Fiscal Policy Impacts on Growth in the OECD: Are They Long- or Short-Run?

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ABSTRACT

The literature testing for long-run impacts of fiscal policy on growth at the macro level has generally ignored short-run dynamics or treated them as homogeneous across counties. This paper explores fiscal-growth dynamics explicitly, focussing on OECD country experiences. It examines how robust previous 'long-run' results are to new empirical methods that model short-run dynamics and allow fiscal-growth responses to be heterogeneous both across countries and over time. Our results suggest that most of the 'long-run' growth effects of fiscal policy are typically achieved very quickly (within a few years). We also find that positive growth effects of fiscal changes in OCED countries have often been approximately counteracted by fiscal changes with negative growth effects. We test for the robustness and potential endogeneity of those fiscal-growth effects, and conclude that a more appropriate interpretation of the evidence is that fiscal policy effects on growth are *short*-run and significant, but that these are also *persistent,* provided the relevant fiscal policy changes are not reversed.

Introduction

Does fiscal policy have sustained impacts on economic growth? Since the mid-to-late 1990s new developments in both theory and empirics have challenged previous answers to this question. In particular, developments in endogenous and 'semi-endogenous' growth models (see, for example, Eicher and Turnovsky, 1999; Howitt, 2000; Dalgaard and Kreiner, 2003) yield results in which economic policy may only have transitional growth impacts, but convergence to equilibrium following policy shocks may be rapid or slow.[1] Turnovsky (2004), on the other hand, develops a neoclassical model in which transitional effects are very long-lasting. Simulations suggest these should be measured in decades not years. This increasing theoretical focus on the transitional dynamics of fiscal-growth responses has not yet been reflected in the empirical literature, which generally continues to look for long-run effects, whilst ignoring (or assuming away) the short-run transitional path, and treating all countries identically. Nevertheless, these studies increasingly find that various fiscal policy variables impact significantly on 'long-run' growth.

This paper explores fiscal-growth dynamics explicitly, focussing on OECD country experiences. It examines how robust previous 'long-run' results are to new empirical methods that model short-run dynamics and allow fiscal-growth responses to be heterogeneous both across countries and over time. Our results suggest that most of the previously identified 'long-run' growth effects of fiscal policy are typically achieved very quickly (within a few years). A more appropriate interpretation of the evidence would therefore appear to be that fiscal policy effects on growth are *short*-run and significant, but that these are also *persistent*, provided the relevant fiscal policy changes persist.

Testing for Fiscal-Growth Effects: The Story So Far

Recent developments in both neoclassical and endogenous growth models with a fiscal policy dimension have demonstrated that both are capable of predicting growth impacts from fiscal policy over a 'transitional period' of several decades. This increases the likelihood that currently available datasets (at least when several countries are the focus of attention) have an insufficient time dimension to discriminate between such models. Nevertheless the range of models now available has identified a number of fiscal variables hypothesised to impact on growth. Most of these, as in the early Barro (1990) model, rely on policies that generate distortions to private savings/investment decisions (including human capital investment and technological innovations) or affect the allocation of public spending between alternative uses, only some of which are growth enhancing. Thus the Turnovsky (2004) model, for example, hypothesises growth impacts associated with taxes on (from largest impact to smallest) capital, labour income and consumption respectively. Peretto (2004) elaborates a model in which there are growth benefits from switching away from corporate income and capital gains taxes, due to associated reallocations between productivity-enhancing and product-proliferating innovations. Likewise, Peretto (2003) shows that, in a semi-endogenous growth model, the level and composition of public expenditures affect transitional growth even though they have no steady-state effects. Capital and corporate income taxes do however have steady-state effects while labour income and consumption taxes do not.

Unfortunately many empirical studies examining fiscal effects on growth have been based only loosely on theoretical models, often testing *ad hoc* hypotheses relating to government size such as government consumption spending or some aggregate measure of tax burden. Not surprisingly, early results were ambiguous or contradictory and frequently non-robust (see Agell et al, 1997, and Myles, 2000 for reviews). Kneller et al (1999) argue that one reason for such apparently contradictory results is their failure to incorporate the government budget constraint formally into testing procedures. Empirical models which do control for the government budget have generally found more robust associations between fiscal policy and economic growth (Devarajan et al, 1996; Kocherlakota and Yi, 1997; Miller and Russek, 1997; de la Fuente, 1997; Kneller et al, 1999). More recently, Bleaney et al (2001), Wildman (2001), Padovano and Galli (2002), Li and Sarte (2004), and Lee and Gordon (2005) have each found that tax *structure* and/or public expenditure *composition* are correlated with growth. These results are more in line with recent theoretical models that highlight the growth impacts of particular distortionary taxes and distinguish productivity-enhancing from welfare-enhancing expenditures.

Nevertheless a number of methodological concerns continue to cast doubt on the reliability of much of this evidence. Firstly, the almost exclusive use of cross-sections or panels of 'period-averaged' data, typically without any lag structure, ensure that so-called 'long-run' effects of policy are captured fairly crudely and short-run dynamics cannot be explored. These methods

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[1] Recent endogenous growth models in which fiscal policy continues to have long-run effects include Kaas (2003), Kilyvitis (2003), Zagler and Durnecker (2003), Park and Philippopoulos (2003) and Ho and Wang

also impose parameter homogeneity across countries and over time, which Peseran and Smith (1995) show can induce biased parameter estimates. With the various fiscal aggregates tested having very different compositions in different countries and with numerous differences in macroeconomic conditions etc across countries, it would be surprising if this assumption was warranted. Certainly it has not been tested so far.

Secondly, partly because of the 'single snap-shot' nature of much of the evidence, establishing that observed fiscal-growth correlations are not simply the result of endogeneity has proved difficult. Bleaney et al (2001) explored lag structures on both period-averaged and annual data and tried to assess whether endogeneity could account for observed growth effects associated with distortionary taxes and 'productive' public expenditures. However, they continued to assume parameter homogeneity and, as a result, could not examine the role of transitional dynamics in their sample countries. The following sections therefore examine:

(a) The robustness of previous estimates of long-run fiscal-growth effects to the inclusion of short-run dynamics and heterogeneous parameters?
(b) How long does it take to reach the long-run equilibrium following a fiscal shock, and does this differ by country?
(c) Does the evidence point to *en*dogenous or *ex*ogenous relationships?

Heterogeneous fiscal-growth effects

The Bleaney et al (2001) results (hereafter, BGK) provide a convenient starting point for our analysis because, with an identical dataset, we can make direct, *ceteris paribus*, comparisons with previous results. It is also, to our knowledge, the only multi-country study of fiscal-growth effects that has previously allowed for (albeit it, limited) lag structures within annual panel data.

Following the spirit of the Barro (1990) model, BGK used GFS fiscal data for 16 OECD countries to construct measures of distortionary and non-distortionary taxes,[2] productive and unproductive expenditures, and budget surpluses/deficits. They argued that, with the exception of non-distortionary taxes and unproductive expenditures, which are hypothesised not to affect growth, all other element of the government budget constraint should be included

(2005).

[2] The term 'less distortionary' may be more appropriate in this case since the, mainly consumption, taxes in this category can distort investment decisions via labour supply effects in models such as Mendoza et al (1997).

in regressions.[3] Using a dynamic fixed effects (DFE) model, BGK found strong support for positive, long-run growth effects associated with productive public expenditures and budget surpluses and negative effects for distortionary taxes. To identify these long-run effects appeared to need up to 8 annual lags of data.

As noted above, in estimating individual country fiscal-growth effects, the BGK results assumed that the homogeneous parameters estimated over all countries apply to each (though country fixed-effects were included). This section considers an alternative potential source of difference between countries: that *marginal* effects differ across countries. That is, do regression parameters differ across the sample such that, *ceteris paribus*, some OECD countries experience stronger fiscal effects on growth than others? If so, do these differences relate only to the short-run or do they persist?

It is known that the results from a DFE regression are likely to be biased if, as Pesaran and Smith (1995) suggest, the assumption of homogeneity of the short-run parameter estimates across countries cannot be accepted. They show that this may be a more serious problem than the bias generated by the inclusion of lagged dependent variables and can lead to inconsistent and misleading results even for large T and large N. To overcome this bias they suggest the use of either the pooled mean group (PMG) or mean group (MG) estimators (Pesaran, et al., 1999). A comparison of the results from these two here has the additional advantage of allowing us to address formally the question of whether the long-run effect of fiscal policy on growth is identical across countries.[4] Acceptance of this restriction implies that the results from the PMG estimator are more efficient than those from the MG estimator (Pesaran, et al., 1999).

The estimated regression for the MG model is of the following ARDL form,

$$\Delta g_{it} = f_i (g_{i,t-1} - b_{1i} F_{it-1}) + \sum_{j=1}^{k} g_{0ij} \Delta g_{it-j} + \sum_{l=0}^{m} g_{1il} \Delta F_{it-l} + e_{it} \qquad (1)$$

where i indicates the country, t is time, g is the rate of growth, F is a matrix of fiscal and control variables, f, b and γ are parameters to be estimated and e_{it} a classical error term.

[3] If any fiscal variables which *do* have growth effects are excluded from regressions, then parameters on included fiscal variables must be interpreted differently (see Kneller et al, 1999 and BGK).

[4] The PMG estimator has the additional advantage over the alternative mean-group (MG) estimator in that it performs well even when, as is the case here, N is small (Hsiao et al., 1997). The MG estimator tends to be

The test for the long run effect of fiscal policy is made on the parameter vector b_{1i} (the long run fiscal policy parameters adjusted for lagged growth). Consistent with the general-to-specific approach, the lag structure of the regression is chosen on the basis of the Schwartz information criteria. The long run effect of fiscal policy across countries is taken as the (unweighted) average of the estimates from the N individual country regressions. The PMG model differs from these single country time series regressions by imposing homogeneity of the long-run parameters: f_i and b_{1i} become f and b_1 respectively. A Hausman test can be used to test the statistical plausibility of this restriction.[5]

The disadvantage of the MG and PMG estimators is of course that unless the available time series is very long a degrees of freedom problem is soon reached. For this reason we restrict the right-hand-side variables to include the investment rate and three fiscal variables: the budget surplus, distortionary taxation and productive expenditure. These are chosen in light of the results from BKG (2001) and it is worth remembering that the coefficients on these terms must be interpreted as conditional on those excluded fiscal variables, some of which the results from BGK suggest may be significant.[6] We are also forced to restrict the regression equation to include a maximum of two lags. At first sight this may appear to be an unhelpful limitation, since one objective is to identify how long it takes for any persistent fiscal effects to emerge. This turns out not to be a problem and the inclusion of the lagged dependent variable ensures that the impact of shocks can persist for many periods.

We begin by estimating equation (1) for the 16 OECD countries. Results are reported in regression 3 of Table 1, for the 'unrestricted' case where the PMG method selects the optimal lag length separately for each country and variable. Table 1 also reports two alternative PMG cases: where one annual lag (regression 4) and two annual lags (regression 5) are imposed for all countries and variables, though of course parameters remain heterogeneous.

Focussing first on the unrestricted case, Table 1 provides the individual test statistics (*p*-values) from the Hausman test of homogeneity of the long-run parameters as well as the test

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thought of as providing better information about the short-run and error correction coefficients of the PMG model (Pesaran et al., 1999).

[5] Results were estimated using Hashem Pesaran's GAUSS programme, available from the following website: http://www.econ.cam.ac.uk/faculty/pesaran/jasa.exe .

[6] BGK found that 'other revenues' (those not readily classified as either distortionary or non-distortionary) also had a significant (negative) effect on growth.

statistic from a joint test. We find we can accept long-run homogeneity both collectively and individually, except for distortionary taxation where the decision is less clear, with acceptance at the 8 per cent level. Having accepted homogeneity we report the (common) long-run parameters from the PMG estimator in the table, and test later for parameter stability. (We omit the short-run parameters at this stage to conserve space).

Bassanini & Scarpetta (2001) argue that in small country samples the estimated parameters may be sensitive to the inclusion or exclusion of any one country, even when the Hausman tests do not reject the assumption of homogeneity of the long-run parameters. Following their example, we re-estimate the PMG (regression 3) excluding in turn one country from the sample. Figure 1 reports the coefficients for each of the fiscal variables when a single country is omitted. We also indicate the standard errors from the full sample results to provide 95% confidence intervals for the results. As can be seen the parameter estimates remain stable from such a test and, except when Denmark is omitted (for productive expenditures: eprd), never stray outside of the confidence bands.

Concentrating on the fiscal parameters, consistent with the results from BGK the budget surplus and productive expenditures are found to affect the growth rate positively whereas distortionary taxation is found to lower growth. All of these long-run parameter estimates are significant at standard confidence levels. PMG parameter estimates are in line with those from BGK: the effect of distortionary taxation and the surplus are very close to the estimates found in that paper whereas the effect of productive expenditure is slightly lower. Differences between the two cases could be because of the use of a different set of countries, a slightly longer time period and/or the removal of several variables from the right hand side of the regression. To test this we first re-estimate the DFE model of BGK with these restrictions – reported as regression 1 in Table 1. The coefficients are, in fact, similar in value to those reported by BGK, although with an apparently stronger effect from the budget surplus.

Secondly, to see whether the difference between the DFE and PMG estimators is due to the restriction of a maximum of two lags in the PMG case, Regression 2 (Table 1) imposes the same lag length on the DFE as in the PMG estimates. In fact, comparing regressions 1 - 3 reveals that the DFE regression with a *longer* lag structure, rather than just two lags, produces similar results to the PMG regressions. The difference between the DFE and PMG estimates therefore would appear to be more the result of the greater parameter flexibility of the latter than its shorter lag structure. That is, to estimate the long-run fiscal-growth effects the DFE model would appear to need a long lag structure because of the unwarranted imposition of homogenous short-run effects. The long lag structure required by BGK (2001) would appear to be conditional on the homogeneity assumption.

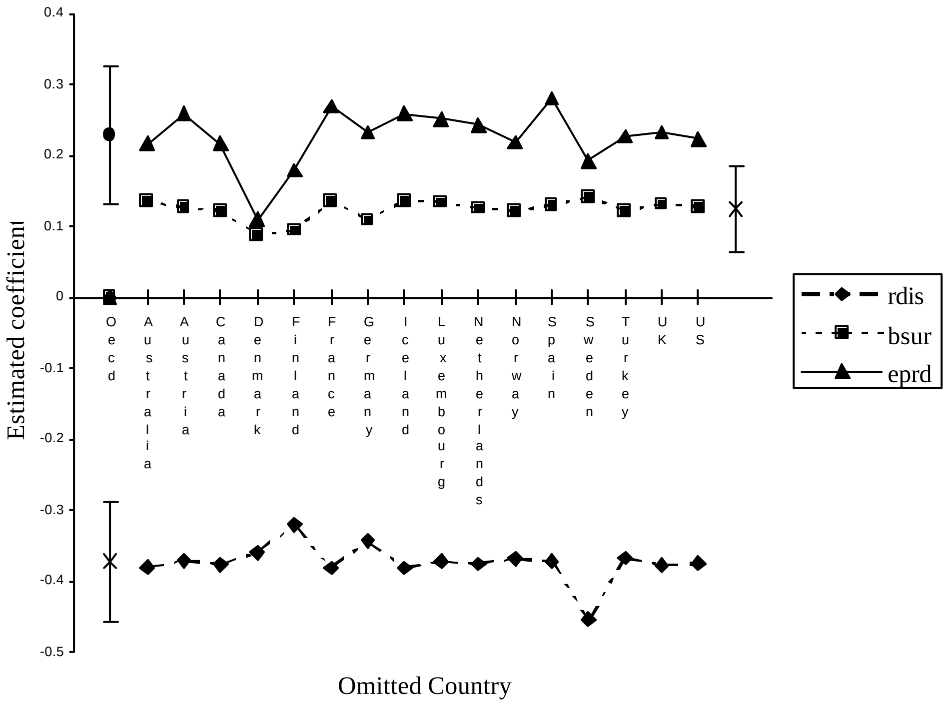
Table 1 DFE & PMG Regression Results (16 OECD Countries)

Method:	***DFE*** *[8-lags]*	***DFE*** *[2-lags]*	***PMG*** *[up to 2 lags]*	*Hausman tests (p-values)*	***PMG*** *[1-lag]*	***PMG*** *[2-lags]*
Regression No.	*1*	*2*	***3***		*4*	*5*
Budget surplus	0.068 (0.84)	-0.033 (0.65)	**0.125 (4.09)**	*2.23 (0.14)*	0.106 (2.77)	0.164 (4.66)
Distortionary taxation	-0.395 (-3.68)	-0.377 (5.48)	**-0.372 (-8.71)**	*2.97 (0.08)*	-0.380 (-7.39)	-0.386 (-7.40)
Productive expenditure	0.287 (1.79)	0.063 (0.44)	**0.228 (4.58)**	*2.54 (0.11)*	0.204 (3.11)	0.272 (3.91)
Investment ratio	0.119 (1.41)	0.110 (1.75)	**-0.080 (-2.72)**	*0.98 (0.32)*	-0.047 (-1.24)	-0.111 (-2.86)
Adjusted-R^2	*0.74*	0.65	*0.74*		*0.70*	*0.72*
Joint Hausman test (p-values)				*3.25 (0.52)*		

Note: t-statistics in parentheses below parameters.

Table 1 also reports results for the PMG model with fixed lag lengths (of 1 and 2 years) for all variables (regressions 4 & 5). Compared to the typically shorter lag lengths selected in the unrestricted case, it can be seen that, in general, imposing 1 lag (2 lags) generates only slightly smaller (larger) parameter estimates. In fact, the 2-lag PMG parameter estimates are generally close to the 8-lag DFE estimates. Thus it would seem that, even with only 2 lags, the PMG model is capable of identifying long-run effects of similar magnitude to a DFE model with much longer lags. In addition, despite somewhat different fiscal parameter estimates across the three PMG cases, these are not statistically significant. All three cases, however, confirm significant positive long-run growth effects from productive expenditures and budget surpluses and negative effects from distortionary taxes.

Figure 1 Testing Parameter Homogeneity



Note: bsur = *budget surplus;* rdis = *distortionary taxation;* eprd = *productive expenditures*

Exploring Short-Run Dynamics

Our analysis so far appears to confirm a common, persistent growth effect across the OECD sample from changes in the key fiscal variables, but different short-run dynamics for getting there. These dynamics also identify how quickly the long-run fiscal-growth effects feed through in each country. For example, does the bulk, or all, of the growth impact of an increase in productive expenditures occur quickly or only after several years? Does growth respond much more quickly in some countries than others?

The trajectory followed by each country is determined by the estimated lag structure on each fiscal variable for that country together with its error-correction parameter, f_i. Since this is an asymptotic process, (except where $f_i = -1$) one way to summarise these trajectories is to consider the number of years taken to reach within a given percentage of the long-run equilibrium, following a fiscal shock.[7] We examine the number of years taken for each fiscal-growth effect to achieve 90% of its long-run value.[8] This is shown in Table 2. The

[7], Half-lives (the more usual indicator of adjustment speeds in such cases) are not very helpful in this case because of the relatively rapid adjustment observed, as shown below.

[8] In those cases where countries oscillate towards the long-run equilibrium, we choose the number of years until the relevant fiscal-growth effect *remains* within 90% of its long-run value. Results are not sensitive to the particular percentage chosen.

error correction parameter itself, f_i, of course, also provides a measure of the speed of adjustment of growth following an exogenous fiscal shock. These are summarised in Table 3

As previously, three PMG cases are considered: **A**: the 'unrestricted' case where the PMG method selects the optimal lag length separately for each country and variable (using a 10% Schwartz information criterion, SIC); **B**: where one annual lag is imposed for all countries and variables; and **C**: where 2 annual lags are imposed. In each case, the final row of the table shows the long-run effect. Two important issues to consider are: are two lags sufficient? And, which of cases A – C is preferred?

On the first question, when allowed a choice of lag length (case **A**), the PMG model generally chooses fewer than two lags: of the 80 short-run parameters estimated (16 countries x 5 variables, including the lagged dependent variable), in only 11 cases are 2 lags selected. This suggests that including more than two lags (were this option available to us) would be unlikely to have much effect on our results. Secondly, the unrestricted case involves the selection of zero lags for the *dependent* variable in 8 of the 16 countries. It is this that largely generates the very short dynamics observed in Table 2 for case **A**. However, in view of the short time-series for each country and the consequent low power of the SIC test, we are inclined to prefer results based on the inclusion (rather than exclusion) of one or two lags.[9]

Table 2 records the number of countries achieving 90% of the (common) long-run fiscal effects within 0, 1-2, 3-4 etc years, following a one percentage point (positive) shock to each fiscal variable. For example, in the unrestricted case in Panel **A**, a 1 percentage point increase in the budget surplus (as a ratio of GDP) increases growth by 0.10 percentage points *in the long-run*. Column 1 shows that 8 countries achieve 90% of this effect within the current year (year 0); 3 countries take 1-2 years; a further 3 countries take 3-4 years, etc. Similar results are obtained for distortionary taxes and productive expenditures in Panel **A**.

Table 3 (Panel A) reveals a consistent picture: 12 of the 16 OECD countries respond rapidly to a fiscal shock, with more than 90% of the initial disequilibrium being corrected within

[9] In addition, inclusion of the lagged dependent variable might be expected to help deal with any serial correlation in the data.

one year. The remaining 4 countries correct more than 50% of the disequilibrium within a year.

Table 2 Number of Countries & Years Following Shock to Achieve 90% of Long-Run Fiscal-Growth Effect (16 OECD Countries)

Years	**A.** Unrestricted			**B.** 1 lag imposed			**C.** 2 lags imposed		
	Budget surplus	**Distort. taxes**	**Prod. exp.**	**Budget surplus**	**Distort. taxes**	**Prod. exp.**	**Budget surplus**	**Distort. taxes**	**Prod. exp.**
0	7	7	7	0	0	0	1	0	0
1 – 2	4	3	4	14	14	10	1	0	0
3 – 4	3	6	3	2	2	5	4	3	4
5 - 7	2	0	1	0	0	1	8	11	8
8 +	-	-	1	-	-	-	2	2	4
Long-run Effect (st error)	0.125 (0.03)	-0.372 (0.04)	0.228 (0.05)	0.106 (0.04)	-0.380 (0.05)	0.204 (0.07)	0.168 (0.04)	-0.386 (0.05)	0.293 (0.07)

Table 3 Speed of Fiscal-Growth Responses

Proportion of Error Corrected within 1 Year	**Number of Countries:**		
	A.	**B.**	**C.**
> 90%	12	5	6
50 - 90%	4	11	5
< 50%	0	0	5

Comparing all three panels in Tables 2 & 3, the clear message is that the speed of response to a fiscal shocks, and the number of years required to achieve the estimated long-run impacts are quite different depending on the assumptions made about lag structures in the PMG estimation. In particular, imposing a common lag length – even though estimated short-run parameters may vary across countries – generates noticeably longer dynamic adjustments than when the model is allowed to select its preferred (shorter) lag length.

Table 3 also shows that using two lags generates both longer short-run dynamics and slightly larger long –run estimates of fiscal-growth responses, compared to the other cases. Nevertheless, the alternative long-run fiscal parameter estimates are not statistically different, and whatever lag option is chosen (**A**, **B** or **C**), Table 3 shows that for most countries, more than 50% of any fiscal shock is corrected within one year. These results continue to support the hypothesis of significant fiscal-growth effects - of the order of 0.23 for productive expenditures, -0.37 for distortionary taxes and 0.13 for budget surpluses. The evidence of short lag lengths and rapid adjustment speeds towards long-run values, however, raises questions concerning the exogeneity of fiscal policy. We turn next to this issue.

Endogeneity of Fiscal-Growth Effects

The evidence in Tables 2 & 3 of relatively rapid short-run dynamic adjustment suggests the possibility that the observed effects may represent simultaneous relationships between growth and fiscal policy. That is, instead of, or as well as, direct impacts of fiscal variables on GDP growth, changes in GDP growth may be inducing changes in these fiscal variables, albeit that the latter would not be expected to persist over the long-run. This section adopts two procedures to investigate this possibility. First we consider the specific endogeneity arguments in this case.

The hypothesis that faster growth induces changes in *total* government expenditure or taxation is well known: economic downturns reduce taxable capacity and generate demands for additional public expenditure such as unemployment benefits and social insurance payments.[10] These arguments might be expected to apply with equal or greater force to the tax/expenditure components considered here. Since distortionary taxes here are mainly capital and personal income taxes, these would be expected to be income-elastic and therefore pro-cyclical (both absolutely and relative to non-distortionary taxes). As a result they would be expected to rise, as a share of GDP, when income grows more rapidly and fall (or rise more slowly) when income grows slowly.

Productive expenditures would also be expected to rise when faster growth generates additional revenues and demands for unproductive expenditures (such as social insurance) weaken. This would raise the share of productive expenditure in total expenditures and probably also in GDP – public investment tends to be pro-cyclical. In addition, budget deficits/surpluses are, of course, well known to be counter-cyclical, which could reflect slower growth *causing* increased deficits (reduced surpluses), and *vice versa*.

As a result, endogeneity in our previous results could result from effects running from growth to fiscal variables, with predicted effects on our three fiscal variables as follows:

[10] The argument that *total* expenditures rise during downturns of course hinges on the assumption that increases (or faster growth) in counter-cyclical expenditures are not compensated by reductions (or slower growth) in pro-cyclical expenditures, such as public investment spending. This is usually plausible since

Distortionary taxes	**positive**	Budget surplus	**positive**
Productive expenditures	**positive**		

Therefore, endogeneity arguments reinforce the chances of finding positive effects for productive expenditures and budget surpluses, but they *reduce* the likelihood of finding the strong negative effects of distortionary taxes on growth that our results thus far indicate. Nevertheless, for all three fiscal variables we pursue two strategies to test for endogeneity effects.

Firstly, in his critique of the investment and growth literature, Jones (1995) suggests that, at one extreme, regressions including contemporaneous investment can be considered to represent the maximum possible causal effect of investment on growth. At the other extreme, *none* of the observed contemporaneous correlation could be considered to represent causation. Analogously, for our fiscal variables, by repeating our earlier regressions but disallowing any contemporaneous correlation between the fiscal variables and growth, the estimated parameters could be considered as representing the minimum causal effect of those fiscal variables on growth. This approach was followed by BGK.

Secondly, where endogenous relationships are suspected, a preferred solution is to fully specify separate equations for both endogenous variables and test simultaneously. In the present case, however, as in most such growth regressions, we have insufficient exogenous variables to allow both equations to be identified. As an alternative, we re-run our PMG regressions but treating each fiscal variable in turn as the dependent variable, with growth as the independent variable. These are conducted both with and without the remaining fiscal variables on the right-hand-side. Where we observe a significant effect (of the expected sign) of growth on our fiscal variables, there is a *prima face* case that growth may be inducing a fiscal response rather than, or as well as, *vice versa*. Of course, the endogeneity suspected in the previous regressions may also affect the estimated parameters here.

short-run contraction of other expenditures is likely to be more difficult to achieve than increases in social expenditures.

Disallowing contemporaneous effects

Table 4 shows the effect on our PMG growth regressions of omitting contemporaneous effects of all three fiscal variables. In Regression 1, we continue to allow for contemporaneous impacts of investment on growth; in Regression 2, these are disallowed. Of course, not only might endogeneity of investment be suspected but, if there are exogenous fiscal effects on growth, these might be expected to impact, in part, indirectly through investment. Results in regression 1 confirm that, when contemporaneous effects are disallowed for fiscal variables but allowed for investment, positive effects of investment on growth are now observed and fiscal parameters become smaller (e.g. the tax parameter falls from (–)0.37 to (–)0.18; and from 0.22 to 0.09 for expenditures).

Table 4 Endogeneity Tests: Removing Contemporaneous Correlation

Method:	***PMG*** (up to 2 lags)	***PMG*** (up to 2 lags)	***PMG*** (up to 2 lags)
Regression:	*Table 2: col. 5*	*1.*	*2.*
Contemporaneous effects allowed?:	*Fiscal: YES* *Investment: YES*	*Fiscal:* ***NO*** *Investment: YES*	*Fiscal:* ***NO*** *Investment:* ***NO***
Budget surplus	0.125 (4.09)	-0.017 (-0.42)	0.095 (1.34)
Distortionary taxation	-0.372 (-8.71)	-0.180 (-3.71)	-0.730 (-9.00)
Productive expenditure	0.228 (4.58)	0.092 (1.48)	0.542 (5.20)
Investment ratio	-0.080 (-2.75)	0.052 (1.43)	-0.473 (-8.33)

Clearly the investment-growth impact is affected by the exclusion of current fiscal effects, implying that some previously identified fiscal-growth effects were transmitted via investment, as might be expected. When possible endogeneity of investment is removed by disallowing contemporaneous effects here too, the investment parameter becomes negative, and strong fiscal effects on growth are again confirmed. (In fact, tax and expenditure parameters are now generally larger than when contemporaneous impacts are allowed)[11].

Reversed causation?

If GDP growth induces fiscal responses rather than the other way round, the arguments outlined above suggest that faster growth would be expected to increase each fiscal

[11] This suggests that the immediate (contemporaneous) effect of a fiscal shock is in the opposite direction from the long-run effect – an effect observed for various individual countries' short-run dynamics.

variable: budget surpluses; productive expenditures and distortionary taxes. The null hypothesis in each case is, therefore, that faster GDP growth generates a more than proportionate response in expenditures, taxes and surpluses. The outcome is expected rises in these fiscal variables as ratios of GDP (the variables in our analysis) when GDP growth increases. What is the alternative hypothesis? Two alternatives suggest themselves. Firstly, the three fiscal variables may change with GDP but only in proportion. In this case, increases in growth would have *no impact* on our fiscal variables. Secondly, in the extreme, if the growth of distortionary taxes etc remain unchanged in the face of changes in GDP growth, they would be expected to decline as shares of GDP when GDP growth increases, and *vice versa*. That is, a *negative* relationship is expected. Thus the alternative hypothesis, is that the fiscal responses to growth are less than or equal to zero.

Table 5 Endogeneity Tests: 'Explaining' Fiscal Variables

	Dependent Variable:			
	Budget surplus	**Distortionary taxation**	**Productive expenditure**	**Other variables included?**
GDP Growth Rate	1.11	-0.25	-0.67	No
(t-ratio)	(11.2)	(-4.0)	(-8.4)	
GDP Growth Rate	1.42	-0.28	-0.50	Yes
(t-ratio)	(12.3)	(-5.4)	(-7.5)	

Table 5 shows the parameters on the GDP growth rate where this is regressed on the fiscal variable of interest, with or without the inclusion of other fiscal/investment variables. The results suggest a clear positive relationship of growth with the budget surplus, but clear negative relationships with distortionary taxes and productive expenditures. That is, the hypothesised endogenous relationship (from growth to fiscal policy) is firmly rejected for the latter two variables but accepted for the budget surplus.

The results of both tests of endogeneity in fiscal-growth responses are consistent. There is some doubt over the true long-run impact of budget surpluses on growth, but strong long-run effects observed for distortionary taxation and productive expenditures do not appear to be the result of endogeneity.

Are the Orders of Magnitude 'Right'?

The results of the previous section appear to offer strong support to the view that there are persistent effects of fiscal policy on growth. Previous evidence of significant fiscal-growth effects have been subject to two further criticisms however. Firstly, the magnitudes of some previously estimated effects are too large to be plausible.[12] Secondly, Jones (1995) and Karras (1999) have argued that evidence of non-stationarity in investment/GDP ratios (Jones) and total tax/GDP ratios (Karras) is at odds with evidence of stationarity in GDP growth rates. Thus, the former cannot plausibly explain the latter, unless *"by some astonishing coincidence all of the movements in variables that can have permanent effects on growth rates have been offsetting*" (Jones; 1995; p.496).

In this section we examine the relevance of these arguments for our evidence that changes in *the composition of* expenditures and taxes have persistent effects on growth. Our evidence suggests, broadly, that fiscal variables do indeed appear to have had persistent but offsetting effects on growth during the period that we study.[13]

The Magnitude of OECD Fiscal-Growth Effects

To examine the implied growth responses to observed fiscal policy changes in OECD countries we apply the (unrestricted) PMG model to each country over 1970-98. We allow investment to vary but hold (all three) fiscal variables constant, and compare the growth outcomes with the estimated growth impacts when both investment and fiscal variables are assumed to follow their observed patterns.

First, it is worth noting that there is considerable cross-section and time-series variation in the key fiscal variables within the sample. Appendix Table 1 illustrates the country values for all three fiscal variables (as shares of GDP) at the end of the period; Table 6 summarises the data for OECD, EU and non-EU groupings.[14] On average around 20% of GDP in the OECD was collected from distortionary taxes, but varied from just over 30% in the Netherlands to 6% in Turkey. EU governments on average had a substantially greater proportion of distortionary taxes (23%) than Non-EU governments (14%). Productive expenditures were also widely dispersed: from 21% in the Netherlands and France to 7% in Canada, with an OECD average of 14%, which is quite similar across EU and non-EU sub-

[12] See, for example, Agell et al. (2005) who criticise previous fiscal-growth estimates.
[13] We do not use the term 'permanent' here since our period of analysis does not allow us to discriminate between 'persistent' but nevertheless transitional effects (in the sense used in neo-classical models), and permanent, effects
[14] In these tables, we 'smooth' the data - to avoid any atypical annual values distorting the picture - by averaging over 1995-97.

samples. Except for Norway and Luxembourg, budget surpluses are all in deficit (negative) in this period, as might be expected, averaging around 3% of GDP.

Table 6 OECD Fiscal Categories (as % GDP; average 1995-97)

	Distortionary taxes	Productive expenditure	Budget Surplus
OECD Average	19.6	14.1	-3.0
(Standard. dev.)	(6.5)	(4.2)	(3.1)
EU Average	23.1	15.0	-3.7
Non-EU Average	13.7	12.6	-1.7

Table 7 Changes in OECD Fiscal Categories, 1970-1998 (% GDP)

	1970-1980			1980-1998		
	? Distort. taxes	? Prod. exp	? Budget Surplus	? Distort. taxes	? Prod. exp	? Budget Surplus
OECD average	3.1	1.8	-2.3	1.6	0.6	0.6
EU Average	4.0	2.8	-3.1	1.8	0.0	-0.1
Non-EU Average	1.7	0.1	-0.9	1.2	1.5	1.6

Changes in our fiscal categories over the period - relevant for calculating growth effects - are also quite substantial. Table 7 compares fiscal changes over 1970-80 and 1980-98 (see Appendix Table 2 for country details).[15] This shows that revenues collected from distortionary taxes in OECD countries, as a share of GDP, generally increased but rose twice as much in the 1970s decade (over 3 percentage points) compared to the 18 years from 1980-98 (1.6 percentage points). A similar picture emerges for productive expenditures. These also increased their shares of GDP on average, but more slowly after 1980 than before. Increases in OECD budget deficits in the 1970s (in 13 of the 16 countries were reversed on average after 1980 (though in 7 countries deficits worsened).

According to our regression results, these changes in distortionary taxes and deficits would be expected to impact adversely on OECD growth, especially during the 1970s, while the increases in productive expenditures would mitigate this. Table 8 summarises the net effects on OECD growth rates. As can be seen, with the exception of three countries before 1980 (and 5 countries after 1980), observed fiscal changes are estimated to have reduced

[15] We follow Tanzi and Schuknecht (2000) here, who distinguish these two periods in their study of the evolution of the size and scope of the public sector in developed countries. As well as being characterised by a rapid expansion of public expenditures, the 1970s also witnessed turbulent macroeconomic conditions when fiscal, monetary and other effects on growth are likely to be interdependent.

average annual growth rates in our sample compared to a counterfactual of unchanged fiscal/GDP shares.

The net effects on growth however were generally fairly small, especially after 1980 when they typically range between 0.1 and 0.6 of a percentage point, 10 of which are statistically significant (at 10% or better). For example, in the UK, 1980-98, the net effect of fiscal changes is effectively zero. The US is one of relatively few countries where fiscal choices may have boosted growth - by up to 0.3 percentage points after 1980. In several countries the estimated net effects are small and statistically insignificant, suggesting that the gross fiscal effects associated with each of the observed changes in distortionary taxes, productive expenditure and budget deficits may indeed be approximately compensating for each other.

Table 8 Growth Effects of Fiscal Changes, 1970-1998
(in percentage points per year)

	1970-1980	**1980-1998**
Australia	-1.0^{b}	-0.5^{a}
Austria	-0.7^{c}	-0.6^{a}
Canada	1.2^{c}	-0.8^{c}
Denmark	-0.5	-1.0^{a}
Finland	-0.6^{b}	-1.4^{b}
France	-0.5^{b}	-0.6^{a}
Germany	-0.5	-0.1
Iceland	0.5	-0.4
Luxembourg	-3.6^{b}	0.3
Netherlands	-0.1	0.2
Norway	-0.2	0.8^{a}
Spain	-1.3^{a}	0.0
Sweden	-1.2^{a}	-0.8^{a}
Turkey	-0.7^{a}	0.7^{a}
UK	0.3	0.0
US	-0.2	0.3^{b}
Weighted Averages: *		
OECD Average	$\mathbf{-0.3^{a}}$	**0.0**
EU Average	$\mathbf{-0.4^{a}}$	$\mathbf{-0.3^{a}}$
Non-EU Average	**-0.1**	$\mathbf{0.2^{c}}$

* Weighted by country GDPs. a (b, c) Differences between growth rates based on actual and constant fiscal variables are significant at a 1% (5%, 10%) level.

The (weighted) average OECD effect is approximately zero, with relatively small effects in EU and non-EU sub-samples at –0.3 and 0.2 of a percentage point respectively. Within the EU, the largest effects appear to be associated with the Scandinavian countries – Denmark, Finland, Norway and Sweden. Thus, though some of the country-specific effects are more substantial, on the whole these net fiscal-growth effects look plausible, implying that the fiscal choices made by OECD governments over this period have typically involved modest growth trade-offs.

Applying Panel-Data Tests

Having suggested that post-World War II growth rates in the OECD are best characterised as stationary, Jones (1995, p.502) argues that

> *"if … permanent movements in some variable X have permanent effects on growth, then either*
> *(a) X must exhibit no persistent movements, or*
> (b) *some other variable (or variables) must also have persistent effects on growth that offset the movements in X".*

Our data above provide *some* evidence of persistence in the "*X*" variables – namely between 1970 and 1998, both distortionary taxes and productive expenditures (as % of GDP) show signs of persistent upward movement in *some* OECD countries (see Appendix Table 2). For others, changes in fiscal categories appear to have been more transitory. In either case the evidence in Table 8 is that the net effect of our fiscal variables is largely to *compensate* rather than *reinforce* their individual effects on growth.

Jones (1995) suggests more formal time-series tests of the properties of individual country time-series: mainly ADF tests for growth and the right-hand-side variables of a growth regression. Given our focus on fiscal variables the available time-series are insufficiently long to permit time-series tests for individual countries. However, we can apply two comparable formal tests.

(a) Panel unit root tests that test the null hypothesis of non-stationarity of our fiscal variables in all countries against the alternative of stationarity in at least one, or all, countries.
(b) Stationarity tests of the residuals from the PMG regressions.

We first examine whether our three fiscal variables, investment and the growth rate are stationary, using panel unit root tests that test the null of non-stationarity for all countries against the alternative that at least one country/variable is stationary. This is a particularly stringent test; we use the tests proposed by Maddala and Wu (1999), Im et al. (2003), and Taylor and Sarno (1998). Each has some advantages and drawbacks for the current application. Results are given in Appendix Table 3.

The Maddala and Wu (1999) test has the advantage here that it does not require a balanced panel data, allowing the full sample to be tested. This reveals that the null hypothesis of all countries being non-stationary is rejected at a 1% significant level for all variables except investment. However, the Maddala and Wu (1999) test does not take into account any cross-section dependence, since it combines the p-values from N independent unit root tests. We further employ the Im et al. (2003) test, which, consistently with the PMG used previously, allows for individual effects, time trends, autoregressive coefficient and numbers of lags[16]. We find that the null hypothesis of non-stationarity for all series is clearly rejected for distortionary taxes, investment and growth, whereas it is not rejected for productive expenditures and the budget surplus. However, like Maddala and Wu (1999), this test does not fully take account of any cross correlation.

Taylor and Sarno (1998) propose a panel unit root test using the SURE method, which takes into account the contemporaneous correlation among the disturbance terms. Appendix Table 3 shows that this test rejects the null hypothesis of non-stationarity for all variables. Finally, Levin et al. (2002) test of the null hypothesis of all countries being non-stationary against the alternative of *all countries* being stationary. This test also allows individual effects, time effects and a time trend, though it does not allow for heterogeneity in the autoregressive coefficient under the null hypothesis of stationarity. Nevertheless, this test rejects the null at a 1% level for all variables.

Recently, Harris et al (2005) have proposed a non-parametric panel unit root rest that allows for cross-sectional dependencies, can accommodate structural breaks, stationary and non-stationary series, and hetergoenous time-series dynamics, within the panel. These properties most closely approximate those relevant to the PMG models estimated here. The Harris et al (2005) approach involves a simple test of the stationarity of the model's residuals, where the test statistic is distributed as a standard Normal. Testing the residuals from the (unrestricted) PMG regression in Table 1 strongly supports a conclusion of stationarity.[17]

[16] This test and all the subsequent tests require a balanced panel; hence, we reduce the sample to the period 1973-1995 and exclude West Germany.

Conclusions

The last few years have seen important advances in methodologies for testing the long-run impact of fiscal policy on growth. These new methods appear to be generating increasing support for the view that *some* taxes and types of public expenditures have long-run growth effects. The "long-run", however, in most studies has typically been identified using methods applied to cross-sections or panels of period-averaged data (BGK (2001) is an exception), and assumed common impacts across countries. As a result fiscal impacts have generally been identified rather crudely and estimated, or assumed, to take 5-10 years or more to feed through to growth. In addition, as Pesaran and Smith (1995) and others have argued, assuming *incorrectly* that such parameters are homogeneous across countries is likely to bias results.

To overcome this Pesaran et al. (1999) suggest using Pooled Mean Group or Mean Group estimators. This paper has applied these methods to address three key questions:

- are previous estimates of long-run fiscal-growth effects biased?
- do they differ across countries?
- to the extent there are long-run effects, how quickly are they achieved?

Our results support the following conclusions:

1. Previous results (for OECD countries), based on dynamic fixed effects models do not appear to be biased in the sense that alternative estimators yield similar long-run parameter estimates.
2. The results here also confirm pervious evidence that distortionary taxes and productive expenditures have, respectively, negative and positive impacts on growth, and these long-run effects can reasonably be regarded as homogenous across OECD countries. However, short-run dynamics are quite different across countries.
3. These dynamics suggest that most of the identified 'long-run' effects are typically achieved quickly (within 1 – 3 years) so that a more appropriate interpretation of the evidence is that short-run effects of fiscal policy on growth are significant and *persistent* (provided fiscal policy changes are not reversed).

We have subjected these results to various tests for endogeneity of our fiscal policy variables. With the possible exception of the budget surplus/deficit, the evidence of

[17] A test statistic of S = 0.242 is obtained, which easily satisfies the null of stationarity (5% (10%) critical values are 1.65 (1.3)). A similar conclusion holds when the model is re-estimated for the same balanced panel

persistent effects of fiscal variables on growth would appear not to be due to endogenous fiscal policy responses. We also considered whether our regression results yield plausible growth effects, when applied to the various fiscal policy changes observed across OECD countries. In particular, Jones (1995), Karras (1999) and others have argued that the non-stationary properties of such variables as investment/GDP ratios and total tax/GDP ratios render them implausible as drivers of stationary GDP growth rates.

How far do these arguments apply to the fiscal-growth effects identified here? The evidence suggests that in models in which *distortionary* taxes, *productive* expenditures and budget deficits are hypothesised to affect growth, these fiscal variables (as ratios of GDP) are often stationary, such that potentially growth-affecting fiscal changes are often reversed. Hence growth effects in practice would be expected to be short-lived. Where relevant fiscal changes persist, their effects on growth are generally compensating so that the *net* effect is expected to be small. Our estimates of these net effects confirm that for the OECD as a whole, and for most of the 16 members considered, estimated fiscal-growth effects over the period examined are relatively small, and certainly within the range of plausibility. For example, for the OECD on average, annual GDP growth is estimated to have been around 0.1 – 0.3 of a percentage point lower over 1970-98 compared to a counterfactual of constant values for our key fiscal variables.

It would appear then that Jones's (1995) view that it would be an "astonishing coincidence" if two non-stationary variables that drive growth compensate for each other in such a way as to generate a stationary growth process, is not so astonishing in this context. Though various governments have chosen to increase their productive expenditures with growth-enhancing consequences, they have simultaneously tended to increase growth-inhibiting distortionary taxes to finance them. In sum, our results would seem largely to confirm Dalgaard and Kreiner's (2003; p.83) conjecture, based on *a priori* reasoning, that:

> "*it may well be the case that a higher tax rate has a significant negative effect on the growth rate, but that this is roughly offset by a significant positive growth effect of the productive government expenditure that is financed by the higher tax rate, thus resulting in a small overall net effect*".

used in the other stationarity tests.

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Appendix

Appendix Table 1 Government Fiscal Categories (% GDP), average: 1995-97.

	Distortionary taxes	Productive expenditure	Deficit
Australia	16.5	10.2	-1.0
Austria	24.5	18.1	-4.0
Canada	14.1	6.6	-3.5
Denmark	17.8	11.4	-1.9
Finland	14.1	13.0	-8.0
France*	26.6	20.8	-5.1
Germany**	20.5	12.3	-2.3
Iceland	10.7	16.8	-1.7
Luxembourg	29.7	16.5	3.7
Netherlands	31.4	21.2	-3.0
Norway	16.9	11.8	3.3
Spain	21.1	7.9	-6.0
Sweden	23.2	12.5	-5.5
Turkey	5.9	18.7	-6.2
UK	21.6	16.3	-5.3
US	18.3	11.7	-1.3
OECD Average	19.6	14.1	-3.0
(St. dev.)	(6.5)	(4.2)	(3.1)
EU Average	23.1	15.0	-3.7
Non-EU Average	13.7	12.6	-1.7

Appendix Table 2: Changes in Fiscal Categories, 1970-1998 (percentage points)*

	1970-1980			1980-1998		
	? Distort. taxes	? Prod. exp	? Deficit	? Distort. taxes	? Prod. exp	? Deficit
Australia	0.9	2.3	-1.3	3.7	0.8	4.3
Austria	5.0	5.2	-4.8	3.3	1.9	0.6
Canada	-1.4	0.3	-2.2	2.4	-1.0	-0.1
Denmark	-1.0	-1.9	-5.0	3.4	-1.0	0.7
Finland	-0.2	1.3	-3.4	3.1	-1.9	-4.2
France	3.1	0.1	2.5	2.1	3.9	-5.6
Germany	5.3	2.3	-2.9	-0.5	0.3	-0.5
Iceland	-1.7	-0.2	1.2	4.4	1.3	1.5
Luxembourg	10.2	7.2	0.1	-0.9	-0.4	1.1
Netherlands	1.7	3.1	-4.5	-2.1	-2.9	2.9
Norway	4.5	-6.5	-0.4	-1.7	1.0	6.8
Spain	7.1	0.9	-3.9	3.1	2.2	-1.8
Sweden	7.6	8.8	-7.3	5.5	-1.3	6.8
Turkey	5.8	4.7	-1.7	-3.3	5.8	-5.3
UK	0.8	1.4	-1.9	0.8	-0.3	-0.7
US	2.2	-0.1	-1.3	1.5	1.0	2.5
OECD Average	3.1	1.8	-2.3	1.6	0.6	0.6
EU Average	4.0	2.8	-3.1	1.8	0.0	-0.1
Non-EU Average	1.7	0.1	-0.9	1.2	1.5	1.6

** An identical data span was not available for all countries. In some case the first year available is 1972 or 1973; the last year is 1996 or 1997.*

Appendix Table 3:
Panel Unit Root Tests: Fiscal variables, Investment and growth in OECD countries (1970-1998)

Test	Distortionary taxes	Productive expenditures	Investment	Budget Surplus	Growth rate
Maddala and Wu (1999)	?(32): 74.29 p-value= 0.00	?(32): 71.48 p-value= 0.00	?(32): 50.84 p-value= 0.02	?(30): 66.65 p-value= 0.00	?(30): 150.74 p-value= 0.00
Im, Pesaran and Shin (2003)	t-bar -2.899 p-value 0.00	t-bar -2.182 p-value 0.48	t-bar -2.675 p-value 0.02	t-bar -2.015 p-value 0.02	t-bar -2.873 p-value 0.00
Levin, Lu and Chu (2002)	t-value -12.054 p-value 0.00	t-value -9.443 p-value 0.01	t-value -11.327 p-value 0.00	t-value -8.195 p-value 0.01	t-value -11.598 p-value 0.00
Taylor and Sarno (1998)	MADF 208.783 CV 45.195	MADF 689.458 CV 45.195	MADF 182.028 CV 45.195	MADF 1417.48 CV 45.195	MADF 790.842 CV 45.195

[a]The lag length is chosen using the Schwartz information criteria. The results are robust to the choice of lag length.
[b]Following test on the individual variables over 1970-1998, panel unit root tests for distortionary taxes, productive expenditure and private investment include a constant and a trend whereas the growth rate and budget deficit include only a constant.