

Testing the endogenous growth model: public expenditure, taxation, and growth over the long run

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Abstract. Endogenous growth models, such as Barro (1990), predict that government expenditure and taxation will have both temporary and permanent effects on growth. We test this prediction using panels of annual and period-averaged data for OECD countries during 1970–95, isolating long-run from short-run fiscal effects. Our results strongly support the endogenous growth model and suggest that long-run fiscal effects are not fully captured by period averaging and static panel methods. Unlike previous investigations, our estimates are free from biases associated with incomplete specification of the government budget constraint and do not appear to result from endogeneity of fiscal or investment variables. JEL Classification: H30, O40

Validation du modèle de croissance endogène: dépenses publiques, fiscalité et croissance à long terme. Des modèles de croissance endogène comme celui de Barro (1990) prédisent que dépenses gouvernementales et fiscalité vont avoir des effets temporaires et permanents sur la croissance. On met cette prévision au test à l'aide de données annuelles et pour certaines moyennes couvrant des sous-périodes pour les pays de l'OCDE (1970–95) dans le but de départager les effets à court et à long terme. Les résultats valident fortement le modèle de croissance endogène et suggèrent que les effets fiscaux à long terme ne sont pas pleinement capturés par des méthodes utilisant des moyennes ou des méthodes statiques. Contrairement aux résultats d'enquêtes antérieures, les résultats proposés ne souffrent pas de distorsions attribuables à une spécification incomplète de la contrainte budgétaire du gouvernement, et ne semblent pas être l'effet d'écho de l'endogénéité des variables fiscales et de l'investissement.

1. Introduction

In the neoclassical growth model of Solow (1956), together with its many subsequent extensions, the long-run growth rate is driven by population growth and the

We are grateful to two referees of this *Journal* for helpful comments on an earlier draft of this paper and to participants at seminars at Keele University, U.K., and the Australian National University, Canberra. E-mail: norman.gemmell@nottingham.ac.uk

rate of technical progress. Distortionary taxation or productive government expenditures may affect the incentive to invest in human or physical capital, but in the long run this affects only the equilibrium factor ratios, not the growth rate, although in general there will be transitional growth effects. Endogenous growth models such as those of Barro (1990) and King and Rebelo (1990), on the other hand, predict that distortionary taxation and productive expenditures *will* affect the long-run growth rate. The implications of endogenous growth models for fiscal policy have been particularly examined by Barro (1990), Jones, Manuelli, and Rossi (1993), Stokey and Rebelo (1995), and Mendoza, Milesi-Ferretti, and Asea (1997).

In testing whether the historical evidence supports the neoclassical or the endogenous growth model, several major difficulties arise. One is that there may be only limited data on government expenditures and revenues, particularly at the required level of disaggregation, and the definition of particular expenditures as productive or unproductive or particular taxes as distortionary or non-distortionary may be open to debate.¹ Much empirical ‘government and growth’ research predates the new generation of theoretical models and therefore fails to address these issues. Even recent contributions, however, have given limited attention to the measurement of fiscal variables.

A second – and often overlooked – problem is that, because of the linear relationship between fiscal variables implied by the government budget constraint, biases can easily be introduced into regression equations if the researcher neglects the implicit financing assumptions built into the specification. This is not simply a point of interpretation of regression results, since it also has implications for the appropriate testing strategy, as we indicate below.

A third problem is that the usefulness of fiscal policy data in testing the endogenous growth model rests on the ability of the empirical methodology employed to separate the effects of policy on the transition from those on the steady state. Much existing evidence is based on single cross-sections or panels of five-year averages (and allows only for contemporaneous effects within each five-year period), relying on the period-averaging process to capture the long-run. Whether this is an adequate procedure or whether longer lags are required remains an under-researched issue. It is an important one, however, since neoclassical and endogenous growth models differ only in their *long-run* predictions. If existing evidence captures short-run behaviour only, it cannot discriminate between alternative theories.²

A fourth and related problem concerns the endogeneity of regressors in growth equations. In this case, does faster growth induce larger government expenditures and taxes (e.g., via Wagner’s Law), or vice versa, or both? Clearly, if fiscal variables

1 See Evans and Karras (1994) for evidence on ‘productive’ government expenditures across U.S. states.

2 Since Jones (1995a), a number of authors have begun to use time-series or dynamic panel methods to distinguish short- from long-run growth effects for investment or convergence variables (see, e.g., Caselli, Esquivel, and Lefort 1996; Evans 1998). Karras (1999) appears to be the first paper to apply Jones’s (1995a) methodology to examine the growth effects of taxes. We discuss his evidence further below.

are not strictly exogenous, evidence based on cross-section or static panel approaches may be misleading.

In Kneller, Bleaney, and Gemmell (1999) we sought to deal with the first two of the above problems. In the context of static panel regressions, we showed that complete specification of the government budget constraint and careful attention to fiscal classifications produces dramatically different results for the growth effects of fiscal policy compared with previous investigations. Our results offered strong support for the endogenous growth models of Barro (1990) and others. To facilitate comparisons with earlier evidence, however, Kneller et al. (1999) used static panel regression techniques on five-yearly averaged data and provided only limited testing for endogeneity of fiscal regressors. Dealing with these latter two problems is the primary focus of this paper – do five-year averages capture long-run behaviour or are longer lags required? Also, are ‘static’ results undermined when we allow for dynamic responses and the endogeneity of fiscal policy?

In this paper we investigate these questions using data from a panel of twenty-two OECD countries during 1970–95. We allow for alternative classifications of fiscal variables and consider various methods of estimating the long-run impact, in each case taking care to avoid the afore-mentioned biases to the parameter estimates. Despite numerous sensitivity tests, our results continue to provide strong support for the endogenous growth model. Compared with previously published work, our findings demonstrate greater consistency between theory and empirics. We attribute this to the inclusion of disaggregated revenues *and* expenditures in the model combined with careful attention to the implicit financing assumptions that would otherwise bias the results sufficiently to make a dramatic difference to the investigator’s conclusions. We find that the long-run effects of fiscal policy take longer than five years to come through, and that these effects are *not* due to ‘fiscal endogeneity.’ In line with Jones’s (1995a) finding, investment does appear to be endogenous, however, and its effects on growth will be exaggerated if fiscal policy is ignored.

The remainder of the paper is organized as follows. In Section 2 we summarize the key predictions of recent public policy endogenous growth models and reiterate the implications of the government budget constraint for empirical testing. In Section 3 we then discuss our empirical methodology; in section 4 we present the results for our OECD sample, subjecting them to several endogeneity and specification tests. In section 5 we draw some conclusions.

2. Theory

As is well known, in the neoclassical growth model, if the incentives to save or to invest in new capital are affected by fiscal policy, this alters the equilibrium capital-output ratio and therefore the level of the output path, but not its slope (with transitional effects on growth as the economy moves onto its new path). The novel feature of the public-policy endogenous growth models of Barro (1990), Barro and

Sala-i-Martin (1992, 1995) and Mendoza, Milesi-Ferretti, and Asea (1997) is that fiscal policy can determine both the level of the output path and the steady-state growth rate.³ This is easily seen in the following model from Barro and Sala-i-Martin (1992). There are n producers, each producing output (y) according to the production function:

$$y = Ak^{1-\alpha}g^\alpha, \quad (1)$$

where k represents private capital and g is a publicly provided input. The government balances its budget in each period by raising a proportional tax on output at rate τ and lump-sum taxes of L . The government budget constraint is therefore

$$ng + C = L + \tau ny, \quad (2)$$

where C represents government-provided consumption ('non-productive') goods.⁴ The lump-sum (or non-distortionary) taxes do not affect the private sector's incentive to invest in the input good, whereas the taxes on output do. With an isoelastic utility function, Barro and Sala-i-Martin (1992) show that the long-run growth rate in this model (ϕ) can be expressed as

$$\phi = \lambda(1 - \tau)(1 - \alpha)A^{1/(1-\alpha)}(g/y)^{\alpha/(1-\alpha)} - \mu, \quad (3)$$

where λ and μ are constants that reflect parameters in the utility function. Equation (3) shows that the growth rate is decreasing in the rate of distortionary taxes (τ), increasing in government productive expenditure (g), but is unaffected by non-distortionary taxes (L) or non-productive expenditure (C).

This is the model we seek to test. In practice, we need to take account of the fact that the government budget is not balanced in every period, so the constraint becomes

$$ng + C + b = L + \tau ny, \quad (4)$$

where b is the budget surplus. The predicted signs of these components in a growth regression would be: g – positive; τ – negative; C and L – zero; b – zero provided that Ricardian equivalence holds *and* that the composition of expenditure and taxation remains unchanged.

To see the implications of this for empirical testing, suppose that growth, ϕ_t , at time t is a function of conditioning (non-fiscal) variables, Y_{it} , and the fiscal variables from equation (4), X_{jt} :

$$\phi_t = \alpha + \sum_{i=1}^k \beta_i Y_{it} + \sum_{j=1}^m \gamma_j X_{jt} + u_t. \quad (5)$$

3 Of course, not all endogenous growth models predict long-run growth effects from fiscal policy. The 'semi-endogenous,' R&D-based model of Jones (1995b), for example, yields endogenous growth via R&D activities, but the long-run growth rate depends only on the exogenous rate of population growth.

4 Government consumption goods are defined as those that enter consumers' utility functions but do not enter the production function in (1).

Because of the linear constraint represented by equation (4), we have

$$X_{mt} = - \sum_{j=1}^{m-1} X_{jt}, \quad (6)$$

so one element of X must be omitted in the estimation of equation (5) in order to avoid perfect collinearity. The omitted variable is effectively the assumed compensating element within the government's budget constraint. Thus, for estimation equation (5) must be rearranged to give

$$\phi_t = \alpha + \sum_{i=1}^k \beta_i Y_{it} + \sum_{j=1}^{m-1} (\gamma_j - \gamma_m) X_{jt} + u_t. \quad (7)$$

This shows that the coefficient of X_{jt} should be interpreted as $(\gamma_j - \gamma_m)$ rather than γ_j . In other words, the correct interpretation of the coefficient on each element of the government budget is as the effect of a unit change in the relevant variable *offset by a unit change in the element omitted from the regression* (or some mix of the omitted elements, if there is more than one). To give an example, the coefficient on productive expenditure will tend to be higher if it is financed by non-distortionary taxation rather than by distortionary taxation or by some mixture of the two. The problem is not solved by omitting many elements of the government budget constraint from the regression instead of only one. This is a straightforward point, but one that has frequently been ignored.

The implications for the regression specification are as follows. At the first stage, omit from the regression one of those budget elements for which theory suggests that $\gamma = 0$ (e.g., L) and test whether the hypothesis of a zero coefficient for any other 'neutral' budget elements can be rejected (in this case, C). If the theory passes this test (i.e., the hypothesis is not rejected), then estimate a second regression with both L and C omitted. This should yield the most precise estimates of the fiscal parameters and ensures that the assumed financing is innocuous. If the hypothesis of a zero coefficient for C is rejected at the first stage, this is of course evidence against the model under test. It should be noted that, if this procedure is followed, the budget surplus would be expected to have a positive coefficient even under Ricardian equivalence, because the constraint of neutral financing applies to the current period only (this point is elaborated below).

As we have demonstrated in detail in an earlier study (Kneller, Bleaney, and Gemmell 1998), much previous research has ignored the effect of the implicit financing assumptions on the coefficients of fiscal variables in growth regressions. Only Helms (1985), Mofidi and Stone (1990), and Miller and Russek (1997) have addressed the issue explicitly, although Kocherlakota and Yi (1997) refer to it in interpreting their results.

3. Data and empirical methodology

The theoretical model requires the classification of expenditures into productive and non-productive and of taxation into distortionary and non-distortionary.

Table A1 shows how we do this, using data from the IMF *Government Finance Statistics Yearbook* (GFSY); Kneller, Bleaney, and Gemmell (1998) provide more detail. On the expenditure side, the most important component of the non-productive category is social security. We treat most other expenditure (e.g., transport and communication, education, health) as productive and later test the validity of this aggregation. We classify consumption taxes as non-distortionary, which is valid if the utility function does not contain leisure as an argument (as in Barro 1990). With leisure as an argument in the utility function, consumption taxes will have some impact by distorting the labour-leisure choice. This would leave the category of non-distortionary taxation as an empty set. Since consumption taxes do not distort the choice between consumption at different dates, it seems valid to argue that they should still appear as a separate category on the grounds that they are less distortionary than income taxes.⁵ Whether they are non-distortionary or merely less distortionary than other taxes is a question that can be answered in the empirical estimation. Finally, there are some revenues and expenditures whose classification is ambiguous (we label these 'other revenues' and 'other expenditures'). Since these are small in magnitude, we discuss them only briefly in what follows.⁶

Our annual dataset covers twenty-two developed countries for various periods during 1970–95, from two sources.⁷ Government budget data come from the GFSY; remaining data are from the World Bank Tables. Summary descriptive statistics are given in table A2.⁸ An important issue is how to extract the long-run information from this annual data. One method is to estimate the model in (7) with annual data, but use many lags of the independent variables to pick up long-run effects (as is done by Kocherlakota and Yi 1997 for a century of U.K. and U.S. data). The more commonly chosen alternative is to take averages of five years or more of data in order to iron out cyclical fluctuations, but at the expense of losing degrees of freedom (e.g., Grier and Tullock 1989; Cashin 1995; Devarajan, Swaroop, and Zou 1996; Mendoza, Milesi-Ferretti, and Asea 1997; Kneller, Bleaney, and Gemmell 1999). In section 4 we start by taking five-year averages and allowing for current-period effects only, as most previous authors have done. We then show that this is inadequate, since there is evidence of lagged effects (i.e., fiscal variables in one five-year period have a significant effect in the subsequent as well as in the current

5 In addition, since the income and substitution effects operate in opposite directions in the choice between labour and leisure, the impact of consumption taxation on labour input may, in practice, be small.

6 In Kneller, Bleaney, and Gemmell (1999) we examine alternative classifications and disaggregations of taxes and expenditures and find that results are not sensitive to productive/non-productive or distortionary/non-distortionary definitions. Note, also, that 'other revenues' includes taxes on international trade, which, though they are very small in the OECD and do not tax investment directly, can be expected to distort some investment decisions.

7 For details of country coverage and time periods, see tables 1 and 2.

8 Based on five-yearly averages, our sample countries grew, on average, around 2.8 per cent per capita per annum, with investment ratios in excess of 20 per cent and labour force growth around 1 per cent per annum. Among the fiscal variables, distortionary taxation yields about twice as much revenue as non-distortionary taxes (18.8 per cent of GDP compared with 9.2 per cent), while each of the two main expenditure categories accounts for about 15 per cent of GDP (see table A2).

five-year period), and we estimate a dynamic (five-year) panel that incorporates these effects. Thirdly, we allow the data to determine the appropriate number of lags in an annual dynamic model. The results of this exercise suggest that long-run effects take more than five years to come through.

In each case our regression equations follow the form of equation (7) above, using the two-way fixed-effects model (Least Squares Dummy Variables (LSDV), with time and country-specific intercepts).⁹ Dynamic fixed-effects models, however, are known to generate biased and/or inefficient coefficient estimates arising mainly from the presence of the lagged dependent variable and omitted variable bias associated with the country-specific effects.¹⁰ These biases typically depend on the characteristics of the panel (e.g., its time and cross-section dimensions, the exogeneity of the regressors), which also influences the optimal solution. Following Nickell (1981) and Anderson and Hsiao (1981), a number of instrumental variable (IV) solutions have been proposed to overcome these problems (e.g., Arellano and Bond 1991; Arellano and Bover 1995; Blundell and Bond 1997). Recent applications of these methods to a number of empirical growth models have demonstrated that they can overturn or substantially modify established results from cross-section or static panel methods.¹¹ In section 4.2 we therefore adopt these IV methods to investigate the robustness of our fiscal policy results.

An important issue which these methods attempt to deal with is the endogeneity of the regressors, essentially by exploiting the panel's time-series information to use predetermined variables as instruments. An alternative approach to overcome the paucity of instruments in growth regressions has been suggested by Jones (1995a). He argues that the OLS estimate of the contemporaneous effect of the suspected endogenous variable (investment, in his case) on growth is likely to be the upper bound of its 'true' value, under the assumption that the contemporaneous correlation of investment with growth is positive and therefore likely to bias the coefficient upwards. The lower bound is set to zero, where theory provides unambiguous sign predictions. In our case the potentially endogenous variables are investment and the set of fiscal variables. As we noted above, in each case theory provides unambiguous predictions regarding the direction of growth effects allowing us to treat zero as the lower bound for contemporaneous effects and measuring the long-run growth effects from the sum of the remaining $t - j$ coefficients ($j = 1 \dots T$).¹² For fiscal

9 We experimented with alternative random and fixed-effects estimators and found that the two-way fixed-effects model gave the best fit to the data.

10 Growth regressions typically contain an implicit lagged dependent variable (even if an 'initial income' term is omitted), since they can be rewritten in terms of income *levels* with lagged income as a right-hand-side variable.

11 See, for example, Caselli, Esquivel, and Lefort (1996) on country growth convergence rates; Forbes (1998) on the relationship between inequality and growth; and Levine, Loayza, and Beck (1999) on financial intermediation and growth.

12 In fact Jones (1995a, appendix A) experiments with setting the contemporaneous coefficient to alternative values between the upper and lower bounds. For such intermediate values, however, it is not possible to obtain relevant standard errors on the long-run parameters, since the latter are a mixture of imposed and estimated effects.

variables, however, OLS estimates may not represent the upper bound (in absolute value); in particular, the use of expenditure policy and budget surpluses as automatic stabilizers will likely result in a negative bias to these coefficients. For (distortionary) taxes, since these are generally non-proportional, any bias to the tax coefficient can be expected to be positive.

4. Empirical results

Table 1 (columns 1–3) presents the results when the data are arranged as five-year averages with only current-period effects (i.e., estimated as a static panel). The regression includes the ratio of investment to GDP and labour force growth as conditioning variables, although neither, in fact, is significant in this case. (Below, we investigate the sensitivity of our results to the conditioning set¹³). Following the procedure proposed in section 2, in the first column of the table we use only non-distortionary taxation as the implicit financing element, and in the second column we use non-productive expenditure. Each of these items should have a zero coefficient according to the Barro (1990) model, and the results confirm that each has a coefficient close to zero when the other is omitted, implying that they do, indeed, have very similar coefficients.¹⁴ In the third column, therefore, we omit both non-distortionary taxation and non-productive expenditures, which is equivalent to imposing a common coefficient (which we presume to be zero, according to the theory). Omission of both reduces the standard errors associated with the other fiscal variables, and the coefficients are intermediate between the column 1 and column 2 results, as would be expected.

The signs of the fiscal variables in table 1 are consistent with theory. Productive expenditures have a significant positive coefficient; the point estimate suggests that an increase by one percentage point of GDP raises the growth rate by 0.30 percentage points. Other (unclassified) expenditures also have a significant positive coefficient, which is slightly larger than that of productive expenditure (0.33).¹⁵ Distortionary taxation, on the other hand, significantly reduces growth: its estimated coefficient is -0.46 , while other revenues also have a negative (but smaller and statistically insignificant) effect. The budget surplus has a surprisingly large positive coefficient of 0.39. A positive coefficient is to be expected, since we are constraining the current surplus to finance neutral elements of the budget, which have no growth effects, whereas the compensating future deficits anticipated under Ricardian equivalence may partially finance productive expenditure or cuts in distortionary taxation, raising the anticipated returns to investment and hence growth.

13 Kneller, Bleaney, and Gemmill (1999) show, using period averaged data, that regressions are not sensitive to the inclusion/exclusion of initial GDP. In annual panel regressions below we use lagged growth to capture ‘convergence’ effects.

14 Because of the linear constraint among the fiscal variables, the coefficients of the non-fiscal variables and the residuals are the same for these two columns.

15 This surprisingly strong effect is weakened considerably in the annual panel.

TABLE 1
 Static and dynamic panels with five-year-averaged data

Estimation technique: LSDV: Two-way FE; Five-year averages

Dependent variable: per capita GDP growth

	1: Static	2: Static	3: Static	4: Dynamic
Omitted fiscal variable(s)	Non-distortionary taxation	Non-productive expenditure	Non-dis. taxation Non-prod. exp.	Non-dis. taxation Non-prod. exp.
Investment ratio	0.020 (0.32)	0.020 (0.32)	0.021 (0.35)	0.052 (0.75)
Labour force growth	-0.015 (-0.05)	-0.015 (-0.05)	0.001 (0.00)	0.124 (0.55)
Other revenues	-0.101 (-0.51)	-0.140 (-1.27)	-0.140 (1.28)	-0.095 (-0.69)
Other expenditures	0.301 (1.82)	0.340 (2.86)	0.329 (3.01)	0.305 (2.57)
Budget surplus	0.357 (2.17)	0.400 (4.23)	0.389 (4.41)	0.368 (3.15)
Distortionary taxation	-0.427 (-2.36)	-0.467 (-4.66)	-0.463 (-4.72)	-0.449 (-3.55)
Non-dis. taxation	-	-0.039 (-0.23)	-	-
Productive expenditure	0.273 (1.77)	0.312 (2.31)	0.296 (2.56)	0.233 (1.69)
Non-prod. expenditure	-0.039 (-0.23)	-	-	-
Net lending	0.314 (1.32)	0.353 (1.89)	0.349 (1.89)	0.587 (3.65)
Lagged growth	-	-	-	-0.504 (-2.96)
No. of obs.	98	98	98	76
Adj-R ²	0.574	0.574	0.581	0.660

NOTES

Regressions are based on five-year averages for 1970–95 (Australia, Austria, Canada, Denmark, Finland, Germany, Iceland, Luxembourg, Netherlands, Norway, Spain, Sweden, Turkey, United Kingdom, United States); 1975–95 (France); 1970–90 (Belgium); 1970–85 (Greece, Switzerland); 1975–90 (Italy, Portugal); and 1980–95 (Ireland). Figures in parentheses are *t*-statistics. Country and time intercepts are included in all regressions. Coefficients shown in column 4 are total effects (calculated from current, lagged, and lagged growth parameters). For definitions of fiscal variables see table A1. Time dummies, which were collectively insignificant, are omitted from the dynamic regression in column 4.

This argument would tend to suggest, however, a somewhat smaller positive coefficient than our estimates yield.

These results appear to give strong support to the Barro (1990) model; indeed, fiscal effects are surprisingly strong. Could they, however, be the result of short-run effects persisting in the five-yearly averaged data and/or endogeneity of our investment and fiscal variables? We consider these issues in turn below.

4.1. Long-run effects?

To address the issue of whether five years is long enough to capture long-run effects we start by allowing for lagged effects of fiscal variables from the previous five-year period. The results are shown in the final column of table 1 (regression coefficients are for the sum of current and lagged effects).¹⁶ The numbers are similar (though point estimates are generally smaller for the fiscal variables), but the lagged effects are collectively significant, with a Wald χ^2 test statistic of 25.4 against a 5 per cent critical value of 16.9, implying that a static panel of five-yearly averaged data is not the right specification and is likely to result in the contamination of the long-run effects by short-run responses.

To explore the appropriate lag length more precisely we use the annual observations instead of five-year averages and investigate how many lags are significant. Testing alternative lag structures (again with a Wald χ^2 test) is made on the null of the joint significance of the last lag of the explanatory variables within the regression.¹⁷ We find that eight lags are required: the Wald values for 7, 8, and 9 lags, respectively, are 30.8, 12.7, and 3.4 against critical values (with six degrees of freedom) of 10.6 (10 per cent) or 12.6 (5 per cent).¹⁸ Table 2 presents the results using eight lags (together with results for five-yearly averaged data for a comparable sample).¹⁹ Once again, we cannot reject the hypothesis that non-productive expenditures and non-distortionary taxation have identical coefficients, equal to zero. The main fiscal variables of interest – distortionary taxation, productive expenditures, and the budget surplus – all are significant at the 1 per cent level, with the point estimate for the budget surplus now around 0.1, somewhat lower than the estimated effects for taxes and expenditures. At around 0.4 (in absolute value) both productive expenditures and distortionary taxes continue to exhibit strong growth effects. (Other revenues and expenditures appear to have no discernible growth effects). Investment now appears to have a larger, and statistically significant, estimated effect on growth, compared with those obtained using five-yearly averaged data. As can be seen from column 3, these changes are not due to the smaller sample of countries used in the annual panel regressions – both the seventeen- and the twenty-two-country, period-averaged panels yield similar results.

We noted above, however, that the LSDV technique applied to a dynamic panel may bias coefficient estimates due to endogeneity from the presence of the lagged

16 Omitting each of non-distortionary taxation and non-productive expenditures, in turn, from the regression again supports the omission of both variables.

17 The maximum number of lags that we felt could reasonably be used was nine. Only countries with eighteen or more annual observations were included. Excluded countries are: Greece (ten years), Ireland (eleven), Italy (eleven), Portugal (eleven), and Switzerland (fifteen).

18 These results are obtained using the six fiscal variables in the regression. Similar results are obtained if all variables are included in the lag structure test. It is interesting that both Kocherlakota and Yi (1997) and Jones (1995a) find eight lags are required to capture long-run behaviour in their empirical growth models.

19 All annual dynamic panel regression results are obtained using DPD98; see Arellano and Bond (1998). 'Robust' *t*-statistics (i.e., those consistent in the presence of heteroscedasticity) are reported for the annual panel regressions in table 2.

TABLE 2
Dynamic panel with eight lags of annual data

Estimation technique: *Columns 1 & 2*: LSDV; 2 way FE; annual data
Column 3: five-yearly averaged data; restricted sample

Dependent variable: per capita GDP growth

	Omitted fiscal variable(s)		
	1. Non-distortionary taxation	2. Non-distort. taxation Non-prod. expenditure	3. Non-distort. taxation Non-prod. expenditure
Investment ratio	0.120 (4.21)	0.126 (3.99)	0.079 (1.05)
Labour force growth	-0.350 (-2.67)	-0.226 (-1.51)	0.128 (0.56)
Other revenues	-0.041 (0.32)	0.040 (0.63)	0.029 (0.18)
Other expenditures	0.013 (0.10)	0.040 (0.59)	0.283 (2.10)
Budget surplus	0.109 (1.12)	0.105 (2.07)	0.368 (3.15)
Distortionary taxation	-0.393 (-2.92)	-0.411 (-6.18)	-0.478 (-3.49)
Productive expenditure	0.337 (3.67)	0.387 (4.88)	0.316 (2.20)
Non-prod. expenditure	0.045 (0.49)	-	-
Net lending	-3.865 (-4.03)	-3.44 (-2.58)	0.612 (3.63)
Lagged growth	-2.062 (-9.11)	-1.336 (-6.40)	-0.472 (-2.51)
No. of obs.	237	237	66
Adj-R ²	0.758	0.723	0.626

NOTES

Regressions are based on annual data for seventeen of the twenty-two countries used in table 3. (Greece, Ireland, Italy, Portugal, and Switzerland are omitted, owing to short time-series). Column 3 of the table shows results for five-yearly averaged data for the comparable data set of seventeen countries. Heteroscedasticity-robust *t*-statistics are shown in parentheses. Coefficients are total effects (current plus lagged, adjusted for lagged growth). Country and time intercepts are included. For definitions of fiscal variables see table A1.

dependent variable and/or endogenous regressors. The relatively large parameter estimates on investment and lagged growth in table 2 are suggestive that this may be the case. We proceed to test for this in the following section.

4.2. Endogeneity

Perhaps the problem most often discussed when referring to growth regressions is the potential endogeneity between the right-hand-side variables and the growth rate. Obvious candidates in this case are investment and the fiscal variables. As we noted earlier, this problem is compounded in dynamic regressions involving a lagged

growth rate. In this section we consider possible IV estimators to deal with these problems, together with the Jones (1995a) approach, which attempts to identify 'bounds' for the true value of the suspected endogenous parameter.

Recent developments in dynamic panel data models (e.g., Arellano and Bond 1991; Arellano and Bover 1995; Kiviet 1995) have focused mainly on those applicable to micro data sets, which generally have a large cross-section dimension but a limited time-series dimension (large N , small T). These properties also typically match well the dimensions of growth data sets based on five-year averages – T around 5, N up to around 100 – and, as we noted earlier, their application has led to important challenges to existing growth empirics.²⁰ For our annual panel however, $T > N$ ($N = 17$; $T = 18$ to 24), a characteristic more common among macro data sets; as Judson and Owen (1999) have shown, the appropriate method for correcting endogeneity bias differs with the time dimension of the panel. For panels with a time dimension similar to that here ($T = 20$) they find (i) OLS and LSDV estimators lead to average biases to the parameter on the lagged dependent variable around 13 per cent and up to 3 per cent on the (presumed) exogenous regressors; and (ii) the Anderson-Hsiao (AH) estimator consistently performs best (minimum bias) for panels with a larger time dimension.²¹ We therefore use the AH IV-estimator in this section. This involves first differencing the growth regression in (7) and instrumenting the first difference of the lagged dependent variable with lagged levels.²² Results for the variables of interest – investment, the six fiscal variables, and lagged growth – are shown in table 3, which also reports results using the Jones (1995a) method of constraining the contemporaneous parameters on the variables suspected of endogeneity. Two constrained values are selected in applying this method: (i) half the LSDV estimate in table 2; (ii) zero (the LSDV result is repeated for comparison).

The AH result in column 1 confirms the upward bias on the lagged growth parameter using LSDV, but it is important to note that this does not substantially affect the long-run estimates of the main fiscal variables: strong negative, statistically significant effects of distortionary taxation and similar positive effects of productive expenditures are confirmed.²³ Similar conclusions emerge from the Jones

20 Kneller, Bleaney, and Gemmell (1999) have also applied a variant of the Arellano and Bond (1991) approach to their five-yearly averaged data for fiscal variables. Though standard errors increased, parameter estimates for distortionary taxation and productive expenditures were not much affected.

21 Judson and Owen (1999) compare five panel data estimators: the LSDV; the LSDV 'correction' (LSDVc) proposed by Kiviet (1995); the Anderson-Hsiao estimator (AH); and two Arellano-Bond generalized method of moment estimators (GMM1, GMM2). Their results suggest that LSDVc performs best for T small and AH performs best for larger T . Both consistently outperform LSDV and GMM1 or 2.

22 As Judson and Owen (1999) note, AH is effectively a special case of the Arellano and Bond GMM estimators, where the latter use all possible lagged values (of the dependent variable and exogenous regressors) as instruments. For panels with $T > N$, however, this can lead to over-identification of the system.

23 Note that, *ceteris paribus*, any upward bias in the lagged growth parameter using LSDV methods will impart a *downward* bias to the long-run parameter estimates of the exogenous variables in the regression. This could mitigate or enhance any biases associated with tax or expenditure variables, owing to the 'non-proportionality' or automatic stabilizer effects mentioned earlier.

TABLE 3
Annual dynamic panels: instrumental variables

Dependent variable: per capita GDP growth				
Estimation technique:	1: AH	2: Jones ($\gamma_t = \text{LSDV}/2$)	3: Jones ($\gamma_t = 0$)	4: LSDV
Investment ratio	0.105 (1.09)	0.094 [-0.37]	0.020 (1.31)	0.126 (3.99)
Labour force growth	0.069 (0.34)	-0.032 [-1.60]	0.083 (0.05)	-0.226 (-1.51)
Other revenues	0.096 (0.86)	0.044 [2.61]	0.051 (3.35)	0.040 (0.63)
Other expenditures	-0.119 (-1.20)	0.108 [1.58]	0.191 (0.39)	0.040 (0.59)
Budget surplus	0.103 (0.99)	0.173 [0.30]	0.143 (7.15)	0.105 (2.07)
Distortionary taxation	-0.408 (-4.64)	-0.426 [-3.21]	-0.442 (-6.63)	-0.411 (-6.18)
Productive expenditure	0.300 (2.94)	0.418 [4.70]	0.467 (5.50)	0.387 (4.88)
Net lending	-1.745 (-0.91)	-3.13 [2.58]	-2.64 (-0.71)	-3.44 (-2.58)
Lagged growth	-0.644 (-1.74)	-1.417 [-4.67]	-1.15 (-5.40)	-1.336 (-6.40)
No. of obs.	237	237	237	237
Adj-R ²	0.657	0.887	0.385	0.723

NOTES

AH = Anderson-Hsiao IV estimator; 'Jones' = Jones (1995a, appendix A) approach; t -statistics are in parentheses. $\gamma_t = \text{LSDV}/2$ ($\gamma_t = 0$): the values of the parameters on the current lag (γ_t) for all fiscal variables and investment are set to half their LSDV estimated values (zero). Parameter values shown are the long-run values (i.e., the sum of γ_t and the estimates for the $t - j$ lags ($j = 1 - 8$), adjusted by the lagged growth parameters. Standard errors/ t -statistics cannot be calculated for the ($\gamma_t = \text{LSDV}/2$) case because of the imposed component within the long-run parameters. The [t -statistics] shown in column 2 refer to the test of the significance of the long-run coefficient for lags 1 to 8, that is, it does not include any contemporaneous effect.

methodology in columns 2 and 3. Indeed, allowing *no* contemporaneous correlation (column 3) reveals larger and more robust effects from distortionary taxes and productive expenditures compared with LSDV results. Estimates of the growth effects of budget surpluses are also similar to (or larger than) those obtained using LSDV, though the AH standard errors appear somewhat larger. Evidence on the endogeneity of investment is less clear cut: parameter estimates are only slightly reduced when IV techniques are used, except in the case where a zero contemporaneous effect is imposed. Standard errors are again larger, however, so that positive growth effects of investment cannot be confirmed. It is clear that the major impact on the long-run investment parameter is *contemporaneous* (the long-run parameter falls to 0.02 in column 3), casting doubt on the exogeneity of investment and in line with Jones's (1995a) results.

4.3. Specification testing

The lack of robustness of fiscal effects on growth in previous studies is apparent from the wide range of estimates. Easterly and Rebelo (1993), for example, found their fiscal estimates were sensitive to the exclusion of an initial income term from their growth regressions.²⁴ In this section we examine the robustness of our earlier results to three changes: (i) the set of conditioning variables; (ii) the countries in the sample; and (iii) the classification of productive expenditures. In all cases, though fiscal parameters change somewhat, our main results are robust, with consistent evidence of long-run adverse effects on growth from distortionary taxation but growth-enhancing effects from productive expenditures.

4.3.1. Alternative conditioning variables and samples

Table 4 (columns 1–3) reports results omitting investment, labour force growth, and lagged growth,²⁵ and it is clear that the inclusion of these variables is not responsible for the strong fiscal effects we identified earlier. Indeed, parameter estimates for distortionary taxation and productive expenditures are larger (in absolute value) and more significant when conditioning variables are omitted, compared with the counterpart regression in table 3 (column 4). Similarly, the inclusion of a terms of trade index in column 5 (which Mendoza, Milesi-Ferretti, and Asea (1997) found to be a significant growth determinant) has minimal impact on fiscal-growth estimates.

Table 4 also includes results for the case where fiscal variables are omitted. As might be expected, investment now appears to have strong positive effects on growth. Together with our earlier results this suggests a simultaneous relationship between investment and fiscal policy, which causes erroneously strong growth effects to be attributed to investment when fiscal policy variables are omitted.²⁶

Though the OECD is usually regarded as a fairly homogeneous country grouping, it nevertheless includes some relatively underdeveloped countries (e.g., Turkey) and table A2 reveals a fairly wide cross-country dispersion in the values of some fiscal and other variables. As we showed in tables 1 and 2, however, reducing the sample to seventeen countries (omitting Greece, Ireland, Italy, Portugal, and Switzerland) had no substantive effect on our results. Examining our remaining sample for outliers and using the regression residuals from column 2 in table 2 revealed only eight residuals (out of 237) outside a two standard error confidence interval and none larger than 2.5 times the standard error. Germany and the United Kingdom each have two such residuals and no country has more than two ‘outlying’

24 In Kneller, Bleaney, and Gemmill (1999) we tested for this using a comparable dataset and found that our results were *not* sensitive to the inclusion or otherwise of initial income.

25 Note that the inclusion of lagged growth in the annual panel regressions is analogous (but not identical) to the inclusion of initial income in cross-section or static panel regressions. Our conclusions are also unaffected by adding initial income to the regressions in table 1 (and lagged initial income in column 4).

26 Applying Jones’s (1995a) method to this regression, with contemporaneous effects set to zero, again leads to a long-run investment parameter that is both small and insignificantly different from zero, further suggesting that investment is endogenous.

TABLE 4
Altering the conditioning set

Estimation technique: LSDV: two-way FE; annual data

Dependent variable: per capita GDP growth

Omitted variables	1: Investment	2: Investment, lab. force growth	3: Investment, lab. force growth, lagged growth	4: All fiscal variables	5: Including terms of trade
Investment ratio	–	–	–	0.257 (3.52)	0.074 (1.14)
Labour force growth	0.064 (0.37)	–	–	–0.067 (–0.20)	–0.062 (–0.23)
Terms of trade inverse	–	–	–	–	–18.54 (–1.72)
Other revenues	–0.019 (–0.23)	–0.058 (–0.58)	–0.370 (–1.83)	–	0.145 (1.31)
Other expenditures	0.105 (1.56)	0.189 (2.26)	0.528 (2.85)	–	0.121 (1.15)
Budget surplus	0.269 (5.00)	0.283 (4.35)	0.485 (3.30)	–	0.203 (2.45)
Distortionary taxation	–0.522 (–7.91)	–0.539 (–7.65)	–0.854 (–6.17)	–	–0.455 (–6.25)
Productive expenditure	0.405 (5.89)	0.341 (4.55)	0.598 (3.50)	–	–0.302 (3.64)
Net lending	–3.700 (–3.31)	–1.333 (–1.09)	–0.010 (0.003)	–	–3.237 (–2.00)
Lagged growth	–1.696 (–5.55)	–1.254 (–4.69)	–	–0.487 (–2.92)	–1.179 (–3.65)
No. of obs.	237	237	237	237	226
Adj-R ²	0.656	0.616	0.588	0.403	0.702

NOTES

t-statistics are shown in parentheses. Non-productive expenditures and non-distortionary taxes are also omitted from all regressions. The terms of trade variable is obtained from the Penn World Table 5.6 and is defined such that an increase in the index represents a *worsening* of the terms of trade. Limited coverage for this variable reduces the sample size in column 5.

residuals.²⁷ In Table 5 we omit, in turn, Germany, the United Kingdom, Turkey (the poorest country in the sample), the United States, and Sweden (which have among the smallest and largest public sectors, respectively).²⁸ It is clear that inclusion/exclusion of these countries does not substantively affect the fiscal results. For example, the parameter on distortionary taxation is always significantly negative and falls in the range -0.41 to -0.45 (except when Turkey is excluded, when the parameter becomes -0.54); and productive expenditure is always significantly positive, in the range 0.34 to 0.40 .

4.3.2. Classification of expenditures

Finally, certain types of expenditure that we have classified as productive might be considered to include elements of consumption. Health and education are obvious candidates. Accordingly, we investigated the effect of separating health and education expenditures from other productive expenditures. The coefficient on health expenditures is found to be virtually identical to that for other productive expenditures (at 0.33), while the coefficient of education expenditures is about 50 per cent greater (at 0.48 , although the *difference* is not significant), suggesting that all three forms of expenditure have similarly productive effects on growth.

4.4. Comparisons with previous results

The robustness of these results contrasts quite markedly with those in the published literature. Comparable recent studies, using static panel techniques are Mendoza, Milesi-Ferretti, and Asea (1997) and Devarajan, Swaroop, and Zou (1996).²⁹ Using data from eleven OECD countries over the period 1965–91, Mendoza et al., found estimated effective tax rates on capital, consumption and labour all to be insignificant in a growth regression using five-yearly averaged data, but significant (although not robustly so) in some regressions using annual data. They conclude from this that taxation has short-run effects on the growth rate but no long-run effect – consistent with the neoclassical rather than the endogenous growth model. Devarajan et al. (table 2) use data from twenty-one developed countries from 1970 to 1990 to study the growth effects of the expenditure mix. They find consistently positive (but not always statistically significant) effects of total government expenditure on growth.³⁰ In each of these cases, however, the estimation procedure incorporates a bias towards a statistically insignificant result, either because the taxes are implicitly part-financing productive expenditures that will offset the negative growth impact of distortionary taxation (Mendoza et al. 1997), or because the expenditures are

27 A number of alternative ‘outlier tests’ are available to test individual parameters or the regression as a whole (see Belsley, Kuh, and Welsch 1980).

28 To conserve space we focus on the fiscal parameters; parameters on other variables are not substantively affected.

29 Devarajan, Swaroop, and Zou (1996) however, use a five-year forward-moving average for their dependent variable to help to deal with endogeneity issues.

30 Their results for different components of expenditure suggest that health should be classified as productive expenditure and education as unproductive.

TABLE 5
Alternative samples

Estimation technique: LSDV; two-way FE; annual data

Dependent variable: per capita GDP growth

	1: Germany omitted	2: UK omitted	3: Turkey omitted	4: US omitted	5: Sweden omitted
Investment ratio	0.156 (3.28)	0.152 (3.15)	0.078 (1.61)	0.175 (3.37)	0.155 (2.90)
Labour force growth	-0.544 (-2.15)	-0.050 (-0.27)	-0.074 (-0.36)	-0.302 (-1.54)	-0.068 (-0.35)
Other revenues	0.083 (1.03)	0.058 (0.68)	-0.017 (-0.21)	0.036 (0.44)	0.010 (0.10)
Other expenditures	-0.025 (-0.37)	0.052 (0.74)	0.077 (0.88)	0.052 (0.78)	0.024 (0.29)
Budget surplus	0.244 (4.05)	0.199 (3.59)	0.200 (3.63)	0.169 (3.03)	0.165 (2.43)
Distortionary taxation	-0.413 (-6.61)	-0.421 (-6.33)	-0.542 (-5.19)	-0.449 (-5.93)	-0.425 (-5.83)
Productive expenditure	0.342 (5.21)	0.404 (5.42)	0.339 (4.72)	0.406 (5.20)	0.373 (4.46)
Net lending	-3.575 (-3.68)	-3.663 (-3.22)	-3.726 (-2.44)	-3.516 (-3.03)	-3.300 (-2.75)
Lagged growth	-2.170 (-5.31)	-1.694 (-4.99)	-1.930 (-5.59)	-1.765 (-5.12)	-1.678 (-5.15)
No. of obs.	237	237	237	237	237
Adj-R ²	0.722	0.701	0.668	0.693	0.689

NOTES: see notes to table 4.

partly financed by distortionary taxation (Devarajan et al. 1996). We have shown elsewhere that these biases to estimation results are substantial (Kneller, Bleaney, and Gemmell 1999), and this is the most likely explanation for the difference between our results and theirs. From the evidence in this paper, differences do not appear to be due to the use of annual versus period-averaged data.

Two studies using essentially time-series methods also find contradictory results. Kocherlakota and Yi (1997), using long time-series for the United States and United Kingdom, find insignificant tax effects when public capital expenditures are excluded from the regression, but significant tax effects when they are included (in which case the tax parameter no longer reflects implicit financing of growth-enhancing expenditures). In other words, their evidence is consistent with ours when they come close to specifying the government budget constraint fully. An alternative approach is followed by Karras (1999), who adopts the Jones (1995a) method discussed above, but where taxes, rather than investment, are the proposed source of endogenous growth. For a sample of eleven OECD countries over 1960–92, Karras finds that GDP growth rates are stationary, while (total) tax rates appear to be non-stationary (and increasing over the long run) and therefore incapable of explaining the long-run behaviour of GDP. As Karras (1999) notes, however, this evidence *would* be consistent with a model where taxes are the source of endogenous growth, *if* there was some countervailing, non-stationary influence on growth. Given the government budget constraint, our evidence is suggestive that such a factor may be productive public expenditure. As public expenditure ratios rise in OECD countries, financed by rising tax ratios (especially for distorting taxes such as income tax), our evidence suggests that these ratios can be expected to exert similar, but opposite, effects on GDP growth. Thus, time-series models that include an overall tax rate as the only fiscal variable are also likely to be misspecified.

5. Conclusions

Endogenous growth theory predicts that the impact of fiscal policy on growth depends on the structure as well as the level of taxation and expenditure. Any empirical test of this theory needs to address the implications of the linear relationship between the elements of the government budget. This constraint implies that the coefficient of any budget element must be interpreted as the impact of a unit increase in that element financed by a unit change in the element or elements omitted from the regression. Using an OECD data set, we have found that, when financed by a mixture of non-productive expenditures and non-distortionary taxation, productive expenditures raise the growth rate and distortionary taxes reduce it, in accordance with the predictions of the Barro (1990) model. A budget surplus financed in this way also raises the growth rate, which is consistent with the assumption of Ricardian equivalence because the anticipated compensating future deficits may partially finance growth-enhancing productive expenditures or cuts in distortionary taxation. Our results also suggest that consumption taxation can realistically be regarded as non-

distortionary, rather than as merely less distortionary than income taxation. When education and health were separated out, we could find no evidence that they have any smaller impact on growth than other productive expenditures.

A primary objective of this paper has been to test the robustness of these results to alternative methods of isolating long-run effects, either by taking five-year averages (with current-period effects only or with current and lagged effects), or by estimating the model on the original annual data but with long lags. We found that period averaging, the most common method employed in previous studies, does not appear to capture long-run responses fully, but both approaches produce consistent evidence in favour of fiscal effects on growth.

We have also investigated the robustness of our results to potential endogeneity of the fiscal regressors, to sample selection, and to regression specification. Again, in all cases we continue to find evidence in favour of the predictions of the endogenous growth model. The high degree of theory-consistency of the empirical results contrasts sharply with the more ambiguous findings that characterize much of the previous literature. At least some of this ambiguity must be attributed to misinterpretation of regression results by authors who ignore the implicit financing assumptions imposed, or who omit 'non-neutral' financing elements from their specifications.

Appendix: Data sources and characteristics

Data are available for twenty-two OECD countries from 1970. This time span is not consistent across countries; for example, data for Switzerland exists only for 1970–84. The fiscal data used in this paper are collated from IMF, *Government Financial Statistics Yearbook*. The data used in the study refer to expenditure by function (table A2) and the breakdown of revenues and grants (table A1). All fiscal variables are expressed as percentages of GDP. In accordance with usual practice, the growth rate is taken as the log difference between annual per capita GDP figures taken from the World Bank CD ROM. The investment rate and the labour force growth rates were taken from the same source.

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TABLE A1
Functional/theoretical classifications

Theoretical classification	Functional classification
Budget surplus	Budget surplus
Distortionary taxation	Taxation on income and profit Social security contributions Taxation on payroll and manpower Taxation on property
Non-distortionary taxation	Taxation on domestic goods and services
Other revenues	Taxation on international trade Non-tax revenues Other tax revenues
Productive expenditures	General public services expenditure Defence expenditure Educational expenditure Health expenditure Housing expenditure Transport and communication expenditure
Unproductive expenditures	Social security and welfare expenditure Expenditure on recreation Expenditure on economic services
Other expenditure	Other expenditure (unclassified)

TABLE A2
Summary descriptive statistics

Variable	Mean	Standard deviation	Minimum (country)	Maximum (country)
GDP p.c. growth (% p.a.)	2.79	1.66	1.54 – Switzerland	5.09 – Turkey
Investment	22.06	3.61	18.11 – UK	29.43 – Portugal
labour force growth (% p.a.)	1.06	0.80	–0.06 – Germany	2.06 – Iceland
Budget surplus	–3.08	3.39	–11.76 – Portugal	1.65 – Luxembourg
Net lending	1.22	1.39	0.11 – Ireland	4.49 – Norway
Distortionary taxation	18.76	7.25	7.10 – Iceland	33.47 – Netherlands
Non-distortionary taxation	9.15	4.22	0.96 – US	16.77 – Norway
Other revenues	4.56	2.96	1.51 – Germany	16.72 – Ireland
Productive expenditure	14.69	4.57	7.35 – Canada	23.74 – Italy
Non-productive expenditure	15.24	6.05	4.96 – Turkey	24.31 – Luxembourg
Other expenditures	4.44	3.07	0.98 – Finland	9.16 – Ireland

NOTES: All variables are percentages of GDP, except where stated, and are based on five-year averages of data.

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