

Web Appendix

for

Public Employment and Labor Market Performance

by

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The appendix reports additional results meant to document our result's robustness to alternative specifications. Our data set is also posted on the Web (data sources and definitions are listed in the published paper's Appendix 2), and interested readers may want to perform their own robustness tests.

1. TIMING AND DYNAMIC SPECIFICATION

The paper's specification lets labor market be influenced by the contemporaneous realization of public employment, and accounts for possible channels of reverse causation by instrumenting that realization (either by its own lagged value, or by political and institutional variables). The theoretical mechanism through which public employment affects labor market outcomes (via production substitution, wage bargaining, and labour allocation) is far from instantaneous, however. Accordingly, a more direct empirical specification may simply use lagged employment as a right-hand side variable. **Table W.1** reports OLS estimates, both in levels and in first differences.

In levels, the impact of lagged public employment on current unemployment rate is estimated at coefficient of 0.70, and significant at the 10 percent level (standard errors robust to country clustering). This is quite similar to what we found when instrumenting current public employment with its own lag : since public employment evolves very slowly over time (see Appendix 3), its lagged value predicts current realizations very well. The delayed estimated effect of public employment on private employment is similarly consistent with theoretical predictions and similar to the results reported in the paper (coefficient estimate is -0.25, significant at the 5 percent level with robust standard errors).

The regressions on first-differenced data test the results' robustness to potentially important spurious covariation, and remove fixed country effects. As in the paper, we include first-differenced institutional variables (only the unemployment insurance replacement rate and of employment protection index are time-varying among the institutional variables we consider). The estimated coefficients of lagged employment are significant at the 5 percent level and within one standard deviation of those estimated in levels.

2. SPURIOUS COVARIATIONS AND DETRENDING

Since public employment, private employment, and unemployment all move slowly over time (see Appendix 3) and increase steadily in some countries over the period 1960-2000, our results may in principle be spuriously driven by common trends and lack a causal interpretation. The first-differenced specifications in the paper and in Section 1 of this Web appendix indicate that results are indeed robust if the trends are similar across countries. Here, we further assess their robustness estimating first-differenced regressions on subsamples of countries grouped according to the size of rents in the public sector and the degree of substitutability between public jobs and private ones. The growth rate of public employment is instrumented by its 5 year lagged value, and we use a generalised least squares procedure to account for heteroskedasticity (differencing suffices to eliminate serial correlation, hence we do not report robust standard errors). We see in **Table W.2** that the impact of public employment on unemployment is quite similar and positive in all specifications, but statistically significant only in countries displaying high public sector rents (as measured by the wage differential or by the “corruption” index). Similarly, in **Table W.3** we see that public employment tends to increase unemployment in all groups of countries, but the effect is significant only where public and private activities are highly substitutable.

Spurious covariation might be driven by trends that, while common to public and private employment and unemployment in each country, differ across countries. In his Discussion, Jörn-Steffen Pischke outlines theoretical reasons why this might be realistic, and shows that including country-specific time trends in OLS regressions suffices to make public employment insignificant. Of course, as he admits, such trends cannot be given a structural interpretation, and may well be driving our theoretically plausible variables out of the regression by absorbing most relevant variation in the data. In **Table W.4** we report regressions with country-specific trends where, consistently with our theoretical arguments, the current realisation of public employment is instrumented by its (5-year average) lag. Like in Pischke’s OLS regressions, the trend coefficients are strongly significant, and the effects of public employment appear to be insignificant. The contrast between the regressions that do and do not include country-specific trends is much less pronounced in the IV specification, however; in particular, the sign of the coefficients is not affected. We view these results as further empirical support for our estimation of structural IV effects.

3. SAMPLE CUTS

We have also found the results to be robust to changes in the definitions of country groups. **Table W.5** reports estimation results from subsamples selected on the basis of theoretical considerations. We excluded the group of countries featuring both high rents and high substitutability (Belgium, Japan and Spain), or the group of countries featuring both low rents and low substitutability (Australia, Canada and Netherlands). In both cases public employment remains statistically significant, but the size of the crowding out effect is much more pronounced when the latter group is excluded. We also report a regression excluding the countries with the highest public employment share (the three Scandinavian countries: Sweden, Denmark and Norway). The results are statistically and economically similar to those obtained for the full sample, and foster our confidence in the empirical relevance of our theoretical views regarding the role of public-job rents

and substitutability: the composition (rather than only the size) of public employment matters for labor market outcomes.

Table W.1: OLS estimation of lagged public employment effects

Dependent variable	Unemployment rate	Private employment rate	Growth rate of unemployment rate	Growth rate of private employment rate
Public employment (-1)	.70 (.25)** [.42]*	-.25 (.04)** [.07]**		
Growth rate of public employment (-1)			.949 (.46)** [.39]**	-.19 (.07)** [.06]**
Fixed effects	Yes**	Yes**	No	No
Time effects	Yes**	Yes**	Yes**	Yes**
Growth rate of institutions	No	No	Yes**	Yes**
R ²	.87	.81	.44	.25
Number of observations	115	115	98	98

Note: Public employment, private employment and unemployment rates are taken in log. Public employment (-1) refers to the 5 period lagged value. The growth rate of institutions refers to the one of the replacement rate and the employment protection. Source: Blanchard and Wolfers (2000) (Standard errors) - [Standard errors robust to country clustering]
 **: significant at the 5%level, *: significant at the 10% level

Table W.2: G2SLS estimation of public employment effects on the unemployment rate according to the size of public rents - Estimation in first differences

Dependent variable:	High wage premium	Low wage premium	High corruption	Low corruption
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Growth rate of

Growth rate of Yes Yes* Yes*

Table W.3: G2SLS estimation of public employment effects on the unemployment rate according to the degree of substitutability of public production - Estimations in first differences.

Dependent variable :	High public expenditure substitutable goods	Low public expenditure substitutable goods	High public expenditure in health sector	Low public expenditure in health sector
Growth rate of unemployment				
Growth rate of public employment	2.11* (1.34)	.26 (1.09)	1.00 (.79)	2.47 (2.05)
Growth rate of institutions	Yes*	Yes*	Yes*	Yes*
Time effects	Yes**	Yes**	Yes**	Yes**
R ²	.380	.562	.482	.292
Number of observations	35	40	47	51

Note: The growth rate of public employment is instrumented by its lagged value.

Sub-samples size varies according to the availability of data for each clustering criteria

** : significant at the 5% level, * : significant at the 10% level

Table W.4: IV estimation of public employment effects with country specific trends

Dependent variable	Unemployment rate		Private sector employment rate	
Public employment	.83 (.30)** [.51]*	.32 (.80) [2.36]	-.30 (.05)** [.08]**	-.06 (.12) [.23]
Fixed effects	Yes**	Yes**	Yes**	Yes**
Time effects	Yes**	Yes**	Yes**	Yes**
Country specific trends	No	Yes**	No	Yes**
R ²	.86	.92	.80	.92
Number of observations	115	115	115	115

Note: Public employment, private employment and unemployment rates are taken in log.

Public employment is instrumented by its lagged value

(Standard errors) - [Standard errors robust to country clustering]

** : significant at the 5% level, * : significant at the 10% level

Table W.5: IV Estimation of Public employment effects according to sample cuts

Dependent variables	Exclusion of countries with high indicators of corruption and substitutability		Exclusion of countries with low indicators of corruption and substitutability		Exclusion of the Scandinavian countries				
	Unemployment rate	Private employment rate	Unemployment rate	Private employment rate	Unemployment rate	Private employment rate			
Public employment	.726* (.38)	-.25 ** (.06)	.80** (.32)	.27** (.09)	1.00** (.31)	-.33** (.06)			
Fixed-effects	Yes	Yes**	Yes**	Yes**	Yes**	Yes**			
Time-effects	Yes**	Yes**	Yes**	Yes**	Yes**	Yes**			
R ² - overall	.828	.701	.863	.826	.877	.833			
Number of observations		95	95		96	96		101	101

Note: Public employment, private employment and unemployment rate are taken in log. Period of estimation: 1960-2000

Public employment is instrumented by its lagged value

Sub-samples size varies according to the availability of data for each clustering criteria

** : significant at the 5% level, * : significant at the 10% level