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## Does the composition of government expenditure matter for long-run GDP levels?

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### Abstract

26 We examine the long-run GDP impacts of changes in total government expenditure and in  
27 the shares of different spending categories for a sample of OECD countries since the 1970s,  
28 taking account of methods of financing expenditure changes and possible endogenous  
29 relationships. We provide more systematic empirical evidence than available hitherto for  
30 OECD countries, obtaining strong evidence that reallocating total spending towards  
31 infrastructure and education is positive for long-run output levels. Reallocating spending  
32 towards social welfare (and away from all other expenditure categories pro-rata) may be  
33 associated with modest negative effects on output in the long run.  
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## I. Introduction

The fiscal stimulus packages enacted in many countries from 2009 onwards to combat the global economic crisis typically included changes in various public expenditure programs. Much debate surrounded the merits of these fiscal packages, not least with respect to the wisdom of governments' attempts, and ability, to stimulate the economy via expenditure injections. The specific public spending choices in these short term fiscal packages were motivated in part by a desire to be consistent with long term growth objectives, such as expanding infrastructure spending perceived as benefiting long-run GDP levels or growth rates. The austerity programs that have been used since then to deal with the resulting high levels of public debt have led to the same debates but from the opposite side; which types of expenditure should be cut and what are the longer term growth effects from doing so. This brings to the fore the twin questions: how strong is the evidence that long-run income levels or growth rates respond to public expenditure changes, and which expenditure categories have greatest impact?

This paper focuses on these two questions. We first briefly review the relevant theory. This builds on Barro (1990) and Devarajan *et al.* (1996) who proposed that some 'productive' public expenditures can influence long-run growth rates via public sector inputs in private production functions. Following a brief discussion of the existing evidence on long-run public expenditure-output relationships, we provide more systematic empirical evidence than available hitherto for OECD countries.<sup>1</sup>

In addition to the above political economy motivations, there are a number of academic reasons to re-examine this issue. Firstly, most empirical studies focus on a subset of individual expenditure categories, such as defence, education or transport and communication expenditures. These have produced a mixed set of findings and do not typically allow the trade-offs between different forms of public expenditure to be identified explicitly. Our approach provides evidence on these trade-offs.

Secondly, a number of recent papers have used alternative methodologies to examine long-run impacts of *tax* policy on GDP levels or growth rates (see Adam and Bevan, 2005; Lee and Gordon, 2005; Romero-Avila and Strauch, 2008; Arnold *et al.*, 2011; Gemmell *et al.*, 2011, 2015). However, *public expenditure* is rarely the primary focus of attention in these studies, or it is tested as an aggregate 'productive' spending category. In this paper we limit attention to potential effects of public expenditures on long-run GDP levels, focusing both on total public expenditure (suitably financed via the government budget constraint) and specific functional spending categories.

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<sup>1</sup> We abstract from the question of whether there are short-run benefits from stimulus packages, analysis of which requires quite different empirical methodologies.

1 Thirdly, dealing with endogeneity associated with estimates of fiscal impacts on GDP has  
2 proved difficult in previous studies. While we do not claim to have resolved these concerns in  
3 this paper, we do carefully address potential endogeneity problems. Our fiscal variables would  
4 appear to be at least ‘weakly exogenous’ (based on standard econometric definitions, see  
5 Johansen, 1992, and Boswijk, 1995) with respect to their estimated effects on GDP.  
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10 Fourthly, use of panel data methods has increased the reliability of recent studies.  
11 However, these generally use fixed effects estimators which impose parameter homogeneity  
12 across countries. Furthermore, as Haque’s (2004) re-examination of the Devarajan *et al* (1996)  
13 results shows, how the time-series properties of the data are handled can be crucial for  
14 estimated output effects of fiscal variables.  
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19 In this paper, access to an extended panel dataset with a longer time dimension than  
20 previous studies has permitted application of the more flexible Pooled Mean Group (PMG)  
21 estimator proposed by Pesaran *et al.* (1999). This enables us to explore both short run dynamics  
22 and long-run equilibrium relationships among the variables of interest, and account for  
23 heterogeneity across countries in their short run dynamic relationships. We compare these  
24 results with those obtained using more restrictive dynamic fixed effects (DFE) methods, and  
25 the more flexible, but data-intensive, Mean Group (MG) approach.  
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31 These improvements provide more robust evidence on the potential long-run association  
32 and causation between public spending and GDP. This supports some traditional views, such as  
33 that a spending reallocation towards infrastructure and education spending (and to a lesser  
34 extent, health) can raise GDP over the long run. Our results also suggest that, relative to public  
35 spending ‘on average’, social welfare spending may have moderate GDP-reducing effects. This  
36 implies a possible trade-off between public spending aimed at income redistribution via social  
37 welfare spending, and spending aimed at raising overall income levels.  
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43 The remainder of this paper is structured as follows. Section II briefly describes the links  
44 between public expenditure composition and GDP growth hypothesised by recent theory, and  
45 summarises current evidence for OECD countries. Section III then discusses our testing  
46 methodologies and dataset; while section IV reports results for a sample of 17 OECD countries  
47 over 1972-2008. Some conclusions are drawn in section V.  
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## 52 **II. Public spending, taxes and growth**

### 53 **Theory**

54 As is well known, in the neoclassical growth model, if the incentives to save or to invest in new  
55 capital are affected by fiscal policy, this alters the equilibrium capital-output ratio, and  
56 therefore the level of the output path but not its slope. There are effects on growth rates only for  
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1 a transitional period as the economy moves onto its new output path, though the length of this  
2 transition remains subject to debate.<sup>2</sup> There is, however, a resulting permanent impact on the  
3 long-run *level* of output (GDP). The 1990s saw the development of a number of growth models  
4 with a permanent, or at least persistent, role for fiscal policy such as those of Barro (1990),  
5 Futagami *et al.* (1993) and Devarajan *et al.* (1996).  
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10 A novel feature of these models was that fiscal policy can determine both the level of the  
11 output path and the steady-state (long-run) growth rate.<sup>3</sup> Key fiscal aspects include some  
12 ‘productive’ public expenditures affecting private sector productivity while other  
13 ‘unproductive’ expenditures only impact on citizens’ welfare (including the possibility of zero  
14 welfare effects), and that some taxes levied to fund public expenditures distort investment  
15 decisions. In a model with multiple productive expenditures Devarajan *et al.* (1996) show that  
16 the long-run growth effects depend upon a combination of the *relative* productivities of these  
17 expenditures and their relative budget shares.  
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24 A number of recent papers have modelled the relationship between particular public  
25 spending categories and growth. For example, Blankenau and Simpson (2004), Agénor and  
26 Neanidis (2006), Semmler *et al.* (2007), Agénor (2008) and Agénor (2012) have examined  
27 various extensions of the Barro/Devarajan framework which explicitly model infrastructure,  
28 education and/or health spending as inputs into private production; and interactions between  
29 these spending types. For example, health services or infrastructure spending may enter the  
30 production function for education.<sup>4</sup>  
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36 These models propose mechanisms by which permanent impacts on GDP growth rates can  
37 arise in association with changes in particular public expenditure categories, especially those  
38 related to infrastructure and human capital production. However, permanent growth effects in  
39 these endogenous growth models generally depend on so-called ‘knife-edge’ properties  
40 whereby reproducible factors of production must display exactly constant returns to scale  
41 (Solow, 1994; Dalgaard and Kreiner, 2003). While the conditions for such permanent effects  
42 may be restrictive, and hard to verify empirically, even in their absence, the mechanisms  
43 embedded in endogenous growth models suggest that public spending could have effects on  
44 growth that are highly persistent.  
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### 51 **Existing empirical evidence on public expenditure and growth**

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55 <sup>2</sup> Turnovsky (2004), for example, estimates ‘transitional’ output adjustments to fiscal policy changes in terms of  
56 decades, while Lee *et al.* (1997) find convergence to equilibrium with half-lives of a few years, rather than  
57 decades.

58 <sup>3</sup> Not all endogenous growth models predict long-run growth effects from fiscal policy, such as the ‘non-scale’  
59 growth model of Eicher and Turnovsky (1999).

60 <sup>4</sup> See Albertini *et al.* (2014) for an examination of short-run fiscal multipliers for different components of  
government spending at the zero lower bound, based on a New Keynesian model.

1 Much of the empirical literature testing for fiscal policy impacts on long-run GDP levels or  
2 growth rates suffers from various methodological weaknesses.<sup>5</sup> In particular it is now  
3 recognised that tests of the output effects of public expenditure decompositions (and other  
4 fiscal variables) must accommodate the total government budget (expenditures, revenues,  
5 deficits); a feature missing from much of the earlier literature. Nevertheless, for specific  
6 spending decompositions, some early studies found education, health and/or transport &  
7 communication (T&C) spending to be positively associated with GDP growth; see Nijkamp  
8 and Poot (2004) for a meta-analysis of early studies.

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15 As shown by Kneller *et al.* (1999), without accounting for the government budget  
16 constraint (GBC), evidence on the long-run output effects of public expenditures is difficult to  
17 interpret and can appear to be non-robust. Recent studies, such as Acosta-Ormaechea and  
18 Morozumi (2013) and Afonso and Jalles (2014), have sought to accommodate the GBC, to  
19 varying degrees, in their empirical approaches. Both studies focus on social security, education  
20 and health spending (and T&C in the case of Acosta-Ormaechea and Morozumi), generally  
21 finding negative associations between GDP growth and social security spending but positive  
22 associations for education and health. These results are obtained for large samples of  
23 developing and developed countries, rely on period averaging of annual data (hence ignoring  
24 short-run dynamics), and impose parameter homogeneity across countries.

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32 As noted above, we prefer to focus on a more homogeneous group of OECD countries,  
33 using annual data and panel techniques that can identify both short and long-run effects whilst  
34 allowing for heterogeneity across countries in responses to fiscal shocks. Like Acosta-  
35 Ormaechea and Morozumi (2013), and Arnold *et al.*'s (2011) analysis of tax decompositions,  
36 we examine relationships between fiscal variables and long-run GDP *levels* rather than growth  
37 rates. An advantage of this 'levels specification', discussed further below, is that it allows the  
38 data to identify the degree of persistence in GDP growth responses, rather than impose a  
39 functional form embodying permanent effects.

### 46 **III. Methodology and data**

#### 47 **Growth versus level effects**

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49 The empirical specification adopted by much of the empirical literature testing for public  
50 expenditure-GDP growth effects since Devarajan *et al.* (1996) is based on their endogenous  
51 growth model. This model, derived from the Barro (1990) framework, essentially generates an  
52 estimating equation in which the growth rate of GDP in country  $i$  at time  $t$  is a function of the  
53 ratio of total government expenditure,  $E$ , to GDP and a vector of shares of  $j$  individual  
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59 <sup>5</sup> See Slemrod (1995), Tanzi and Zee (1997), Myles (2000) for earlier, more detailed reviews and Temple (1999)  
60 for a more wide-ranging review of growth empirics.

categories within total government expenditure,  $e_j$ . Sets of conditioning variables, consistent with a more general growth model or more pragmatic considerations, are often included; see Devarajan *et al.* (1996; p.322). In a panel context, the Devarajan *et al.* endogenous growth model takes the form:

$$g_{i,t} = \Delta y_{i,t} = \beta_1 (E/Y)_{i,t} + \sum_k \beta_k (E_k/E)_{i,t} + \mu_i + \omega_t + \varepsilon_{i,t} \dots + \text{control variables} \quad (1)$$

where  $y_{it}$  is the log of GDP per capita in country  $i$  at time  $t$ ,  $g_{it}$  is the growth rate (log difference:  $\Delta y_{it}$ ),  $Y$  is GDP,  $E$  is total public expenditure and  $E_k$  is the  $k^{\text{th}}$  expenditure component,  $\mu_i$  and  $\omega_t$  are country and time fixed effects, and  $\varepsilon_{it}$  is a classical error term.<sup>6</sup>

A difficulty with equation (1) is that it is specific to the endogenous growth model with permanent growth effects from fiscal policy changes, and no transitional dynamics. To allow for Solow-type transitional dynamics, but where the effects of fiscal policy may be persistent, requires a more flexible functional form. This is particularly important when working with annual data. We follow Arnold *et al.* (2007, 2011) and use an autoregressive distributed lag, ARDL( $p$ ,  $q$ ), model, parameterised in error correction form.<sup>7</sup> This allows both the short run dynamic, and the long-run equilibrium, relationships between GDP and fiscal variables to be separately identified.

Consider the following general ARDL( $p$ ,  $q$ ) specification:

$$y_{i,t} = \sum_{j=1}^p \alpha_{i,j} y_{i,t-j} + \sum_{j=0}^q \beta_{i,j} X_{i,t-j} + \mu_i + \varepsilon_{i,t} \quad (2.1)$$

$$\Delta X_{i,t} = \zeta_1 \Delta X_{i,t-1} + \zeta_2 \Delta X_{i,t-2} + \dots + \zeta_s \Delta X_{i,t-s} + u_{i,t} \quad (2.2)$$

where the vector  $X_{i,t}$  in (2.1) includes both the fiscal variables of interest (e.g. the level and mix of expenditures,  $E/Y$  and  $E_k/E$  respectively), and control variables. The  $\alpha$ ,  $\beta$ , and  $\zeta$  are parameters to be estimated; where the  $\zeta_s$  capture the autoregressive process in  $\Delta X_{i,t}$ .

A number of parameterisations of (2.1) are possible (see Wickens and Breusch, 1988), but it is convenient to express (2.1) in error correction (ECM) form:

$$g_{i,t} = \Delta y_{i,t} = \phi_i (y_{i,t-1} - \beta_i X_{i,t}) + \sum_{j=1}^{p-1} \alpha_{i,j}^* \Delta y_{i,t-j} + \sum_{j=0}^{q-1} \beta_{i,j}^* \Delta X_{i,t-j} + \mu_i + \varepsilon_{i,t} \quad (3)$$

where  $\phi_i = -(1 - \sum_{j=1}^p \alpha_{i,j})$  captures the error correcting component, and  $\beta_i = (\sum_{j=0}^q \beta_{i,j} / \phi_i)$  captures the long-run equilibrium relationships between  $y$  and  $X$ , with short run effects measured by  $\beta_{i,j}^*$  – the parameters associated with the  $\Delta X$  variables in (3). The error correction term,  $\phi_i$ , is a measure of the speed at which the model returns to equilibrium after a shock. Equation (3) allows  $\phi_i$  and  $\beta_i$  to vary across countries, though alternative econometric

<sup>6</sup> The dependent variable in some cases is per capita GDP growth, which simply involves a re-parameterisation when GDP is on the left-hand side and the vector of control variables includes a measure of population growth on the right-hand side.

<sup>7</sup> Arnold *et al.* (2007) show that such a specification can be consistent with, and nest, alternative augmented Solow and (Uzawa-Lucas) endogenous growth models.



1 approaches (PMG, MG, DFE etc.) involve differing homogeneity restrictions, as discussed  
 2 further below.  
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4 The degree of persistence of any fiscal effects on output *growth* in the model is, of course,  
 5 determined by the model's convergence properties.<sup>8</sup> Although the dependent variable in (3) is  
 6 per capita GDP *growth*, the regression measures the impacts of fiscal and other variables on the  
 7 long-run per capita GDP *level*, since (3) is merely a re-parameterisation of (2.1).  
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### 11 The government budget constraint

12 Since the GBC describes a 'closed system', whatever parametrisation is adopted the  
 13 estimating equation needs to recognise that expenditures,  $E_{i,t}$ , must be financed by revenues,  
 14  $R_{i,t}$ , and/or the budget surplus/deficit,  $D_{i,t}$ , each with potential output effects, since  $D_{i,t} = R_{i,t} - E_{i,t}$   
 15 (see Kneller *et al.*, 1999; Gemmell *et al.*, 2011). As a result, in addition to the variables  $E/Y$  and  
 16  $\Delta(E/Y)$  within the vectors  $X$  and  $\Delta X$  in (3),  $(R/Y)$  and  $(D/Y)$  also potentially have effects on  
 17 output with any net output effect depending on the particular financing combinations assumed.  
 18 Hence a decomposition of  $\beta_i X_{i,t}$  in equation (3) includes (setting  $\beta_i = \beta$  for convenience):  
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$$20 \dots \beta_1 (E/Y)_{i,t-1} + \beta_2 (R/Y)_{i,t-1} + \beta_3 (D/Y)_{i,t-1} + \sum_k \beta_{4,k} (E_k / E)_{i,t-1} \dots \quad (4)$$

21 and similarly for the short run output effects of expenditures, revenues and deficits. However,  
 22 since introducing all three variables would be perfectly collinear in a regression, one must be  
 23 chosen to omit. Omitting  $(D/Y)_{i,t-1}$ , (4) becomes:  
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$$25 \dots (\beta_1 - \beta_3)(E/Y)_{i,t-1} + (\beta_2 - \beta_3)(R/Y)_{i,t-1} + \sum_k \beta_{4,k} (E_k / E)_{i,t-1} \dots \quad (5)$$

26 This demonstrates the correct interpretation of the output effects of expenditure and  
 27 revenue variables, within this model. Estimates of the fiscal parameters in (5) capture the  
 28 effects of increases in total expenditure, or decreases in revenues, financed by changes in the  
 29 budget deficit (the omitted category in this example). These effects can be seen to depend on  
 30 the signs and relative sizes of  $\beta_1$ ,  $\beta_2$  and  $\beta_3$ . In comparison, the interpretation of the coefficients  
 31 on the individual expenditure *share* components of interest,  $\beta_{4,k}$ , remains unaffected.  
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33 In the applications below, each regression includes one of the  $k = 1 \dots K$  expenditure share  
 34 elements,  $E_k/E$ , where the included  $k^{\text{th}}$  expenditure category is rotated across the  $K$  different  
 35 categories. Including each expenditure share in turn, rather than all  $K-1$  expenditure shares  
 36 simultaneously, saves on degrees of freedom in our panel regression model which requires a  
 37 large number of parameters to be estimated; see below.<sup>9</sup>  
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39 <sup>8</sup> Though the parameter  $\phi$  can be interpreted as a speed of adjustment, it is not equal to the more familiar rate of  
 40 conditional convergence in this model. This is because the short-run dynamics are more complicated than in a  
 41 conditional convergence regression.

42 <sup>9</sup> By repeating regressions on equation (3) but including a different expenditure category each time (from (5)), the  
 43 per capita GDP impacts of a bilateral switch between any two expenditure categories,  $l$  and  $m$ , can be obtained.  
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### Allowing for heterogeneous fiscal-output effects

We estimate an equation of the form in (3) above using the pooled mean group (PMG) methodology of Pesaran *et al.* (1999), and compare this with the equivalent results using fixed effects (FE), and mean group (MG), estimators. The PMG estimator provides a useful intermediate alternative between estimating separate country regressions (the MG case) and the fixed effects estimator which imposes homogeneity on all slope coefficients and error variances across countries. The PMG estimator allows the intercepts, short run coefficients and error variances to differ freely across groups, but constrains the long-run coefficients to be the same.

If homogeneity of the long-run responses is not rejected, the PMG is the preferred estimator because of its efficiency relative to the MG estimator which permits long-run heterogeneity. Pesaran *et al.* (1999) demonstrate that the PMG's allowance for short run parameter heterogeneity yields more reliable estimates of the long-run responses, and can affect estimated speeds of convergence towards long-run equilibrium.

A disadvantage of the PMG estimator is that, unless the available time series are long, a degrees of freedom problem is soon reached. For the dataset available here this requires some restrictions on lag lengths and/or the set of right-hand-side (RHS) variables. For this reason we restrict the RHS variables to include each expenditure share separately in turn, with up to two lags. Though this lag length is relatively short, inclusion of the lagged dependent variable ensures the specification can accommodate a high degree of persistence in the short run adjustment process following a shock.

We also include two non-fiscal control variables – the private non-residential investment rate and employment growth. These variables appear in many empirical growth models capturing the role of private sector inputs. Also, recognising that some public expenditures may impact on GDP partially through their impact on private investment, we omit the latter variable from some specifications.

An important aspect of equation (3) when testing for the output effects of public expenditures is that it allows the degree of persistence of these effects to differ across expenditure types, at least to the extent that these can be captured by the 30+ years of our data. In addition, we test the PMG assumption of long-run parameter homogeneity against the mean group (MG) and fixed effects (FE) alternatives, using a Hausman test.

### Addressing endogeneity

A major concern when running regressions of the form in (3) is, of course, the potential for simultaneity between GDP per capita and the right-hand-side variables – especially the fiscal and investment variables, a point stressed by Slemrod (1995) in a cross-section growth

1 regression context. In addition, country-specific, time-varying factors such as changes to  
2 political and institutional settings may influence both fiscal policy and GDP per capita.<sup>10</sup> These  
3 may be compounded by persistence in the annual fiscal series and potentially long lags in the  
4 impacts of fiscal variables on GDP per capita, generating serial correlation in the error process  
5 that must be dealt with adequately. As a result the fiscal variables of interest here may be  
6 endogenous, with well-known problems of interpretation for the short and long-run OLS  
7 parameters.  
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13 In a series of papers in the mid-1990s, Pesaran and associates (see, for example, Pesaran  
14 and Smith, 1995; Pesaran, 1997; Pesaran and Shin, 1999) demonstrated that, under a number of  
15 conditions such as a unique cointegrating relationship, estimates of the long-run parameter  
16 vector,  $\beta$ , obtained from OLS regressions of models such as (3) are consistent.<sup>11</sup> Further, based  
17 on simulation results, Pesaran *et al.* (1999) demonstrate that even in small samples standard *t*-  
18 and *F*-tests on the long-run parameters from the ECMs are valid, given suitable specification of  
19 the lag structures of dependent and independent variables.<sup>12</sup>  
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25 Further, where serial correlation is a concern, ‘appropriate modification of the orders of the  
26 ARDL model’ (Pesaran and Shin, 1999, p.386) is sufficient to deal with both the serial  
27 correlation in the error process and/or regressor endogeneity (see also Kanas and Kouretas,  
28 2005). We discuss these aspects further in section IV when we present our empirical results.  
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### 33 **The updated dataset**

34 Results reported below are based on an extension of the Bleaney *et al.* (2001) dataset, which  
35 uses the IMF’s Government Financial Statistics (GFS) fiscal data for 17 OECD countries to  
36 construct measures of total expenditure and individual expenditure shares, distortionary and  
37 non-distortionary taxes, and budget surpluses/deficits.<sup>13</sup> The original data, available from the  
38 early 1970s to 1995, have been updated to 2007 or 2008, providing around 30-35 annual time-  
39 series observations each for most of the 17 countries.<sup>14</sup> We exclude data after 2008 to avoid  
40 long-run estimates being unduly affected by the large shock (associated with the global  
41 financial crisis and its aftermath) to fiscal, GDP and other variables after 2008.  
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48 Data on GDP, the private investment/GDP ratio and employment growth were obtained for  
49 the same period from OECD sources. An important difference from previous studies is that our  
50 investment variable is *private non-residential* investment (PNRI) instead of total investment  
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53 <sup>10</sup> We do however deal with time-specific shocks common to all countries in the sample.

54 <sup>11</sup> Indeed, where variables are I(1), they argue that parameter estimates are super-consistent.

55 <sup>12</sup> See, for example, Pesaran (1997, pp. 182-185), Pesaran and Shin (1999, pp. 381-387; 404-405).

56 <sup>13</sup> The 17 OECD countries are Australia, Austria, Canada, Denmark, Finland, France, Germany, Iceland,  
57 Luxembourg, Netherlands, New Zealand, Norway, Spain, Sweden, Turkey, UK and US. Other OECD countries  
58 are excluded due to missing values for several years; see online appendix.

59 <sup>14</sup> This is not straightforward, however, because of changes in the GFS methodology, which moved from a cash  
60 accounting, to an accruals accounting, basis for fiscal data from the late 1990s onwards; see online appendix.

(gross fixed capital formation). Since all regressions include various public expenditure variables, the use of PNRI avoids the possibility of double counting much public investment which otherwise would contribute to both the investment and public expenditure data. Before running all regressions we de-mean all data series as recommended by Lee *et al.* (1997) and Pesaran *et al.* (1999) to deal with potential cross-sectional error correlation. Measuring all variables as deviations from their sample means in each year removes the effects of common shocks in the data.

#### IV. Empirical results

##### Pre-testing for lag lengths

As discussed in section III, following Pesaran and Shin (1999), regressions on equation (3) require knowledge of the appropriate lag structures if the  $X_{it}$  in (3) are to be interpreted as exogenous or weakly exogenous; see below. Before running our PMG regressions, our procedure therefore follows the pre-test proposed by Pesaran and Shin (1999) to select the preferred lag structure for the dependent, and each independent, variable based on the Schwarz Information Criterion (SIC; see Pesaran and Shin, 1999). This uses up to two lags for each variable and country; in practice we find that two lags are required relatively rarely.<sup>15</sup> The results of PMG regressions on equation (3) reported below are those for the SIC-based lag structures.

##### Testing for total public expenditure effects

Before examining the GDP effects associated with the shares of particular expenditure categories it is worth noting the impact of the omitted financing category in the government budget constraint because it affects the interpretation of the included parameters. As the parameter estimates on the shares of individual spending categories in total expenditure are unaffected by this we omit them from the regressions for the moment.

Table 1 shows the long-run and short run parameters for six PMG regressions in which total spending is included, financed by combinations of three alternative fiscal variables (non-distortionary taxes, deficits and distortionary taxes) in columns 1-4. Column 5 repeats the regression in column 2, but with the private investment control variable omitted.<sup>16</sup> Results for alternative (MG and DFE) estimators are then reported in Table 2. The role of different implicit

<sup>15</sup> For example, in each of regressions 1–3 of Table 1, there are a maximum of 204 possible parameters (17 countries  $\times$  6 included variables  $\times$  2 lags). Regression 4 involves 5 variables, hence 170 possible parameters. For those four regressions, two lags were selected in only 11%, 8%, 9% and 4% respectively of the possible cases.

<sup>16</sup> In general the ‘non-distortionary’ taxes in this context are consumption taxes. The term ‘less growth distortionary’ may be more appropriate in this case since these taxes can distort investment decisions in models where labour supply effects are allowed for. The method of aggregating the GFS functional classification into these sub-aggregates is described in Bleaney *et al.* (2001). ‘Other revenues’ refers to the GFS categories ‘taxes on international trade and transactions’, ‘other taxes’ and ‘non tax revenues’. It is unclear how distortionary these might be. Other revenues (non-tax revenues) are approximately 13% (9%) of total revenues in our sample.

1 financing of fiscal variables has been discussed by Gemmell *et al.* (2011) so is only briefly  
2 reviewed here.  
3

4 [Table 1 about here]

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6 Regression 1 in Table 1 reveals that increasing total public spending, financed by the  
7 (omitted) non-distortionary taxes has a long-run output effect that is positive and statistically  
8 significantly different from zero. However, the same spending increase can alternatively be  
9 financed by either a reduced budget surplus (regression 2), or by an increase in distortionary  
10 taxes and/or increases in any/all taxes (regressions 3 and 4), or by some combination.  
11 Regressions 2-4 in Table 1 show that each of those cases produces a negative, statistically  
12 significant long-run parameter on the public expenditure variable. The different regression  
13 specifications in Table 1 confirm that the assumed (or imposed) method of financing a public  
14 expenditure increase is crucial for estimates of their likely impact on long-run levels of GDP  
15 per capita.  
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18 The Table 1 results suggests that, within our dataset, use of ‘non-distortionary’ forms of  
19 taxation (such as VAT) to fund a given unit of additional public spending has been associated,  
20 *ceteris paribus*, with increases in GDP per capita levels in the long run, whereas other forms of  
21 financing such spending have been associated with decreases in GDP per capita.<sup>17</sup> Table 1  
22 results for total public expenditure parameters also tend to suggest that the adverse long-run  
23 effects on GDP per capita from distortionary tax financing (at  $-0.024$ ) are significantly greater  
24 than those associated with other forms of financing (around  $-0.012$ ).<sup>18</sup> However this should be  
25 interpreted cautiously. As we show below, specifications that also allow for different effects on  
26 GDP per capita from differences in specific public expenditure shares (Table 3) suggest that the  
27 magnitude of the total expenditure parameter can be somewhat sensitive to the included  
28 expenditure share variables.  
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31 Recognising that some fiscal-GDP effects could be mediated through private investment  
32 (as distinct from via factor productivity), regression 5 in Table 1 examines whether omitting  
33 investment from the control variables affects conclusions regarding the GDP impacts of public  
34 expenditures. Comparing the equivalent, budget deficit-financed cases in regressions 2 and 5,  
35 the estimates suggest that parameters on fiscal variables, including total public expenditure, are  
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<sup>17</sup> The control variables in these regressions generally perform as expected: larger investment ratios and employment growth (proxying labour force growth) are associated with higher GDP per capita, though the former is not statistically robust, sometimes taking a negative sign. The error correction parameter, at around  $-0.03$  to  $-0.09$ , implies a fairly high degree of persistence.

<sup>18</sup> Testing differences in the parameters on total expenditure in regressions 2 and 3 in Table 1 confirms that the parameter on total expenditure financed by distortionary taxes has a statistically (at 5%) greater negative impact on growth than when financed by a budget deficit. The same test for regressions 3 and 4 yields the same conclusion: the negative growth impact of public spending financed by distortionary taxes is significantly more negative than when financed by non-distortionary taxes.

1 little affected by the omission. That is, estimated public expenditure effects on per capita GDP  
2 are largely orthogonal to private investment effects on per capita GDP.<sup>19</sup>  
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5 Overall, while the results in Table 1 yield similar estimates of fiscal variable impacts on  
6 GDP (when the appropriate interpretation is recognised as discussed above), our preferred  
7 specification is regression 2 which omits the budget surplus. This facilitates interpretation  
8 because deficit-funded tax or expenditure changes have an intuitive economic interpretation  
9 and because across all countries a one unit change in a budget surplus/deficit is a more uniform  
10 metric. This is less true for a one unit change in distortionary or non-distortionary tax because  
11 the composition of these tax variables (income tax, VAT etc.) can differ across countries, and  
12 each component tax may differ in its effects on GDP. We therefore base our further testing of  
13 public expenditure effects using the specification that omits the budget surplus.  
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20 As noted above, the PMG estimator provides a greater degree of flexibility, compared to  
21 dynamic fixed effects models, in estimating long-run parameters by allowing short run  
22 parameters to differ across countries. The Mean Group estimator allows for further flexibility  
23 via heterogeneous *long-run* effects across countries but at a further cost in terms of degrees of  
24 freedom. We test for possible sensitivity of our Table 1 results to the PMG specification, based  
25 on regression 2 in Table 1. Table 2 presents equivalent results for dynamic fixed effects and  
26 mean group models (and repeats PMG results for comparison). Non-fiscal variables are  
27 included in regressions but omitted from the table to save space.  
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34 In Table 2, the parameters on each fiscal variable take the same sign but with standard  
35 errors generally larger using the MG or DFE estimators. As a result, parameter estimates for  
36 those cases are generally not statistically significant at conventional levels. That this is the case  
37 for the MG estimates is no surprise, given the large number of parameters to be estimated.  
38 However the comparison between PMG and DFE suggests that allowing for heterogeneous  
39 short run parameters across countries enables more precise long-run parameter estimates to be  
40 obtained. This mirrors results obtained by Gemmell *et al.* (2011, pp. F43-44) who also find that  
41 the DFE, unlike the PMG, requires a lag structure of up to eight lags to identify long-run  
42 parameters.  
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49 The far right-hand column of Table 2 reports results from Hausman tests of the PMG  
50 restrictions against those associated with the less restrictive MG estimators for each fiscal  
51 variable and for the regression specification as a whole. The final row compares the PMG  
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55 <sup>19</sup> This is perhaps not surprising, given the limited contribution to long-run output apparently due to private  
56 investment in Table 1. To investigate this issue further we ran similar regressions with original, rather than de-  
57 meaned, data. This yielded a larger, statistically significant investment parameter. This may reflect a high degree  
58 of co-movement in private investment across countries such that, after de-meaning the data, there is little country-  
59 specific variation in the investment data, or that any such variation has only a modest influence on long-run per  
60 capita GDP levels (though it may still have a strong effect on short-run growth rates).

1 regression with the DFE equivalent. The Hausman tests fail to reject the null hypothesis of non-  
2 systematic differences in parameters between the PMG and MG estimators, supporting the  
3 PMG restriction of homogeneous long-run effects across countries. This also holds for the  
4 regression as a whole ( $\chi^2(5) = 7.20$ ; p-value = 0.21). By contrast, the Hausman test rejects the  
5 null hypothesis that differences between the PMG and DFE are not systematic ( $\chi^2(5) = 16.51$ ;  
6 p-value = 0.00), rejecting the additional short run restrictions of the DFE model.  
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11 Finally, though results in Table 2 appear to support long-run parameter homogeneity, in an  
12 online appendix (Table A1) we report MG parameter estimates for total public expenditure by  
13 country, as well as cross-country means and medians. From Table 2, the mean long-run MG  
14 parameter for total public spending is  $-0.020$  ( $t = 1.65$ ). The online appendix shows that, when  
15 long-run parameters are allowed to differ, three countries, Sweden, Australia and New Zealand,  
16 take relatively large (absolute) parameter values for total expenditure ( $-0.20$ ,  $0.07$ ,  $-0.06$   
17 respectively). However, for 13 of the 17 countries parameter estimates are negative and  
18 generally close to the arithmetic mean of the long-run estimates,  $-0.020$ .  
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### 26 **Public Expenditure Composition and GDP**

27 To explore the potential long-run effects of public spending composition on GDP, we again  
28 focus on the specification in which changes in total spending are implicitly funded by a change  
29 in the budget deficit. Table 3 shows the results from repeating this PMG regression, but adding  
30 the shares of each public spending category – transport & communications (T&C), education,  
31 health, etc. – in total expenditure (excluding interest payments). To save space the table shows  
32 only the parameters on total public expenditure and the spending decompositions of interest.  
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38 Due to a lack of degrees of freedom, it is not possible to include all ( $n-1$ ) categories in one  
39 regression though, as noted earlier, it would be possible to construct the growth trade-off  
40 associated with any bilateral expenditure share switch. The parameter on each expenditure  
41 share in regressions (1)–(9) should therefore be interpreted as the impact on long-run per capita  
42 GDP levels of switching spending into the included expenditure category (say, T&C) and away  
43 from remaining expenditure categories on a *pro rata* basis, holding total spending constant as a  
44 ratio of GDP. A significant positive (negative) parameter indicates that the category in question  
45 has a greater (smaller) impact on long-run GDP than the remaining expenditure categories.  
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52 All regressions (except for T&C) reveal net negative *total* spending growth effects when  
53 funded from increased budget deficits (consistent with Table 1 results for this case). Whilst  
54 possible positive short term GDP impacts of fiscal stimulus packages have figured prominently  
55 in recent macroeconomic debates, these results suggest a possible adverse impact on longer  
56 term per capita GDP levels if higher deficit-financed expenditure levels persist.  
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1 Switching into some spending categories might be expected to have limited effects on  
2 GDP either where there are negligible short run impacts from the relevant public expenditures  
3 or where the 'favoured' category has similar (possibly substantial) GDP impacts to those  
4 categories where spending shares decrease. For example, as in Barro (1990), if public spending  
5 levels and shares have each been set in a growth-maximising manner there should be no  
6 evidence of output benefits or costs from reallocating expenditures at the margin. However,  
7 clearly some public spending in OECD countries is allocated to meet non-growth objectives  
8 such as social welfare provision or redistribution. A lack of knowledge of the growth effects of  
9 different spending types may also inhibit growth-maximising policy choices.

16 [Table 3 about here]

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18 In Table 3 we find that most of the expenditure shares exhibit small positive or negative  
19 long-run GDP effects that are not statistically different from zero. However, some changes in  
20 the mix of public expenditures are found to have significant effects on GDP over the long-run.  
21 In interpreting regression parameter estimates it is important to remember that these represent  
22 the combined impact of one additional unit of a particular type of spending (say, education),  
23 financed by reductions in the other spending categories.

24  
25 Table 3 provides evidence of potential *positive* GDP effects from changes in T&C,  
26 education and (less reliably identified) housing spending shares. The parameter for health  
27 spending is positive but small and with a relatively large standard error. Negative long-run  
28 associations with GDP are observed for spending shares for social welfare, defence, economic  
29 services, general public services and recreational services, but only for general public services  
30 is the estimate statistically significant at 10% or less.<sup>20</sup>

31  
32 The parameters in Table 3 can be interpreted as follows: a permanent 1 percentage point  
33 increase in the T&C share in total spending is associated, on average, with a long-run level of  
34 GDP per capita that is 2.2% higher than the counterfactual of an unchanged T&C spending  
35 share.<sup>21</sup> For example, if GDP grows at 2% per year with an unchanged T&C share, GDP would  
36 rise from 100 to 148.6 after 20 years. In comparison a 1 percentage point T&C share increase  
37 would instead raise GDP to around 150.8 after 20 years. This seems a plausible order of  
38 magnitude arising from a persistent non-marginal reallocation (about one-third of the sample  
39 standard deviation) towards a directly growth-enhancing spending category, and away from all  
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55 <sup>20</sup> We have also examined whether the PMG results in Table 3 are preferred to either a MG or DFE approach.  
56 Results reported in the online appendix again confirm that the PMG estimates are preferred, on a Hausman test, to  
57 either the MG or the DFE estimates. Both these other approaches yield noisy parameters for public expenditure  
58 variables.

59 <sup>21</sup> Recall that our results relate to a change *relative to the average of the OECD sample*; hence we assume here that  
60 only one country experiences the simulated T&C spending share increase.



1 other spending categories pro rata. We estimate these other spending types to have smaller  
2 positive, or even negative, impacts on GDP.  
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5 These results align with a number of findings within the current literature. For example,  
6 Nijkamp and Poot's (2004) meta-analysis of fiscal policy and growth supports positive impacts  
7 of infrastructure and public education on GDP. They conclude (p.91): 'On balance, the  
8 evidence for a positive effect of conventional fiscal policy on growth is rather weak, but the  
9 commonly identified importance of education and infrastructure is confirmed'. Our results are  
10 also broadly consistent with those obtained by Acosta-Ormaechea and Morozumi (2013) and  
11 Alfonso and Jalles (2014) for education, health and social security spending, despite quite  
12 different samples and approaches to estimation.  
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### 18 **Dealing with endogeneity concerns**

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20 A commonly cited reason for skepticism regarding the validity or interpretation of aggregate  
21 growth regressions is the possibility that estimated relationships represent correlations but not  
22 causation.<sup>22</sup> We cannot discount the possibility that our evidence so far arises from  
23 simultaneous relationships between GDP and fiscal variables. As well as direct impacts of  
24 fiscal variables on GDP, changes in GDP may induce changes in these fiscal variables. Fiscal  
25 policy changes may also be associated with country-specific time-varying variables, such as  
26 political conditions, that influence GDP levels or growth rates.  
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33 Such arguments with respect to *total* government expenditure or taxation are well known.  
34 Economic downturns reduce taxable capacity and lead to increases in certain types of public  
35 expenditure such as unemployment benefits and social insurance payments. Though these may  
36 be at the expense of other types of expenditure, this is often insufficient to prevent total  
37 spending from rising in downturns. Short run contractions of less cyclically-dependent  
38 expenditures, such as public investment, are typically more difficult to achieve when social  
39 expenditures increase; see Sanz and Velázquez (2004) and Sanz (2010).  
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45 The effect on expenditure components is less clear. As already noted, social welfare  
46 expenditures might be expected to rise in response to an economic downturn yielding negative  
47 correlations with GDP. On the other hand, more 'productive' expenditure shares may rise when  
48 faster GDP growth generates additional revenues, and demands for social welfare-related  
49 expenditures weaken. This would have contrasting effects on the shares of these different  
50 components of expenditure in total expenditure and for total expenditure as a ratio to GDP. In  
51 addition, over the longer term, the income elasticity of demand for education and health may be  
52 high, leading to upward pressure on public spending (Slemrod, 1995).  
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60 <sup>22</sup> See Slemrod (1995) for robust criticisms along these lines regarding cross-country empirical evidence.

1 Pesaran and Shin (1999) contend that, in the context of ARDL models, the problem of  
2 endogenous regressors can readily be handled by the PMG estimator where the regressors are  
3 I(1), subject to some restrictions (such as a unique cointegrating relationship among the  
4 variables, but where regressors are not cointegrated among themselves). In this case, Pesaran *et*  
5 *al.* (1999) and Pesaran and Shin (1999) show that potential endogeneity can be dealt with by  
6 appropriate augmentation of the lag structure of the ARDL(p, q) model – to an ARDL(p, m)  
7 model, where  $m \geq q$ . Endogeneity of I(1) regressors can be accommodated by a projection of  
8 the errors on the regressors. An ARDL with sufficiently long lags overcomes the endogeneity  
9 problem provided the regressors are not cointegrated among themselves, and where interest  
10 focuses on the *long-run* parameters (see Pesaran and Shin, 1999, pp. 372-3; 384-5).<sup>23</sup>

11 To explore these endogeneity issues we first check whether our variables are I(0) or I(1)  
12 and whether they are cointegrated. We then consider the appropriate ARDL(p, m) lag structure.

### 13 *Testing the order of integration and cointegration*

14 We begin by testing whether our variables are I(0) or I(1), using the panel unit root tests of  
15 Harris-Tzavalis (1999), Breitung (2000) and Pesaran (2007). Results are reported in online  
16 appendix Table A3. The Harris-Tzavalis (1999) test assumes that all panels have the same  
17 autoregressive parameter and that the number of time periods is fixed. The Breitung (2000) test  
18 also assumes that all panels have a common autoregressive parameter, but Breitung shows that  
19 the test has power in the heterogeneous case, where each can take its own autoregressive  
20 parameter.<sup>24</sup> The Harris-Tzavalis test rejects the null hypothesis of non-stationarity only for the  
21 budget surplus and employment growth. The Breitung test rejects the null for these two  
22 variables along with the housing and economic services spending shares. In the regression  
23 specifications that are the main focus of our analysis, the budget surplus is omitted.

24 The second generation panel unit root test of Pesaran (2007) – the cross-sectionally  
25 augmented Im, Pesaran and Shin (CIPS) test – allows for heterogeneity in the autoregressive  
26 coefficient of the Augmented Dickey-Fuller regression with lagged cross-sectional mean and  
27 its first difference capturing the cross-sectional dependence. Pesaran (2007) shows that the  
28 CIPS test has satisfactory size and power even for relatively small samples. According to this  
29 test, all of our variables are I(1) except employment growth and the defence spending share.

30 Overall, from these three tests it seems that our variables are best treated as non-stationary  
31 except for the budget surplus and employment growth. After taking first differences, however,

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<sup>23</sup> As Pesaran (1997) emphasises, for the order-augmented ARDL(p, m) model to deal with endogeneity as described above, it is also required that the long-run relationship is unique. Where there is more than one cointegrating relationship, an identification problem must first be resolved; see Pesaran (1997, section II.2).

<sup>24</sup> Breitung's (2000) Monte Carlo simulations suggest that his test is substantially more powerful than other panel unit-root tests for the modest-size dataset he considered (N=20, T=30), which is similar to the sample size here.

1 the panel test (not reported) rejects the null of non-stationarity for each of the variables. From  
2 this we conclude that all the variables are I(1), except employment growth and budget surplus.  
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5 For the cointegration test, we implement the error-correction panel cointegration tests  
6 developed by Westerlund (2007), based on structural rather than residual dynamics, which  
7 therefore do not impose any common factor restrictions. The idea is to test the null hypothesis  
8 of no cointegration by inferring whether the error-correction term in a conditional panel error-  
9 correction model is equal to zero. The tests ( $P_a$  and  $P_t$ )<sup>25</sup> are general enough to allow for a large  
10 degree of heterogeneity, both in the long-run cointegrating relationship and in the short run  
11 dynamics, and dependence within as well as across the cross-sectional units. These test the  
12 alternative hypothesis that the panel is cointegrated as a whole.  
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19 First we test whether the variables included in the PMG regression 1.2 (the base estimation  
20 for subsequent regressions) are cointegrated, including GDP per capita, the fiscal variables and  
21 investment. Online appendix Table A3 shows that  $P_t$  rejects the null hypothesis of no  
22 cointegration for all of our PMG specifications at a 5% level whereas  $P_a$  marginally fails to  
23 reject the hypothesis at a 10% level (p-value 0.13). In contrast, these two tests do not reject the  
24 hypothesis of no cointegration among the regressors. Including expenditure shares in the  
25 cointegration test does not change this conclusion.  
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31 Next we perform the panel cointegration rank test for regression 2 in Table 1 following the  
32 approach of Larsson and Lyhagen (2000) and Larsson et al. (2001). They define the LR-bar  
33 statistic as the average of the  $i$  individual trace statistics, where the test statistic is  
34 asymptotically distributed as a standard normal. We find that the LR-bar test rejects the null  
35 that the maximum rank is zero (the test statistic of 30.25 is far above the 5% critical value of  
36 1.96), but cannot reject the null of a maximum rank of one.<sup>26</sup> Our model therefore appears to  
37 fulfill the first set of Pesaran and Shin (1999) conditions which allows the ARDL model to  
38 overcome endogeneity concerns: namely, I(1) variables, a single cointegrating vector and non-  
39 cointegrated regressors.  
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#### 46 *Testing the ARDL lag structure*

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48 Pesaran *et al.* (1999) and Pesaran and Shin (1999) stress the importance of allowing a  
49 sufficiently long lag structure to deal with endogeneity problems in this ARDL context. Their  
50 proposed method of correcting for endogeneity – in the form of contemporaneous correlation  
51 among the error terms of equations (2.1) and (2.2) – is by ‘appropriate augmentation’ of the lag  
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57 <sup>25</sup> The two differ because they start from a weighted average of the individually estimated coefficients ( $P_a$ ), or their  
58 respective  $t$ -ratios ( $P_t$ ).

59 <sup>26</sup> Similar results (supporting rank  $r = 1$ ) are obtained when the cointegration rank test is applied to the regressions  
60 in Table 3

1 structure of the ARDL(p, q) model. Their method assumes zero cross-correlation among lagged  
2 errors;  $\text{Cov}(\varepsilon_{i,t-i}, u_{i,t-i}) = 0$ , for  $i \neq j$ .

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5 In particular, they show that running an ARDL(p, m) model where  $m = \max(q, s+1)$ , where  
6  $s$  is the appropriately selected lag length of the autoregression of  $\Delta X_{i,t}$  in (2.2), yields consistent  
7 estimates of the long-run parameters of interest. Hence they argue ‘in the context of the ARDL  
8 model inference on the long-run parameters is quite simple and requires *a priori* knowledge or  
9 estimation of the orders of the extended ARDL(p, m) model. Appropriate modification of the  
10 orders of the ARDL model is sufficient to simultaneously correct for the residual serial  
11 correlation and the problem of endogenous regressors.’ (Pesaran and Shin, 1999, p.386).

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17 These arguments for ‘augmentation’ may be thought of as applying to an initial ARDL(p,  
18 q) model where lags are chosen *a priori* as described by Pesaran and Shin above. For example,  
19 where an initially selected ARDL(1, 1) suffers from endogeneity, this may be corrected by  
20 running an ARDL(p, m) model where  $m \geq 1$ , based on estimates of the lag structure from  
21 regression 2.2 in Table 2.<sup>27</sup>

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26 The PMG regressions discussed earlier in this section were obtained using the Pesaran and  
27 Shin (1999) two-step strategy in which we first chose  $p$ , the number of lags for (log) GDP per  
28 capita, and  $m$  the number of lags for our independent variables (fiscal policy, investment and  
29 employment growth). Following Pesaran and Shin (1999), these were chosen using the  
30 Schwarz Criterion (SC) to identify the appropriate lag structure.<sup>28</sup> Hence, arguably our previous  
31 estimates should be regarded as obtained from an ARDL(p, m) model, with suitably long lag  
32 structures.

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38 However, to allow for the possibility that a longer lag structure is required to deal with  
39 possible endogeneity problems, we also ran equivalent regressions to those reported in Table 3  
40 but based on an ARDL(2, 2) model rather than the ARDL( $p \leq 2, m \leq 2$ ) reported there.<sup>29</sup>  
41 Results from the two models, for the expenditure variables of interest, are compared in Table 4.

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45 The key differences between the two specifications, in terms of the expenditure share  
46 parameter estimates, are as follows. Firstly, the two expenditures previously found to have  
47 significant effects on GDP – T&C and education – have smaller parameter estimates in Table 4  
48 but still statistically significant (at the 5% level). Secondly parameters for the other major  
49 spending shares of health, housing, social welfare and defence– are generally larger (in  
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57 <sup>27</sup> Standard errors for long-run PMG parameters were obtained by the delta method.

58 <sup>28</sup> Pesaran and Shin (1999) argue from Monte Carlo experiments on the ARDL(p, m) model that the Schwarz Criterion  
59 is slightly superior to the Akaike Information Criterion.

60 <sup>29</sup> Testing more than two lags is inhibited by the loss of degrees of freedom in our case.

absolute value) in Table 4 and are now statistically different from zero; negative in the case of social welfare and defence.<sup>30</sup>

[Table 4 about here]

Our interpretation of these results is that allowing the model to select the appropriate lag length is preferable. However, including additional lags by imposing two lags for all variables accommodates the possibility that the Schwarz Criterion eliminates some relevant lags. Imposing two-lags tends to *strengthen*, rather than weaken, the case for significant causal effects from a number of public expenditure categories on long-run per capita GDP levels.

#### *Weak exogeneity tests*

As noted earlier, Pesaran and Shin (1999) contend that the PMG overcomes endogeneity, assuming zero cross-correlation among lagged errors. We address this by checking whether our fiscal variables are ‘weakly exogenous’ or ‘long-run forcing’ for the cointegrating parameters. Specifically, we test whether changes in the fiscal variables are statistically unrelated to the error correction term from the Table 3 regressions. Each fiscal variable may still react to its own lagged changes, lagged changes of other variables, or lagged changes in GDP growth. Based on Johansen (1992) and Boswijk (1995), we can test for weak exogeneity by estimating marginal models for each of our fiscal variables, using a variable addition test to assess the statistical significance of the error correction terms obtained from Table 3 regressions for each of those models. If the fiscal variables are confirmed as weakly exogenous, results in Tables 1 and 3 may be interpreted as causal effects.

Following Calderón *et al.* (2015), we test the following marginal models:

$$\Delta x_{i,t} = \beta_{i,1}\Delta X_{i,t-1} + \beta_{i,2}\Delta X_{i,t-2} + \alpha_{i,1}\Delta Y_{i,t-1} + \alpha_{i,2}\Delta Y_{i,t-2} + \delta_i \xi_{i,t}(\hat{\beta}) + \varepsilon_{i,t} \quad (4)$$

where  $x_{i,t}$  represents each element of the vector  $X_{i,t}$ ;  $\xi_{i,t}(\hat{\beta}) = (Y_{i,t-1} - \hat{\beta}X_{i,t-1})$  are the estimated long-run equilibrium error correction (ECM) terms, and  $\varepsilon_{i,t}$  is a random error term.<sup>31</sup> Hence  $X_{i,t}$  includes total expenditure, distortionary and non-distortionary taxes, non-residential private investment (all as percentages of GDP), and the shares of each expenditure component in total public expenditure. The null hypothesis of weak exogeneity involves testing  $\delta_i = 0$ , as a  $t$ -test on the  $\delta_i$  for each variable, or as a Wald test that the  $\delta_i$ s for all suspected endogenous variables are jointly zero. Rejection of the null implies rejection of weak exogeneity.

For each of the nine estimations in Table 3, we estimate five different marginal models (for the four fiscal variables and investment) country by country. Like Calderón *et al.* (2015), we use the MG approach to average the coefficients on the ECM term across countries and then

<sup>30</sup> The smaller spending categories (general public, and recreation, services) appear non-robust, involving a change of sign.

<sup>31</sup> Results reported below from regressions on (4) also include a constant term.

1 test its significance across the panel countries as a whole using the MG standard error. We also  
2 perform the equivalent weak exogeneity test for each country separately.  
3  
4

5 Row 1 of Table 5 shows results for weak exogeneity tests on the three fiscal variables and  
6 investment entering Table 1 regression (2) – the equivalent regression to those reported in  
7 Table 3 but excluding any expenditure share variables. Each cell contains the estimated  $t$ -ratio  
8 for the unweighted average MG error correction parameters for each country. Beneath these  
9 values, the cell indicates the number of individual countries for which the null hypothesis of  
10 weak exogeneity is rejected. It can be seen that, for three of the four fiscal/investment variables  
11 we cannot reject weak exogeneity across the sample of countries. While the null hypothesis is  
12 rejected across all countries combined for one variable, ‘distortionary and other taxes’, it is  
13 clear that for all four variables in row 1, the null hypothesis is not rejected in all, or nearly all,  
14 countries. Hence one or two countries appear to drive the overall result for distortionary taxes.  
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21 [Table 5 about here]  
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23 Rows 2-10 show weak exogeneity test results for each expenditure share in Table 3. Of  
24 particular interest here is the  $t$ -ratio on each expenditure share variable (column 1). This reveals  
25 that, only in the case of the T&C and economic services share is the mean MG parameter in the  
26 marginal model statistically significantly different from zero (at 5%). More importantly  
27 perhaps, across the 9 expenditure shares there is typically only 1 or 2 of the 17 countries for  
28 which the individual country value of  $\delta_i$  is statistically non-zero. These results offer fairly  
29 strong support to the view that the expenditure share variables can be considered weakly  
30 exogenous allowing us to interpret the estimated long-run expenditure parameters as capturing  
31 causal effects on GDP.  
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39 A similar conclusion emerges from Table 5 for the other variables in the regression. That  
40 is, the average MG parameter (across all countries) is not significantly different from zero in  
41 most cases, and this is also true for the vast majority of individual country estimates. Out of a  
42 total of 36 parameter sets (9 spending shares  $\times$  4 variables) for 17 countries, weak exogeneity is  
43 never rejected in more than 4 out of 17 countries, is rejected for 3 or 4 out of 17 countries only  
44 8 times (out of a possible 36), with the remainder rejected in only 0–2 out of the 17 countries.  
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49 Finally, the parameter  $\hat{\beta}$  in (4) which we have subjected to the weak exogeneity test is  
50 obtained from the previous model in which lag lengths were selected endogenously using the  
51 Schwarz Criterion, as described above. Given some uncertainty regarding the number of lags  
52 required to remove potential endogeneity, we re-ran Table 1 and Table 3 regressions on  
53 equation (3) imposing an ARDL (2, 2) structure. Weak exogeneity tests on these  $\hat{\beta}$  estimates  
54 are reported in the online appendix. If anything, they reveal even stronger support for weak  
55 exogeneity.  
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1 From results in the previous sub-section we are inclined to be circumspect and, based on  
2 Table 3, claim evidence of exogenous positive impacts on GDP from changes in transport &  
3 communications and education spending. However, there is some, more limited support for  
4 positive (negative) effects on GDP from reallocating spending towards health and housing  
5 (social welfare and defence).  
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### 9 **Short run dynamics**

10 As the short run dynamics in the PMG regressions are allowed to differ across countries, it  
11 is possible that the short run output effects of a given fiscal change differ across countries both  
12 in initial direction and in the speed with which the long-run effect is realised. The trajectory  
13 followed by each country is determined by the estimated lag structure on each fiscal variable  
14 for that country together with its error-correction parameter.  
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20 Given the noise associated with estimates of the short run dynamics for each country, little  
21 reliance can be placed on these estimates and we do not discuss them here.<sup>32</sup> Instead, Figure 1  
22 shows the average time path of GDP per capita growth rates associated with a 1 percentage  
23 point change in year 0 in each of the larger expenditure categories: T&C, education, health, and  
24 social security, based on Tables 3 regressions. We use the average of the country short run  
25 PMG parameters; hence obtained under the assumption of homogeneous long-run parameters  
26 which, for health and social welfare, are not statistically different from zero.  
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32 [Figure 1 about here]

33 As expected the figure suggests that GDP per capita *growth* converges on zero, as the level  
34 of GDP per capita converges on its new long-run value following the 1% ‘shock’. However for  
35 the two significantly positive spending types (T&C, education) there is considerable persistence  
36 with effects on growth rates still evident after 20 years. In addition, the immediate impact (in  
37 years 1-2) is noticeably more positive for T&C than for education – a result that seems  
38 plausible given the known immediate output effects of infrastructure-related investment  
39 compared to education spending which can be expected to take longer to affect output. For  
40 social security, Figure 1 suggests the possibility of negative short run growth effects – also a  
41 plausible result as spending is reallocated from categories likely to be less dominated by  
42 transfers spending.  
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### 51 **V. Conclusions**

52 This paper has offered new evidence for OECD countries on the impact of the size and  
53 composition of public expenditure on per capita GDP. Previous regression analyses have been  
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57 <sup>32</sup> In addition, Pesaran and Shin (1999, pp.372, 384-5) warn that, even though the long-run parameters obtained  
58 from the PMG are consistent and converge to their asymptotic values relatively quickly, short-run parameters  
59 converge more slowly. They also conclude that consistent estimation of short-run parameters requires explicit  
60 modelling of the contemporaneous dependence among the error terms of equations (2.1) and (2.2).



1 unclear about which specific elements of the government budget are omitted from their  
2 analyses, or interpretation of parameter estimates is made difficult where several elements are  
3 explicitly or implicitly omitted. Clearly however, the method of financing any spending  
4 increase matters, whether this involves higher taxes (with differing distortionary  
5 characteristics), higher deficits, or reductions in alternative spending categories. Previous  
6 studies have also generally tested endogenous growth model specifications with *permanent*  
7 growth effects, and endogeneity concerns remain regarding the reliability of these estimates.  
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13 Our examination of the impact of public expenditure on GDP for OECD countries has  
14 sought to deal explicitly with the financing aspects, allowed output growth effects to be  
15 persistent rather than permanent, and specifically addressed potential endogeneity concerns.  
16 Using longer time-series data than has been examined hitherto allowed us to apply more  
17 flexible (pooled mean group) methods that can accommodate heterogeneous short run  
18 responses across countries. We examined the GDP impacts of changes in total government  
19 expenditure, alongside changes in the shares of spending devoted to various categories.  
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25 Both our initial PMG results, and those that explicitly account for contemporaneous  
26 correlation, find robust long-run positive effects on per capita GDP levels for transport &  
27 communication and education, with some evidence supporting positive (negative) effects for  
28 housing and health (social welfare) spending. Results do not support positive long-run output  
29 effects from switching expenditure towards defense spending (sometimes claimed to have  
30 growth benefits). In our estimates, this switch involves pro-rata reductions in other spending.  
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36 Though our growth model specification precludes permanent effects on the *growth rate* of  
37 per capita GDP, we nevertheless estimate relatively persistent effects on growth rates from  
38 simulated T&C and education expenditure changes that last for 20 years or more. Our results  
39 also confirm that the assumed form of expenditure financing is crucial and we find evidence of  
40 negative long-run effects on output from deficit-financed increases in total public spending.  
41 Our interpretation is that such spending increases cannot be expected to be growth-enhancing  
42 unless the specific forms of that expenditure are considered carefully.  
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48 The evidence of positive long-run effects on GDP per capita from increases in transport &  
49 communications, and education, spending *shares* appear to be robust and are consistent with  
50 the message from an increasing body of recent research. This points to the possibility that  
51 current levels of such spending in OECD countries are sub-optimal from a growth perspective,  
52 though clearly this may be consistent with a wider social welfare objective.  
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TABLE 1

*Testing expenditure levels & implicit financing: pooled mean group estimates*

Dependent variable: Annual GDP per capita growth rate

Regression No.:	1	2	3	4	5
<i>Fiscal variables financed by:</i>	<i>Non-distort. taxes</i>	<i>Budget Surplus</i>	<i>Distortionary taxes</i>	<i>Distortionary &amp; non-distort. taxes</i>	<i>Budget Surplus</i>
<i>Long-run effects</i>					
Budget surplus	0.022** (2.34)	-	-0.020** (4.64)	-0.013** (3.08)	-
Distortionary taxes	-0.060** (-4.56)	-0.012** (2.84)	-	-	-0.014** (3.11)
Non-distortionary taxes	-	0.023** (4.71)	0.051** (7.78)	-	0.029** (5.27)
Total Public Expenditure	0.020** (2.17)	-0.013** (5.29)	-0.024** (6.12)	-0.012** (3.44)	-0.012** (4.94)
Investment ratio	0.029** (2.79)	-0.007** (2.41)	0.021** (3.32)	-0.004 (1.08)	-
Employment growth	0.063** (4.72)	0.020** (4.93)	0.041** (7.03)	0.039 (6.37)	0.026** (5.12)
Lagged residual	-0.039** (4.80)	-0.092** (6.16)	-0.062** (5.12)	-0.077* (6.32)	-0.092** (5.49)
<i>Short-run effects (average)</i>					
Short-run. Total Public Expenditure. 1 <sup>st</sup> Difference	-0.002 (1.83)	-0.001 (1.44)	-0.001* (2.07)	-0.001 (1.57)	-0.001** (2.93)
Short-run. Total Public Expenditure. 2 <sup>nd</sup> Difference	-0.000 (1.00)	0.000 (1.19)	0.000 (1.14)	0.000 (0.00)	0.000 (1.18)

Notes: *t*-statistics in parentheses below parameters; \* (\*\*) = significant at the 5% (1%) respectively.

TABLE 2

*Testing expenditure levels: comparing regression methods*

Dependent variable: Annual GDP per capita growth rate

Regression No.	2.1	2.2	2.3	
<i>Financed by: Budget Surplus</i>	Pooled Mean Group	Dynamic Fixed Effects:	Mean Group:	Hausman Tests
Distort. tax & other revenues	-0.012** (2.84)	-0.015 (1.71)	-0.018 (0.74)	$\chi^2(1)$ 0.08 p-value 0.78
Non-distortionary taxes	0.023** (4.71)	0.024 (1.72)	0.094 (1.50)	$\chi^2(1)$ 1.30 p-value 0.25
Total public expenditure	-0.013** (5.29)	-0.007 (1.28)	-0.020 (1.65)	$\chi^2(1)$ 0.36 p-value 0.55
MG versus PMG				$\chi^2(5)$ : 7.20 p-value 0.21
PMG versus DFE				$\chi^2(5)$ : 16.51 p-value 0.00

Notes: *t*-statistics in parentheses below parameters; \* (\*\*) = significant at the 5% (1%) respectively.

All regressions include the three non-fiscal control variables shown in Table 1. All coefficients are estimated long-run effects

TABLE 3

*Testing public expenditure composition: pooled mean group estimates*

Dependent variable: Annual GDP per capita growth rate

Regression:	1	2	3	4	5	6	7	8	9
Share of:	T&C	Educ- tion	Health	Hous- ing	Social welfare	Defence	Econ. Serv.	Gen. pub. Serv.	Recreat- ion
Expenditure share	0.022** (9.47)	0.020** (4.92)	0.001 (0.80)	0.009 (1.56)	-0.001 (1.00)	-0.001 (0.57)	-0.005 (1.35)	-0.005** (2.86)	-0.005 (0.30)
Total expenditure	0.003 (1.80)	-0.014** (5.24)	-0.006 (1.90)	-0.016** (6.09)	-0.008 (4.00)	-0.004* (2.32)	-0.014** (3.17)	-0.011** (5.86)	-0.007 (1.99)
<i>Short-run effects (average)</i>									
Expenditure share (1 <sup>ST</sup> dif)	0.000 (0.40)	-0.001 (1.29)	0.000 (0.22)	-0.000 (0.04)	-0.001* (2.21)	-0.001 (0.61)	0.001 (0.67)	0.000 (0.38)	0.005 (1.23)
Expenditure share (2 <sup>ND</sup> dif)	0.001 (1.08)	-0.001 (1.42)	-0.000 (0.10)	-0.000 (0.17)	-0.000 (0.63)	0.001 (1.44)	0.001 (1.37)	0.001 (1.47)	-0.001 (1.00)

Notes:

- (1) All regressions in the Table take the same form as regression 2 in Table 1, augmented to include each individual expenditure share. That is, included variables are total public expenditure; distortionary taxes, 'other' revenues; non-distortionary taxes; investment ratio; employment growth, lagged GDP per capita; the Budget surplus is excluded.
- (2) *t*-statistics in parentheses below parameters; \* (\*\*) = significant at the 5% (1%) respectively.

TABLE 4

*Testing public expenditure composition under alternative lag structures*

Regression:	1'	2'	3'	4'	5'	6'	7'	8'	9'
Share of:	T&C	Education	Health	Hous- ing	Social welfare	Defence	Econ. Serv.	Gen. pub. Serv.	Recreat- ion
<i>ARDL (2, 2) model</i>									
Expenditure share	0.003** (2.58)	0.004** (2.48)	0.012** (3.64)	0.037** (4.95)	-0.008** (3.90)	-0.007** (10.10)	-0.001 (0.37)	0.013** (3.97)	0.029* (2.12)
Total Expenditure	-0.012** (7.19)	-0.012** (6.65)	-0.015** (5.66)	-0.016** (4.81)	-0.004 (1.88)	-0.002** (2.77)	-0.014** (6.17)	-0.003 (0.74)	-0.015** (7.19)
<i>ARDL (p, q) lag structure (p, m ≤ 2) chosen by Schwarz criterion (Table 3)</i>									
Expenditure share	0.022** (9.47)	0.020** (4.92)	0.001 (0.80)	0.009 (1.56)	-0.001 (1.00)	-0.001 (0.57)	-0.005 (1.35)	-0.005** (2.86)	-0.005 (0.30)
Total Expenditure	0.003 (1.80)	-0.014** (5.24)	-0.006 (1.90)	-0.016** (6.09)	-0.008 (4.00)	-0.004* (2.32)	-0.014** (3.17)	-0.011* (5.86)	-0.007 (1.99)



TABLE 5

*Weak exogeneity tests for fiscal and investment variables*

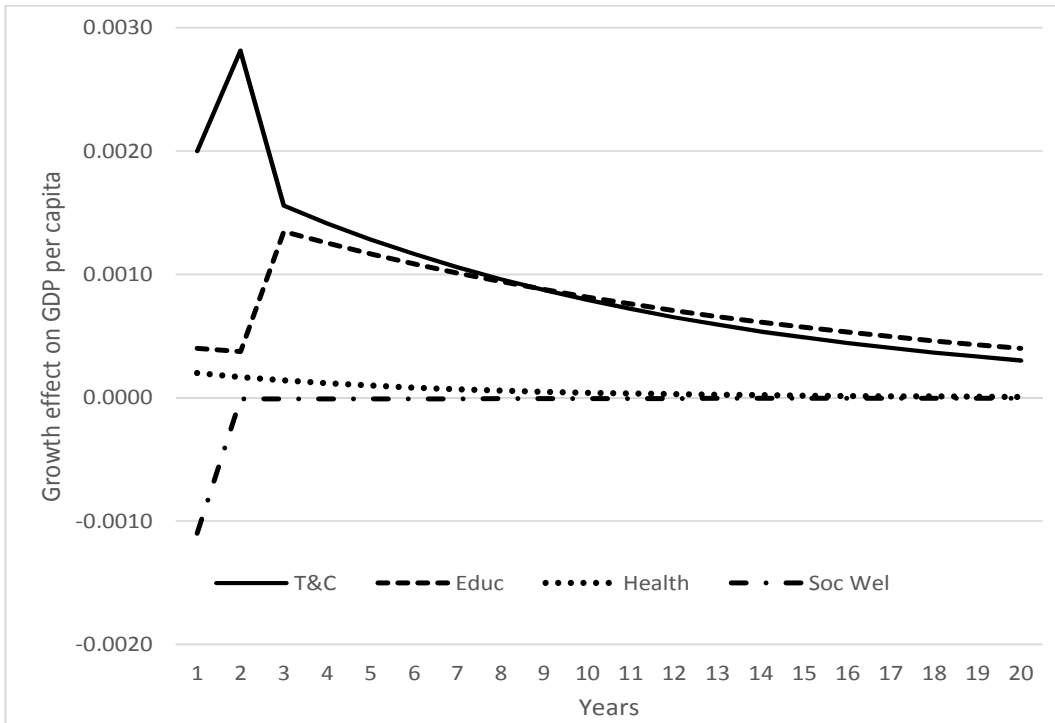
	<i>Expenditure share</i>	<i>Total expenditure</i>	<i>Distortional &amp; other taxes</i>	<i>Non-distortional taxes</i>	<i>Investment ratio</i>
Table 1: regression 2	-	-0.928 (0.99)	-3.429 (2.08)**	-1.110 (1.01)	-0.868 (0.74)
		0 countries	2 countries	0 countries	2 countries
Table 3 regressions:					
T&C	1.747 (2.46)**	0.522 (0.21)	-2.911 (1.99)**	0.714 (0.55)	0.756 (0.72)
	1 country	2 countries	1 country	1 country	1 country
Education	-0.735 (1.03)	-2.382 (2.04)**	-1.790 (1.79)	0.227 (0.30)	-1.013 (0.90)
	0 countries	2 countries	1 country	1 country	1 country
Health	-0.948 (1.29)	-3.901 (2.71)***	-3.023 (2.75)***	-1.002 (1.59)	1.589 (1.56)
	0 countries	2 countries	4 countries	4 countries	2 countries
Housing	0.427 (0.62)	-2.523 (2.19)**	-3.685 (2.18)**	-1.675 (1.40)	-0.046 (0.06)
	1 country	0 countries	1 country	1 country	0 countries
Social Welfare	-0.435 (0.12)	-3.294 (1.61)	-1.356 (1.40)	0.535 (0.95)	-1.753 (0.88)
	4 countries	0 countries	1 country	1 country	3 countries
Defence	0.179 (0.20)	-0.482 (0.21)	-3.397 (1.47)	-2.328 (1.56)	0.073 (0.05)
	1 country	2 countries	1 country	2 countries	0 countries
Economic Services	-1.973 (2.06)**	-3.489 (3.29)***	-2.270 (2.57)**	-0.651 (1.38)	1.333 (2.06)**
	2 countries	2 countries	4 countries	1 country	3 countries
General Public Services	-0.669 (0.54)	-3.402 (1.94)*	-2.369 (1.52)	-0.175 (0.22)	3.068 (3.50)***
	1 country	1 country	1 country	2 countries	2 countries
Recreation	-0.124 (0.80)	-3.189 (2.46)***	-2.782 (2.52)***	-0.609 (0.83)	2.003 (1.71)
	1 country	1 country	3 countries	4 countries	3 countries

Notes: *t*-statistics in parentheses below parameters. . \* (\*\*, \*\*\*) = significant at the 10% (5%, 1%) level. Terms such as “2 countries” refers to the number of country-specific weak exogeneity tests where exogeneity is rejected at the 5% level.



FIGURE 1

Growth effect of a 1 percentage point change in the expenditure mix (in year 0)



Note: Based on the results from Table 3.

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