# How reliable is pooled analysis in political economy?: the globalization-welfare state nexus revisited

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# How Reliable is Pooled Analysis in Political Economy? The Globalization–Welfare State Nexus Revisited

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## Abstract

Panel data analysis has become very popular in comparative political economy. However, in order to draw meaningful inferences from such data, one has to address specification and estimation issues carefully. This paper aims to demonstrate various pitfalls that typically occur in applied empirical work. To illustrate this, we refer to the debate on the globalization-welfare state nexus. We reexamine a model by Garrett and Mitchell (2001), a leading study in this regard. Utilizing a data set of 17 OECD countries and the time period 1961 to 1993, they find evidence that globalization and partisan composition have a significant impact on the extent of public activity. However, because they apply a dynamic specification in levels, they do not adequately take into account both the dynamic and spherical nature of the data. In contrast, we propose an autoregressive model in first differences that is shown to perform well in statistical terms. Further, we explicitly pay attention to the time pattern of the globalization-welfare state nexus. Substantively, we find evidence that government spending is primarily driven by the state of the domestic economy. Neither partisan effects nor the international economic environment have affected public expenditure considerably.

## Zusammenfassung

Panel-Daten erfreuen sich in politisch-ökonomischen Analysen zunehmender Beliebtheit. Allerdings enthalten derartige Daten einige ökonometrische Fallstricke, die wir in der vorliegenden Arbeit aufzeigen. Zur Illustration nehmen wir auf die Diskussion über den Zusammenhang zwischen Globalisierung und Wohlfahrtsstaat Bezug. Dazu greifen wir eine Arbeit von Garrett und Mitchell (2001) auf, in der gezeigt wird, dass Globalisierung und die parteimäßige Zusammensetzung der Regierung einen signifikanten Einfluss auf die Staatstätigkeit ausüben. Wir argumentieren, dass dieses Ergebnis von ihrer Modellspezifikation (dynamische Spezifikation in Niveaugrößen) getrieben wird. Demgegenüber zeigen wir, dass in der vorliegenden Datenkonstellation die statistischen Eigenschaften des Störterms ökonometrisch korrekt nur durch ein autoregressives Modell in ersten Differenzen berücksichtigt werden können. Unter Beachtung von unterschiedlichen Phasen der Internationalisierung finden wir weiters, dass die Staatsausgabentätigkeit primär durch binnenwirtschaftliche Faktoren erklärt wird. Weder Parteieneffekte noch „Globalisierungsphänomene“ haben die Veränderung der Staatsausgaben nennenswert beeinflusst.

## Contents

# 1 Introduction

"Regression models make it all too easy to substitute technique for work."
(David A. Freedman 1991: 300)

During the 1990s, advances in the statistical methodologies used for assessing hypotheses on the interrelations between economic developments and political institutions have been impressive. The availability of data for a set of usually about 15 to 25 OECD countries over a period of up to about 40 years has placed techniques of panel data analysis in the forefront of applied research. It is no exaggeration to say that it has become difficult to defend the decision not to use panel data in the analysis of comparative political economy.

At least, the importance of panel data analysis is well documented in recent studies on the relationship between globalization and the several dimensions of the welfare state. There are two conflicting views on this issue (Schulze / Ursprung 1999): The *efficiency hypothesis* states that the emerging internationalization of the economies induces a downward pressure on tax rates and government spending (e.g. Bretschger / Hettich 2001; Rodrik 1997, 1998; Swank / Steinmo 2001). In contrast, the *compensation hypothesis* (see Cameron 1978; Katzenstein 1985) claims that increasing factor mobility is associated with a higher demand on social security, which in turn causes an upward shift of taxation and spending levels (Garrett 1995, 1998; Hicks / Swank 1992; Huber / Stephens 2001; Quinn 1997; Swank 1998; ambivalent evidence is given by Garrett / Mitchell 2001). An emerging line of reasoning aims to overcome the simplified juxtaposition of efficiency and compensation by drawing attention to the complex interaction of various institutional determinants (e.g. Ganghof 2001), an advance attained at the price of losing quantitative information.

This paper is motivated out of the concern that the use of panel data analysis as a universal remedy for all the problems of cross-country comparative analysis has led the field into an impasse. In the debate on the globalization-welfare state nexus, the impasse appears at three levels:

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1. Little effort is made to develop a concise theory as to why specific variables are entered into a regression and how they are related to each other. Instead, we observe the discussion of singular, often ad hoc, propositions under the guise of hypotheses. Sometimes, this is associated with an unreflecting use of theoretical concepts (for a critique, see Moses 2001).

2. We observe empirical specifications that rarely discuss the assumptions about the spherical nature of the data. Specifically, numerous studies simply take for granted the finding of Beck and Katz (1995) that panel data tend to violate the classical assumptions of the error term (groupwise heteroskedasticity, cross-sectional and serial correlation) and regard autoregressive models with panel-corrected standard errors as a panacea. However, Beck and Katz (1995: 645; see also Greene 2000: 592–607) argue that it is necessary to test the assumptions on the error term *before* applying such correction, a suggestion that is often ignored in applied empirical work. Further, the problems induced by the time dimension of pooled data, notably due to nonstationary data, are often underestimated.

3. Consequently, the combination of weak theoretical reasoning and an ambivalent statistical foundation results in highly problematic conclusions.

This paper focuses exclusively on the second level, the *statistical modeling* of the globalization-welfare state nexus. Although we suspect that this is close to being the paramount problem in recent quantitative contributions to comparative political economy, we will not address the topic in a generalized manner. Instead, we will discuss the problem by reanalyzing a data set used in a recent paper by Geoffrey Garrett and Deborah Mitchell (2001). Utilizing panel data for 17 OECD countries and the time period 1961 to 1993,[1] they find evidence that globalization and the partisan composition of governments have a serious impact on revenue and expenditure levels. In developing this result, they claim credit for applying state-of-the-art econometric techniques.[2] However, their study – though stimulating in many respects – exemplifies very clearly the various pitfalls we may encounter when analyzing panel data.

1 Garrett and Mitchell (2001) claim to use 18 countries. However, due to missing values for New Zealand they actually base their inferences on 17 countries. Similarly, although the time period extends from 1961 to 1994, they only use data up to 1993.

2 This claim comes only implicitly in the paper (e.g., Garrett / Mitchell 2001: 153). In his widely celebrated earlier contribution, Garrett (1998: 10) claims to apply “the most appropriate econometric techniques to test the empirical merits of my arguments” and recommends the book as a means to overcome the ignorance of economists and policy makers about the work of political scientists, an ignorance that he feels is caused by the hitherto applied statistical methodology.

Our emphasis on model selection, specification and estimation issues implies that we do not directly address the question concerning which substantive variables should be included to explain the extent of government expenditure (see, for example, Mueller 1989: 320–347; or, more recently, Holsey / Borcherding 1997 for an overview). Consequently, we do not discuss the theoretical justification of the inclusion of particular variables but proceed from the variables included by Garrett and Mitchell (2001). Our contribution is to explore the statistical properties of different assumptions salient to panel data. By reassessing the empirical results of Garrett and Mitchell (2001), we demonstrate that model specifications widely used in the globalization literature typically suffer from misspecifications. Worse, the parameter estimates are usually biased, inefficient, as well as inconsistent, thereby placing the validity of the inferences drawn from these studies at stake.

The paper is organized as follows. Section 2 introduces a static panel data model and discusses the implications of controlling for cross-sectional and time-specific idiosyncrasies. Section 3 re-estimates the dynamic model specification of Garrett and Mitchell (2001) and demonstrates the major shortcomings therein. Section 4 shifts to a dynamic specification in first differences and discusses the justification of this model. Section 5 addresses issues of the country- and time-dependency of our findings. Section 6 presents our conclusions.

## 2 Static Model

We focus our discussion of model specification on one of the models presented by Garrett and Mitchell (2001). More specifically, we have chosen the 'total spending'-model in the first column of their Table 5 (Garrett / Mitchell 2001: 166), reprinted as the column indicated by "G / M" in our Table 5, on which they base their discussion of the relationship between total government expenditure (defined as a percentage of GDP) and the partisan composition of government as well as economic internationalization. While partisan composition is measured as the share of leftist (LEFT) and Christian democratic parties (CDEM) in government, the proxy for globalization consists of trade openness (imports plus exports related to GDP, TRADE), the share of imports from low-wage countries on total imports (LOWWAGE), and flows of foreign direct investment (FDI). In addition, Garrett and Mitchell introduce three control variables: the unemployment rate, GDP growth per capita, and the dependency ratio (defined as the share of citizens aged above 60 and below 19). Although the choice of the total spending model for our discussion seems somewhat arbitrary given the other models presented in their paper, two reasons make it a reasonable choice: First, total government expenditure is a crucial dependent variable in the substantive argument made by Garrett and Mitchell (2001), and second, the model has properties along which the problems of model specification in the pooled context can be exemplified rather clearly.[3] It should be noted, however, that the remaining specifications of Garrett and Mitchell (2001) not discussed here suffer from similar deficiencies. In order to motivate our modeling decisions, we proceed step by step.

We start by exploring the panel structure of the model. Without further testing we assume poolability of the data in the sense that the estimation of one coefficient over 'space' and 'time' for each variable is a reasonable approximation to reality. Since this is a rather far-reaching assumption, it needs some further attention (see Hsiao 1986: 11 on poolability tests). If this assumption does not hold – which often occurs in practice – we have to go back either to pure time series analysis and / or cross-sectional regressions (Hsiao / Sun 2000; Pesaran / Shin / Smith 1999) or to shift to the seemingly unrelated regressions (SUR) framework (Zellner 1962). A third option would be to move into the realm of random coefficients (Beck / Katz 2001a; Swamy 1971; Western 1998), which is an interesting alternative if we are not concerned about the nature of the cross sections. This is reasonable for individual-level analyses, e.g. in a typical household panel, where we wish to make inferences from a sample to a larger population. However, in a set of OECD countries, we are primarily interested in the country-specific situation. Moreover, the random coefficients approach assumes that the cross sections and the independent variables are uncorrelated, an assumption that is seldom fulfilled (see Hsiao 1986: 131). Hence, in the context of a small set of OECD countries, following one of the first two avenues seems to be the most valid conclusion if poolability tests fail. Since our aim is to discuss specification issues in the analysis of panel data in comparative political economy, we do not follow this lead here.

To decide to reject poolability means to sacrifice the feature that most comparative political economists regard as one of the prime advantages of panel data sets: the (often only apparent) increase in degrees of freedom obtained by repeating cross-sectional observations over time (see Hsiao 1986: 2). Since much effort has been put into the collection of panel data sets, and practically all published contributions to comparative political economy using panel data assume poolability by fiat, we simply note that this might be problematic and proceed to the next

3 A practical reason is that the data set made available on the web did not contain the data for covered interest rate differentials, which are used as additional proxy for globalization. Garrett and Mitchell (2001) refer to data compiled by Shepherd (1994). Unfortunately, this data set contains fewer observations than they have used for estimating their model for social expenditure, at least as far as can be inferred from the number of observations mentioned in Table 5 (see Garrett / Mitchell 2001: 166).

step, which is to impose common slopes but to allow for varying intercepts. Hence we test for the need to include country and time effects.

Including country effects (known as the fixed effects, least squares-dummy variables, or within estimator) captures the unobserved country-specific variation in a country-specific intercept. Since this removes the average country effect, such a model focuses on the within-country variation over time, and the coefficients represent a cross-country average of the longitudinal effect. Time effects, in contrast, capture developments over time that are common to all countries. This 'within-time' estimator eliminates any common trends and external shocks to which all countries are jointly exposed. The coefficient refers to the average cross-sectional effect over time and takes into account shifts over time in the position of the countries relative to each other. The variance of a coefficient estimate becomes smaller, the greater the persistency of the relative position of the countries becomes on both the dependent and the independent variable. Combining both country and time intercepts in a single specification results in a model from which all unobserved country- and time-specific effects are removed. What remains is a 'pure' effect, stripped of all exogenous noise and all variance components that are constant in either the time or cross-section dimension. Finally, like the within-time estimator, the 'between estimator' focuses on the cross-sectional dimension but first removes the within-country variation by averaging over time and then performs a cross-sectional regression on the country means.

Table 1 presents the various fixed-effects estimates and compares them to that of the pooled OLS specification. At this point, we assume a static model in levels and test whether the structure of the error term is adequately captured. Since panel data typically exhibit serial correlation, cross-sectional correlation, and groupwise heteroskedasticity (Greene 2000: 592–608), we expect to find such a structure in the residuals. The lower block of Table 1 reports the tests we have performed on the models. They suggest that we have to include both country and time effects and that the residuals do indeed reveal the perceived panel structure. Therefore, all of the significance tests in the upper block of Table 1 are invalid because the residuals do not conform to the OLS assumptions. Nevertheless, it is worthwhile to explore the coefficient estimates and their standard errors because they reveal a core problem of pooled analysis with political and institutional variables, which tend to be constant or which vary little over longer periods of time. This is the case for leftist and Christian democratic cabinet portfolios. If we ignore the bias in the standard errors, both coefficients are considerable in size and highly significant in the pooled specification. Holding all the other variables constant gives a 5.8 percentage point change in average total government expenditure when moving from a government with 0 percent to one with 100 percent leftist party participation. For Christian democratic parties this effect is smaller, yielding 2.9 percentage points. However, as soon as the country effects are in-

**Table 1 Static Specification in Levels**

| | Total Government Expenditure | | | | |
|---|---|---|---|---|---|
| | POOL | FE(C) | FE(T) | FE(CT) | FE(CT)/PW |
| Unemployment | 0.932*** (0.093) | 1.262*** (0.106) | 0.461*** (0.107) | 0.706*** (0.080) | 0.610*** (0.071) |
| GDP Growth/capita | –0.901*** (0.130) | –0.817*** (0.085) | –0.758*** (0.153) | –0.510*** (0.069) | –0.196*** (0.028) |
| Dependency ratio | –0.445*** (0.124) | –0.058 (0.143) | 0.451*** (0.160) | 1.329*** (0.111) | 0.930*** (0.148) |
| Left cabinet portfolios | 5.802*** (0.872) | –1.209* (0.699) | 5.867*** (0.836) | –1.261*** (0.459) | –0.199 (0.318) |
| Christian democratic portfolios | 2.876** (1.318) | –5.098*** (1.582) | 7.219*** (1.362) | –0.784 (1.049) | 0.302 (0.900) |
| Trade | 0.129*** (0.015) | 0.176*** (0.027) | 0.118*** (0.015) | 0.025 (0.020) | –0.021 (0.020) |
| Low wage imports | –0.157*** (0.048) | –0.061 (0.048) | –0.018 (0.049) | 0.338*** (0.035) | 0.107*** (0.034) |
| Foreign direct investment | 0.093 (0.216) | 0.420** (0.167) | –0.154 (0.232) | 0.142 (0.131) | –0.098 (0.079) |
| Rho | .. | .. | .. | .. | 0.800 |
| $R^2$ | 0.584 | 0.482 | 0.480 | 0.941 | .. |
| N (Observations) | 529 | 529 | 529 | 529 | 529 |
| k (Coeffficients) | 9 | 25 | 40 | 56 | 57 |
| F (Country effects) | .. | 54.41*** | .. | 146.66*** | 714.27*** |
| F (Time effects) | .. | .. | 2.77*** | 23.95*** | 498.45*** |
| F (Country & time effects) | .. | .. | .. | 60.76*** | 1088.18*** |
| LM (CC), $\chi^2$ (136) | 537.09*** | 582.92*** | 434.04*** | 487.03*** | 771.76*** |
| Mod. Wald (GH), $\chi^2$ (17) | 1745.20*** | 1131.27*** | 14479.24*** | 495.12*** | 1001.69*** |
| LM (AR1), $\chi^2$ (1) | 471.48*** | 408.86*** | 474.42*** | 365.72*** | 447.60*** |

*Notes:* POOL = simple OLS on pooled specification; FE(C) = fixed country effects; FE(T) = fixed time effects; FE(CT) = fixed country & time effects; PW = Prais-Winsten transformation; all models in this and the following Tables refer to 17 OECD countries and the period 1963–1993; constant and fixed effects not shown $\alpha \leq 0.10$; ** $\alpha \leq 0.05$; *** $\alpha \leq 0.01$; Rho = autocorrelation coefficient; $R^2$ = regression coefficient; N (observations) = total number of observations; k (coefficients) = number of estimated coefficients; F (Country effects) = F-Test for the inclusion of country dummies; F (Time effects) = F-Test for the inclusion of year dummies; F (Country & time effects) = F-Test for the inclusion of both country and year dummies; LM(AR1): Lagrange-multiplier test for first order residual serial correlation in panel data (Baltagi 2001: 95); degrees of freedom in parentheses; LM(CC): Breusch-Pagan LM test for cross-sectional correlation (Greene 2000: 601); Mod. Wald(GH): modified Wald test for groupwise heteroskedasticity (Greene 2000: 598); left cabinet portfolios and christian democratic cabinet portfolios are expressed as ratios instead of percentages in order to shift the coefficient estimates presented in the table.
.. = not applicable.

cluded, both coefficients become negative but remain significant. Substituting time effects for country effects turns the estimates back to positive coefficients that are now considerably larger in the Christian democratic case. Finally, including both effects makes the party coefficients both negative and now insignificant. Hence, the decision on how to model the fixed effects makes a real difference. Similar conclusions may be drawn for the dependency ratio, low-wage imports and foreign direct investment. Contrary to the party coefficients, the coefficients of unemployment, economic growth per capita, and trade are much more stable.

Here, we only discuss the party coefficients because they are the core variables in most of the globalization literature. Their sign and significance is decisive in the substantive interpretation of the salience of partisan effects that Garrett (1998) strongly advocates. Why is the sign of partisanship unstable? The answer is twofold: First, in some of the countries – Canada and the USA for leftist cabinet portfolios, and Australia, Canada, Ireland, Japan, and the UK for Christian democratic portfolios – these variables do not vary over time. This implies that these countries do not enter the partisan estimates in the FE(C) specification because the country effects remove all cross-sectional variation that otherwise would be captured by the partisanship variables.[4] Since Canada and the USA both combine no leftist government portfolios with low levels of government expenditure, their exclusion from the sample in practice changes the estimate of the partisan effect considerably. In addition, the partisanship variables have extremely small variation in a couple of other countries (e.g. Denmark, Finland, or Switzerland). Second, by focusing on the longitudinal dimension, the static model in levels assumes an immediate shift of government expenditure each time the partisan composition of the government changes. While this is somewhat implausible, notably due to the persistency of public expenditure, it is not surprising that the significance of our estimates is rather low. Since the time effects – FE(T) – focus on the cross-sectional dimension, the differences in total government expenditure between the countries are captured without interference of the general upward trend (which increases the variance of the coefficient estimates in the POOL specification), leading to more pronounced effects. The FE(CT) specification combines both effects on the coefficient estimates, yielding a weighted average of both FE specifications in which the weight depends on the relative variation in the two dimensions (see Baltagi 2001: 31–33). Hence, from a substantive perspective, the size and the sign of the coefficient estimates in the FE(CT) specification depend on the practical exclusion of particular countries.[5] We will come back to this issue when discussing the dynamic specification (see Section 4).

4 This is the way Garrett and Mitchell (2001: 168–169) interpret their exploration of the impact of the fixed effects. We will come back to this later.

5 Similarly, Maddala (1999: 434) concludes: “Thus, choosing the countries used in the

**Table 2 Static Specification in Levels: Between Estimator**

| | Total Government Expenditure as a % of GDP | |
|---|---|---|
| | 1 | 2 |
| Unemployment | 1.673<br>(1.100) | 1.770**<br>(0.678) |
| GDP/capita growth | 0.978<br>(2.711) | – |
| Dependency ratio | –0.355<br>(1.346) | – |
| Left cabinet portfolios | 23.445**<br>(9.651) | 25.484***<br>(7.078) |
| Christian democratic portfolios | 5.362<br>(10.080) | 11.378*<br>(5.992) |
| Trade | 0.083<br>(0.115) | – |
| Low wage imports | –0.029<br>(0.382) | – |
| Foreign direct investment | 0.915<br>(2.478) | – |
| $R^2$ | 0.665 | 0.573 |
| Nobs | 17 | 17 |
| k (coefficients) | 9 | 4 |
| White, $\chi^2$ (9) | .. | 6.40 |

*Notes:* see Table 1.
White: White (1980) general test for heteroskedasticity.
Constant included but not reported.

At this point it is useful to look at the results obtained from the between estimator. Since this is a pure, cross-sectional regression on the country means, we do not have to bother about the panel structure. Column 1 of Table 2 reports the results of the between regression. Despite the low degrees of freedom we find a highly significant 23 percentage point difference between 0 percent and 100 percent leftist portfolios. The model captures 66 percent of the variation in the dependent variable. Since most coefficients are far from being significant, we reestimate a restricted specification including only the partisan effects and unemployment, which failed the 10-percent significance level only by a narrow margin. In the restricted specification, the coefficient of Christian democratic portfolios

panel study is as crucial as (if not more than) the choice of the estimation method.”

becomes highly significant, too. Since the White (1980) test does not suggest the presence of heteroskedasticity, we have decided to stick with this specification.

In contrast to the between estimator, the FE-specifications cannot clearly confirm the presence of partisan effects. At this point, Garrett and Mitchell (2001: 169) decide to interpret the country intercepts as an 'equilibrium' level of spending. We see little reason to object to this approach if a substantive meaning can be attributed to the intercepts. This is the case if they can be read as long-term, steady-state solutions. For example, Daveri and Tabellini (2000: 72) interpret the intercepts in their unemployment model as the country-specific equilibrium unemployment in a NAIRU framework. In the present case, such an interpretation seems to be inconsistent because Garrett and Mitchell (2001) do not provide a theoretical model from which a steady state of government expenditure could be inferred.

In a less strict line of reasoning, one might claim that the country intercepts simply produce the average of the dependent variable as it is conditional on the other explanatory variables. However, what does the average indicate if the variable is nonstationary, as we will show below? And even if there is a reasonable answer to this question, how do we deal with changes in government composition when interpreting the intercepts? Finally, since the fitted country average is, as Garrett and Mitchell (2001: 165) themselves note, the net of the effect of all regressors, the average level cannot be attributed to partisan effects because these are already included in the model.[6]

An alternative approach is to estimate a random effects model, which is presented in Table 3. This estimator is a weighted average of the within and the between estimators and is based on two (related) assumptions: First, the intercepts are of no substantive relevance, which is the case in a large N, small T panel, and second, the fixed effects and the regressors are uncorrelated (see Baltagi 2001: 15). Although we could already reject random effects *a priori* because of the small number of cross sections, the Hausman (1978) test for random country effects specification – RE(C) – gives additional evidence that the fixed effects specification is preferable.[7] Hence we reject the random effects solution both on substantive and on statistical grounds.

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6 Garrett and Mitchell (2001: 163) emphasize that country effects should be included in order to capture idiosyncrasies and that any time-constant variable should be regarded as being part of the "underlying historic fabric of a country." As Beck and Katz (2001b) note (in the context of binary dependent variables), this argument throws out the baby with the bath water, because one of the main interests of political economists in this kind of quantitative analysis is determining whether institutional variables capture cross-sectional variation to an extent that makes the inclusion of country dummies unnecessary. Hence the ultimate aim is a model in which country effects are jointly insignificant.

7 Note that in the case of the random time effects specification – RE(T) – the estimator

**Table 3 Static Specification in Levels: Random Effects**

| | Total Government Expenditure as a % of GDP | |
|---|---|---|
| | RE(C) | RE(T) |
| Unemployment | 1.239*** | 0.932*** |
| | (0.102) | (0.093) |
| GDP/capita growth | –0.820*** | –0.901*** |
| | (0.085) | (0.130) |
| Dependency ratio | –0.095 | –0.445*** |
| | (0.139) | (0.124) |
| Left cabinet portfolios | –0.975 | 5.802*** |
| | (0.693) | (0.872) |
| Christian democratic portfolios | –4.655*** | 0.029*** |
| | (1.532) | (0.013) |
| Trade | 0.174*** | 0.129*** |
| | (0.025) | (0.015) |
| Low wage imports | –0.069 | –0.157*** |
| | (0.048) | (0.048) |
| Foreign direct investment | 0.400** | 0.093 |
| | (0.166) | (0.216) |
| $R^2$ | 0.490 | 0.584 |
| Median Theta | 0.882 | 0.000 |
| Nobs | 529 | 529 |
| Estimated coefficients | 10 | 10 |
| Breusch-Pagan | 1763.80*** | 0.25 |
| Hausman | 131.41*** | 80.88*** |

*Notes:* see Table 1.
RE(C) = random country effects; RE(T) = random time effects.
Constant included but not reported.
Breusch-Pagan = Breusch-Pagan test OLS vs random effects.
Hausman = Hausman test random vs. fixed effects.

From this it follows that we are confronted with a considerable problem with regard to the relationship of partisanship and public spending: While the between estimator clearly indicates the presence of partisan effects on government expenditure, a pooled specification with both fixed country and time effects does not confirm this finding. The standard econometric solution would be to instrument

degenerates to the pooled estimator (as indicated by $\theta = 0.000$; for details see Baltagi 2001: 18).

the time invariant partisanship variables with the remaining independent variables (Hausman / Taylor 1981). However, since we are interested in the cross-sectional variation – as is typically the focus of comparative political economics – this solution is not viable here. An alternative would be to ignore the fixed country effects and to attempt to model the cross-sectional dimension via institutional variables that capture enough of the cross-sectional variation to make the inclusion of country effects insignificant. Unfortunately, such variables are usually not available.

## 3 Dynamic Specification in Levels

It is well known from the panel literature that autocorrelation in the residuals, as in the single time series case, causes seriously inefficient estimates. In principle, there are two ways to deal with this issue (Beck / Katz 1995, 1996). On the one hand, autocorrelation can be regarded as a nuisance in the residuals that has to be corrected. On the other hand, autocorrelation may indicate persistency in the dependent variable that can be captured by modeling an autoregressive process including a lagged dependent variable. The relationship between the two approaches can be seen by noting that the autoregressive model is a special case of the Cochrane-Orcutt transformation used for the autocorrelation-corrected model in which the coefficients of the lagged independent variables are restricted to zero.

To see this, consider a model with one exogenous regressor $x_{it}$ and an error term following a first-order autoregressive process (for simplicity we do not consider fixed effects)

$$y_{it} = \alpha + \beta x_{it} + \varepsilon_{it}, \text{ where} \tag{1}$$

$$\varepsilon_{it} = \rho\varepsilon_{i,t-1} + v_{it}$$

with $i$ denoting the i[th] country and $t$ the t[th] time period, respectively. The error term is $v_{it}$ ~ IID $(0,\sigma^2_v)$, as usual. The (one-step) Cochrane-Orcutt transformation of (1) gives

$$y_{it} - \rho y_{i,t-1} = \alpha(1-\rho) + \beta x_{it} - \rho\beta x_{i,t-1} + v_{it} \tag{2}$$

which is known as the autoregressive distributed lag (ARDL) model (Greene 2000: 724). If $\rho\beta = 0$, which, given $\rho \neq 0$, is the case if $\beta = 0$, (2) gives the first order autoregressive specification

$$y_{it} = \alpha^* + \rho y_{i,t-1} + \beta x_{it} + v_{it} \tag{3}$$

where $\alpha^* = \alpha(1-\rho)$. Equation (3) is the approach taken by Garrett and Mitchell (2001), as by most contributions to comparative political economy. However, this reasoning seems somewhat odd. If $\beta$ has to be zero in (2), then equation (2) also states that the effect of the level of $x_{it}$ on the partial adjustment process in $y_{it}$ (i.e. $y_{it}-\rho y_{i,t-1}$) has to be zero. In addition, equation (3) states that the effect of the level of $x$ on the level of $y$ must be zero. Further, $\beta$ has to be zero in (1), again suggesting that $x$ does not affect $y$. Otherwise, $\rho$ has to be zero, which implies that there is no autocorrelation in the residuals of (1) and a correction for autocorrelation is not necessary. Hence, (3) is a mutilated version of (2), which is based on a rather inconsistent restriction. Nevertheless, it often leads to plausible results and is widely used in practice because, as can be seen from our specification, (3) has a different substantive interpretation than (1) and (2). It refers to the effect of the level of the regressor on the partial adjustment process in the level of the dependent variable. Sometimes, it is possible to derive predictions for particular constellations in which adjustment processes in the endogenous variable depend on the levels of the regressors.

Before proceeding to the lagged dependent variable specification, we take a look at the first approach, which explicitly models an AR(1) process in the residuals by applying a Prais-Winston transformation. The result is reported in the last column of Table 1 (FE(CT) / PW, where PW refers to Prais-Winston). The estimate of the autocorrelation coefficient $\rho$ is quite large, 0.8, and compared to the FE(CT) specification, the coefficients are shuffled again, notably those of the partisanship variables.

Table 4 presents the findings for different dynamic specifications of the expenditure model, which differ from Table 1 by the inclusion of a lagged dependent variable. As in the static specification, the adequate model suggested by the F-tests on country and time effects is the two-way fixed effects specification FE(CT). This is the specification that Garrett and Mitchell (2001: 166) presented in the first column of their Table 5 (and reproduced in our Table 5),[8] except for the calculation of the standard errors that we will discuss later. For the moment, we will focus our discussion on the dynamic FE(CT) specification.

With respect to the partisan composition, the results may be interpreted as follows. From Table 1 we know that controlling for the country effects removes the

8 We were unable to reproduce their results exactly, although we used the data set published on Garrett's webpage. The most serious deviation is the change in sign of the FDI variable. One possible reason is that the procedure they used (xtpcse in STATA 6) may be slightly different from ours (xtpcse in STATA 7). However, we believe that our estimates are close enough to proceed with our analysis. Note that we shifted the partisanship effects by a factor 100 in order to obtain nicer table entries.

**Table 4 Dynamic Specification in Levels**

| | Total Government Expenditure as a % of GDP | | | | |
|---|---|---|---|---|---|
| | POOL | FE(C) | FE(T) | FE(CT) | FE(CT)[a] |
| Spending $_{t-1}$ | 0.978*** (0.008) | 0.935*** (0.012) | 1.002*** (0.008) | 0.914*** (0.019) | 0.941*** (0.018) |
| Unemployment | –0.079*** (0.019) | –0.001 (0.034) | –0.044** (0.020) | 0.021 (0.036) | |
| GDP/capita growth | –0.415*** (0.025) | –0.470*** (0.024) | –0.331*** (0.028) | –0.365*** (0.028) | |
| Dependency ratio | 0.058** (0.023) | 0.076* (0.040) | –0.051* (0.029) | 0.084 (0.053) | |
| Left cabinet portfolios | 0.154 (0.169) | –0.025 (0.197) | –0.047 (0.160) | –0.225 (0.190) | |
| Christian democratic portfolios | 0.361 (0.246) | –0.491 (0.449) | –0.210 (0.256) | –0.416 (0.432) | |
| Trade | 0.006* (0.003) | –0.007 (0.008) | 0.003 (0.003) | –0.026** (0.008) | |
| Low wage imports | 0.010 (0.009) | 0.008 (0.014) | –0.007 (0.009) | 0.027* (0.016) | |
| Foreign direct investment | –0.150*** (0.040) | –0.050 (0.047) | –0.109*** (0.042) | –0.007 (0.054) | |
| $R^2$ | 0.986 | 0.983 | 0.984 | 0.990 | 0.986 |
| N (Observations) | 529 | 529 | 529 | 529 | 529 |
| k (Coefficients) | 10 | 26 | 41 | 57 | 49 |
| F (Country effects) | .. | 6.01*** | .. | 4.81*** | 2.11*** |
| F (Time effects) | .. | .. | 3.57*** | 3.01*** | 7.61*** |
| F (Country and time effects) | .. | .. | .. | 4.28*** | 5.87*** |
| F (Substantive variables) | 38.13*** | | | 25.87*** | |
| LM (CC), $\chi^2$ (136) | 174.12** | 166.48** | 147.88 | 146.87 | 222.00*** |
| Mod. Wald (GH), $\chi^2$ (17) | 133.57*** | 287.83*** | 252.14*** | 372.04*** | 278.58*** |
| LM (AR1), $\chi^2$ (1) | 39.62*** | 13.63*** | 23.45*** | 12.01*** | 25.51*** |

*Notes:* see Table 1.
OLS = simple OLS; FE(C) = fixed country effects; FE(T) = fixed time effects; FE(CT) = fixed country & time effects; constant and fixed effects included but not reported; Breusch-Godfrey LM = Test for first order residual serial correlation.
.. = not applicable.
F (Substantive variables) = F-Test for the inclusion of all variables except the lagged dependent variable and the fixed effects.

positive association between leftist government participation and government expenditure. Now we add a dynamic element. According to this specification, the short-run impact of a shift from 0 percent to 100 percent leftist portfolios in government is to reduce government expenditure by (insignificant) 0.23 percentage points. In the long run, this shift amounts to a decrease of 0.225 / (1–0.914) = 2.62 percentage points. Thus, according to this model, leftist governments apparently were better able to cut back government expenditure than conservative or Christian democratic governments. This is at odds with the existing evidence of partisan effects on one important element of government expenditure – social expenditure – which either found positive effects or no significant ones (for an overview, see Kittel / Obinger 2002).

Let us explore the model in more detail. First, note that the tests for groupwise heteroskedasticity and cross-sectional correlation are highly significant. Hence it seems reasonable to follow the recommendation by Beck and Katz (1995: 638) to apply panel corrected standard errors. Table 5 reproduces the FE(CT) specification using weighted least squares (the first step of the Parks-Kmenta FGLS procedure) as well as OLS with panel-corrected standard errors (PCSE). For the sake of completeness, we also report the Prais-Winsten transformed specification – FE(CT) / PW – from Table 1 with PCSEs (PCSE / AR1). The difference between the OLS standard errors and the PCSE is minor. In effect, the PCSEs are somewhat more permissive than the OLS standard errors and somewhat less optimistic than the WLS standard errors. This is what we would expect given that the PCSEs make use of the panel structure. While this makes them less efficient, it also makes them – as Beck and Katz (1995) have stressed – more realistic than WLS.

We contend, however, that heteroskedasticity and cross-sectional correlation are not the real problems in these data. Beck and Katz (1995) stress that autocorrelation has to be handled adequately *before* their standard errors are calculated. In the models presented in Tables 4 and 5, two problems are apparent: According to the Breusch-Godfrey LM-test for serial correlation tabulated in the last rows of Tables 4 and 5, none of the specifications is free of autocorrelation. An auxiliary regression of the residuals on the lagged residuals still gives a highly significant autocorrelation of 0.148 and Durbin's m-test (Kmenta 1990: 333) gives a coefficient of 0.172, which is also highly significant. It is well known that in the presence of a lagged dependent variable, autocorrelation in the residuals prevents the OLS estimate from remaining unbiased and consistent (Greene 2000: 534; for applications see Achen 2000). In addition, inserting a lagged dependent variable in a model with fixed country effects induces an additional bias via the correlation between the lagged dependent variable and the individual effects. Since $y_{it}$ is a function of $\mu_i$, the country effects, $y_{i,t-1}$ is also a function of $\mu_i$ because $\mu_i$ is constant over time (Baltagi 2001: 129–130; Kiviet 1995; Nickell 1981). The bias increases with the magnitude of the autoregressive coefficient (for Monte Carlo evidence,

**Table 5 Dynamic Specification in Levels: Correction for Heteroskedasticity and Cross-sectional Correlation**

| | Total Government Expenditure as a % of GDP | | | |
|---|---|---|---|---|
| | WLS | PCSE | G/M | PCSE/AR1 |
| Spending $_{t-1}$ | 0.902***<br>(0.017) | 0.914***<br>(0.024) | 0.911*** | – |
| Unemployment | 0.061**<br>(0.031) | 0.021<br>(0.039) | 0.033 | 0.610***<br>(0.085) |
| GDP/capita growth | –0.367***<br>(0.025) | –0.365***<br>(0.032) | –0.362*** | –0.200***<br>(0.034) |
| Dependency ratio | 0.092**<br>(0.040) | 0.084*<br>(0.045) | 0.097** | 0.929***<br>(0.143) |
| Left cabinet portfolios[a] | –0.133<br>(0.172) | –0.225<br>(0.204) | –0.003 | –0.198<br>(0.381) |
| Christian democratic portfolios[a] | –0.534*<br>(0.319) | –0.416<br>(0.373) | –0.005 | 0.303<br>(0.776) |
| Trade | –0.029***<br>(0.008) | –0.026**<br>(0.011) | –0.030** | 0.021<br>(0.028) |
| Low wage imports | 0.036***<br>(0.013) | 0.027<br>(0.017) | 0.023 | 0.107***<br>(0.040) |
| Foreign direct investment | –0.013<br>(0.048) | –0.007<br>(0.057) | 0.006 | –0.098<br>(0.088) |
| N (Observations) | 529 | 529 | 529 | 529 |
| k (Coefficients) | 57 | 57 | 57 | 57 |
| Rho | .. | .. | – | 0.801 |
| LM (AR1), $\chi^2$ (1) | 12.94*** | 12.01*** | – | 447.65*** |

*Notes:* see Table 1.
WLS = Weighted Least Squares; PCSE = Panel-corrected Standard Errors; PCSE/AR1 = Panel-corrected Standard Errors in Prais-Winsten transformed Model.
G/M = Coefficient estimates reported by Garrett and Mitchell (2001: 166) in the first column of their Table 5 (Note: They did not report standard errors but claim to use panel-corrected standard errors for the significance tests designated by the stars).
Constant and fixed effects included but not reported.
.. = not applicable.
– = not reported.
a Our estimates and those reported by Garrett/Mitchell differ by a factor 100 because we have redefined their percentage values as ratios in order to improve readability.

see Judson / Owen 1999). Since the coefficient of the lagged dependent variable is above 0.9, this implies that we are confronted with a specification that is ridden with bias in *all* parameter estimates. Hence, the substantive interpretation of the coefficients and their standard errors is, as such, meaningless.

These considerations give strong evidence that we have to reject the autoregressive specification of Tables 3 and 4 and, by implication, that of Garrett and Mitchell (2001). If we were able to get rid of autocorrelation, we would proceed by an instrumental variable (IV) or a methods of moments (GMM) approach in order to deal with the correlation between the country effects and the lagged dependent variable (Baltagi 2001: 131–153). One approach that is sometimes suggested is to add a second lag in order to capture the remaining autocorrelation. However, the short-run and long-run relationships we are interested in are derived by the coefficient estimate and the first-order lagged dependent variable (for details see Greene 2000: 722). As the second-order lagged variable is superfluous for this calculation, there is no need to include this variable in the regression. Besides, it is hard to believe that a second-order lag would have any substantial meaning in our context.

A further problem relates to the extremely high autoregressive coefficient. As Achen (2000) has pointed out, in some situations this coefficient *artificially* 'inflates' all variation because of the extreme persistency in the data. The comparison of Table 1 and Table 4 reveals that we are confronted with such a situation. While the $R^2$ of the POOL specification in Table 1 is 0.58, the FE(CT) specification in Table 1 inflates this statistic to 0.94, and the lagged dependent variable further inflates it to 0.99. In addition, we have the finding (reported in Table 4) that the $R^2$ of a regression of government expenditure on lagged government expenditure – the models in Table 4 skimmed of all regressors except the lagged dependent variable – is 0.986. Hence, all regressors add only 0.4 percentage points (the corresponding F-statistic of 0.25 is clearly insignificant) to the regression coefficient in the presence of a lagged dependent variable. Nevertheless, the F-values reported for the substantive variables – F = 38.13 for POOL and F = 25.87 for FE(CT) – indicate that these variables jointly do significantly reduce the residuals sum of squares. Also, the coefficient of the lagged variable is 0.941 (0.018), which suggests that we might well be faced with a case of nonstationarity.[9]

A glance at the plots of expenditure to GDP ratio over time reveals that the analysis may indeed be hampered by nonstationarity, more specifically by a I(1)-stationary process (see Figures 1 and 2 in the Appendix). While the series of all countries tend to rise over time, some countries seem to follow a trend and others

9 We have also estimated a pooled model including no other variable than the lagged dependent, yielding an autoregression coefficient of 0.991 (s.e. = 0.007). Hence, including fixed effects does alleviate the problem of nonstationarity somewhat but does not seem to solve it, as the IPS-tests below show. Technically, an autoregressive coefficient above unity, as in the FE(T) specification of Table 4, violates the *invertability condition* that is needed to reassure stable solutions in a stochastic process (see Maddala / Kim 1998: 13). Consequently, the conventional unit roots tests turn out to be invalid. See Elliott / Stock (2001) for tests on autoregressive coefficients near one.

**Table 6 Panel Unit Roots Tests**

| | Total Government Expenditure: Levels | | | |
|---|---|---|---|---|
| *Levin & Lin* | coefficient | t-value | t* | p |
| constant | –0.113 | –6.104 | –0.871 | 0.192 |
| constant, trend | –0.170 | –5.835 | 3.184 | 0.999 |
| *Im, Pesaran & Shin* | t-bar | cv10 | $\psi$ | p |
| demeaned, no trend | –1.606 | –1.780 | –0.383 | 0.351 |
| demeaned, trend | –1.589 | –2.410 | 2.727 | 0.997 |
| not demeaned, no trend | –1.226 | –1.780 | 1.287 | 0.901 |
| not demeaned, trend | –1.733 | –2.410 | 2.056 | 0.980 |
| | Total Government Expenditure: Changes | | | |
| *Levin & Lin* | coefficient | t-value | t* | p |
| constant | –0.836 | –15.201 | –10.030 | 0.000 |
| constant, trend | –0.911 | –16.569 | –9.201 | 0.000 |
| *Im, Pesaran & Shin* | t-bar | cv10 | $\psi$ | p |
| demeaned, no trend | –3.684 | –1.780 | –9.498 | 0.000 |
| demeaned, trend | –3.921 | –2.410 | –8.162 | 0.000 |
| not demeaned, no trend | –3.580 | –1.780 | –9.044 | 0.000 |
| not demeaned, trend | –3.797 | –2.410 | –7.584 | 0.000 |

*Note:* These tests are performed on a restricted sample in order to meet the requirement of a balanced panel. We have dropped Switzerland and the years 1991–1993. Dropping Switzerland and Norway but retaining the years 1991–1993 did not change the results significantly, except for the not-demeaned IPS tests for the levels, for which the p-value of $\psi$ became clearly significant if the trend is excluded but clearly insignificant if the trend is included. These considerable changes due to small changes to the sample suggest that the power of these tests seems to be rather low.

Levin & Lin (1993) (LL) tests augmented by 1 lag, $H_0$: nonstationarity.
coefficient: Coefficient on lagged levels.
t-value: t-value of coef.
t*: transformed t-value, ~N(0,1).
p: p-value of t*.
Im-Pesaran-Shin (1997) (IPS) tests augmented by 1 lag, $H_0$: nonstationarity.
t-bar: mean of country-specific Dickey-Fuller tests.
cv10: 10% critical value of IPS test.
$\psi$: transformed t-bar statistic, ~N(0,1).
p: p-value of $\psi$.

tend to be better characterized by a random walk. This suggests that we should test for unit roots in a more systematic way. Table 6 presents the results of Levin-Lin (LL) tests and Im-Pesaran-Shin (IPS) tests for unit roots (for details, especially on the difference between these tests, see Maddala / Wu 1999; see also Baltagi 2001: 236–239). The upper panel refers to the levels. According to LL, the data appear nonstationary. The IPS tests clearly confirm this finding. Demeaning (i.e. removing the time effects) does not substantively alter the conclusions. Although these tests are notorious for their low power (see Maddala / Kim 1998: 137), we tend to be cautious and assume that the expenditure data are indeed nonstationary, given the behavior over time as exhibited in Figure 1. If this assumption is valid, this is an additional cause of bias and probably of spurious associations.

There are two approaches for dealing with nonstationary data. First, we could remain in the single equation context and take first differences in order to proceed with a dynamic specification in differences. Second, we could explore the possibility of cointegrating relationships. This, however, requires more theoretical elaboration of the expected long-run association between the potentially cointegrating variables. Since the literature on panel cointegration is still in its early stages of development (e.g. Pedroni 2000; Kao / Chiang 2000) and rather difficult for the applied researcher to access (see Smith 2000 for an overview), we leave this task to a future endeavor and focus on the shift to a model in first differences.[10]

## 4 Dynamic Specification in First Differences

A model in first differences focuses on systematic associations between the annual changes in the variables – i.e. the short-term effects – while removing all level variation (i.e. the long-run effect) from the data. As argued before, our decision in favor of this specification is *solely* motivated by the statistical properties of the underlying data. Note that our data are I(1), which justifies the choice of a lag length of 1 and, therefore, of first differences.

We do have second thoughts concerning the variables that refer to government composition. If we take the first difference of these variables, we lose the information about the strength of the leftist or Christian democracy in government and keep only the information about its change. This implies that we make inferences about the effect that the size of the change of, say, leftist party strength in government has on the size of the change in public expenditure. By implication, this approach assumes that a reduction in leftist party strength *directly* translates into a decline in public expenditure. Given our knowledge from qualitative research about the persistency and path-dependency of government programs (see, e.g. Pierson 1996), such an assumption is only plausible if we claim that governments can fully and immediately change the level of public expenditure at their

10 A more technical reason is that, in general, time series with $T \cong 30$ are too short for the estimation of reliable parameters in the cointegration framework (see Maddala / Wu 1999).

will. Instead, it is more reasonable to assume that parties in government can influence not the level of expenditure but the extent of expenditure growth during their term in office. Thus, we are faced with a serious problem in the initial level specification chosen by Garrett and Mitchell (2001), highlighted by the autoregression coefficient of near unity and the apparent nonstationarity of public expenditure: The level of public expenditure should not be related to the strength of leftist or Christian democratic parties in government during their term in office but to the *cumulative* share of these parties in past terms, as advocated by Huber and Stephens (2001). This variable, however, is nonstationary by definition in the sense of an ever-increasing and thus non-constant mean, which returns us to the problems discussed in section 3. But the share of these parties during a term is, also by definition, the first difference of this cumulative measure. Therefore, by shifting to this perspective, we move to a more plausible expectation about the potential impact of the composition of government on public expenditure. Consequently, we have decided to first-difference all variables in the Garrett / Mitchell (2001) specification, except for the two variables referring to the composition of government.

It remains an empirical question whether first-differencing fully removes the cross-country and time variation that is captured by the *fixed country and time effects*. On the one hand, if there are systematic cross-country differences in the annual expenditure growth, fixed country effects should capture enough variation to attain statistical significance in an F-test. On the other hand, if there are common developments in public expenditure, we would expect significant time effects. The latter may well be the case in OECD countries: while the 1960s witnessed a massive increase of the public sector in virtually all OECD countries, the 1980s were characterized by attempts at bringing that increase to a halt. Such policy reorientation – though to different degrees and with some variation in the exact timing for each country – are captured by time effects. Hence, in the models presented in Tables 7 to 11, all variables except the composition of government are first-differenced, and we still test for fixed effects.

The results of the first-difference specifications are presented in Tables 7 to 9. First, Table 7 reports the estimates of the fixed effects model, similar to Table 1, which accounts for the level information. A striking feature of the first-difference model is, however, that the estimates do not vary considerably, which suggests a robust empirical relationship. In all specifications we find a highly significant impact of our core economic variables. Changes in unemployment are positively associated with changes in government expenditure, while the opposite holds for economic growth. For example, in the specification with country effects (FE(C), first column), a change of the economic growth rate (per capita) by 1 percentage point is associated with a decrease in the change in government spending by 0.25 percentage points. Substantively, this indicates an income elasticity for public

**Table 7 Dynamic Specification in First Differences: Fixed Effects**

| | Δ Total Government Expenditure as a % of GDP | | |
|---|---|---|---|
| | FE(C) | FE(T) | FE(CT) |
| Δ Govt. Expenditure$_{t-1}$ | 0.231*** | 0.226*** | 0.188*** |
| | (0.045) | (0.047) | (0.048) |
| Δ Unemployment | 0.552*** | 0.401*** | 0.423*** |
| | (0.075) | (0.079) | (0.080) |
| Δ GDP/capita growth | –0.250*** | –0.213*** | –0.206*** |
| | (0.024) | (0.026) | (0.026) |
| Δ Dependency ratio | 0.503** | 0.436** | 0.452** |
| | (0.204) | (0.206) | (0.222) |
| Left cabinet portfolios | –0.147 | 0.040 | –0.249 |
| | (0.206) | (0.145) | (0.197) |
| Christian democratic portfolios | –0.744 | –0.069 | –0.629 |
| | (0.474) | (0.208) | (0.459) |
| Δ Trade | –0.015 | –0.027 | –0.023 |
| | (0.015) | (0.021) | (0.021) |
| Δ Low wage imports | –0.012 | 0.020 | 0.014 |
| | (0.033) | (0.035) | (0.035) |
| Δ Foreign direct investment | –0.224*** | –0.120* | –0.134* |
| | (0.072) | (0.071) | (0.072) |
| $R^2$ | 0.428 | 0.461 | 0.520 |
| N (Observations) | 512 | 512 | 512 |
| k (Coefficients) | 26 | 40 | 56 |
| F (Country effects) | 0.80 | .. | 1.01 |
| F (Time effects) | | 2.86*** | 2.92*** |
| F (Country & time effects) | .. | .. | 2.22*** |
| LM (CC), $\chi^2$ (136) | 190.81*** | 161.74* | 162.22* |
| Mod. Wald (GH), $\chi^2$ (17) | 337.03*** | 440.48*** | 362.37*** |
| LM (AR1), $\chi^2$ (1) | 2.03 | 1.32 | 1.30 |

*Notes:* see Table 1.
FE(C) = Fixed country effects; FE(T) = Fixed time effects; FE(CT) = Fixed country & time effects.
Constant and fixed effects included but not reported.
.. = not applicable.

goods of less than unity, a result that is in line with most empirical findings (see Mueller 1989: 324). The change in the dependency ratio turns out to be positively correlated with government growth, as expected, and statistically significant. In

**Table 8 Dynamic Specification in First Differences: Fixed Effects: Correction for Heteroskedasticity and Cross-sectional Correlation**

| | Δ Total Government Expenditure as a % of GDP | | |
|---|---|---|---|
| | WLS | White | PCSE |
| Δ Govt. Expenditure $_{t-1}$ | 0.266*** (0.044) | 0.226*** (0.055) | 0.226*** (0.068) |
| Δ Unemployment | 0.438*** (0.069) | 0.401*** (0.085) | 0.401*** (0.091) |
| Δ GDP/capita growth | –0.216*** (0.023) | –0.213*** (0.030) | –0.213*** (0.033) |
| Δ Dependency ratio | 0.348** (0.157) | 0.436** (0.184) | 0.436** (0.213) |
| Left cabinet portfolios | 0.154 (0.118) | 0.039 (0.145) | 0.039 (0.164) |
| Christian democratic portfolios | –0.042 (0.161) | –0.069 (0.205) | –0.069 (0.212) |
| Δ Trade | –0.031* (0.018) | –0.027 (0.024) | –0.027 (0.026) |
| Δ Low wage imports | 0.025 (0.028) | 0.020 (0.031) | 0.020 (0.038) |
| Δ Foreign direct investment | –0.130** (0.063) | –0.120 (0.100) | –0.120 (0.082) |
| LM (AR1), $\chi^2$ (1) | 4.49** | 1.32 | 1.32 |

*Notes:* see Table 1.
The models reproduce the FE(T) specification in Table 6. WLS refers to the GLS-transformed estimator taking into account groupwise heteroskedasticity (Greene 2000: 594–599), White refers to the OLS model with White's (1980) heteroskedasticity-robust standard errors and PCSE refers to OLS with Beck and Katz's (1995) panel-corrected standard errors.
Constant and fixed effects included but not reported.

contrast, apart from FDI, the political as well as the globalization variables are of limited importance for explaining government spending. Further, as shown in the F-tests on the significance of the country and time effects, we have to consider time effects in our model. Since the F-test on the country effects in both the FE(C) and the FE(CT) specifications indicates that the country effects are insignificant – which is not unexpected since taking first differences wipes out the cross-country effects – we end up with a model including time effects only – FE(T). Apart from the shift to the difference specification, which is necessary as pointed out previously, this constitutes a second major difference to the specification of Garrett and Mitchell (2001).

**Table 9 Dynamic Specification in First Differences: Random Effects**

| | Δ Total Government Expenditure as a % of GDP | |
|---|---|---|
| | RE(C) | RE(T) |
| Δ Govt. Expenditure $_{t-1}$ | 0.263*** | 0.249*** |
| | (0.044) | (0.045) |
| Δ Unemployment | 0.523*** | 0.476*** |
| | (0.073) | (0.074) |
| Δ GDP/capita growth | –0.259*** | –0.240*** |
| | (0.024) | (0.024) |
| Δ Dependency ratio | 0.450** | 0.440** |
| | (0.190) | (0.195) |
| Left cabinet portfolios | 0.103 | 0.075 |
| | (0.151) | (0.146) |
| Christian democratic portfolios | –0.114 | –0.096 |
| | (0.217) | (0.210) |
| Δ Trade | –0.016 | –0.021 |
| | (0.015) | (0.017) |
| Δ Low wage imports | –0.004 | 0.006 |
| | (0.033) | (0.033) |
| Δ Foreign direct investment | –0.209*** | –0.171** |
| | (0.072) | (0.071) |
| $R^2$ | 0.413 | 0.412 |
| Median Theta | 0.000 | 0.293 |
| Nobs | 512 | 512 |
| Breusch-Pagan | 1.23 | 23.67*** |
| Hausman | n.a.[a] | 25.86*** |

*Notes:* see Table 1.
RE(C) = Random country effects; RE(T) = Random time effects.
Constant included but not reported.
a Not applicable because random effects estimator has degenerated to pooled estimator.

Table 8 presents the estimates of the fixed time effects – FE(T) – model of Table 7, taking heteroskedasticity and cross-sectional correlation into account. First, we estimated the WLS-transformed model, then we used the White correction for the OLS standard errors, and finally we applied PCSEs as proposed by Beck and Katz (1995). The WLS standard errors are much lower than those of the PCSE and White-corrected ones. This finding is in line with Beck and Katz (1995), who have emphasized that WLS gives downward-biased standard errors. As outlined above, Beck and Katz (1995) have also stressed that in a correctly specified model, the standard errors of the OLS specification should not deviate considerably from the PCSE ones. This is exactly the case in our situation, which, in turn, is an indication that our model in Table 7 is correctly specified. Finally, Table 9 presents the results of the random effects model. We are interested in the question whether the time effects, which have little relevance for the substantive argument, can be represented by a distributional assumption. Again, by applying a Hausman (1978) test, we can reject the null of random effects and remain with the fixed time effects specification. Note that for the random country effects specification – RE(C) – $\theta$ collapses to zero. Thus, the random country effects specification is not different from the pooled OLS estimator because taking first differences removes cross-sectional variation.

To conclude, in the first-difference specification we find overwhelming evidence for a model with time effects. Most importantly, by testing for AR(1), we cannot find residual autocorrelation in our first-differenced data, suggesting that our dynamic specification does not suffer from endogeneity. Applying the panel-corrected standard errors in this situation, as elaborated in Table 7, seems acceptable. Differencing the data wipes out the country effects. Thus, the endogeneity bias caused by the correlation between the lagged dependent variable and the country effects vanishes (see Hsiao 1986: 75). In order to take into account the remaining endogeneity due to correlation of the lagged dependent variable and the error term, it is usually suggested to apply more consistent estimation procedures (such as IV or GMM; see Baltagi 2001: 131, Wawro 2000). However, Monte Carlo simulations have shown that for an unbalanced panel, as in our case, *and* T = 30, the FE performs just as well as or better than the alternatives (see Judson and Owen 1999: 13). This is an additional justification for our specification.

In all specifications, the coefficient of the lagged dependent variable is stable and around 0.25, suggesting a persistency of spending decisions equaling about 25 percent. If one accepts the view that current spending decisions depend on the previous year's budget, notably due to indisposable positions in the budgetary process) (e.g., wages and salaries of public employees), the magnitude of the lagged dependent variable seems plausible.

## 5 Robustness and Stability Analysis

By estimating a single coefficient for the whole period, Garrett and Mitchell (2001) assume a constant and stable globalization-welfare state nexus. However, the impact of both globalization and political pressures on government spending may well change over time. In addition, in applied work one is often confronted with structural breaks. Such breaks may be caused by economic shocks (e.g. the oil crisis in the 1970s) or simply by changing statistical conventions. In the data set of Garrett and Mitchell (2001), we suspect there are structural breaks especially for FDI, as is clearly shown in Figure 3 of the Appendix (notably for Belgium, the Netherlands, Sweden, and Switzerland). To account for these considerations, we estimate the first-differenced, fixed time-effects model of Table 8 with interaction dummies for three subperiods: 1964–1973 (up to the first oil crisis), 1974–1983 (characterized by growing unemployment and inflation as well as decreasing growth rates), and finally, 1984–1993, a period of stable economic growth and, according to conventional wisdom, the emergence of globalization. We interact these period dummies with our variables of interest, i.e. both the partisan composition of government and the globalization variables. The results are presented in Table 10.

Compared to our favored specification in Table 8 – FE(T) / PCSE – the coefficients of the core economic variables, unemployment, economic growth, and the dependency ratio, are almost unchanged. Most important, however, is that by applying the likelihood ratio (LR) tests on the (unrestricted) specification containing period-specific coefficients for the core variables, we get results indicating a rejection of the null of significant differences between the period-specific effects. In period-specific coefficients for the core variables, we get results indicating a rejection of the null of significant differences between the period-specific effects. In other words, the association between government expenditure and partisan composition as well as globalization remains largely stable over time. This finding is confirmed by the t-statistics of the coefficients: Again, our core economic variables are significant, whereas the only significant effects concerning partisanship and globalization are those of trade and FDI, both in the period of 1984–1993. It is worth noting that the coefficient on FDI reversed its sign between the late 1970s and the 1980s. From a statistical point of view, this may be due to structural breaks inherent in these series (see Figure 3 in the Appendix). From a substantive point of view, this may be indicative of an impact of economic internationalization: on average, an increase in foreign direct investment was associated with an increase in government spending during the late 1970s / early 1980s, which suggests that the compensation hypothesis was salient at that time. By contrast, the efficiency hypothesis tends to be confirmed for the period since the mid-1980s. Hence the efficiency-compensation debate might be resolved by the proposition that governments first attempted to compensate their citizens for greater exposure to the world market but later had to succumb to the pressures from growing deficits and debt as well as from economic internationalization. However, this is an interpretation based on rather weak findings that we do not wish to overemphasize.

**Table 10 Dynamic Specification in First Differences: Period-specific Slopes**

| | Δ Total Government Expenditure as a % of GDP | LR Test for period-specific slopes |
|---|---|---|
| Δ Government expenditure $_{t-1}$ | 0.230*** (0.047) | |
| Δ Unemployment | 0.412*** (0.080) | |
| Δ GDP/capita growth | –0.207*** (0.026) | |
| Δ Dependency ratio | 0.458** (0.223) | |
| Left cabinet portfolios 1963–1973 | 0.146 (0.250) | Left cabinet portfolios $\chi^2$ (2) = 1.15 |
| Left cabinet portfolios 1974–1983 | –0.178 (0.249) | |
| Left cabinet portfolios 1984–1993 | 0.133 (0.264) | |
| Christian democratic portfolios 1963–1973 | –0.178 (0.354) | Christian democratic portfolios $\chi^2$ (2) = 0.81 |
| Christian democratic portfolios 1974–1983 | 0.164 (0.389) | |
| Christian democratic portfolios 1984–1993 | –0.277 (0.364) | |
| Δ Trade 1963–1973 | 0.001 (0.053) | Δ Trade $\chi^2$ (2) = 1.82 |
| Δ Trade 1974–1983 | –0.010 (0.031) | |
| Δ Trade 1984–1993 | –0.060* (0.033) | |
| Δ Low wage imports 1963–1973 | 0.080 (0.061) | Δ Low wage imports $\chi^2$ (2) = 1.82 |
| Δ Low wage imports 1974–1983 | –0.029 (0.060) | |
| Δ Low wage imports 1984–1993 | 0.015 (0.061) | |
| Δ Foreign direct investments 1963–1973 | –0.062 (0.217) | ΔForeign direct investments $\chi^2$ (2) = 1.94 |
| Δ Foreign direct investments 1974–1983 | 0.139 (0.211) | |
| Δ Foreign direct investments 1984–1993 | –0.157* (0.083) | |
| $R^2$ | 0.512 | |
| Nobs | 512 | |
| k (coefficients) | 50 | |
| F (Time effects) | 2.47*** | |
| LR (Period-specific slopes), $\chi^2$ (10) | 9.09 | |
| LM (CC), $\chi^2$ () | 153.77 | |
| Mod. Wald (GH), $\chi^2$ () | 387.60*** | |
| LM (AR1), $\chi^2$ (1) | 1.52 | |

*Notes:* see Table 1; constant and fixed time effects included but not shown.

**Table 11 Jackknife Analysis**

| | Δ Total Government Expenditure as a % of GDP | | | | |
|---|---|---|---|---|---|
| | Minimum coefficient | Country excluded | Estimate | Maximum coefficient | Country excluded |
| Δ Government expenditure $_{t-1}$ | 0.213 | DNK | 0.226 | 0.245 | ITA |
| Δ Unemployment | 0.368 | CAN | 0.401 | 0.444 | IRL |
| Δ GDP/capita growth | –0.234 | IRL | –0.213 | –0.199 | ITA |
| Δ Dependency ratio | 0.397 | SWE | 0.436 | 0.501 | NLD |
| Left cabinet portfolios | –0.005 | FRA | 0.040 | 0.124 | SWE |
| Christian democratic portfolios | –0.143 | BEL | –0.069 | 0.024 | DNK |
| Δ Trade | –0.040 | ITA | –0.027 | 0.003 | IRL |
| Δ Low wage imports | 0.007 | NOR | 0.020 | 0.046 | AUT |
| Δ Foreign direct investment | –0.182 | NOR | –0.120 | –0.065 | SWE |

*Notes:* Entries are coefficient estimates from FE(T) in Table 6 together with minimum and maximum coefficient estimates resulting from re-estimates of the model while excluding each country one at a time. They show the responsiveness of the coefficient estimates to the inclusion of particular countries.

Finally, as emphasized in Section 2 (see footnote 6), our attention should be devoted to the robustness of our results against the impact of cross-sectional outliers. To discover such effects we perform a Jacknife analysis (Efron / Tibshirani 1993: Ch. 11) on our favored FE(T) / PCSE specification of Table 8. In particular, we re-estimate the model by excluding one country after the other. The resulting minimum and maximum values of the point estimates are presented in Table 11. The minimum values as well as the excluded country for which that estimate was obtained are shown in the first double column, and the maximum ones in the last double column. The point estimates of the entire sample (taken from Table 8) are reported in the middle column. A glance at the partisanship variables shows us that the coefficient estimates strongly depend on the countries included. The most extreme lower and upper deviations from the estimated coefficient are caused by the exclusion of France and Sweden for leftist governments and by the exclusion of Belgium and Germany for Christian democratic governments. The other coefficients are much more stable. None of them, except trade, changes sign due to the exclusion of a particular country. The most extreme coefficient estimates of the domestic socioeconomic factors are within approximately a 10-percent range of the coefficient estimate calculated by using the full set of countries. The economic variables relating to the external effects (trade, imports from low-wage countries and FDI) are less stable – deviations of 50 percent to 100 percent of the coefficient estimate are caused by the exclusion of a single country – which gives additional salience to the inference derived from the lack of significance that there is no clear effect. These findings clearly indicate that decisions on government expenditure are driven by the domestic socioeconomic environment rather than by political or globalization issues (as claimed by Garrett and Mitchell 2001).

## 6 Summary and Conclusions

It has become common practice in the empirical analysis of political economy to use panel data for drawing inferences on a wide range of research topics. In fact, data that combine cross-sectional and time-series information have given us new insights into the course of political processes. However, despite these merits, our paper calls for great caution in applying panel data in political economy. Exemplified by the controversy over the impact of globalization and the partisan composition of government on total government expenditure, we identify a lack of concern about a consistent empirical specification. In particular, we have re-estimated a model presented by Garrett and Mitchell (2001). Our findings may be summarized as follows:

1. In general, panel data inferences are sensitive to the model specification. In our context, the underlying data are highly persistent, and therefore the specification has to focus on the time pattern of the data.

2. Introducing a lagged dependent variable to get rid of autocorrelation causes additional problems, in particular if autocorrelation remains. Most importantly, the estimates become biased due to endogeneity. Our proposition for this particular model is to specify an autoregressive model in first differences with time effects, the latter taking into account common shocks. Testing for remaining violations of the panel assumptions, we end up with a statistically acceptable model that allows us to apply panel-corrected standard errors as proposed by Beck and Katz (1995), although these hardly differ from OLS standard errors, despite the fact that the tests for country-specific heteroskedasticity and cross-sectional correlation suggested the presence of such a residual structure. Apparently, these problems matter less in this particular model.

3. It is rather unlikely that social phenomena, such as the globalization-welfare state nexus considered by Garrett and Mitchell (2001) and subsequent studies, are stable over time. We take into account this argument by specifying a

> model with period-specific effects of the core variables, but we find no convincing evidence that the lack of statistically significant estimates in the model for the full period analyzed is due to systematic changes in the coefficients over time. However, the restricted number of observations from which each of these parameters is estimated does not allow us to make a definite judgement.

In sharp contrast to Garrett and Mitchell (2001), our empirical analysis indicates that neither globalization nor the partisan composition of government play a decisive role in explaining cross-country variation in the dynamics of government expenditure. Hence, we find a confirmation neither of the efficiency hypothesis nor of the compensation hypothesis. Instead, most parts of the dynamics in government expenditure are explained by the domestic economic environment such as growth, unemployment, or the dependency ratio (for a similar conclusion, see Castles 2001). Some of these may be affected by partisan politics or economic globalization, but that is a different story. Therefore, while we have focused on the statistical model used by Garrett and Mitchell (2001), much could be said about the lack of theoretical foundations in their empirical model and the way in which globalization interacts with politics to cause particular outcomes, among which is the development of government expenditure dynamics.

Substantively, the move from a model in levels to a model in first differences implies that the object of analysis has changed from long-term differences (i.e. the level of public expenditure) to short-term adjustments (i.e. the growth of public expenditure). We should not be too surprised that an analysis of short-run adjustments does not support propositions about long-run effects. Further, if we compare the results of a purely cross-sectional 'between model' in levels with that of our first-differenced specification, we can draw a second major conclusion: While the former indicates an unmistakably positive effect of leftist cabinet portfolios and a somewhat more ambiguous positive effect of Christian democratic portfolios on the levels of public expenditure, the latter clearly rejects any partisan effects on the first differences. However, this result is not necessarily a contradiction. Despite the shifting nature of cabinet portfolios, countries with a historically strong social democracy (e.g., Sweden, Denmark, Austria) or Christian democracy (e.g., Germany), or, conversely, a strong liberal tradition (e.g., USA, Canada), tend to have prolonged periods with high values on these variables during the period analyzed, too. Therefore, the country means of government composition in the between-model practically serve as a proxy for the historical power constellation. Since all countries exhibited an upward trend in public expenditure during the period analyzed, the differences in mean public expenditure are accounted for by the government composition variables. However, no systematic variation in growth is observable just because all countries experienced the upward trend.

Finally, what do our findings tell us about the usefulness of panel analysis for answering the type of questions asked by comparative political economists? We contend that those who are interested in the consequences of cross-country differences in political institutions – that by definition exhibit little short-term variation – should be much more aware of the longitudinal dimension of panel data. Neither persistency nor dynamics should be regarded as nuisances to be corrected. Quite the contrary: the longitudinal dimension of panel data makes this type of data ill-suited for answering research questions posed in a strictly cross-sectional context. The solution is not to start attaching a variety of bells and whistles to the model in order to correct autocorrelation or autoregression, but to pay much more attention to the short-term dynamics themselves. And if a question definitely refers to the cross-sectional dimension, it makes sense to answer that question by emphasizing the cross-sectional dimension.

Long-term variation, of course, is the result of systematic variation in short-term adjustments. This points to an important avenue for future research: the theoretical debate about partisan effects on short-term adjustments in government expenditure is notoriously underdeveloped and sometimes even dismissed as ill-guided (Huber / Stephens 2001: 36–38). However, if path-dependency has any substantive meaning in the context of government expenditure, it is that political actors usually change expenditure programs only at the margin. If one believes, for instance, that the partisan composition affects the long-run political outcome, it is caused by marginal changes in program designs. Therefore it is worthwhile to study not only the long-run developments but also the short-run political-economic processes. An important contribution in this regard would be the development of theories of short-term adjustments on which empirical analysis could be based.

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## Appendix

**Figure 1 Total Government Expenditure as a Percentage of GDP**

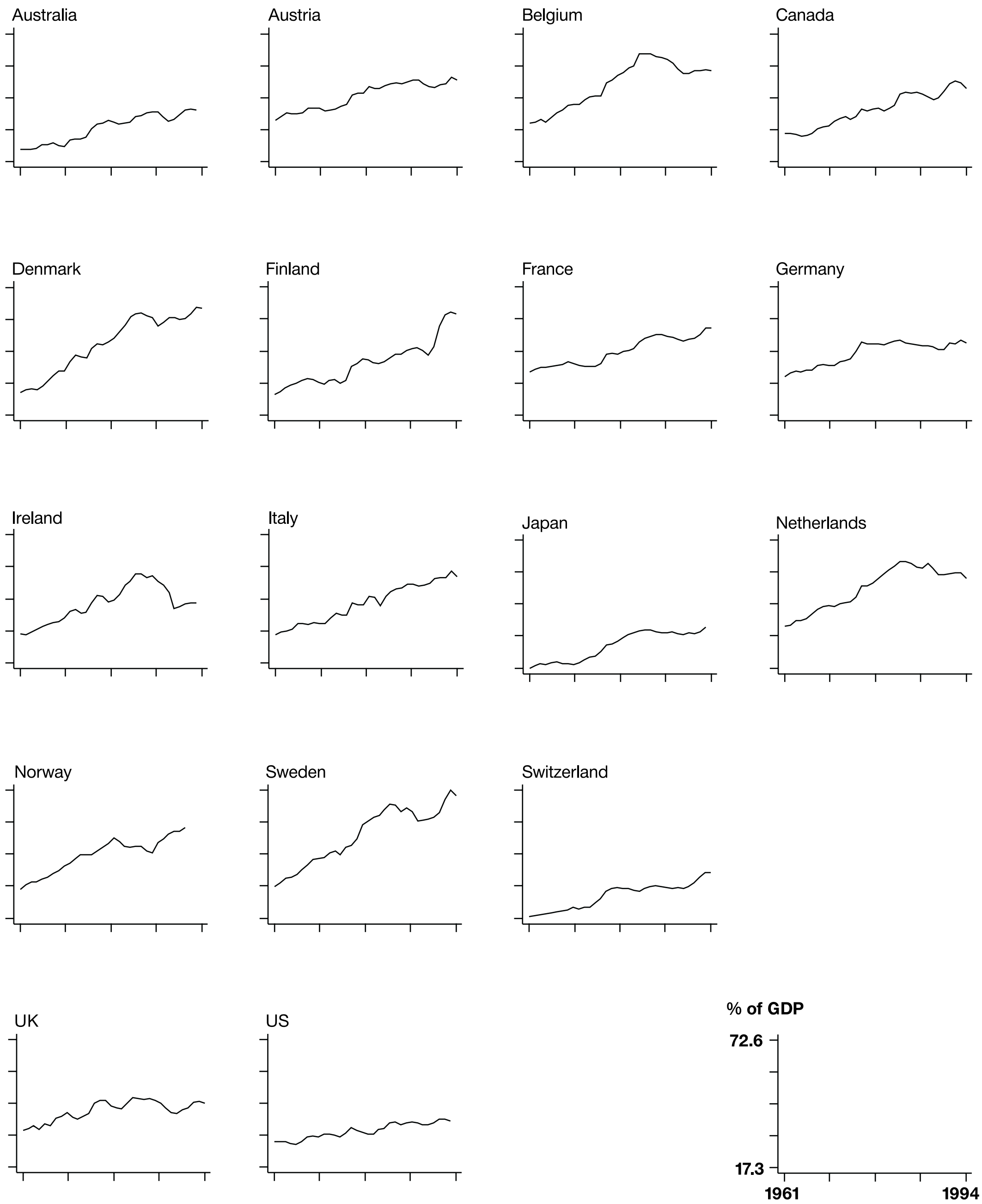

**Figure 2 First Differences of Total Government Expenditure as a Percentage of GDP**

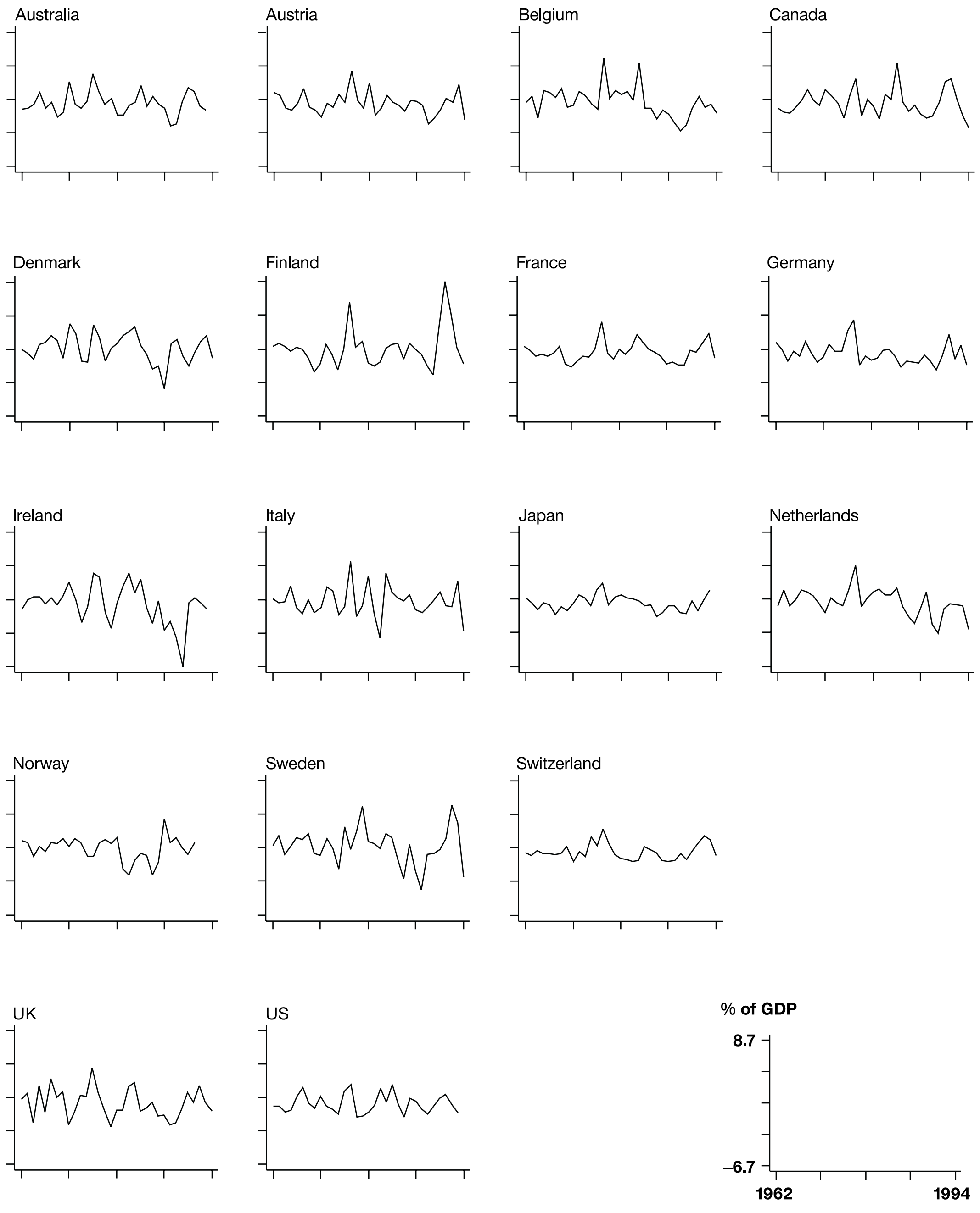

**Figure 3 Foreign Direct Investment**