted measure of union density for our full sample period, this analysis of the ISSP data suggests that had we been able to employ a demographically-adjusted measure of union density in our analyses above, the results would have been quite similar.

A final issue we considered concerned school enrollment. We interpret the stronger effects we find for youth employment than youth unemployment as suggesting that unions price young people out of work and thereby lower the opportunity cost of schooling, leading to labor force exits.[28] Alternatively, it may be that unmeasured propensities to be enrolled in school are correlated with our unionization measures, leading to a potential spurious negative correlation between unionization and youth employment. We examined this question directly by using World Bank World Tables data on gross secondary and tertiary enrollment rates for our sample of countries and time periods, interpolating where necessary. These are defined as total secondary or tertiary school enrollments divided in each case by what the World Bank considers to be the target age group population and are thus indicators of relative enrollment rates. We estimated models similar to equation (11) for the determinants of these enrollment rates and, in each case, found positive although insignificant effects of unionization on enrollment. However, including these (admittedly endogenous) enrollment rates in our basic employment and unemployment equations did not affect the estimated union impacts. Thus, our youth employment and unemployment effects are not accounted for by school attendance.[29]

[28] A similar argument applies to the results for older individuals, with retirement being the alternative to employment in their case, and motivates inclusion of retirement-system characteristics alongside unionization indicators in our employment and unemployment equation.

[29] These results are consistent with Kahn's (2000) findings for a cross section of 15 OECD countries. He found that collective bargaining coverage had a negative effect on youth relative employment and was positively associated with school attendance among young adults, but that enrollment did not fully account for the negative effect of union coverage on the relative employment of youth. Taking Kahn's findings in conjunction with those reported above suggests that unions may increase the share of out-of the-labor force youth who are neither at work nor at school.

4. Conclusion

In this paper we have investigated the impact of labor market institutions on the relative employment of labor market subgroups. We pointed out that the effects of institutions on different groups' employment may be taken into account by unions and fine-tuned so as to concentrate reduced employment opportunities on individuals who can find good uses of their time outside of employment. Our empirical approach controls for country-specific fixed effects and macroeconomic and demographic conditions. The results suggest that countries where union wage-setting institutions exert a more pervasive influence on labor market outcomes tend to feature relatively low employment levels among the young, older individuals, and women, and relatively high unemployment rates among prime age women and young men, while preserving high employment-population ratios for prime age men. The lack of evidence of union effects on unemployment for young women and older individuals suggests that disemployed individuals in these groups move predominantly into non-labor-force (education, home production or retirement) states.

These patterns are fully consistent with the model we have proposed here, where rent-extracting unions purposely negotiate the largest wage premiums for groups with the most elastic labor supply because employment losses are less costly for those with alternatives that are nearly as good as paid employment. The empirical regularities are much less consistent with a number of alternative models of union behavior that we have briefly reviewed. Without denying the validity of alternative views of unions' role and objectives, our model contributes by highlighting the relatively high value of non-employment for some groups of low-wage workers. Such demographically biased negative employment effects of union policies are also likely to be more socially acceptable than employment losses among prime age males would be.

Appendix A: Theoretical effect of demand elasticity on the union impact on employment

Equation (7) shows the ratio of the log of union to nonunion wages (referring to group i and suppressing the subscript i):

$$\mathrm{Log}(W_u/W_n) = \{\eta/(\varepsilon+\eta)\} \log [1/(1-\eta+\eta/\beta)],$$

where η is the inverse labor demand elasticity ($0 < \eta < 1$) and β is the union bargaining power parameter ($\beta > 1$). And equation (8) shows the ratio of the log of union to non-union employment:

$$\log(L_u/L_n) = \log(1-\eta+\eta/\beta)/(\varepsilon+\eta)$$

where ε is the inverse labor supply elasticity and is positive. Denote $\log(L_u/L_n)$ by r. Taking the derivative of r with respect to η, we have:

$$dr/d\eta = (\varepsilon+\eta)^{-2}\{(\varepsilon+\eta)(-1+1/\beta)(1-\eta+\eta/\beta)^{-1} - \log(1-\eta+\eta/\beta)\}. \tag{A1}$$

To simplify this expression, define:

$$z \equiv \eta - \eta/\beta \tag{A2}$$

where, $0<z<1$, since $0<\eta<1$ and $\beta>1$. Substituting (A2) into (A1), we have:

$$\begin{aligned} dr/d\eta &= [\varepsilon+\eta]^{-2}\{(\varepsilon+\eta)(-1+1/\beta)(1-z)^{-1} - \log(1-z)\} \\ &= [\varepsilon+\eta]^{-2}\ \{\varepsilon(-1+1/\beta) + z(z-1)^{-1} - \log(1-z)\} \end{aligned} \tag{A3}$$

From the concavity of the log function, we have $0>\log(1-z)/z > (z-1)^{-1}$, and , since $\varepsilon(-1+1/\beta)$ is negative it follows that $dr/d\eta$ is negative: the less elastic labor demand is, the higher the union wage markup is and the lower is union employment relative to nonunion employment. Intuitively, with more monopoly power, the gain to restricting labor supply is greater.

Appendix B: Data sources and definitions

This paper's data set is based on that constructed by Blanchard and Wolfers (2000), documented at http://econ-wp.mit.edu/RePEc/2000/blanchar/harry_data/. The data set contains macroeconomic and institutional data on 26 OECD-countries for 8 five-year periods covering the time span 1960-1999. We have added data on labor force by age groups, population by age groups, and unemployment rates by age groups for male and female workers separately.

Demographic disaggregration of employment and unemployment:

The labor force and population data are taken directly from the ILO database "Economically Active Population 1950-2010". The data on unemployment rates by age group were constructed from data found in the OECD-publication *Labour Force Statistics* (various issues). These are country-source data, and we did not attempt to harmonize their definition. To compute the average unemployment rate for each 5 year period we calculate the arithmetic mean of the yearly unemployment rates within the period. To obtain similar data on as many countries as possible, we also aggregate the data to broad age groups by computing the labor force weighted average of the time-averaged unemployment rate of the relevant age groups. The labor force weights themselves are constructed as linearly interpolated weights from the labor force data used above.

Institutional indicators:

Time-varying employment protection legislation indicators are from the Blanchard and Wolfers (2000) dataset.

Union density, collective bargaining coverage and coordination, labor tax rates are from the data appendix to Nickell, Nunziata, Ochel and Quintini (2003), kindly attached by the authors to the Discussion Paper version of their study at http://cep.lse.ac.uk/papers/. Collective bargaining coverage was available for some countries from 1960 to 1999 and for other countries from 1980-94. We used interpolation and assigned the authors' earliest figure to all dates before its date.

The UI year 1 and year 5 replacement rates were taken from a OECD database, and were available for the entire 1960-96 period.

Data on retirement system characteristics were interpolated from those available in from Blöndal and Scarpetta (1999): Male and Female retirement ages in 1961, 1975, 1995 from Table III.1; 10 Year pension accrual rate in 1967 and 1995 from Table III.4; Pension Replacement Rates for 1961, 1975, and 1995 from Table III.3; Disability and Unemployment Special Scheme Replacement Rates for 1961, 1975, and 1995 from Table IV.3.

Individual union membership data were taken from the International Social Survey Programme (ISSP) for 1985-1998.

Data on enrollment in education were taken from the World Bank's 1995 CD edition of the World Tables.

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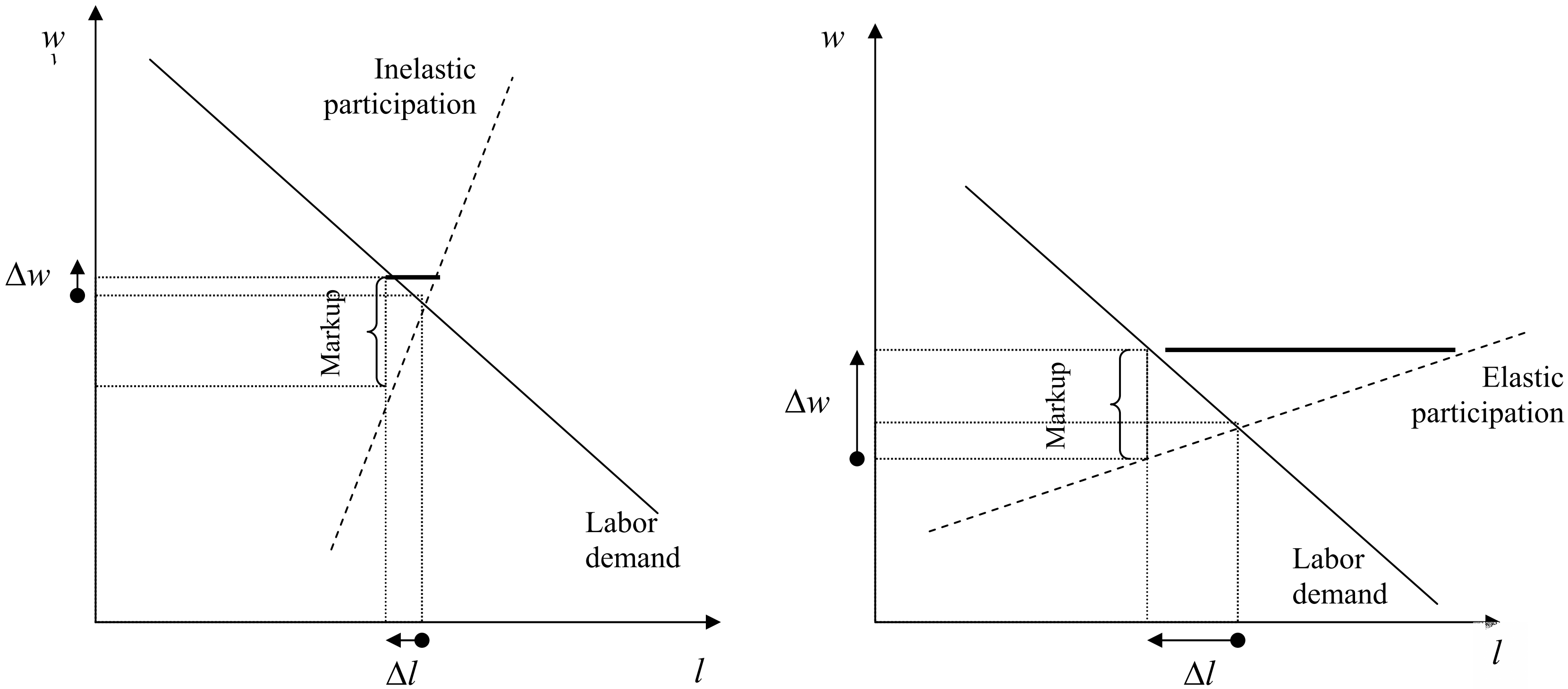
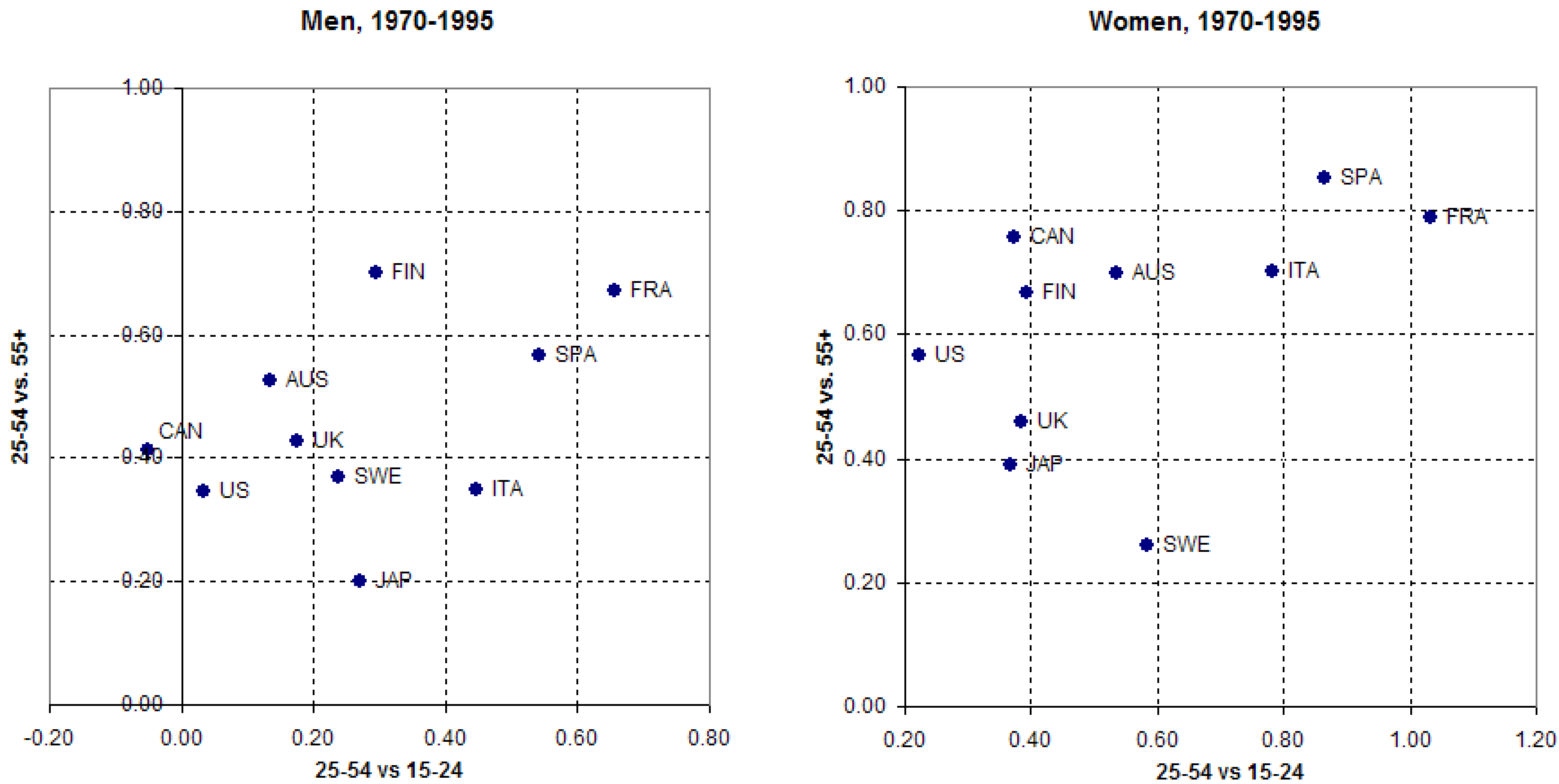


Figure 1: The different effects, relative to *laissez faire*, of the same union markup for groups with differently sloped market participation schedules.

Figure 2: Changes Over Time in Relative Employment-to-Population Ratios Across Age Groups



Country-specific changes, across the 1970-74 and 1995-96 periods, in the difference in the log of employment rates across the indicated age groups.

Table 1: Institutional Patterns in Selected Countries, 1970-1995

	Coll. Barg. Coverage [1]		Coordination		Union Density		Labor Tax Rate		Emp. Protection Index		UI Rep. Rate: First Year	
	1970	change 70-95	1970	change 70-95	1970	change 70-95	1970	change 70-95	1970	change 70-95	1970	change 70-95
AUSTRALIA	85.0	-5.00	2.25	-0.75	43.37	-8.17	32.18	7.82	1.00	0.00	0.12	0.09
CANADA	40.0	-4.00	1.00	0.00	30.62	6.78	42.44	9.56	0.60	0.00	0.49	0.09
FINLAND	95.0	0.00	2.25	0.00	51.30	28.30	51.69	12.31	2.40	-0.30	0.29	0.35
FRANCE	85.0	11.00	1.75	0.25	21.70	-11.80	57.91	10.09	1.97	1.13	0.47	0.08
ITALY	85.0	-3.00	1.50	1.00	37.00	1.70	55.71	15.29	4.00	-0.60	0.04	0.11
JAPAN	28.0	-7.00	3.00	0.00	31.74	-7.94	25.88	-1.88	2.80	0.00	0.41	-0.12
SPAIN	68.0	10.00	2.00	0.00	9.00	9.20	25.91	20.09	4.00	-0.90	0.38	0.27
SWEDEN	86.0	3.00	2.50	-0.50	66.76	23.22	59.47	14.53	1.20	1.20	0.24	0.49
UK	70.0	-32.00	1.50	-0.50	49.80	-13.10	43.19	3.81	0.58	0.12	0.31	-0.13
USA	27.0	-10.50	1.00	0.00	27.24	-12.34	40.06	5.94	0.20	0.00	0.20	0.07

	UI Rep. Rate: Fifth Year		Retirement Benefits Wage Rep. Ratio		Rep. Rate, Older Workers, Disability		Rep. Rate, Older Workers, UI		10-Yr Pension Accrual Rate, Men Age 55[2]	
	1970	change 70-95	1970	change 70-95	1970	change 70-95	1970	change 70-95	1970	change 70-95
AUSTRALIA	0.12	0.10	0.30	0.11	0.20	0.08	0.21	0.06	0.00	0.00
CANADA	0.10	0.00	0.42	0.09	0.22	0.11	0.16	0.01	0.19	-0.19
FINLAND	0.10	0.06	0.54	0.06	0.46	0.14	0.30	0.34	0.09	-0.05
FRANCE	0.07	0.06	0.60	0.05	0.50	-0.25	0.38	-0.15	0.24	-0.07
ITALY	0.00	0.00	0.62	0.18	0.48	0.12	0.25	0.49	0.22	-0.12
JAPAN	0.00	0.00	0.48	0.04	0.16	0.09	0.04	-0.01	0.05	-0.02
SPAIN	0.00	0.00	0.50	0.50	0.55	0.16	0.42	-0.05	0.00	0.00
SWEDEN	0.00	0.00	0.72	0.02	0.74	0.00	0.12	0.03	0.17	-0.17
UK	0.16	-0.03	0.34	0.16	0.33	-0.05	0.19	-0.02	0.02	0.08
USA	0.04	0.00	0.47	0.09	0.38	0.07	0.06	0.00	0.00	0.00

[1] Due to data availability, data shown for Sweden are 1990 data for 1970 and the average of 1990 and 1994 data for 1990. As explained in Appendix B, for countries for which the first period we observe is, say, t_0, we assign the t_0 value to all prior periods. Our basic results were the same when we included a dummy variable for these observations.

[2] Increase in Retirement Benefit Replacement Rate for a 55-year old male who works 10 more years.

Table 2: Generalized Least Squares Regression Results for Employment

Explanatory Variables	log(epop men1524)		log(epop men 2554)		log(epop men 55+)		log(epop women1524)		log(epop women 2554)		log(epop women 55+)	
	Coeff	Std Err	Coeff	Std Err	Coeff	Std Err	Coeff	Std Err	Coeff	Std Err	Coeff	Std Err
overall unemployment rate	-2.260	0.242	-0.710	0.052	-1.128	0.233	-3.183	0.278	-1.118	0.375	-0.696	0.394
prior births/population	-3.468	5.041	-0.751	0.500	2.516	3.293	-3.048	5.164	1.005	5.006	8.990	4.316
coll barg coverage	-0.003	0.001	0.0004	0.0002	-0.003	0.001	-0.002	0.001	0.002	0.001	-0.004	0.002
coordination	-0.068	0.045	0.006	0.007	-0.025	0.030	-0.114	0.049	-0.019	0.043	-0.139	0.048
union density	0.002	0.001	0.001	0.000	-0.004	0.001	0.001	0.001	-0.003	0.002	-0.006	0.002
employment protection	0.011	0.017	0.004	0.003	-0.048	0.014	-0.020	0.017	0.014	0.019	0.033	0.026
UI rep rate: year 1	0.076	0.061	-0.007	0.010	0.065	0.036	0.270	0.068	0.204	0.066	0.400	0.079
UI rep rate: year 5	0.075	0.119	-0.020	0.023	0.141	0.075	-0.029	0.116	-0.111	0.109	0.285	0.090
labor tax rate	-0.309	0.223	0.035	0.026	0.303	0.144	-0.471	0.203	-0.040	0.211	-0.211	0.219
public pension replacement rate	-0.002	0.002	-0.0001	0.0003	0.0004	0.001	0.003	0.002	0.004	0.002	0.001	0.002
accrual rate, 10 yrs, age 55	0.002	0.003	0.001	0.001	-0.013	0.002	-0.002	0.004	-0.007	0.003	-0.015	0.003
UI rep rate: older workers	-0.164	0.100	0.021	0.014	-0.262	0.067	-0.113	0.120	-0.017	0.141	-0.148	0.121
Disability rep rate: older workers	0.011	0.366	-0.038	0.050	-0.179	0.235	-0.193	0.424	0.281	0.328	0.085	0.385
female retirement age	0.031	0.012	0.007	0.001	0.033	0.006	0.019	0.013	0.002	0.010	0.011	0.010
male retirement age	0.030	0.019	-0.003	0.002	0.026	0.009	0.040	0.022	-0.003	0.018	0.010	0.019
country dummies	yes		yes		yes		yes		yes		yes	
period effects	yes		yes		yes		yes		yes		yes	
sample size	101		101		101		101		101		101	

Notes: Standard errors are corrected for country-specific heteroskedasticity and country-specific first order autocorrelation. Accrual rate is the change in the retirement replacement rate if a 55 year old works an additional ten years.

Table 3: Generalized Least Squares Regression Results for Unemployment

Explanatory Variables	u rate men 1524		u rate men 2554		u rate men 55+		u rate women 1524		u rate women 2554		u rate women 55+	
	Coeff	Std Err	Coeff	Std Err	Coeff	Std Err	Coeff	Std Err	Coeff	Std Err	Coeff	Std Err
overall unemployment rate	1.791	0.109	0.707	0.037	0.822	0.056	2.223	0.122	0.765	0.052	0.529	0.073
prior births/population	4.066	1.234	-0.145	0.339	-2.066	0.582	4.081	1.714	2.320	0.487	-0.421	0.577
coll barg coverage	-0.0003	0.0002	-0.0001	0.0001	0.0001	0.0001	-0.0003	0.0003	0.0008	0.0002	0.0004	0.0002
coordination	0.014	0.013	-0.002	0.005	-0.019	0.005	0.025	0.018	-0.002	0.007	-0.008	0.008
union density	-0.0012	0.0004	-0.0003	0.0001	0.0000	0.0002	-0.0007	0.0004	0.0002	0.0002	0.0006	0.0002
employment protection	0.018	0.008	0.001	0.003	0.001	0.002	0.022	0.009	0.006	0.003	0.003	0.003
UI rep rate: year 1	-0.030	0.021	0.003	0.007	-0.010	0.007	-0.030	0.024	-0.033	0.009	-0.030	0.011
UI rep rate: year 5	0.032	0.047	-0.010	0.016	-0.010	0.010	-0.037	0.046	-0.079	0.022	-0.037	0.021
labor tax rate	-0.064	0.066	-0.014	0.020	0.135	0.023	-0.361	0.075	-0.095	0.029	0.026	0.029
public pension replacement rate	-0.0020	0.0006	-0.0003	0.0002	-0.0021	0.0003	0.0018	0.0007	0.0022	0.0003	-0.0005	0.0004
accrual rate, 10 yrs, age 55	-0.0010	0.0011	-0.0001	0.0003	0.0017	0.0004	-0.0047	0.0015	-0.0010	0.0004	0.0014	0.0006
UI rep rate: older workers	-0.005	0.033	-0.003	0.011	0.011	0.013	0.079	0.040	0.053	0.013	0.061	0.019
disability rep rate: older workers	0.284	0.111	0.040	0.037	0.087	0.046	0.147	0.138	-0.006	0.045	0.118	0.054
female retirement age	-0.011	0.003	-0.004	0.001	-0.003	0.001	-0.006	0.004	0.001	0.001	-0.001	0.002
male retirement age	0.002	0.005	0.001	0.002	-0.001	0.002	-0.007	0.007	-0.004	0.002	-0.009	0.003
country dummies	yes		yes		yes		yes		yes		yes	
period effects	yes		yes		yes		yes		yes		yes	
sample size	101		101		101		101		101		101	

Notes: Standard errors are corrected for country-specific heteroskedasticity and country-specific first order autocorrelation. Accrual rate is the change in the retirement replacement rate if a 55 year old works an additional ten years.

Table 4: Union Effects on Employment and Unemployment: Impact of Simultaneous One Standard Deviation Changes of Collective Bargaining Coverage, Coordination, and Density Within or Between Countries

Dependent Variable	I. Overall Unemployment Rate in Model, Std. Deviation Changes: Within Countries coef		std err	Between Countries coef		std err	II. Overall Unemployment Rate Out of Model, Std. Deviation Changes: Within Countries coef		std err	Between Countries coef		std err
Log epop ratios:												
Men 15-24	-0.0075		0.0103	-0.0582	*	0.0349	-0.0497	***	0.0125	-0.1902	***	0.0440
Men 25-54	0.0065	***	0.0017	0.0216	***	0.0059	-0.0047	**	0.0022	-0.0106		0.0073
Men 55+	-0.0467	***	0.0067	-0.1562	***	0.0217	-0.0592	***	0.0066	-0.1889	***	0.0216
Women 15-24	-0.0195	*	0.0117	-0.0900	**	0.0384	-0.0552	***	0.0150	-0.1996	***	0.0499
Women 25-54	-0.0157		0.0127	-0.0268		0.0380	-0.0298	**	0.0120	-0.0717	**	0.0356
Women 55+	-0.0884	***	0.0141	-0.2965	***	0.0489	-0.0837	***	0.0138	-0.2852	***	0.0458
Unemployment Rates:												
Men 15-24	-0.0080	***	0.0030	-0.0205	**	0.0099	0.0181	***	0.0052	0.0524	***	0.0174
Men 25-54	-0.0029	**	0.0012	-0.0089	**	0.0042	0.0067	***	0.0019	0.0177	***	0.0059
Men 55+	-0.0026		0.0019	-0.0097	*	0.0054	0.0082	***	0.0019	0.0238	***	0.0062
Women 15-24	-0.0031		0.0040	-0.0065		0.0135	0.0230	***	0.0053	0.0621	***	0.0193
Women 25-54	0.0047	***	0.0018	0.0190	***	0.0060	0.0150	***	0.0026	0.0493	***	0.0084
Women 55+	0.0048	**	0.0022	0.0146	**	0.0068	0.0116	***	0.0025	0.0352	***	0.0076

Notes: Sample size is 101. Control variables include: births/population 15-24 yrs earlier; employment protection index; 1st and 5th year UI replacement rates; labor tax rate; public pension replacement rate; pension accrual rate for 10yrs for a 55 yr old worker; UI replacement rate for older workers; disability replacement rate for older workers; and male and female retirement ages. One standard deviation changes within countries are: 5.28 percentage points for collective bargaining coverage; 7.24 percentage points for union density; 0.157 for coordination index. One standard deviation changes between countries are: 23.51 percentage points for collective bargaining coverage; 18.51 percentage points for union density; and 0.599 for coordination. .Standard errors are corrected for country-specific heteroskedasticity and country-specific first order autocorrelation.

*, **, ***: significantly different from zero at the 10%, 5%, or 1% level (two tailed tests).

Table 5: Union Effects on Relative Employment and Unemployment: Impact of Simultaneous One Standard Deviation Changes of Collective Bargaining Coverage, Coordination, and Density Within or Between Countries

Dependent Variable	I. Overall Unemployment Rate in Model Std. Deviation Changes:						II. Overall Unemployment Rate Out of Model Std. Deviation Changes:					
	Within Countries			Between Countries			Within Countries			Between Countries		
	coef		std err	coef		std err	coef		std err	coef		std err
Log epop ratios:												
Men 25-54 vs. Men 1524	0.0140		0.0104	0.0798	**	0.0354	0.0450	***	0.0127	0.1796	***	0.0446
Men 25-54 vs. Men 55+	0.0532	***	0.0069	0.1778	***	0.0225	0.0545	***	0.0070	0.1783	***	0.0228
Women 25-54 vs. Women15-24	0.0038		0.0173	0.0632		0.0540	0.0254		0.0192	0.1279	**	0.0613
Women 25-54 vs. Women 55+	0.0727	***	0.0190	0.2697	***	0.0619	0.0539	***	0.0183	0.2135	***	0.0580
Men 25-54 vs. Women 25-54	0.0222	*	0.0128	0.0484		0.0385	0.0251	**	0.0122	0.0611	*	0.0363
Unem. Rates:												
Men 25-54 vs. Men 1524	0.0051		0.0032	0.0116		0.0108	-0.0114	**	0.0055	-0.0347	*	0.0184
Men 25-54 vs. Men 55+	-0.0003		0.0022	0.0008		0.0068	-0.0015		0.0027	-0.0061		0.0086
Women 25-54 vs. Women15-24	0.0078	*	0.0044	0.0255	*	0.0148	-0.0080		0.0059	-0.0128		0.0210
Women 25-54 vs. Women 55+	-0.0001		0.0028	0.0044		0.0091	0.0034		0.0036	0.0141		0.0113
Men 25-54 vs. Women 25-54	-0.0076	***	0.0022	-0.0279	***	0.0073	-0.0083	**	0.0032	-0.0316	***	0.0103

Notes: Entries are based on the estimates in Tables 2 and 3. One standard deviation changes within countries are: 5.28 percentage points for collective bargaining coverage; 7.24 percentage points for union density; and 0.157 for coordination index. One standard deviation Changes between countries are: 23.51 percentage points for collective bargaining coverage; 18.51 percentage points for union density; and 0.599 for coordination. Standard errors are corrected for country-specific heteroskedasticity and country-specific first order autocorrelation.

*, **, ***: significantly different from zero at the 10%, 5%, or 1% level (two tailed tests).

Table 6: Effects of One Standard Deviation Changes in Unionization Variables on Relative Employment Divided by Standard Deviation of Relative Employment

Dependent Variable	I. Overall Unemployment Rate in Model Std. Deviation Changes:						II. Overall Unemployment Rate Out of Model Std. Deviation Changes:					
	Within Countries			Between Countries			Within Countries			Between Countries		
	absolute effect		relative effect	absolute effect		relative effect	absolute effect		relative effect	absolute effect		relative effect
Log epop ratios:												
Men 25-54 vs. Men 1524	0.0140		0.1007	0.0798	**	0.5466	0.0450	***	0.3237	0.1796	***	1.2301
Men 25-54 vs. Men 55+	0.0532	***	0.2608	0.1778	***	0.8387	0.0545	***	0.2672	0.1783	***	0.8410
Women 25-54 vs. Women15-24	0.0038		0.0150	0.0632		0.3511	0.0254		0.1000	0.1279	**	0.7106
Women 25-54 vs. Women 55+	0.0727	***	0.2653	0.2697	***	0.8148	0.0539	***	0.1967	0.2135	***	0.6450
Men 25-54 vs. Women 25-54	0.0222	*	0.0978	0.0484		0.2316	0.0251	**	0.1106	0.0611	*	0.2923

Notes:Entries are based on the estimates in Table 3. One standard deviation changes within countries are: 5.28 percentage points for collective bargaining coverage; 7.24 percentage points for union density; and 0.157 for coordination index. One standard deviation changes between countries are: 23.51 percentage points for collective bargaining coverage; 18.51 percentage points for union density; and 0.599 for coordination. The "absolute effect" entries are reproduced from Table 3. The "relative effect" entries are the absolute effects divided by within or between country standard deviation of the corresponding relative employment measure.

*, **, ***: significantly different from zero at the 10%, 5%, or 1% level (two tailed tests).

Table A1: Interaction Effects Between Overall Unemployment and Simultaneous One Standard Deviation Changes of Collective Bargaining Coverage, Coordination, and Density Within or Between Countries

Dependent Variable	I. Overall Unemployment Rate in Model Std. Deviation Changes:					
	Within Countries			Between Countries		
	coef		std err	coef		std err
Log epop ratios:						
Men 15-24	-0.0851		0.0848	-0.5796	*	0.3021
Men 25-54	-0.0308	*	0.0160	-0.1050	*	0.0662
Men 55+	0.0853		0.0813	-0.4435		0.2788
Women 15-24	0.0122		0.0947	-0.4104		0.3445
Women 25-54	-0.1132		0.0971	0.1993		0.3353
Women 55+	0.2164	*	0.1144	-0.3470		0.4183
Unemployment Rates:						
Men 15-24	0.0244		0.0337	0.1189		0.1282
Men 25-54	0.0404	***	0.0116	0.1428	***	0.0423
Men 55+	0.1082	***	0.0166	0.3486	***	0.0596
Women 15-24	-0.0918	***	0.0246	-0.2089	***	0.0867
Women 25-54	0.0193		0.0211	0.0207		0.0702
Women 55+	0.0306		0.0189	0.1134	*	0.0687

Notes: Sample size is 101. Control variables include: births/population 15-24 yrs earlier; employment protection index; unemployment rate; collective bargaining coverage; union density; coordination index; 1st and 5th year UI replacement rates; labor tax rate; public pension replacement rate; pension accrual rate for 10yrs for a 55 yr old worker; UI replacement rate for older workers; disability replacement rate for older workers; male and female retirement ages; and interactions between the unemp. rate and these variables. One standard deviation changes within countries are: 5.28 percentage points for collective bargaining coverage; 7.24 percentage points for union density; 0.157 for coordination index. One standard deviation changes between countries are: 23.51 percentage points for collective bargaining coverage; 18.51 percentage points for union density; and
0.599 for coordination. .Standard errors are corrected for country-specific heteroskedasticity and country-specific first order autocorrelation.
*, **, ***: significantly different from zero at the 10%, 5%, or 1% level (two tailed tests).