DISCUSSION PAPER SERIES

No. 8563

ESTIMATING INCOME ELASTICITY OF GOVERNMENT EXPENDITURES: EVIDENCE FROM OIL PRICE SHOCKS

Markus Brückner, Alberto Chong
and Mark Gradstein

PUBLIC POLICY

Centre for Economic Policy Research

www.cepr.org

Available online at: **www.cepr.org/pubs/dps/DP8563.asp**

ISSN 0265-8003

ESTIMATING INCOME ELASTICITY OF GOVERNMENT EXPENDITURES: EVIDENCE FROM OIL PRICE SHOCKS

Markus Brückner, University of Adelaide
Alberto Chong, George Washington University
Mark Gradstein, Ben-Gurion University and CEPR

Discussion Paper No. 8563
September 2011

Centre for Economic Policy Research
77 Bastwick Street, London EC1V 3PZ, UK
Tel: (44 20) 7183 8801, Fax: (44 20) 7183 8820
Email: cepr@cepr.org, Website: www.cepr.org

CEPR Discussion Paper No. 8563

September 2011

ABSTRACT

Estimating Income Elasticity of Government Expenditures: Evidence from Oil Price Shocks

We estimate the income elasticity of government expenditures using variation in the international oil price as a plausibly exogenous source of within-country variation of countries' permanent income. Our short run elasticity estimates, between 0.25-0.50, are generally somewhat smaller than the previously obtained ones, and they, in particular, indicate that Wagner's law does not hold; long run elasticities are larger, but still smaller than unity. We also explore the correlates of the income elasticity of government spending and find no support for views that either democracy, inequality, or openness are associated with a larger elasticity. However, we find evidence consistent with "voracity" theories: cross-country differences in ethnic polarization are associated with a significantly higher oil price driven income elasticity of government spending.

JEL Classification: H1
Keywords: Wagner law

Markus Brückner
University of Adelaide
Department of Economics
10 Pulteney Street
5000 Adelaide
AUSTRALIA

Email:
markus.bruckner@adelaide.edu.au

For further Discussion Papers by this author see:
www.cepr.org/pubs/new-dps/dplist.asp?authorid=167251

Alberto Chong
Research Department
Inter-American Development Bank
Stop W-0436
1300 New York Avenue NW
Washington DC 20577
USA

Email:
albertoch@iadb.org

For further Discussion Papers by this author see:
www.cepr.org/pubs/new-dps/dplist.asp?authorid=152471

Mark Gradstein
Department of Economics
Ben-Gurion University
Beer-Sheva 84105
ISRAEL

Email: grade@bgu.ac.il

Submitted 04 September 2011

1. Introduction

This paper is concerned with estimating the elasticity of government expenditures with respect to national income in recent decades. Its major contribution is in using, in addition to country and time fixed effects, a plausible source of exogenous variation in countries' permanent income to address the issue at hand. This approach, it is hoped, should lead to a more accurate assessment of the causal effect that within-country changes in permanent income have on government spending. As such, this work is related to two main lines of research, one that speaks to long term income elasticity, and another that is concerned with short term variations in government expenditures.

A common theme in the public finance literature is the emergence of the modern state, endowed with fiscal capacity and committed to the provision of an array of public services to its citizens (Atkinson and Stiglitz, 1980, Lindert, 1994, 1996, 2004a, 2004b). Indeed, the growth of government, specifically, the volume of government expenditures relative to national income, in the course of the past century has been documented and discussed (see Holsey and Borcherding, 1997, for a survey; Tanzi and Schuknecht, 2000, consider the growth of government from a long term perspective). Among other explanations, the so called Wagner's law stipulates that, as the economy experiences economic growth, the relative government size increases. It has been suggested that broad historical accounts to a certain extent confirm this pattern of co-evolution (see Lindert, 2004a, 2004b). This stipulation has also been subjected to numerous empirical tests, rooted

in time series analyses. The very large literature to date (see Durevall and Henrekson, 2011, for a comprehensive reference list) has come up with ambiguous results,.[1] Generally, the results seem to depend on the period covered, the sample of countries, and the model specification. In particular, several recent papers (see Durevall and Henrekson, 2011, for their review) conclude that, for considerable periods, the relative size of the public sector has not increased. This literature by and large employs standard panel data analyses or co-integration techniques to address the joint co-evolution of national income and the size of the government.

A key challenge that the literature is well aware off but continuing to struggle with is how to identify the causal effects of within-country changes in national income on government expenditures. If government spending itself has an effect on national income, then a partial correlation between income and government spending will not identify the causal effect that changes in national income have on the relative size of government. A further key challenge in estimating the income elasticity of government expenditures is how to distinguish the effects of transitory changes in national income from the effects of permanent changes. This is particularly an issue when using within-country variation as in

1 For example, Ram, 1987, argues that there is qualified support for Wagner's law; Shelton, 2007, finds none; whereas Akitoby et al., 2006, and Easterly and Rebelo, 1993, find that it does hold in their respective data. Durevall and Henrekson, 2011, provides a very useful comprehensive list of references, from which the controversy is evident.

this case a partial correlation between income and government spending will reflect a combination of permanent and transitory changes in income.

In this paper, therefore, our objective is to complement existing work by explicitly estimating the causal effect of permanent within-country changes in income on the size of government. To accomplish this goal, we employ time and country fixed effects, and use year-to-year variation in the international oil price multiplied by countries' average (and thus time-invariant) share of oil exports in GDP as an instrumental variable to generate exogenous within-country variation in national income. Because year-to-year variation in the international oil price follows a random walk, our instrumental variables estimates can be interpreted as capturing the effects of permanent within-country changes in income on government spending.

Our empirical analysis of cross country panel data, covering 184 countries during the period 1960-2007, reveals that, as national income increases, so does the average size of the government. The effect is statistically significant and economically large. In most specifications, the average government size increases by more than a quarter in response to a one percent increase in the national income. Adding lagged income shocks increases the obtained elasticity of government spending with respect to national income to about one half; long run elasticities (for longer periods of changes) are somewhat higher but still less than one. This is also true when particularly large importers and exporters of oil, as well as countries with nationalized oil production, are excluded from the analysis. The estimated

elasticities imply, inter alia, that the relative government size decreases on average with national income in the period covered by this study. Distinguishing between countries based on their level of economic development, we find that the elasticity of government spending is somewhat smaller in high-income countries versus low- and medium-income countries, although not significantly so. We also find that the investment component is more elastic than the consumption component of the government spending.

A separate literature that is also relevant for this research addresses the income elasticity of public spending from a short term perspective. Its point of departure is the cyclicality of public spending over the business cycle. This literature typically finds procyclicality among developing economies and is somewhat more ambiguous in regard to cyclical behavior among developed countries (Lane, 2003, Talvi and Vegh, 2005, Ilsetzki and Vegh, 2007). Our findings indicate a positive response of government spending to permanent changes in income even in the short run, which suggests that, when examining short run responses of government spending, it is crucial to separate transitory from permanent changes in national income.

We further undertake analysis of the correlates of the income elasticity of government spending. To do so, we first estimate the income elasticity of government spending separately for each country in our sample. Then, treating the obtained estimates as a left-hand-side variable, we consider the effects of some variables commonly perceived as having an impact on the size of government. This exercise allows us to examine whether there are

systematic differences in country characteristics that determine the size and sign of the income elasticity response of government spending. We consider in our analysis the impact of the level of democracy (see Lindert 2004a, 2004b, arguing that democratization brought about an increase in the size of government); trade openness (see Rodrik, 1998, who finds that trade openness leads to a larger government size); the level of national income (to examine whether the income elasticity of government spending changes as the economy becomes more developed); as well as a measure of ethnic polarization (the theoretical work of Lane and Tornell, 1998, 1999, explicitly suggests that, due to appropriation motives by various population groups, the increase in the size of government to income shocks should be particularly large in polarized countries). Among these we find that only the latter is significantly positively correlated with the income elasticity of government spending, thus offering empirical support to the appropriation theory of Lane and Tornell, 1998, 1999.

The paper proceeds as follows. The next section describes the data and the construction of our instrumental variable. Section 3 presents the empirical framework. Section 4 contains the main results, and Section 5 concludes.

2. Data

Oil Price Shocks. Data on the oil price shocks instrument are from Bruckner et al. (2011). Bruckner et al. obtain data on the international oil price for the 1960-2007 period from UNCTAD Commodity Statistics and data on oil exports and imports from the NBER-United Nations Trade Database. Because the level of the oil price displays a unit root (the AR(1)

coefficient is 0.99; see Bruckner et al. (2011) for a more detailed analysis that demonstrates the unit-root behavior of the oil price for the 1960-2007 period and the permanent effects of this variable on income), oil price shocks are identified by the log-change of the oil price:

$$\text{(1)} \qquad \text{OilPriceShock}_{ct} = \Delta \ln(\text{OilPrice})_t * \theta_c$$

Note that equation (1) takes into account that the impact of the oil price shock is larger for countries that are very dependent on oil exports (imports), by weighting the oil price by the average (i.e. time-invariant) share of net oil exports in GDP θ_c.[2] The average share of net oil exports in GDP is computed as the period average value of oil exports minus imports divided by GDP. The sample maximum (minimum) value of θ_c is 0.18 (-0.03); the mean (median) is 0.009 (-0.001); and the interquartile range is [-0.005, 0.002].

Income and Government Expenditures. Annual real per capita GDP data are for the period 1960-2007 from the Penn World Table, version 6.3 (Heston et al. 2009). We also obtain from the Penn World Table data on real government consumption expenditures per capita.

2 This functional form of the oil price shock is motivated by log-linearizing output around steady-state and taking the total differential with respect to output, $y_{c,t}$, and the oil price, P_t. This yields that $\Delta\log(y_{c,t})=\theta_c\Delta\log(P_t)$, where θ_c is the steady-state share of net oil exports in output of country *c*. Also note that the constructed oil price shock variable does not use within-country changes in the amount of oil produced to identify the oil price shock, because within-country changes in the amount of oil produced could be endogenous to within-country changes in output.

3. Estimation Framework

A variety of specifications have been used to test the Wagner hypothesis (see e.g., Ram, 1987, Durevall and Henrekson, 2011, for some examples). In accord with much of the literature, we employ the following econometric specification:

$$\Delta \ln(G)_{ct} = a_c + b_t + \beta \Delta \ln(GDP))_{ct} + z_{it} \quad (2)$$

where a_c and b_t are country and year fixed effects; $\Delta \ln(G)$ is the annual change of the log of real government consumption expenditures per capita; $\Delta \ln(GDP)$ is the annual change of the log of real GDP per capita; $z_{c,t}$ is an error term that is clustered at the country level.

Our main method of estimation is two-stage least squares. In the two-stage least squares estimation we instrument real per capita GDP by our oil price shock variable. By doing so, we use a plausibly exogenous source of variation in countries' GDP per capita to examine the link between income and government spending. Because year-to-year variations in the international oil prices are very persistent, it is important to note that in the two-stage least squares estimation we identify the effects that permanent shocks to GDP per capita have on government expenditures. The exclusion restriction for the two-stage least squares estimation states that oil price shocks should have no systematically strong effects on a country's government spending beyond the effect that oil price shocks have on GDP. We will discuss and examine this exclusion restriction in detail in the next Section.

A further important issue in our panel data estimation is that the response of government spending to permanent income shocks might not be on impact. In fact, in the fiscal policy literature a common assumption is that on a quarterly basis government spending policy does not react contemporaneously to changes in the economic environment (e.g. Blanchard and Perotti, 2002). The reason is that there are significant implementation lags in government spending. However, it is unclear whether on an annual basis a similar argument holds. We therefore approach this issue from an agnostic standpoint and examine both the impact and the lagged effects of income on government spending. The way equation (2) is written, the coefficient β captures the impact effect that year-to-year changes have on government spending. We can readily examine whether there is a significant lagged response of government spending to changes in income by including further lags of income on the right-hand side of the estimating equation. Indeed, in the empirical analysis we will present estimates from a regression that includes both the impact as well as the lagged effect of income on government spending. We note that one of the key advantages of our annual panel data approach is that it allows us to examine the short run as well as the longer run effects that within-country changes in income have on government spending by including the impact and the lagged effects of income on the right-hand side of the estimating equation. Subsequently, we also explore long run elasticities by considering changes in both national income and government spending over longer periods.

4. Results

4.1. Baseline Estimates

Panel A of Table 1 presents our baseline two-stage least squares estimates of the effect that oil price driven changes in GDP per capita have on government consumption expenditures per capita. Column (1) shows estimates where we include the effect of year *t* changes in GDP per capita as well as the effects of year *t*-1 and *t*-2 changes in GDP per capita on the right-hand side of the estimating equation. The main result is that the coefficients on all three variables are positive; however, statistically significant is only the *t*-1 effect. In column (2) we show that a similar result holds if we include the effect of year *t* changes in GDP per capita and the year *t*-1 effect without the year *t*-2 effect on the right-hand side of the estimating equation. The estimated coefficients are very similar in size, and only the *t*-1 effect is statistically significant at the 5 percent significance level. Quantitatively, the year *t*-1 effect is more than half the size of the year *t* effect and the estimated elasticity coefficient implies that on average a one percent increase in GDP per capita due to oil price shocks was associated with an increase in government consumption expenditures by about a quarter of a percent. (Column (3) documents that result are also very similar if we only include the year *t*-1 effect on the right-hand side of the estimating equation.) Yet, the cumulative elasticity of government spending with respect to GDP changes over the years t, t-1, and t-2 is 0.56, and this cumulative elasticity is statistically significant at the 1 percent level.[3]

3 We have also checked the results when using lags of GDP changes over the years t, t-1,…,t-5. The main finding is that the t-1 lag continued to be statistically significant, the other lags were statistically

These baseline estimates suggest that the average response of government expenditures to within-country changes in GDP per capita is positive but not significantly larger than one. Hence, these two-stage least squares estimates suggest that the share of government consumption spending in GDP as a response to oil price shocks decreased on average during the 1960-2007 period.

Another way to illustrate this result is to use as the main dependent variable the share of government spending in GDP. Columns (4)-(6) of Table 1 report these estimates. The main result is that in year *t* the share of government in GDP significantly declined. However, in the following year where government spending responded significantly to the oil price driven change in GDP per capita, the government expenditure share increased significantly. We note that the sum of the impact and lagged effect is not significantly positive and hence, that there is no evidence from these regressions that on average the government expenditure share increased due to a plausibly exogenous and permanent within-country increase in GDP per capita.

Panel B of Table 1 presents the corresponding least squares estimates. Columns (1)-(3) show that in year *t* the least squares estimates produces a significant positive coefficient on government expenditures. The estimated coefficient is 0.5, significantly different from zero at the 1 percent significance level, but not significantly larger than 1. Hence the least-

insignificant, and the cumulative effect was 0.56 and significant at the 1 percent level. Hence, using further lags of income does not produce a different cumulative elasticity response than what is reported in our baseline estimates in Table 1.

squares estimates also indicate that the share of government spending in GDP did not increase significantly as income per capita increased.

It is likely, however, that the least-squares estimate does not reflect the true causal effect that a permanent within-country change in GDP per capita has on government spending. This is because government spending itself may have a significant positive effect on GDP per capita. Hence, a least-squares estimate is likely to suffer from an upward bias due a positive reverse causal effect of government spending on GDP. Stated differently the partial correlation between GDP per capita and government spending that drives the least-squares estimate does not reflect a causal relationship because a partial correlation is silent about the direction of causality. When we use our international oil price instrument in the two-stage least squares estimation, we capture the effect that a permanent and plausibly exogenous change in GDP per capita has on government spending. A positive reverse causality bias may therefore be one reason why the least-squares estimate is quantitatively larger than the two-stage least squares estimate. Another potential reason for why the least-squares estimates deviate from the two-stage least squares estimate is classical measurement error in national accounts statistics. Classical measurement error will attenuate the least-squares estimates towards zero, but not the two-stage least squares estimates. Classical measurement error can, therefore, explain why quantitatively the t-1 effect of the two-stage least squares estimate is quantitatively larger than the corresponding least-squares estimate.

4.2. Discussion of Instrument Quality and Exclusion Restriction

We note that the quality of our instrumental variable in terms of the first-stage fit is reasonable. The first-stage F-statistic for the two-stage least squares regression where we instrument *t*-1 GDP per capita with the *t*-1 oil price shock is above 30. Hence, the first-stage F-statistic is well above the Staiger and Stock (1997) rule-of-thumb criteria of 10 for instruments to be declared weak. Also for the regression where we use both year *t* and *t*-1 GDP per capita as endogenous regressors, instrumented by year *t* and *t*-1 oil price shocks, the first-stage F-statistic is reasonably large: according to the tabulations in Stock and Yogo (2005) the value of 8.39 allows us to reject at the 5 percent significance level that the maximal IV size distortion is larger than 10 percent. Because our baseline regressions show that it is the *t*-1 effect of GDP per capita on government spending that is statistically significant we report in the following tables robustness checks that examine the robustness of the *t*-1 effect to various different specifications and sub-samples.

In Table 2 we examine the robustness of our instrumental variables estimates to including the lagged level of government spending per capita on the right-hand side of the estimating equation. The convergence coefficient on lagged government spending is -0.07 and highly statistically significant. More importantly, however, column (1) of Table 2 shows that when we instrument GDP per capita by oil price shocks we continue to obtain a positive and statistically significant effect of lagged GDP per capita on government spending. In fact, the estimated coefficient is quantitatively very similar to our baseline estimate in Panel A of

Table 1. For comparison purposes, we show in column (2) the estimates that we obtain if we do not instrument GDP per capita, but instrument lagged government spending instead; in column (3) if we instrument only GDP per capita by the oil price shock; and in column (4) if we instrument neither lagged government spending nor lagged GDP. The main conclusion from these regressions is that instrumenting GDP per capita by our oil price shock variable is crucial for obtaining a significant positive and quantitatively sizable effect of income on government spending.

A further important check on our instrumental variables estimation is whether conditional on lagged income the lagged oil price instrument is statistically insignificant. This is the implicit exclusion restriction underlying our instrumental variables regressions. It would be violated if, for example, oil price shocks significantly affected the distribution of income and if this change in the income distribution significantly affected government spending beyond the average income per capita effect.

As a first step to examining the exclusion restriction, we report in column (1) of Table 3 instrumental variables estimates that use, in addition to the oil price shock, the *t*-2 change in income per capita as an instrumental variable. With two instruments in hand, we can conduct the Hansen overidentification test that examines jointly the validity of the instruments. The p-value of this Hansen test is 0.67 and, hence, it does not point to a significant violation of the exclusion restriction.

To show also in a more intuitive way that, indeed, beyond income per capita the effects of oil price shocks on government spending are insignificant, we report in column (2) instrumental variables estimates where we instrument income with the *t*-2 lag and include the oil price shock instrument on the right-hand side of the second-stage equation. The result is that the oil price shock instrument does not exhibit a significant effect on government spending in this equation. Hence, conditional on per capita income oil price shocks do not seem to have a systematic average effect on government spending in our sample.

To complete the picture, column (3) of Table 3 shows that unconditional on income oil price shocks do exhibit a significant effect on government spending. This is in fact the reduced form equation of our instrumental variables regressions. The significant unconditional effect of oil price shocks in this regression resonates with our findings from the two-stage least squares regression that showed a significant positive effect of oil price driven changes in income per capita on government spending.

4.3. Extensions and robustness

The literature on fiscal cyclicality has found ratcheting of government spending – that is, countercyclical spending in recessions and procyclical spending in booms (Hercowitz and Strawczynski, 2004) – which is consistent with suggestions from some political economy models, see Buchanan and Wagner (1978). To address this issue in our context, we now distinguish between positive and negative oil price shocks, see Table 4. We detect positive

coefficients for both types of shocks. Further, column (1) of Table (4) shows that we cannot reject the hypothesis that the coefficient on income, that is driven exclusively by the positive within-country variations in income, is significantly different from the coefficient that is driven exclusively by the negative within-country variations in income. Although the coefficient on negative variations in income is quantitatively larger than the coefficient on positive income variations a formal test does not indicate that the difference is significant. Importantly, a test of whether jointly these two coefficients are equal to zero rejects this hypothesis at the 1 percent significance level. Hence, we do not find evidence that there is a significant asymmetry in the response of government spending to positive and negative oil price driven changes in income per capita.

Our instrumental variables estimates also do not indicate a significant difference in the marginal effect that oil price driven changes in income per capita have on government spending in the high income versus middle and low income group of countries. For both groups the two-stage least squares coefficient on income per capita is positive. While jointly these coefficients are significant at the 5 percent level, their difference is not significant at any conventional confidence level. We note that, while quantitatively the size of the coefficient in the low and middle income group is larger than the coefficient in the high income group, it is also less precisely estimated.

In column (1) of Table 5 we show that our instrumental variables estimates are robust to excluding from the sample countries where oil companies are nationalized. This is an

important robustness check because it allows us to examine whether the positive response of government spending to oil price driven income changes, that we documented in the previous tables, is exclusively a consequence of oil revenues accruing directly to the government sector or whether the positive response reflects more generally the average marginal effect that income changes have on government spending. Column (1) of Table 5 shows that even if we exclude those countries where oil companies are in the hands of the government, our main finding survives: there is a positive and significant lagged effect of within-country changes in income per capita on government spending; quantitatively the elasticity effect is about one-third.

Our identifying assumption for the two-stage least squares estimation is that, because the majority of the countries in our sample are price takers on the international oil market variations in the international oil price are a plausibly exogenous source of variation in income per capita. To demonstrate that our results are robust to excluding those countries from our sample where changes in politico-economic conditions might have an effect on year-to-year variation in the international oil price, we report in column (2) of Table 5 instrumental variables estimates that exclude potentially large oil importing and oil exporting countries. We find that in this case the two-stage least squares coefficient is positive and quantitatively larger than in the benchmark case where large oil exporting and oil importing countries are included. However, we also note that the two-stage least squares coefficient in column (2) is less precisely estimated, and that statistically it is not significantly different

from the benchmark coefficient that is based on including potentially large oil importing and oil exporting countries.

Beyond correcting for the reverse causality bias, another advantage of our two-stage least squares estimation framework that exploits the persistent nature of year-to-year variations in the international oil price is that the two-stage least squares estimation identifies the effect that permanent within-country changes in income per capita have on government spending. To strengthen this point further, we report in column (3) of Table 5 two-stage least squares estimates for the sample-period that excludes the years prior to 1973. Evidence on variations in the international oil prices indicates that in particular for the post-1973 period variations in international oil prices were highly persistent (see Kilian, 2009, and Dvir and Rogoff, 2010). Hence, when we exclude the pre-1973 period, we should find that the two-stage least squares regression produces estimates that are at least as strong as when we use the entire 1960-2007 span. Column (3) shows that indeed this is the case. The two-stage least squares coefficient is significant at the 2 percent significance level, and indicates that quantitatively a one percent increase in income per capita was associated on average with an increase in government spending by nearly 0.3 percent.

4.4. Elasticity correlates

We now explore some of the factors that may potentially be correlated with the country-specific income elasticity of government spending. That is, in Table 6 we consider how

cross-country differences in average income per capita, democracy, trade openness, and ethnic polarization affect the size of the elasticity response of government spending to income across countries. We do this by estimating country by country the elasticity response of government spending to income using the oil price shock variable as an instrument. The mean of these coefficients is 0.85, and the median is 0.77.[4] We then use these country-specific slope estimates as the left-hand-side variable to explore how cross-country differences in country characteristics are correlated with the size of the elasticity response of government spending to income.

Our first main finding is that the elasticity response of government spending to income is not significantly larger in poorer countries. While column (1) shows a negative coefficient on average GDP per capita, this coefficient is insignificant at the conventional confidence levels. Hence, column (1) resonates with our finding in Table 4 that the elasticity response of government spending to oil price driven income changes is not significantly different between rich and poor countries.[5] Column (2) shows that the elasticity response of government spending to income is not significantly larger in more democratic countries; and column (3) shows that there is also no significant difference in the elasticity effect of income on government spending in more open economies.[6] In addition, our estimates in column (4)

4 The estimation results for the complete list of countries are available upon request.

5 We have also experimented with a quadratic term for national income, in which case the estimates are jointly insignificant; these results are available upon request.

6 We have also done these regressions for initial (1960) income per capita, polity2, and trade openness and in that case we continue to find an insignificant effect; results are available upon request.

do not indicate a significantly larger elasticity response of government spending to income in countries with more income inequality as measured by the GINI coefficient.

The only indication for a systematic difference in the elasticity response of government spending to income that we find is for cross-country differences in ethnic polarization.[7] The significant positive coefficient in column (5) suggests that the government elasticity response to changes in income is particularly large in countries that are highly ethnically polarized. The coefficient implies that a one standard deviation increase in ethnic polarization is associated on average with a higher elasticity of government spending to income of about 0.4 standard deviations. This result is consistent with the voracity model of Lane and Tornell (1998, 1999) that predicts a particularly large increase in government transfers to positive income shocks in countries where highly polarized population groups seek to appropriate revenues from the government budget. We show in column (6) that similar results are obtained once we include average income per capita, the Polity2 score, trade openness, income inequality and the ethnic polarization measure jointly on the right-hand side of the estimating equation.

4.5. Government spending composition

We now take a look at the composition of government spending, distinguishing between public investment and government consumption; and between several major disaggregated

7 The data on ethnic polarization are from Montalvo and Reynal-Querol (2005).

expenditure items, such as on social protection, education, and health. The data on government consumption are from from the World Development Indicators (2010); and the other series are from the Global Financial Statistics (2010).

The methodology is similar to the one used in the main analysis, i.e., the first stage is generated using our instrumental variable; and Table 7 presents the second stage estimation results. The elasticity coefficient of government consumption (0.57) is significantly smaller than one, consistent with the results above. The elasticity on public investment, however, while significantly positive at the 5 percent level, marginally exceeds unity.[8] This difference in results between government consumption and public investment echoes results in the cyclicality literature, which find the latter to be much more procyclical than the former (e.g., Lane, 2003). In regard to expenditure items, we find that social protection is the most elastic, with an elasticity coefficient of almost 2, whereas the elasticity of education and health is not significantly different from zero.

4.6. Short run versus long run

The above analysis has considered, in line with the existing literature, annual changes in both national income and government consumption spending. These results could be interpreted as short run elasticities. We now extend this analysis by exploring longer periods – which leads to considering long run elasticities. In other words, now the (log) changes in the

8 Both elasticities are well consistent with the literature.

baseline equation (2) will refer, from t to t+Δt, where t is the year and Δt is the time period within which the change will be recorded; in the baseline short run analysis, Δt =1.

Table 8 presents the results of a two stage estimation, with Δt = 5; 10; 15. We first note that, as Δt increases, the significance of the first estimation stage decreases somewhat which possibly reflects the decrease in the number of observations. When Δt = 15, the first stage F-statistic barely passes the Staiger and Stock's (1997) rule of declaring the instrument weak; however, when Δt = 5 or 10, the instrument appears strong. Also noteworthy is that as Δt increases the elasticity increases as well. While still less than unity, these estimates exceed the short run elasticities; for example, for Δt = 10, the elasticity is 0.67.

5. Conclusions

Existing literature on income elasticity of government expenditures has generated varied estimates, some larger than unity – thus, consistent with Wagner's law – some less than that.[9] One difficulty in providing accurate estimates has to do with the endogeneity of national income, potentially arising both because of reverse causality as well as because of omitted unobservable variables. Another difficulty is that when using time-series data on income and government spending, the obtained least squares estimate of the elasticity response may reflect a combination of transitory and permanent changes in income.

9 A very broad summary of the literature suggests that most elasticities fall in the 0.50-1.10 range.

To deal with these issues, in this paper we introduce two novelties in our analysis of panel data of over 180 countries during the period 1960-2007. One is country and time fixed effects. The other, which we believe is more important and novel, is a plausible instrument for permanent within-country changes in national income – oil price shocks. The obtained elasticity estimates when employing these features are somewhat smaller than existing ones, in the range of 0.25-0.50, somewhat larger for low income countries; larger for the investment rather than consumption component; and larger, but still less than one for long run versus short run elasticities.

These results broadly hold when large oil producers/consumers are excluded from the analysis; when countries with nationalized oil production are excluded; and when the analysis is restricted to the post-1973 period – which is particularly highly persistent in terms of oil price shocks. Interpreting the results in light of the literature on fiscal cyclicality, we also look at the elasticity of government spending generated via positive versus negative shocks. The main finding here is that government spending appears to be procyclical in both types of cases, which is only partly consistent with the previous results for OECD countries – see Hercowitz and Strawczynski, 2004; while our findings share with Hercowitz and Strawczynski, 2004, the feature that government spending grows in good times, unlike that paper we find that it decreases in bad times. We also undertake analysis of the correlates of obtained elasticities, and find that these elasticites are not significantly correlated with neither democracy nor openness. Ethnic polarization is the only moderately significant correlate that

is associated with a larger income elasticity of government spending – consistent with the existing theory of the voracity effect.

References

Akitoby, Bernardin, Benedict Clements, Sanjeev Gupta and Gabriela Inchauste, 2006, "Public Spending, Voracity, and Wagner's Law in Developing Countries," *European Journal of Political Economy* 22(4), 908–924.

Atkinson, Anthony B. and Joseph E. Stiglitz (1980), *Lectures on Public Economics.* New York: McGraw-Hill.

Baumol, William J., 1967, "Macroeconomics of unbalanced growth: The anatomy of an urban crisis," *American Economic Review* 57, 415-426.

Blanchard, Olivier and R. Perotti, 2002, "An Empirical Characterization of the Dynamic Effects of Changes in Government Spending and Taxes on Output," *Quarterly Journal of Economics*, 1329-1368.

Brückner, M., Antonio Ciccone, and Andrea Tesei, 2011, "Oil Price Shocks, Income, and Democracy," *Review of Economics and Statistics*, forthcoming.

Buchanan J. and R. Wagner (1978), "Fiscal Responsibility in Constitutional Democracy," *Studies in Public Choice*, 1, 79-97.

Durevall, D. and M. Henrekson, 2011, "The Futile Quest for a Grand Explanation of Long-Run Government Expenditure," *Journal of Public Economics*, 95, No. 7–8, 2011.

Dvir, E. and K.S. Rogoff, 2010, "The three epochs of oil," mimeo.

Easterly, W. and S. Rebelo, 1993, "Fiscal Policy and Economic Growth: An Empirical Investigation," *Journal of Monetary Economics*, 32, 1993, 417 – 458.

Hercowitz, Zvi, and Michel Strawczynski, 2004, "Cyclical Ratcheting in Government Spending: Evidence from the OECD," *Review of Economics and Statistics* **86**(1), 353–361.

Holsey, Cheryl M. and Thomas E. Borcherding, 1997, "Why does government's share grow? An assessment of the recent literature on the US," in *Perspectives on Public Choice: A Handbook*, ed. Dennis C. Mueller. Cambridge University Press, New York, 562-591.

Jaimovich, D. and U. Panizza, 2007, "Procyclicality or reverse causality?" IADB WP.

Ilzetzki, E. and Carlos A. Vegh, 2008, "Procyclical fiscal policy in developing countries: Truth or fiction?" mimeo.

Kilian, Lutz, 2009, "Not All Oil Price Shocks Are Alike: Disentangling demand and Supply Shocks in the Crude Oil Market," *American Economic Review* 99(3): 1053-69.

Lane, Philip R. (2003), "The Cyclical Behavior of Fiscal Policy: Evidence from the OECD," *Journal of Public Economics* 87(12), 2661–2675.

Lane, P.R. and A. Tornell, 1998, "Are Windfalls a Curse? A Non-Representative Agent Model of the Current Account", *Journal of International Economics*, 83-112.

Lane, P.R. and A. Tornell, 1999, "The Voracity Effect" *American Economic Review*, 89, 22-46.

Lindert, Peter H. (1994), "The Rise of Social Spending, 1880–1930?" *Explorations in Economic History* 31(1), 1–34.

Lindert, Peter H. (1996), "What Limits Social Spending?" Explorations in Economic *History* 33(1), 1–34.

Lindert, Peter H. (2004a), *Growing Public: Volume 1, The Story: Social Spending and Economic Growth since the Eighteenth Century*. Cambridge and New York: Cambridge University Press.

Lindert, Peter H. (2004b), *Growing Public: Volume 2, Further Evidence: Social Spending and Economic since the Eighteenth Century*. Cambridge and New York: Cambridge University Press.

Meltzer, Allan and Scott Richard (1981) "A Rational Theory of the Size of Government" *Journal of Political Economy*, 89, 914-927.

Montalvo, J.G. and M. Reynal-Querol, 2005, "Ethnic polarization, potential conflict and civil war," *American Economic Review,* 95*,* 796-816.

Ram, Rati, 1987, "Wagner's Hypothesis in Time-Series and Cross-Section Perspectives: Evidence from Real Data for 115 Countries," *Review of Economics and Statistics* **62**(2), 194–204.

Rodrik, D., 1998, "Why do more open economies have bigger governments?" *Journal of Political Economy*, 106, 997-1032.

Shelton, C.A., 2007, "The Size and Composition of Government Expenditure," *Journal of Public Economics* **91**, 2230–2260.

Staiger, D. and J. Stock, 1997, "Instrumental Variables Regression with Weak Instruments," *Econometrica* 65: 557-586.

Talvi, Ernesto, and Carlos Végh (2005), "Tax Base Variability and Procyclical Fiscal Policy in Developing Countries," *Journal of Development Economics* 78, 156–190.

Tanzi, V. and Schuknecht, L., 2000, *Public spending in the 20th century*. Cambridge: Cambridge University Press.

Table 1. The Government Spending Elasticity Response

	Δln(Gov)			Δln(Gov/GDP)		
	Panel A: Instrumental Variables Estimation					
	(1)	(2)	(3)	(4)	(5)	(6)
	IV	IV	IV	IV	IV	IV
Δln(GDP), t	0.18 (1.44)	0.17 (1.32)		-0.82*** (-6.33)	-0.83*** (-6.40)	
Δln(GDP), t-1	0.25** (1.99)	0.26** (2.05)	0.29** (2.46)	0.25** (1.99)	0.26** (2.05)	0.12 (0.62)
Δln(GDP), t-2	0.12 (1.49)			0.12 (1.50)		
Country Fe	Yes	Yes	Yes	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes	Yes	Yes	Yes
Endogenous Regressors	Δln(GDP), t, t-1, t-2	Δln(GDP), t, t-1	Δln(GDP), t -1	Δln(GDP), t, t-1, t-2	Δln(GDP), t, t-1	Δln(GDP), t-1
Instrumental Variables	Oil Shock, t, t-1, t-2	Oil Shock, t, t-1	Oil Shock, t-1	Oil Shock, t, t-1, t-2	Oil Shock, t, t-1	Oil Shock, t-1
First-Stage, F-Statistic	4.37	8.72	30.09	4.37	8.72	30.09
Observations	5567	5709	5709	5567	5709	5709
	Panel B: Least-Squares Estimation					
	(1)	(2)	(3)	(4)	(5)	(6)
	LS	LS	LS	LS	LS	LS
Δln(GDP), t	0.51*** (8.66)	0.53*** (9.12)		-0.49*** (-8.21)	-0.47*** (-8.09)	
Δln(GDP), t-1	0.04 (0.79)	0.04 (0.83)	0.07 (1.36)	0.04 (0.79)	0.04 (0.83)	0.01 (0.20)
Δln(GDP), t-2	0.02 (0.80)			0.02 (0.80)		
Country Fe	Yes	Yes	Yes	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5567	5709	5709	5567	5709	5709

Note: The dependent variable in columns (1)-(3) is the log-change in real government expenditures per capita; columns (4)-(6) the log-change in the ratio of government expenditures over GDP. The explanatory variable is the log-change in real GDP per capita. The method of estimation in Panel A is two-stage least squares; Panel B least squares. T-values in parentheses are based on Huber robust standard errors that are clustered at the country level. *Significantly different from zero at the 10 percent significance level, ** 5 percent significance level, *** 1 percent significance level.

Table 2. The Government Spending Elasticity Response
(Robustness to Convergence Dynamics)

	Δln(Gov)			
	(1)	(2)	(3)	(4)
	IV	IV	IV	LS
Δln(GDP), t-1	0.29** (2.39)	0.08 (1.60)	0.29** (2.37)	0.08* (1.65)
ln(Gov), t-1	-0.07*** (-6.85)	-0.07*** (-7.44)	-0.09*** (-7.46)	-0.08*** (-7.53)
Country Fe	Yes	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes	Yes
Endogenous Regressors	Δln(GDP), t-1 ln(Gov), t-1	ln(Gov), t-1	Δln(GDP), t-1	.
Instrumental Variables	Oil Shock, t-1 ln(Gov), t-2	ln(Gov), t-2	Oil Shock, t-1	.
First-Stage, F-Statistic	15.03	8460.19	30.07	.
Observations	5709	5709	5709	5709

Note: The dependent variable is the log-change in real government expenditures per capita. The method of estimation in columns (1)-(3) is GMM; column (4) least squares. T-values in parentheses are based on Huber robust standard errors that are clustered at the country level. *Significantly different from zero at the 10 percent significance level, ** 5 percent significance level, *** 1 percent significance level.

Table 3. The Government Spending Elasticity Response
(Test of Exclusion Restriction)

	Δln(Gov)		
	(1)	(2)	(3)
	IV	IV	LS
Δln(GDP), t-1	0.30*** (2.69)	0.46 (1.06)	
ln(Gov), t-1	-0.09*** (-7.37)	-0.09*** (-6.99)	-0.08*** (-7.40)
Oil Price Shock, t-1		-0.19 (-0.41)	0.31** (2.52)
Country Fe	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes
Endogenous Regressors	Δln(GDP), t-1	Δln(GDP), t-1	.
Instrumental Variables	Δln(GDP), t-2 Oil Shock, t-1	Δln(GDP), t-2	.
First-Stage, F-Statistic	16.87	4.55	.
Hansen J Test, p-value	0.67	.	.
Observations	5567	5567	5709

Note: The method of estimation in columns (1) and (2) is GMM, column (3) least squares. The dependent variable is the log-change in real government expenditures per capita. T-values in parentheses are based on Huber robust standard errors that are clustered at the country level. *Significantly different from zero at the 10 percent significance level, ** 5 percent significance level, *** 1 percent significance level.

Table 4. The Government Spending Elasticity Response
(Test of Difference Between Positive and Negative Growth Shocks; Test of Difference Between High Income and Middle&Low Income Countries)

	Δln(Gov)	
	(1)	(2)
	IV	IV
$\Delta\ln(GDP)^{+}$, t-1	0.16 (0.93)	
$\Delta\ln(GDP)^{-}$, t-1	0.70** (2.24)	
$\Delta\ln(GDP)^{HI}$, t-1		0.26** (2.61)
$\Delta\ln(GDP)^{MI\&LI}$, t-1		0.41 (1.58)
Test, p-value $\Delta\ln(GDP)^{+}$, t-1 = $\Delta\ln(GDP)^{-}$, t-1	0.22	
Test, p-value $\Delta\ln(GDP)^{+}$, t-1 = $\Delta\ln(GDP)^{-}$, t-1 = 0	0.003	
Test, p-value $\Delta\ln(GDP)^{HI}$, t-1 = $\Delta\ln(GDP)^{MI\&LI}$, t-1		0.53
Test, p-value $\Delta\ln(GDP)^{HI}$, t-1 = $\Delta\ln(GDP)^{MI\&LI}$, t-1 = 0		0.03
Country Fe	Yes	Yes
Year Fe	Yes	Yes
Endogenous Regressors	$\Delta\ln(GDP)^{+}$, t-1 $\Delta\ln(GDP)^{-}$, t-1	$\Delta\ln(GDP)^{HI}$, t-1 $\Delta\ln(GDP)^{MI\&LI}$, t-1
Instrumental Variables	Oil Shock^{+}, t-1 Oil Shock^{-}, t-1	Oil Shock^{HI}, t-1 Oil Shock$^{MI\&LI}$, t-1
First-Stage, F-Statistic	9.71	21.16
Observations	5709	5709

Note: The method of estimation is two-stage least squares. The dependent variable is the log-change in real government expenditures per capita. $\Delta\ln(GDP)^{+}$ is defined as the log-change in real GDP per capita over its positive range (the variable takes on the value of zero over its negative range); $\Delta\ln(GDP)^{-}$ is defined as the log-change in real GDP per capita over its negative range (the variable takes on the value of zero over its positive range). $\Delta\ln(GDP)^{HI}$ is the log-change in real GDP per capita in High Income Countries (the variable takes on the value of zero in all other countries); $\Delta\ln(GDP)^{MI\&LI}$ is the log-change in real GDP per capita in Middle and Low Income Countries (the variable takes on the value of zero in all other countries). The income group categories follow WDI (2010). T-values in parentheses are based on Huber robust standard errors that are clustered at the country level. *Significantly different from zero at the 10 percent significance level, ** 5 percent significance level, *** 1 percent significance level.

Table 5. The Government Spending Elasticity Response
(Robustness to Excluding Countries Where Oil Companies are Nationalized; Excluding the Pre-1973 Period; Excluding Large Oil Exporters and Importers)

	Δln(Gov)		
	(1)	(2)	(3)
	IV	IV	IV
Excluding:	Countries Where Oil Companies are Nationalized	Countries that are Large Oil Exporters and Importers	Pre-1973 Period
Δln(GDP), t-1	0.36*	0.63**	0.29**
	(1.79)	(1.66)	(2.42)
Country Fe	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes
Endogenous Regressors	Δln(GDP), t-1	Δln(GDP), t-1	Δln(GDP), t-1
Instrumental Variables	Oil Shock, t-1	Oil Shock, t-1	Oil Shock, t-1
First-Stage, F-Statistic	10.06	21.22	29.83
Observations	4209	4797	4473

Note: The method of estimation is two-stage least squares. The dependent variable is the log-change in real government expenditures per capita. Excluded in column (1) are countries where oil companies are nationlized; column (2) excludes countries that produce or consume more than 1 percent of world oil production. Column (3) excludes country-years that fall into the pre-1973 period. The excluded countries in column (1) are Algeria, Angola, Argentina, Bahrain, Bangladesh, Bolivia, Burma, Cambodia, Chad, Colombia, Republic of Congo, Ecuador, Egypt, Ethiopia, Gabon, Ghana, Guyana, India, Indonesia, Iran, Iraq, Kuwait, Libya, Malaysia, Morocco, Mozambique, Nepal, Nigeria, Oman, Pakistan, Peru, Philippines, Qatar, Russian Federation, Saudi Arabia, Sudan, Trinidad and Tobago, Uganda, United Arab Emirates, Venezuela, Yemen, Zambia. The excluded countries in column (2) are Algeria, Canada, China, Germany, Indonesia, Iran, Iraq, Italy, Kuwait, Libya, Mexico, Netherlands, Nigeria, Norway, Oman, Qatar, Russia, Saudi Arabia, United Arab Emirates, United Kingdom, United States, and Venezuela. T-values in parentheses are based on Huber robust standard errors that are clustered at the country level. *Significantly different from zero at the 10 percent significance level, ** 5 percent significance level, *** 1 percent significance level.

Table 6. The Country Specific Elasticity Response
(The Role of Country Characteristics)

	Country Specific Elasticities					
	(1)	(2)	(3)	(4)	(5)	(6)
	LS	LS	LS	LS	LS	LS
Average Log GDP Per Capita	-1.26 (-0.95)					0.09 (0.09)
Average Polity		0.14 (0.44)				-0.08 (-0.48)
Average Openess			-0.02 (-0.60)			-0.04 (-0.67)
Average Gini				0.09 (0.93)		0.03 (0.21)
Ethnic Polarization					18.23** (2.20)	23.15** (2.16)
Regional Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	186	154	186	147	128	99

Note: The method of estimation is least squares. The dependent variable are the coefficients of an instrumental variables regression that estimates for each country the elasticity response of government spending to income using the change of the log of the international oil price as an instrumental variable. *Significantly different from zero at the 10 percent significance level, ** 5 percent significance level, *** 1 percent significance level.

Table 7. Results Using Different Government Spending Data

	Government Consumption	Public Investment	Social Protection	Health	Education
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
Δln(GDP)	0.57**	1.05**	1.98**	0.34	0.32
	(2.26)	(2.41)	(2.50)	(1.21)	(0.99)
First-Stage, F-Statistic	21.47	16.68	34.10	33.08	35.68
Country Fe	Yes	Yes	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes	Yes	Yes
Observations	3754	3087	1532	1590	1597

Note: The method of estimation is two-stage least squares. The instrumental variable is the oil price shock variable. The dependent variable in column (1) is the change in the log of government consumption expenditures. In column (2) the dependent variable is the change in the log of public investment expenditures. In column (3) the dependent variable is the change in the log of government expenditures on social protection. In column (4) the dependent variable is the change in the log of government health expenditures. In column (5) the dependent variable is the change in the log of government education expenditures. These alternative data on government spending are from the WDI and WEO. *Significantly different from zero at the 10 percent significance level, ** 5 percent significance level, *** 1 percent significance level.

Table 8. Results Using 5-Year, 10-Year, and 15-Year Averages

	5-Year Average	10-Year Average	15-Year Average
	(1)	(2)	(3)
	IV	IV	IV
Δln(GDP)	0.45***	0.67***	0.91***
	(3.68)	(3.98)	(4.01)
First-Stage, F-Statistic	34.32	24.12	10.33
Country Fe	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes
Observations	5141	4431	3732

Note: The method of estimation is two-stage least squares. The instrumental variable is the oil price shock variable. The dependent variable is the change in the log of government expenditures. *Significantly different from zero at the 10 percent significance level, ** 5 percent significance level, *** 1 percent significance level.

Data Appendix Table

	Mean	Stdv.	Obs.	Source
Δln(GDP)	0.056	0.086	5709	Penn World Table 6.3
Δln(Gov)	0.060	0.133	5709	Penn World Table 6.3
Δln(Gov/GDP)	0.004	0.128	5709	Penn World Table 6.3
Gini	38.14	11.20	1249	UN-WIDER
Ethnic Polarization	0.506	0.246	3907	Montalvo and Reynal-Querol (2005)
Polity2	0.393	7.450	5102	Polity IV
Openness	69.78	49.94	5701	Penn World Table 6.3
Oil Price Instrument	0.001	0.018	5709	UNCTAD Commodity Price Statistics, NBER-UN Trade Database