DO AFFLUENT COUNTRIES FACE AN INCOMES-JOBS TRADE-OFF?

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According to an influential view, in the United States pay for less skilled workers is low and government benefits are stingy, but this facilitates the creation of new jobs and encourages such individuals to take those jobs. In much of Western Europe, relative pay levels are higher for those at the bottom and benefits are more generous, but this is said to discourage job creation and job seeking. This article offers a comparative assessment of this trade-off view based on pooled time-series cross-section analyses of 14 countries in the 1980s and 1990s. The findings suggest that greater pay equality and a higher replacement rate do reduce employment growth in low-productivity, private-sector service industries and in the economy as a whole. However, these effects are relatively weak. The results point to a variety of viable options for countries wishing to maintain or move toward a desirable combination of jobs and equality.

Keywords: *inequality; incomes; employment; comparative*

Between 1979 and 2000, the share of the working-age population that is employed increased from 68% to 75% in the United States but only from 67% to 69% in Western Europe. The average unemployment rate in the United States dropped from 6% in 1979 to 4% in 2000, whereas in Western Europe it jumped from 4.5% to 6%. However, incomes for those at the bottom, relative to the median, are lower in the United States. As of the late 1990s, on average, a Western European at the 10th percentile of the earnings distribution earned two thirds (68%) as much as the median worker, whereas his or her American counterpart earned slightly less than half (48%) of the median. And, a worker in Europe who becomes unemployed is likely to

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receive more generous unemployment compensation and related benefits. In the late 1990s, such benefits, often referred to as the replacement rate, averaged about one half (54%) of former earnings in Western Europe compared to one quarter (27%) in the United States.[1]

To a number of observers, these differences suggest a trade-off between incomes and jobs. In the U.S. model, wages and unemployment benefits for those at or near the bottom of the labor market are relatively low. This makes companies eager to hire such workers and makes workers willing to accept such jobs. The U.S. labor market is thus characterized by extensive job creation and high employment but also by low incomes for those at the bottom. In the European model, higher relative wages at the low end of the distribution and a more generous replacement rate encourage companies to employ fewer workers and lead job seekers to increase their reservation wage (the wage at which they are willing to accept a job). Countries in Europe therefore feature relatively high incomes for those at the bottom—via earnings or benefits—but little job creation and high unemployment. In this view, affluent countries can choose good incomes or healthy employment performance, but they cannot have both. This argument has been articulated most prominently in several reports by the Organization for Economic Cooperation and Development (OECD, 1994, 1996). Although by no means uncontested, it appears to hold considerable sway in academia (Becker, 1996; Bertola & Ichino, 1995; Blanchard & Wolfers, 2000; Krugman, 1996; Siebert, 1997; Wilson, 1996, p. 153) and in the popular media ("Europe Hits a Brick Wall," 1997; Samuelson, 1996; Wessel & Benjamin, 1994).

This article explores the merits of the trade-off view via an analysis of the effects of pay equality and unemployment benefits on employment performance in 14 OECD countries in the 1980s and 1990s. The first section describes existing evidence and research on this issue. The second outlines the methods and data I use in my analysis. The third section and discusses the findings, and the fourth considers their implications for the viability of a high-equality, high-employment society.

EXISTING EVIDENCE

A number of analysts have highlighted evidence that is inconsistent with the trade-off view. First, cross-country bivariate analyses suggest no associa-

1. Western Europe here refers to Austria, Belgium, Denmark, Finland, France, Germany, Ireland, Italy, the Netherlands, Norway, Sweden, Switzerland, and the United Kingdom. The replacement rates are for a worker at the 33rd earnings percentile. See the appendix for definitions and data sources.

tion between pay equality and low employment rates or high unemployment rates (Bazen, 2000; Galbraith, Conceição, & Ferreira, 1999; Howell, 2002). Yet bivariate relationships can be misleading; it is possible that a trade-off between incomes and jobs exists but is only evident when other factors are held constant. Also, analyses of changes in employment or unemployment may yield more support for the trade-off notion than analyses that focus on levels. Second, although European wage structures tend to be more rigid than those in the United States and tend to raise the wages of those on the bottom, the unemployment and employment rates of low-skilled workers relative to high-skilled workers are no better in the United States than in most European nations (Glyn & Salverda, 2000, p. 47; Howell, 2002; Mishel, Bernstein, & Schmitt, 2001, pp. 402-05; Nickell & Bell, 1996; Salverda, Bazen, & Gregory, 2001). Yet the relevant comparison might instead be between younger and older workers, and European countries do tend to have higher relative unemployment rates among the youth than does the United States (Blau & Kahn, 2002a, pp. 28-38). Third, cross-state differences in minimum wage levels in the United States have been found to have no adverse effects on minimum-wage employment (Card & Krueger, 1995). However, this finding has been questioned (Keil, Robertson, & Symons, 2001; Neumark & Wascher, 2000).

What evidence is there to support the trade-off view? Proponents frequently point to the broad comparison between the United States and Western Europe described above. Yet this overlooks a variety of other possible causes of labor market outcomes, and it ignores the considerable diversity in employment and unemployment rates across European countries (Esping-Andersen & Regini, 2000).

Recently, Torben Iversen and Anne Wren (1998) have sharpened the theoretical argument for a trade-off and have provided cross-country multivariate evidence to support it. Iversen and Wren suggest that pay equality is most likely to reduce growth of employment in private-sector consumer-oriented services—particularly wholesale and retail trade, restaurants and hotels, and community/social/personal services. Because productivity in these industries is low and difficult to increase, "the most important source of market-generated expansion of employment in services, apart from the effects of changing consumption patterns, becomes lower wages, which translate into cheaper prices and higher effective demand" (Iversen & Wren, 1998, p. 512). These industries, according to Iversen and Wren, have been the main locus of job growth in affluent countries in the past several decades. Hence, pay equality is likely to have had an adverse impact on overall employment growth.

The scatterplots in Figures 1a, 1b, and 1c illustrate the plausibility of this argument. Pay equality is measured here as the ratio of earnings of a worker at the 10th percentile (P10) of the earnings distribution to those of a worker at the 50th percentile (P50). The employment rate is the share of the working-age population that is employed, either in private consumer services or in total, depending on the chart. The incomes-jobs trade-off is commonly presumed to apply to the period of the late 1970s when demand for less skilled employees began to decrease as the result of globalization, technological change, and/or other factors. In earlier decades, pay equality was not an impediment to job creation (see Freeman, 1995, p. 64; Howell, 2002, p. 15; Siebert, 1997). The vertical axis in each figure shows the change in employment since the late 1970s, measured as the employment rate for the most recent year for which data are available minus the rate in 1979.

Figure 1a suggests a strong negative impact of pay equality on employment growth in private consumer-related services. (Unfortunately, data on private-sector employment in these industries are not available for Austria, Ireland, New Zealand, and Switzerland, and they are not available after 1995 for any of the countries.) The only exception to this pattern is Canada. However, the pay equality data for Canada are very likely wrong; Canada's P10/P50 ratio probably lies somewhere between those of the United States and the United Kingdom, in which case it would fall fairly closely in line with the other nations.[2] Iversen and Wren did not empirically investigate the second element of their hypothesized causal chain, nor the overall relationship between pay equality and total employment. But the patterns shown in Figures 1b and 1c are consistent with their argument. Figure 1b shows that countries with rapid job growth in private consumer services have tended to enjoy faster growth of total employment, and Figure 1c suggests an adverse impact of pay equality on total employment growth.

Slow growth in private service jobs can be offset by the creation of public-sector service jobs. In the 1960s and 1970s, the Scandinavian countries used sizeable expansions of public-sector employment to help generate the highest aggregate employment rates among all affluent nations. Yet according to Iversen (1999, chap. 6), this strategy may have reached its limit. It depends on relatively high tax rates, which have come under increasing strain because of

2. The OECD data suggest that Canada has the lowest earnings ratio among the countries analyzed here—lower even than the United States (see Figure 1a or 1c). However, data from the Luxembourg Income Study (LIS, n.d.), which may be preferable to those of the OECD in terms of their cross-country comparability, suggest that Canada's earnings ratio is actually slightly higher than that of the United States (my calculations; see also Gottschalk & Smeeding, 1997, p. 643). The LIS data are not used in the analyses here because they are available for far fewer years per country than the OECD data.

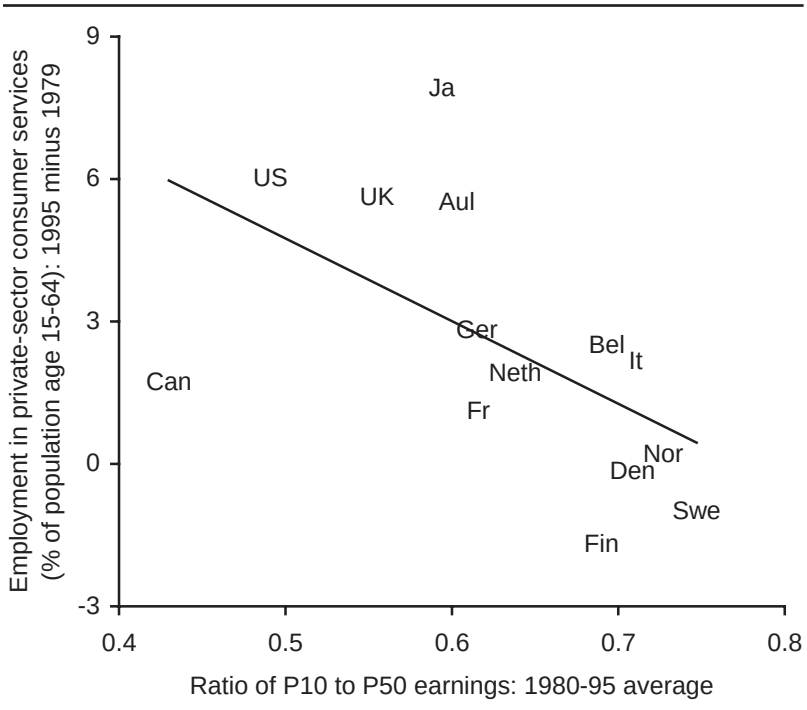

Figure 1a. **Pay Equality and Private Consumer Services Employment Growth.**

economic pressures for fiscal austerity coupled with political resistance to heavy tax burdens (see also Esping-Andersen, 1999, p. 153). Recent trends suggest that this may be correct. Among the Scandinavian countries, only oil-rich Norway continued to expand public employment in the 1990s.

The Iversen-Wren argument is seemingly a compelling one, and their empirical analysis of employment growth in private consumer services in 14 OECD countries yields supportive results. Yet there are several reasons to question it. One is that the second component of their hypothesized causal chain—the notion that employment growth in private consumer-related services is a key determinant of aggregate employment performance—may be suspect. A recent OECD (2001, p. 107) study finds that only half of the employment gap between the United States and Western Europe lies in low-paying jobs; the other half is in high-paying positions. Another OECD (2000a) study examines employment growth in the 1980s and 1990s and finds that

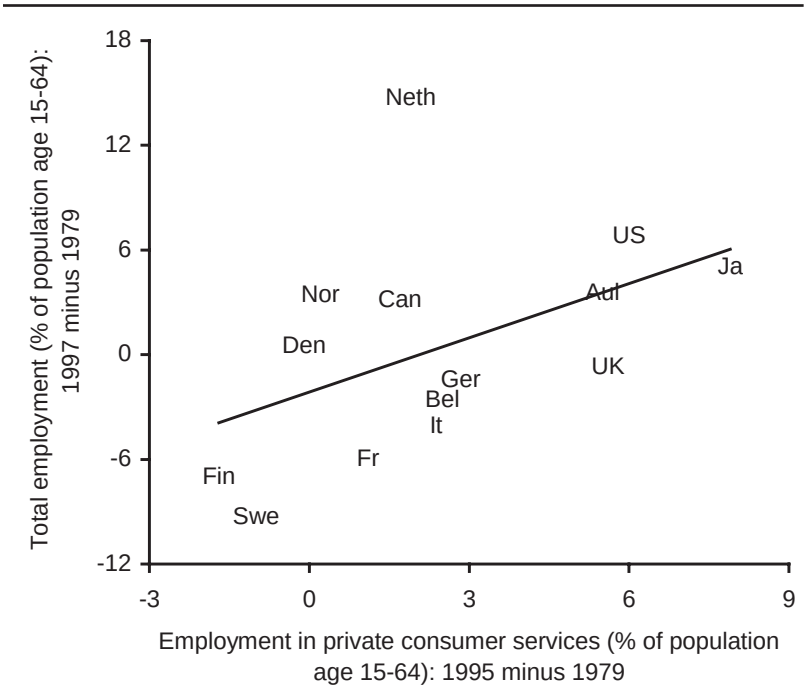

Figure 1b. **Private Consumer Services Employment Growth and Total Employment Growth.**

> countries in which employment grew fastest tended to have above-average gains across all sectors. This suggests either that economy-wide factors have been the dominant determinants of international differences in employment growth or that the presence of one or a few especially dynamic sectors generates "spill-over" effects that raise growth rates in the rest of the economy. (p. 110)

If the first of these alternatives is true, then pay equality should be relevant for overall employment growth only if it has employment-reducing effects across a variety of sectors, which Iversen and Wren do not claim. They suggest, for example, that there is likely to be no such adverse impact in manufacturing because higher productivity eases the cost constraint imposed by high wages. Their findings suggest that is, indeed, the case (Iversen & Wren, 1998, p. 531). If the second alternative is correct, pay equality could have an important effect on overall employment growth even if its direct impact were confined to private consumer-related services, but it would be necessary to

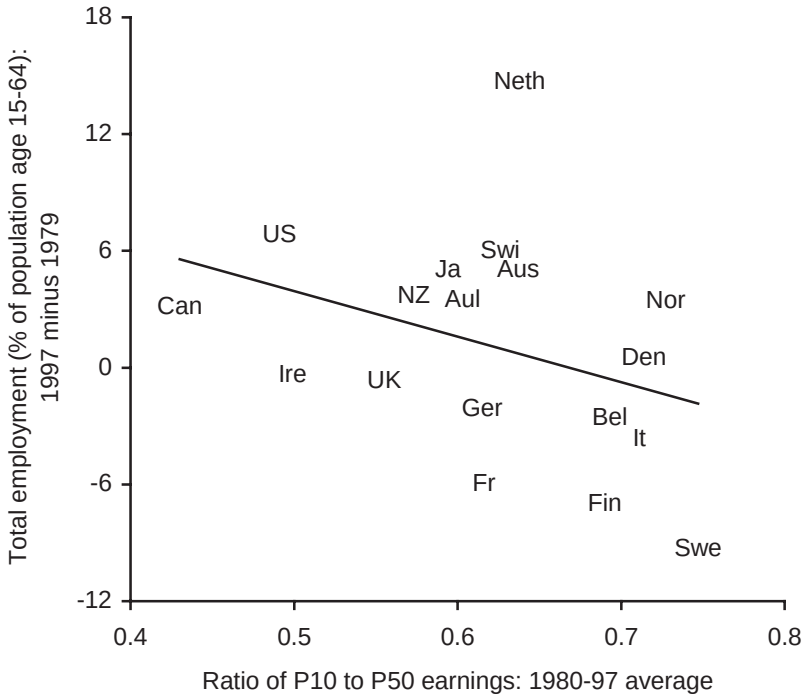

Figure 1c. **Pay Equality and Total Employment Growth.**

show that job growth in private consumer services creates spillover effects which generate job growth in other sectors.

However, my focus in this article is not on this second link in the hypothesized causal chain. Instead, it is on the first link—that is, the link between pay equality and growth of private-sector consumer services employment. There are a variety of reasons why high pay levels may not reduce the demand for labor. Efficiency wage theory, for instance, posits that employers willingly pay more to increase worker effort and commitment (Akerlof & Yellen, 1986). Low-productivity service jobs are among the most likely to be characterized by low employee commitment and high turnover. Employers may therefore find it profitable to pay higher wages to the extent that doing so helps to alleviate these problems. In this context, higher relative pay levels at the bottom of the labor market would not deter employment, and thus countries with greater pay equality would not be expected to have slower job growth in these low-productivity private-sector services.

More generally, productivity levels in low-wage jobs may differ widely across countries because of differences in skills, work organization, and mechanization, among other factors. A relatively high P10/P50 ratio therefore might simply be a function of relatively high productivity. If so, pay equality should not act as a deterrent to job growth.

Yet Iversen and Wren do find an adverse effect of pay equality on employment growth in private consumer services. The chief reason to question this finding is that the association they discover could be spurious. Their analysis did not control for a number of labor market policies and institutions that may affect employment performance, including active labor market policy, public employment, employment regulations, tax rates, the generosity of unemployment benefits, the duration of those benefits, wage-setting coordination, and unionization (Alderson & Nielsen, 2001; Gustafsson & Johansson, 1999; Hall & Franzese, 1998; Kenworthy, 2002; Korpi, 1991; Nickell & Layard, 1999; Rueda & Pontusson, 2000; Scarpetta, 1996; Scharpf, 2000). Active labor market policy consists of expenditures on activities for the unemployed that are aimed at helping them return to work, such as training, assistance with job searches, and employment subsidies. The creation of public-sector jobs is a direct employment-boosting policy. Employment regulations limit employers' flexibility in labor deployment, most notably in reducing their ability to lay off employees during a downturn, and may thereby discourage hiring. A higher tax rate on employees increases nonwage labor costs. More generous and long-lasting unemployment benefits reduce the incentives for unemployed workers to get a new job. Wage setting coordination and unionization may affect employment by reducing and increasing, respectively, the rate of wage increase. Most of these eight variables are moderately or strongly correlated with pay equality; the zero-order correlations across 14 OECD countries over the period 1980-1997 are .74, .48, .69, .59, .46, .10, .63, and .67, respectively. Thus, the pay equality variable in Iversen and Wren's analysis may have, in fact, been capturing effects of some or all of these other labor market policies and institutions rather than the effect of pay equality itself.

Consider Figure 2, which replicates Figure 1a but with pay equality replaced on the horizontal axis by the tax rate on a typical worker. The pattern looks strikingly similar. An ordinary least squares regression (OLS) of the change between 1979 and 1995 in private-sector consumer services employment on pay equality and the tax rate yields the following estimates, with absolute *t*-ratios in parentheses:

$$\Delta \textit{ private-sector consumer services employment} = 14.31 - 6.49\ (0.90)\ \textit{pay equality} - 0.15\ (2.69)\ \textit{tax rate}.$$

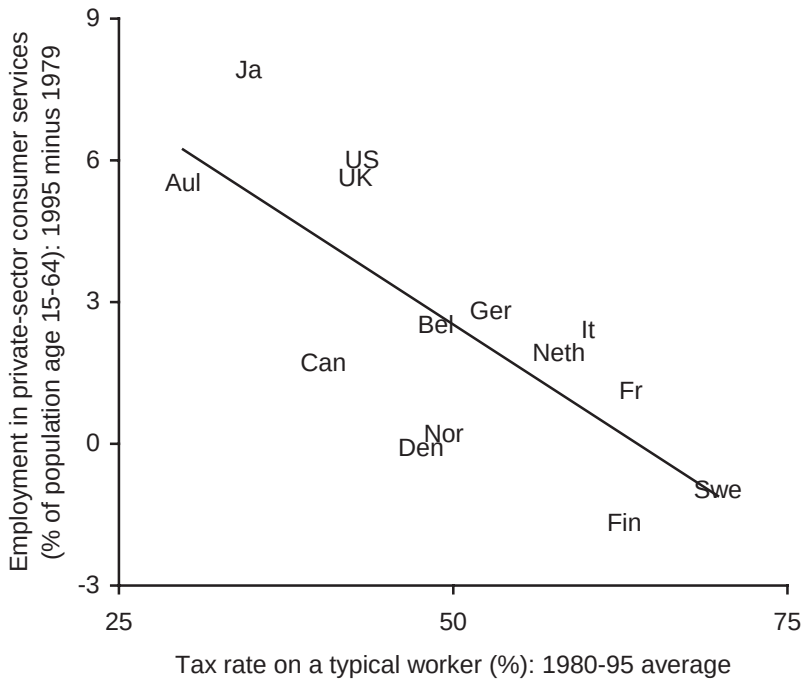

Figure 2. **The Tax Rate and Private Consumer Services Employment Growth.**

Both variables are negatively signed as expected, but only the tax rate variable is statistically significant. The standardized coefficients are –.21 for pay equality and –.69 for the tax rate, indicating a much stronger impact for the latter than for the former. This regression is merely suggestive, of course. But what it suggests is that including the tax rate and other institutional features of labor markets in the analysis may yield a different conclusion than that reached by Iversen and Wren about the effect of relative pay levels on employment growth.[3]

What about the replacement rate? It certainly seems reasonable to suspect that the higher the benefits are relative to former earnings, the higher unemployment will be. Generous unemployment benefits, however, may make

3. An additional limitation of the Iversen and Wren study is that it extends only to the end of the 1980s. However, Iversen (2000) finds similar effects when the years 1990 through 1996 are added.

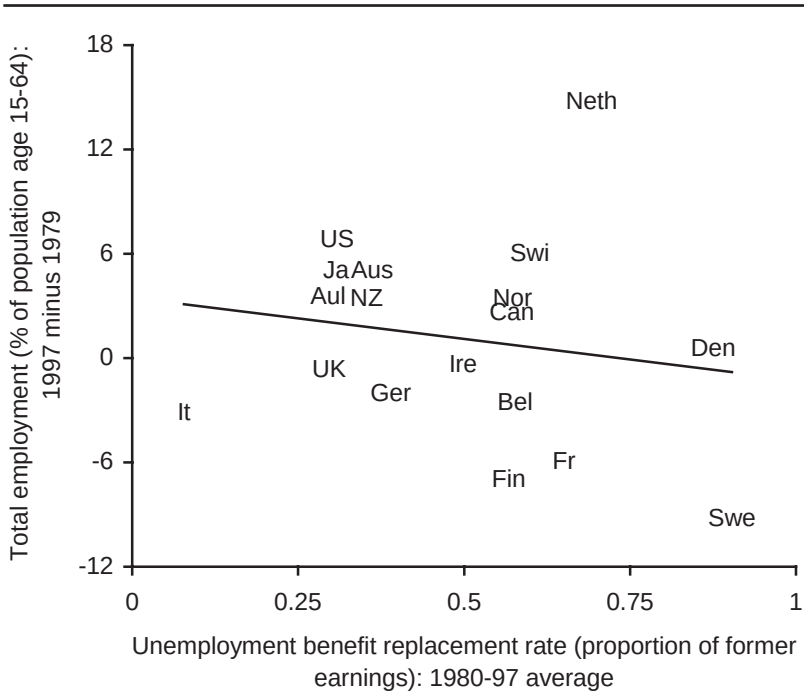

Figure 3. **The Replacement Rate and Total Employment Growth.**

employees and unions willing to accept weaker employment protection laws and practices, thereby contributing to healthier employment performance (Hemerijck & Schludi, 2000, pp. 220-221). Several studies have found a detrimental impact of the generosity of jobless benefits on unemployment and/or employment (Adsera & Boix, 2000; Blanchard & Wolfers, 2000; Nickell & Layard, 1999; OECD, 1994, chap. 8). However, this effect is frequently found to be much less important than that of the duration of unemployment benefit eligibility (Freeman, 1998, p. 8; Graafland, 1996). Moreover, none of these studies controlled for relative pay levels, which seem likely to influence the degree to which unemployment compensation deters reentry into the work force. Here too, therefore, previous findings of an employment-reducing effect could potentially be a product of omitted variable bias. Figure 3 gives no indication of any relationship between the replacement rate and the aggregate employment growth at the bivariate level.

METHOD AND DATA

This article offers a multivariate analysis of the effects of pay equality and unemployment benefit levels on variation in employment performance across 14 OECD countries in the 1980s and 1990s. All variables are described and their data sources listed in the appendix. The dependent variables are private-sector consumer services employment and total employment, both measured as a share of the working-age population. (Results with unemployment as the dependent variable are largely similar to those for total employment. These and other results not shown here are available from the author on request.) I use a pooled time-series cross-section regression design in which each observation is a country-year. By increasing the number of observations, this permits inclusion of a larger number of control variables than is possible in a single-period cross-sectional analysis (such as Esping-Andersen, 1999, chap. 7, 2000; Galbraith et al., 1999; Glyn & Salverda, 2000; Howell, 2002; Scharpf, 2000, p. 83). I focus on the 1980s and 1990s—specifically, the years 1980-1997. Data limitations prevent inclusion of more recent years. I include 14 of the "OECD-18" nations commonly used in cross-national analyses of this type. Austria, Ireland, New Zealand, and Switzerland are left out—in part to assure direct comparability with the Iversen and Wren (1998) study, but mainly because of data constraints. Data on employment in private-sector services are not available for these four countries, and data on pay equality are available for only a small number of years for several of them (1 year for Austria, 3 years for Ireland, 6 years for Switzerland). Because of gaps in the pay equality data and in the data for a few of the control variables, the 14 countries used in the analysis differ in the number of years for which they can be included: Australia (16), Belgium (11), Canada (10), Denmark (11), Finland (18), France (18), Germany (12), Italy (11), Japan (18), the Netherlands (16), Norway (14), Sweden (18), the United Kingdom (16), and the United States (18). The total number of observations is 207. Unfortunately, it is no longer possible to separate private employment from public employment in consumer services in the OECD database. Hence, the time series for this variable extends only through 1995, and the analyses for it include only 189 observations.

If pay equality and the replacement rate had little or no effect on labor market performance prior to the 1980s, as is commonly presumed, then the rates of employment with which countries entered the 1980s are exogenous in this analysis. I therefore include a regressor representing the average level of the dependent variable during 1974-1979. Results without this variable, which focus straightforwardly on levels of employment in the 1980s and

1990s rather than their change relative to the mid-to late 1970s—are substantively similar (not shown here).

The two key independent variables are pay equality and the unemployment benefit replacement rate. Data for both come from unpublished OECD databases. Pay equality is measured as the ratio of annual earnings at the 10th percentile of the earnings distribution to the 50th percentile.[4] As noted earlier, these data are not available for all countries in all years. I use interpolated values where there is a gap of 3 years or less in a country's time series.[5] (This adds only 15 observations and does not affect the results of the analysis.) The replacement rate is the proportion of a worker's former earnings that is replaced by unemployment compensation and related benefits for a worker with earnings at two thirds of the national median (i.e., the 33rd percentile) in the first year after losing the job.[6] The data are for the gross replacement rate; taxes on earnings or benefits are not taken into account. Some argue that the net replacement rate, which does factor in taxes, is a more appropriate measure (e.g., Esping-Andersen 2000, p. 79). Yet because of imperfect information, there is reason to think that that the gross replacement rate may be more likely to affect individuals' decisions about how quickly to reenter the work force. As an OECD (1996) study has noted,

> calculating the net incomes of someone in and out of work, taking account of family allowances, earnings additions, peculiarities of the tax system, the interactions of benefits, and the timing of payments requires knowledge of many pages of regulations. Small wonder, then, that surveys suggest people have very little idea of how much net income they might have were they to move from being employed to being unemployed or vice versa. (p. 42)

In any event, data on net replacement rates are not available over time.

A number of additional variables are included in the regressions as controls for the general economic environment and for labor market policies and institutions. Each has been found in previous studies to affect employment performance and is potentially correlated with pay equality and/or the replacement rate (Alderson & Nielsen, 2001; Blau & Kahn, 2002a; Gustafsson & Johansson, 1999; Hall & Franzese, 1998; Kenworthy, 2002;

4. Another source of data on pay equality is the International Adult Literacy Survey (IALS) conducted by the OECD. Blau and Kahn (2002b) have calculated pay ratios for seven countries based on the wage data in the IALS. These figures correlate at .85 with the OECD figures I use here.

5. Rueda and Pontusson (2000) do the same.

6. Data are also available for the replacement rate for a worker with earnings at the median (50th percentile). The two replacement rate measures are highly correlated ($r = .93$), so the choice of which to use in the analyses makes little difference.

Korpi, 1991; Nickell & Layard, 1999; Palley, 2001; Rueda & Pontusson, 2000; Scarpetta, 1996; Scharpf, 2000).[7]

Growth of real GDP (average of t, t–*1, and* t–*2):* Faster economic growth is conventionally presumed to increase employment.

Trade (exports plus imports as a share of GDP; average of t, t–*1, and* t–*2):* Labor market outcomes in nations more heavily dependent on trade may be influenced to a greater extent by trends in the international economy, and they may be adversely affected to the extent that such trade is with lower cost and/or higher productivity competitors.[8]

Real long-term interest rates (average of t, t–*1, and* t–*2):* Restrictive monetary policy in the form of high interest rates is likely to have adverse effects on employment.

Active labor market policy (logged): Expenditures on activities for the unemployed that are aimed at helping them return to work—such as training, assistance with job search, and subsidized employment—may increase employment.

Public employment (as a share of the population age 15 to 64): Higher levels of government employment may crowd out private employment and/or contribute to higher overall employment.

Employment regulations: This index gauges the strictness of legislation on working time, fixed-term contracts, employment protection, minimum wages, and employees' representation rights on works councils and company boards. These types of regulations are commonly thought to weaken employers' willingness to hire additional employees.

Tax rate (on a typical worker): This is a measure of the tax wedge between labor costs for firms and take-home pay for workers.[9] A higher wedge is expected to reduce employer demand for labor.

Duration of unemployment benefit eligibility: The longer one can receive unemployment compensation, the weaker the incentive to get a new job.[10]

Left government (average of t, t–*1, and* t–*2):* Leftist parties are widely presumed to be more likely to use various policy tools to reduce unemployment. I include this variable to capture employment-promoting policies not directly tapped by the other policy variables in the regressions.

7. I also tried including female labor force participation (as a share of the working-age female population) as a control variable. It did not alter the findings for pay equality or the replacement rate in the regressions for employment in private-sector consumer services. This is consistent with the findings of Iversen (2000). When the female labor force participation variable was included in regressions for total employment, the pay equality coefficient often switched sign (although it was not statistically significant). However, the female labor force participation rate correlates at .85 with total employment. Because it is not clear in which direction the causality primarily runs—from growing women's labor force participation to growing employment or the reverse—this variable is best left out of the total employment regressions.

8. I also tried including a variable representing terms of trade (export prices divided by import prices) and an interaction between the trade and terms of trade variables, as per Hall and Franzese (1998). However, this interaction term turned out to be statistically irrelevant, and it did not alter the results for other variables.

Wage setting coordination: Where wage setting is coordinated, externalities of large wage increases, such as unemployment, tend to be taken into account by union negotiators. This creates an incentive for wage restraint. By inducing such restraint and/or by encouraging governments to pursue unemployment-reducing measures in exchange for it, wage coordination may increase the employment rate.

Union density (percentage of the labor force unionized): Controlling for wage coordination, more extensive unionization is expected to generate greater wage increases and therefore lower employment.

I focus the analysis on the cross-country variation in labor market outcomes by including a set of year dummy variables. This is consistent with Nickell and Layard (1999), among others. Institutional factors such as relative pay levels and the replacement rate are thought to primarily affect variation across countries, as longitudinal variation in employment performance within affluent nations is attributable mainly to "exogenous shocks," such as falling demand for labor (because of technological change, globalization, and perhaps other factors), declining rates of productivity growth, and rising real interest rates (Blanchard & Wolfers, 2000; Freeman, 1998; Nickell & Layard, 1999; Phelps, 1994). The bulk of the variation in the pay equality and replacement rate variables is between countries rather than over time within countries.

Following common practice in recent comparative political economy research, I estimate the models using OLS with panel-corrected standard errors and a common-rho adjustment for AR(1) autocorrelation (Beck & Katz, 1995). Substantively similar results were obtained using random-effects generalized least squares (GLS). A country fixed-effects model cannot be used here because some of the key explanatory variables—such as employment regulations, the tax rate, the duration of unemployment benefits, and wage setting coordination, as well as pay equality and the replacement rate—vary little over time (Alderson & Nielsen, 2001; Beck & Katz, 2001; Traxler, Blaschke, & Kittel 2001, pp. 27-28). A test for unit roots (Im, Pesaran, & Shin 1997) revealed no problem with nonstationarity in the two dependent variable measures.

I conduct two types of sensitivity checks: (a) extreme bounds: since the "correct" model is not known with certainty, I reestimate the regressions with all possible combinations of the control variable; and (b) jackknife: this consists of reestimating the regressions with countries dropped one at a time.

9. I obtained similar results using an alternative tax measure suggested by Scharpf (2000, pp. 81-82)—social security contributions plus consumption taxes as a share of GDP.

10. It would be helpful to also control for the strictness of criteria used in deciding who is eligible to receive unemployment compensation, but no comparative measures exist (see Grubb, 2000/01).

Table 1
Regression Results: Employment Performance, 1980-1997

	Employment in Private-Sector Consumer Services		Total Employment	
	1	2	3	4
Pay equality	–.13***	–.07	–.20***	–.11*
	[–8.45]	[–4.86]	[–16.88]	[–9.43]
	(2.59)	(1.27)	(3.55)	(1.66)
Replacement rate	–.07***	–.01	.10	.16
	[–1.63]	[–.09]	[3.70]	[5.29]
	(2.21)	(.14)	(1.85)	(2.80)
Growth of real GDP	.03***	.03***	.10***	.10***
	(3.06)	(2.91)	(3.92)	(3.32)
Trade	–.04*	–.05**	–.06	–.10*
	(1.50)	(1.71)	(1.16)	(1.56)
Real long-term interest rates	–.04***	–.05***	–.08***	–.15***
	(2.67)	(3.08)	(3.25)	(4.30)
Active labor market policy		.02		.14***
		(.94)		(2.37)
Public employment		–.11***		.14**
		(2.68)		(1.55)
Employment regulations		–.07*		–.24***
		(1.47)		(3.52)
Tax rate		–.08*		–.12*
		(1.35)		(1.53)
Unemployment benefit duration		–.04		–.17***
		(1.20)		(3.45)
Left government		.05***		.06**
		(4.87)		(2.20)
Wage-setting coordination		.01		.05*
		(1.17)		(1.29)
Union density		–.05*		.05
		(1.33)		(.49)
Average level of the dependent variable, 1974-79	.84***	.77***	.77***	.55***
	(24.36)	(19.48)	(9.08)	(6.98)
R^2	.93	.95	.97	.98
N	189	189	207	207
Pay equality	–11.12 to –3.73[a]		–30.20 to –3.38[a]	
	–6.00 to –3.04[b]		–17.91 to –3.21[b]	
Replacement rate	–2.92 to .40[a]		–1.86 to 8.56[a]	
	–.52 to .51[b]		1.60 to 9.21[b]	

Table 1 (Continued)

Note: Standardized regression coefficients, with absolute *t*-values in parentheses. Unstandardized coefficients for the pay equality and replacement rate variables are shown in brackets. Standardized coefficients are calculated as the unstandardized regression coefficient multiplied by the standard deviation of the independent variable and divided by the standard deviation of the dependent variable. Ordinary least squares (OLS) estimates with panel-corrected standard errors and a common-rho adjustment for AR(1) autocorrelation. Results for year dummy variables are not shown. For variable descriptions and data sources see the appendix.
a. Range of unstandardized coefficients in regressions with all possible combinations of the control variables (extreme bounds).
b. Range of unstandardized coefficients in regressions with countries omitted one at a time (jackknife).
*$p < .10$. **$p < .05$. ***$p < .01$ (one-tailed tests).

Outliers may be of particular concern for the period under consideration here; Finland experienced a deep economic shock as a result of the sudden collapse of the Soviet market, Germany took on the burden of unification, Norway benefited from substantial oil revenues, the United States lowered its unemployment rate in part by incarcerating a larger share of its unskilled males, and so on.

FINDINGS

The regression results are displayed in Table 1. Two models are shown for each employment measure. One includes only the growth, trade, and interest rate controls; the other adds the controls for labor market policies and institutions. The control variables perform largely as expected, although not all reach statistical significance in all models. Economic growth, active labor market policy, left government, and coordinated wage setting tend to be associated with better employment outcomes, whereas trade, interest rates, employment regulations, the tax rate, the duration of unemployment benefit eligibility, and unionization are associated with worse outcomes. Public employment appears to crowd out job growth in private-sector consumer services but boost aggregate employment growth. Employment levels in 1974-1979 are strongly related to 1980s and 1990s levels. This partly accounts for the very large R^2 values, although even without this variable the R^2 values are .80 or better.

Consistent with the finding of Iversen and Wren (1998, p. 527), the pay equality coefficient is negatively signed and statistically significant in the first regression for employment growth in private consumer services (column 1). But when the controls for labor market policies and institutions are added, the coefficient is no longer statistically significant (column 2). It is

unclear how much we should make of this. It is possible, for instance, that the estimate for the pay equality variable is biased downward because of inclusion in the regression of several other variables that may influence the distribution of earnings. Wage-setting institutions and unionization levels have been linked with cross-country differences in earnings inequality (Alderson & Nielsen, 2001; Rueda & Pontusson, 2000; Wallerstein, 1999), and the pay equality variable I use here is fairly strongly correlated with each (r = .63 with wage coordination and .67 with union density). These variables could therefore be soaking up part of the effect of pay equality on employment growth. When these two variables are removed, the metric coefficient for the pay equality variable increases in absolute value—although only slightly—from –4.86 to –5.25, and it reaches statistical significance at the .10 level (not shown here). As indicated at the bottom of the table, the extreme bounds and jackknife regressions yielded coefficients for the pay equality variable that were always negative and almost always significant at the .10 level or better. Thus these results support the notion that there is a trade-off between pay equality and employment growth in private-sector consumer services.

However, the estimated employment-reducing effect of pay equality is relatively modest in magnitude. The standardized coefficients indicate that pay equality is one of the more important determinants of job growth in private consumer services; yet it is only one among a large number of determinants, and its estimated effect is no stronger than the effects of employment regulations and the tax rate. The extreme bounds regressions provide a lower and upper bound for the estimated effect. The coefficients in column 1 and column 2 offer what is likely a more accurate lower and upper bound because the column 1 regression includes none of the labor market policy and institution variables, whereas the column 2 regression includes all of them. Collinearity with some of these variables almost certainly depresses the pay equality coefficient in the column 2 model somewhat. Yet leaving those variables out of the model is not necessarily more informative, because the pay equality variable is then permitted to capture part or all of the effects of the omitted variables.

Suppose we consider the best estimate of pay equality's true effect to be midway between the coefficients for the column 1 and column 2 regressions—that is, approximately –6.50. This suggests that if we compare two countries that differ by one standard deviation in the P10/P50 earnings ratio, the country with the higher level of earnings equality would have had approximately one half of a percentage point less growth in private-sector consumer services employment over the 1980s and 1990s. To get a better sense of the magnitude of the effect, consider two extreme cases that are frequently contrasted in discussions of this issue, the United States and Sweden. Between 1979 and

1995 (the most recent year for which data are available), private-sector consumer services employment in the United States increased from 20% of the working-age population to 26%, whereas in Sweden it fell from 14% to 13%. The difference between these two countries was thus 7 percentage points (+6 for the United States, –1 for Sweden). The United States had the second-lowest (after Canada) P10/P50 earnings ratio during this period, averaging .49, whereas Sweden had the highest at .75. If we multiply the "best estimate" pay equality regression coefficient (–6.50) by the difference between these values for the pay equality variable (.75 – .49 = .26), we can get an estimate of pay equality's contribution to the difference in employment growth in private consumer services between these two countries (1.7 percentage points). When compared to the actual difference of 7 percentage points, this seems relatively small—not trivial, to be sure, but certainly not overwhelming. Differences between these and other countries in a variety of factors—not just pay equality, but also GDP growth, trade, interest rates, public employment, employment regulations, the tax rate, and government partisanship—appear to have contributed to the differences in job growth in low-productivity private-sector consumer services.

For total employment, the findings are fairly similar. Again the coefficient for the pay equality variable is negatively signed. Here, it remains statistically significant (at the .10 level) even with inclusion of the labor market institution and policy controls (column 4), and this result proved robust in extreme bounds and jackknife regressions. Here too, however, the magnitude of the estimated effect is not particularly large. Suppose we again take the midpoint between the coefficients in columns 3 and 4 (approximately –13.0) as the best estimate of pay equality's true effect. This suggests that if we compare two countries that differ by one standard deviation in the P10/P50 earnings ratio, the country with greater earnings equality would have experienced roughly 1.2 percentage points less growth in total employment in the 1980s and 1990s. Once again the United States and Sweden represent extreme cases (if we disregard the Netherlands; see Figure 1c), with the former experiencing an increase of 7 percentage points in total employment between 1979 and 1997 and the latter experiencing a decline of 9 percentage points. The regression estimates suggest that pay equality accounted for approximately 3.4 percentage points out of the total difference of 16. The standardized coefficients in column 4 indicate that pay equality has been a less important determinant of cross-country differences in aggregate employment growth than monetary policy, active labor market policy, public employment, employment regulations, the tax rate, and the duration of unemployment benefits.

These results, which suggest a rather modest employment-reducing effect of pay equality, could potentially be misleading—an artifact of certain fea-

tures of the analysis. One possibility is that the effect has occurred only gradually. If so, analyzing the 1980s and 1990s together might hide its true magnitude. Yet when I ran the regressions for the 1990s only (not shown here), the coefficients for the pay equality variable suggested effects slightly weaker, rather than stronger, than those reported in Table 1.

Another possibility is that poor employment performance may increase earnings inequality. Reverse causality of this form would bias the regressions against finding an adverse effect of pay equality on employment. This bias can be avoided by using a measure of pay equality at the beginning of the period, since changes in employment during the 1980s and 1990s cannot have caused the level of pay equality in 1979. As a practical matter, however, this makes no difference, because there is very little within-country variation in pay equality over time. In fact, for the 14 countries pay equality measured in 1979 (or the earliest available year) correlates at .98 with the annual pay equality measure used in the analyses here. Thus, there is little likelihood of a reverse causality problem in these analyses.

It also is possible that the employment-reducing effect of pay equality is stronger in some countries than in others. In particular, we might reasonably suspect that in more liberal, market-oriented economies, such as the United States and United Kingdom, employers' hiring decisions will tend to be more sensitive to labor costs. Other economies may feature better or more widespread institutional supports for worker commitment and for higher productivity, such as Germany's apprenticeship system and Japan's tradition of employment security and seniority-based pay structures (see, e.g., Aoki, 1988; Soskice, 1999). In such contexts, high pay levels might have less impact on hiring. To test this possibility, I used a dummy variable with Belgium, Denmark, Finland, France, Germany, Italy, Japan, the Netherlands, Norway, and Sweden coded 0 and Australia, Canada, the United Kingdom, and the United States coded 1. I added this variable and an interaction term between it and the pay equality variable to the regressions (results not shown here). For employment growth in private consumer services, the interaction term had the expected negative sign and was usually statistically significant at the .10 level or better, depending on the particular combination of control variables included. This suggests that pay equality has a stronger employment-reducing impact in low-productivity services in the market-liberal, Anglo countries than in Scandinavia, Continental Europe, and Japan. For aggregate employment growth, the interaction coefficient again generally had the expected sign but was seldom statistically significant.

What effect does the generosity of unemployment-related benefits have on employment growth? In the private consumer services regressions, the replacement rate coefficient is negative in both equations but does not reach

statistical significance in the regression that includes the full set of controls (column 2). In a number of the extreme bounds regressions and several of the jackknife regressions, the coefficient turned positive. Perhaps more importantly, even if benefit generosity does have an employment-reducing effect, it appears to be quite weak. An increase of one standard deviation (.233) is estimated to have reduced private consumer services employment by somewhere between one twentieth and one third of a percentage point in the 1980s and 1990s relative to 1974-1979. For total employment, the replacement rate variable yields an unexpected positive sign in both regressions, suggesting that any detrimental impact it may have on job growth in private consumer services does not adversely affect aggregate employment trends. This finding proved relatively robust to inclusion of various combinations of the control variables and to omission of individual countries. Interactions between the replacement rate and the market-liberal economy dummy variable yielded no statistically significant results.

An alternative estimation strategy to the one used here would be to substitute the employment level lagged 1 year for the 1974-1979 employment level in the regressions. The rationale is that employment levels may be sticky from year to year because of factors not fully captured by the independent variables I have included. Inclusion of a 1-year lagged dependent variable substantially alters the results for a number of the variables (not shown here). In particular, for both private-sector service employment and total employment, the pay equality coefficient is seldom statistically significant and frequently switches signs, depending on the particular combination of labor market institution and policy variables used in the regression. The same is true for several of the labor market variables themselves (see also Palley, 2001). This suggests that perhaps pay equality has no effect at all on employment growth. However, as Achen (2000) has pointed out, it is quite possible that this is merely a statistical artifact of the lagged dependent variable squashing the effects of substantively relevant independent variables such as pay equality. For this reason, I place more confidence in the results in Table 1.

A HIGH-EQUALITY, HIGH-EMPLOYMENT SOCIETY?

Large-scale unemployment has been arguably the prime economic, social, and political issue in Western Europe over the past decade. Europe's jobs problem is frequently said to be a product of labor market regulatory rigidities. Although the institutional features of European labor markets are sometimes lumped together, it is important to distinguish among them. They are of

the following main types: (a) employment regulations regarding job protection, working time, and related matters; (b) the unemployment compensation system; (c) the tax wedge; (d) wage-setting arrangements and their effects on real labor cost growth and on government policy choices; and (e) high relative earnings for workers at the low end of the labor market.

The findings here suggest that portions of the conventional wisdom regarding the deleterious effects of labor market rigidities on employment are accurate. Specifically, more extensive employment regulations, a larger tax wedge, a longer period of eligibility for unemployment benefits, and higher unionization rates appear to have adverse effects on employment growth. However, to the extent wage coordination has an effect on employment performance, it tends to be a beneficial one.

The last rigidity (high relative earnings at the bottom of the labor market) has received perhaps the most attention of the five in policy discussions. Yet there has been only one prior multivariate cross-country empirical study on this issue (Iversen & Wren, 1998), and it did not include controls for a variety of factors that influence employment performance and that are correlated with pay equality. My analysis of employment patterns in 14 OECD countries during the 1980s and the 1990s suggests that higher relative pay levels in low-wage jobs have indeed tended to reduce growth of low-productivity private-sector service employment and of overall employment. In other words, there does seem to be a trade-off. The trade-off, however, is a relatively mild one (see also Kenworthy, in press). Those who have overestimated the extent of the trade-off appear to have done so largely because they have attributed to pay equality what actually are the employment-reducing effects of a multitude of labor market policies and institutions.

What implications do these findings have for future labor market performance in affluent nations? Iversen (1999, chap. 6) and Esping-Andersen (1999) have suggested recently that such nations have two principal options if they wish to generate an ample supply of jobs. One is to reduce wages at the bottom of the earnings distribution to stimulate job creation in low-productivity private services. The other is to rely on expansion of public employment. Countries such as the United States and the United Kingdom have pursued the former strategy, whereas the Nordic nations have traditionally relied on the latter. However, both Iversen and Esping-Andersen contend that the public employment route is likely to encounter increasing economic pressure and political resistance, limiting a country's ability to sustain the high tax rates necessary to finance extensive public-sector job creation. Thus, in their view, a solution to the jobs problem in Western Europe will likely require increased pay inequality (Esping-Andersen, 1999, p. 173; Iversen, 1999,

p. 174; see also Hemerijck & Schludi, 2000, pp. 142, 213; Scharpf & Schmidt, 2000, pp. 312-13, 323, 333).

My findings suggest a variety of alternatives to these two options. Pay equality is only one among a number of labor market policies and institutions that influence employment. Countries could just as effectively stimulate job growth by reducing employment regulations, tax rates, or the duration of unemployment compensation or by upgrading active labor market policy. That is to say, even if the public-sector route to high employment is now effectively blocked, nations with a jobs problem could potentially go a long way toward alleviating it without having to reduce relative pay levels.

Denmark appears to be a useful illustration. Although aggregate employment in Denmark has increased only slightly since the late 1970s (see Figure 1b), that is largely because the country began this period with a very high employment rate. As of 2000, Denmark's employment rate of 77% was the third highest among the OECD-18 countries, behind only Switzerland and Norway. It has succeeded in maintaining a high level of employment despite no expansion of public-sector jobs since the early 1980s and without any notable reduction in its relatively high degree of pay equality or its extremely generous replacement rate. The sudden unwillingness to further expand public employment beginning in the early 1980s contributed to a jump in the unemployment rate toward the end of that decade. Then, in the early 1990s, Denmark was hit hard by the international economic recession; employment dropped and unemployment increased even further.

In 1994 and 1996, the Danish government introduced two major labor market reforms; it initiated several new active labor market measures to facilitate the transition from unemployment to a job, and it reduced the duration of unemployment benefit eligibility. Between 1994 and 2000, the Danish unemployment rate fell from 8.2% to 4.7%, and the employment rate climbed from 73% to 77%. Although the two policy reforms by no means deserve full credit for this improvement, a number of recent studies conclude that they helped substantially (Auer, 2000; Benner & Vad, 2000; Björklund, 2000; Madsen, 1999). Denmark's high employment level is also aided by the country's comparatively easy job dismissal rules, which encourage hiring. Dismissals and employee turnover are more common in Denmark than elsewhere in Europe, and there is less long-term unemployment (Esping-Andersen, 2000, pp. 94-95; OECD 2000b, p. 220). Weak employment protection is politically sustainable in Denmark in part because of generous unemployment benefit levels (Benner & Vad, 2000, p. 459; Madsen, 1999, p. 76).

An additional strategy is suggested by developments in the Netherlands over the past two decades. Pay equality and the replacement rate in the Neth-

erlands are fairly high and very high, respectively, when compared to other nations (see Figures 1a and 3 above). Dutch employment performance since the late 1970s has been unique in two respects: the country has had, by far, the fastest rate of employment growth among affluent nations, and it has achieved this very high rate of growth without a substantial increase in employment in private-sector consumer services (see Figure 1b). The phenomenal rate of overall employment growth must be placed in context. The employment rate in the Netherlands was one of the lowest among the OECD-18 countries at the end of the 1970s, so the improvement started from a very low base. Even so, it has been impressive. Unemployment has also declined substantially, although this is a bit less impressive because of the large number of people who are classified as disabled and therefore do not count as unemployed (Becker, 2001; Gorter, 2000).

Underlying the rise in employment in the Netherlands has been a substantial increase in part-time employment. Part-time jobs have been an important source of employment expansion in a number of countries, but they have grown most rapidly in the Netherlands, climbing from 18% to 30% of total employment between 1983 and 1999. Indeed, in the 1980s and 1990s, part-time jobs accounted for two thirds to three quarters of total job growth in the Netherlands, depending on the estimate (Hartog, 1999; Salverda, 1998; Visser, 2002). Still, since 1990 total hours worked have increased more rapidly than in the United States. The Dutch story is one of genuine job creation, not merely job sharing ("Neighbourly Lessons," 2002).

A large majority of Dutch part-time workers say they prefer not to have a full-time job (Auer, 2000, p. 19; Salverda, 1998; Visser, 2002, p. 36), so there does not appear to be great cause for concern about underemployment. But what about earnings? P10 to P50 pay equality has declined somewhat in the Netherlands, particularly since 1994. Moreover, the OECD earnings data include only full-time workers. Because part-time employees tend to get paid less per hour than their full-time counterparts, there is reason to suspect that if part-timers were included in the figures, we would find an even more substantial drop in earnings equality. Then again, for women, who account for most part-time employment, the median hourly earnings of part-time employees were 93% of those for full-time employees as of 1995 (Visser, 2002, p. 33). Wiemer Salverda (1998) has calculated hourly wages at the 10th and 50th percentiles in the Netherlands with both full-time and part-time workers included. As it turns out, the trend over time for these figures is no different from that for the full-time annual pay data, although Salverda's data extend only through the late 1980s. In addition, Joop Hartog (1999) has examined the industries in which there was an increase in employment, including part-time employment, between 1987 and 1995. He finds that

"employment growth is really across the board of the entire wage distribution, with neither support for a marked dualization nor for a concentration of employment growth in low-wage pockets." The overall degree of pay equality in the Netherlands thus does not appear to have been adversely affected by the acceleration of part-time employment. And although pay equality has decreased somewhat, it remains a good bit above the U.S. level. The Dutch-style part-time, or "one-and-a-half jobs," economy therefore seems to be another reasonably egalitarian option for affluent countries searching for a solution to the jobs problem.

Plainly, this brief discussion of two national cases should be taken as merely suggestive. Data for recent years are somewhat sparse, and it is by no means clear that what works in one national context can be successfully implemented in another (Scharpf, 2000; Schwartz, 2000). We need more research on developments in individual countries. It also is worth emphasizing that my findings here in no way imply that affluent countries shouldn't elect to permit a bit more earnings inequality or to reduce unemployment benefit levels somewhat. That is a political choice; and in some contexts, it is perhaps a more feasible and/or attractive one than, for example, reducing tax rates or relying on part-time jobs to fuel employment growth. What the findings do suggest is simply that there appear to be other viable options for countries wishing to maintain or move toward a desirable combination of jobs and equality.

APPENDIX
Variable Descriptions and Data Sources

Active labor market policy: expenditures on active labor market policy per unemployed person as a percentage of average production worker earnings (Martin, 2000/2001, p. 86); logged to reduce skewness. Range = 1.5 to 5.3. Mean = 3.0. *SD* = 0.8.

Employment in private-sector consumer services: employment in private-sector consumer-oriented services—wholesale and retail trade, restaurants and hotels, and community/social/personal services—as a percentage of the population age 15 to 64 (Torben Iversen, Department of Government, Harvard University, calculated from Organization for Economic Cooperation and Development data; see Iversen & Wren, 1998, for discussion). Range = 8.6 to 28.9. Mean = 16.3. *SD* = 5.6.

Employment regulations: index ranging from 0 to 10, with each country scored from 0 (lax or no legislation) to 2 (strict legislation) on each of the following five types of employment regulations—working time, fixed-term contracts, employment pro-

tection, minimum wages, and employees' representation rights (on works councils, company boards, etc.) (Centre for Economic Performance, London School of Economics [CEP], n.d.; see Nickell, 1997, for discussion). Range = 0 to 7. Mean = 3.7. *SD* = 2.4.

Growth of real GDP: average of *t*, *t*–1, and *t*–2 (my calculations from data in OECD, 2003). Range = –3.5 to 5.4. Mean = 2.4. *SD* = 1.5.

Left government: left party cabinet portfolios as a proportion of all cabinet portfolios; average of *t*, *t*–1, and *t*–2 (Swank, n.d.). Range = 0 to 1.00. Mean = .30. *SD* = .36.

Pay equality: ratio of annual earnings of a person at the 10th percentile of the earnings distribution to a person at the 50th percentile—pretax and pretransfer, for full-time year-round employees (OECD, n.d.-b). Range = .41 to .77. Mean = .63. *SD* = .09.

Public employment: government employment as a percentage of the population age 15 to 64 (OECD, various years). Range = 4.4 to 26.1. Mean = 13.6. *SD* = 5.6.

Real long-term interest rates: average of *t*, *t*–1, and *t*–2 (my calculations from data in OECD, 2003). Range = –1.0 to 9.2. Mean = 5.0. *SD* = 1.9.

Replacement rate: proportion of a worker's former earnings (pretax) that is replaced by unemployment compensation and related benefits for a worker with earnings at two thirds of the national median (i.e., the 33rd percentile) in the first year after losing the job (OECD, n.d.-a; see Martin, 1996, for discussion). Range = .01 to .92. Mean = .51. *SD* = .23.

Tax rate: sum of the average payroll, income, and consumption tax rates for a typical worker (CEP, n.d.; see Nickell, 1997, for discussion). Range = 28.7 to 70.7. Mean = 50.4. *SD* = 12.0.

Total employment: total employment as a percentage of the population age 15 to 64 (my calculations from data in OECD, 2003). Range = 51.8 to 81.7. Mean = 67.2. *SD* = 7.4.

Trade: exports plus imports as a percentage of GDP; average of *t*, *t*–1, and *t*–2 (my calculations from data in OECD, 2003). Range = 16.7 to 144.5. Mean = 57.3. *SD* = 30.5.

Unemployment benefit duration: length of eligibility for unemployment benefits, in years; four indicates infinite duration (CEP, n.d.; see Nickell, 1997, for discussion). Range = 0.5 to 4.0. Mean = 2.4. *SD* = 1.5.

Union density: employed union membership as a percentage of the labor force (Ebbinghaus & Visser, 2000; Golden, Lange, & Wallerstein, 1997). Range = 8.6 to 88.6. Mean = 41.8. *SD* = 23.0.

Wage-setting coordination: index with five categories; larger numbers indicate greater coordination (Kenworthy, 2001). Range = 1 to 5. Mean = 3.1. *SD* = 1.5.

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