Forthcoming *European Journal of Political Economy*

Growth and the Public Sector: A Critique of the Critics*

by

Stefan Fölster[1] & Magnus Henrekson[2]‡

Final version, June 10, 1998

Abstract: In a recent review article Jonas Agell, Thomas Lindh and Henry Ohlsson (1997) claim that theoretical and empirical evidence does not allow any conclusion on whether there is a relationship between the rate of economic growth and the size of the public sector. They illustrate their conclusion with simple cross-country regressions where the relation between growth and public expenditure tilts from negative to positive when control variables are introduced. In our article we argue that Agell, Lindh and Ohlsson base their conclusion on empirical studies, and on their own regressions, without evaluating the econometric problems that arise. We extend Agell *et al.*´s review in order to highlight some of these problems. Furthermore, we present evidence showing that once a number of econometric issues are dealt with the relationship between growth and public expenditure may be more robustly negative than it first appears.

JEL Classification: E62, H20, H50, O23, O40.

Keywords: Economic growth, Government expenditure, Public sector, Taxation, Cross-country regressions.

[1]IUI, The Research Institute of Industrial Economics
Box 5501
S-114 85 Stockholm
SWEDEN
Phone: +46-8-783 84 36
Fax: +46-8-661 79 69
e-mail: stefanf@iui.se

‡Corresponding author
[2]IUI, The Research Institute of Industrial Economics
Box 5501
S-114 85 Stockholm
SWEDEN
Phone: +46-8-783 84 13
Fax: +46-8-661 79 69
e-mail: magnush@iui.se

* We are grateful for skilful research assistance from Per Thulin and Pavlos Petroulas and for useful comments and suggestions from an anonymous referee, Pontus Braunerhjelm, Ulf Jakobsson, Assar Lindbeck, Erik Mellander and Mats Persson. Needless to say, the usual caveats apply. Financial support from the *Swedish Council for Research in the Humanities and Social Sciences* (HSFR) is gratefully acknowledged.

1. Introduction

The conventional wisdom of the neoclassical model has been that fiscal variables such as the level of taxation and the level of government spending can affect the level of income but have no impact on the rate of economic growth in the long run. However, the explosive development in the theory of economic growth following Paul M. Romer‘s (1986) article on endogenous growth has also stimulated a great deal of empirical work on the determinants of economic growth. In particular, the influential article by Robert J. Barro (1991a), using a data set covering a large cross-section of both rich and poor countries, appeared to present strong empirical evidence favoring the view that a large public sector is growth-impeding.

At the same time, several recent review articles of the effect of government spending on economic growth have come to the conclusion that no negative effect can be confirmed. In a recent review article in this journal Jonas Agell, Thomas Lindh and Henry Ohlsson (1997, p. 33), henceforth called ALO, conclude that "the theoretical and empirical evidence…is found to admit no conclusion on whether the relation is positive, negative or non-existent".[1] Atkinson (1995, p. 196)) infers that "study of the aggregate relationship between economic performance and the size of the Welfare State is unlikely to yield conclusive evidence." Slemrod (1995, p. 401) places greater emphasis on the econometric problems in most studies, concluding that "there is no persuasive evidence that the extent of government has either a positive or a negative impact on either the level or the growth rate of income, largely because the fundamental problems of identification have not yet been adequately addressed."

The purpose of the present paper is to complement the reviews of the literature, taking issue primarily with the review by ALO. Most of the empirical studies they review suffer from econometric problems. Apart from the simultaneity problem noted by Slemrod, there are problems with the selection of countries, with inefficient use of data, and, notably, with heteroscedasticity between countries. It is therefore no surprise that different studies arrive at different conclusions. ALO report the conclusions of various studies with little evaluation of the extent to which econometric problems are dealt with, and in their own regression analyses they do not address any of the econometric problems that arise. This approach creates a bias toward agnostic conclusions.

In our view, a meaningful review should evaluate the empirical work in the light of the various econometric issues. In an attempt to fill this lacuna we focus on four main

[1]The authors hedge their conclusion by saying that "we have not shown in a scientific sense that the tax share and the public expenditure share lack importance for growth" (p. 47).

themes. First, we argue that the neglect of econometric issues can be traced back to ALO‘s review of theory. Their theoretical review focuses on the growth effects of taxes and certain growth-promoting public programs, largely ignoring potentially negative growth effects of public programs. Second, considering potential growth effects of public programs has implications for the selection of countries in empirical studies. Studies that mix rich and poor countries, or studies that use OECD membership as a sample of rich countries may be problematic. Third, few studies make any attempt to address simultaneity problems. Each of these aspects are dealt with in a separate section (sections 2–4). In section 5 we then present our own empirical analysis where we repeat ALO‘s regressions, addressing a number of the econometric issues. We find a tendency towards a more robust negative growth effect of large public expenditures, compared to the case when essential econometric problems are ignored. Section 6 concludes.

We do not claim that our regressions are sufficient to conclusively settle the controversial issue of the effect of public spending on growth. Clearly, it would be preferable to base conclusions on microeconomic evidence concerning, for example, how taxes affect the choice between household work and market work or how saving is affected by the design of the social security system. For the time being, however, there does not exist a coherent framework for summing up the growth effects of such bottom-up investigations. Until such a framework is developed, we believe that top-down studies will remain an important source of knowledge enhancement in the field. Our overall conclusion is that this source has not yet been tapped to its full potential.

2. Growth Effects of Public Expenditure

ALO‘s review of theory leads them to conclude that there is an “abundant selection of competing models“ allowing all kinds of conclusions about the relationship between growth and the size of the public sector. ALO arrive at their conclusion after briefly discussing the literature on growth effects of taxation, and more extensively discussing positive growth effects that have been claimed for some types of public expenditure such as education and public R&D. In our view, this review is not balanced because the various programs that have been hypothesized in theoretical work to have positive growth effects – e.g., schooling, infrastructure and R&D subsidies – typically amount to less than one-fifth of public expenditure in OECD countries (see *Table 1*). This means that more than 80 per cent of public expenditure in OECD countries consists of expenditure that has not been claimed to have positive growth effects. Moreover, most of the variance in public expenditure between countries is explained by differences in public expenditure that has not been claimed to have positive growth effects.

ALO do not discuss potential growth effects of these 80 per cent or so of public expenditure, even though there is an extensive literature indicating that many public programs may have negative effects on saving and capital accumulation, and create marginal effects in addition to those that emanate from the tax system. Since this has important implications for the interpretation of empirical studies we believe it is important to complement ALO's review in this respect.

Table 1 Productive Government Spending as a Share of Total Spending in Selected OECD Countries, 1985 or Closest Available Year (per cent).

Country	Subsidies to R&D (i)	Education (ii)	Transport & Communic. (iii)	i + ii + iii
Australia	1.4	11.8	4.3	17.5
Austria	0.4	8.4	1.0	9.8
Belgium	0.7	10.5	1.6	12.8
Denmark	0.6	9.3	1.3	11.2
Finland	1.1	11.4	2.1	14.6
France	2.0	9.6	1.2	12.8
Germany	1.6	8.6	1.2	7.4
Iceland	1.2	10.7	3.9	15.8
Norway	1.1	10.5	3.3	14.9
Spain	0.4	6.0	1.0	11.4
Sweden	0.8	8.9	1.4	11.1
United Kingdom	1.9	9.6	1.2	12.7

Note: The selection of countries was governed by data availability.
Source: Total Government Spending, Education and Transport & Communcation are from *OECD National Accounts, Vol. II*. Subsidies to R&D are taken from *OECD Basic Science and Technology Statistics*, 1991.

Admittedly, many studies that find economic disincentives created by public programs do not themselves draw conclusions about consequences for economic growth. In particular, the older studies tend to interpret their findings in terms of the traditional Solow model where increased taxation as well as increased saving and investment (equal by assumption in the model) only have transitory effects on the rate of growth, while the economy moves toward the new higher steady-state equilibrium.[2] In this type of model growth depends solely on exogenous technological change, leaving no role for

[2]However, it should be noted that these transitory effects can be of substantial duration. Barro and Sala-i-Martin (1995) estimate that it takes 25–35 years to eliminate one-half of the deviation from the steady state.

changing economic policies and institutions in explaining changing long-run growth rates.

In contrast, the rapidly expanding literature on endogenous growth highlights the fact that if productivity (measured per capita, per hour worked, *et cetera*) is to increase year after year, the economy must continuously provide the workforce with more “tools“. By tools the theorists mean a very broad concept of reproducible capital including physical capital, human capital and knowledge capital (technology). In these models reproducible capital exhibits nondecreasing returns to scale. This can be achieved in a number of ways, e.g., by a human capital accumulation externality (Lucas, 1988) or an externality provided by the aggregate stock of knowledge (Romer, 1990).

Thus, growth is driven by accumulation. But even more importantly, accumulation is the result of deliberate investment by private, profit-motivated firms and individuals. Hence, endogenous growth theory directs our attention to the only way by which government can affect long-run growth, namely via its impact on investment in machines, skills and technology. To the extent that capital and labor taxation deter such investments they reduce growth.[3] Similarly, public expenditures that deter such investments by creating additional marginal tax wedges over and beyond those induced by the taxes required to finance these programs, or that reduce incentives to save and accumulate capital in other ways, reduce growth in these models.

Examples of studies finding such additional marginal tax wedges or reduced incentives to accumulate capital abound. Since the differences in public expenditure between OECD countries are to a large extent determined by differences in the size of transfer programs, we therefore give some examples, focusing on effects of transfer programs.[4] For instance, Hubbard, Skinner and Zeldes (1995) find that social assistance discourages saving because it is usually conditional on the individual not having any assets.[5] Another mechanism is that countries where public expenditure is high also tend to channel considerable transfers to ailing industries. Such transfers have also been shown to have negative growth effects – see, e.g., Leonard and Audenrode (1993). A third, admittedly more speculative, mechanism worthy of attention is that in the case of

[3]Taxes on labor are for the most part a tax on the rate of return on human capital. See, for example, Mankiw, Romer and Weil (1992), who estimate that 50–70 per cent of labor income can be considered as return on human capital.
[4]This does not mean that we deny that transfers may also have positive effects. Sala-i-Martin (1997a) constructs a model where redistributional transfers are modeled as a mechanism to buy poor people out of disruptive activities such as crime and revolutions.
[5]Other studies find negative effects on precautionary saving of more generous unemployment insurance. See, for example, Engen and Gruber (1996).

a large public sector the potential profits from rent-seeking activities are larger, which may lead to a greater diversion of resources into unproductive use (Buchanan, 1980).[6]

A fourth mechanism focuses on the fact that countries with large transfer programs tend to have pay-as-you-go systems. These tend to lower national savings and investment compared to funded systems (Feldstein, 1996).[7] Also, the rate of return tends to be higher in a funded system, where it equals the return on capital, than in a pay-as-you-go system, where the rate of return equals the rate of real wage growth. A lower rate of return implies that the same benefits require more tax revenue, and thus the deadweight loss due to taxation is higher.

A negative relationship between public expenditure on social security and welfare and private saving has been established at the macro level by a number of studies (Edwards, 1995; Feldstein, 1974, 1995). Furthermore, there is the literature that has followed in the wake of the puzzle raised by Feldstein and Horioka (1980) regarding the strong correlation between saving and investment across countries.[8]

This connection is also confirmed in micro-economic studies, showing for example that countries with high taxes and extensive transfer programs tend to have lower household savings and a smaller share of entrepreneurs.[9] Entrepreneurial investment should be largely determined by access to financing and expectations of profitability. A great deal of evidence suggests that potential entrepreneurs are constrained by limited access to capital.[10] This means that welfare state systems that replace individual saving by pay-as-you-go social security may limit entrepreneurial activity.

We readily recognise that initially, public expenditure is likely to have a positive effect on the level and rate of growth of income. At low levels of government spending (on productive goods) and taxation, the productive effects of public goods are likely to

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[6]See Nitzan (1994) for a recent review of the rent-seeking literature.
[7]In theoretical models pay-as-you-go systems can be shown to have no effect on saving (Barro, 1974), that it depresses saving but not growth (Diamond, 1965), or that it depresses both saving and growth (Saint-Paul, 1992). Thus the effect of pay-as-you-go systems on savings has to be settled empirically.
[8]One explanation, consistent with our story, has been presented by Barro, Mankiw and Sala-i-Martin (1995) in a model where some investments, particularly investments in human capital, cannot be financed by foreign borrowing.
[9]E.g., Fölster and Trofimov (1996).
[10]Access to financing in the presence of moral hazard depends on the size of the entrepreneur‘s collateral. Lower individual incentives to save mean that fewer people have access to a collateral. Aghion and Bolton (1992) provide an overview of studies exploring such mechanisms. Lindh and Ohlsson (1998) also show that insufficient private wealth can be detrimental to entrepreneurship. A more equal wealth distribution is typically an outcome of a more extensive welfare state.

exceed the social cost of raising funds.[11] This is the message of the models by Barro (1990) and Slemrod (1995). There are also empirical studies estimating the growth effects of different types of spending. For instance, Barro and Sala-i-Martin (1995) and Hansson and Henrekson (1994b) found a significant positive effect on growth from educational spending and Aschauer (1989) found that public investment in infrastructure had a positive effect on the growth rate in the United States. However, when government expenditure is increasingly channeled to transfer payments and taxes are raised commensurately, there are theoretical reasons to believe that there is a point where higher government spending begins to have a negative effect on growth.

This also throws a different light on a number of recent studies that attempt to estimate the negative growth effects from increased taxation with simulations. King and Rebelo (1990), for example, simulate that an increase of the income tax from 20 to 30 per cent would have a very strong negative effect on the rate of growth of the U.S. economy.[12] In other models the simulated effects are smaller, but still negative (e.g., Lucas, 1990). If, as we have argued above, the larger share of public expenditure creates additional disincentives to accumulation, these simulations should underestimate the true effects of higher taxes. Furthermore, these simulations focus on the U.S., which has low tax rates relative to most other OECD countries. For countries with higher tax rates the simulated growth effects of a tax change would be larger.

In sum, we have given only a few examples of an extensive literature on disincentives for accumulation arising from public expenditure effects, a literature that ALO overlook in their review. The type of public programs that have been addressed by this literature constitute a large share of public expenditure in OECD countries, while public expenditure that has been hypothesized to be growth promoting constitutes less than 20 per cent. For developing countries, where the public sector is generally much smaller, it is likely that a much larger proportion of public expenditure can be hypothesized to have positive growth effects. The next section discusses the implications of this for the selection of countries in empirical studies.

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[11]It should be noted that the impact of public expenditure on private savings and investment and hence on growth is influenced by the degree of complementarity and/or substitutability between public and private expenditure. But for a given degree of complementarity/substitutability, growth is likely to be negatively affected after a certain point by further increases in public expenditure (Tanzi and Zee, 1997).

[12]Jones, Manuelli and Rossi (1993, 1997) show that the optimal long-run tax on both capital and labor income may be zero when the accumulation of both physical and human capital is endogenous. In their 1997 study they show that optimal tax rates become positive when the assumption of perfect competition is relaxed and when there are restrictions on the feasible tax rates. In a numerical example they still find, however, that these positive tax rates are quite low: 7% per cent for capital income and about 22% for pure labor income.

3. Selection of Countries in Empirical Studies

Following ALO we restrict our discussion to what Slemrod (1995, p. 374) denotes as "top-down" studies,[13] i.e., studies that investigate the association between a measure of aggregate government and a measure of economic growth. In reviewing these studies, ALO draw heavily on studies that analyze growth effects of public expenditure in rich and poor countries simultaneously.[14] The most influential studies are Barro (1991a, 1991b), using Summers-Heston data for a sample of 98 countries he finds that the level of government consumption, excluding education and defense as a share of GDP, has a negative effect on the growth of GDP per capita in the 1960–85 period. The negative effect on growth of the spending measure remains when investment and other background factors are controlled for.

Landau (1983, 1986) and Kormendi and Meguire (1985) are other studies that use a country sample including developing countries. Landau finds a highly significant negative effect of government consumption as a share of GDP on the growth rate of GDP per capita, although this negative effect disappears if the sample is restricted to the poorest half of the countries. In Landau (1986) the separate effect of transfers, educational expenditure and government investment is also assessed. All three are found to be insignificant. Kormendi and Meguire detect no effect from the change in the government consumption expenditure ratio on the average GDP growth rate in 47 countries during the period 1950–77. Thus, we may conclude that the results of studies including *both* rich and poor countries are inconclusive.

Yet, studies that mix rich and poor countries do not necessarily represent a good test of what theory predicts. Theoretical reasoning such as Barro (1990) and Slemrod (1995) and the issues raised in the previous section point to the fact that we should only expect to find a negative effect in countries where the size of the government sector exceeds a certain threshold. In practice, we only observe very large public sectors in rich countries.

A closely related rationale for restricting the empirical analysis to a sample of rich countries is stressed by Slemrod (1995). It is well known that the scope of government tends to increase with the level of income. This tendency is commonly called Wagner's

[13] In contrast, "bottom-up" studies estimate costs of government involvement program by program and tax by tax. An estimate of the aggregate cost is then arrived at by a summation of the various costs with all interactions among programs appropriately accounted for.

[14] A survey of earlier studies is given by, *inter alia*, Hansson and Henrekson (1997), Tanzi and Zee (1997) and Easterly and Rebelo (1993).

Law, and is often said to imply that the income elasticity of demand for government is larger than unity. Easterly and Rebelo (1993) show that there is a strong positive relationship between government size and per capita income both across a large sample of countries at a point in time (1985) and for a panel of 28 countries from 1870 to 1988. However, this relationship disappears at the highest levels of income.

Slemrod (1995) also identifies a number of additional reasons why cross-country analyses including developing countries may yield misleading results: (i) it is more likely that there are Wagner‘s Law effects in countries at a low level of income; (ii) it is easier and less expensive to raise taxes in rich countries; (iii) a high level of human capital facilitates tax collection; (iv) the valuation of government output is likely to be more difficult in poor countries; (v) nonbudgetary, counterproductive government involvement is likely to be of greater importance in poor countries.

Moreover, in his comment on Slemrod‘s paper, Easterly (1995) makes the important distinction between tax rates and tax revenue. In the theoretical models, tax rates are what cause the detrimental growth effects, whereas in the empirical work tax rates are proxied by tax revenues. Since the tax compliance ratio increases with the level of development, tax revenue is a much better proxy for tax rates in rich countries than in poor countries.

To the extent that ALO rely on studies that cover countries with both small and large public sectors they tend to miss the main hypothesis. One way of dealing with this problem might be to focus on OECD countries, and in the next section we address such studies further, e.g., by Easterly and Rebelo (1993) and Dowrick (1993). However, using a sample restricted to OECD countries entails another selection problem. A number of rich countries are not OECD members. These countries have considerably lower public expenditures (even ignoring OPEC countries) than OECD countries. This may reflect the fact that countries are more readily admitted to the OECD if they are democracies and more "western" in their institutional structure. However, this also means that OECD countries may be a biased sample of rich countries.

4. The Simultaneity Problem and the Levine-Renelt Critique

Several authors find that the inclusion of particular control variables can wipe out the negative bivariate relationship between public expenditure and growth.[15] Easterly and Rebelo (1993), for example, show that the inclusion of initial income as an additional regressor is sufficient to remove the negative relationship. This point is made more systematically by Levine and Renelt (1992), who argue that most partial correlations between economic growth and proposed explanatory variables are not robust to changes in the set of conditioning variables. Neither government consumption nor total government expenditure is, according to their stringent criterion, robustly negatively correlated to the rate of GDP growth. Yet, it should be noted that in no instance does the coefficient change sign when the set of conditioning variables is changed, and the Barro measure of government spending is always significant at least at the 10 per cent level.

ALO do not mention, however, that the type of cross-country regressions scrutinized by Levine and Renelt suffer from a potentially severe simultaneity problem. This problem has been pointed out by Slemrod (1995), and strictly speaking its implication is that simple cross-country regressions and tests of robustness have little meaning. The simultaneity problem is potentially severe because the regressions are usually based on average values of government spending and growth over long time periods, typically twenty-year periods. Over long time spans the level of government spending is likely to be influenced by demografics, in particular an increasing share of elderly. For OECD countries ALO report a correlation of 0.72 between the tax ratio and the percentage share of population aged 65 or above. At the same time the share of elderly is closely correlated with GDP. Higher incomes increase expected life spans. This means that if GDP increases faster over the twenty-year period, growth will be higher, but the share of elderly also increases and government spending rises. Thus, errors in the growth variable will affect GDP, demografics and taxes or government spending. As a result the independent variable, taxes or government spending as a share of GDP, is correlated with the error term in the growth regression.

ALO refer to four studies which they claim deal with the simultaneity problem. In fact, however, three of these (Rao, 1989; Lin, 1994; Dowrick, 1993) consider the effect that a *change* in – rather than the level of – government consumption has on the growth rate, which is quite a separate issue. The fourth study is a Granger-Causality study by Conte and Darrat (1988).[16] While a Granger test of the relation between public expenditure

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[15]One of the more well-known studies finding a negative bivariate relation is Plosser (1993) who finds a strongly negative bivariate relationship between the average ratio of income taxes to GDP and the per capita growth rate in 22 OECD countries for the 1960–88 period.
[16]See also Holmes and Hutton (1990).

and growth throws light on the timing of changes in these variables, it is not clear what it can say about causality. For example, assume that a majority of voters become more favorable to a political party that aims to raise taxes. This may reduce investment and hence growth before taxes are actually raised. Therefore, the timing of these events may not be a good indicator of causality.

The bias that arises due to simultaneity would by itself appear to render cross-country regressions, as well as tests of robustness of these regressions, less meaningful. There are, however, other reasons for being sceptical of the Levine–Renelt approach as well.

Sala-i-Martin (1994) has two important objections against the Levine–Renelt methodology. First, he notes that there is a "reverse data-mining" problem. The control variables are samples drawn with some error from the true population. Therefore, if one keeps trying different combinations of control variables one is almost guaranteed to find one or a combination of several control variables for which the error is such that it renders the coefficient of interest insignificant or even causes it to change sign. "The implication is that the extreme-bounds test may be too strong" (p. 743).

Second, Sala-i-Martin points out that Levine and Renelt in fact always find some group of policy variables that matter. The policy variables are so highly correlated that one often cannot distinguish between them, and the proxies used are always imperfect measures. Depending on the sample and the specific choice of explanatory variables, the data are likely to pick one variable or another because they are all close and imperfect indicators of the same phenomenon. Sala-i-Martin (1997b) has since published extreme bounds analyses with less stringent criteria, where he concludes among other things that "no measure of government spending (including investment) appears to affect growth in a significant way" (p. 182). But these are still based on regressions that suffer from the simultaneity bias noted above. They are also based on a sample including developing countries.

Third, entering control variables in an *ad hoc* manner can involve other econometric problems. An example of this is that if Wagner's Law is operative, there may be a recursive relation from the level of income to government size and from government size to growth. In this case the inclusion of initial income among the regressors, aimed at correcting for catching up may involve a multicollinearity (and a recursivity) problem.[17]

[17]Moreover, as noted by Abramovitz (1986) and Hansson and Henrekson (1994a), catching up may not be as important since the early 1970s as it was in the 1950s and 60s.

ALO rest their case heavily on the Levine–Renelt argument, which they illustrate with a few simple regressions. By including three control variables, initial income and two measures of demography, they manage to reverse the sign of the partial correlation between growth and government size for 23 OECD countries for the 1970–90 period. Our objection to this line of reasoning is simple. For any question considered in economic research it will be possible to find econometrically faulty regression specifications, which give rise to opposing results. The ALO approach could thus be applied to any economic question in a way that leads to an agnostic conclusion. We therefore believe that a meaningful review of the literaure would need to pay much closer attention than ALO do to the extent to which different studies attempt to deal with econometric problems.

5. Our Own Test

In this section we will repeat and extend ALO´s econometric work, trying to address some of the econometric issues. In order to achieve good comparability, we deliberately take their framework as our point of departure and refrain from developing and testing our own preferred model. As mentioned above, ALO‘s approach is to regress the growth rate of GDP per capita for OECD countries on the tax burden (share of GDP) and the public expenditure share, respectively. They show that in both cases there is a negative but insignificant correlation. When they add control variables – for initial GDP and the share of the population of non-working age – the coefficients for the tax share and for public expenditure actually turn slightly positive, albeit insignificantly so.

Initially, we note that ALO‘s results are not very sensitive to extensions in the data. We have a longer time period (1970–95) than that used by ALO (1970–90) and demographic data supplied by the OECD rather than the U.N. In *Table 2* we repeat ALO‘s regressions with these data. The pattern of results is quite similar.[18]

[18] Apart from using OECD data on the demographic structure we have lumped the share of people below 15 and above 64 years of age into one variable, whereas ALO enter them as two separate variables. As it turns out, none of these changes affect the outcome much. In our regressions the demographic variable has a slight, but insignificant, positive effect on growth, while in ALO‘s specification these variables have a slight, but insignificant, negative effect on growth.

Table 2 Repeating ALO's Approach: Cross-Section Regressions for 23 OECD Countries, 1970–95.

Regression		*Con-stant*	*Taxes*	*Expen-diture*	*Initial GDP*	*Demo-graphy*	$\bar{R}^2$
1	Cross-section 1970–95	2.80** (.72)		–1.80 (1.7)			.00
2	Cross-section 1970–95	2.82** (.63)	–2.01 (1.69)				.02
3	Cross-section 1970–95	1.79 (2.17)		–.64 (1.36)	–.012** (.004)	5.2 (5.68)	.36
4	Cross-section 1970–95	.68 (1.74)	–.33 (1.41)		–.012** (.004)	7.77 (4.68)	.40

Notes: * and ** indicate significance at the 5 and 1% levels, respectively. Standard errors in brackets. The cross-section regressions contain 23 countries when taxes are used as the public sector measure. For New Zealand public expenditure is not available for all years, so there are only 22 observations when public expenditure is the public sector measure. See Appendix for data sources and exact variable definitions.

As noted above one objection to this type of regressions is that it suffers from a simultaneity bias. A second problem is that a cross-section regression on 22 OECD countries is inefficient since it discards all information on within-country variation. Finally, running regressions on a sample of 22 or 23 countries, and with only 17 or 18 degrees of freedom left after entering explanatory variables is a fragile basis for drawing conclusions about the statistical significance of coefficients let alone about changes in insignificant coefficients.

Exploiting within-country variation is particularly interesting, since the dispersion across OECD countries of total government outlays as a share of GDP has increased substantially since 1960. In some countries, such as Sweden and Portugal, government size has continued to increase up to the present, while in others, such as the U.K. and the Netherlands, there has been little change over the last 15 years. As a result the expenditure ratio now varies between roughly 70 per cent for Sweden and some 35 per cent for the U.S. and Japan (in 1994), while the dispersion in income per capita among OECD countries is comparatively small.

Within-country variation can be accounted for by running a combined cross-section time-series regression using shorter time periods. This also increases the number of observations, and thus the statistical basis för drawing conclusions. Most importantly, however, a panel study of this type allows some progress toward mitigating the simultaneity problem. Since the period of observation is short, it is much less likely that an error in the growth variable will affect life expectancy and government spending in the same period. Further, a panel study facilitates identification, since lagged values of the explanatory variables can be used as instruments.

In sum, the use of panel data goes some way towards addressing the econometric problems prevalent in the type of regression on which ALO focus their attention. Recent panel studies, notably Barro (1997), should be given much more weight than cross-country studies. Barro finds a significant negative effect of his measure of government spending (government consumption excluding defense and education) on growth. Barro also addresses the simultaneity problem by using instrumental variables.[19]

Our approach here is similar to Barro´s. But, in line with the previous discussion on the problems that may be incurred in mixing rich and poor countries, we focus first on OECD countries and then on an extended sample of rich countries. Also we use total government spending and total taxes, in line with the ALO regressions, rather than Barro´s less comprehensive measure. A motivation for this is that differences in government spending between OECD countries are largely due to differences in transfer spending. As argued in the introduction a number of studies suggest that transfer spending can potentially have important growth effects.

The underlying theory relates to long-term growth, and the precise timing between growth and its determinants is not well specified. For example, estimating relationships using annual data is likely to be dominated by mistiming and thus by measurement error. As a compromise we follow the approach used by Barro (1997) and use five-year intervals.[20] In order to retain comparability with the ALO results, we do not extend the set of control variables.

A particular econometric issue in such panel data concerns the presence of heteroscedasticity. Heteroscedasticity can arise in several dimensions. Most often heteroscedasticity appears in a form where the error term is correlated with one of the independent variables or with the dependent variable. This does not seem to be a problem in our data, however.[21] Not surprisingly, therefore, common corrections for

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[19]Similar results are found in de la Fuente (1997). Mendoza, Milesi-Ferretti and Asea (1997) find a significant negative effect of tax rates on investment. They also claim to be unable to find a significant negative effect of tax rates on growth. The importance of their claim remains unclear, however, because they include several highly correlated tax rates in each regression, giving rise to a potential multicollinearity problem. They do note that they find that the tax coefficients have a jointly significant effect on growth rates, but they never elaborate on this statement.

[20]In fact, we obtain similar results using ten-year intervals and three-year intervals. Using annual observations the pattern of relationships is similar, but some of the coefficients are significant at the 10% level rather than the 1 or 5% levels.

[21]A White test for heteroscedasticity yields an F-statistic of 1.03 in the tax equation (and 1.05 in the government spending equation), implying a probability of 0.41. Moreover, testing the relation between

heteroscedasticity along these dimensions, such as the White (1980) and Newey-West (1987) corrections hardly change the results.

However, there is heteroscedasticity along another dimension. The largest country in our sample is approximately 100 times larger than the smallest in terms of the number of inhabitants. Naturally, growth may vary less in large countries than in small ones, partly because growth is measured as the average of growth in subregions, and partly because subregions in large countries are economically integrated areas which through regional policy, factor mobility and other equalizing effects actually tend to have smoother growth than a small and economically more isolated country. Inspection of the standard deviation of the error terms confirms this. Large countries, and small countries that are closely associated with large countries such as Belgium and the Netherlands, have lower standard deviations of the error term. Countries that are small and more isolated such as Iceland and New Zealand have large standard deviations of the error term. A formal test confirms that we have heteroscedasticity between countries.[22] A standard solution to this problem, that we apply in the following, is to use a weighted least squares procedure that weights countries according to the standard deviation of the error term.[23]

In *Table 3* we present these regression results. Fixed country effects and fixed period effects are here taken into account by including dummies.[24] The inclusion of period dummies prevents estimation of spurious correlation that could arise because most countries have experienced a reduction in the growth rate in the 1970s and 1980s.

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the error term and the independent and dependent variables one at a time does not yield a significant relationship in any instance.

[22]The likelihood ratio test for groupwise heteroscedasticity suggested by Fomby, Hill and Johnson (1984) yields a χ^2 of 137.3 which is significant at the 1% level.

[23]It is worth noting that the weighted least squares procedure we use does not induce a bias even if the standard deviation of the error were correlated with one of the explanatory variables. To see this intuitively one need only recall that the central idea in the correction of traditional heteroscedasticity – where the error term is correlated with an explanatory variable – is precisely to weight observations by the values of the independent variable such as the tax rate. However, the correlation between the standard deviation of the error term and the tax rate is weak and by no measure significant.

[24]It turns out that the regression results are quite similar when country dummies are omitted, while the public expenditure coefficient becomes much larger when period dummies are omitted. The latter effect can presumably be explained by the general decline in growth between 1970 and 1995.

Table 3 How Do Taxes and Public Expenditure Affect Growth? Panel Regressions for 23 OECD Countries (including country and period dummies).

Regression		*Taxes*	*Expen-diture*	*Initial GDP*	*Demo-graphy*	$\bar{R}^2$
1	Weighted, 5-year intervals 1970–95		–11.82** (2.32)			.87
2	Weighted, 5-year intervals 1970–95		–13.27** (2.08)	–.05** (.014)	–1.04 (7.09)	.90
3	OLS, 5-year intervals 1970-95		–8.11* (3.41)	–.05* (0.02)	–3.6 (12.43)	.43
4	Weighted, 5-year intervals 1970–95	–10.20* (4.11)				.77
5	Weighted, 5-year intervals 1970–95	–10.4* (4.1)		–.04* (.016)	2.8 (8.8)	.76
6	OLS, 5 year intervals 1970-95	–6.96 (4.8)		–.03 (.02)	–6.8 (12.3)	.36

Notes: * and ** indicate significance at the 5 and 1% levels, respectively. Standard errors in brackets. The Durbin-Watson measure of autocorrelation is close to 2 for all regressions. For New Zealand public expenditure is not available for all years, so there are only 22 observations when public expenditure is the main explanatory variable. The weighted regressions are therefore run on panels with 23x5 observations (22x5 when public expenditure is the main explanatory variable). Fixed country effects and fixed period effects are included in these regressions. There is no constant since these regressions include a fixed effect estimation for each country. See Appendix for data sources and exact variable definitions.

As Table 3 shows, the more efficient estimation accounting for within-country variation and heteroscedasticity yields highly significant and large coefficients for the effects of the tax burden and public expenditure on growth, even after controlling for initial GDP and the demographic structure (regressions 1, 2, 4 and 5 in Table 3). The estimated effects are also much larger, implying that an increase in the tax burden by 10 per cent of GDP is associated with an annual growth rate that is roughly 1 percentage point lower.

Table 3 also shows the regular OLS regressions, leaving heteroscedasticity uncorrected. As expected, standard errors of the estimates in these regressions are larger. Nevertheless, public expenditure has a significant negative coefficient, while the coefficient for taxes is negative, but not significant. Even in the weighted regressions public expenditure coefficients are larger and more significant. This is consistent with the discussion in section 2 concerning potentially negative growth effects of public programs.

It should be noted that there is nothing strange about the fact that the weighting procedure not only leads to reduced standard errors but also to higher coefficient estimates. While both OLS and WLS yield consistent coefficient estimates in the presence of heteroscedasticity, these estimates will typically not be the same. The presence of heteroscedasticity implies that the OLS estimate may be tilted considerably by the presence of a few observations for high-variance countries. The weighted least square procedure gives a more efficient – and often different – estimate by assigning a low weight to obervations that give a less precise indication of where the true regression line lies.

As a more systematic test of robustness we performed an extreme bounds analysis using six control variables. The negative coefficients on taxation and public expenditure were found to be robust according to this more stringent criterion as well.[25]

As noted above, the use of panel data itself mitigates long-run simultaneity problems that arise because, among other things, the demographic structure and political preferences change with rising income.[26] Presumably there is greater justification for assuming that these are exogenous over a five-year period than a twenty-year period. Yet, shortening the period of observation may increase the risk of picking up a correlation driven by business cycle effects. However, this cyclical covariation is largely removed by using five-year periods and by controlling period effects using period dummies. To check this even further, we also entered control variables that vary with the business cycle such as unemployment, the budget deficit, and the rate of investment. This hardly affected the results. To illustrate this, regressions 1 and 2 in *Table 4* show estimates where unemployment is controlled for.

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[25]A detailed presentation of the extreme bounds analysis would be beyond the scope of the paper. The control variables used are: the rate of investment, the budget deficit, the unemployment rate, mean years of schooling, the share of population with secondary schooling and the rate of population growth. As in Levine and Renelt (1992), the control variables are entered three at a time, and we use the 5% significance level as our criterion of robustness. The data are from *OECD Economic Outlook* except for the schooling variables which come from the Barro-Lee data set.

[26]Generally, this is a serious problem in twenty-year (or longer) cross-section studies.

Table 4 How Do Taxes and Public Expenditure Affect Growth? Panel Regressions for OECD Countries, Controlling for the Business Cycle and Using an Instrument for Public Sector Size (including country and period dummies).

Regression		*Taxes*	*Expenditure*	*Initial GDP*	*Demography*	*Unemploym.*	$\bar{R}^2$
1	Weighted, 5 year intervals 1970-95	–8.6* (4.31)		–.04* (.016)	–.09 9.09	–.06 (.05)	.76
2	Weighted, 5 year intervals 1970-95		–13.5** (2.5)	–.05** (.014)	–.29 (7.37)	.01 (.04)	.89
3	First differences, 2SLS	–27.8** (8.1)		–.06** (.021)	15.2 (11.6)		.66
4	First differences, 2SLS		–26.19** (6.14)	–.097** (.034)	12.2 (17.29)		.47

Notes: * and ** indicate significance at the 5 and 1% levels, respectively. Standard errors in brackets. The Durbin-Watson measure of autocorrelation is close to 2 for all regressions. The cross-section regressions contain 23 countries. The weighted regression is therefore run on panels with 23x5 observations (22x5 for public expenditure). In regressions 3 and 4 all variables are in first differences. The first difference of the tax and public expenditure variables are instrumented by the lagged levels of taxes and public expenditure respectively, fixed country effects, and levels and first differences of the population and initial GDP variables.

In some of the literature simultaneity is further addressed by estimating, e.g., the first differences, two-stage least squares regressions using instruments for the tax and government spending variables.[27] Such regressions are presented in regressions 3 and 4 in *Table 4.* As noted by Slemrod (1995), however, there are no ideal instruments.[28] In any case, as argued previously the simultaneity problem should be rendered less severe when panel data are used and business cycle effects are taken into account, compared to cross-section regressions on long-run averages.

[27]The use of first differences is often considered to be a more effective way of correcting for fixed country effects. The first difference of the tax and public expenditure variables are instrumented by the lagged levels of taxes and public expenditure, respectively, fixed country effects, and levels and first differences of the population and initial GDP variables.
[28]For example, it is more common to use the second lag of the instrumental variables as instruments. But this would reduce the number of observations by two fifths.

Table 5 How Do Taxes and Public Expenditure Affect Growth? Cross-section and Panel Regressions for High Income Countries (including country and period dummies).

Regression		Taxes	Expen-diture	Initial GDP	Demo-graphy	$\bar{R}^2$
1	Weighted, 5-year intervals 1970–95		–11.9** (1.7)	–.06** (.01)	–1.04 (7.09)	.93
2	OLS, 5-year intervals 1970-95		–10.3** (2.28)	–.05** (.01)	–1.5 (6.4)	.74
3	Weighted, 5-year intervals 1970–95	–8.7** (2.8)		–.06** (.01)	–4.8 (5.8)	.85
4	OLS, 5 year intervals 1970-95	–7.46* (3.01)		–.05** (.01)	–2.5 (6.8)	.69

Notes: * and ** indicate significance at the 5 and 1% levels, respectively. Standard errors in brackets. The Durbin-Watson measure of autocorrelation is close to 2 for all regressions. For New Zealand public expenditure is not available for all years, so there are only 27 countries when public expenditure is the main explanatory variable. The weighted regressions are run on panels with 28x5 observations (27x5 when public expenditure is the main explanatory variable). Fixed country effects and fixed period effects are included in these regressions. There is no constant since these regressions include a fixed effect estimation for each country. See Appendix for data sources and exact variable definitions.

Finally, we consider the issue of selection bias among rich countries. After all, OECD countries are themselves selected among high income countries, in part for their good growth performance, and in part according to other criteria, such as the existence of democracy. It would therefore be natural to analyze the question using a sample of rich countries which is not restricted by OECD membership. To examine this issue we extend our sample to all non-OPEC countries that have a PPP-adjusted GNP per capita in 1995, the final year of our inquiry, comparable to the 23 OECD countries analyzed so far. These countries have been identified from World Bank (1997, table 1.1). The countries thus included are Hong Kong, Singapore, Israel, Mauritius and Korea. The poorest of these countries are Korea with a PPP-adjusted GDP per capita of USD 260 below the Greek level. Next in line after Korea in terms of income per capita in 1995 is Chile, USD 1,930 below the Korean level. This large gap between these two countries as well as a formal cluster analysis confirms that the 23 richest OECD countries plus the additional five countries is a reasonably well defined group of rich countries.

Regressions for this extended sample of rich countries displayed in *Table 5* now show an overwhelmingly strong relation between tax or government spending on the one hand, and growth on the other hand.

6. Conclusion

Empirical analysis of most economic questions requires a careful consideration of econometric issues. When these are ignored it is easy to produce a variety of contradictory results. A review of such results will typically lead to agnostic conclusions.

The purpose of the present paper is to complement the reviews of the literature concerning the relation between government expenditure and growth, taking issue primarily with the review by Agell, Lindh and Ohlsson (1997). Most of the empirical studies they review, suffer from econometric problems. They report the conclusions of these studies without evaluating to what extent they handle econometric problems, and perform regressions of their own that fail to address any of the econometric problems that arise. This approach creates a bias toward agnostic conclusions.

In our view, a review of previous studies is of limited value unless the empirical work is evaluated in the light of the various econometric issues. In considering this question we focused on four main themes. First, we argued that the neglect of econometric issues has roots in ALO‘s review of theory. Their theoretical review focuses on the growth effects of taxes and certain growth-promoting public programs, largely ignoring potentially negative growth effects of public programs. Second, considering potential growth effects of public programs has implications for the selection of countries in empirical studies. Studies that mix rich and poor countries, or studies that restrict the sample of rich countries to OECD members may be problematic. Third, few studies make any attempt to address simultaneity problems or to fully exploit the time-variation in the data.

Finally, we present our own empirical analysis where we repeat ALO‘s regressions, addressing a number of the econometric issues. While we do not claim that this analysis settles the issue, we do find a tendency toward a more robust negative growth effect of large public expenditures in rich countries compared to many other studies where these econometric problems were ignored or treated more cursorily. From a methodological perspective, we therefore also conclude that there is still new knowledge to be gained from top-down studies.

Appendix: Data Description

Data for GDP growth, tax shares, public expenditure demography and unemployment for the OECD countries are from *OECD Economic Outlook,* Vol. 60, December 1996, data on diskette. Initial GDP was taken from *OECD National Accounts, Main Aggregates, Vol. 1*, *Comparative Tables 1970–1994*, 1996. For non-OECD countries: GDP growth is from World Bank (1997), tax shares and public expenditure shares are computed from *Government Financial Statistics* except for Hong Kong where it was received directly from Hong Kong Trade Development Council in Stockholm, demography is from Statistics Sweden, *Statistical Yearbook* (various years), and initial income is from *Penn World Tables 5.6*.

Average annual growth was computed for GDP per head (*Y*) at the price levels and exchange rates of 1990 (data on disk), as $\left(\left(\frac{Y_E}{Y_B}\right)^{\frac{1}{E-B}} - 1\right) \cdot 100$. (*B* = beginning of period, *E* = end of period).

Initial GDP is defined as GDP per head PPP-adjusted (OECD = 100), and in the panel estimations it is defined as the income of the initial year for each subperiod. The tax and expenditure shares were computed as the average of (Total direct taxes + Social security contributions received by government + Indirect taxes)/GDP and (Government investment + Current disbursements of government)/GDP, respectively.

The tax shares for Luxembourg and New Zealand are from OECD (1995), *Revenue Statistics of OECD Member Countries,* Table 3. Data for government expenditure were missing for New Zealand, and for Luxembourg they were missing after 1987.

The demographic variable is defined as the share of population younger than 15 and older than 64, computed as an average for each period.

The tax shares for Luxembourg and New Zealand were available only until 1994, therefore the averages computed for these observations include only four years for the last period in the estimations.

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