

Fiscal Effects of Constitutions: Evidence from a ‘Natural Resource Experiment’*

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Abstract

Several recent studies document robust correlations between constitutional rules and fiscal policies. It is however not clear whether the correlations are causal, and the main concerns are omitted variables and reverse causation (Acemoglu 2005). This paper uses fluctuations in oil prices to investigate whether exogenous shocks to the tax bases of oil producing countries have different effects on fiscal policies, dependent on the constitutional arrangement. The empirical strategy allows for country- and year fixed effects, which reduces the probability of estimation biases due to omitted variables. Moreover, because the study focuses on short-term effects, the estimates are not likely to reflect reverse causation from fiscal policy outcomes to constitution selection. The evidence suggests strong and significant effects of constitutional features. Among the oil producing countries with a parliamentary form of government, oil price induced revenue shocks have no short-term effect on government expenditures, and hence positive short-term effects for government saving. The presidential oil producers, on the other hand, are inclined to spend their oil price windfalls. This finding contrasts existing evidence, which suggests that shocks have a stronger fiscal impact among parliamentary systems (Persson and Tabellini 2003). In addition, there is evidence of significant inertia in the policy adjustment in countries with proportional electoral rules, compared with countries with majoritarian electoral rules. The latter effect is consistent with the veto players theory.

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1 Introduction

A growing body of literature documents strong and robust correlations between constitutional features—such as the rules for election and the rules for legislative bargaining associated with the form of government—and the level and composition of fiscal policy.¹ It is however not clear that the cross-country correlations reflect causal relationships (Acemoglu 2005).² Arguably, constitutions and fiscal policies are simultaneously determined, and reverse causation from fiscal policy outcomes to the constitutional rules must be taken into account. Moreover, the cross-country correlations between constitutions and fiscal policies may reflect omitted variables affecting both constitutional selection and fiscal policies.³ This paper proposes a novel empirical strategy for the identification of constitutional effects, addressing both the concern of reverse causation and the concern of omitted variables. Instead of relying on cross-sectional evidence of correlations between constitutions and levels of fiscal policies, I investigate whether the short-term responses in fiscal policies to exogenous economic shocks depend on constitutional features.

From the early- to mid 1970s to the mid 1980s, the government expenditures in the U.S. and in Canada grew at very different rates. Consistent with the constitutions literature (Persson and Tabellini 2003, 2004), the expenditures of the parliamentary Canadian government grew considerably faster—more than twice as fast from 1970 to 1985—than the presidential U.S. government’s expenditures. At the same time, the world experienced two major oil price shocks, one in 1973-74, and a second one in 1978-79. The short-term fiscal responses to the shocks were also very different in the U.S. and Canada, only this time it was the other way around; the shocks were followed by relatively strong short-term fiscal expansions in the U.S., but not in Canada. Consequently, government expenditures were during this period much more volatile in the U.S. than in Canada. Although there may be many explanations for these particular fiscal patterns, the diverging fiscal responses in the U.S. and in Canada is highly consistent with the evidence presented in the present paper, suggesting that constitutional features may play an important role in determining the fiscal responses to economic shocks.

Oil price shocks constitute a particularly interesting quasi-natural experiment on the comparative effects of formal political institutions. In most political economic models, an important variable is the size of the tax base, or the level of the tax revenues, which indirectly affects the

¹Persson and Tabellini (2000, 2003, 2004) provides extensive overviews of the theoretical and empirical advances in this literature.

²Causal effects of constitutions have proven difficult to estimate for two main reasons, which also explains the widespread reliance on cross-sectional evidence. First, reforms in the electoral rules and the form of government are very rare. Within the present panel of up to 64 countries and an average of almost 20 years, only one country (Cyprus) have experienced a lasting reform in the electoral rules or in the form of government. This explains why empirical analyses mainly have been cross-sectional. Second, when reforms once in a while take place, they often come as a result of endogenous political processes within the country, and are very likely to coincide with other reforms taking place at the same time. It is hence difficult to separate the effect of the constitutional reform from other coinciding effects.

³By applying a variety of state-of-the-art techniques, such as matching estimators, instrumental variables methods and parametric selection corrections, Persson and Tabellini (2003) indeed go a long way in their efforts to solve the problems of endogenous institutions selection and omitted variables biases. However, Acemoglu (2005) shows that several problems remain, and we can hence not be confident that the effects are causal and unbiased.

level of public goods provision, redistributive policies and the value for politicians of being in office. The political institutions—or the rules of the game—determine *how* the tax base is being transformed into economic- or fiscal policies, or into rents for the politicians. Naturally, the tax base is again a function of state capacity, which in turn may rely on the political institutions (Besley and Persson 2007). Hence, effects of shifts in the tax base are, as many other dimensions of the politico-economic environment, generally difficult to study empirically. Oil price shocks, however, constitute a quasi-natural experiment which enables us to study the effects of tax base shocks on fiscal policies. Among oil producers, oil price shocks positively affect the size of the tax base and the government budgets via its effect on the profitability in the oil industry; governments of oil producing countries derive considerable revenues from the oil production sector, either through taxation, royalties or direct state ownership. Moreover, shifts in the governments' budgets due to shifts in the world market price of oil are exogenous to the political environment within each country, at least for small countries and countries that are not members of the OPEC cartel. The present study investigates whether oil price shocks, via their effects on the governments' tax bases and revenues, have different effects for contemporaneous and lagged government expenditures and budget balances, dependent on the constitutional arrangements in the countries. Taking advantage of an unbalanced panel of up to 64 democratic countries—of which more than two-thirds are oil producers—over an average of 15-20 years, makes it possible to isolate the effects of the oil price shocks on fiscal policies from the effects of the shocks working through other channels in the economy. Because I estimate short-term responses, reverse causality from the fiscal effects of the shocks to the design of the political institutions is not likely to constitute a problem. Moreover, the panel structure makes it possible to control for unobserved and otherwise omitted country- and time specific effects.

Investigating and comparing the effects among democratic countries with a parliamentary versus a presidential form of government, and with majoritarian versus proportional electoral rules, I find evidence suggesting significant constitutional effects. The strongest effects are related to the form of government. While oil price induced tax base shocks have positive, and economically and statistically significant, short-term lagged effects for government expenditures among the presidential democracies, there are no such effects among the parliamentary democracies. As a result, the cumulative effect of the oil price shocks for the net government savings (debt) is highly positive (negative) among the oil producing parliamentary countries, while it is in fact neutral among the oil producing presidential countries. The comparative effects for the budget balances of the electoral rules are less pronounced. However, there is robust evidence that electoral systems, in particular in combination with a presidential form of government, determine the speed of fiscal adjustment: while positive shocks to the government budget have immediate positive effects for government expenditures among the presidential regimes with majoritarian electoral rules, the effects enter with lags of up to two years among the presidential countries with proportional electoral systems.

The findings in the present paper relate to the existing literature in several interesting ways.

In particular, Persson and Tabellini (2003) find that the fiscal effects of unobserved common events (i.e., the time dummies) depend on the form of government; events that have expansionary effects on government expenditures have significantly stronger effects under a parliamentary form of government. Unfortunately, however, Persson and Tabellini (2003) do not distinguish between different types of events, which makes it difficult to learn about the underlying mechanisms. In the present study, on the other hand, the causes of the fiscal effects are well defined and empirically identified. Second, and related to the first problem, comes the fact that Persson and Tabellini's (2003) empirical design by construction precludes time fixed effects. In the present study, time fixed effects are always employed to correct for both alternative fiscal effects of the oil price shocks, and for other common events that might correlate with fiscal policies. Interestingly, the findings in the present study are quite different from Persson and Tabellini's (2003) findings: while Persson and Tabellini find that shocks have a stronger fiscal impact among parliamentary than among presidential systems, I find the opposite.⁴ The reason for the diverging results may be that the two studies investigate different types of shocks. However, since Persson and Tabellini (2003) do not allow for country specific fiscal policy trends it may be that their results reflect more the fact that the trends in fiscal policies are steeper among parliamentary countries than among presidential countries, than that economic shocks per se have a stronger impact in parliamentary regime types.

Theoretically, there are few models that have been designed to study the comparative short-term effects of shocks within different constitutional regimes. There is however one strand of literature that have several implicit predictions for how the constitutional rules should be determinants for the short-term effects of shocks. Tsebelis (1995, 1999, 2002) study how the number of veto players in the political system determines the ability *in the short run* to handle outside shocks. In addition, several studies point out that different political institutions are associated with different numbers, and constellations, of veto players (Roubini and Sachs 1989, Alesina and Drazen 1991). The bulk of the veto player literature predicts a higher degree of legislative inertia in countries with a proportional electoral system, and in countries with coalition governments. Both of these predictions are highly consistent with our findings.

The estimates of the effects of the oil shocks on fiscal policies are remarkably stable, and survives a number of robustness checks. First, the dynamic panel data model is very stable, in the sense that the number of included lags of both the endogenous fiscal policy variables and the oil shock variable has negligible impact on the point estimate of each single lag. Second, the effects are robust to employing stricter democracy thresholds. In the baseline sample, the threshold for an observation to be part of the sample is that the country's democracy score in the corresponding year is higher than 6 on the Polity Index.⁵ Increasing the threshold to 9 on the Polity Index does not change any of the main results, even though the sample size is reduced by about 25 percent. Third, I show that the results are robust to employing different types of

⁴See, e.g., Figure 8.1 in Persson and Tabellini (2003).

⁵This threshold corresponds to approximately 2 on the Gastil Index. See the Appendix for more details about these indecies.

weights in the construction of the country specific oil shock proxy. Fourth, the results are robust to employing measures of central, instead of total, government fiscal policies. Fifth, the results are robust to the removal of large countries that may affect the world market price of oil, such the two OPEC countries in the sample, Venezuela and Ecuador, and the U.S. Finally I show, by investigating the effect of the shocks on two other fiscal policy measures, that the effects are not spuriously generated by the model.

The paper proceeds as follows. Section 2 provides a theoretical discussion of the optimal policy responses to exogenous revenue shocks, and of how the responses might be expected to depend on constitutional features. Section 3 reviews and characterizes the data. The identification strategy and the empirical model are defined in Section 4. Section 5 displays and discuss the results and the transmission channel of the oil shock pass-through. In Section 6, I discuss how the results relate in interesting ways to two other and distinct literatures—namely the so-called ‘resource curse’ literature and the so-called ‘flypaper effect’ literature. The ‘resource curse’ literature studies the negative empirical correlation between initial resource abundance and growth (Sachs and Warner 1995, 1997, 2001). Lately, the focus in the ‘resource curse’ literature has shifted from pure economic mechanisms, to the performance and the design of institutions. Interestingly, my results on the effects of constitutions harmonize with some recent findings in this literature, and I include a brief discussion. I finally discuss how my results suggest that ‘flypaper effects’ (e.g., Hines and Thaler 1995) may be associated with the implicit incentive structures embedded in countries’ constitutions. Section 7 provides some final remarks and avenues for future research.

2 Theoretical predictions

There exists no explicit political economic theory with concise predictions regarding what kind of policy responses one should expect from the proposed ‘natural resource experiment’. The purpose of this section is therefore merely to sketch out some loose predictions, or expectations, using standard consumption theory as a benchmark, and then to discuss policy responses that are consistent with existing theory and predictions on constitutional effects. Questions I ask, and discuss, in this section are: What does economic theory have to say about the fiscal short-term effects of exogenous variation in government revenues? And should we expect the formal political institutions of constitutions to matter for the fiscal policy responses to shocks?

Let me turn to the former question first. Both Friedman’s (1957) permanent income hypothesis (PIH) and the life-cycle hypothesis (LCH) of Ando and Modigliani (1963) and Modigliani (1966) predicts transitory revenue shocks to have negligible effects on consumption. This is also the essence of Barro’s (1979; 1981) ‘tax smoothing’ hypothesis. Given the insights of Friedman, Ando and Modigliani, and Barro, the public finance problem becomes quite simple and one can make a couple of straight forward predictions about optimal policy responses. Assuming that the revenue shocks are partly transitory, that taxation is costly and that there are costs associated with transferring the government revenue shock to the public, the PIH and the LCH

both imply that the optimal fiscal response would be to partly smooth the revenue shocks.⁶ If the revenue shocks constitute a cheaper source of public funds than taxation, which is quite likely in the current context, it would be optimal to partially substitute general tax revenues with the presumably ‘cheaper’ oil shock revenues. The government should substitute between the two sources of revenues until the marginal benefits from tax reduction, due to the reduced costs of taxation, equals the marginal adjustment costs of implementing and administrating the changes in the tax code. Moreover, if the shocks are partly permanent and the adjustment costs are convex, the government should gradually phase in and convert the increased revenues into increased government expenditures. The scheme of phasing in the revenues, by increasing government expenditures and/or decreasing taxes, would generally depend on the costs of taxation and the curvature of the costs related to changing the tax code. Making the final assumption that there is some inertia associated with policy making and implementation, it is straight forward to verify the following policy predictions. A positive revenue shock to the government budget should be associated with: (i) a positive and large—relative to the size of the revenue shock—shift in the government budget balance; (ii) possibly a small—relative to the size of the revenue shock—,and negative, substitution effect on tax revenues, possibly with time lags; (iii) relatively small—dependent on the mix of transitory and permanent components in the shocks—and positive effects on contemporaneous and lagged government expenditures.⁷

I now turn to the second question, regarding the role of constitutional political institutions. Although there are, to my knowledge, no models in the literature which explicitly analyzes the comparative effects of revenue- or tax base shocks, there is a large body of models that analyzes how different constitutional features produce different equilibrium policy outcomes.⁸ In particular, there are two distinct theories that have relatively straight forward implications for the expected comparative effects of tax base shocks. First, in a couple of influential contributions Persson, Roland and Tabellini (1997, 2000) show that presidential forms of government should be associated lower levels of taxation than parliamentary forms of government. There are three main reasons for this: (1) The strong separation of powers in the presidential regime type provides the agenda setter(s) for public funds—the ‘tax minister’—with strong incentives to limit the redistributive powers of the agenda setter(s) for public expenditure—the ‘expenditure minister’—,by proposing a relatively low level of taxes. The reason is that the constituency

⁶The government could transfer the whole revenue shock to the private sector and let the households themselves smooth consumption. However, assuming convex costs of changing the tax code, a policy of changing the tax code as an immediate and mechanical response to volatile government revenues would clearly be suboptimal.

⁷Bodkin (1959) and Lee (1975) show that it is empirically not clear that the short-term marginal propensity to consume out of a windfall income is very different from the short-term marginal propensity to consume out of a shift in the permanent income. Section 4 provides a brief discussion of the distinction between transitory and permanent shocks in the present context.

⁸Empirically, some work has been done to identify regime specific effects of shocks. Milesi-Feretti *et.al.* (2002) test the fiscal effects of shocks in unemployment and both Milesi-Feretti *et.al.* (2002) and Persson and Tabellini (2003) employ common unobserved shocks (i.e., time-dummies) to identify the regime specific policy responses to shocks. There are however, as discussed in the introduction, several problems with using the time-dummies to identify the effects of shocks in this context. Moreover, regressing fiscal policy responses on unemployment introduces simultaneities in the analysis—both economically, assuming that the two are determined simultaneously, and statistically, assuming that both measures are autocorrelated—which will bias the estimates.

of the ‘tax minister’ is not a residual claimant of public funds, in contrary to the ‘expenditure minister’ who has incentives to redistribute any residual funds to her own constituency. In the parliamentary system, on the other hand, all of the members of the government coalition, and their respective constituencies, are residual claimants of tax revenues, implying strong incentives to implement a high level of taxation. (2) The government coalition in the parliamentary regime type represents a majority of the population, and has thus stronger incentives to internalize the demand for public goods than has the ‘expenditure minister’ in the presidential regime type, who represents only a minority. (3) The threat of a vote of confidence provides the parliamentary coalition government with stronger powers to divert—or, equivalently, waste—resources than the agenda setters in the presidential system have. Effects (1) to (3) all imply a larger size of government and a higher level of government expenditures in the parliamentary system than in the presidential system, for any given tax base. Simple comparative statics on the theory of Persson, Roland and Tabellini (1997, 2000) implies that a positive shift in the tax base should have a stronger effect, relative to GDP, on government expenditures in parliamentary than in presidential countries. Although the theory of Persson, Roland and Tabellini deals with static environments, it is interesting to investigate whether its equilibrium properties are dynamically consistent in the short run—that is, whether short run fluctuations in the tax base generates fiscal policy responses that are consistent with the static equilibrium theory.

Another interesting aspect associated with the political system—in addition to its quantitative effects for fiscal policy—is its ability to respond quickly to economic shocks. Although there is no theory that specifically analyzes the comparative policy responsiveness—or, equivalently, policy inertia—, of different constitutional rules, there is a literature that has been concerned with closely related mechanisms. Arguably, the degree of policy responsiveness to exogenous shocks is negatively related to the number of veto players in the political system, and to the distance in policy preferences between them. These ideas relate to Tsebelis’ (1995, 1999, 2002) studies, where Tsebelis shows that a large number of veto players tends to “lock in” economic policy, implying limited ability *in the short run* to handle outside shocks. Coalition governments have more veto players, and should be subject to more serious status quo bias in their adjustment to shocks (Roubini and Sachs 1989, Alesina and Drazen 1991). Because the incidence of coalition governments are higher among parliamentary forms of governments one should observe a stronger policy inertia among parliamentary than presidential systems. Moreover, because proportional electoral systems are associated with a larger number of political parties, coalition governments are also more frequent under proportional electoral rules. Proportional systems should hence be associated with slower policy adjustments than majoritarian systems.

Based on the discussion above, the following tentative predictions seem to be consistent with the existing theories: (*iv*) short run dynamic consistency of the models of Persson, Roland and Tabellini (1997, 2000) implies that a tax base shock should have a stronger expansionary effect on government expenditures in parliamentary systems than in presidential systems; (*v*) the role of veto players in delaying legislation implies that one should observe stronger policy inertia

among parliamentary and proportional systems.

The analyses that follows find support for all of the theoretical predictions, with the exception of prediction (*iv*).

3 Data

3.1 Variables and sources

The empirical study is based on four groups of data. First, I draw on fiscal policy data collected from three different sources: Persson and Tabellini (2003); Brender and Drazen (2005); and the World Bank’s World Development Indicators (WDI) database. The four policy outcome measures from Persson and Tabellini (2003) are central government expenditure (*CGEXP*), central government revenues (*CGREV*), social security and welfare spending by central government (*CGSSW*) and central government budget surplus (*CGBAL*). From Brender and Drazen (2005), I use the three outcome measures total government expenditure (*TGEXP*), total government revenue and grants (*TGREV*) and total government budget balance (*TGBAL*). All of the policy measures from Persson and Tabellini (2003) and Brender and Drazen (2005) are originally collected from the IMF’s International Financial Statistics (IFS) database. Finally, I use the item “General government final consumption expenditure” (*TGCONS*) from the WDI.

Second, I use data on the value of oil depletion and oil prices that are collected from the WDI and the World Bank’s Adjusted Net Savings dataset, also called the genuine savings dataset. The variable, *oil_gdp*, measures the net value of countries’ oil extraction—or, equivalently, production—measured as unit rents times quantity, and divided by GDP in current prices. Unit rent is computed as the world price of crude oil, subtracted lifting costs.

Third, countries are assigned indicator variables dependent on the specific constitutional features they are recognized by. More precisely, years (countries) in which the lower house was elected exclusively through plurality rule in the most recent elections are coded as *maj* = 1, whereas years (countries) in which the lower house was elected through proportional rules are coded as *maj* = 0. Finally, years (countries) in which a government is subject to a confidence requirement are coded as *pres* = 1, while years (countries) in which the opposite is true are coded as *pres* = 0. The political variables are borrowed from Persson and Tabellini (2003) and from the World Bank’s Dataset on Political Institutions (DPI). (For more precise definition of the variables, consult the Appendix.)

Finally, I employ a battery of control variables that for various reasons are suggested, or have been found, to affect the level and composition of government spending.⁹ The control variables are the (log of) countries real per capita income (*LGDP*), the degree of a country’s openness (*TRADE_GDP*, measured as exports plus imports over GDP), measures of demographic compositions (*PROP_1564* and *PROP_65*, equivalent to the percentage of the population be-

⁹Persson and Tabellini (2003) provides a short survey of the most robust determinants of the size and composition of government spending.

tween 15 and 64 years of age and over 65, respectively), and a measure of idiosyncratic economic shocks (*gdpgap*, the log difference between real GDP in a particular country and its trend as computed with the so-called Hodrick-Prescott filter). The former four controls are taken from the WDI database, the latter is taken from Persson and Tabellini (2003).

Given the data at hand, inference is in the baseline sample based on 64 countries and a little more than 1000 observations, implying an average of around 16 observations per country. Because I am ultimately interested in the political economic effects of *democratic* political institutions, all observations must satisfy a democracy requirement to be allowed into the sample. I construct a democracy index, *democracy*, which is based on both the Gastil index provided by the Freedom House, and the Polity index from the Polity IV Project, and is normalized to lie between 0 and 1. I define the democracy threshold to be allowed into the baseline sample to be 0.75 on the *democracy* index, which corresponds to a Polity score of about 6 and a score on the Gastil Index of about 2. This threshold is deliberately restrictive, to avoid that the estimates reflect other institutional processes that may be endogenous to the endowments of oil.¹⁰ I also experiment with even more demanding thresholds, and show that the results are robust also when considering only the most stable democracies in the sample.

3.2 Data characteristics

Table 1 provides an overview over the countries in the main sample, and list their respective constitutional classifications based on observable constitutional features. In addition, the table indicates the maximum net value of oil production in GDP in one single year for all of the countries in the sample period. Finally, all of the countries listed in Table 1 are considered democratic according to the democracy rule discussed in the previous section. Several things are worth noting from Table 1. First, note the high degree of inertia associated with constitutional classification. There is only one registered lasting constitutional reform in the sample: Cyprus reformed its electoral system in 1981 from majoritarian to proportional. All of the other countries in the sample are associated with stable constitutional classifications throughout the sample period.¹¹ Second, note that a majority (42) of the countries in the sample derives, or has derived, positive revenues from oil production within the sample period. A significant fraction—about 30%—of the oil producers have experienced years within the sample period in which the net value of oil extraction have exceeded five percent of GDP. Moreover, the oil producers are evenly distributed across regime types. Finally, note that even when employing the strong democracy threshold, the size of the sample and the variation in oil revenues are considerable. The final column in Table 1 indicate which observations that drop out of the sample when employing the

¹⁰Several studies find that oil affects the level of democracy (Ross 2001; Tsui 2005; Aslaksen 2007). In addition, oil has been found to affect the level of corruption in countries (Ades and Di Tella 1999; Sala-I-Martin and Subramanian 2003; Aslaksen 2007). Both effects are most prevalent among the less stable democracies.

¹¹Note that France conducted a brief experiment with proportional electoral rules in the period 1985-1986, Spain has been classified as presidential during two years of transition to democracy in 1976-1977, and Pakistan has been classified as presidential for a couple of years in the 70'. None of these countries or observations, however, drive any of the effects highlighted in this paper.

stricter democracy threshold.¹²

The descriptive statistics in Table 2 describes the pattern of fiscal policy, for the whole sample and across regime types. The patterns evident from Table 2 reflect some of the general insights from the literature on the economic effects of constitutions (e.g., Persson and Tabellini 2003, 2004). On average, total and central government expenditures are higher in countries with a parliamentary form of government and/or a proportional electoral rule. Moreover, there is evidence that within parliamentary systems, a larger fraction of the government budget is allocated towards social security and welfare spending, *ssw*. Note that central government revenues and expenditures, as defined by Persson and Tabellini (2003), and total government revenues and expenditures, as defined by Brender and Drazen (2005), seem to capture essentially the same—their respective means differ only by decimals.¹³ Because the two sets of fiscal policy variables are highly correlated, also evident from the partial correlations in Table 3, I take advantage of both sets of variables to explore a larger set of observations than would have been possible if I had to rely only on either single one of them. Finally note that for most of the fiscal policy measures, the within group variation is large relative to the overall variation—in Table 2, indicated by high values of *W./O.*—a feature that is particularly useful when the statistical inference is based on country fixed effects regressions.

Table 3 displays the pairwise correlation coefficients of a subset of the variables in the sample. Except from *oilshock* (to be defined below), *oil_gdp* and the real PPP adjusted GDP per capita, *GDP_CAPppp*, all of the variables in Table 3 measures relative changes in the respective variables from one year to the next. Again, note the high and significant pairwise correlations between the fiscal policy variables of Brender and Drazen (2005), and of Persson and Tabellini (2003). There is also a positive and highly significant correlation between changes in the two subsets of government expenditures—social security and welfare spending (*ssw*) and total government final consumption expenditure (*tgcons*)—and total (or central) government expenditures (*tgexp* and *cgexp*), indicating that measures of government expenditures tend to move together. Finally, there is no evidence of correlations between the *level* of oil revenue, *oil_gdp*, and the contemporaneous changes in fiscal policies. However, Table 3 reveals a strong and highly positive correlation between *oilshock*—which, as will be shown later, proxies for the change in the tax base of the oil sector as a result of the change in the world market price of oil—and government revenues.

The rest of the variables that will be used are standard in the literature, and will hence not

¹²The countries that do not survive the more restrictive democracy threshold, and hence fall out of the sample whenever this threshold is employed, are Romania, Nepal, Pakistan, Argentina, Brazil, Dominican Republic, El Salvador, Guatemala, Honduras Mexico, Nicaragua, Paraguay, South Korea (Republic), Mali and Phillipines. In addition, there are several other countries which have experienced episodes of weaker democracy, and these periods are also dropped from the sample.

¹³The high correlation between central and total government fiscal policies may indicate inconsistencies in the definition and measurement of fiscal centralization. It will, however, not constitute a major problem for the present study; the research question at hand is whether and how fiscal policies in general are affected by an exogenously determined revenue shock, and whether the effects are different depending on regime types. I do not address the question of how to define and measure fiscal centralization, and whether the effects on fiscal policies of resource shocks depend on the degree of fiscal centralization (although this is an interesting topic in itself).

be summarized in detail. For descriptive statistics and more background for the set of control variables, the reader is referred to Persson and Tabellini (2003).

4 Identification and econometric model

To identify the idiosyncratic effects on the tax bases of exogenous variations in the world market price of oil, I construct the following proxy variable,

$$oilshock_{it} = \frac{oilprice_t - oilprice_{t-1}}{oilprice_{t-1}} oil_gdp_{it-1}.$$

The proxy variable $oilshock_{it}$ is a product of two derived, or composite, variables. The first variable, $(oilprice_t - oilprice_{t-1}) / oilprice_{t-1}$, is derived from annual data on the average world market price of crude oil, and measures the relative change in the oil prices from year $t - 1$ to year t . Because price innovations have different effects on government revenues in different countries, dependent on the production volumes and the cost structures of oil production in the respective countries, I employ idiosyncratic weights to the price innovations. Arguably, the effect of price fluctuations for government revenues depend positively on the level of production in the country, and on the efficiency in the oil production sector. The variable oil_gdp , borrowed from the World Banks Adjusted Net Savings dataset, is a composite measure of the unit rents from oil production—an indicator of the efficiency in the oil production sector—and of the production volume relative to GDP in country i in year t . Weighing the relative price changes with the oil_gdp variable takes into account the relative impact of a price innovation on the tax base in country i in year t .¹⁴ Finally, because price innovations may have immediate effects for fiscal and other policies, which again may influence on both the efficiency of the oil sector and the capacity and production volume in the same sector, I proxy for current oil_gdp_{it} using the lagged, and hence predetermined, variable oil_gdp_{it-1} .¹⁵ Hence, the estimates of the effect of the $oilshock$ on fiscal policies is not likely to be biased due to the reverse effects from fiscal policies to contemporaneous adjustments of the production in the oil sector.

One final attractive feature of utilizing price innovations to identify exogenous shifts in government revenues, is that the relative year-to-year changes in oil prices within the sample period from 1970 to 2001 possesses the statistical property of a ‘random walk’. The ‘random walk’ of oil prices carries over to the shock variable $oilshock$, which display no evidence of autocorrelation either.¹⁶ Hence, given that the relevant controls are included and any endogenous dynamics are

¹⁴As a robustness check, I also construct an oil shock variable, $oilvolshock$, which relies only on the production volumes and oil prices (as defined in the Appendix A1 and A2.4).

¹⁵Note that oil_gdp is a slow-moving variable, reflecting that capacity and production of oil is slow to adjust. Lagged values are hence strong proxies for current values.

¹⁶The variable $oilshock$ was tested for autocorrelation using the *corrgram* command in STATA, resulting in no statistical significant evidence of autocorrelation. By definition, the serial correlation in oil_gdp carries over to $oilshock$. However, in the $oilshock$ variable the dominant source of variation is the variation in the world market price of oil, which displays no pattern of autocorrelation. Hence, even though, as expected, oil_gdp is serially correlated, the transformed variable $oilshock$ is not statistically serially correlated within conventional levels of significance. Hamilton (1983) provides a thorough discussion of the exogeneity properties of oil prices.

controlled for, Ordinary Least Squares (OLS) should produce unbiased estimates of the effect of *oilshock* on fiscal policy outcomes.

As a point of departure, consider the following baseline empirical model,

$$y_{it} = \sum_{j=1}^p \alpha_{t-j} y_{it-j} + \sum_{j=0}^s [\gamma_{t-j}^0 + S_{it} (\gamma_{t-j}^1 - \gamma_{t-j}^0)] oilshock_{it-j} + \mathbf{x}'_{it} \boldsymbol{\beta} + \epsilon_{it}, \quad (1)$$

where y_{it} refers to the time differentiated fiscal policy measure, $oilshock_{it-j}$ is the proxy for the shock to the tax base in country i in year $t-j$, \mathbf{x}_{it} is the vector of time differentiated control variables, $\epsilon_{it} = \mu_t + \delta_i + u_{it}$ is the error term, and S_{it} is a constitutional dummy characterizing the political regime type in country i in year t . Assume that u_{it} is a white noise error term, such that $E(u_{it}) = 0$ for all i and t . The time differentiated set of controls is given by gdp_cap_{it} , $trade_gdp_{it}$, $prop_1564_{it}$, $prop65_{it}$ and $gdpgap_{it}$.¹⁷

Because model (1) is on a time-differentiated form, the country specific effects, δ_i , refer to country specific policy trends. Hence, including δ_i allows for the size and/or composition of government spending to display different long run paths (e.g., country specific ‘catch-up effects’). A central insight from the literature on the economic effect of constitutions is that countries indeed display different policy trends, which correlates with constitutional features (e.g., Persson and Tabellini, 2003). Including country specific trend terms in the specification ensures that country- and regime specific long term trends do not bias the estimates of the short-term effects of the shocks on fiscal policies.

Finally, the rationale for including a full set of time-dummies should be straight forward. Clearly, oil price innovations do not only affect the government revenues of oil producers, but constitute major adverse supply shocks affecting all open economies. Because shocks in the world market price of oil, and many other types of supply side shocks, hit all countries simultaneously, time-dummies should pick up biases due to internationally correlated counter-cyclical fiscal policies. In addition, the time dummies pick up the effects of other unidentified common events that affects policies in all countries, such as, e.g., international waves of ideologies. Examples of such waves may be the worldwide rise of left-wing ideologies in the late 1960s and 1970s accompanied by worldwide increases in the size of government, and the turn to the right in the mid-1980s associated with a clear halt in the growth of the size of government.

A couple of additional empirical challenges in estimating (1) may arise. First, when making inference from dynamic panel data models, there is always a concern whether the mechanical correlation between the compound disturbance and the lagged endogenous variable biases the parameter estimates of interest. Several GMM approaches, involving instrumenting for the lagged dependent variable, have been suggested in the literature to deal with the mechanical endo-

¹⁷It is not *a priori* clear whether all, if any, of these variables really belong in a model designed to capture short term changes in fiscal policies. We show in the empirical analysis that the effects of our main variable of interest, *oilshock*, on the fiscal policy measures do not depend on the inclusion or exclusion of any single of these control variables.

geneities of dynamic panel data models.¹⁸ The main findings in the present paper are shown to be robust to employing GMM estimators.

A final concern might be that the analysis does not explicitly take into account that transitory and permanent shocks might have different fiscal policy effects. Moreover, there is in general also a role for expectations; some shocks come as a surprise and some shocks may be anticipated. While expectations are indeed important when assessing the effects of shocks on fiscal- or other policy-, political-, or economic outcomes, they are unlikely to constitute a concern in the present study. The reason is that none of these mechanisms—expectations and the permanence of shocks—are likely to be robustly and significantly correlated with constitutional features. There is no reason to believe that the incidence of permanent shocks are systematically higher in, e.g., parliamentary democracies than in presidential democracies, or that expectations about future spot prices are any different in countries with, e.g., proportional electoral rules than in countries with majoritarian electoral rules.

5 Results

5.1 The fiscal pass-through of oil price innovations

Table 4 displays the estimates of the effects of the *oilshock* variable—contemporaneous, and with five lags—on the three different fiscal policy measures of total government revenues, budget balance, and expenditures. The regressions in Table 4 estimate the average effects of the shocks for the whole baseline sample of 64 countries and a little more than 1000 observations. The general pattern from the three regressions in Table 4 is remarkably clear and quite consistent with the economic predictions discussed in Section 2. By just looking at the regressions one can easily trace the effect of the tax base shocks as they are transmitted through the government records.

First, the shocks have both an immediate and a one year lagged, highly significant and positive, effect on the government revenues, *tgrev*. It is somewhat difficult to precisely quantify the effects without knowing more about the cost structure in the oil production industry of the respective oil producers. However, making the crude assumption that the taxable revenue, or profits, on average are growing proportionally with the oil prices, the estimates suggest that more than 30 percent of the net windfall value of the profitability shock in the oil sector flows into the government budgets within the first two years of the shock. Note that the mechanical correlation between the shock measures, *oilshock* and *l.oilshock*, and government revenues, *tgrev*, implies that the estimates of the effects of the shocks will be biased downwards: Among the oil exporters, the shocks simultaneously affect both the numerator and the denominator in the measure of government revenues. An increase in oil prices, increases the value of the oil exports, which simultaneously induces increases in government revenues *and* in the GDP of the oil exporters.

¹⁸See, for example, Arellano and Bond (1991) and Arellano and Bover (1995).

Assuming that the tax base shock have zero, or non-negative, effects on government expenditures, the results in Table 4 suggest that the mechanical bias is negative, as expected, and significant. Considering the contemporaneous effect of the shock, the bias would be equal to the difference between the effect on the budget balance and on the government revenues, which in Table 4 is statistically significant at the one percent level, and equal -0.09 . Note however that because the oil price innovations are not autocorrelated, this mechanical correlation in the contemporaneous and one year lagged effects should not bias the estimates of any of the remaining four lags of the *oilshock* variable.

Second, the estimates in Table 4, third column, suggest that the increases in government revenues from an oil shock translates into increased total government expenditures, *tgexp*, over the one- to three consecutive years of the initial shock. The strongest and most significant effect is observed after a lag of one year. The estimates suggest that the cumulative expansionary effects of the exogenous revenue shock is moderate, relative to the initial revenue shock. Yet, the effect is still considerable—depending on the extent of the bias in the estimate of the contemporaneous effect—,suggesting that the marginal short-term propensity to consume out of an oil price generated exogenous revenue shock is non-negative and significant.

A few additional regularities in Table 4 are worth noting. Interestingly, the tax base shocks are associated with negative lagged effects on the total government revenues. These effects are, as discussed in Section 2, consistent with the notion of government tax revenue substitution. First, consider the estimated effects on the government revenues, and assume an oil price shock in time t . As discussed previously, the price shock is associated with lagged increases in government expenditures. Moreover, as pointed out when discussing the econometric model, the *oilshock* is not autocorrelated—hence a positive shock in time t is not likely to be followed by a correction in the following periods. Given this interpretation, the discussion in Section 2 implies that the optimal response would be to substitute between revenues from the oil sector and other sources of government revenues, such as, e.g., different taxes and government fees. If the government indeed substitutes between sources of revenue, and if it takes some time to implement the new tax legislation, one would expect that the direct *lagged* effects of the oil shock would be reductions in the government revenues—which is exactly what we see in the first column in Table 4. Finally, the estimates in Table 4 indicate that the cumulative dynamic effect for the net government debt of an oil shock is slightly positive—the positive revenue effect is almost cancelled out by lagged increases in government expenditures and revenue substitution, both of which exert negative effects on the net government debt.

None of the time-differentiated control variables are associated with significant effects on the change in total government expenditures. This is not surprising, given that most of these variables are indeed slow-moving, with the exception of the change, or growth, in (the log of) GDP per capita, *lgdpcap*. In fact, it turns out that the effects of the oil shocks are not sensitive to the inclusion of the set of controls. As dropping the set of controls also implies an increase in the number of degrees of freedom in the estimations, as well as making it possible to take advantage

of a larger subset of the full sample, the set of controls will be dropped in most of the regressions that follow. Note that (the log of) GDP per capita, $lgdpcap$, is mechanically correlated with all of the output variables, which explains the robust and strongly negative correlation between these variables in Table 4. Because GDP per capita in the literature have been shown to correlate with fiscal policies, I always include $lgdpcap$ in the regressions. However, none of the main results are sensitive to the inclusion of this variable either.

5.2 Comparative politics

5.2.1 Forms of government

This section investigates and compares the performance of parliamentary and presidential forms of government. Table 5a displays the results from running the full model given by equation (1). The dummy S_{it} equals one if the form of government is presidential, and zero if the form of government is parliamentary. As model (1) is a standard dynamic panel data model where the first lag of the regressand is included among the regressors, there is the potential worry that the mechanical correlation between the lagged endogenous variable y_{it-1} and the error term u_{it} might bias the estimates. To meet this challenge, the Arellano and Bond GMM estimator, which instruments for the lagged endogenous variable, is employed on all three regressions in Table 5a. The difference between the OLS fixed effects estimates and the GMM estimates are however negligible, suggesting that the OLS estimates are only to a very small degree biased due to the endogeneity from the inclusion of the lagged fiscal policy variables. This finding is also consistent with the findings in the literature suggesting that the fixed effects estimator is consistent, and in fact often preferable to the GMM estimator, when the panel is sufficiently long (see, e.g., Wooldridge, 2002).

Consider first the estimates of the parliamentary democracies, corresponding to the top four rows in Table 5a. In accordance with Table 4, oil shocks have immediate and positive effects for government revenues and the budget balance, and negative, although insignificant, effects for lagged government revenues. However, in contrast to Table 4 there are no signs of the positive revenue effects translating into increases in government expenditures. The estimates hence suggest that the cumulative effects of the oil shocks for government net savings in the parliamentary countries are highly positive.

Now turn to the presidential countries, for which the estimated effects of the oil shocks equal the estimate of *oilshock* (with lags) plus the estimate of the interaction term *oilshock_pres* (with lags). First note that the immediate effects for government revenues and the budget balance are more or less similar to the parliamentary countries. However, unlike the parliamentary governments, the evidence suggests that the presidential governments quickly transform the increased revenues into increases in government expenditures. As a consequence, both the contemporaneous and the lagged effects for the government budget balances are significantly worse, or more negative, in the presidential countries than in the parliamentary countries.

One potential problem with the regressions in Table 5a is that the model forces the fiscal policy dynamics to be identical across regime types; it is possible that the effects of the lagged fiscal policy variables are depending on constitutional features. Nonlinear, regime dependent fiscal policy dynamics is consistent both with the findings above, suggesting a higher degree of inertia in fiscal policy adjustments among parliamentary democracies than among presidential democracies, and with the findings of Persson and Tabellini (2003), suggesting that there is more inertia in the fiscal policy adjustment in parliamentary regime types. In addition, there may indeed be other regime dependent nonlinearities at play as well, for example with regard to the effect of the controls, or with regard to the effect of the unobserved common events (i.e., the time dummies). The easiest and most straight forward way to take these concerns into account, is to run separate regressions for the two different regime types. Table 5b shows the result of running separate regressions for parliamentary and presidential countries, respectively.¹⁹

The most important thing to note from Table 5b is that the estimates of the *oilshock* variable and its respective lags are very similar as in Table 5a. Consistent with Persson and Tabellini (2003), there is more inertia in the policy adjustment within the parliamentary systems. Taking into account the bias caused by the effect of the shocks on the denominator in the fiscal policy measures, the only effect of the oil shock among the parliamentary countries is to raise government revenues and improve the government budget balance. Among the presidential countries, on the other hand, the revenue shocks have strong and significant effects for the two subsequent lags of government expenditures following the initial oil price shock. Finally, the estimates suggest that the revenue substitution effect is statistically more significant among the presidential countries.

The graphs in Figure 1 simulate the dynamic fiscal effects of an oil price induced tax base shock in parliamentary and presidential systems, using the parameter estimates from Table 5b. The magnitude of the simulated shock corresponds to an increase the tax base of 10% in GDP.²⁰ The three diagrams in Figure 1 illustrates the stark differences in the dynamic effects of the shocks between parliamentary and presidential systems along the policy dimensions of total government revenues, expenditures, and budget balance, respectively. The overall picture is clear: the revenue effect of the tax base shock is very similar between the regime types, but the expenditure- and budget balance effect is different. Note that the negative effect for government expenditures among the parliamentary regime types is statistically insignificant, while the positive effect among the presidential systems is significant (see Table 5b). Moreover,

¹⁹Running the regressions separately on the two subsamples, as is done in Table 5b, is different from running a single regression and allowing for interaction effects between the dependent variable and dummies for constitutional features (in this case *pres*); running two separate sets of regressions implies that one allows *any* variable in the model to have nonlinear effects on the endogenous variables, with respect to the presidential dummy. Running separate regressions is hence slightly less efficient and more restrictive than the alternative of only interacting some of the regressors with the presidential dummy. I have also run regressions where the only additional nonlinearity is in the effects of the lagged dependent variable, i.e., letting α_{t-j} vary between the regime types. It turns out that the estimates of γ_{t-j}^0 and γ_{t-j}^1 are stable and consistent throughout.

²⁰Note that an *oilshock* of 10% is not equivalent to a price shock of 10%, because the variable *oilshock* also depend on the oil production in the respective countries. In the model, if the net value of oil production in GDP (*oil_gdp*) is 50%, the construction of *oilshock* implies that it requires a fossil energy price shock of 20% to induce a 10% change in *oilshock*.

Figure 1: Fiscal policy simulations

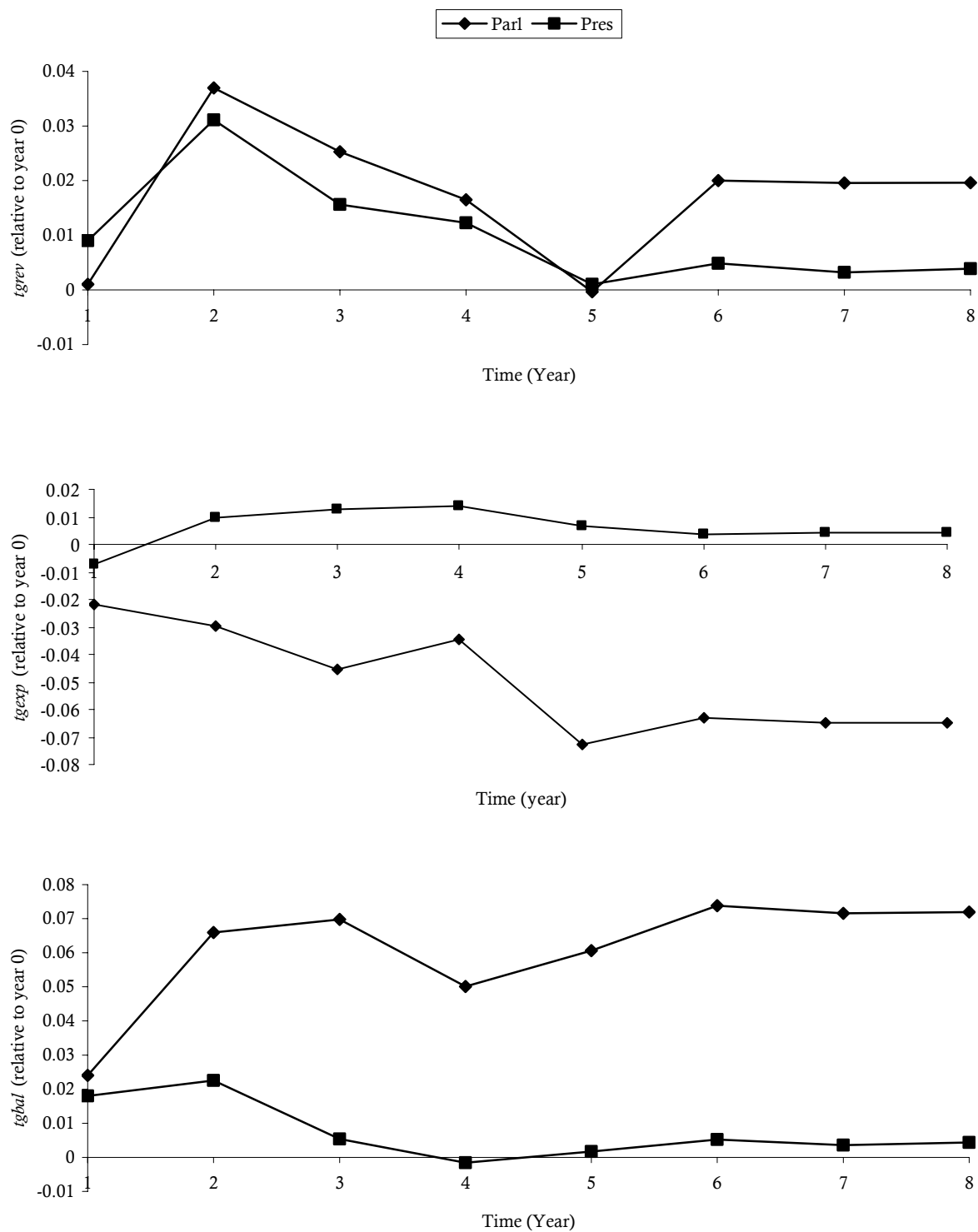


Figure 1. The graphs simulate the short run dynamic effects on total government revenues, expenditures and budget balance of a tax base shock (*oilshock*) in year 1 equal to 10% in GDP, in parliamentary and presidential systems, respectively. The simulations are calibrated employing the parameter estimates of Tables 5b and 6b in model (1).

Figure 2: Fiscal policy simulations, adjusted for measurement biases

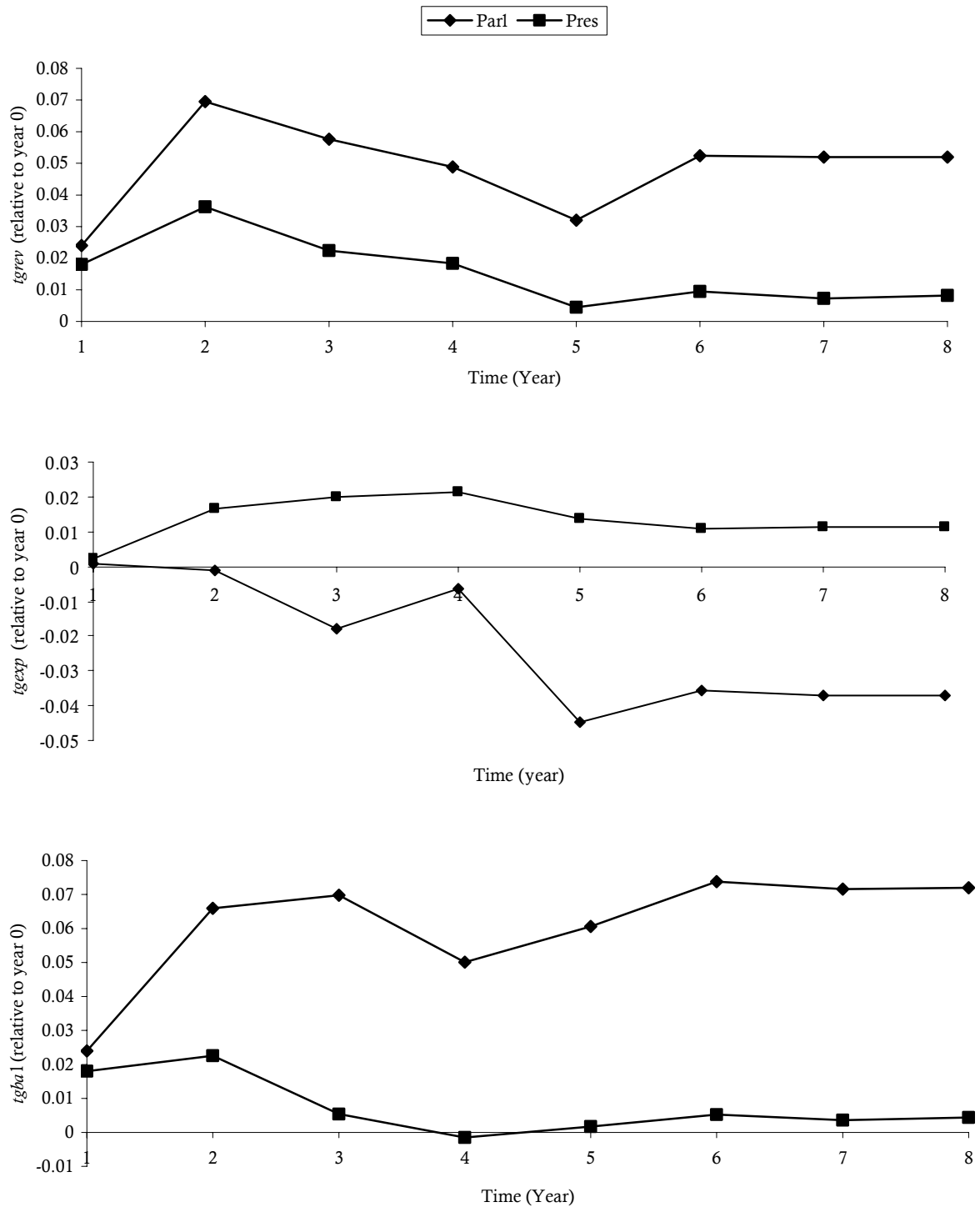


Figure 2. The graphs simulate the *adjusted* short run dynamic effects on total government revenues, expenditures and budget balance of a tax base shock (*oilshock*) in year 1 equal to 10% in GDP, in parliamentary and presidential systems, respectively. The simulations are calibrated employing the parameter estimates of Tables 5b and 6b in model (1). Fiscal policies in base year 0 are normalized to equal 0, and government revenues- and expenditures in periods 1 and 2 are adjusted to correct for the mechanical downward bias in the estimates due to the effects of *oilshock* on GDP (adjustment procedure is explained in the Section 5.2.1.).

the effects are, as discussed in Section 5.1, likely to be biased downwards. In figure 2, I employ the assumption that the real (unbiased) expenditure effects are in fact non-negative, assuming that the downward biases are given by the differences between the budget balance effects and the revenue effects. Consequently, the graphs for government revenues- and expenditures shift upward. However, the broad picture remains—in the parliamentary systems, a positive shift in the tax base leads in the short run to a positive shift in government net savings, while in the presidential systems the effect is mainly an increase in government expenditures.

It is interesting to note that the main finding in the present analysis—that the shocks have a stronger fiscal impact among the presidential regime types—is inconsistent with the pattern found by Persson and Tabellini (2003, Table 8.1 and Figure 8.1). However, as Persson and Tabellini exploits the time dummies to investigate nonlinearities in the fiscal policy response to economic shocks, they are not able to distinguish between the effects of different types of shocks, and whether the shocks works via government revenues, stabilizing policies, or increases in the cost of public goods production due to adverse supply side shocks. One plausible interpretation of the diverging results is that Persson and Tabellini's (2003) results reflect more the steep fiscal policy trends among the parliamentary countries than differences in the responses to shocks. An alternative interpretation is that while presidential systems are more inclined to consume out of an exogenous revenue shock, the parliamentary governments may be comparatively more responsive to the adverse supply side effects of shocks in the world market price of oil. In the present study, I control for counter cyclical policies by including time fixed effects. However, it may be that on average, the counter cyclical fiscal effects dominate the budget balance effect among parliamentary oil producers, implying that the *aggregate* effect on fiscal policies among parliamentary countries from an oil price shock is stronger than among presidential regimes.

5.2.2 Electoral rules

In Table 6a countries are sorted with respect to electoral rules rather than the form of government. Except from the estimated effects on government revenues, which is positive among both regime types, but slightly more so among the majoritarian countries, there are no significant differences in the effect of the oil shocks between the two electoral systems. Table 6b confirms the pattern from Table 6a. However, there is another interesting difference between the regime types; the endogenous dynamics suggests that government expenditures adjust more slowly among the proportional electoral systems. This finding is consistent with the veto player theory, as discussed in Section 2—proportional systems are usually being associated with a higher incidence of veto players in the government, slowing down the legislative process. In the next section, when I investigate the transmission channel more in depth and combine electoral rules and forms of government, this feature of inertia among the proportional systems will become evident also with regard to the effect of the oil shocks.

5.2.3 Transmission channel

So far I have assumed that as long as time dummies are included in the regressions, the estimated effect of the oil shocks on the government expenditures is a government budget effect, working via the government revenues. In this section, I investigate this assumption empirically, by employing an IV-strategy where *oilshock* is used as an instrument for government revenues. A test of the exclusionary restrictions provides an indication of whether the oil shocks affect government expenditures exclusively via their effects on government revenues.

All of the IV-regressions in this section are two-stage least squares regressions, where contemporaneous and lagged values of *oilshock* are instruments for contemporaneous and lagged government revenues. In particular, I use five lags, in addition to the contemporaneous shock, to instrument for contemporaneous and two lags of government revenues.²¹ All variables are instrumented and estimated simultaneously.²²

The estimates from the IV-regressions are displayed in Table 7. The F-statistics of the simultaneous significance of the instruments in the first-stages are statistically significant throughout. The variable *oilshock* is hence a strong predictor of innovations in government revenues. The strongest first-stages are among the presidential systems and the countries with proportional electoral rules. The weakest first-stages are among the parliamentary systems and the countries with majoritarian electoral rules, where some of the first-stage F-statistics are below 10. This pattern partly carries over to the regressions in Columns (6)-(9), where the forms of government and the electoral rules are combined; the strongest first-stages are in the group of presidential countries with proportional electoral rules. For the instruments to be valid—in this context, for *oilshock* to affect government expenditures exclusively through government revenues—they also have to satisfy an excludability requirement. The exclusion restrictions are difficult to test empirically, although some indirect tests are available. One available test is the Sargan (1958) statistics. The *p*-values from the Sargan tests in Table 7 suggest that the exogeneity of the over-identifying restrictions can not be rejected in most of the regressions. In some regressions, and in particular among the presidential regimes, the rejection is however somewhat weak, indicating that *oilshock* may have an independent effect on government expenditures.

The results from the IV-regressions in Table A5 are highly consistent with the empirical patterns in tables 5a through 6b. Interestingly, the difference between the two electoral systems

²¹Note that all six sets of shock observations (the contemporaneous observation *oilshock*, as well as the five lags $L.oilshock - L5.oilshock$) are used as instruments for all of the three government revenue variables (the contemporaneous variable *tgrev*, as well as the two lags $L.tgrev - L2.tgrev$). This is done for methodological consistency; one should not discriminate among which instruments to use in each first stage regression whenever employing more than one first-stage regression. Each regression in Table 7 implicitly contains three first-stage regressions. There are two things to note about the first-stage regressions. First, they two-way fixed effects regressions, which, as discussed previously, hypothetically may be biased because of the dynamic features of the model. Second, because the second-stages include one lag of the respective endogenous regressands, so do all of the first-stages. Table A4 illustrates to what degree these features bias our first-stage estimates (only the first first-stage of column (1) in Table A5 is shown). Comparing the two GMM regressions in Table A4 with the FE-regression in the first column, we see that neither of the abovementioned potential problems seem to significantly bias our first-stages. The estimates are very stable and not sensitive to neither GMM or the inclusion of *L.tgexp*.

²²The IV-regressions are estimated using the program *ivreg2* in the STATA 9.2.

which was less pronounced in Table 6b is now significant; while exogenous shocks in the government revenues have significant lagged effects for government expenditures among the countries with proportional electoral rules, there are no significant effects among the majoritarian systems. More interestingly, however, there is evidence that the political institutions interact. The form of government appear to be the most dominant of the two types of constitutional features, and combined with a parliamentary form of government none of the electoral rules are associated with significant effects. Turning to the presidential systems, however, both electoral rules are associated with positive expenditure effects of exogenous shocks to the government revenues. Consistent with the dynamic aspects of the electoral rules from Table 6b—that proportional systems were associated with stronger inertia in the adjustment of government expenditures—the effects appear with one more lag among the proportional presidential systems than among the majoritarian presidential systems.

6 Relation to two other literatures

6.1 The ‘resource curse’ literature

As mentioned in the introduction, there is evidence that institutions matter for the so-called ‘resource curse’.²³ Although attention mainly has been drawn to the experiences of developing countries, there is evidence of a ‘resource curse’ also among modern democracies (Andersen and Aslaksen 2008). Moreover, there is an increasing amount of evidence pointing in the direction that not only the *quality*, but also the *design*, of institutions are key to understanding the diverging experiences of resource abundant countries (see, e.g.: Mehlum et.al. 2006; Boschini et.al. 2007; Andersen and Aslaksen 2008; Collier and Goderis 2007). Andersen and Aslaksen (2008) find that constitutional features are highly and robustly correlated with the effect of natural resource abundance, or dependence, on economic development.²⁴ In particular, the form of government appear to be important, and the evidence suggest that the ‘resource curse’ among democratic countries are primarily driven by presidential countries—there is no statistical evidence of a resource curse among the countries with a parliamentary form of government. Is it possible that the findings in the present paper may shed light on the constitutional empirical patterns in the resource curse literature? Interestingly, recent evidence suggest that policy volatility exerts a

²³Some researchers have contested the notion of a ‘resource curse’. In particular, Brunnschweiler and Bulte (2008_a, 2008_b) claim that the so-called ‘curse’ is in fact a ‘red herring’, and that natural resource abundance may be conducive, not detrimental, to economic growth. Interestingly, however, Brunnschweiler and Bulte do find significant constitutional effects. In particular, presidentialism is found to be a significant determinant of natural resource dependence.

²⁴The idea that the political incentives and the degree of accountability associated with the political system are important factors in understanding the ‘resource curse’ has been emphasized also by others (e.g., Robinson, Torvik and Verdier 2006). In addition, other political mechanisms may interact with the constitutional political institutions. Robinson and Torvik (2005) show how resource rents make ‘white elephants’—that is, economically inefficient public projects—more attractive to politicians. Although outside the scope of the present paper, it is possible that ‘white elephants’ are more attractive, and hence more frequent, within some political institutional frameworks than others. Investigating interactions between the *quality* and the *design* of political institutions is certainly an interesting avenue for future research.

strong and direct negative impact on growth (Fatás and Mihov 2005). Moreover, Poelhekke and van der Ploeg (2008) find that the ‘resource curse’ is robustly related to output volatility, which in turn may be attributed to commodity price volatility. The evidence from the present analysis suggests that fiscal policy responses to price volatility may enforce the output volatility caused by commodity price volatility. Moreover, the effect is highly dependent on the constitutional framework—price volatility induce fiscal volatility only among the presidential oil producing countries, not among the oil producers with a parliamentary form of government. Given the evidence at hand, fiscal policy induced output volatility may thus be a plausible explanation for why only presidential resource abundant countries are hurt by the ‘resource curse’, while parliamentary resource abundant countries stay clear of the ‘curse’. It is however still an open question *why* the oil producing presidential countries are more prone to increase government expenditures when subject to an oil price induced revenue windfall, than oil producing countries with a parliamentary form of government. One plausible hypothesis, building on the veto player theory, is that the high incidence of coalition governments in parliamentary democracies implies a relatively high frequency of veto players, and hence a higher tendency to “lock in” economic policy. If so, the evidence suggests that in some circumstances having political institutions that slow down the legislative processes may in fact be a blessing, and not a curse.

6.2 The ‘flypaper effect’ literature

The so-called ‘flypaper effect’-literature is concerned with the empirical regularity that the fiscal effects of lump-sum transfers often are disproportionately large. Unconditional grants and lump-sum transfers have the tendency to ‘stick where they hit’, hence the name ‘flypaper effect’. In the present context, the additional revenues that oil exporters receive as a consequence of innovations in oil prices may be treated as unconditional grants. As discussed in Section 2, it is inconsistent with standard economic theory for transitory shocks to induce significant increases in government expenditures and, presumably, consumption. Hines and Thaler (1995, p. 218) note: “... that state governments will use the grants they receive from the federal government to increase local spending, is inconsistent with simple economic theory. To see why, consider the simplest case, in which residents of the local jurisdiction receive a lump-sum grant, unaccompanied by any significant change in taxes or spending. This grant is the equivalent of an increase in income. The residents of the local jurisdiction should spend this increase in income just like any other increase, and the share devoted to government goods and services should be equivalent to the marginal propensity of local governments to spend out of income: for states this would be on the order of 5-10 cents on the dollar.”

The magnitude of the estimates in the present analysis suggest that the effects on government expenditures are on average well above “5-10 cents on the dollar”. The evidence hence suggests that there are ‘flypaper effects’ at work also with regard to the fiscal effects of government revenue shocks in general, as suggested by the IV-estimates in Table A5, and with regard to oil price shocks in oil abundant economies, as shown in Tables 5a through 6b. Even more interesting,

however, the effects appear to be dependent on the formal political institutional context; the ‘flypaper effect’ seems to be much more pronounced among presidential countries than among countries with a parliamentary form of government. These observations suggest that ‘flypaper effects’, in addition to the hypotheses that have been put forward in the literature, may be determined by the incentive systems that are embedded in formal political institutions. To my knowledge, the idea that ‘flypaper effects’ may be institutionally, or constitutionally, determined, has not been yet been put forward and may deserve some attention.

7 Final remarks and avenues for future research

This paper takes advantage of a previously unexplored source of exogenous variation in the tax bases of countries—the variation in the profits in the oil sector that may be accrued to variation in the world market price of oil—to identify effects of constitutional features on fiscal policies. The proposed ‘natural resource experiment’ constitutes a new strategy of identification in political economy which is less afflicted by the many problems of endogeneity that are common in the literature, such as biases due to simultaneity, reversed causality and omitted variables (Acemoglu 2005). Constructing a proxy for the effect on profits in the oil sector from variation in oil prices, it is shown that the proxy is a strong determinant for government revenues among all oil producing countries. However, the countries in the sample display great variation in to what extent the variability in government revenues induce variability in government expenditures, and the evidence suggests that constitutional features are strong determinants for the fiscal policy responses to the oil revenue shocks.

The present analysis focuses on the effect of the two features of the constitutions of countries that in the literature has been widely accepted as the most fundamental constitutional rules in modern representative democracies—namely the legislative rules associated with different forms of government and electoral rules (Persson and Tabellini 2003). Both dimensions of the constitutions are associated with significant effects, although the strongest differences are found with regard to the form of government. In particular, while oil price variability have no effects for government expenditures among the parliamentary oil producers, the effects are relatively strong among the presidential oil producing countries. Consequently, an increase in the oil prices induces increased net government saving if the form of government is parliamentary, while the effect on government net saving is neutral if the form of government is presidential. The electoral rules exert no effect on the inclination to save or consume out of an oil price induced revenue shock. However, the electoral rules are important for the dynamics of the fiscal policy responses. First, fiscal policies adjust more slowly if the electoral rule is proportional, as opposed to majoritarian. Second, and consistent with the first finding, the effects on the government expenditures among the presidential countries are more delayed if the electoral rule is proportional.

There are to my knowledge no theories in the literature which can explain the effects of the form of government found in the present study. In fact, the effects go in the opposite

direction than what is consistent with the existing static theories on the effects of the forms of government, where shifts in the tax base should have a stronger effect on government expenditures if the form of government is parliamentary (Persson and Tabellini 1997, 2000). The effects of the electoral rules, on the other hand, are highly consistent with the veto players theory (e.g., Tsebelis 1995, 1999, 2000), which predicts a higher degree of legislative inertia if the electoral rule is proportional.

The ultimate goal of the present research project is to achieve a better understanding of the performance of different constitutional political institutions. Hence, theories that address the comparative short-term effects of positive shocks to the government budget, and which can explain the empirical patterns with regard to the effects of constitutional features that is documented in this study, should be developed.

8 Literature

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9 Tables and figures

9.1 Tables

Table 1
Countries, constitutions, samples and fossil energy intensity

Country	<i>pres</i>	<i>maj</i>	Dem. years	Maximum annual net value of oil depletion in GDP (<i>oil_gdp</i>)			Strong Dem.
				>0%	>1%	>5%	
Austria	0	0	1970-2001	Yes			Yes
Belgium	0	0	1970-2001				Yes
Cyprus*	0	0	1981-2001				Yes
Czech Rep.	0	0	1992-2001	Yes			Yes
Denmark	0	0	1970-2001	Yes			Yes
Estonia	0	0	1992-2001				No
Finland	0	0	1970-2001				Yes
France*	0	0	1985-1986	Yes			Yes
Germany	0	0	1989-2001	Yes			Yes
Greece	0	0	1974-2001	Yes			1985-2001
Hungary	0	0	1990-2001	Yes			Yes
Ireland	0	0	1970-2001				Yes
Israel	0	0	1970-2001	Yes	Yes		Yes
Italy	0	0	1970-2001	Yes			Yes
Japan	0	0	1970-2001	Yes			1973-2001
Netherlands	0	0	1970-2001	Yes			Yes
Norway	0	0	1970-2001	Yes	Yes	Yes	Yes
Poland	0	0	1990-2001	Yes			1994-2001
Portugal	0	0	1975-2001				1976-2001
Romania	0	0	1996-2001	Yes	Yes		No
Slovak Rep.	0	0	1992-2001	Yes			1997-2001
Slovenia	0	0	1993-2001				Yes
South Africa	0	0	1992-2001	Yes	Yes		1993-2001
Spain**	0	0	1977-2001	Yes			Yes
Sweden	0	0	1970-2001	Yes			Yes
Turkey	0	0	72-77/82-01	Yes			72-77/88-92
[n(<i>parlprop</i>)=26]				[=19]	[=4]	[=1]	[=24]
Australia	0	1	1970-2001	Yes	Yes		Yes
Botswana	0	1	1970-2001				1996-2001
Bulgaria	0	1	1991-2001	Yes			2000-2001
Canada	0	1	1970-2001	Yes	Yes	Yes	Yes
Cyprus*	0	1	1973-1980				Yes
France*	0	1	70-84/86-01	Yes			1986-2001
India	0	1	1970-2001	Yes	Yes		70-74/94-01
Mauritius	0	1	1970-2001				Yes
Nepal	0	1	1990-2001				No
New Zealand	0	1	1970-2001	Yes	Yes		Yes
Pakistan**	0	1	1988-1998	Yes	Yes		No
Papua NG	0	1	1975-2001	Yes	Yes	Yes	Yes
Thailand	0	1	1991-2001	Yes			Yes
Trin. &Tob.****	0	1	1970-2001	Yes	Yes	Yes	1983-2001
UK	0	1	1970-2001	Yes	Yes	Yes	Yes
[n(<i>parlmaj</i>)=15]				[=11]	[=8]	[=4]	[=13]

Table 1 (continued)

Country	<i>pres</i>	<i>maj</i>	Dem. years	>0%	>1%	>5%	Strong Dem.
Argentina	1	0	72-75/82-01	Yes	Yes	Yes	No
Bolivia	1	0	1982-2001	Yes	Yes	Yes	1984-2001
Brazil	1	0	1985-2001	Yes	Yes		No
Colombia	1	0	1970-2001	Yes	Yes	Yes	1990-1994
Costa Rica	1	0	1970-2001				Yes
Dom. Rep.	1	0	1978-2001				No
Ecuador***	1	0	1979-2001	Yes	Yes	Yes	1979-1999
El Salvador	1	0	1982-2001				No
Fiji	1	0	1974-1986				Yes
Guatemala	1	0	1995-2001	Yes	Yes		No
Honduras	1	0	1981-2001				No
Mexico	1	0	1996-2001	Yes	Yes	Yes	No
Nicaragua	1	0	1990-2001				No
Paraguay	1	0	1991-2001				No
Peru	1	0	79-91/00-01	Yes	Yes	Yes	2000-2001
Spain**	1	0	1976-1977	Yes			No
Sri Lanka	1	0	1970-2001				1970-1977
Switzerland	1	0	1970-2001				Yes
Uruguay	1	0	1984-2001				Yes
Venezuela***	1	0	1970-2001	Yes	Yes	Yes	1970-1991
[n(<i>presprop</i>)=20]				[=10]	[=9]	[=7]	[=10]
Chile	1	1	70-72/88-01	Yes			1999-2001
Gambia	1	1	1970-1993				No
Korea, Rep.	1	1	1987-2001				No
Lithuania	1	1	1992-2001	Yes			Yes
Madagascar	1	1	1992-2001				1992-1996
Mali	1	1	1991-2001				No
Pakistan**	1	1	1987-1988	Yes			No
Philippines	1	1	1986-2001	Yes			No
US	1	1	1970-2001	Yes	Yes	Yes	Yes
[n(<i>presmaj</i>)=9]				[=5]	[=1]	[=1]	[=4]

Note: Classifications follow criteria described in the text: exclusive reliance on plurality rule in (lower house) legislative elections are coded *maj* = 1, other countries *maj* = 0; countries in which the executive is relying on the confidence of the legislature are coded *pres* = 1, others *pres* = 0. * Country has undergone electoral reform. ** Country has undergone reform in the form of government. *** OPEC member. **** An extreme outlier which is excluded from the baseline sample (see the Appendix, Section A2.2). "Dem. years" mean years in which the country has received a *democracy* score >.75 (~ a Polity score > 6). "Strong Dem" mean a *democracy* score > .9 (~ a polity score > 9).

Table 2
Descriptive statistics: Fiscal policies, levels of income and annual net value of oil depletion in gdp

Variable	All countries			Parliamentary gov't		Presidential gov't		Proportional el. sys.		Majoritarian el. sys.		
	Mean	Std. Dev.		Mean	N/n/T	Mean	N/n/T	Mean	N/n/T	Mean	N/n/T	
		Overall	W./O.									N/n/T
<i>CGEXP</i>	29.2	12.0	0.40	1173/64/18	34.0	750/37/20	20.7	423/28/15	30.1	798/46/17	27.2	375/20/19
<i>TGEXP</i>	28.1	11.4	0.44	1235/67/19	33.1	801/37/22	18.8	434/29/15	28.5	858/46/19	27.0	377/20/19
<i>CGREV</i>	25.8	10.6	0.33	966/48/20	29.8	630/26/24	18.3	336/23/15	26.1	644/35/18	25.2	322/15/21
<i>TGREV</i>	25.7	10.3	0.32	1232/64/19	30.0	805/37/22	17.4	427/29/15	25.9	853/46/19	25.1	379/20/19
<i>CGBAL</i>	-3.3	4.4	0.77	969/49/20	-3.9	621/26/24	-2.4	348/23/15	-3.8	654/36/18	-2.4	315/15/21
<i>TGBAL</i>	-2.5	4.0	0.74	1227/64/19	-3.0	801/37/22	-1.4	426/29/15	-2.7	850/46/18	-1.9	377/20/19
<i>CGSSW</i>	8.7	6.3	0.27	809/47/17	10.7	548/27/20	4.7	261/21/12	10.0	539/34/16	6.3	270/15/18
<i>TGCONS</i>	16.8	6.2	0.44	1577/68/23	19.1	1014/40/25	12.7	563/30/19	16.7	1058/46/23	17.0	519/24/22
<i>GDP_CAP_{ppp}</i>	12.9	8.7	0.32	1467/68/22	15.7	931/40/23	7.9	536/30/18	13.7	987/46/21	11.3	480/24/20
<i>oil_gdp</i>	2.5	6.5	0.39	1618/68/23	1.6	1041/40/26	4	577/30/19	2.5	1070/46/23	2.5	548/24/23

Table 3
Descriptive statistics: Pairwise correlations

	<i>cgexp</i>	<i>tgexp</i>	<i>cgrev</i>	<i>tgrev</i>	<i>cgbal</i>	<i>tgbal</i>	<i>cgssw</i>	<i>tgcons</i>	<i>oil_gdp</i>	<i>oilshock</i>	<i>l.oilshock</i>	<i>d_gdp</i>
<i>tgexp</i>	0.73*											
<i>cgrev</i>	0.34*	0.33*										
<i>tgrev</i>	0.28*	0.41*	0.74*									
<i>cgbal</i>	-0.50*	-0.57*	0.21*	0.32*								
<i>tgbal</i>	-0.45*	-0.64*	0.32*	0.44*	0.81*							
<i>cgssw</i>	0.43*	0.41*	0.18*	0.15*	-0.25*	-0.26*						
<i>tgcons</i>	0.28*	0.34*	0.11*	-0.04	-0.36*	-0.37*	0.18*					
<i>oil_gdp</i>	0.00	0.01	0.02	0.03	0.03	0.02	0.02	-0.02				
<i>oilshock</i>	0.01	-0.01	0.19*	0.22*	0.09*	0.19*	-0.02	-0.05	0.39*			
<i>l.oilshock</i>	0.07	0.07	0.05	0.11*	-0.03	0.02	0.01	0.03	0.33*	0.13*		
<i>d_gdp</i>	-0.11*	-0.09*	-0.06	-0.02	0.05	0.07*	-0.25*	-0.07*	-0.07*	0.00	0.04	
<i>GDP_CAPppp</i>	-0.06	-0.05	-0.02	-0.01	0.02	0.04	-0.04	0.03	-0.17*	-0.01	-0.02	-0.08*

The table displays the respective pairwise correlation coefficients. * indicates a significance level of one percent.

Table 4
Oil shock pass-through. Baseline sample.

	<i>tgrev</i>	<i>tgbal</i>	<i>tgexp</i>
<i>oilshock</i>	0.10 (0.03)**	0.19 (0.03)**	-0.09 (0.04)*
<i>L.oilshock</i>	0.22 (0.05)**	0.11 (0.05)*	0.13 (0.02)**
<i>L2.oilshock</i>	-0.08 (0.01)**	-0.11 (0.02)**	0.02 (0.02)
<i>L3.oilshock</i>	-0.09 (0.01)**	-0.13 (0.03)**	0.02 (0.02)
<i>L4.oilshock</i>	-0.05 (0.02)*	0.04 (0.03)	-0.10 (0.03)**
<i>L5.oilshock</i>	-0.01 (0.02)	0.04 (0.02)	-0.04 (0.03)
<i>lgdpcap</i>	0.06 (0.26)	0.47 (0.27)	-0.43 (0.24)
<i>trade_gdp</i>	0.03 (0.02)	0.04 (0.02)*	-0.02 (0.02)
<i>prop_1564</i>	-0.22 (0.36)	-0.62 (0.40)	0.32 (0.43)
<i>prop_65</i>	1.15 (0.71)	0.83 (0.79)	0.35 (1.03)
<i>L.tgrev</i>	-0.20 (0.10)		
<i>L.tgbal</i>		-0.31 (0.09)**	
<i>L.tgexp</i>			-0.19 (0.07)*
Estimation method	OLS	OLS	OLS
Observations	1032	1030	1038
Countries	64	64	64
R-squared	0.13	0.21	0.12

*-Significant at the 5 percent level; **-significant at the 1 percent level. The numbers in parentheses are robust standard errors. All regressions are fixed-effects regressions, including a full set of country- and year indicators.

Table 5a
Oil shock pass-through and forms of government. Baseline model and baseline sample.

	<i>tgrev</i>		<i>tgbal</i>		<i>tgexp</i>	
<i>oilshock</i>	0.06 (0.06)	0.05 (0.07)	0.27 (0.08)**	0.29 (0.09)**	-0.21 (0.04)**	-0.23 (0.06)**
<i>L.oilshock</i>	0.42 (0.12)**	0.38 (0.12)**	0.44 (0.09)**	0.39 (0.12)**	0.00 (0.17)	0.01 (0.19)
<i>L2.oilshock</i>	0.02 (0.15)	0.01 (0.14)	0.07 (0.11)	0.08 (0.12)	-0.02 (0.11)	-0.05 (0.11)
<i>L3.oilshock</i>	-0.09 (0.09)	-0.12 (0.08)	-0.25 (0.16)	-0.26 (0.16)	0.14 (0.18)	0.10 (0.17)
<i>L4.oilshock</i>	-0.20 (0.14)	-0.23 (0.14)*	0.16 (0.16)	0.12 (0.17)	-0.38 (0.23)	-0.41 (0.22)
<i>L5.oilshock</i>	0.13 (0.09)	0.08 (0.09)	0.27 (0.10)**	0.18 (0.11)*	-0.11 (0.11)	-0.14 (0.11)
<i>oilshock_pres</i>	0.05 (0.06)	0.06 (0.07)	-0.10 (0.08)	-0.12 (0.09)	0.13 (0.05)*	0.16 (0.07)*
<i>L.oilshock_pres</i>	-0.22 (0.12)*	-0.19 (0.12)	-0.34 (0.11)**	-0.29 (0.13)*	0.12 (0.17)	0.12 (0.19)
<i>L2.oilshock_pres</i>	-0.10 (0.15)	-0.09 (0.14)	-0.19 (0.11)	-0.20 (0.12)	0.04 (0.11)	0.06 (0.10)
<i>L3.oilshock_pres</i>	0.00 (0.08)	0.02 (0.08)	0.12 (0.16)	0.14 (0.16)	-0.13 (0.17)	-0.10 (0.16)
<i>L4.oilshock_pres</i>	0.16 (0.14)	0.19 (0.13)	-0.12 (0.16)	-0.08 (0.16)	0.29 (0.23)	0.31 (0.22)
<i>L5.oilshock_pres</i>	-0.14 (0.09)	-0.09 (0.09)	-0.23 (0.09)**	-0.13 (0.10)	0.07 (0.11)	0.09 (0.11)
<i>L.tgrev</i>	-0.21 (0.11)	-0.22 (0.11)*				
<i>L.tgbal</i>			-0.32 (0.10)**	-0.33 (0.09)**		
<i>L.tgexp</i>					-0.19 (0.07)*	-0.19 (0.07)**
Estimation method	OLS	GMM	OLS	GMM	OLS	GMM
Observations	1014	965	1012	963	1020	972
Countries	64	64	64	64	64	64
R-squared	0.14		0.21		0.13	

*-Significant at the 5 percent level; **-significant at the 1 percent level. The numbers in parentheses are robust standard errors. All regressions are fixed-effects regressions, including a full set of country- and year indicators. Included controls not shown in table is *lgdpcap*. GMM refers to the Arellano- and Bond (1991) dynamic panel-data estimator.

Table 5b
Oil shock pass-through and forms of government. Baseline sample, regime types estimated separately.

	Parliamentary			Presidential		
	<i>tgrev</i>	<i>tgbal</i>	<i>tgexp</i>	<i>tgrev</i>	<i>tgbal</i>	<i>tgexp</i>
<i>oilshock</i>	0.01 (0.05)	0.24 (0.07)**	-0.22 (0.07)**	0.09 (0.04)*	0.18 (0.04)**	-0.07 (0.05)
<i>L.oilshock</i>	0.36 (0.10)**	0.46 (0.07)**	-0.12 (0.11)	0.26 (0.07)**	0.13 (0.07)	0.15 (0.03)**
<i>L2.oilshock</i>	-0.11 (0.14)	0.11 (0.10)	-0.17 (0.11)	-0.06 (0.01)**	-0.15 (0.02)**	0.07 (0.02)**
<i>L3.oilshock</i>	-0.09 (0.10)	-0.19 (0.16)	0.08 (0.18)	-0.10 (0.02)**	-0.15 (0.03)**	0.02 (0.03)
<i>L4.oilshock</i>	-0.17 (0.13)	0.19 (0.16)	-0.36 (0.22)	-0.06 (0.02)*	0.00 (0.03)	-0.07 (0.03)*
<i>L5.oilshock</i>	0.20 (0.12)	0.15 (0.09)	0.02 (0.13)	-0.01 (0.02)	0.05 (0.02)	-0.05 (0.02)**
<i>L.tgrev</i>	-0.02 (0.09)			-0.43 (0.13)**		
<i>L.tgbal</i>		-0.17 (0.08)*			-0.47 (0.10)**	
<i>L.tgexp</i>			-0.19 (0.09)*			-0.24 (0.09)**
Estimation method	OLS	OLS	OLS	OLS	OLS	OLS
Observations	641	639	639	390	390	398
Countries	36	36	36	30	30	30
R-squared	0.08	0.19	0.20	0.33	0.38	0.20

*-Significant at the 10 percent level; **-significant at the 5 percent level; ***-significant at the 1 percent level. The numbers in parentheses are robust standard errors. All regressions include the control *lgdpcap*. All regressions are fixed-effects regressions, including a full set of country- and year indicators.

Table 6a
Oil shock pass-through and electoral rules. Baseline model and baseline sample.

	<i>tgrev</i>		<i>tgbal</i>		<i>tgexp</i>	
<i>oilshock</i>	0.1 (0.03)**	0.1 (0.03)**	0.17 (0.02)**	0.16 (0.02)**	-0.08 (0.03)*	-0.08 (0.04)*
<i>L.oilshock</i>	0.21 (0.05)**	0.21 (0.05)**	0.12 (0.05)*	0.13 (0.05)**	0.11 (0.03)**	0.11 (0.03)**
<i>L2.oilshock</i>	-0.08 (0.01)**	-0.09 (0.01)**	-0.12 (0.02)**	-0.12 (0.02)**	0.03 -0.02	0.01 -0.03
<i>L3.oilshock</i>	-0.1 (0.02)**	-0.1 (0.01)**	-0.12 (0.03)**	-0.12 (0.03)**	0.01 -0.02	0 -0.03
<i>L4.oilshock</i>	-0.05 (0.02)**	-0.05 (0.01)**	0.04 -0.03	0.04 -0.03	-0.09 (0.03)**	-0.11 (0.04)**
<i>L5.oilshock</i>	-0.01 -0.02	-0.01 -0.01	0.04 (0.02)*	0.05 (0.02)*	-0.04 -0.03	-0.05 (0.02)**
<i>oilshock_maj</i>	0.03 -0.08	0.04 -0.09	0.19 -0.09	0.19 -0.12	-0.13 -0.12	-0.08 -0.13
<i>L.oilshock_maj</i>	0.41 (0.16)*	0.42 (0.15)**	0.18 -0.11	0.15 -0.13	0.23 -0.15	0.29 -0.18
<i>L2.oilshock_maj</i>	-0.14 -0.11	-0.1 -0.11	-0.02 -0.12	-0.03 -0.14	-0.12 -0.16	-0.13 -0.17
<i>L3.oilshock_maj</i>	-0.15 -0.14	-0.12 -0.13	-0.34 (0.11)**	-0.35 (0.12)**	0.18 -0.15	0.14 -0.2
<i>L4.oilshock_maj</i>	-0.21 (0.09)*	-0.18 -0.1	-0.19 -0.14	-0.19 -0.16	-0.04 -0.14	-0.06 -0.16
<i>L5.oilshock_maj</i>	0.12 -0.13	0.17 -0.14	0.01 -0.12	-0.01 -0.12	0.08 -0.12	0.02 -0.15
<i>L.tgrev</i>	-0.24 (0.12)*	-0.25 (0.11)*				
<i>L.tgbal</i>			-0.33 (0.09)**	-0.35 (0.09)**		
<i>L.tgexp</i>					-0.2 (0.07)**	-0.2 (0.07)**
Estimation method	OLS	GMM	OLS	GMM	OLS	GMM
Observations	975	922	973	920	981	929
Countries	63	63	63	63	63	63
R-squared	0.15		0.21		0.14	

*-Significant at the 5 percent level; **-significant at the 1 percent level. The numbers in parentheses are robust standard errors. All regressions are fixed-effects regressions, including a full set of year indicators. Included controls not shown in table are *lgdpcap* and the first lag of all of the endogenous variables. The GMM estimator is the Arellano- and Bond (1991) dynamic panel-data estimator.

Table 6b
Oil shock pass-through and electoral rules. Baseline sample, regime types estimated separately

	Proportional			Majoritarian		
	<i>tgrev</i>	<i>tgbal</i>	<i>tgexp</i>	<i>tgrev</i>	<i>tgbal</i>	<i>tgexp</i>
<i>oilshock</i>	0.11 (0.03)**	0.18 (0.02)**	-0.08 (0.03)*	0.00 (0.08)	0.32 (0.17)	-0.33 (0.18)
<i>L.oilshock</i>	0.20 (0.06)**	0.11 (0.06)	0.13 (0.03)**	0.51 (0.15)**	0.24 (0.10)*	0.26 (0.15)
<i>L2.oilshock</i>	-0.09 (0.01)**	-0.12 (0.02)**	0.02 (0.02)	-0.24 (0.20)	0.01 (0.12)	-0.23 (0.27)
<i>L3.oilshock</i>	-0.10 (0.01)**	-0.12 (0.03)**	0.00 (0.03)	0.06 (0.27)	-0.56 (0.14)**	0.62 (0.25)*
<i>L4.oilshock</i>	-0.05 (0.02)*	0.04 (0.03)	-0.10 (0.03)**	-0.28 (0.14)	-0.23 (0.26)	-0.03 (0.18)
<i>L5.oilshock</i>	-0.01 (0.02)	0.04 (0.02)	-0.04 (0.03)	0.01 (0.07)	-0.07 (0.17)	0.06 (0.14)
<i>L.tgrev</i>	-0.22 (0.14)			-0.24 (0.07)**		
<i>L.tgbal</i>		-0.34 (0.11)**			-0.25 (0.13)	
<i>L.tgexp</i>			-0.14 (0.06)*			-0.28 (0.12)*
Estimation method	OLS	OLS	OLS	OLS	OLS	OLS
Observations	719	717	725	298	298	298
Countries	46	46	46	19	19	19
R-squared	0.15	0.22	0.14	0.26	0.21	0.21

*-Significant at the 5 percent level; **-significant at the 1 percent level. The numbers in parentheses are robust standard errors. All regressions include the control *lgdpcap*. All regressions are fixed-effects regressions, including a full set of country- and year indicators.

Table 7
IV estimates: Effect of shocks in government revenues on government expenditures.

	All countries	<i>par1</i>	<i>pres</i>	<i>prop</i>	<i>maj</i>	<i>par1prop</i>	<i>par1maj</i>	<i>presprop</i>	<i>presmaj</i>
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<i>tgrev</i>	0.04 (0.25)	0.19 (0.36)	0.10 (0.22)	0.08 (0.23)	0.01 (0.61)	-0.12 (0.75)	0.06 (0.54)	0.01 (0.24)	0.90 (0.35)**
<i>L.tgrev</i>	0.57 (0.23)*	-0.40 (0.47)	0.61 (0.21)**	0.55 (0.20)**	-0.32 (0.69)	-0.37 (0.69)	-0.23 (0.74)	0.66 (0.23)**	0.08 (0.79)
<i>L2.tgrev</i>	0.21 (0.16)	0.00 (0.35)	0.27 (0.14)	0.20 (0.14)	0.66 (0.66)	-0.73 (0.54)	0.61 (0.64)	0.24 (0.16)	-0.20 (0.35)
$F(tgrev) [p]$	56.4 [0.00]	5.0 [0.00]	11.1 [0.00]	52.1 [0.00]	14.9 [0.00]	4.4 [0.00]	12.2 [0.00]	12.0 [0.00]	200.8 [0.00]
$F(L.tgrev) [p]$	157.6 [0.00]	11.4 [0.00]	136.6 [0.00]	115.0 [0.00]	3.8 [0.01]	11.4 [0.00]	20.0 [0.00]	121.1 [0.00]	5.8 [0.00]
$F(L2.tgrev) [p]$	90.7 [0.00]	1.9 [0.10]	53.0 [0.00]	80.3 [0.00]	5.5 [0.00]	5.1 [0.00]	3.7 [0.02]	42.1 [0.00]	273.8 [0.00]
Sargan [<i>p</i>]	[0.05]	[0.08]	[0.04]	[0.09]	[0.79]	[0.26]	[0.53]	[0.09]	[0.26]
Estimation m.	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Observations	1090	630	382	704	294	422	207	281	87
Countries	64	36	30	46	19	26	12	21	8

*-Significant at the 5 percent level; **-significant at the 1 percent level. The numbers in parentheses are robust standard errors. All regressions are fixed-effects regressions, including a full set of year indicators. Contemporaneous and two lags of total government revenues, *tgrev*, are instrumented using the following instruments: *oilshock*, *L.oilshock*, *L2.oilshock*, *L3.oilshock*, *L4.oilshock*, *L5.oilshock*, a full set of country specific- and time specific effects, and finally *lgdpcap* and *L.tgexp*. The resulting five 1. stage regressions belonging to each IV-regression are not shown in the table. F-test refer to the test of joint significance of all the instruments in each 1. stage regression, with corresponding *p*-values in brackets. The Sargan-statistics refer to the Sargan (1958) test of the overidentifying restrictions, where only the *p*-values are reported.

Appendix

A1 Variable definitions

Constitutional indicator variables:

pres: Dummy variable for forms of government, equal to 1 in presidential regimes, 0 otherwise. Regimes in which the confidence of the assembly is not necessary for the executive to stay in power, even if an elected president is not chief executive or if there is no elected president, are classified as presidential. Most semipresidential and premier-presidential systems are classified as parliamentary (see chapter 4 in PT (2003) for further discussion). Sources: PT (2003) and the World Bank Database on Political Institutions (DPI).

maj: Dummy variable for electoral systems, equal to 1 if all the lower house in a country is elected under plurality rule, 0 otherwise. Only legislative elections are considered (see chapter 4 in PT (2003) for clarification). Sources: PT (2003) and the World Bank Database on Political Institutions (DPI).

Fiscal policy variables:

CGEXP: Central government expenditures as a percentage of GDP at current prices, constructed using the item Government Finance–Expenditures in the IFS. Sources: PT (2003) and the IMF-IFS CD-ROM.

cgezp: The first difference of *CGEXP*.

TGEXP: Total government expenditure, taken from the IFS. Source: Brender and Drazen (2005).

tgexp: The first difference of *TGEXP*.

CGREV: Central government revenues as a percentage of GDP at current prices, constructed using the item Government Finance–Revenues in the IFS. Sources: PT (2003) and the IMF-IFS CD-ROM.

cgrez: The first difference of *CGREV*.

TGREV: Total revenue and grants, calculated as Revenue plus Grants from the IFS dataset. Source: Brender and Drazen (2005).

tgrev: The first difference of *TGREV*.

CGBAL: Central government budget surplus (if positive) or deficit (if negative) as a percentage of GDP at current prices. Sources: PT (2003).

cgbal: The first difference of *CGBAL*.

TGBAL: Total government budget balance, calculated as the difference between total revenue and grants (*tgrev*) and total expenditure (*tgexp*). Source: Brender and Drazen (2005).

tgbal: The first difference of *TGBAL*.

CGSSW: Central government expenditures consolidated on social services and welfare as a percentage of GDP at current prices. Sources: PT (2003).

cgssw: The first difference of *CGSSW*.

TGCONS: General government final consumption expenditure as a percentage of GDP at current prices. The variable includes all government current expenditures for purchases of goods and services (including compensation of employees). It also includes most expenditures on national defense and security, but excludes government military expenditures that are part of government capital formation. Source: The World Bank's World Development Indicators database.

tgcons: The first difference of *TGCONS*.

Oil variables:

oil_gdp: The value of oil extraction (calculated as the product of oil production volume and the crude oil prices net of average unit extraction costs) as a percentage of GDP at current prices. Sources: Oil production volume and crude oil price are from the World Bank's dataset on genuine savings (adjusted net savings). GDP in current USD are from World Development Indicators.

oilshock: $oilshock_{it} = \frac{oilprice_t - oilprice_{t-1}}{oilprice_{t-1}} oil_gdp_{it-1}$.

oilvolshock: $oilvolshock_{it} = \frac{oilprice_t - oilprice_{t-1}}{oilprice_{t-1}} \frac{oil_vol_{it-1} * oilprice_{t-1}}{GDP}$, where *oil_vol_{it-1}* is the volume of production, or depletion, (in tonnes) of oil in country *i* in year *t* - 1, and *GDP* is measured in constant U.S. dollars, base year 2000. Source: The World Bank's dataset on genuine savings (adjusted net savings) and the World Bank's World Development Indicators.

oilshock: Defined in Section 4.

oilprice: The crude oil annual average world price, \$/ton. Source: The World Bank's dataset on genuine savings (adjusted net savings).

Control variables:

GDP_CAPppp: Real PPP adjusted GDP per capita in constant U.S. dollars (in thousands), base year 2000. Source: The World Bank's World Development Indicators database.

GDP_CAP: Real PPP adjusted GDP per capita in constant U.S. dollars (in thousands), base year 2000. Source: The World Bank's World Development Indicators database.

lgdpcap: First difference of the natural logarithm of real GDP per capita in constant U.S. dollars, base year 2000. Source: The World Bank's World Development Indicators database.

d_gdp: The relative change in GDP per capita in constant U.S. dollars, base year 2000. Source: The World Bank's World Development Indicators database.

democracy: Interpolated version of *polity* and *gastil*. Computed as the forecasted value obtained by regressing the rescaled values of *polity* on *gastil*, and normalized to lie between 0 and 1, where 0 is the lowest possible score and 1 is the highest. Here, *polity* refer to the Polity IV Project <<http://www.cidcm.umd.edu/inscr/polity/index.htm>> variable *POLITY2*, while

gastil is an average of indecies for civil liberties and political rights provided by the Freedom House, “Annual Survey of Freedom Country Ratings”.

prop_1564: Percentage of a country’s population between 15 and 64 years old in the total population. Source: The World Banks World Development Indicators database.

prop_65: First difference of the percentage of the population over the age of 65 in the total population. Source: The World Banks World Development Indicators database.

trade_gdp: First difference of the sum of exports and imports of goods and services in GDP at current prices. Source: The World Banks World Development Indicators database.

gdpgap: First difference of the deviation of aggregate output from its trend value in percent, computed as the difference between the natural logarithm of real GDP in a country and its country-specific trend (obtained using the Hodrick-Prescott filter). Source for GDP: The World Banks World Development Indicators database.

A2 Robustness

A2.1 Model properties

This section investigates to what degree the estimates rely on the specification of the model. First, it is not *a priori* clear how many lags, including the contemporaneous value, of the *oilshock* variable that should be included in the regression model; the value of s in equation (1) is a feature of the data. Second, it is an *a priori* open empirical question whether including a number of lagged endogenous variables, $\sum_{j=1}^p y_{it-j}$, introduces a source of mechanical endogeneity that will bias the estimated parameters. Since I am primarily interested in estimating consistent values of γ_{t-s}^0 and γ_{t-s}^1 , I perform a series of regressions without the constitutional dummies S_i to check to what degree the estimates are sensitive to the model specification.

In Table A1, consider the effects of the successive inclusion and exclusion of the different lags of the *oilshock* variable. Note that the effect of every individual shock, $s = 1, 2, 3, 4, 5$, is very stable and insensitive to the number and the identity of the other included lags. This property of model stability is desirable, and goes for the rest of the regressands as well. Stability is an indication of correct model specification, and reflects that the *oilshock* variable is not statistically autocorrelated. Given that the different lags appear to have independent and stable effects on the fiscal policy measures, it seems reasonable to use all five lags whenever possible. However, it is interesting to note that the estimates in Table A1 suggest that dropping individual lags should not cause significant biases in the estimation of the effects of the remaining lags.

Comparing column (9) with the other columns in the table suggests that the number of lags of the dependent variable has only minor effects for the estimated coefficients of *oilshock*—if anything, including an extra lag of the dependent variable implies a stronger estimated effect on government expenditures of the *oilshock* variable. Because employing only one lag of y_{it} appears sufficient to achieve unbiased estimates of the shock variable, this is the baseline specification throughout the analysis.

One final thing to note about the model—which is not shown in the table but possibly vital for achieving unbiased estimates of the effect of *oilshock*—is that the included year dummies indeed captures the common and unidentified fiscal effects of the oil price shocks. This is particularly important in the present context, as the proposed proxy *oilshock* by design is correlated with international business cycles caused by the oil shocks. And indeed, the time dummies do capture the oil price shocks. For example, the strongest common fiscal expansions in the whole sample period, as estimated by the time dummies, follow in the wake of the two OPEC induced oil price hikes in 1973-1974 and in 1979-1980.²⁵ The fact that the sample includes also countries-/years without oil production—hence where the value of *oilshock* equals zero—contributes to the identification of the effects of the common and unidentified shocks.

A2.2 Sample selection and outliers

As discussed in Section 3.1, there is reason to worry that resource abundance may interfere with processes of democratization, bad governance and corruption. For the inference to be valid, it is important that the constitutional rules are in fact being followed. Because political agents are more likely to play by the rules in countries with a democracy score, I check whether the results holds when employing an even stricter democracy threshold. Tables A2 and A3 reproduce the results of Tables 5b and 6b, respectively, but now the threshold for being allowed into the sample has been increased to a score of 0.9, as opposed to the previous threshold of 0.5, on the democracy index, *democracy*.²⁶ Consequently, the sample is reduced from 36 to 32 parliamentary countries, from 20 to 16 presidential countries, from 46 to 34 proportional countries and from 19 to 14 majoritarian countries. Tables A2 and A3 show that all of the main results remain.

Another potential worry is that the results are driven by the two OPEC members in the sample, Venezuela and Ecuador, since these countries may affect the world market price of oil via their say in the output decisions of the OPEC cartel. I find, however, that excluding these countries has no significant effects on any of the main estimates (regressions are not shown). Moreover, the results are also robust to the exclusion of the final large country in the sample which may be said to have a significant influence on the world market price of oil, namely the U.S.. Finally, the results are robust to excluding all of the three countries Venezuela, Ecuador and the U.S. simultaneously.

Among the parliamentary countries, there is only one clear outlier, Trinidad and Tobago. In the wake of the oil boom in the late '70s, the oil abundant Caribbean country experienced an extraordinary growth in the size of the public sector in general, and in the government expenditures in particular, which is unparalleled by any country in the sample, regardless of

²⁵The estimates of the effect of the time dummies, which are statistically significant at conventional levels, indicate that the average country in the sample expanded its government expenditures by one to two percent of GDP during the period 1975-76, and then again in 1980-82. Note that these effects are net of the effect of *oilshock*—which is included in the regression—and thus net of the effect of the oil price hikes that worked via the government revenues of the oil producers. Hence, the time dummies captures the countercyclical fiscal policies induced by the adverse supply side shocks of the oil price shocks.

²⁶This threshold corresponds to a *polity*-score of about 6 and a score on the Gastil-index of about 2.

level of oil abundance or constitutional classification. From the early to the late 1970s, total government revenues in Trinidad and Tobago more than doubled, from some 20 percent of GDP, to more than 40 percent of GDP. As a delayed response to the increase in revenues from the oil sector, government consumption expenditures increased by more than eight percent of GDP from 1981 to 1982, while total government expenditures increased by the incredible amount of 18 percent of GDP in the same period. While the extraordinary experiences of Trinidad and Tobago certainly is a case worth studying, they are hardly representative for any of the other countries or political systems in the sample.²⁷ Because of its status as a clear outlier, Trinidad and Tobago has been excluded entirely from the analyses, and is hence not part of the baseline sample. Including Trinidad and Tobago has only minor, and no substantial, effects on the average estimates in Table 4. Moreover, being parliamentary, the country does not contribute to the estimates of the effects among the presidential regime. However, including Trinidad and Tobago generates a statistically significant and strong effect of the three-year lagged *oilshock* on government expenditures among the parliamentary countries (note that 1981-82 is exactly three years after the latest of the oil shocks, which took place in the period 1978-79). Tables 5a and 5b, together with Table A2, clearly shows that such an effect is not representative for the sample of parliamentary countries as such.²⁸

A2.3 Employing different fiscal policy measures and fiscal policy outcomes

The regressions in Table A5 replicates Table 4 using alternative measures and definitions of fiscal policies. First, note that the effects are almost identical when employing the measure of central government expenditures instead of the total government expenditures variables, even though the sample size is reduced by some 15 countries. If anything, the effects are somewhat stronger. Second, note that although the lagged effects of the shock variables are always associated with significant effects on governments expenditures, it is not clear that similar effects should be found with regard to various subcomponents of fiscal policy. One such subcomponent is social security- and welfare spending, which usually is associated with well defined social benefit programs that are planned long ahead and hence are less susceptible to being affected in the short run by shifts in government revenues. The estimates in the fourth column of Table A5 confirms this intuition, and show no significant short-term effects of the shocks changes in social security- and welfare spending, *ssw*. The final column of Table A5 shows that there are no significant effects of the shocks on total government consumption expenditure, *tgcons*, either. These results are consistent with the notion that the estimated effects throughout are generated by variations in the data, rather than by constructs of the model.

²⁷ Venezuela is the country that comes closest to Trinidad and Tobago in the largest single-period fiscal expansion in the sample, with an increase in total government expenditures from 1980-81 of about seven percent of GDP.

²⁸ A final potential outlier among the most oil abundant parliamentary countries is Papua New Guinea. Papua New Guinea, however, has no effect on the results for the parliamentary countries and is hence part of the baseline sample.

A2.4 Production volumes as weights in the oil shock proxy

One potential concern about the proposed oil shock proxy *oilshock*, is that the effect of the unit costs of oil extraction is somewhat unclear. In Table A6, *oilshock* is replaced by the variable *oilvolshock*, defined as

$$oilvolshock_{it} = \frac{oilprice_t - oilprice_{t-1}}{oilprice_{t-1}} \frac{oil_vol_{it-1} * oilprice_{t-1}}{GDP}$$

The estimates in Table A6 display the same pattern as before, implying that all the main results go through even when not taking into account the different costs of production in the different countries, and at the different points in time.

A3 Appendix tables

Table A1

Experimenting with the number of lags of *oilshock*. LHS variable: *tgexp*.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<i>oilshock</i>	-0.05 (0.02)**	-0.06 (0.02)**	-0.05 (0.02)**	-0.06 (0.02)**	-0.11 (0.05)*				-0.08 (0.03)**
<i>L.oilshock</i>		0.07 (0.01)**	0.07 (0.01)**	0.07 (0.01)**	0.08 (0.01)**	0.06 (0.01)**			0.12 (0.02)**
<i>L2.oilshock</i>			0.02 (0.02)	0.02 (0.02)	0.02 (0.02)	0.03 (0.02)			0.02 (0.03)
<i>L3.oilshock</i>				0.02 (0.03)	0.03 (0.03)		0.02 (0.03)		0.02 (0.02)
<i>L4.oilshock</i>					-0.10 (0.02)**		-0.09 (0.03)**		-0.09 (0.03)**
<i>L5.oilshock</i>								-0.08 (0.03)*	-0.04 (0.03)
<i>L.tgexp</i>	-0.17 (0.07)*	-0.17 (0.07)*	-0.17 (0.07)*	-0.17 (0.07)*	-0.18 (0.08)*	-0.17 (0.07)*	-0.17 (0.08)*	-0.19 (0.07)*	-0.20 (0.09)*
<i>L2.tgexp</i>									-0.08 (0.06)
Observations	1162	1141	1118	1094	1069	1118	1069	1040	1023
Countries	64	64	64	64	64	64	64	64	64
R-squared	0.12	0.12	0.12	0.12	0.13	0.12	0.12	0.11	0.12

*-Significant at the 5 percent level; **-significant at the 1 percent level. The numbers below the estimated coefficients are robust standard errors. All regressions include *gdppcap* and a constant term. All regressions are fixed-effects regressions, including a full set of country- and year indicators.

Table A2
Forms of government. Strictly democratic sample ($democracy > 9 \sim polity > 9$).

	Parliamentary			Presidential		
	<i>tgrev</i>	<i>tgbal</i>	<i>tgexp</i>	<i>tgrev</i>	<i>tgbal</i>	<i>tgexp</i>
<i>oilshock</i>	0.00 (0.05)	0.18 (0.07)*	-0.17 (0.07)*	0.09 (0.04)	0.14 (0.04)**	-0.05 (0.07)
<i>L.oilshock</i>	0.36 (0.10)**	0.49 (0.07)**	-0.15 (0.11)	0.18 (0.06)*	0.04 (0.05)	0.14 (0.03)**
<i>L2.oilshock</i>	-0.13 (0.14)	0.07 (0.10)	-0.19 (0.12)	-0.07 (0.01)**	-0.10 (0.01)**	0.04 (0.02)
<i>L3.oilshock</i>	-0.10 (0.10)	-0.19 (0.17)	0.09 (0.19)	-0.07 (0.01)**	-0.10 (0.02)**	0.03 (0.02)
<i>L4.oilshock</i>	-0.15 (0.13)	0.19 (0.17)	-0.34 (0.23)	-0.05 (0.02)*	0.01 (0.02)	-0.06 (0.02)*
<i>L5.oilshock</i>	0.20 (0.12)	0.18 (0.10)	0.00 (0.13)	-0.01 (0.02)	0.04 (0.02)*	-0.05 (0.01)**
<i>L.tgrev</i>	-0.01 (0.10)			-0.06 (0.07)		
<i>L.tgbal</i>		-0.07 (0.05)			-0.03 (0.09)	
<i>L.tgexp</i>			-0.10 (0.06)			-0.09 (0.08)
Estimation method	OLS	OLS	OLS	OLS	OLS	OLS
Observations	638	636	636	191	191	193
Countries	32	32	32	16	16	16
R-squared	0.09	0.21	0.22	0.31	0.34	0.28

*.Significant at the 5 percent level; **.significant at the 1 percent level. The numbers in parentheses are robust standard errors. All regressions include *lgtcap* and a constant term. All regressions are fixed-effects regressions, including a full set of country- and year indicators.

Table A3
Electoral rules. Strictly democratic sample (*democracy* > 9 ~ *polity* > 9).

	Proportional			Majoritarian		
	<i>tgrev</i>	<i>tgbal</i>	<i>tgexp</i>	<i>tgrev</i>	<i>tgbal</i>	<i>tgexp</i>
<i>oilshock</i>	0.08 (0.02)**	0.14 (0.04)**	-0.07 (0.06)	0.08 (0.09)	0.18 (0.12)	-0.12 (0.12)
<i>L.oilshock</i>	0.16 (0.06)**	0.07 (0.04)	0.09 (0.03)**	0.42 (0.15)*	0.21 (0.16)	0.31 (0.18)
<i>L2.oilshock</i>	-0.08 (0.01)**	-0.10 (0.01)**	0.02 (0.02)	-0.31 (0.25)	0.08 (0.15)	-0.51 (0.30)
<i>L3.oilshock</i>	-0.07 (0.01)**	-0.10 (0.03)**	0.02 (0.03)	-0.03 (0.28)	-0.62 (0.19)**	0.72 (0.34)
<i>L4.oilshock</i>	-0.04 (0.01)**	0.05 (0.02)*	-0.09 (0.02)**	-0.20 (0.12)	-0.15 (0.32)	-0.15 (0.18)
<i>L5.oilshock</i>	0.00 (0.01)	0.05 (0.02)*	-0.05 (0.02)*	0.06 (0.06)	0.14 (0.18)	0.01 (0.11)
<i>L.tgrev</i>	0.05 (0.07)			-0.27 (0.12)*		
<i>L.tgbal</i>		-0.07 (0.05)			-0.01 (0.08)	
<i>L.tgexp</i>			-0.10 (0.05)			0.10 (0.12)
Estimation method	OLS	OLS	OLS	OLS	OLS	OLS
Observations	524	522	524	200	200	200
Countries	34	34	34	14	14	14
R-squared	0.13	0.17	0.18	0.32	0.33	0.32

*-Significant at the 5 percent level; **-significant at the 1 percent level. The numbers in parentheses are robust standard errors. All regressions include *lgtgcap* and a constant term. All regressions are fixed-effects regressions, including a full set of year indicators.

Table A4
IV estimates: First 1.stage of column (1) in Table A5

	<i>tgrev</i>	<i>tgrev</i>	<i>tgrev</i>
<i>oilshock</i>	0.13 (0.02)**	0.10 (0.03)**	0.10 (0.03)**
<i>L.oilshock</i>	0.18 (0.05)**	0.21 (0.05)**	0.22 (0.05)**
<i>L2.oilshock</i>	-0.09 (0.01)**	-0.09 (0.01)**	-0.09 (0.01)**
<i>L3.oilshock</i>	-0.08 (0.02)**	-0.09 (0.01)**	-0.10 (0.02)**
<i>L4.oilshock</i>	-0.04 (0.02)	-0.04 (0.02)**	-0.05 (0.02)**
<i>L5.oilshock</i>	-0.01 (0.03)	-0.01 (0.02)	-0.01 (0.02)
<i>L.tgexp</i>	0.00 (0.08)		0.05 (0.09)
<i>L.tgrev</i>		-0.20 (0.10)*	-0.23 (0.13)
F-test [<i>p</i>]	56.4 [0.00]		
Estimation method	OLS	GMM	GMM
Observations	1034	985	984
Countries	64	64	64
R-squared	0.08		

*-Significant at the 5 percent level; **-significant at the 1 percent level. The numbers in parentheses are robust standard errors. All regressions are fixed-effects regressions, including a full set of country- and year indicators, and the control *lgdpcap*. The F-test tests the joint significance of all the excluded instruments (contemporaneous and all five lags of *oilshock*). The GMM estimator is the Arellano- and Bond (1991) dynamic panel-data estimator.

Table A5
Effects on other dimensions of fiscal policy

	<i>cgrev</i>	<i>cgbal</i>	<i>cgexp</i>	<i>cgssw</i>	<i>tgcons</i>
<i>oilshock</i>	0.01 (0.04)	0.20 (0.03)**	-0.16 (0.05)**	-0.02 (0.02)	-0.11 (0.02)**
<i>L.oilshock</i>	0.24 (0.05)**	0.08 (0.04)	0.16 (0.03)**	0.02 (0.01)	0.02 (0.03)
<i>L2.oilshock</i>	-0.07 (0.02)**	-0.08 (0.02)**	0.01 (0.01)	0.00 (0.00)	0.00 (0.02)
<i>L3.oilshock</i>	-0.12 (0.03)**	-0.08 (0.02)**	0.02 (0.04)	-0.01 (0.01)	0.00 (0.01)
<i>L4.oilshock</i>	-0.07 (0.02)**	0.07 (0.02)**	-0.13 (0.04)**	-0.01 (0.01)	-0.03 (0.02)
<i>L5.oilshock</i>	0.03 (0.02)	0.04 (0.03)	0.06 (0.04)	0.01 (0.01)	0.02 (0.01)
<i>L.cgrev</i>	-0.22 (0.06)*				
<i>L.cgbal</i>		-0.29 (0.12)*			
<i>L.cgexp</i>			-0.16 (0.04)**		
<i>L.cgssw</i>				0.14 (0.08)	
<i>L.tgcons</i>					-0.35 (0.06)**
Estimation method	OLS	OLS	OLS	OLS	OLS
Observations	779	779	854	657	891
Countries	47	49	50	46	50
R-squared	0.13	0.18	0.12	0.17	0.17

*-Significant at the 5 percent level; **-significant at the 1 percent level. The numbers in parentheses are robust standard errors. All regressions include the controls *lgdpcap*, *trade_gdp*, *prop_1564*, *prop_65* and *gdpgap*. All regressions are fixed-effects regressions, including a full set of country- and year indicators

Table A6
Oil shocks using lagged production volumes as country weights

	All countries			Parliamentary	Presidential	Majoritarian	Proportional
	<i>tgrev</i>	<i>tgbal</i>	<i>tgexp</i>	<i>tgexp</i>	<i>tgexp</i>	<i>tgexp</i>	<i>tgexp</i>
<i>oilvolshock</i>	9.71 (2.77)**	18.28 (2.59)**	-9.39 (3.72)*	-32.19 (9.73)**	-6.30 (4.67)	-8.17 (3.72)*	-27.56 (16.18)
<i>L.oilvolshock</i>	21.92 (5.03)**	11.83 (4.72)*	12.76 (2.38)**	-16.86 (11.47)	14.07 (3.27)**	12.34 (2.74)**	29.33 (17.39)
<i>L2.oilvolshock</i>	-8.60 (1.14)**	-10.91 (1.75)**	1.95 (2.21)	-16.33 (13.75)	7.18 (2.26)**	1.59 (1.88)	-21.17 (32.68)
<i>L3.oilvolshock</i>	-9.01 (1.50)**	-11.87 (2.81)**	1.47 (2.37)	1.34 (18.35)	2.18 (2.67)	-0.08 (2.54)	59.43 (29.86)
<i>L4.oilvolshock</i>	-4.90 (1.94)*	4.14 (2.48)	-9.84 (2.90)**	-43.03 (22.95)	-6.97 (3.05)*	-9.78 (2.77)**	-13.98 (21.27)
<i>L5.oilvolshock</i>	-0.64 (2.13)	3.86 (2.11)	-3.50 (2.84)	1.84 (12.37)	-6.00 (1.86)**	-3.56 (2.86)	-0.16 (13.16)
<i>L.tgrev</i>	-0.20 (0.10)						
<i>L.tgbal</i>		-0.32 (0.09)**					
<i>L.tgexp</i>			-0.19 (0.07)*	-0.20 (0.09)*	-0.25 (0.09)**	-0.14 (0.06)*	-0.29 (0.13)*
Est. method	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Observations	1009	1007	1015	628	384	720	278
Countries	63	63	63	36	29	46	17
R-squared	0.13	0.21	0.13	0.2	0.21	0.14	0.21

*-Significant at the 5 percent level; **-significant at the 1 percent level. The numbers in parentheses are robust standard errors. All regressions include the controls *lgdpcap*, *trade_gdp*, *prop_1564*, *prop_65* and *gdpgap*. All regressions are fixed-effects regressions, including a full set of country- and year indicators