NBER WORKING PAPER SERIES

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Working Paper 9043
http://www.nber.org/papers/w9043

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
July 2002

We are grateful to Richard Disney and to seminar participants at Cornell, Turin, and Juan March Institute (Madrid) for helpful comments; to Justin Wolfers for help in assembling and using the macroeconomic data set made available by him and Olivier Blanchard; and to David Neumark for providing us with demographic data. Excellent research assistance was supplied by Julian Messina, Abhijay Prakash, and especially Thomas Steinberger. The views expressed herein are those of the authors and not necessarily those of the National Bureau of Economic Research.

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JEL No. J1, J2, J5, J6, E2

ABSTRACT

Using data from 17 OECD countries over the 1960-96 period, we investigate the impact of institutions on the relative employment of youth, women, and older individuals. Theoretically, we show that labor market institutions meant to improve workers' income share imply larger disemployment effects for groups whose labor supply is more elastic. Using an empirical model that allows us to control for unmeasured country-specific factors that affect relative employment and unemployment, we find that, for both men and women, more extensive involvement of unions in wage-setting significantly decreases the employment rate of young and older individuals relative to the prime-aged, with no significant effects on the relative unemployment of these groups. In contrast, a larger role for unions has insignificant effects on male-female employment differentials, but raises female unemployment relative to male unemployment. These results suggest that union wage-setting policies price the young and elderly out of employment and drive disemployed individuals in these groups to non-labor-force (education, retirement) states. A possible scenario for women is that high union wages encourage female labor force participation, but that women who would otherwise be disemployed by high wage floors are able to find work in unregulated sectors or are absorbed by public employment.

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1. Introduction

In 1973, OECD standardized unemployment rates were between 2 and 3.2% for most European countries, and even lower in several (OECD 1983). By 1995, unemployment had risen in all of these countries, averaging 10.7% in the European Union (OECD 2000). The experience of the United States strongly contrasts with that of these other OECD countries. In 1973, U.S. unemployment was 4.8% or roughly double that of other OECD countries. By 1995 it was 5.6% or about half that of the European Union. This reversal of unemployment fortunes motivates a vast literature aimed at explaining these and other patterns of cross-country unemployment evolution. Some studies emphasize European labor market institutions, such as high levels of union coverage and generous social insurance benefits, as reasons for high unemployment (OECD 1994; Siebert 1997; Nickell and Layard 1999; Nickell, Nunziata, Ochel, and Quintini 2001). Restrictive monetary policy in Europe (Ball 1997 and 1999) and other macroeconomic shocks are found to explain a large portion of diverging unemployment experiences, especially when interacted with institutional wage rigidities (Blanchard and Wolfers 2000; Ball 1997; Bertola, Blau and Kahn 2002). Public employment patterns have also been shown to play a potentially important role (Algan et al, 2002). Finally, and most relevantly to our approach here, within-country unemployment developments are empirically related to broad wage-inequality changes, and such demographic factors as a more rapidly falling size of the youth population also arguably contributed to the decrease in the U.S. relative unemployment rate (Bertola, Blau and Kahn 2002).

This paper's perspective is complementary to that of aggregate unemployment analyses. We investigate the effects of macroeconomic forces and labor market institutions on the relative employment of specific demographic groups—youth, women, and older individuals. Our focus on the labor market outcomes of these groups is most readily justified by the fact that their unemployment and (especially) employment rates are much more variable than those of prime-age males (Bertola, 1999). The labor market position of demographic groups other than prime-age males has, not surprisingly, featured very prominently in the policy debates of industrialized countries. Considerable attention has been paid to youth employment problems in Europe (Blanchflower and Freeman 2000). The labor market prospects of older workers importantly affect national policies to insure the living standards of the elderly and the sustainability of pension systems in the face of an aging population. And, while it is unclear whether or not equal-opportunity prescriptions and parental leave policies have actually raised female employment and labor force participation (Blau and Kahn 2000a, Ruhm 1998), the relative employment outcomes of women are closely scrutinized in most OECD countries.

There are, in addition, important methodological reasons to focus on the relative employment of subgroups. This approach 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, centralization of union wage setting and employment protection legislation (EPL). 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; Nickell and Layard 1999). Centralized wage setting does, however, tend to cause some compression of the distribution of wages in practice (Blau and Kahn 1996a, b), 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. EPL also has ambiguous effects on overall employment, since it discourages both layoffs and hires. But if, as is likely, it has smaller effects on inflows from out of the labor force than on layoff rates, it should increase unemployment (and decrease employment) of labor market entrants, such as youths and women, *relative* to incumbents. In these 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. First, reverse causality (such as increasing generosity of unemployment insurance benefits in response to high unemployment) may well be less important when one is examining relative employment or unemployment than their corresponding aggregates. Second, studying relative employment alleviates 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. However, these omitted factors may affect relative employment outcomes by influencing the various subgroups differently. For this reason, in our empirical work, we control for country effects. We are able to do so because we have compiled a data base with time-varying institutional measures.

This paper offers not only new empirical results, but also a simple and novel interpretation of wage compression and disemployment of "outsiders." Specifically, in Section 4, we present a model of union behavior which shows that policies meant to increase workers' surplus from employment imply larger union wage markups and hence larger falls in employment for groups with more elastic labor supply, other things equal. This model implies that union bargaining will raise the relative wages and lower the relative employment of youth, older individuals and women (compared to the prime-aged and males) to the extent that these groups have more elastic labor supply schedules, as is likely. Intuitively, unions choose to raise the wages most for groups with the best alternatives to paid employment: schooling (youth), home production (women), and retirement (older individuals). As we discuss below, wage compression (at least for young workers and women relative to prime age men) and relative disemployment of outsiders are hard to rationalize under other models of union behavior.

2. Previous empirical approaches

Our paper is similar in spirit to a group of cross-country, time series studies of the impact of institutions on the overall unemployment rate (Belot and van Ours 2000; Bertola, Blau and Kahn 2002; Scarpetta 1996; Daveri and Tabellini 2000; Nickell, Nunziata, Ochel and Quintini 2001). We offer new insights and evidence by focusing on relative employment, as is also recently done by Jimeno and Rodriguez-Palenzuela (2001), who however 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.

There is abundant evidence that labor market institutions affect the wage distribution, but evidence on the impact of institutions on relative employment is mixed (Blau and Kahn 1999; 2002). Within-country studies focusing on the impact of changes in union coverage or in institutions associated with collective bargaining have found evidence of negative effects on low-skill employment from union intervention.[1] Evidence from cross-country studies, however, is scarce and less conclusive. Studies comparing two or three countries with different levels of unionization offer mixed support for theoretical predictions: in most of the studies, unionization is found to imply more compressed and less flexible wage structures, but not less favorable employment opportunities for low-skill workers; however, in one study both the predicted wage and employment outcomes for the less-skilled were observed, although the employment effects were small.[2]

Country studies may offer valuable (if often only implicit) detailed controls for country-specific factors.[3] However, they yield evidence that is not only mixed, but also hard to extrapolate to other countries and periods. More readily generalizable are cross-sectional studies that pool data across a number of countries with different institutional arrangements. Nickell and Bell (1995) find little evidence of more pronounced relative unemployment increases for the

[1] 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).

[2] 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 (2000a) 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. However, in Freeman and Schettkat's (2000) study of the US and Germany from the 1970s to the 1990s, 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 US. But these effects were too small to account for much of the rise in the overall German unemployment rate compared to the US.

[3] Among the many country-specific features influencing employment outcomes alongside standard labor market institutions, availability of public sector jobs for low-skill workers may play an important role. See Blau and Kahn (2000a) 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 (forthcoming) for theory and evidence on the impact of public jobs on aggregate employment and unemployment.

less-educated in countries with more rigid labor markets. In contrast, 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 (1996a) find for the 1980s that, among men, the employment-to-population ratio of low skilled relative to middle skilled workers (defined by age and 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. A problem with these and other cross-sectional studies is that omitted country-specific factors may account for the observed relative wage and relative employment patterns. For example, Nickell (1997, p.66-67) notes that most of the apparent employment effects of EPL are accounted for by low female employment rates in Southern Europe – with no effect on prime-age males – and that the evidence may thus reflect cultural difference rather than policy effects.

We overcome these problems with earlier country studies by examining the impact of institutions on relative employment in a regression context. Unlike this earlier work, we examine 17 countries over the 1960-96 period and hence obtain more readily generalizable results. And, in contrast to earlier cross-sectional studies, our data on time-varying institutions enable us also to control for country effects and thereby address concerns of country-specific omitted variables, such as the cultural factors Nickell (1997) mentions. Finally, none of these earlier studies provides any theoretical justification for the phenomena of wage compression and relative disemployment of outsiders, a central focus of the theoretical model presented below.

3. The facts

Figure 1 displays descriptive graphs of each country's employment rate for 1995-96 plotted against the rate for 1970-74. The data are presented separately by sex and refer to three age groups: young (15-24), prime-aged (25-54), and older (55+). The employment rate is the proportion of the population in the indicated age-sex group that is employed. Employment rates are averaged over the indicated period and the graphs include the subset of countries for which complete data are available in both periods: Australia, Canada, Finland, France, Italy, Japan, Spain, Sweden, the United Kingdom, and the United States. Each graph includes a 45° line to clarify the trend (note that the origins of the horizontal and vertical axes are not the same across graphs).

The first column of graphs in Figure 1 displays information on youth. Employment rates of young men (in the top graph) fell over the period in all countries. The decline was relatively small in the United States, where the employment rate fell from 58.5% to 53.6%, compared to a considerably larger decrease from 67.3% to 46.3%, on average, in the other countries. Only Canada had a smaller decline than the United States. Employment rates of young women (in the bottom graph) also fell in most countries, with the exception of the United States and Canada, where the ratio increased, and Australia, where it was stable. As in the case of males, employment rates of young women increased in the United States relative to the average for other countries: the rate rose from 40.0% to 49.6% in the United States and fell from 49.2% to 41.2% elsewhere on average. Thus, for both young men and young women, the U.S. went from being a relatively low employment rate country in 1970 to a relatively high employment rate country in 1995. France, Italy, and Spain had especially large declines in the incidence of employment for youth.

Consider next prime-age employment rates, shown in the middle column of graphs. In marked contrast to the comparison for youth, the U.S. falls squarely in the middle of the cross-country cluster of observations. For men, the employment rate declined from 93.1% to 88.3% in the United States and from 94.3% to 86.0% elsewhere on average. The employment rates of this demographic group are very narrowly concentrated around 95% in the earlier period. Their dispersion increases in 1995-96, when all employment rates are lower (by modest amounts, except in Spain and Finland). Both the declines and their dispersion, however, are modest in comparison to that observed for young men. Employment of prime age men appears to have been much better protected against the adverse labor market of the 1980s and 1990s than employment of the young.

In contrast, to the declines in employment rates of prime age men, employment of prime-age women rose substantially everywhere. The U.S. experienced a larger increase in employment rates for this group (from 47.4% in 1970 to 73.4% in 1995) than the average for the other countries (from 46.1% to 65.2%), but the difference was not large. And Canada and Sweden had substantially larger increases in prime age women's employment rates than did the U.S.

Finally, consider the information displayed in the last column of graphs in Figure 1 for older men and women. Employment rates of older men declined sharply in all countries. Those of older women also tended to decline but more modestly, and from much lower initial levels. Differences between the U.S. experience and that in the other countries for these groups were not as large as for youth. The employment rate of older males fell by slightly less in the United States (from 53.0% to 35.5%) than elsewhere (averaging 51.0% in 1970 and 30.2% in 1995). For older women, the trends in employment rates in the United States and the other countries were quite similar, falling from 23.6% to 20.8% in the U.S. and from 18.1% to 14.4% on average elsewhere.

4. A simple model of labor market institutions and relative employment effects

It may appear somewhat puzzling that, in labor markets that are more unionized than in the United States, employment of secondary worker groups ("outsiders") is relatively low. If prime-age male "insiders" wield greater bargaining power, should they not use that power to boost their wages relative to outsiders, and work less as a result? In this section, we proceed to show with a simple model that unions—or, more generally, policies and institutions aimed at improving workers' welfare—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 abstracts from many important aspects of union-management bargaining. Combining optimizing behavior by union leaders and realistic differences in group-specific participation elasticities, however, the model 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 in terms of within-group composition effects at the level of an entire labor market, supporting a stylized representation of industrial relations in many European countries. To focus on the relationship between group *i*'s employment and wage, demand or supply cross-group interaction terms are omitted in the formal model (we discuss the scope for such interactions briefly when summarizing the results below).

Consider the willingness-to-work function

$$w_i = s_i + \varepsilon_i (l_i - n_i), \tag{1}$$

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*.

Under competition, supply equals demand, and we have for log of competitive wages and competitive employment:

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

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

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.[4] The possibility of discrimination can easily be accommodated in the model by adjusting "true" productivity by the discrimination coefficient with a representing adjusted productivity. Since this issue is not central to our concerns here and leaves our basic reasoning unchanged, we do not explore it further but note that the adjusted productivity interpretation of a is most likely the relevant one for women.

4.1 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. For simplicity, we determine employment from a "right-to-manage" perspective, where firms are free to adjust the quantity of labor demanded.[5] 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 $WL - S(L)$, where $F(\bullet)$ is the (concave) revenue function whose log marginal revenue product is expressed by equation (2), L

[4] See for example, Blau and Kahn (2000b).

[5] If there is employer monopsony or if there is efficient bargaining over both pay and employment, then wage compression need not result in less employment for the groups whose wages are raised the most (Farber 1986; Card and Krueger 1995).

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).

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

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

where β is the relative weight of union objectives in the bargained outcome. This objective function generalizes the outcome of competitive equilibrium (where β=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 β >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 $W=F'(L)$,

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

can be rearranged to read

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

where $\eta(L)>0$ is the elasticity of the inverse labor demand curve. The β=1 case yields $S'(L_c)=W_c=F'(L_c)$, the competitive solution. At the other extreme, $S'(L_m)=F'(L_m)[1-\eta(\cdot)]$ when $\beta\rightarrow\infty$, 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, β>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.

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.[6] 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.

It is highly likely that the same groups (skilled, prime age, males) that command high wages in an unregulated labor market are also those whose labor supply is relatively inelastic (Blundell and MaCurdy 1999). Compared 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. Further, we may note that, at least with respect to youth and older individuals, union policy could be viewed as rational in the context of life-cycle labor supply decisions. From the individual's perspective, it is optimal to allocate periods of non-employment to stages in the life cycle when the value of alternative uses of time

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[6] 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, rather than the shape of each individual's utility function.

are highest. 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.

4.2 Other determinants of relative-employment outcomes

Of course, other features of the economic environment bear on union behavior and labor market outcomes. Within this model, however, only the labor supply elasticity effect can explain both wage compression and high disemployment of people other than prime-age males. First, a larger wage markup is optimal for worker groups with less elastic labor demand (see, for example, Farber 1986). While low demand elasticity also reduces the employment implications of higher wages, we show in Appendix A.1 that the combined effect is a greater employment loss: intuitively, steeper labor demand endows the union with more monopoly power, and implies a larger gain from restricting labor supply. International data on demographically-disaggregated demand elasticities (or markups) are not available, and even in theory such parameters would 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 – likely to include predominantly prime-age males – that are less easily substituted by non-labor factors of production (Rosen, 1970). Thus the labor demand elasticity effect predicts higher relative wages and lower relative employment for prime-age men than for other groups, the exact opposite of what one finds. Second, a larger union bargaining power parameter β also implies higher relative wages and lower relative employment. To the extent that union bargaining power varies across demographic groups, as in Jimeno and Palenzuela's (2001) theoretical model, we would expect it to be larger for 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.

Wage-setting centralization also bears intuitively on the model's implications. The derivations above assume that a union worker who loses his/her job has no alternative employment available, an assumption that might characterize an encompassing union that negotiates a contract covering a country's entire workforce, a stylized view of Scandinavian or Austrian corporatism. At the opposite end of the spectrum is the U.S.: in our data for 1994, unions cover roughly 18% of American workers, and a disemployed union worker may well have 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 outsiders 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 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 outsiders.

Raising wages of "outsiders" like youth, older workers and women may also be a way for "insiders" (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 and prevailing wage laws in the United States, which place a floor under wages paid to contractors with local governments (Neumark 2001; Kessler and Katz 2001). Our model without demand-side interactions suggests an alternative, perhaps 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.

To the extent this is the case, the negative employment effects of union policies that price out low-wage labor become more socially acceptable.

4.3 Taxation

The model above characterizes unions as negotiating wages subject to being on the labor demand curve. A downward sloping labor demand schedule yields inframarginal surplus to employers and gives workers incentives to increase wages and accept employment losses as long as the higher wage earned by those who are employed more than compensates for the labor income lost by those who would be employed at the competitive wage. This compensation is implicit in writing a standard union objective function as a linear sum of employment and non-employed incomes. In reality, the relevant compensating transfers may take place within families, or may be redundant if workers are risk-neutral or take turns in unemployment over their lifecycle. However, it may be both theoretically and empirically interesting to note that the relevant transfers can be quite explicit: institutions can leave the work choice to individuals, insert tax wedges between employer costs and take-home pay, and distribute tax revenues to workers who can no longer be employed.[7]

Suppose workers' representatives in government enact a labor tax, let employment be determined by individual price-taking demand and supply behavior, and distribute the tax proceeds to workers. This arrangement might characterize countries with dominant labor or social democratic parties, such as several in Continental Europe, and represents well arrangements (still common in Scandinavian countries) whereby unions administer unemployment benefit systems. Employment must maximize employers' profits in light of the gross wage, denoted W_r, to imply that employment satisfies $W_r= F'(L_d)$, where L_d is labor demand. And workers' optimal acceptance of employment, on the basis of their non-employment opportunities and of the net wage $(1-\tau)W_r$, requires $(1-\tau)W_r=S'(L_s)$, where L_s is labor supply.

[7] See Spilimbergo (1999) for a discussion of tax and subsidy determination as means of preventing competition from lower-productivity workers in a cross-regional context. The relevant mechanism is the same as in Lazear (1983) discussed above.

Taking into account these constraints, suppose the revenue of labor taxes is redistributed to workers, and the tax rate is thus chosen to maximize:

$$F(L_d)\text{-}WL_d+\beta[W(1\text{-}\tau)L_s\text{-}S(L_s)+\tau WL_s].$$

The optimal tax rate is then $\eta(\cdot)(\beta\text{-}1)/\beta$. The relationship between $F'(\bullet)$ and $L'(\bullet)$ here is the same as in the union wage bargaining model: the elasticity of supply again has no implications for this tax rate, while it would bear on the Ramsey-optimal structure of tax rates aimed at minimizing distortions induced by raising revenue for general purposes rather than for distribution to workers. And the elasticity of labor supply has the same effects on relative wages and relative employment, here as in the union bargaining model, in the comparison of *laissez faire* and regulated labor market outcomes (see Appendix A.2).

Even if taxes are imposed by government and not necessarily spent on union members' benefits, our framework predicts that the impact of taxes on wages and employment will in general depend on labor supply and demand elasticities, as in ordinary models of payroll tax incidence. If τ is the tax rate and $\log(1+\tau) \approx \tau$, then under competition we have

$$l(\tau) \approx (a\text{-}s+\varepsilon n\text{-}\tau)/(\varepsilon+\eta) \qquad (9),$$

and, after taxes,

$$w(\tau) \approx (\varepsilon+\eta)^{-1}[\varepsilon a + \eta s - \varepsilon\tau - \varepsilon n\eta]. \qquad (10).$$

Obviously, a larger wedge τ decreases the group's employment, and the effect is more pronounced if $\varepsilon+\eta$ is small (i.e., if the group's participation and/or demand schedules are flatter). And after tax wages fall more for groups with less elastic labor supply. Thus, a uniform payroll tax will compress wages and lead to larger disemployment effects for outsiders. The role of different labor supply elasticities in determining these effects is exactly the same as in the above analysis of union bargaining. When employment and wage differentials are driven by taxes, however, lower employment is accompanied by reduced labor market participation, rather than unemployment (see Appendix A.3).

4.4 Summary

The basic implications of out theoretical approach are easily illustrated. The left-hand diagram in **Figure 2** shows the effects on wages and employment of a wedge between labor demand and labor supply. The right-hand diagram repeats the exercise for the same labor demand function and a different labor market participation function: the impact of the same wedge on wages and employment, relative to the competitive outcome, is larger because the participation schedule is flatter. In both diagrams, the counterpart of lower employment can be open unemployment if the wedge is implemented by a binding wage floor, to imply that more individuals are willing to work than can be employed at the going wage. Or it can be lower participation, if the wedge represents taxes and lowers take-home pay along the labor supply curve (an outcome equivalent from the collective point of view of workers, as discussed above, if the revenue of tax and contribution schemes is redistributed to disemployed individuals).

The key point illustrated by the model is that, upon insertion of wedges between marginal productivity and marginal willingness to work, differences in group supply elasticities can (everything else given) imply higher relative pay and larger negative employment effects for groups whose labor market attachment is more strongly influenced by the wage. This is obvious in Figure 2, and the formal expressions above also indicate that examining the implications of the bargaining influence of such "insider" subgroups as prime-age men cannot as readily explain why unionized workers do not raise their own wages as much as the wages of "outsiders".

Needless to say, many additional factors are potentially relevant in reality to the outcomes of interest. While the model treats each group as a separable unionized entity, complementarity or substitutability interactions across groups of workers are realistic on both the demand and supply side of the labor market. Our baseline model features no labor demand interactions across groups. This may offer a satisfactory approximation: empirically, skilled prime-age workers are not close substitutes for youth, female, and elderly workers, while individuals within these groups are closely substitutable for each other (Disney, 1996; see Jimeno and Rodriguez-Palenzuela, 2001, for a formal model of imperfect substitutability). It may be theoretically and empirically more important to account explicitly for income effects in individual labor supply, and in particular for the effects (based on models of within-family resource distribution) of primary workers' wages on secondary workers' participation incentives. Other distributional effects could result from realistic uses of labor-tax revenues, and implications for relative employment outcomes could easily derive from age- and gender-dependent subsidization of employment and non-employment states. Moreover, different groups within unions may have different levels of bargaining power. As long as realistic differences between group-level participation elasticities are preserved, however, these and other extensions would complicate the analysis without affecting the basic insight illustrated by Figure 2's reduced-form representation. In what follows we discuss the insight further and confront it with empirical information.

5. Evolution over time of relative employment outcomes and institutions

Our cross-country time-series data set 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. We have also included additional labor market institutions indicators as well as additional data on changes in institutions over time (see Appendix B for details.) The countries included are Australia, Belgium, Canada, Denmark, Finland, France, Germany, Italy, Japan, the Netherlands, Norway, New Zealand, Portugal, Spain, Sweden, the UK, and the U.S. 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.

Table 1 reports cross-sectional and time-series demographic employment patterns, for the set of countries with complete observations in 1970-74 and 1995-96; **Figure 3** illustrates for the same countries patterns of relative changes in employment incidence for prime-age vs. young and prime-age vs. older individuals, separately by sex. 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 across age groups 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 the Anglo-Saxon group including Canada and Australia. These contrasts are stronger for the youth than for older individuals.

Our empirical specifications below aim at explaining these developments in terms of variation in institutional features, also summarized for the same countries in Table 1. The institutional variables most directly relevant to our theoretical arguments pertain to the extent and character of union wage setting. Theory indicates that union involvement in relative-wage setting, as indexed by the model's parameter β, should concentrate employment losses on "secondary" workers. And union power may 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 (OECD 1995; Casey 1992), or that the most recent (and younger) employees are laid off on a last-in-first-out basis.

Time-varying data are available for collective bargaining coverage and degree of coordination, as well as for union density. We see in Table 1 that 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 four of the remaining countries, including the U.S. Coverage increased significantly in France and Spain and was fairly stable in the remaining two countries. On average, coverage in the U.S. fell by 3.5 percentage points, only somewhat more than the average decrease of 2.3 percentage points in the other 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. Union density declined by 12 percentage points in the U.S., but rose by 3 percentage points, on average, in the non-U.S. countries. 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. Theory also indicates that labor taxes should also have a differential effect on the relative employment of groups with differently elastic demand and supply schedules. Accordingly, we include tax indicators as explanatory variables in our relative-employment regression below. The model presented above, however, implies that taxes should not affect unemployment, and we test this prediction as well. 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. Taxes in the U.S. rose by four percentage points less than the average for the other countries and the U.S. tax rate remained below the other country average. France, Finland, Italy and Sweden had especially high labor tax rates as of the mid-1990s.

In reality, of course, tax and expenditure patterns are more complex than in our simplified theoretical framework. To the extent that the future benefits of individual workers are linked to their own contribution history and this is internalized by labor supply behavior, labor taxation would tend to be offset by reduced take-home pay at unchanged labor cost levels.[8] However, such wage decreases may be impossible for workers at or near binding wage floors, particularly youth and possibly adult women as well. Thus, the impact of labor taxes depends on details of

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

benefit schemes, and interacts with the structure of wage distributions and wage negotiations. Some of our empirical specifications will allow for the latter interactions, but only limited information is available on the former aspects.

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 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 would raise the young-prime age employment rate 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 in the United States were less positive than those 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 with a smaller rise for the US than for the other countries, on average, although this average is strongly driven by Spain's large increase. Replacement ratios for special disability and unemployment schemes for older workers rose on average with a slightly larger rise in the US than elsewhere for disability schemes (.07 vs. .04) and a moderately larger rise for unemployment schemes in other countries than in the US (.08 vs. no change). Finally, 10-year accrual rates were constant at zero in the US but fell elsewhere on average, a change that reduced work incentives for older workers outside the US on average (the 10-year accrual rate is the change in the replacement rate of retirement benefits for a 55-year old male who works an additional ten years). With the exception of the slightly larger increase in US disability replacement rates, retirement institutions changed in ways that lowered work incentives for older individuals by more outside the US than for the US.[9]

To summarize, on average, the institutions shown in Table 1 appear to have become more interventionist in other countries relative to the United States between 1970 and 1995. To the extent that these institutions produce reduced relative employment for younger and older individuals, pattern of these changes is consistent with the different relative employment experiences summarized in the top portion of Table 1, and in Figure 3. Specifically, as noted, the gap between prime aged and younger individuals sexes and prime aged and older people tended to rise much more in other countries than in the U.S., with especially large differences for youth. These relationships are simply descriptive, however, and so far our qualitative comments on the empirical fit of theoretical predictions were narrowly focused on the comparison of the U.S. experience to that of other countries with complete data in the early 1970s and at the end of the sample period. Below, we look more systematically at the relationship between changing institutions and employment outcomes of demographic groups in a regression context that makes

[9] 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 for the impact of the collective bargaining and tax rate variables, our primary focus. Results for these variables were similar when the retirement variables were excluded.

it possible to control for other influences and exploit all available time-series and cross-section information.

6. Regression evidence

On the basis of the simple theoretical considerations above and in Appendix A, our empirical specifications seek evidence of relative employment effects (from both collective wage-setting and taxes) and relative unemployment effects (only from collective wage setting). We are also interested in studying relative employment and unemployment effects from flow-based demographic patterns induced by EPL, and from UI and pension arrangements.

We estimate equations of the following general form, with (11a) and (11b) estimated separately for men and women, and subscripts denoting "prime aged" (p) or ages 25-54, "young" (y) or ages 15-24, and "older" (o) or aged 55 and over:

$$\log(e_p/e_y)_{jt} = B'X_{jt} + a_j + b_t + u_{jta}, \tag{11a}$$

$$\log(e_p/e_o)_{jt} = B'X_{jt} + a_j + b_t + u_{jtb}, \tag{11b}$$

$$\log(e_{pm}/e_{pw})_{jt} = B'X_{jt} + a_j + b_t + u_{jtc}, \tag{11c}$$

where for country j and period t, e is the employment-to-population ratio (which we sometimes refer to as the employment rate), m refers to men and w refers to women, 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 average wage replacement rate, replacement rates for older workers under special disability and unemployment schemes, the change in the retirement wage replacement rate for 55 year old males who work an additional ten years (the accrual rate), male and female normal retirement ages under public pensions, and the average total labor tax rate (income plus payroll plus consumption taxes), a is a country effect, b is a period effect, and u_a, u_b and u_c are disturbance terms.[10]

In equations (11a) and (11b), the dependent variable is the relative employment rate of the prime aged compared to younger and older individuals, respectively. In equation (11c), we estimate the determinants of the employment rate gap between prime age men and prime age women. Measuring relative employment effects in terms of log 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). 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.

We are primarily interested in ascertaining whether labor market institutions affect relative employment rates 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 with relative unemployment as the dependent variable. In these alternative equations, we use the raw difference between the unemployment rates of the various groups as dependent variables.[11]

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 rate gap:

[10] As noted by Ruhm (1998), availability of paid parental leave can influence relative employment and wage levels of women. We were able to obtain some data on weeks of paid parental leave, which were kindly provided by Christopher Ruhm and used in Ruhm (1998) and Ruhm (2000). Unfortunately, there was too little overlap between his data and ours to allow us to allow us to control for parental leave policies.

[11] Freeman and Schettkat (2000) argue that in comparing unemployment rates over time, raw differences (rather than, for example, log differences) are the appropriate functional form.

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 (Blau and Kahn 1981), 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. 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.[12]

We include births/population 15-24 years prior to the current observation to control for the relative supply of young individuals (see Korenman and Neumark 2000, and Jimeno and Palenzuela 2001). 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 rate 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

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[12] It is also possible that the overall unemployment rate is itself affected by the demographic composition of the population, rather than by macroeconomic conditions only. Thus, in models not shown here, we replaced the raw unemployment rate with one that corrects 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+. For each observation, we used the same set of weights, which were taken from the 16 country sample of 1980 observations. The results were virtually identical to the ones reported below.

conditions, through migration, and is therefore more likely to be exogenous with respect to current employment outcomes.

6.1 Relative employment

Table 2 contains basic regression results for relative employment.[13] We begin by examining the results for wage–setting institutions: collective bargaining coverage, coordination, and union density.[14] We find positive effects of collective bargaining coverage and coordination on the prime age-youth employment rate differential for both men and women, with significant effects in three of the four cases. However, union density has unexpected significant negative effects. With respect to the prime age-older employment gap, the three union variables have positive effects on the employment gap for both men and women that are significant, in all but one case. Finally, in the case of the male-female employment gap, union density has a significantly positive effect on the gap; however the coefficients on the remaining two union variables are insignificant, with one positive (coordination) and one negative (collective bargaining coverage).

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

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[13] 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 models in Tables 2 and 3 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.

[14] 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.

the regression coefficients to predict the change in relative employment which would occur if all the union-related variables were simultaneously changed from their average level in one region of the world to their average level in another. Since the coefficients are estimated controlling for country effects, these simulations give useful indications as to the effects of plausible long-run differences in the three interrelated explanatory variables.

The "Scandinavia vs. North America" line of Table 2 shows the estimated relative-employment effects of changing the union environment from that observed in Canada and the U.S. (union density=27.8%, coverage=29.6%, coordination=1.0 on average) to that observed in Denmark, Finland, Norway and Sweden (density=68.2%, coverage=80.4%, coordination=2.35). The following lines compare North America to Northern Europe, North America to Southern Europe, and the U.S. to the full set of non-U.S. countries, all comparisons involving substantial increases in all of the union-related variables.[15] Simulation results are similar in all cases. Moving from North American values for the union-related variables to the values in each of the other regions, or from U.S. to non-U.S.values, substantially increases the prime age-youth and prime-age older employment gaps for both men and women. The prime age-youth effect ranges from .16 to .25 log points for men and from .16 to .36 log points for women, while the prime age-older effect ranges from .23 to .40 log points for men and from .37 to .53 log points for women. The estimated effects are all highly statistically significant, with the exception of the female prime-age youth effect for Scandinavia vs. North America, which is significant at the 11% level. In contrast to our results for the prime age-youth and prime age-older comparisons, union wage-setting institutions are not found to have significant effects on male-female employment differentials.

Turning to our findings for the other policies, we see that employment protection is found to widen employment gaps by age in three out of four cases (the prime age-youth contrast for

[15] Northern Europe consists of Belgium, France, Germany and the Netherlands and has density of 32.7%, coverage of 87.5%, and coordination of 2.21; Southern Europe includes Italy, Portugal, and Spain and has values of 34.1% for density, 76.3% for coverage, and 1.86 for coordination. In the U.S., density is 20.5%, coverage is 22.0% and coordination takes its minimum value of 1.0, while in the non-U.S. countries, these variables average 43.6% (density), 72.2% (coverage), and 2.02 (coordination) respectively.

women, and the prime age-older contrast for both men and women) with two of the estimated effects being statistically significant. The negative coefficients estimated for male prime age-youth employment differentials and male-female employment differentials are counter to expectations, but not statistically significant.

Labor taxes are found to have positive effects on both male and female prime age-youth employment differentials and the male-female prime age employment differential. While these point estimates are consistent with the idea that tax wedges lead young workers and women to forsake employment and engage in their relatively more attractive non-employment activities (such as education or home production), the effects are not statistically significant. Moreover, taxes have negative effects on the prime-age older worker relative-employment comparisons, with a significant effect for the male prime age-older employment differential. These results are not easily interpreted, and may reflect the pattern of public expenditures not accounted for by the retirement and UI variables included in the model.[16]

Overall UI one and five year replacement rates narrow prime age-young and prime age-older worker employment differentials in each case, with several significant effects. These results suggest perhaps greater coverage by regular UI systems for prime age workers than for other groups (we discuss the special UI systems for older workers below). However, we discount this interpretation because, when we examine relative *unemployment* rates of prime age vs. older or younger workers (**Table 3**), we find opposite-signed effects for the one and five year replacement rates in three of four cases, and these are usually insignificant. With respect to male-female comparisons, though, higher first and fifth year replacement rates both significantly raise the male relative unemployment rate, and the first year replacement rate significantly

[16] In particular, the availability and quality of schooling may be an important determinant of cross-country and time-series patterns in youth labor market participation. As an attempt to partially control for the effects of total government spending, which may be confounded with labor taxes, we added a control for the fraction of employment in the public sector, using the data used and kindly made available by Algan, Cahuc, and Zylberberg (2002). We do not emphasize these results because government employment is clearly endogenous and because we lose about 9% of the sample because of missing data. Nonetheless, the impact of labor taxes was similar to that in Table 2. Even controlling for the level of public expenditures, the types of public expenditures may be correlated with labor tax rates, complicating the interpretation of the tax variable.

lowers the male relative employment rate. The fifth year replacement rate has an insignificantly positive effect on the male relative employment rate. These male-female comparisons suggest greater UI system coverage of men than women.

The retirement-related variables have mixed effects on relative employment. Several variables have the expected effects on the relative employment of older workers. For example, a higher pension replacement rate significantly raises the employment of prime age vs. older workers for women. Moreover, higher replacement rates for special disability and unemployment programs for older workers, and lower retirement ages each have a positive effect on the male prime age-older worker employment gap, with significant effects in each case except for disability programs. On the other hand, a higher overall pension replacement rate has an insignificant, negative effect on male prime age vs. older employment, and a higher accrual rate for 55 year olds actually lowers the relative employment of men over 55.[17]

Finally, looking at economic and demographic conditions, we find that the overall unemployment rate has a positive effect on the prime age-youth gap for men and women and the prime age-older gap for men. The prime-age youth coefficients are highly significant, while the male prime age vs. older worker unemployment rate coefficient is significant at the 7% level (two-tailed test). The births variable has a positive effect on the prime age-youth employment gap, with a significant effect for women, suggesting cohort crowding and imperfect substitution between youth and adults (see also Korenman and Neumark 2000).

6.2 Relative unemployment

Table 3 shows results for relative unemployment rates using the same specification as in Table 2. Focusing on the combined effect of the wage-setting variables shown in the last four rows of the table, we usually find insignificant effects of unions on the relative unemployment of youth and

[17] 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 labor market institutions than in their paper, as well as a considerably different focus.

older individuals.[18] These age-related results for unemployment are consistent with our theoretical analysis if these groups have good alternative uses of their time when disemployed: younger individuals may pursue educational options,[19] while older workers may retire.

Interestingly, while union wage-setting institutions were found to have small and insignificant effects on male-female employment differentials, they are found to lower the male unemployment rate relative to the female unemployment rate by 4.4 to 6.2 percentage points, and these effects are significant at the 0.02%-0.05% level. These are sizable effects, since on average the male-female unemployment rate differential is –1.4 percentage points. The results are consistent with women being drawn into the labor force by the prospect of landing a high-paying union job in countries where strong wage centralization tends to price them out of work. Although the relative-employment effects that would be implied by this mechanism are not apparent in the data, it may be that some women are able to find work in unregulated sectors, or that public employment absorbs the women who would otherwise be disemployed by high union wage floors (Blau and Kahn 1999; 2002).

We now turn to a brief consideration of some of the other variables in the model. Lagged birth rates appear highly relevant to youth relative unemployment, confirming the results of Jimeno and Palenzuela (2001) and their references. With respect to policy variables, UI indicators are not a significant determinant of youth relative unemployment. This is not surprising since in many countries young labor market entrants are not entitled to unemployment benefits. The most interesting results are for the employment protection index which is found to lower unemployment rates of the prime aged compared to younger workers—the coefficient on employment protection is significant at the 5.6% level for men and the 0.7% level for women

[18] The exceptions are two cases (Northern Europe vs. North America and Southern Europe vs. North America) in which greater unionization raises the unemployment of prime age women vs. older women.

[19] This reasoning is consistent with Kahn's (2000) finding for a cross section of 15 OECD countries that collective bargaining coverage had a negative effect on youth relative employment but was positively associated with school attendance among young adults. However, he found 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.

(two-tailed tests). There is no evidence that employment protection raises unemployment of older individuals relative to the prime aged. If downsized older workers flow out of the labor market (into retirement) rather than into unemployment, this finding is consistent with our finding above that EPL tends to lower the relative employment of older men. Further, for both men and women, labor taxes appear to lower the unemployment of the prime aged relative to older individuals, to also lower the unemployment of young vs. prime age women and to raise male unemployment rates compared to women's. As was the case for relative employment, taxes may be correlated with the composition of spending which may affect participation rates and thus unemployment rate.[20] Finally, we again obtained mixed results for the retirement variables. Higher overall retirement replacement rates lower older workers' relative unemployment rates. In conjunction with the negative (women) to roughly zero (men) effect of this variable on older workers' relative employment (Table 2), these unemployment effects suggest that retirement benefits reduce older workers' labor force participation. A higher 10 year accrual rate raises older workers' relative unemployment as do higher disability and UI replacement rates for older workers. The accrual rate result may reflect greater incentives for older workers to look for work, while the UI effect likely reflects the job search requirement of this program. While the disability results are counterintuitive (i.e. disability benefits raise older workers' relative unemployment rates), Blöndal and Scarpetta (1999) suggest that the OECD-based measure of disability scheme generosity may not accurately capture the extent to which countries in practice use the disability system as a way of reducing the labor supply of older workers.

6.3 Interactions

The results so far suggest an important role for labor market institutions in explaining international differences in the relative employment of youth and older individuals and the unemployment of women compared to men. The models in Tables 2 and 3 had a simple linear

[20] Adding the public employment share to the regressions did not have a major impact on the tax effects.

structure in which, for reasons of parsimony, only main effects of each variable appear. However, we might expect recessionary overall macroeconomic conditions or cohort crowding to have more severe negative relative employment effects on youth, for example, the more rigid relative wages are with respect to economic conditions. And employment institutions such as seniority-based layoffs will 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 and between the prior births variable and each institution. We do not present full results of these models, which feature many interaction coefficients of limited individual interest. However, as we now show, these interaction models do help explain changes in relative employment from the 1970s to the 1990s.[21]

6.4 Institutions and relative employment trends

In **Figures 4-6** we consider the extent to which institutions can help us explain relative employment trends and the relative unemployment of women (we omit other unemployment comparisons, for which wage-setting institutions had insignificant effects). We focus on a comparison of changes in outcomes between 1975 and 1995 because we have a fairly large set of countries—14 in total—with data for each of these two years.[22] Each figure plots the predicted changes in relative employment (unemployment) against the actual changes. The predicted changes are based on three models: a) a model including only controls for economic and demographic conditions, and country and period effects; b) a model with these controls plus the institutional variables (i.e., indicators of wage setting institutions, employment protection, unemployment insurance generosity, retirement variables and labor taxation) as reported in

[21] An additional possible set of interactions concerns those between the various indicators of labor market institutions, as emphasized by Belot and van Ours (2000). We were unable to find any clear pattern of results when we tried interacting various institutions with each other, and we concluded that the data are not rich enough to support such models.

[22] The countries are Australia, Canada, Finland, France, Germany, Italy, Japan, Netherlands, Norway, Portugal, Spain, Sweden, United Kingdom and United States.

Tables 2 and 3; and c) the same model with unemployment-institutions, unemployment-prior births, and prior births-institution interactions. We then fit OLS regression lines to each set of predicted changes. We have also added a 45° line along which the predicted changes exactly equal the actual changes. The closeness with which each regression line tracks the actual changes line indicates how well the model fits the data on actual changes. By comparing the fit of the model with institutions included to that with institutions excluded, we can get an idea of the contribution of the institutional variables to an understanding of the observed trends within countries.

Figures 4 a and b summarize our results for the employment of young men and women relative to the prime aged. In both cases, the models with institutions perform extremely well and considerably better than the models with institutions excluded. Moreover, the interaction model does even better than the main effects model for women and about the same as the main effects model for men. The results for the employment of older men and women relative to the prime aged are shown in Figure 5 a and b. The results are qualitatively similar to the findings for the young. Again, institutional main effect models improve our explanatory power for both men and women, and allowing interactions substantially improves the fit for older women.

The results for the employment of prime-aged women compared to prime-aged men are shown in Figure 6a, while Figure 6b gives comparable results for the relative unemployment of this group. The findings for relative employment of prime-aged women are similar to those for older individuals. The model with institutions included tracks the changes to some extent and performs better than a model that does not include institutions, and adding interactions leads us to do even better. Finally, the main effects results for the changes in the relative unemployment of prime-age women are very strong: the trend line fitted to the main effects model including institutions is much closer to the actual changes than the model excluding these variables. And, the model with interactions does even better and tracks the actual changes quite closely.

Overall, Figures 4-6 show that institutional change is an important component of the explanation for changes in relative employment from the 1970s to the 1990s. The interaction between institutions and overall unemployment and demographic factors is also often an important factor, but usually no more important quantitatively than adding institutional main effects. In earlier work on the overall unemployment rate, Blanchard and Wolfers (2000) and Bertola, Blau and Kahn (2002), found that interactions between institutions and macro shocks were the single most important factor explaining unemployment rate changes, although Nickell, Nunziata, Ochel and Quintini (2001) dispute whether it is necessary to appeal to such interactions. The results in Figures 4-6 show noticeable but less important effects for interactions between institutions and the economic environment for explaining relative employment changes than Blanchard and Wolfers (2000) and Bertola, Blau and Kahn (2002) find for explaining overall unemployment. Part of this discrepancy is likely to be due to the fact that we are controlling for overall unemployment here, which itself already contains the effects of institution-macro shock interactions.[23]

6.5 Omitting aggregate unemployment

So far, all of the results we have discussed have come from equations that controll for the overall unemployment rate. This implies that the positive effects of, for example, collective bargaining institutions on prime age-youth employment differentials are not simply due to high overall unemployment levels. The union effects estimated here mean that, at a given overall level of labor market tightness, more unionized economies with more centralized wage setting have lower employment of youth and older individuals relative to the prime aged. Roughly speaking, for the same size queue of job seekers (the unemployed), the young and the old are less likely to be employed relative to the prime aged in more unionized and coordinated regimes. However, by controlling for overall unemployment, we may be understating the full effects of labor market institutions on employment differentials, since institutions can also affect overall unemployment.

[23] The interaction models in Figures 4-6 include an unemployment rate-births interaction, in addition to the institution-unemployment and institution-births interactions. When we re-estimated the models excluding the unemployment rate-births interaction, the results were virtually identical to those in the Figures. We therefore conclude that the additional explanatory power we derive from the interaction models comes from interactions involving institutions.

We examined this issue by re-estimating the models of Tables 2-3, excluding the overall unemployment rate. This modification computes the full effects of institutions. Selected results from this exercise are shown in Appendix Tables A1-A2.

The results not controlling for unemployment are qualitatively very similar to those controlling for unemployment. As would be expected, the effects of collective bargaining institutions on prime aged vs. youth employment are larger in magnitude when we do not control for unemployment. This can be seen by comparing Table 2 with Table A1. The difference in magnitude suggests that these institutions raise overall unemployment which then in turn raises the employment gap between the prime-aged and the young. We caution the reader, however, that the differences in union effects across the two specifications are not statistically significant, except for the Scandinavia-North American comparison for young men vs. prime age men, in which the impact of unionization is significantly larger (at roughly the 6% level, two-tailed test) not controlling for unemployment than controlling for unemployment.

7. 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 policymakers 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 and the elderly, and relatively high unemployment rates among women, while preserving high employment rates for prime age men.

The emphasis of our theoretical perspective on distributional features could be motivated from first principles invoking financial market imperfections, or other departures from the standard representative-agent paradigm underlying competitive equilibrium (along the lines of e.g. Bertola's, forthcoming, analysis of EPL's motivation and effects). Similar assumptions could rationalize reasons for wage compression beyond simple monopoly union models. For example, union members may advocate wage compression for purposes of *ex post* insurance (Agell and Lommerud, 1992), whereby 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).

From these points of view, the non-employment of low-productivity workers brought about by higher wage floors may be something the union has to accept, in order to achieve insurance and maintain solidarity through wage compression. While employment of women, youth, and older individuals may be reduced, it is still possible that these institutional interventions raise the expected income or utility of families. These mechanisms, however, focus on employed workers and are more easily applicable to unions representing homogeneous pools of workers than to 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 (Blau and Kahn 2002). This paper's results suggest that loss of employment is the price of relatively high wages for low-productivity individuals who are *ex ante* identifiable.[24] And, 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 - at least in the absence of conceptually independent subsidies to workers who fail to obtain a job because of the union's wage structure.

[24] Acemoglu et al (2001) suggest that unions may redistribute income across workers with different skills in a model where *ex post* wage compression offers insurance and commitment benefits. Variables correlated with skills, such as age and gender, are observable and persistent in reality, and it may be less than fully realistic to suppose that they only become known after employment is obtained.

Observed employment and wage patterns, however, 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. 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, to imply that demographically biased negative employment effects of union policies that price out low-wage labor are more socially acceptable.

Appendix A: Further Theoretical Results

A.1 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:

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

To simplify this expression, define:

$$z \equiv \eta - \eta/\beta \quad \text{(A2)}$$

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

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

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.

A.2 Relative employment effects of the tax wedge

Inserting a wedge $\tau_i>0$ between the group's marginal demand and participation schedules increases group i's log unit labor costs to

$$w_i(\tau_i) = (\eta_i s_i + \varepsilon_i a_i - \eta_i \varepsilon_i n_i + \eta_i \tau_i)/(\varepsilon_i+\eta_i) = w_i(0) + \tau_i\ \eta_i/(\varepsilon_i+\eta_i) \quad \text{(A5)}$$

and decreases its employment to

$$l_i(\tau_i) = (a_i - s_i + \varepsilon_i n_i - \tau_i)/(\varepsilon_i+\eta_i) = l_i(0) - \tau_i/(\varepsilon_i+\eta_i). \quad \text{(A6)}$$

As noted in the main text, a smaller ε_i magnifies the impact of a given wedge τ_i: taking differences of expressions in the form (A5) and (A6) for groups i=1,2 with the same τ_i and η_i yields the text's expressions (9) and (10) for relative wage and employment effects. In this Appendix, we discuss the implications of different wedges τ_i and different elasticities η_i, linking these parameters to each other and to labor's welfare weight (β) in the simple framework outlined above.

Optimizing for group i (or maximizing an additively separable objective function with similar terms for this and other groups) implies that

$$\tau_i = \eta_i(\beta_i - 1)/\beta_i \quad \text{(A7).}$$

Hence:

(i.) The policy wedge is larger (and so are its wage and employment effects, for given elasticities) for groups of workers with larger β_i weight. In principle, variation of this parameter across demographic groups could be measured by institutional information or by observed markups. In practice, the relevant variables are not available on a cross-country comparable basis. To the extent that this parameter indexes the group's bargaining power, however, it seems unlikely to be relatively large for the young, female, and elderly groups that bear the brunt of labor market regulation's employment impact.

(ii.) Keeping β_i constant, the policy wedge is also larger for groups with larger η_i, since a larger wage markup is optimal for worker groups with less elastic labor demand (see, for example, Farber 1986). In addition, since the solution with tax wedge leads to the same union wage markup as the union bargaining model, lower labor demand elasticity has the same implications as shown in Appendix A.1: lower demand elasticity raises the union relative wage effect and makes for a larger reduction in union employment.

A.3 Relative unemployment effects

As noted before, optimal wage floors and optimal taxes (from the point of view of the asymmetric wage bargain) have the same implications for the wedge between the marginal revenue product of labor and the marginal willingness to work. While the wage markup under the optimal tax and union bargaining scheme is the same, the two models have different implications for unemployment. Unlike labor taxes, collective wage setting influences unemployment levels, to an extent that also depends on participation elasticities. In fact, denoting with L_d and L_s the quantities of labor demanded and supplied at the new union wage, the unemployment rate of group i,

$$U_i=(L_{si}-L_{di})/L_{si}, \tag{A8}$$

is increased not only by the decline of log employment shown by equation (8), but also by the wage increase Δw_i, shown in equation (7), which attracts $\Delta w_i/\varepsilon_i$ additional workers into the labor market. Not only the wage increase implied by a given degree of union bargaining power β and inverse labor demand elasticity η, but also that increase's impact on labor force participation are larger with groups with more elastic participation schedule (smaller ε_i).

Of course, more complicated models of labor supply could imply that workers facing fewer jobs will leave the labor force even if wages are high (Mincer 1976). But this Appendix shows that unemployment effects of unions could be even larger than employment effects if high wage floors attract people into the labor force.

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.

The labor force and population data are taken directly from the ILO database "Economically Active Population 1950-2010". The name format of the labor force and population data is **vv(v)gcccc** where

vvv = lf for labor force,
vvv = pop for population
g = m for male, f for female
cccc=xxyy for age group from xx to yy years of age (65_ refers to 65 and above)

The data on unemployment rates by age group have been 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. The name format of the unemployment data is ur**gxxyy** where **g**= m (male) or f (female), and **xxyy**=age group from xx to yy years of age (55_ refers to 55 and above).

The measures for the labor market institutions are taken from several sources. We use Blanchard and Wolfers' (2000) measures of time-varying employment protection legislation, and we collected additional institutional indicators.

We take union density, collective bargaining coverage and coordination, and labor tax rate data from the data appendix to Nickell, Nunziata, Ochel and Quintini (2001). 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 refer to the entire 1960-96 period. Data on retirement system characteristics were taken from Blöndal and Scarpetta (1999).

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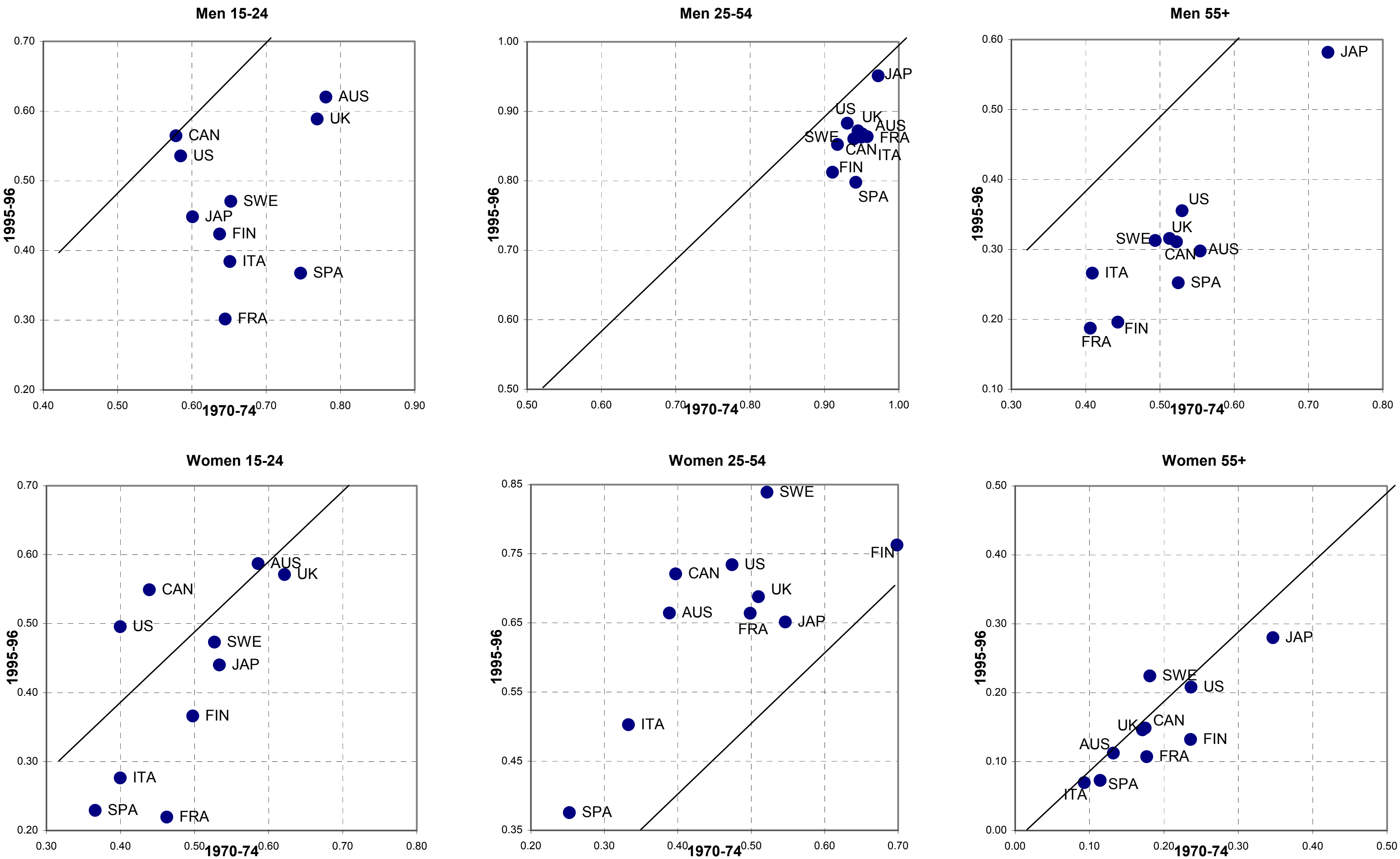
Figure 1: Employment-to-Population Ratios By Gender and Age, Early 1970s and Mid-1990s



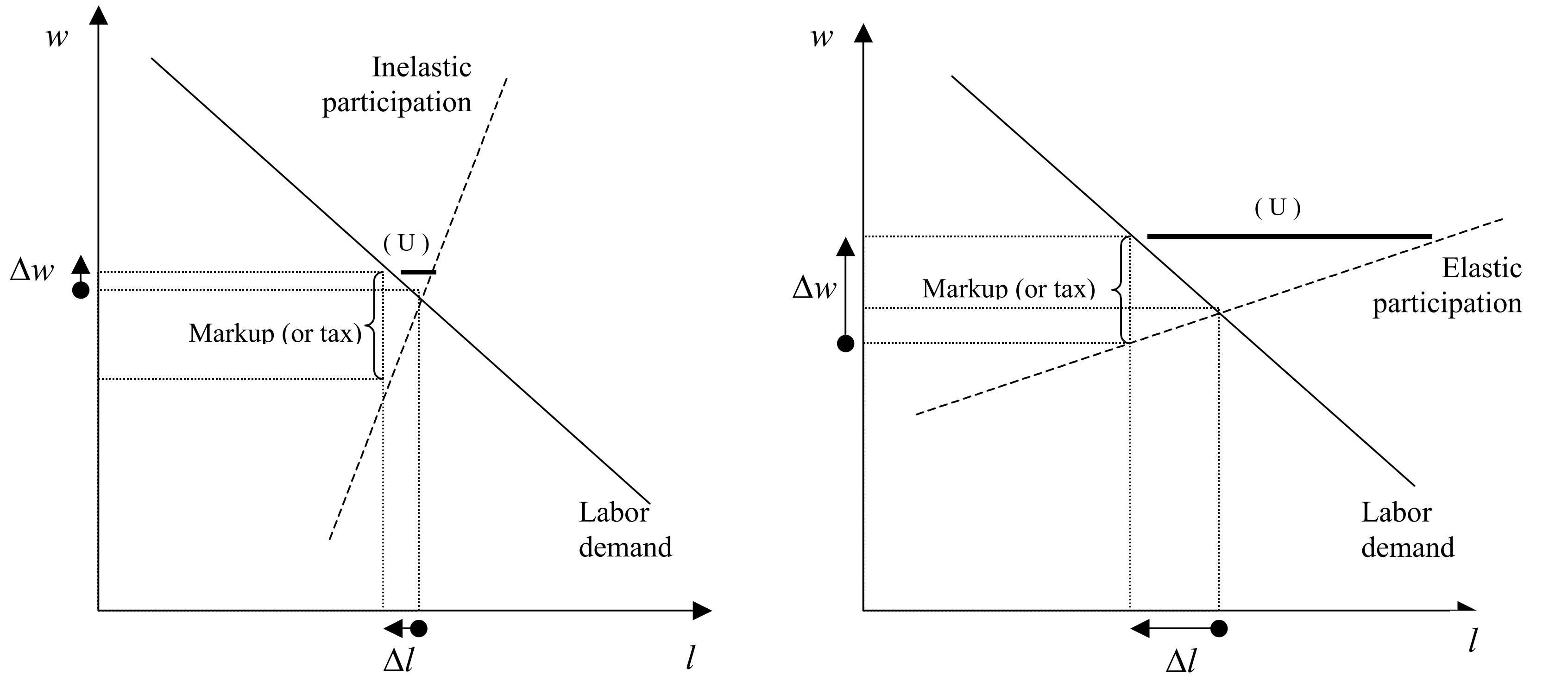
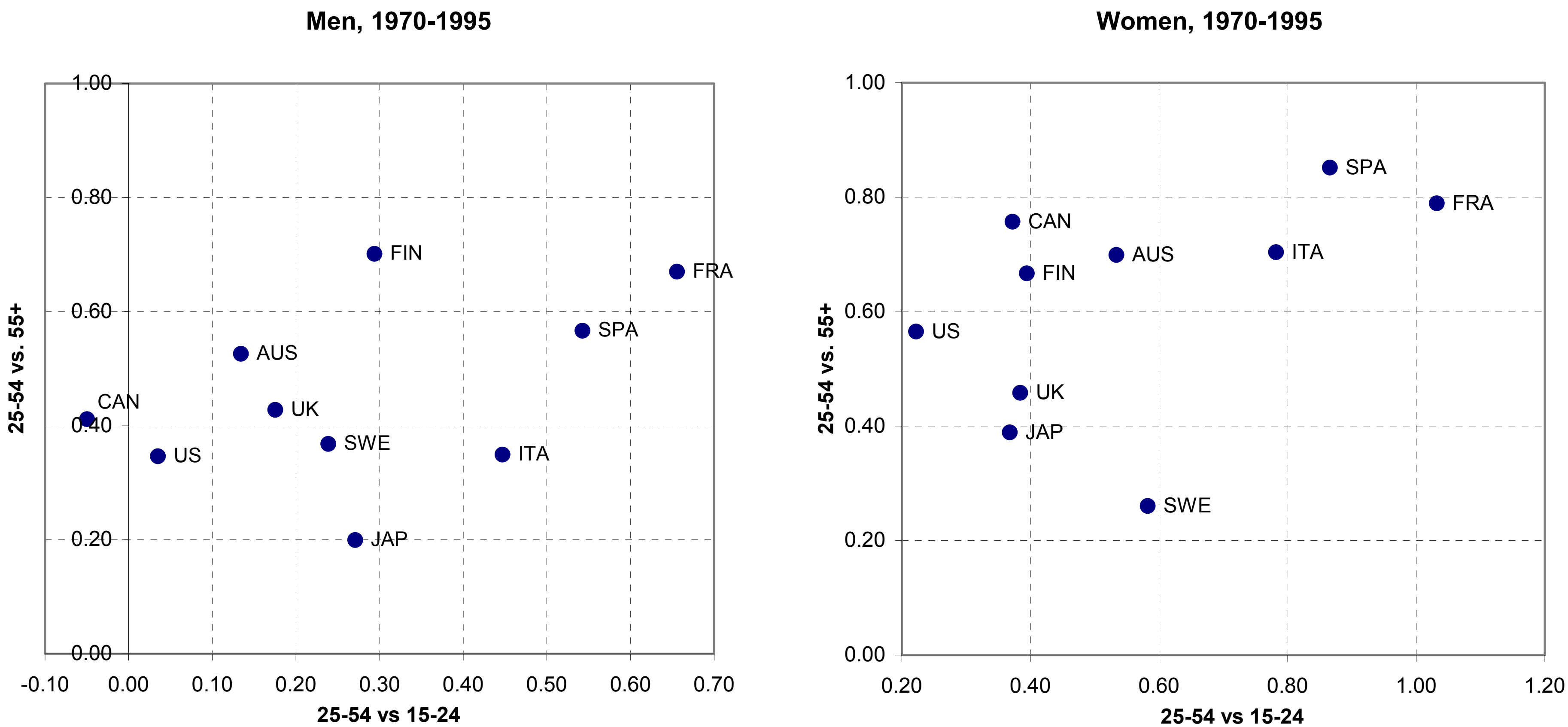


Figure 2: The different effects of similar institutional wedges for groups with differently sloped market participation schedules. Arrows indicate wage and employment effects relative to *laissez faire*; thick lines denote unemployment, if the wedge is implemented by minimum wage.

Figure 3: 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.

Figure 4a: Actual and Predicted Changes in Relative Employment (1975-95), 14 Countries: Men 25-54 vs. Men 15-24

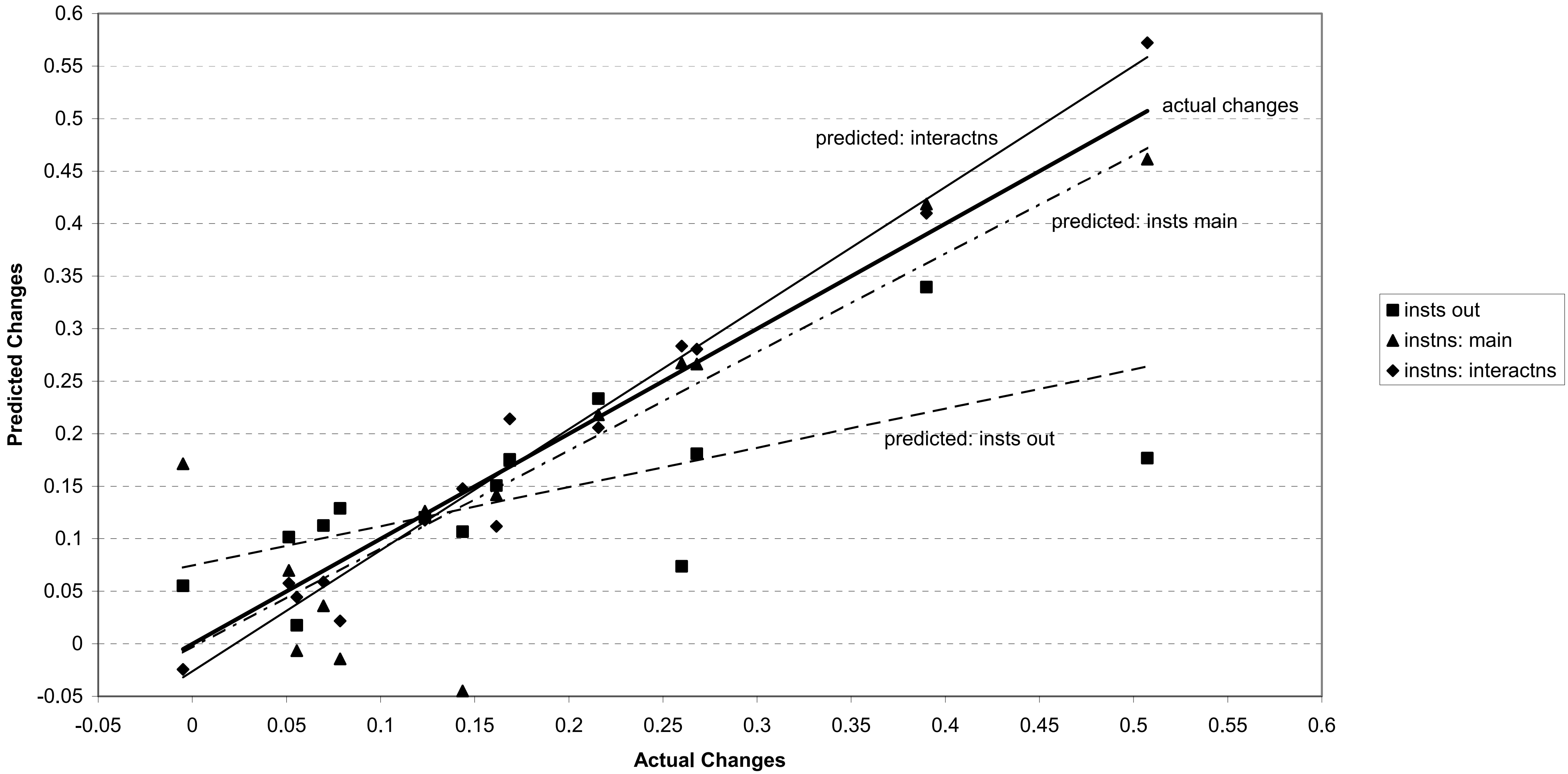


Figure 4b: Actual and Predicted Changes in Relative Employment (1975-95), 14 Countries: Women 25-54 vs. Women 15-24

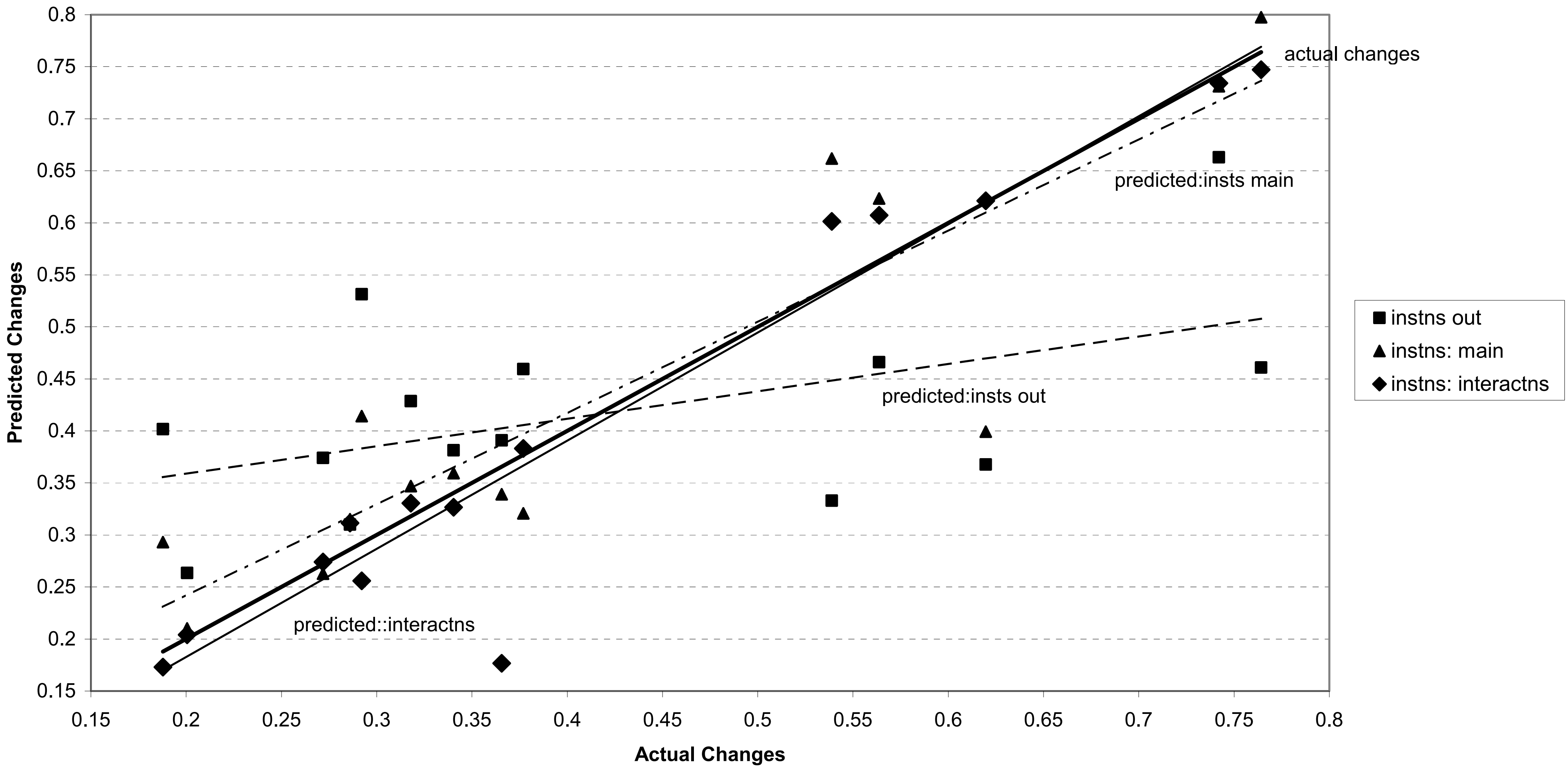


Figure 5a: Actual and Predicted Changes in Relative Employment (1975-95), 14 Countries: Men 25-54 vs. Men 55+

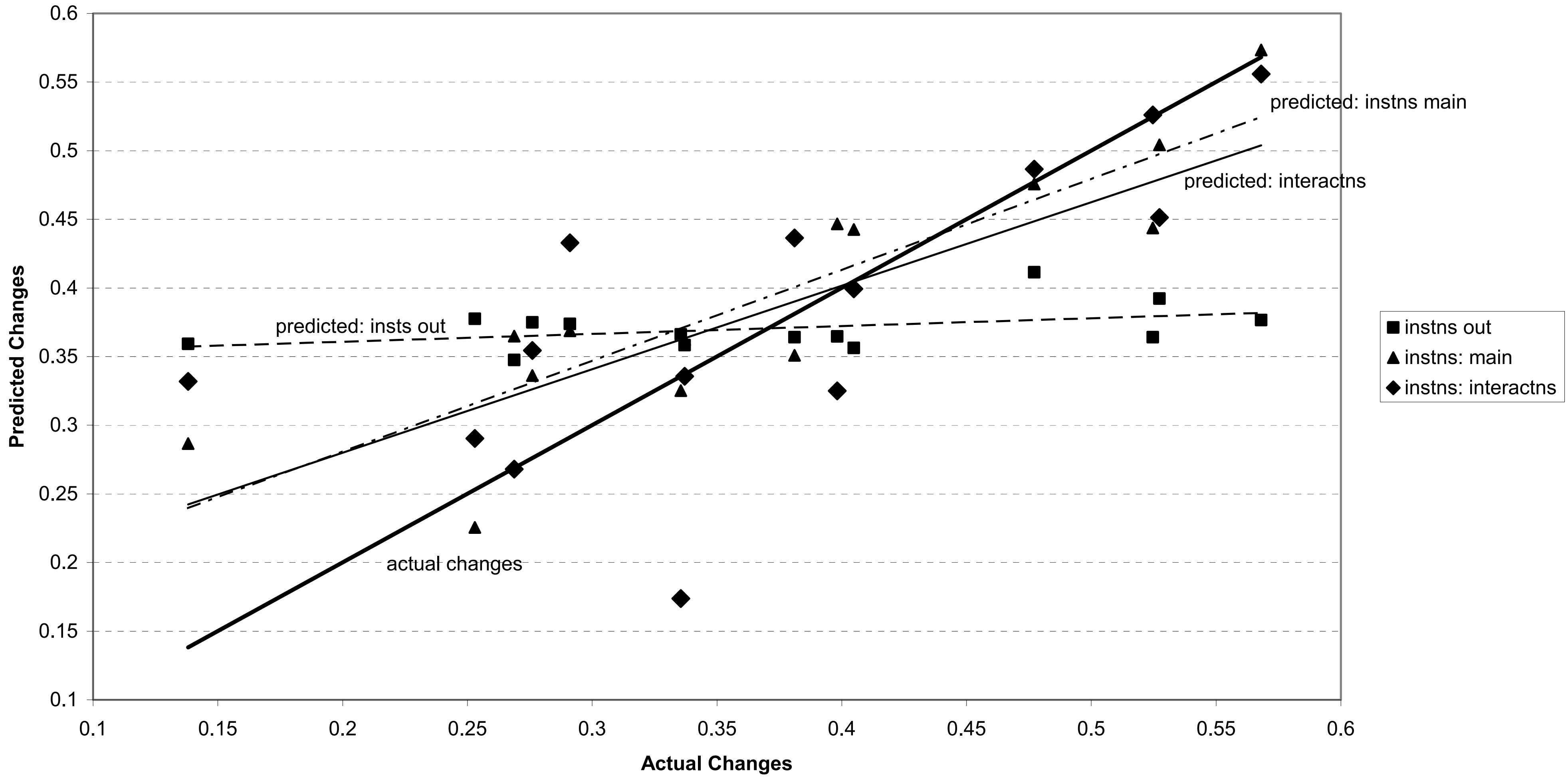


Figure 5b: Actual and Predicted Changes in Relative Employment (1975-95), 14 Countries: Women 25-54 vs. Women 55+

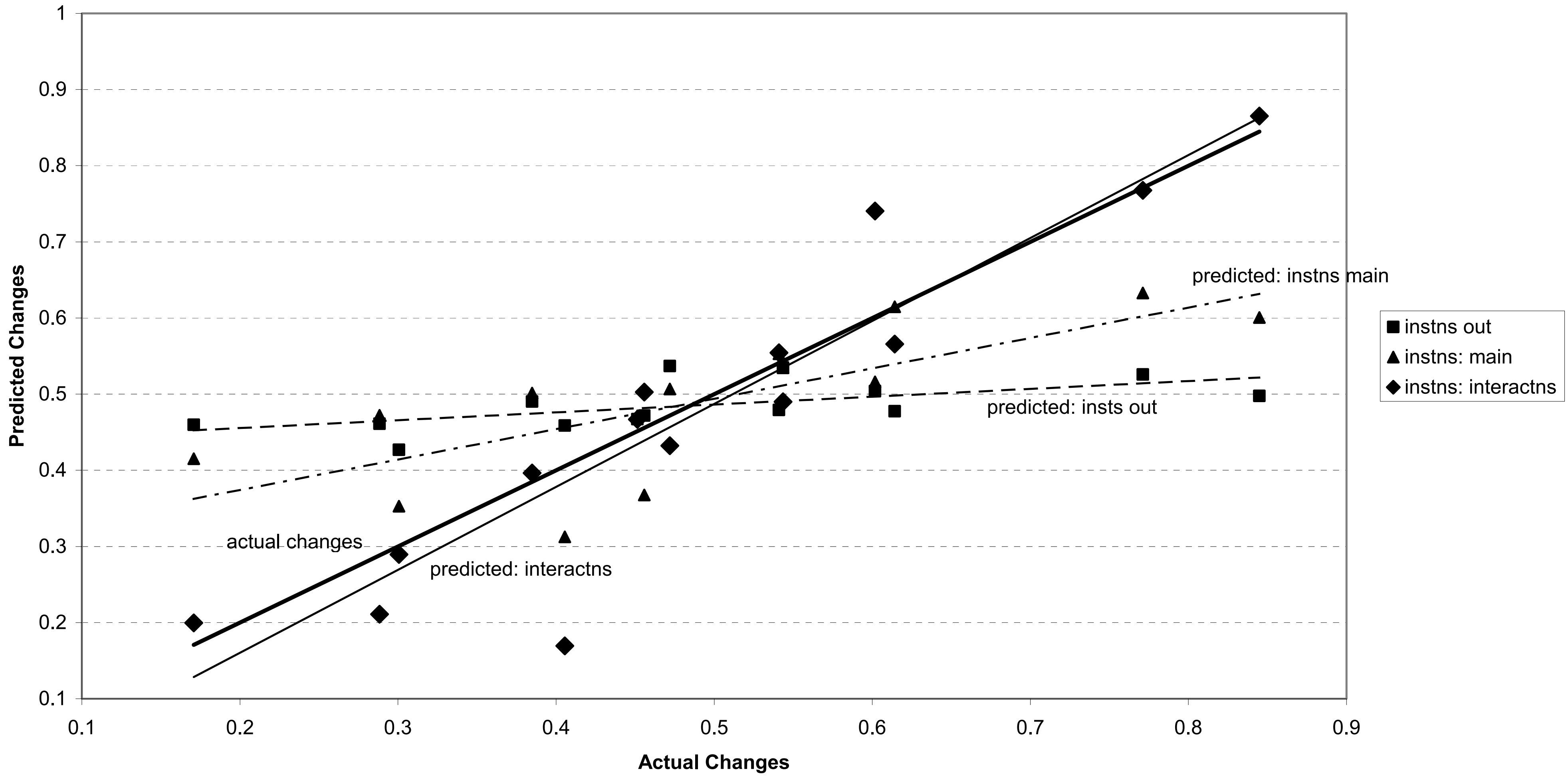


Figure 6a: Actual and Predicted Changes in Relative Employment (1975-95), 14 Countries: Men 25-54 vs. Women 25-54

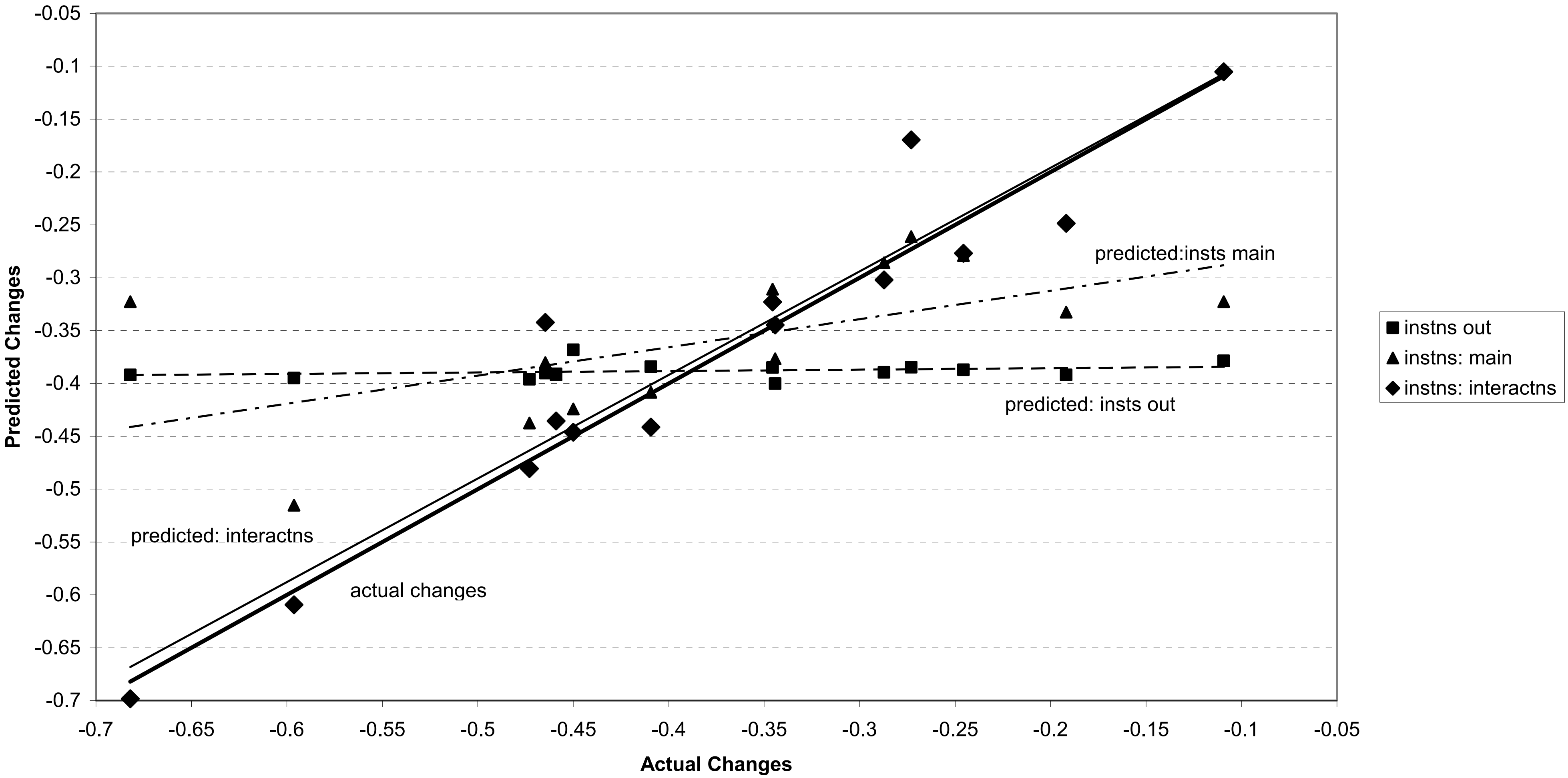


Figure 6b: Actual and Predicted Changes in Relative Unemployment (1975-95), 14 Countries: Men 25-54 vs. Women 25-54

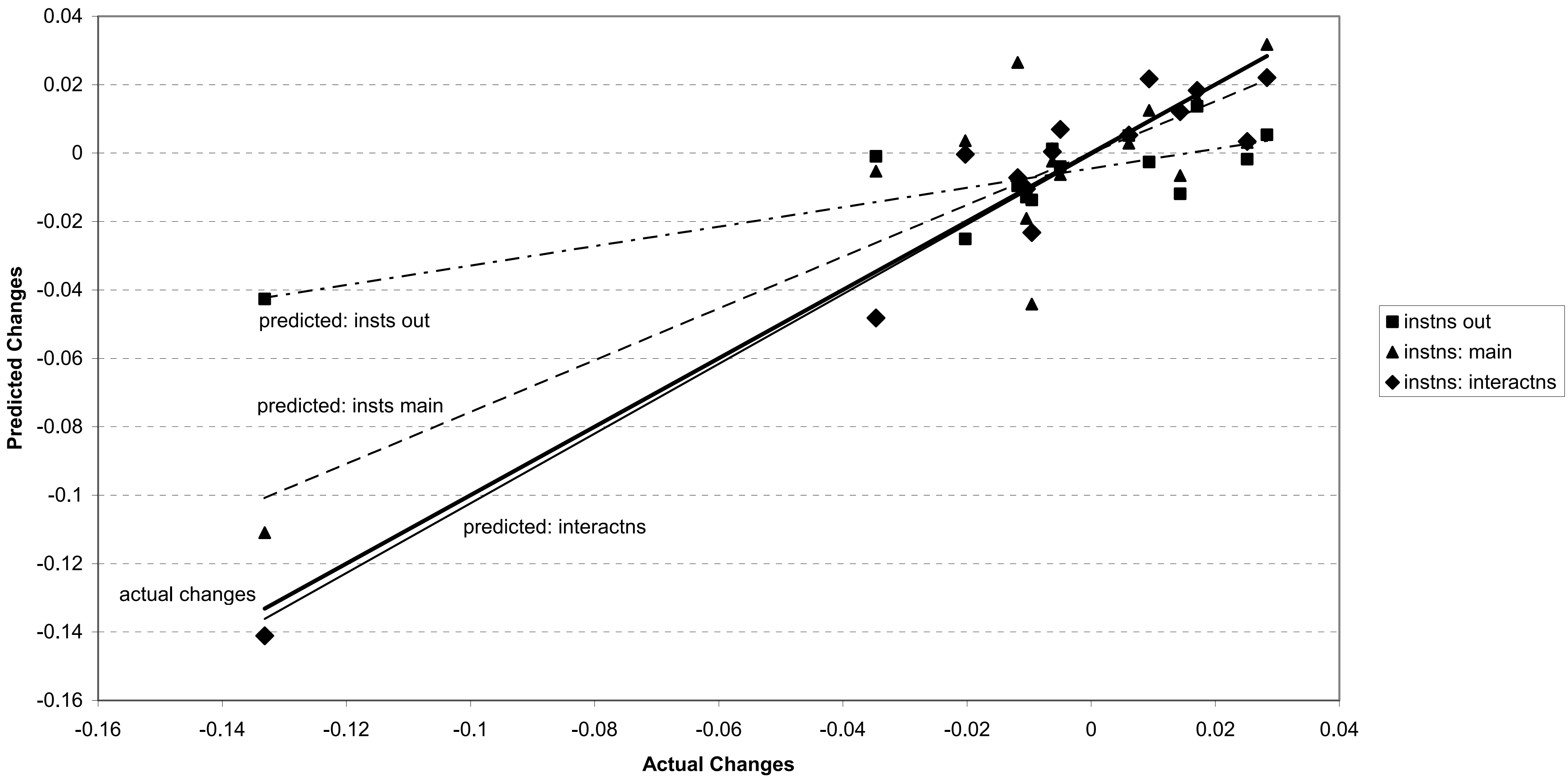


Table 1: Relative Employment and Institutional Patterns in Selected Countries, 1970-1995

Relative employment: (1)	Men, 25-54 vs. 15-24		Men, 25-54 vs. 55+		Women, 25-54 vs. 15-24		Women, 25-54 vs. 55+		15-24, Men vs. Women		25-54, Men vs. Women		55+, Men vs. Women	
	1970	change 70-95	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	0.20	0.13	0.54	0.53	-0.41	0.53	1.08	0.70	0.29	-0.23	0.89	-0.63	1.43	-0.46
CANADA	0.46	-0.05	0.58	0.41	-0.10	0.37	0.82	0.76	0.28	-0.25	0.84	-0.67	1.08	-0.32
FINLAND	0.36	0.29	0.72	0.70	0.34	0.39	1.09	0.67	0.25	-0.10	0.27	-0.20	0.63	-0.24
FRANCE	0.40	0.66	0.86	0.67	0.08	1.03	1.04	0.79	0.33	-0.01	0.65	-0.39	0.83	-0.27
ITALY	0.37	0.45	0.84	0.35	-0.18	0.78	1.28	0.70	0.49	-0.16	1.04	-0.49	1.48	-0.14
JAPAN	0.48	0.27	0.29	0.20	0.02	0.37	0.46	0.39	0.12	-0.10	0.58	-0.20	0.74	-0.01
SPAIN	0.23	0.54	0.59	0.57	-0.37	0.87	0.79	0.85	0.71	-0.24	1.32	-0.56	1.52	-0.28
SWEDEN	0.36	0.24	0.64	0.37	-0.01	0.58	1.06	0.26	0.21	-0.22	0.59	-0.56	1.00	-0.67
UK	0.21	0.18	0.60	0.43	-0.20	0.38	1.09	0.46	0.21	-0.18	0.62	-0.39	1.12	-0.36
USA	0.46	0.03	0.56	0.35	0.17	0.22	0.70	0.57	0.38	-0.30	0.67	-0.49	0.81	-0.27
NON-US AVERAGE	0.34	0.30	0.63	0.47	-0.09	0.59	0.97	0.62	0.32	-0.17	0.76	-0.46	1.09	-0.31

Labor market institutions:	Collective Bargaining Coverage		Coordination		Union Density		Labor Tax Rate		Employment Protection Index		UI Replacement Rate: First Year		UI Replacement Rate: Fifth 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	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	0.12	0.10
CANADA	40.0	-3.00	1.00	0.00	30.62	6.78	42.44	9.56	0.60	0.00	0.49	0.09	0.10	0.00
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	0.10	0.06
FRANCE	85.0	8.50	1.75	0.25	21.70	-11.80	57.91	10.09	1.97	1.13	0.47	0.08	0.07	0.06
ITALY	85.0	-2.50	1.50	1.00	37.00	1.70	55.71	15.29	4.00	-0.60	0.04	0.11	0.00	0.00
JAPAN	28.0	-6.00	3.00	0.00	31.74	-7.94	25.88	-1.88	2.80	0.00	0.41	-0.12	0.00	0.00
SPAIN	68.0	9.00	2.00	0.00	9.00	9.20	25.91	20.09	4.00	-0.90	0.38	0.27	0.00	0.00
SWEDEN (2)	86.0	1.50	2.50	-0.50	66.76	23.22	59.47	14.53	1.20	1.20	0.24	0.49	0.00	0.00
UK	70.0	-23.00	1.50	-0.50	49.80	-13.10	43.19	3.81	0.58	0.12	0.31	-0.13	0.16	-0.03
USA	21.0	-3.50	1.00	0.00	27.24	-12.34	40.06	5.94	0.20	0.00	0.20	0.07	0.04	0.00
NON-US AVERAGE	71.33	-2.28	1.97	-0.06	37.92	3.13	43.82	10.18	2.06	0.07	0.30	0.14	0.06	0.02

Table 1: Relative Employment and Institutional Patterns in Selected Countries, 1970-1995 (ctd)

Labor market institutions:	Retirement Benefits Wage Replacement Ratio		Wage Replacement Ratio for Older Workers, Disability Schemes		Wage Replacement Ratio for Older Workers, Unemployment Schemes		10-Year Retirement Benefit Accrual Rate, Males Age 55(3)	
	1970	change 70-95	1970	change 70-95	1970	change 70-95	1970	change 70-95
AUSTRALIA	0.30	0.11	0.20	0.08	0.21	0.06	0.00	0.00
CANADA	0.42	0.09	0.22	0.11	0.16	0.01	0.19	-0.19
FINLAND	0.54	0.06	0.46	0.14	0.30	0.34	0.09	-0.05
FRANCE	0.60	0.05	0.50	-0.25	0.38	-0.15	0.24	-0.07
ITALY	0.62	0.18	0.48	0.12	0.25	0.49	0.22	-0.12
JAPAN	0.48	0.04	0.16	0.09	0.04	-0.01	0.05	-0.02
SPAIN	0.50	0.50	0.55	0.16	0.42	-0.05	0.00	0.00
SWEDEN (2)	0.72	0.02	0.74	0.00	0.12	0.03	0.17	-0.17
UK	0.34	0.16	0.33	-0.05	0.19	-0.02	0.02	0.08
USA	0.47	0.09	0.38	0.07	0.06	0.00	0.00	0.00
NON-US AVERAGE	0.50	0.14	0.41	0.04	0.23	0.08	0.11	-0.06

(1) Country-specific levels and changes, for the 1970-74 and 1995-96 periods, of the difference in the logarithms of employment rates across the age and gender groups indicated.

(2) Note that for Sweden, due to data availability, the following collective bargaining infomation is shown above: 1990 data for 1970 and the average of 1990 and 1994 data for 1990.

(3) Increase in Retirement Benefit Replacement Rate for a 55-year old male who works 10 more years. Data shown are for 1967 and 1995.

Table 2: Selected Generalized Least Squares Regression Results for Relative Employment

Explanatory Variables	log(epop men2554/epop men1524)		log(epop men2554/epop men55+)		log(epop women 2554/epop women1524)		log(epop women2554/epop women55+)		log(epop men2554/epop women2554)	
	Coeff	Std Err	Coeff	Std Err	Coeff	Std Err	Coeff	Std Err	Coeff	Std Err
overall unemployment rate	1.5942	0.2387	0.4033	0.2215	1.9120	0.3794	-0.0494	0.3623	0.3507	0.3936
prior births/population	2.0193	4.9472	-2.9796	3.1092	12.1457	3.8803	-17.1193	4.6425	-0.8606	5.2685
coll barg coverage	0.0028	0.0008	0.0036	0.0006	0.0055	0.0017	0.0061	0.0008	-0.0016	0.0012
coordination	0.0789	0.0447	0.0340	0.0276	0.0526	0.0423	0.0834	0.0452	0.0158	0.0455
union density	-0.0021	0.0009	0.0041	0.0008	-0.0047	0.0014	0.0026	0.0013	0.0035	0.0016
employment protection	-0.0103	0.0160	0.0554	0.0145	0.0857	0.0185	0.0133	0.0218	-0.0142	0.0198
UI rep rate: year 1	-0.0494	0.0605	-0.0821	0.0333	-0.2034	0.0729	-0.0870	0.0690	-0.2110	0.0696
UI rep rate: year 5	-0.0751	0.1077	-0.1833	0.0564	-0.1517	0.1816	-0.3449	0.1014	0.1243	0.1261
labor tax rate	0.3389	0.2107	-0.2946	0.1357	0.0137	0.2053	-0.2395	0.2491	0.0919	0.2185
public pension replacement rate	0.0016	0.0017	-0.0004	0.0011	0.0049	0.0018	0.0050	0.0021	-0.0037	0.0020
accrual rate, 10 yrs, age 55	-0.0009	0.0034	0.0153	0.0019	-0.0025	0.0037	0.0015	0.0026	0.0069	0.0035
UI rep rate: older workers	0.1650	0.0963	0.2953	0.0639	0.4475	0.1236	0.0756	0.1000	0.0495	0.1480
Disability rep rate: older workers	0.0021	0.3485	0.2147	0.2222	-0.1256	0.3574	-0.5769	0.3332	-0.3038	0.3524
female retirement age	-0.0229	0.0119	-0.0311	0.0058	-0.0171	0.0118	0.0017	0.0065	0.0031	0.0103
male retirement age	-0.0348	0.0190	-0.0297	0.0084	-0.0242	0.0190	0.0081	0.0129	-0.0010	0.0195
country dummies	yes		yes		yes		yes		yes	
period effects	yes		yes		yes		yes		yes	
sample size	101		101		101		101		101	
<u>Net effect of collective bargaining coverage, union density and coordination:</u>										
Scandinavia vs. North America	0.1647	p=.0334	0.3988	p<.0001	0.1624	p=.1110	0.5285	p<.0001	0.0813	p=.3573
Northern Europe vs. North America	0.2493	p=.0002	0.2729	p<.0001	0.3610	p=.0003	0.4661	p<.0001	-0.0554	p=.4034
Southern Europe vs. North America	0.1868	p=.0002	0.2258	p<.0001	0.2740	p=.0005	0.3723	p<.0001	-0.0384	p=.4602
Total Sample other than US vs US	0.1760	p=.0033	0.3087	p<.0001	0.2283	p=.0091	0.4481	p<.0001	0.0128	p=.8411

Note: 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. Joint effect of collective bargaining coverage, union density and coordination is evaluated at sample differences between the two groups (e.g. Scandinavia vs. North America) for these variables. Scandinavia includes Denmark, Sweden, Finland and Norway; North America includes Canada and the US; Northern Europe includes Germany, France, the Netherlands and Belgium; Southern Europe includes Italy, Portugal and Spain.

Table 3: Selected Generalized Least Squares Regression Results for Relative Unemployment

Explanatory Variables	(urate men2554 - urate men1524)		(urate men2554 - urate men55+)		(urate women2554 - urate women1524)		(urate women2554 - urate women55+)		(urate men2554 - urate women2554)	
	Coeff	Std Err	Coeff	Std Err	Coeff	Std Err	Coeff	Std Err	Coeff	Std Err
overall unemployment rate	-1.0047	0.0911	0.0449	0.0589	-1.4358	0.1220	0.1985	0.0708	-0.0397	0.0660
prior births/population	-3.8631	1.0275	1.6949	0.5831	-3.1016	1.6560	3.4742	0.6161	-2.5510	0.6913
coll barg coverage	0.0003	0.0003	-0.0004	0.0001	0.0008	0.0004	0.0002	0.0002	-0.0008	0.0003
coordination	-0.0136	0.0096	0.0081	0.0063	-0.0131	0.0143	0.0210	0.0069	-0.0066	0.0078
union density	0.0007	0.0003	-0.0002	0.0002	0.0005	0.0004	-0.0006	0.0002	-0.0003	0.0002
employment protection	-0.0112	0.0059	-0.0026	0.0028	-0.0176	0.0065	0.0034	0.0045	-0.0044	0.0032
UI rep rate: year 1	0.0192	0.0175	0.0159	0.0087	0.0240	0.0236	0.0076	0.0119	0.0255	0.0121
UI rep rate: year 5	-0.0114	0.0317	-0.0324	0.0166	-0.0341	0.0361	-0.0095	0.0255	0.0660	0.0225
labor tax rate	0.0468	0.0515	-0.1107	0.0254	0.2265	0.0631	-0.1425	0.0329	0.1430	0.0378
public pension replacement rate	0.0014	0.0005	0.0013	0.0003	0.0009	0.0008	0.0036	0.0003	-0.0029	0.0004
accrual rate, 10 yrs, age 55	0.0008	0.0009	-0.0028	0.0004	0.0029	0.0013	-0.0027	0.0006	0.0008	0.0005
UI rep rate: older workers	0.0069	0.0267	-0.0317	0.0142	-0.0448	0.0344	-0.0084	0.0207	-0.0606	0.0190
Disability rep rate: older workers	-0.1825	0.0907	-0.1510	0.0466	-0.1847	0.1281	-0.1066	0.0563	0.0369	0.0572
female retirement age	0.0063	0.0025	0.0008	0.0011	0.0091	0.0035	0.0015	0.0017	-0.0036	0.0016
male retirement age	-0.0012	0.0042	0.0060	0.0020	-0.0028	0.0070	0.0047	0.0032	0.0055	0.0025
country dummies	yes		yes		yes		yes		yes	
period effects	yes		yes		yes		yes		yes	
sample size	101		101		101		101		101	
<u>Net effect of collective bargaining coverage, union density and coordination:</u>										
Scandinavia vs. North America	0.0224	p=.2285	-0.0161	p=.1941	0.0448	p=.1324	0.0122	p=.3965	-0.0617	p=.0005
Northern Europe vs. North America	0.0016	p=.9347	-0.0132	p=.1741	0.0329	p=.2372	0.0317	p=.0135	-0.0540	p=.0004
Southern Europe vs. North America	0.0045	p=.7631	-0.0120	p=.1044	0.0293	p=.1696	0.0217	p=.0297	-0.0436	p=.0004
Total Sample other than US vs US	0.0140	p=.3814	-0.0151	p=.1005	0.0385	p=.1135	0.0162	p=.1622	-0.0527	p=.0002

Note: 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. Joint effect of collective bargaining coverage, union density and coordination is evaluated at sample differences between the two groups (e.g. Scandinavia vs. North America) for these variables. Scandinavia includes Denmark, Sweden, Finland and Norway; North America includes Canada and the US; Northern Europe includes Germany, France, the Netherlands and Belgium; Southern Europe includes Italy, Portugal and Spain.

Table A1: Selected GLS Regression Results for Relative Employment, Unemployment Rate Excluded

Explanatory Variables	log(epop men2554/epop men1524)		log(epop men2554/epop men55+)		log(epop women 2554/epop women1524)		log(epop women2554/epop women55+)		log(epop men2554/epop women2554)	
	Coeff	Std Err	Coeff	Std Err	Coeff	Std Err	Coeff	Std Err	Coeff	Std Err
overall unemployment rate	----	----	----	----	----	----	----	----	----	----
prior births/population	8.8967	5.3054	-1.9542	3.0090	17.1633	3.6909	-11.6889	4.3496	-0.6167	5.2024
coll barg coverage	0.0040	0.0011	0.0037	0.0006	0.0061	0.0020	0.0051	0.0007	-0.0014	0.0012
coordination	0.1111	0.0496	0.0290	0.0279	0.0418	0.0518	0.0132	0.0493	0.0167	0.0457
union density	0.0007	0.0010	0.0047	0.0008	-0.0018	0.0014	0.0041	0.0011	0.0039	0.0015
employment protection	-0.0240	0.0184	0.0472	0.0142	0.0410	0.0203	0.0072	0.0148	-0.0259	0.0171
UI rep rate: year 1	-0.0665	0.0750	-0.0724	0.0327	-0.1389	0.0782	-0.0631	0.0562	-0.1912	0.0677
UI rep rate: year 5	-0.2177	0.1282	-0.1999	0.0535	-0.2462	0.1801	-0.3298	0.0817	0.1069	0.1274
labor tax rate	0.5002	0.2233	-0.2268	0.1365	0.2041	0.2008	-0.0326	0.2487	0.1166	0.2145
public pension replacement rate	0.0035	0.0022	0.0004	0.0010	0.0089	0.0020	0.0050	0.0018	-0.0027	0.0016
accrual rate, 10 yrs, age 55	0.0036	0.0039	0.0155	0.0019	-0.0021	0.0039	-0.0039	0.0030	0.0068	0.0035
UI rep rate: older workers	0.1880	0.1030	0.2896	0.0635	0.4258	0.1294	0.0804	0.0936	0.0533	0.1474
Disability rep rate: older workers	-0.1842	0.4015	0.1257	0.2205	-0.6258	0.3789	-1.0448	0.2737	-0.4290	0.3256
female retirement age	-0.0119	0.0125	-0.0310	0.0059	-0.0154	0.0127	0.0095	0.0072	0.0041	0.0104
male retirement age	-0.0374	0.0214	-0.0298	0.0084	-0.0180	0.0191	0.0246	0.0120	-0.0007	0.0194
country dummies	yes		yes		yes		yes		yes	
period effects	yes		yes		yes		yes		yes	
sample size	101		101		101		101		101	
<u>Net effect of collective bargaining coverage, union density and coordination:</u>										
Scandinavia vs. North America	0.3807	p<.0001	0.4177	p<.0001	0.2984	p=.0093	0.4426	p<.0001	0.1071	p=.1840
Northern Europe vs. North America	0.3700	p<.0001	0.2726	p<.0001	0.3984	p=.0008	0.3306	p<.0001	-0.0430	p=.4916
Southern Europe vs. North America	0.2870	p<.0001	0.2275	p<.0001	0.3123	p=.0009	0.2748	p<.0001	-0.0276	p=.5695
Total Sample other than US vs US	0.3289	p<.0001	0.3187	p<.0001	0.3131	p=.0022	0.3590	p<.0001	0.0307	p=.5952

Note: 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. Joint effect of collective bargaining coverage, union density and coordination is evaluated at sample differences between the two groups (e.g. Scandinavia vs. North America) for these variables. Scandinavia includes Denmark, Sweden, Finland and Norway; North America includes Canada and the US; Northern Europe includes Germany, France, the Netherlands and Belgium; Southern Europe includes Italy, Portugal and Spain.

Table A2: Selected GLS Regression Results for Relative Unemployment, Unemployment Rate Excluded

Explanatory Variables	(urate men2554 - urate men1524)		(urate men2554 - urate men55+)		(urate women2554 - urate women1524)		(urate women2554 - urate women55+)		(urate men2554 - urate women2554)	
	Coeff	Std Err	Coeff	Std Err	Coeff	Std Err	Coeff	Std Err	Coeff	Std Err
overall unemployment rate	----	----	----	----	----	----	----	----	----	----
prior births/population	-6.7270	1.1743	1.8892	0.5724	-6.9801	1.7053	4.1169	0.4952	-2.5826	0.6899
coll barg coverage	-0.0003	0.0004	-0.0004	0.0001	0.0003	0.0005	0.0003	0.0002	-0.0008	0.0003
coordination	-0.0061	0.0134	0.0086	0.0063	-0.0100	0.0202	0.0176	0.0067	-0.0069	0.0075
union density	-0.0011	0.0004	-0.0002	0.0002	-0.0017	0.0006	-0.0003	0.0002	-0.0004	0.0002
employment protection	0.0019	0.0066	-0.0033	0.0025	-0.0028	0.0097	-0.0014	0.0040	-0.0034	0.0028
UI rep rate: year 1	0.0314	0.0232	0.0174	0.0084	0.0179	0.0348	0.0035	0.0114	0.0245	0.0120
UI rep rate: year 5	0.0577	0.0456	-0.0346	0.0161	0.0351	0.0560	-0.0188	0.0233	0.0691	0.0218
labor tax rate	0.0316	0.0624	-0.1114	0.0249	0.1108	0.0794	-0.1186	0.0291	0.1406	0.0376
public pension replacement rate	-0.0017	0.0007	0.0014	0.0002	-0.0024	0.0011	0.0038	0.0003	-0.0030	0.0003
accrual rate, 10 yrs, age 55	0.0000	0.0011	-0.0028	0.0004	0.0027	0.0017	-0.0023	0.0006	0.0007	0.0004
UI rep rate: older workers	-0.0458	0.0341	-0.0339	0.0136	-0.0665	0.0519	0.0071	0.0200	-0.0626	0.0189
Disability rep rate: older workers	0.0830	0.1035	-0.1559	0.0450	0.0724	0.1561	-0.1461	0.0534	0.0470	0.0541
female retirement age	0.0058	0.0029	0.0006	0.0011	0.0039	0.0050	0.0013	0.0015	-0.0037	0.0016
male retirement age	-0.0019	0.0052	0.0062	0.0020	-0.0032	0.0089	0.0038	0.0030	0.0057	0.0026
country dummies	yes		yes		yes		yes		yes	
period effects	yes		yes		yes		yes		yes	
sample size	101		101		101		101		101	
<u>Net effect of collective bargaining coverage, union density and coordination:</u>										
Scandinavia vs. North America	-0.0680	p=.0228	-0.0136	p=.2473	-0.0640	p=.1176	0.0247	p=.0927	-0.0658	p<.0001
Northern Europe vs. North America	-0.0304	p=.2887	-0.0116	p=.2260	-0.0004	p=.9916	0.0350	p=.0163	-0.0557	p=.0002
Southern Europe vs. North America	-0.0263	p=.2365	-0.0107	p=.1391	-0.0031	p=.9140	0.0254	p=.0247	-0.0451	p=.0001
Total Sample other than US vs US	-0.0455	p=.0708	-0.0132	p=.1314	-0.0298	p=.3647	0.0243	p=.0521	-0.0554	p<.0001

Note: 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. Joint effect of collective bargaining coverage, union density and coordination is evaluated at sample differences between the two groups (e.g. Scandinavia vs. North America) for these variables. Scandinavia includes Denmark, Sweden, Finland and Norway; North America includes Canada and the US; Northern Europe includes Germany, France, the Netherlands and Belgium; Southern Europe includes Italy, Portugal and Spain.