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Abstract

The authors investigate how labor market institutions such as unemployment insurance, unions, firing regulations, and minimum wages have affected the evolution of wage inequality among male workers. Results of estimations using data on institutions in eleven OECD countries indicate that changes in labor market institutions can account for much of the change in wage inequality between 1973 and 1998. Factors found to have been negatively associated with male wage inequality are union density, the strictness of employment protection law, unemployment benefit duration, unemployment benefit generosity, and the size of the minimum wage. Over the 26-year period, institutional changes were associated with a 23% reduction in male wage inequality in France, where minimum wages increased and employment protection became stricter, but with an increase of up to 11% in the United States and United Kingdom, where unions became less powerful and (in the United States) minimum wages fell.

KEYWORDS: labor market Institutions and wage inequality

LABOR MARKET INSTITUTIONS AND WAGE INEQUALITY

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The authors investigate how labor market institutions such as unemployment insurance, unions, firing regulations, and minimum wages have affected the evolution of wage inequality among male workers. Results of estimations using data on institutions in eleven OECD countries indicate that changes in labor market institutions can account for much of the change in wage inequality between 1973 and 1998. Factors found to have been negatively associated with male wage inequality are union density, the strictness of employment protection law, unemployment benefit duration, unemployment benefit generosity, and the size of the minimum wage. Over the 26-year period, institutional changes were associated with a 23% reduction in male wage inequality in France, where minimum wages increased and employment protection became stricter, but with an increase of up to 11% in the United States and United Kingdom, where unions became less powerful and (in the United States) minimum wages fell.

Wage inequality is substantially lower in continental European countries than in the United States and United Kingdom, and its evolution over time has differed greatly across countries. The same holds true for the skill (or education) wage premium. Changes in the supply of and demand for skills are unlikely to fully account for these marked differences (Acemoglu 2003). A substantial amount of research on wage inequality has examined the forces that may shift the relative demand for skills, such as changing trade patterns and skill-biased technical change. However, since developed economies operate in the same global environment, with integrated trade and equal access to technology, exogenous shifts in demand are likely to have been fairly similar across these countries; and on the supply side, the proportion of the work force that is educated has risen throughout these economies, although the education systems have expanded at different times. Hence, differences across these countries in the evolution of wage inequality seem likely to reflect, in part, country-specific variation in the way labor market institutions have changed.

In this paper we use panel data on institutions in OECD countries to determine how much of the increase in wage inequality can be attributed to changes in institutions within countries. Our study extends previous research in several directions. By assessing the quantitative relationship between institutions and male wage inequality, we build on the literature investigating the determinants of unemployment rates (see, for example,

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The data and computer programs used for this paper are available from the authors upon request. Contact Marco Leonardi, Department of Labor Studies, University of Milan, via Conservatorio 7, 20122 Milan, Italy; marco.leonardi@unimi.it.

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Blanchard and Wolfers 2000 and, especially, Nickell et al. 2005) and average labor costs (Nunziata 2005) across OECD countries. Under the "Krugman hypothesis," macroeconomic shocks increase wage inequality in countries where wages are flexible and unemployment in countries where wages are constrained by institutions. Thus, it has been argued that the effect of such institutions on the wage differential can be considered as just the other side of the same coin (Bertola et al. 2002).

Much of the previous empirical literature has studied how specific labor market institutions affect wage differentials. For example, Card (2001) for the United States, Machin (1997) for Britain, Card et al. (2004) for a comparison of the United States, the United Kingdom, and Canada, and Kahn (2000) for OECD countries have found that higher union density is associated with lower wage inequality. DiNardo et al. (1996), Lee (1999) for the United States, and Dickens et al. (1999) for the United Kingdom have found that higher minimum wages reduce wage inequality. Moreover, wage-setting institutions have been found to be important for wage inequality by Erickson and Ichino (1995) and Manacorda (2004) for Italy and by Edin and Holmlund (1995) for Sweden. We broaden the scope of investigation not only by examining all of the institutions focused on by those earlier studies, but also by looking at many OECD countries over a long time period.

Most of the literature has investigated cross-country differences using cross-sectional data (for example, Blau and Kahn 1996, 2005). We focus instead on cross-country differences in the *evolution* of wage inequality over time. The only previous longitudinal study of wage inequality and institutions is Wallerstein (1999), examining 16 developed countries in the years 1980, 1986, and 1992. Our analysis builds on Wallerstein's work, but the sample we use, consisting of an unbalanced panel of 11 countries, not only is more than four times larger than that in the earlier study, but also covers a period of time twice as long, with a maximum of 26 years. Our analysis includes institutions Wallerstein's did not, such as employment protection regulation, the "tax wedge" (the sum of the employment tax rate, direct tax rate, and indirect tax rate; see the Appendix), and unemployment benefit generosity and duration. We also include additional controls for other factors that might affect the evolution of wage inequality—R&D intensity, for example, which approximates technology change, and a measure for the age composition of the labor force.

How Do Institutions Affect Wage Inequality?

In our empirical analysis we focus on labor market institutions like the strictness of employment protection regulation, the tax wedge, unemployment benefit generosity and duration, union density, union coordination, and minimum wages. One simple framework for evaluating how all these institutions affect wage differentials is a model in which unions bargain with employers over the wage. In such a model, all the institutions listed above change the outside option—the best fall-back position in the event that bargaining breaks down—of employers or unions (see Koeniger et al. 2004). If labor market institutions improve the outside option more for unskilled workers than for skilled workers, this will strengthen their bargaining position and tend to compress the skill wage differential. Such a differential impact seems likely for at least two important institutions. In most OECD countries, unemployment benefit replacement rates are progressive due to benefit floors and ceilings that imply relatively larger rates for unskilled workers. Furthermore, employment protection involves a substantial fixed administrative burden, which makes it more costly for unskilled workers than for skilled workers (see Boeri et al. 2006).

In reality, institutions are quite complex and affect wage differentials in various other ways. For example, union coordination or centralization might compress wage differentials if the union agreement's influence extends widely throughout the economy and if the agreement allows unions to better insure its members (Wallerstein 1990), or if centralized unions mitigate the hold-up problem in the context of aggregate shocks (Teulings and

Hartog 1998). Furthermore, employment protection affects labor shares and wages over the business cycle, as it renders labor demand dynamic (Bertola 1999). Finally, if labor supply is elastic and the elasticity differs across demographic groups (Bertola et al., forthcoming), wage-compressing unions price young workers, old workers, and female workers out of the labor market because these groups are less strongly attached to the labor force than are others. We control for changes in the relative supply of skills in the econometric specification, which is predicated on a simple model of relative labor demand (see Koeniger et al. 2004).

Econometric Specification

The principal purpose of the analysis is to measure the effect of institutions on wage differentials after controlling for other exogenous factors that shift the relative supply of and demand for skills. In order to control for changes in demand conditions, we use measures of technology and trade shocks. As further controls for supply and demand conditions, we use the relative skill endowment log(Skill), the aggregate unemployment rate log(Unempl.), and the interaction of the two.

Trade and technology affect the wage differential through relative prices of skill-intensive and low–skill-intensive goods and through relative factor productivity. Following common practice in the literature, we approximate the effect of trade by the ratio of imports over value added—*import intensity*—and technology by the ratio of R&D expenditure over value added in the manufacturing sector—*R&D intensity* (see Machin and van Reenen 1998). Of course, in contestable markets imports might not change if foreign competition does, but in practice openness (and thus the exposure to competition) and trade volumes are highly correlated. Our hypothesis is that R&D increases relative productivity in skill-intensive sectors while trade intensity increases with the relative price of skill-intensive goods. In this case both variables should have a positive coefficient in our estimations.

Our specification is

$$\log\left(\frac{w_{90}}{w_{10}}\right)_{it} = \alpha + \beta'\upsilon_{it} + \gamma' z_{it} + \theta' s_{it} + d_i + \tau_t + \varepsilon_{it}, \tag{1}$$

where w_{90}/w_{10} is the differential between the 90th and 10th percentiles of the gross male wage distribution, z_{it} is a vector of labor market institution indicators, υ_{it} is a vector with controls for relative supply and demand conditions, s_{it} is a vector of controls for trade and technology shocks, d_i is a fixed country effect, τ_t is a year dummy, and ε_{it} is the stochastic error term. The institutions included in z_{it} are employment protection, the benefit replacement ratio, a measure of benefit duration, union density, coordination in wage bargaining, the tax wedge, and the minimum wage. The vector s_{it} contains R&D intensity and import intensity, and υ_{it} contains the natural logarithm of the skill endowment (log(Skill)), the unemployment rate (log(Unempl.)), and their interaction.

In order to get efficient estimates, we adopt a feasible fixed-effect GLS estimator, with a variance-covariance matrix that incorporates heteroskedasticity across countries (Nunziata 2005). Because we find some evidence of a mild autoregressive error structure (assuming an AR(1) error structure, the common first-order autocorrelation is below 0.4), we also tried estimating an alternative specification allowing for serial correlation of the errors within countries. Since the estimated coefficients turned out to be almost identical, and given the limited time series dimension in our sample, the estimation results presented throughout the paper do not correct for serial autocorrelation of the errors within countries.

Data

Table 1 contains summary statistics for the variables used in the estimation. The unbalanced panel for the period 1973–98 includes the following eleven countries: Australia, Canada, Finland, France, Germany, Italy, Japan, the Netherlands, Sweden, the United Kingdom, and the United States. We now discuss the data in more detail.

Because data limitations rule out using wage differentials by skill as the dependent variable—the available data cover too short

Table 1. Summary Statistics.

Variable	*No. Obs.*	*Mean*	*Std. Dev.*	*Min.*	*Max.*
Wage Differential: w_{90}/w_{10}	175	2.988	0.672	2.020	4.752
Unemployment Rate	175	6.647	3.293	1.300	16.800
Skill Ratio	175	0.304	0.210	0.059	1.126
Unemp. Rate * Skill Ratio	175	2.110	1.880	0.146	9.672
Employment Protection Indicator	175	0.963	0.611	0.100	2.000
Benefit Replacement Ratio	175	0.414	0.196	0.010	0.821
Benefit Duration	175	0.349	0.302	0.000	1.023
Tax Wedge	175	0.518	0.144	0.243	0.831
Union Coordination Indicator	175	1.922	0.698	1.000	3.000
Net Union Density	175	0.397	0.224	0.099	0.886
Minimum Wage Indicator	175	0.221	0.237	0.000	0.646
R&D Intensity	175	0.061	0.030	0.010	0.133
Import Intensity	175	0.071	0.039	0.012	0.217

Notes: The unbalanced panel of countries for the period 1973–98 includes the following eleven countries: Australia, Canada, Finland, France, Germany, Italy, Japan, the Netherlands, Sweden, the United Kingdom, and the United States. For variable definitions and data sources, see the Data Appendix.

a time period for too few countries, resulting in a very small sample—we instead adopt as our main dependent variable the ratio of the 90th to the 10th wage percentiles, w_{90}/w_{10}, for male workers. We use gross wages for all wage and salary workers in the public and private sector, as provided by the OECD (see the Data Appendix for the data sources). We focus on men because data are available for a longer time period for men than for women and the male wage is less affected by changes in labor force participation. Although the measure w_{90}/w_{10} is highly correlated with the wage differential by skill, we acknowledge that it might capture some within-group wage inequality. Moreover, w_{90}/w_{10} is an aggregate measure and thus captures the effect of union bargaining in the unionized sector as well as spillovers in the non-union sector. However, the estimation results below suggest that the effect in the unionized sector dominates the possible spillovers in the non-union sector.

To control for aggregate labor supply and demand conditions, we use OECD data on unemployment rates and the relative skill endowment measured as educational attainment. We define skilled workers as those with at least some college education. In some regressions we add controls for work force composition: the share of women in the total labor force, the share of workers above age 24 in total employment, and, as a proxy for the share of workers in public employment, the ratio of public expenditure to GDP.

Our dataset contains measures of wage bargaining institutions, generosity and duration of unemployment benefits, strictness of employment protection legislation, tax wedge, and minimum wages. We argue that these institutions tend to compress wages. In a model of union bargaining this would happen because their relative effect on workers' outside option is larger for unskilled workers than for other workers. This hypothesis is quite plausible for the minimum wage, employment protection legislation (EPL), and the unemployment benefit replacement rate and duration.

A binding minimum wage clearly increases the relative wages of the unskilled. Concerning EPL, Boeri et al. (2006) showed that EPL strictness affects unemployment inflows of high-skilled workers less than those of other workers and suggested that EPL protects unskilled workers more than skilled workers because of a substantial fixed-cost component. Further evidence shows that judges tend to give more protection to unskilled workers who have relatively low re-employment probabilities than to other groups (Ichino et al. 2003). Finally, unemployment benefit replacement rates are decreasing with earnings as a result of benefit floors and ceilings: in OECD data the unemployment benefit replacement rate of a production worker earning two-thirds of the average wage is at least as high as the replacement rate of the average worker. Below, we further discuss how this alternative measure of the replacement rate affects wage inequality. Similarly detailed data for the other institutional measures like union density and the tax wedge are unfortunately not available. If we accept the hypothesis that the effects of institutions on workers' outside option are greatest for the unskilled, however, we should expect a negative effect of our aggregate institutional measures on wage differentials.

Detailed information on the sources of the institutional data is contained in the Data Appendix. We have two measures of wage bargaining institutions: the union membership rate among active workers, or union density, and the index of coordination (a measure of the degree to which different unions and the employer side in a country coordinate in a given bargaining round). An alternative measure of union bargaining power is union coverage, that is, the proportion of contracts covered by collective agreements. This variable has the advantage of giving more weight to unions in countries—France, for example—where union density is quite low but unions' bargaining power is high. Consistent series on union coverage for all countries are not available, however, apart from a few observations every ten years (see Nickell et al. 2005). While union coverage is an omitted variable here, it is measured less frequently than and exhibits less variability than union density. As coverage is relatively stable over time, differences in union coverage are controlled for by country fixed effects (the same holds for all other *unobservable* characteristics of countries that are constant over time). Moreover, coverage is known to be correlated with coordination (Bertola, Blau, and Kahn, forthcoming), which we do measure on a time-varying basis. We control for this other source of union heterogeneity using an index of coordination in wage bargaining. This measure captures the extent to which unions moderate wage demands as they recognize the macroeconomic consequences of their decisions on employment.

Concerning unemployment benefits, we have data on benefit replacement rates and benefit duration. The benefit replacement rate is the unemployment benefit as a proportion of pre-tax earnings, averaged over family types of recipients. The variable *Benefit Duration* measures the duration of the entitlement to unemployment benefits in each country. The data on employment protection legislation summarize the set of rules and procedures governing dismissals of employed workers. The tax wedge is defined as the sum of the employment tax rate, the direct tax rate, and the indirect tax rate (see the Data Appendix for further explanation). Finally, the measure of minimum wages is defined as the ratio of the official minimum wage to the median wage. Not all countries in our sample have an official minimum wage, and our fixed-effects estimates will depend on the six countries in which the minimum wage changes over time in our sample period: Australia, Canada, the United States, the Netherlands, France, and Japan.

Estimation Results

Our estimation results, presented in Tables 2 and 3, show that institutions are strongly associated with wage inequality. Table 2 presents the results of the baseline model, which is augmented with the interactions between institutions in Table 3. Finally, in Tables 4, 5, and 6 we present some simulations that illustrate quantitatively how changes in institutions are related to wage differentials.

In Table 2 the coefficients on employment protection, the benefit replacement rate and duration, union density, and the minimum wage are found to be highly statistically significant across alternative specifications. Consistent with the explanation given above, the negative signs of the coefficients suggest that these institutions improve the outside option and the bargaining position relatively more for unskilled workers than for other workers and thus compress the skill wage differential. Columns (1) and (2) contain estimation results for the 90-10 male wage differential, whereas columns (3) and (4) report the results of our preferred specification for the 90-50 and 50-10 male wage differentials, respectively. The standard errors

Table 2. Labor Market Institutions and Male Wage Inequality. (Log of Percentile Ratio)

Variable	*(1)* $log(w_{90}w_{10})$	*(2)* $log(w_{90}w_{10})$	*(3)* $log(w_{90}w_{50})$	*(4)* $log(w_{90}w_{50})$
Employment Protection	–0.299***	–0.261***	–0.174***	–0.130***
	(7.27)	(5.90)	(7.39)	(6.09)
Benefit Replacement	–0.189***	–0.229***	–0.115***	–0.073***
	(2.99)	(4.47)	(3.45)	(2.05)
Benefit Duration	–0.163**	–0.266***	–0.096**	–0.068
	(2.22)	(4.45)	(2.59)	(1.56)
Tax Wedge	–0.046	–0.252***	0.000	–0.039
	(0.49)	(3.20)	(0.01)	(0.68)
Union Coordination	–0.002	0.093***	0.028*	–0.030*
	(0.06)	(3.19)	(1.66)	(1.73)
Union Density	–0.429***	–0.584***	–0.303***	–0.140**
	(3.92)	(6.59)	(5.24)	(2.17)
Minimum Wage	–0.268***	–0.161***	–0.145***	–0.121***
	(5.24)	(3.56)	(6.22)	(3.56)
Log(Unempl.)	–0.012	–0.008	–0.060***	0.040***
	(0.46)	(0.29)	(4.65)	(2.60)
Log(Skill)	0.176**	0.395***	0.062*	0.109***
	(2.51)	(5.68)	(1.63)	(2.76)
Log(Unempl.) * Log(Skill)	0.014	0.029	–0.036***	0.043***
	(0.99)	(1.50)	(4.96)	(5.00)
R&D Intensity	–1.025***	0.469	–0.530***	–0.432*
	(2.62)	(1.28)	(2.60)	(1.90)
Import Intensity	2.048***	0.372	1.081***	0.927***
	(3.84)	(0.82)	(3.99)	(3.01)
Gov. Exp./GDP	—	–0.700**	—	—
		(2.41)		
Female Labor Supply	—	–1.127***	—	—
		(2.79)		
Employment Share > 24	—	1.533***	—	—
		(6.98)		
Observations	175	160	175	175
Countries	11	11	11	11
RMSE	0.0380	0.0329	0.0199	0.0237
R^2	0.9702	0.9799	0.9601	0.9756

Notes: All estimations include dummies for countries and years and correct for country-level heteroskedasticity. Absolute values of z-statistics are in parentheses. Employment protection and union coordination are indices (ranges 0–2, 1–3, respectively). See the notes to Table 1 and the Data Appendix for further details.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

used for the z-statistics reported in brackets below the coefficient estimates allow for heteroskedasticity across countries. At the bottom of the table we report two measures of fit: the root mean-squared error (RMSE) of the model allowing for heteroskedasticity, and the R^2 statistic of the corresponding OLS fixed-effect model. Both statistics indicate a high fit for the model specification.

Baseline Results

Our preferred specification for the 90-10 male wage differential in column (1) includes time and country dummies as well as measures of trade and technology, indicators of aggregate supply and demand conditions, and institutional indicators. In column (1) the regressors on institutions and trade are highly statistically significant. In particular, the 90-10 male wage differential is more compressed in the presence of stricter employment protection legislation, higher unemployment benefits, higher union density, and higher minimum wages. The index of coordination and the tax wedge are also negatively associated with the wage differential but are not statistically significant. Moreover, the male wage differential is positively associated with import intensity[1] but negatively associated with R&D intensity. This suggests that R&D expenditure is not a good proxy for the stock of technology, being both an input and a flow variable. The effect of the stock of technology on the wage differential is likely to be captured at least partly by the country and time dummies in our regression. R&D should be much more useful for explaining changes in the wage differential than the level of that differential. Indeed, if we estimate a specification with three-year changes of the wage differential, the coefficient on R&D intensity is positive and statistically significant.[2]

Finally, the skill endowment of the labor force is positively associated with higher wage inequality. Recall that the skill endowment does not necessarily correspond to the relative employment of skilled workers. For this reason, to proxy the relative skill supply we also include the total unemployment rate and its interaction with the skill endowment; neither, however, is statistically significant. In general, the sign and significance of the coefficients on skill endowment and unemployment rate are not robust across all specifications. Unfortunately, we do not have better measures for aggregate supply and demand conditions for all countries in our sample period.

For further insight on our hypothesis that institutions are more important for strengthening the bargaining position of *unskilled* workers than that of skilled workers, we use the OECD benefit replacement rate of a production worker earning two-thirds of the average wage instead of the average replacement rate in column (1). The results, which are not reported, show that the coefficient is negative, statistically significant, and twice as large in absolute size as the coefficient on the average replacement rate. This suggests that unemployment benefit replacement rates are more generous for unskilled workers than for other workers and thus compress the wage differential.

In column (2) we augment the model with controls for work force composition effects: the share of the women in the total labor force, the ratio of government expenditure to GDP, and the age composition of employment measured by the share of workers above the age of 24 in total employment. The share of women in the total labor force is relevant for male wage inequality if women are substitutes for low-skilled men, as claimed by Topel (1994). We use the ratio of government expenditure to GDP as a proxy for the share of public employment. The empirical evidence shows that wages are more compressed in the public sector than in the private sector, possibly reflecting the fact that unions are more powerful in the public sector (Checchi and Lucifora 2002). Therefore we expect the ratio of government expenditure to GDP to be negatively associated with wage inequality. Finally, the share of workers above age 24 in total employment controls for the possible effects of age-varying wage profiles on wage inequality.

The presence of work force controls in column (2) does not affect the results on institutions except for the coefficients on union coordination and the tax wedge, which now become statistically significant: a decrease in taxes and an increase in union coordination are associated with increased wage inequality. The coefficients on the work force controls are all statistically significant. The ratio of government expenditure to GDP enters with the expected

[1]According to the literature, trade with non-OECD countries is particularly relevant for wage inequality because it decreases the price of low–skill-intensive goods and thus the price for unskilled labor. We investigate this hypothesis using the import intensity of trade with non-OECD countries, although the data are only available from the 1980s onward. Consistent with the hypothesis, we find that the positive coefficient on non-OECD import intensity is significantly larger (at the 10% level) than the coefficient on total import intensity.

[2]Results are available on request to the authors.

negative sign. The coefficient on the share of workers over age 24 is positive, suggesting that a higher proportion of workers at the top of their experience-wage profile is reflected in higher aggregate wage inequality. Finally, the share of women in the labor force is negatively associated with male wage inequality. This is in contrast with results reported by Topel (1994), who found a positive relationship between female labor force participation and male wage inequality in the United States. Controlling for age and skill groups, he argued that the big increase in participation of skilled women increased male wage inequality because women are substitutes for low-skilled workers. Our results suggest that this substitution effect may not be robust for other countries. The negative coefficient on the share of women in the total labor force could also be explained if in some countries, like the Scandinavian countries in our sample, government intervention decreases male wage inequality while creating conditions favorable for female participation in the labor market.

Controlling for work force composition, we have slightly fewer observations (160 instead of 175) due to the lack of data on age composition of employment for the United Kingdom at the beginning of the sample period. In what follows we prefer to keep all available observations in the sample, presenting the results without controlling for work force composition. However, we consistently check to ensure that our results are robust with respect to the inclusion of these controls.

To measure the explanatory power of our institutional indicators, we compare the results shown in column (1) with the results of a regression that only includes time and country dummies. The additional regressors in column (1) substantially improve the fit.[3] The RMSE changes from 0.084 to 0.038 and the R^2 from 0.935 to 0.970. In a regression with only measures of trade, technology, and relative unemployment, without measures for institutions, the RMSE changes to 0.077 and the R^2 to 0.950. These numbers imply that the institutional measures in column (1) substantially reduce the RMSE (from 0.077 to 0.038) and increase the R^2 (from 0.95 to 0.97). Therefore the increment in the variation explained by institutions exceeds the amount explained by our trade and technology measures.

The results for the 90-50 and 50-10 male wage differentials reported in columns (3) and (4) help us to disaggregate the effect of institutions on the entire wage distribution. It turns out that the coefficients on employment protection, replacement rates, and minimum wages are quantitatively similar for the upper and lower part of the wage distribution. This finding is puzzling for the minimum wage. Interestingly, the same pattern is reported in the U.S. literature on the effect of the minimum wage on wage inequality across U.S. states (Autor et al. 2005). The results also show that union density is more important for the upper part of the distribution (90-50) than for the lower part, suggesting that more powerful unions tend to transfer rents from very skilled to less skilled workers. Finally, the results show that union coordination increases the 90-10 wage differential if we control for changes in work force composition. The results in columns (3) and (4) suggest that this is a combination of a negative association with the 50-10 wage differential and a positive association with the 90-50 wage differential. According to the literature, more coordinated unions take into account the adverse employment consequences of higher wages and thus are less aggressive in wage bargaining. Since labor demand is more elastic for low-skill low-income workers than for other workers, one would expect more union coordination to eventuate in more wage moderation at the *bottom* of the distribution. Our results show instead that union coordination matters more for median-income workers than for low-income workers and thus lowers the median wage relative to the wage at the tenth percentile

[3]Note that our explanatory variables also capture much of the *total* variation in the data. We find that a regression with only the explanatory variables (not reported) provides a better fit than a regression with country and year dummies alone.

of the distribution. One explanation for this finding is that institutional factors that are not a direct part of the wage negotiations between unions and employers, like generous unemployment benefits, introduce a wage floor that constrains the bargained wages for unskilled workers.

Institution Interactions

The models in Table 3 include a set of interactions between labor market institutions. They account both for some complementarity in institutions and possible heterogeneity in the institutional coefficients.[4] For example, the effect of the tax wedge on real wages depends on unions' strength (union density) and coordination. If unions are strong and decentralized, they will pass on relatively more of the gross labor cost to employers, with adverse employment consequences. Coordinated unions take these consequences into account and thus moderate their wage demands (see Daveri and Tabellini 2000; Alesina and Perotti 1997). Another interaction arises between employment protection and minimum wages. Employment protection has more bite if wages are downwardly rigid, since firms cannot reduce wages to pass on to workers the expected cost of firing regulations (see Lazear 1990; Bertola and Rogerson 1997). Finally, the generosity of unemployment benefits matters more the longer such benefits are provided (see, for example, Nickell et al. 2005). We expect the latter two policy interactions to compress the wage differential, since they are likely to affect unskilled workers more strongly than other workers. The effect of the interaction between union density and coordination is less straightforward, since union coordination is relevant for wage bargaining across the wage distribution in many countries.

The variables on institutions enter in each interaction as deviations from the sample average. In this way the coefficient on each institution in levels can be read as the coefficient on the *average* country, that is, the country characterized by the mean level of that specific institutional indicator. For this average country, the interaction terms are zero. We experimented with various interactions, but only three interactions—between (a) the two union bargaining variables, (b) employment protection and minimum wages, and (c) the two unemployment benefit variables—turned out to be statistically significant. These three interactions are statistically significant when introduced separately, as can be seen in Table 3, columns (1)–(3). The coefficients of the variable interactions *employment protection * minimum wage* and *benefit replacement rate * benefit duration* both have a negative sign, again suggesting a larger impact of these institutions on unskilled workers than on other workers. The interaction between union coordination and union density has a positive sign. Given the negative and statistically significant coefficient on union density, this indicates that more powerful unions compress wages less if they are more coordinated. As explained above, more coordinated unions moderate their wage demands as they take the adverse employment effects into account. When we include all three interactions at the same time (column 4), only two interactions—*union density * union coordination* and *benefit replacement rate * benefit duration*—remain statistically significant. This is also true for the models using the 90-50 and 50-10 wage differentials in columns (5) and (6). The coefficients of all other variables are robust with respect to the introduction of the interactions.

Robustness

We first check the robustness of the results by dropping one country at a time. We find that in only one case do the estimated coefficients change substantially: excluding Finland reduces the importance of union density. One difference between Finland and most other countries in the sample is that union density has increased in Finland since the 1970s. Our results are also robust with respect to the exclusion of R&D intensity or import intensity. Moreover, our results are qualitatively robust with respect to the

[4]These specifications are in the spirit of Belot and van Ours (2001), who analyzed the effect of institution interactions on unemployment.

Table 3. Baseline Models with Interactions.
(Log of Percentile Ratio)

Variable	*(1)* $log(w_{9010})$	*(2)* $log(w_{9010})$	*(3)* $log(w_{9010})$	*(4)* $log(w_{9010})$	*(5)* $log(w_{9050})$	*(6)* $log(w_{5010})$
Employment Protection	–0.420***	–0.329***	–0.257***	–0.299***	–0.168***	–0.145***
	(9.20)	(8.84)	(7.11)	(7.72)	(8.01)	(5.88)
Benefit Replacement	–0.210***	–0.288***	–0.311***	–0.426***	–0.201***	–0.226***
	(3.41)	(5.26)	(5.07)	(7.98)	(7.13)	(6.71)
Benefit Duration	–0.112	–0.131*	–0.252***	–0.182***	–0.072**	–0.113***
	(1.62)	(1.93)	(3.70)	(3.01)	(2.32)	(3.02)
Tax Wedge	0.067	–0.033	0.089	0.094	0.008	0.088**
	(0.67)	(0.41)	(1.00)	(1.31)	(0.21)	(1.78)
Union Coordination	0.004	0.024	–0.011	0.025	0.058***	–0.023**
	(0.12)	(0.85)	(0.40)	(1.06)	(4.07)	(1.74)
Union Density	–0.499***	–0.312***	–0.421***	–0.260***	–0.208***	–0.091**
	(4.64)	(3.17)	(4.07)	(2.84)	(4.22)	(1.66)
Minimum Wage	–0.633***	–0.205***	–0.246***	–0.180*	–0.097**	–0.098
	(6.47)	(3.91)	(4.96)	(1.89)	(2.17)	(1.53)
Empl. Prot. ∗ Min. Wage	–0.679***	—	—	–0.009	0.009	–0.058
	(4.48)			(0.07)	(0.13)	(0.64)
Union Dens. ∗ Coord.	—	0.673***	—	0.669***	0.419***	0.255***
		(7.62)		(7.70)	(9.16)	(4.74)
Ben. Repl. ∗ Duration	—	—	–0.934***	–0.844***	–0.224***	–0.580***
			(7.16)	(6.84)	(3.45)	(7.18)
Log(Unempl.)	–0.008	–0.034	0.026	–0.006	–0.058***	0.049***
	(0.31)	(1.54)	(1.04)	(0.28)	(5.18)	(3.69)
Log(Skill)	0.095	0.273***	–0.005	0.125**	0.091***	0.037
	(1.36)	(4.62)	(0.08)	(2.12)	(2.89)	(1.06)
Log(Unempl.) ∗ Log(Skill)	0.014	–0.021	0.036**	–0.000	–0.045***	0.041***
	(0.98)	(1.57)	(2.52)	(0.01)	(7.05)	(5.11)
R&D Intensity	–0.368	–0.233	0.072	0.774**	0.159	0.637***
	(0.93)	(0.69)	(0.18)	(2.34)	(0.91)	(3.07)
Import Intensity	2.297***	1.390***	2.062***	1.402***	0.733***	0.767***
	(4.24)	(2.79)	(3.99)	(2.97)	(3.22)	(2.69)
Observations	175	175	175	175	175	175
Countries	11	11	11	11	11	11
RMSE	0.0364	0.0361	0.0347	0.0320	0.0176	0.0206
R^2	0.9727	0.9738	0.9753	0.9792	0.9697	0.9818

Notes: All estimations include dummies for countries and years and correct for country-level heteroskedasticity. Absolute values of z-statistics are in parentheses. When interactions are included, the variables are set as deviations from the mean, so the interactions take the value zero at the sample mean. See notes to Table 1 and the Data Appendix for further details.

exclusion of the minimum wage, whose coefficient is identified by time variation within only six countries.

Second, we investigate whether the impact of import intensity depends on the institutional context of each country. When we introduce a set of interactions between import intensity and each institutional regressor, we find that the effect of import intensity is lower if the unemployment benefit replacement ratio is more generous. Intuitively appealing though this result may be, however, it is not robust across alternative specifications.

Finally, one possible concern is the endogeneity of our indicators of aggregate supply and demand conditions in the labor market. Since we have not found a convincing identification for a system estimation, we evaluate the sensitivity of our results by excluding the unemployment rate and the relative skill endowment from our specification. We find that all our results are robust with respect to this exclusion.

Quantitative Implications

At this point the reader may wonder just how large are the effects of institutions on wage inequality. To illustrate the quantitative importance of the estimates, we calculate the effects of the coefficient estimates for institutions and then perform simulations. First, we show what would happen to wage inequality if we changed from the most rigid to the most flexible regulation in each institutional dimension. Second, we calculate how wage inequality would change in each country if the U.S. institutional regime were adopted. Finally, we compute how wage inequality would have changed in each country by the end of the sample period (1998) if institutional parameters had held constant at their 1973 values. Although these simulations give an immediate sense of the magnitude of the effect of institutions, they should be interpreted with care because, as mentioned above, our institutional indicators are imperfectly measured.

Table 4 presents two sets of simulations based on the coefficient estimates in column (1), Table 2, and the model with interactions in column (4), Table 3. In panel A of Table 4 we calculate the percentage increase in the 90-10 differential associated with a one–standard-deviation reduction in rigidity for each institutional dimension (the standard deviations are shown in Table 1). A reduction of employment protection by one standard deviation turns out to be most important, being associated with an 18% higher wage differential. Reducing the generosity of the unemployment benefit system in size by one standard deviation is associated with a 4–8% higher wage differential (the exact value depending on whether the interactions of institutions are included), and a similar reduction in benefit duration increases wage inequality by 5%. Finally, the wage differential rises by 6–10% or 4–6%, respectively, with a one-standard-deviation increase in union density or the minimum wage.

Panel B of Table 4 shows the increase in the 90-10 log-wage differential associated with a change from the most rigid to the most flexible regulation in each institutional dimension. For employment protection, union density, benefit replacement rates, and the minimum wage, we also compute that effect when we consider specific values of institution interactions as in Table 3, column (4).

Starting with the coefficient estimates without interactions in column (1), Table 2, a change from the most rigid employment protection legislation (Italy) to the most flexible (the United States) is associated with an increase of 55% in the 90-10 differential. The implied increase changes little if we use the coefficients of the model with interactions (column 4, Table 3) or consider the change for countries with low or high minimum wages. The same exercise can be done for the unemployment benefit replacement rate, union density, and the minimum wage. The interactions appreciably affect the size of the association for some indicators. For example, the positive association between lower union density and the wage differential is more than offset if bargaining coordination is higher, and the influence of unemployment benefits greatly increases with benefit duration. In both cases the results are consistent with the literature on institutional interactions discussed above. Finally, changes in the tax wedge and union coordination, which are not reported, have only a small effect on the wage differential (–6% and 5%, respectively, in the model with interactions).

Table 5 shows how the 90-10 log-wage differential would change in each country if institutions were adjusted to match those in the United States. The numbers are obtained using the coefficient estimates of column (1), Table 2, and of column (4), Table 3, and the average values of all institutional variables for each country. We find sizeable positive changes in the wage differential: an increase between 16% and 66% for the baseline model and between 27% and 84% in the specification with institution interactions. For example, the simulations based on the specification with institution interactions

Table 4. Quantitative Predictions of Baseline Model I: Change of $\log(w_{90}w_{10})$.

Panel A: One Standard-Deviation Reduction in Rigidity

	Employm. Protection	*Benefit Repl. Rate*	*Benefit Duration*	*Union Density*	*Min. Wage*	*Tax Wedge*	*Union Coordin.*
Baseline	0.18	0.04	0.05	0.10	0.06	0.01	0.00
Interactions	0.18	0.08	0.05	0.06	0.04	–0.01	0.02

Panel B: Change from Most Rigid to Most Flexible Institutional Regulation

Employment Protection			
Baseline		0.55	
Interacted with:	*Min. Value of Min. Wage*	*Mean Value of Min. Wage*	*Max. Value of Min. Wage*
	0.56	0.57	0.58
Benefit Replacement Rate			
Baseline		0.11	
Interacted with:	*Min. Value of Ben. Duration*	*Mean Value of Ben. Duration*	*Max. Value of Ben. Duration*
	0.11	0.35	0.81
Union Density			
Baseline		0.27	
Interacted with:	*Min. Value of Coordination*	*Mean Value of Coordination*	*Max. Value of Coordination*
	0.69	0.20	–0.36
Minimum Wage			
Baseline		0.16	
Interacted with:	*Min. Value of EPL*	*Mean Value of EPL*	*Max. Value of EPL*
	0.11	0.12	0.12

Note: The simulations for the baseline model and the model with interactions use, respectively, the estimated coefficients in Table 2, column (1), and in Table 3, column (4).

imply the largest change for the Netherlands, where the wage differential would nearly double. This is because institutions in the Netherlands are more rigid than those in the United States on all five measures. We find smaller changes for the other countries because the institutions are not always more rigid than in the United States; France has lower union density than the United States, for example, and Germany, Italy, and Sweden have no official minimum wage. Not surprisingly, the positive changes are smallest in Anglo-Saxon countries, because their institutional environment is most similar to that of the United States.

Finally, in Table 6 we compute the predicted change in the 90-10 log wage differential associated with changes in institutions from 1973 to 1998. We predict wage inequality using the coefficient estimates of column (1), Table 2, and of column (4), Table 3. We compute the predicted change due to time-varying institutions expressed as a fraction of actual wage inequality at the end of the sample period. Had institutions not changed since the 1970s, the 90-10 wage differential in France, for example, would have been 16% higher than the actual value in the 1990s if we consider our baseline model (23% higher if we consider our model with interactions). We

Table 5. Quantitative Predictions of Baseline Model II: Change of $\log(w_{90}w_{10})$ if Institutions Change to U.S. Levels.

	Australia	*Canada*	*Finland*	*France*	*Germany*	*Italy*	*Japan*	*Netherlands*	*Sweden*	*U.K.*
Baseline	0.35	0.21	0.53	0.48	0.48	0.48	0.37	0.60	0.66	0.16
Interactions	0.34	0.30	0.61	0.66	0.72	0.68	0.64	0.84	0.65	0.27

Note: The simulations for the baseline model and the model with interactions use, respectively, the estimated coefficients in Table 2, column (1), and in Table 3, column (4).

decompose this percentage change further, holding constant one institution at a time. We find that holding employment protection constant at the 1970s level is associated with 13% higher wage inequality in France in the 1990s; holding the minimum wage down to its 1970s level increases wage inequality in the 1990s by 4%; and stabilizing union density at the 1970s level (a counterfactual to the decline that actually occurred) decreases wage inequality in the 1990s by 2%.

Similarly, had institutions remained the same as in the 1970s in Sweden, by the 1990s wage inequality would have been around 45% higher than its actual value. The lower actual inequality is mainly owing to increases in union density and in the unemployment benefit replacement ratio, which accounted, respectively, for 10% and 12% lower actual wage inequality than its counterfactual value in the simulation. In the United States and United Kingdom, instead, the decline in union density and (in the United States only) in the minimum wage are associated with higher wage inequality over time. Had all institutions stayed the same as in the 1970s, wage inequality would have been 4% lower in the 1990s in both countries. The decline in union density alone accounts for 3% higher wage inequality in the United States and for 5% in the United Kingdom, and the decline in the minimum wage accounts for 1% higher wage inequality in the United States.

These results are broadly in line with Wallerstein (1999), although we use different measures for institutions and have a longer sample period. Because we control for country and year dummies in all our regressions, the estimates from the earlier study to which ours are most comparable are those that used time variation within countries (see Table 2, column 7 and the discussion in section 5 in Wallerstein's paper). Wallerstein found that changes in wage-setting institutions between 1980 and 1992 could explain a 3% increase in the wage differential in the United States and up to a 14% increase in the United Kingdom. Our results with institution interactions for the United Kingdom suggest a similar order of magnitude.

Our simulations also imply that about a fifth of the actual percentage change in wage inequality in the United States and United Kingdom can be explained by changes in institutions. Since the actual increase in wage inequality has been large in both countries (respectively, 32% and 25% in the period 1975–99), this suggests an important influence of labor market institutions on changes in wage inequality. Wallerstein (1999) similarly found that changes in wage-setting institutions could explain 20% of the increase in wage inequality in the United States and close to 50% of the actual increase in the United Kingdom.

Conclusion

Our empirical results show that stricter employment protection legislation, more generous benefit replacement ratios, longer benefit duration, higher union density, and a higher minimum wage are associated with lower male wage inequality. We find that changes in these institutions can explain a substantial part of observed changes in male wage inequality—at least as much as is explained by our trade and technology measures. We have assessed the quantitative implications of our estimates in two simulation exercises. First, we found that if the regulatory flexibility of all institutions in the studied countries were changed to match that in the United States, wage inequality would increase between

Table 6. Quantitative Predictions of Baseline Model III:
Change of $\log(w_{90}w_{10})$ Associated with Changes in Institutions, 1973–1998.

	Australia	*Canada*	*Finland*	*France*	*Germany*	*Italy*	*Japan*	*Netherlands*	*Sweden*	*U.K.*	*U.S.*
Baseline	–0.04	–0.01	–0.17	–0.16	0.10	0.06	–0.02	0.13	–0.46	0.04	0.04
Interactions	–0.07	–0.03	–0.17	–0.23	0.06	0.13	–0.09	0.09	–0.49	0.11	0.08

Notes: The values for Australia refer to the period 1973–1985; for Germany, 1991–1998. For Italy and the Netherlands the values from 1973–85 are imputed. The simulations for the baseline model and the model with interactions use, respectively, the estimated coefficients in Table 2, column (1), and in Table 3, column (4).

15% and 30% in Anglo-Saxon countries and between 50% and 80% in continental European countries. It is not surprising that the changes in Anglo-Saxon countries would be smaller, since their institutional environment is more similar to that in the United States. Second, actual changes in institutions in the period 1973–98 are associated with a reduction of male wage inequality of 23% in France, where minimum wages increased and employment protection became stricter, but with an increase of up to 11% in the United States and United Kingdom, where unions became less powerful and (in the United States) minimum wages fell.

Further research needs to elaborate on these findings in various directions. Labor market institutions may have different effects across industries, which our aggregate perspective cannot capture. More work should also investigate the effect of specific institutions like employment protection, unemployment benefits, and the tax wedge at the firm or worker level. Finally, institutions may affect wage inequality not only directly but also by changing the incentives for capital investment (Koeniger and Leonardi, forthcoming).

In our estimations we can only provide a variance decomposition; future research should explore the causal links between institutions and the wage differential. In empirical analyses based on data from long time periods, like ours, institutions cannot be considered fully exogenous. Deunionization or minimum wages might be at least partly endogenous to changes in trade and technology (Acemoglu et al. 2001). Thus, we might very well find a weaker association between institutional changes and changes in the wage differential if we were able to control for the endogeneity of institutions.

Data Appendix

Dependent Variable

Wage inequality. Data on male wage dispersion are taken from the *Trends in earnings dispersion database* provided by the OECD. This database collects information on earnings for all employees based on national surveys or administrative data. The measures for wage dispersion are the natural logarithm of the ratio of the percentiles 90-10, 90-50, and 50-10 of gross wages.

Institution Variables

Employment protection. Blanchard and Wolfers (2000) provide a time-varying employment protection indicator for the time period 1960–95, with one observation every five years. This series is built chaining OECD data with data from Lazear (1990). Notice that the OECD data, used from 1985 onward, are constructed based on a more extensive collection of employment protection dimensions than Lazear used. Our data set includes an interpolation of the Blanchard and Wolfers series, readjusted in the mean with a range 0-2 increasing with strictness of employment protection.

Net union density. For non-European countries this variable is constructed as the ratio of total reported union members (gross minus retired and unemployed members), as reported in Visser (1996), to the number of wage and salaried employees, reported in Huber et al. (1997). The data are updated using data from the Bureau of Labor Statistics (United States: 1994 and 1995), the ILO (1997) (Australia: 1995; New Zealand: 1994 and 1995; Canada: 1994 and 1995), and the Japan Ministry of Health, Labor, and Welfare's "Basic Survey on Labor Unions" (Japan: 1995). The data for European countries except Sweden are reported in Ebbinghaus and Visser (2000) using the same criteria. Concerning Sweden, Ebbinghaus and Visser provide data on the gross density only. Therefore we use the same sources that we use for non-European countries, updating the series using the growth rate of gross density in 1995.

Bargaining coordination. This is an index with a range 1-3 constructed by interpolating OECD data on bargaining coordination. It is increasing in the degree of coordination in the bargaining process on the employers' as well as on the unions' side. The resulting series were matched with the data reported in Belot and van Ours (2004).

Benefit replacement ratio. The OECD-provided data for this variable include one observation every two years for each country in the sample. The data refer to the first year of unemployment benefits, averaged over family types of recipients, since in many countries benefits depend on family composition. The benefits are measured as a proportion of average pretax earnings.

Benefit duration. We constructed this index as a weighted average $BD = \alpha BRR_2/BRR_1 + (1 - \alpha)BRR_4/BRR_1$, where BRR_1 is the unemployment benefit replacement rate received during the first year of unemployment, BRR_2 is the replacement rate received during the second and third years of unemployment, and BRR_4 is the replacement rate received during the fourth and fifth years of unemployment. Note that we give more weight to the first ratio than to the second ($\alpha = 0.6$).

Tax wedge. The tax wedge is equal to the sum of the employment tax rate, the direct tax rate, and the indirect tax rate: $TW = t_1 + t_2 + t_3$. The employment tax rate t_1 is calculated as $t_1 = EC/(IE - EC)$, where EC denotes the employers' total contributions and IE denotes wages, salaries, and social security contributions. The direct tax rate is defined as $t_2 = DT/HCR$, where DT is the amount of direct taxes and HCR is the amount of households' current receipts. The indirect tax rate is defined as $t_3 = (TX - SB)/CC$, where TX are total indirect taxes, SB are subsidies, and CC are private final expenditures. All data come from the London School of Economics CEP–OECD data base, updated using the same criteria.

Minimum wage. This is the ratio of the statutory minimum wage to the median wage in each country. It is provided by the OECD.

Other Control Variables

Supply and demand conditions. We use the national aggregate series on **unemployment rates** provided by the OECD to construct log(Unempl.), and the national series of educational attainment provided by Angel de la Fuente and Rafael Domenech at http://iei.uv.es/~domenech. The **relative skill endowment** is the ratio of the population with some college to the rest of the population, which we use to calculate log(Skill).

Import intensity and **R&D intensity.** The OECD STAN database provides information on imports, R&D, and value added in the manufacturing sector from 1973 to 2000. With these data we can build our proxies for trade and technology using information on total manufacturing for imports, R&D, and value added for all countries.

Government expenditure per GDP (Gov. Exp./GDP). These data are obtained from the OECD National Accounts.

Employment share above age 24 (Empl. Share > 24). We use the share of employees above age 24 in total employment. These data are provided by the OECD Labor Force Statistics.

Female labor supply (Fem. Lab. Supply). We use the share of women in the total labor force. These data are taken from the OECD Labor Force Statistics.

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