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Do Flexible Labor Markets Indeed Reduce Unemployment? A Robustness Check

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Abstract Nickell *et al.* (2005) have frequently been cited as empirical evidence that labor market rigidities cause high unemployment. We find that their model is not robust. Leaving their database unchanged and changing three details in their estimation procedure, it turns out that several policy-relevant coefficients change sign or significance. We conclude that their claim from Non Accelerating Inflation Rate of Unemployment (NAIRU) theory that labor market rigidities cause unemployment is rather shaky. There is a remarkable discrepancy between weak empirical results and sweeping conclusions by policy practitioners with respect to the call for deregulation of labor markets.

Keywords: NAIRU, labor market rigidities, unemployment

I. INTRODUCTION

The view that high European unemployment is mainly due to rigid labor market institutions has become quite dominant both among labor market economists and among political practitioners. The following plea for deregulation of labor markets illustrates the policy implications of that view: "It is now vital for the lagging countries to take heart and implement the necessary reforms. The costs of inaction are too high in terms of continued unsatisfactory labor market performance. The successes achieved by some OECD countries show what can be done if there is sufficient political will to reform" (OECD 2006, editorial). Besides the OECD, the International Monetary Fund (2003), and the European Union, in its Lisbon agenda, all propagate the view that Europe suffers from "institutional

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sclerosis" in the labor market. The logical implication is a call for deregulation of labor markets, notably for removal of firing barriers, trimming of minimum wages, and reduction of social benefits.

Clearly, this call is not neutral. Economic policy inspired by the theory of the Non Accelerating Inflation Rate of Unemployment (NAIRU) holds considerable costs both for unemployed and workers. Cutting, for instance, the duration and/or generosity of benefits directly hurts the unemployed (Howell *et al.* 2007). Turning to workers, the corollary of NAIRU theory is the weakening of labor's bargaining position with the aim of diminishing its share in National Income (see among others Carlin and Soskice 1990; Layard *et al.* 1991; Rowthorn 1999). Hence, before encouraging policy-makers "to convince their electorates that it is necessary to swallow the medicine" (OECD researchers Scarpetta *et al.* 1996: 242), one should be quite sure of the robustness of the "evidence" behind such recommendations.

A major milestone for the *Labor-Market-Rigidities* research agenda was the OECD's (1994) *Jobs Study*. It was followed by a number of attempts at substantiating the view that unemployment was primarily caused by rigid labor market institutions (e.g. Belot and Van Ours 2001, 2004; Blanchard and Wolfers 2000; Feldmann 2009; Freeman 2010). In their empirical contributions, authors mention caveats relating to lack of (appropriate) data, the necessity of working with admittedly rough proxies, problems with reversed causality and others. It is remarkable, however, that such caveats are easily neglected in sweeping conclusions by policy practitioners.

In the empirical literature on the impact of labor market rigidities, the work by Nickell and various co-authors (to be traced back to Nickell 1997) appears as a landmark. Its basic approach stems from NAIRU theory: Labor market institutions such as employment protection legislation, the amount and duration of social benefits, union density and employment tax rates, all raise unemployment rates. In a highly influential article, Nickell *et al.* (2005) conclude that "...broad movements in unemployment across the OECD can be explained by shifts in labor market institutions. To be more precise, changes in labor market institutions explain around 55% of the rise in European unemployment from the 1960s to the first half of the 1990s, much of the remainder being due to the deep recession ruling in the latter period" (p. 22).

A detailed re-examination (and re-estimation) of all the econometric contributions that followed the OECD's (1994) *Jobs Study* is beyond the reach of a single paper. Rather, we concentrate on one paper (i.e. Nickell *et al.* 2005) which appears to have had an outstanding influence both in the

scientific literature and in policy papers.[1] After a detailed re-estimate, we question the robustness of their empirical results. In contrast to others who challenge the robustness of the labor market rigidity view of unemployment (e.g. Baker *et al.* 2005; Baccaro and Rei 2005, 2007; Howell *et al.* 2007), we do not alter the time span of observations, nor do we add other countries, nor do we change indicators of labor market institutions. We confine our exercise to implementing three small changes to the original model—and for these changes we have plausible arguments. Our study thus has the advantage that it cannot be criticized for arbitrariness in selecting time spans, country coverage or indicators of labor market institutions.

2. ROBUSTNESS TESTS

Nickell *et al.* (2005) analyze unemployment rates in OECD-countries from the 1960s to the 1990s. We discuss three problems concerning the (non-) robustness of their results. These relate to minor changes in the economic and/or econometric approach of their main regression (Nickell *et al.* 2005: 14, Table 5, column (1)). We demonstrate that their results change substantially when applying minor modifications to the estimation procedure or to the exact specification of the regression equation. Our suggested modifications are suitable according to econometric tests or follow from economic reasoning.

We concentrate on three points:

(1) Nickell *et al.* (2005) use an *Iterated* Generalized Least Squares method (IGLS). They provide no explanation of their preference for the iterated rather than the standard (three steps) *Feasible* GLS (FGLS) method. Applying the standard FGLS method to their data, we obtain results that differ substantially from their IGLS results.

(2) Nickell *et al.* (2005) include only a one-year lag of unemployment in their regression equation. However, inclusion of two-year and three-year lags in the equation yields significant coefficients on these regressors. More importantly, it significantly changes the parameters of several labor market institution indicators and substantially reduces auto-correlation in the residuals. The latter is important because auto-correlation in the residuals in combination with the inclusion of a lagged dependent variable leads to a bias in the estimates. Note that this source of potential bias is additional to the one indicated by Nunziata

1 According to *Google Scholar*, the article was mentioned in 590 sources between 2005 and June 2011.

(2005). He correctly acknowledges that the inclusion of a lagged dependent variable would lead to biased estimates in a fixed-effects context (regardless of the existence of auto-correlation in the residuals). This so-called Nickell-bias is proportional to the size of the auto-correlation coefficient and is not extensive in data sets, where the time dimension (T) is relatively large as compared to the group dimension (N) (Nickell 1981). In our case, the number of years is larger than the number of countries. Hence, Nickell *et al.* (2005) argue that the bias that stems from including the lagged dependent variable in a fixed effects context should be limited. Below, we return to this point. The results of the original Nickell *et al.* (2005) estimates indicate, however, that there is a second source of bias, namely, the fact that they end up with auto-correlation in the residuals. The bias caused by the latter, considering that the lagged dependent variable is included in the regression, may well be of greater magnitude than the Nickell-bias. And it does not diminish with an increase of the time span. In any case, a reduced auto-correlation in the residuals when we include two-year and three-year lags of unemployment leads to a reduction of the bias.

(3) Nickell *et al.* (2005) use interaction variables to estimate the combined effect of two indicators of labor market institutions. They define an interaction term as multiplication of the two variables, *after expressing them as deviations from their country means*. The latter implies that a change in one of the interacted variables at present will affect unemployment rates of a country at <u>all</u> times, even in the past. One may doubt the realism of this approach. We show that the Nickell *et al.* (2005) results are sensitive to a more intuitive definition of the interaction terms, i.e. the simple multiplication of both variables involved.

In interpreting our re-estimates, we focus on how changes in specification affect the sign and statistical significance of estimated effects of determinants of unemployment. While this does not address the concerns raised by McCloskey and Ziliak (1996) that economists tend to prioritize statistical over economic significance in interpreting regression results, our primary concern in the present article is to demonstrate the fragility of key results of existing work to changes in specification. Clearly, a full-fledged analysis of determinants of unemployment should take the McCloskey and Ziliak critique into account but here we do not tackle that issue systematically. For further reading on economic significance, see Baker *et al.* (2005).

2.1. Problem One: Sensitivity with Respect to the Estimation Procedure

Nickell *et al.* (2005) implicitly use an *iterated* GLS procedure rather than the more widely-used standard FGLS approach. Nickell *et al.* do not explain why they consider the *iterated* procedure more suitable than the standard one, leaving this an open question. Employing the standard procedure, leads to different results on several coefficients. The columns labeled "Nickell *et al.* 2005" and "$I \rightarrow F$GLS" in Table 1 give a comparison between the *iterated* and the *standard* FGLS method, estimating the same model with the same data.[2] It turns out that the coefficients for *Benefit Duration* and for *Union Density* become insignificant. Also, the money supply shock becomes insignificant, at twice its original size (coefficients of the money supply shock are not reported in Table 1 which is confined to the core of this article: Labor market institutions).

2.2. Problem Two: Sensitivity with Respect to the Lag Structure

Nickell *et al.* (2005) include a one-year lag of the dependent variable (i.e. unemployment) in the regression equation and comment on the high value of that coefficient: "This reflects a high level of persistence and/or the inability of the included variables to fully capture what is going on" (p. 15). When following this motivation, one could argue that more unemployment lags should be included in the regression if they have a significant meaning in explaining current unemployment. Column "+Lags" of Table 1 shows the results of a regression with two years' extra lags. Adding a four-years' lag does not further add to the explanatory power of the model.

All three lags of unemployment are statistically significant in explaining present unemployment. Also, if we follow Hausman (1978): The fact that coefficients of the other regressors change or become (in-) significant, points to the appropriateness of including these extra lags. Their inclusion is thus appropriate from an econometric point of view. Furthermore, we see that the coefficient of lag 1 of *Unemployment* has become larger, the other two lags of *Unemployment* having a negative coefficient of such magnitude that the total effect of past on current unemployment in the equation with two extra lags differs little from the original. Looking at the coefficients of the other regressors, however, we observe that *Benefit Duration*, *Union Density*, *Coordination of Bargaining*, the *Total Employment Tax Rate,* and the

2 We thank Luca Nunziata for making the data available and for his written comments.

Table 1: Results of Robustness Tests of the Estimates by Nickell *et al.* (2005)

Independent variables	Original estimates, by Nickell *et al.* (2005)[a,b]	Our alternative estimates (four versions)			
		$I \rightarrow FGLS$[c]	+Lags[b]	ΔInteraction[b,d]	(Combi)[c,d,e]
unemployment, lag 1	0.86**	0.87**	1.17**	0.87**	1.21**
	(48.49)	(46.35)	(31.83)	(49.88)	(35.97)
unemployment, lag 2			−0.31**		−0.41**
			(−5.55)		(−11.65)
unemployment, lag 3			−0.08*		
			(−1.92)		
employment protection	0.15	−0.04	0.04	**−0.47****	**−0.38****
	(0.91)	(−0.19)	(0.20)	(−3.42)	(−1.97)
benefit repl. Ratio	2.21**	2.47**	1.80**	**0.26**	**0.27**
	(5.44)	(5.63)	(4.04)	(0.64)	(0.61)
benefit duration	0.47**	**0.38**	**0.38**	**−1.35****	**−1.25****
	(2.49)	(1.54)	(1.52)	(−4.08)	(−3.41)
*ben. dur.*ben. repl.*	3.75**	4.35**	3.18**	4.41**	4.12**
	(3.97)	(4.24)	(3.19)	(4.57)	(3.89)

(*continued*)

Table 1: (Continued)

Independent variables	Original estimates, by Nickell *et al.* (2005)[a,b]	Our alternative estimates (four versions)			
		$I \rightarrow F$GLS[c]	+Lags[b]	ΔInteraction[b,d]	(Combi)[c,d,e]
Δ*union density*	6.99**	**3.23**	**2.65**	7.52**	**2.14**
	(3.17)	(1.42)	(1.34)	(3.34)	(0.96)
Coordination	−1.01**	−0.95**	**−0.12**	**1.05**	**1.04**
	(−3.54)	(−3.23)	(−0.36)	(1.61)	(1.44)
*coord.*union density*	−6.98**	−5.92**	−4.00**	**0.01**	**0.07**
	(−6.12)	(−4.80)	(−4.00)	(0.03)	(0.20)
tot. empl. tax rate	1.51*	2.18**	**1.10**	10.70**	6.79**
	(1.72)	(2.27)	(1.18)	(4.01)	(2.34)
*coord.*tot. empl. Tax*	−3.46**	−2.84**	**−1.81**	−4.65**	−2.69**
	(−3.29)	(−2.41)	(−1.64)	(−4.27)	(−2.25)
time dummies	Yes	Yes	Yes	Yes	Yes
country dummies	Yes	Yes	Yes	Yes	Yes
country-specific trends	Yes	Yes	Yes	Yes	Yes

(continued)

Table 1: (Continued)

Independent variables	Original estimates, by Nickell *et al.* (2005)[a,b]	Our alternative estimates (four versions)			
		$I \rightarrow FGLS$[c]	+Lags[b]	ΔInteraction[b,d]	(Combi)[c,d,e]
N	20	20	20	20	20
NT	600	600	600	600	600
ρ_1	0.38**		0.14**		**0.07**
	(9.52)		(3.18)		(1.63)

Notes:
(a) Authors' reproduction of original estimates by Nickell *et al.* (2005);
(b) Estimated using the *Iterated* GLS procedure c.f. Table (5), column (1) in Nickell *et al.* (2005). Stata-command XTGLS (...), p(hetero) corr (psar1) rhotype(theil), igls;
(c) Same model, but performing FGLS instead of IGLS. Stata-command: XTGLS (...), p(hetero) corr (psar1) rhotype(theil);
(d) In this specification, the interaction variables are defined as: *interaction* $(x_{1,it}, x_{2,it}) = (x_{1,it} * x_{2,it})$, instead of the original *interaction* $(x_{1,it}, x_{2,it}) = (x_{1,it} - \bar{x}_{1,i.})(x_{2,it} - \bar{x}_{2,i.})$;
(e) In this model, only lags 1 and 2 of the dependent are significant and are, therefore, included in the estimate (i.e. lag 3 is insignificant and omitted);
—**=1% significance and *=5% significance level (no coefficient is significant at 10% level);
—ρ_1 denotes the coefficient for first-order auto-correlation in the residuals of the regressions;
—Because of space considerations, the coefficients for the shock variables are not reported. When interesting, they are mentioned in the text. Dependent variable: Unemployment rates; *t*- and *z*-values in parenthesis; values in **bold** indicate that there has been a change of sign and/or significance when compared to the original Nickell *et al.* (2005) estimate in column 1.

interaction of *Coordination*Total Employment Tax Rate* all have become insignificant. It is also interesting to note that the coefficient of the interaction term of *Coordination*Union Density* while remaining significant declines (from −6.98 to −4.00). The money supply shock (not in the table) has become significant. Finally, the problem of auto-correlation in the residuals (resulting in biased coefficients) has decreased, with an order of magnitude of 2.5. The significance of the auto-correlation also decreases considerably.

Turning to the Nickell-bias, Nickell *et al.* (2005) do not worry about this, reasoning that the number of countries in the dataset (N) is fairly small compared to the timespan (T). Following a hint by one of our referees, we nonetheless try to handle this bias problem, using dynamic bias-corrected estimators proposed by Bruno (2005a, 2005b). These estimates are documented in Table A1. Indeed, the Bruno estimator leads to shifts in a few coefficients. Applying the Bruno estimator, between three and eight of the labor market institution variables change either sign or significance when compared to the original Nickell *et al.* estimate, depending on the specific method to generate the initial values of the estimation. We conclude that there remains a serious robustness problem.

2.3. Problem Three: Sensitivity to a More Intuitive Definition of the Interaction Terms

Nickell *et al.* (2005) define interaction terms as follows (p. 14, Table 5): "all variables in the interaction terms are expressed as deviations from the sample means." By sample mean, they denote the mean for a specific country.[3] Calculating the interaction variables after subtracting the country-mean implies that the country-mean over the years 1960–1995 of an interacted variable explains unemployment in for instance 1971. Hence, in the Nickell *et al.* (2005) model, the interacted variables in the year 1995 explain unemployment in the year 1971. In general, their subtraction of

3 This definition is not stated in the original article. We found it through trial and error. First, we calculated the interaction variables using various definitions. If the World mean is used to de-mean both variables interacted, a low correlation with the interaction variable used by Nickell *et al.* (2005) is obtained. If the country-specific means are used, the correlation with the original demeaned variables is above 99% for all interaction terms except union density interacted with coordination. For the latter, the correlation is above 90%. This may be because the union density variable provided in the original database is rounded. Furthermore, comparing two regressions using the Nickell *et al.* (2005) specification and altering only the two highly correlated interaction terms, did not alter the results of the regression.

country-means in the variables involved in the interaction implies that future values of interaction variables explain past unemployment.

Formally, Nickell *et al.* (2005) calculate an interaction term of a pair of variables (x_1, x_2) for a specific country i and time t as: *interaction* $(x_{1,it}, x_{2,it}) = (x_{1,it} - \bar{x}_{1,i.})(x_{2,it} - \bar{x}_{2,i.})$, where $\bar{x}_{1,i.}$ and $\bar{x}_{2,i.}$ denote the average value over time for a specific country i. By definition, $\bar{x}_{n,i.} = (x_{n,i1} + \cdots + x_{n,i\mathrm{T}})/\mathrm{T}; n \in (1,2)$ and T denotes the time-span. Filling in this definition in the expression for the interaction terms, we obtain: *interaction* $(x_{1,it}, x_{2,it}) = (x_{1,it} - (x_{1,i1} + \cdots + x_{1,i\mathrm{T}})/\mathrm{T})(x_{2,it} - (x_{2,i1} + \cdots + x_{2,i\mathrm{T}})/\mathrm{T})$.

As, according to the Nickell *et al.* regression model, $u_{it} = f(\textit{interaction}\ (x_{1,it}, x_{2,it}))$, we obtain $u_{it} = f(\textit{interaction}\ (x_{1,it}, x_{2,it})) = f(x_{n,i1}; \ldots; x_{n,i\mathrm{T}})$. As $\mathrm{T} \geq t$, this expression implies that unemployment at time t depends on *future* values of the interacted variables (x_1, x_2). This is obviously inadequate.

If the interaction terms are modeled more intuitively, e.g. with the simple multiplication of both variables concerned (i.e. *interaction* $(x_{1,it}, x_{2,it}) = (x_{1,it} * x_{2,it})$), such problems are avoided. An additional benefit is that we do not need to know the position of a country relative to its average to be able to interpret the sign of the coefficient of the interaction variables in terms of a change in the size of the interacted variables. More importantly, one can observe from Table 1, column "ΔInteraction" that there are substantial differences between the Nickell *et al.* (2005) estimate and that with our more plausible definition of the interaction variables: *Employment Protection Legislation* turns negative and becomes significant—i.e. it *reduces* unemployment, while NAIRU theorists would expect the opposite. The *Benefit Duration* originally had a highly significant *positive* value. It now turns *negative* with a high significance level. The *Benefit Replacement Ratio* becomes insignificant. These three changes are strongly conflicting with the intuition from NAIRU theory. *Bargaining Coordination* turns insignificant, as does the interaction of *Bargaining Coordination*Union Density*. *Total Employment Tax Rate* now has a far greater impact, increasing unemployment rates.

2.4. A Combination of the Three Modifications

The modifications treated above generate considerable changes of the results obtained by Nickell *et al.* (2005), if implemented in isolation from each other. It is interesting to see what happens to their estimates if all three adaptations are implemented simultaneously. This is shown in the last column of Table 1.

Comparing the column labeled "Nickell *et al.* 2005" with the one labeled "Combi", we observe again that several coefficients of labor market institutions change sign or become (in)significant. NAIRU theory suggests that labor market "rigidities" such as *Employment Protection*, *Benefit Duration*, *Benefit Replacement Ratio's,* or *Union Density* would increase unemployment rates. This is not the case, however, when our three modifications are implemented in combination. In the latter case, *Employment Protection* and *Benefit Duration* even reduce unemployment rates. In fact, the positive coefficients of the interacted *Benefit Duration*Benefit Replacement Ratio* and of the *Total Employment Tax Rate* are the only supports of NAIRU theory that remain significant in both estimates. Finally, *Coordination of Bargaining* seems to reduce unemployment in the Nickell *et al.* estimates but turns out insignificant in our estimate. So does the interaction *Union Density*Coordination of Bargaining*. On the other hand, *Total Employment Tax Rates* turn from weakly to highly significant, together with the money supply shock.

If we look at the problem of first-order auto-correlation in the residuals (which would result in biased estimates due to the inclusion of lagged dependent variables in the regression), we see that it has almost vanished. The coefficient of auto-correlation reduces from 0.38 to 0.07 and becomes (just) insignificant at the 10% level.

Comparing the original Nickell *et al.* estimate to the estimate using the allegedly more reliable Bruno approach (Table A1), differences are less or more pronounced, depending on the specific estimation technique used to generate the initial values. One can conclude, however, that several findings from the Bruno estimate are not comfortable to NAIRU theorists. For example, the Bruno estimate too, finds no positive impact of *Employment Protection* on unemployment rates, regardless of how the initial values are estimated. *Benefit Duration* rather than increasing unemployment, as in the Nickell *et al.* estimate, turns out insignificant in the Bruno estimate and the same holds for *Union Density* (regardless of the initial values procedure). The only comfort for NAIRU advocates comes from the positive coefficients (in two out of three versions) of *Benefit Replacement Ratio's* and the interacted variable *Coordination of Bargaining*Union Density*. *Coordination of Bargaining* interacted with *Total Employment Tax Rates* no longer reduces unemployment but becomes insignificant in two of the three versions. Strangely enough, *Total employment tax rate* shows a higher coefficient in the Bruno estimate, but is no longer significant, regardless of how the initial values are estimated.

3. CONCLUSIONS

Thoroughly re-examining the most influential article in a larger literature, we conclude that its empirical support for NAIRU theory is far weaker than expected. The estimates by Nickell *et al.* (2005) suffer from lack of robustness when subjected to minor changes in specification—and the latter are defendable from an econometric and/or economic point of view. It should be noted that we did not change the database, while it seems obvious that e.g. adding the most recent (US) unemployment rates would further undermine the NAIRU view. This causes doubts about policy recommendations in favor of deregulation of labor markets. While Nickell *et al.* concluded from their model that "...broad movements in unemployment across the OECD can be explained by shifts in labor market institutions" (p. 22), we tend to slightly but decisively change their key conclusion: Nickell *et al.* hardly prove that broad movements in unemployment across the OECD could be explained by shifts in labor market institutions.

Labor market reforms inspired by NAIRU theory are intended to discipline labor and tend to result in a more unequal income distribution (Baker *et al.* 2005; Palma 2009). Moreover, recent research suggests that more flexible labor relations (or "low road" HRM practices) have a negative impact on innovation (Michie and Sheehan 2003; Zhou *et al.* 2011) and on labor productivity growth at macro-level (Storm and Naastepad 2009a, 2009b; Vergeer and Kleinknecht 2011) as well as at firm-level (Kleinknecht *et al.* 2006; Lucidi and Kleinknecht 2010).

Accepting the Nickell *et al.* findings, one could have argued that the immediate costs for workers as well as the loss of innovative dynamism and lower gains in labor productivity growth are the price we might be ready to pay, as greater labor market flexibility would bring down unemployment rates. The results above and notably the outcomes from the Bruno estimates (Table A1) are not providing a base for such an argument. Of course, they neither support strong claims in the opposite direction ("Rigid labor markets *reduce* unemployment rates"). Nonetheless, we conclude that the claim "Rigid labor markets *increase* unemployment rates" can be severely doubted. Other than expected by NAIRU theorists in particular and neoclassical economists in general, the above suggests that it remains at least uncertain that more flexible labor markets would help unemployed people.

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APPENDIX: RESULTS OF ROBUSTNESS TESTS OF THE ESTIMATES BY NICKELL *ET AL.* (2005)

Table A1: Results of three possible *Least Squares Dummy Variable Bias Corrections* (LSDVC) as suggested by Bruno (2005a, 2005b)

Independent variables	Original estimates, by Nickell *et al.* (2005)[a,b]	Alternative estimates using LSDVC (three versions)		
		AH	AB	BB
unemployment, lag 1	0.86***	0.87***	0.91***	0.94***
	(48.49)	(25.42)	(32.73)	(35.71)
employment protection	0.15	−0.26	−0.27	−0.34
	(0.91)	(−0.15)	(−0.74)	(−0.80)
benefit repl. ratio	2.21***	**2.80**	2.60***	2.76***
	(5.44)	(0.92)	(4.01)	(4.21)
benefit duration	0.47***	**0.34**	**0.37**	**0.31**
	(2.49)	(0.16)	(0.81)	(0.67)
*ben. dur.*ben. repl.*	3.75***	**4.36**	3.85***	4.39***
	(3.97)	(0.88)	(3.57)	(3.89)
Δunion density	6.99***	**4.21**	**4.52**	**4.53**
	(3.17)	(0.24)	(1.17)	(1.09)
coordination	−1.01***	**−0.96**	−1.02***	−1.09***
	(−3.54)	(−0.49)	(−2.47)	(−2.39)
*coord.*union density*	−6.98***	**−4.44**	−3.84**	−3.38*
	(−6.12)	(−0.56)	(−2.16)	(−1.81)
tot. empl. tax rate	1.51**	**2.22**	**2.26**	**2.55**
	(1.72)	(0.24)	(1.34)	(1.42)
*coord.*tot. empl. tax*	−3.46***	**−1.83**	−1.59*	**−1.27**
	(−3.29)	(−0.36)	(−1.70)	(−1.26)
time dummies	Yes	Yes	Yes	Yes
country dummies	Yes	Yes	Yes	Yes

(*continued*)

Table A1: (Continued)

Independent variables	Original estimates, by Nickell *et al.* (2005)[a,b]	Alternative estimates using LSDVC (three versions)		
		AH	AB	BB
country-specific trends	Yes	Yes	Yes	Yes
N	20	20	20	20
NT	600	600	600	600

Notes: (a) Authors' reproduction of original estimates by Nickell *et al.* (2005);
(b) Estimated using the Iterated GLS procedure c.f. Table (5), column (1) in Nickell *et al.* (2005). Stata-command XTGLS (...), p(hetero) corr (psar1) rhotype(theil), igls;
—Column "AH" Estimated using the Stata-command: XTDLSVC (...), initial (AH) level(95) vcov(10);
—Column "AB" Estimated using the Stata-command: XTDLSVC (...), initial (AB) level(95) vcov(10);
—Column "BB" Estimated using the Stata-command: XTDLSVC (...), initial (BB) level(95) vcov(10);
—***=1% significance, **=5% significance level, and *=10% significance level;
—Because of space considerations, the coefficients for the shock variables are not reported. When interesting, they are mentioned in the text.
Initial estimates can be based on the procedure by Anderson and Hsiao (AH), Arellano and Bond (AB), and Blundell and Bond (BB).
Dependent variable: Unemployment rates; *z*-values in parenthesis; values in **bold** indicate that there has been a change of sign and/or significance when compared to the original Nickell *et al.* (2005) estimate in column 1.

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