# *The form of government and fiscal dynamics*

**Jørgen Juel Andersen**
**BI Norwegian Business School**

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# The form of government and fiscal dynamics

Jørgen Juel Andersen*
*Norwegian School of Management (BI), Nydalsveien 37, 0484 Oslo.*

**Abstract**

Using a combination of time-series variation in oil prices and cross-section variation in the oil intensity of countries, this paper investigates whether exogenous shifts in the government revenues affect the government expenditures differently depending on the political institutions of the form of government. Comparing the fiscal policy dynamics in parliamentary and presidential systems, a main finding is that the government expenditures appear more responsive to shifts in the revenues when the form of government is presidential.

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*Phone: +47 46410268. E-mail: jorgen.j.andersen@bi.no.

"*If men were angels, no government would be necessary. If angels were to govern men, neither external nor internal controls on government would be necessary. In framing a government which is to be administered by men over men, the great difficulty lies in this: you must first enable the government to control the governed; and in the next place oblige it to control itself.*"

- James Madison, the Federalist No.51 (*Independent Journal*, Feb.6, 1788)

# 1 Introduction

The insight that the form of government matters for policy outcomes is not new, and the literature indeed documents a strong correlation between the form of government and the size and composition of government budgets.[1] In particular, the levels of government taxes and expenditures are significantly higher when the form of government is parliamentary, as opposed to presidential. The mechanisms are, however, not well understood. Moreover, understanding the effects of the form of government for policy dynamics appear key. There is now mounting evidence that fiscal policy is more volatile in presidential systems.[2] No existing theories can, however, explain these empirical regularities,[3] and the few empirical contributions that exist on the dynamic effects of the form of government suffer from different problems of identification.[4]

This paper employs an unbalanced panel of 63 democratic countries, in the period 1970 to 2001, to identify whether the fiscal effects of exogenous budget shocks vary systematically with the form of government. Empirical analysis of fiscal policy is in general demanding, mainly because of the strong degree of endogeneity in policy determination; clearly, regressing the government expenditures on the government revenues would not be informative. The first contribution of the paper is to propose a novel strategy to identify how changes in the government revenues affect the path of contemporaneous and future government expenditures. In particular, oil prices are interacted with predetermined and/or exogenous measures of oil production intensity in the countries to generate a proxy for the windfall revenues from oil production. It is demonstrated that the proxy is a strong predictor for current and future changes in the government revenues, and the estimates suggest that the effective marginal tax rates on the gross value of the oil windfalls are in the range of 30-40 percent. Importantly, this revenue effect does not vary

[1] For a broad overview of this research program, see Persson and Tabellini ($2004_a$, $2004_b$). Persson and Tabellini (2003) provide an empirical investigation of the effects of the constitutional features of the form of government and the electoral rules, while Persson and Tabellini ($2000_a$) summarize the theoretical literature. Acemoglu (2005) offers a critical review of the constitutions literature in general, and of Persson and Tabellini (2003) in particular.

[2] See, e.g., Fatás and Mihov (2005).

[3] Persson, Roland and Tabellini (1997, 2000) analyze the fiscal effects of the form of government, but their theory is static and does not contribute to the understanding of fiscal dynamics. The theory in the literature that most explicitly addresses the dynamic effects of constitutional rules, is perhaps the Holmstrõm (1999) style career-concern model by Persson and Tabellini ($2000_a$, Ch. 9). In addition, Persson and Tabellini (2003) offer a discussion on how some of the static models in the literature may be given dynamic interpretations.

[4] Persson (2002) and Persson and Tabellini (2003) estimate how 'unobserved common events' (i.e., the time dummies) affect fiscal policy, and the effects appear stronger if the form of government is parliamentary. The empirical strategy does however not account for country-specific fiscal policy trends. Moreover, the unobserved nature of the identifying 'events' does not facilitate further theorizing. A similar empirical approach is employed by Milesi-Feretti et al. (2002). Their shock variable (unemployment) is however endogenous, and, thus, their estimates might be biased.

significantly with the form of government. The second contribution is to employ the proposed strategy to investigate whether the fiscal responses to the exogenous government revenue shocks are systematically correlated with the form of government.[5] A main result is that the exogenous government revenue shocks appear to have strong effects for the government expenditures if the form of government is presidential, while there are, at most, weak and unsystematic effects on the government expenditures when the form of government is parliamentary. These results are consistent with the empirical patterns previously documented in the literature (see, e.g., Fatás and Mihov, 2005) of a higher degree of fiscal volatility among presidential systems as compared with parliamentary.

There are several advantages associated with the proposed empirical strategy. First, as the oil intensity measures are predetermined (or, when utilizing proven oil reserves, possibly exogenous) and highly persistent, while the real oil prices are highly volatile, the main variation in the windfall proxy variable is driven by the oil price innovations. In turn, the oil price shocks are exogenous to the economic- and political environment in most economies, which sharply reduces the potential of estimation biases due to simultaneity and reverse causation.[6] Second, the time series properties of annual oil prices validates the interpretation of oil price innovations as permanent, rather than transitory, shifts. Third, because the oil price shocks are highly observable, their effects on the governments' budgets may be identified, given that any additional effects of the oil price shocks are properly accounted for. In the estimations, other unidentified fiscal effects of the oil price shocks are accounted for by including either a full set of time dummies, or, alternatively, the oil price innovations in themselves (i.e., the percentage change in the oil price). Finally, there is a significant amount of variation in the political institutions among the oil producers, which facilitates a meaningful statistical comparison of the fiscal effects of oil price shocks across regime types.

Although the literature offers no fully coherent explanation, or theory, for the main result in the present paper, (at least) two potential interpretations are available. First, the so-called veto player theory (e.g., Tsebelis, 1995, 1999, 2002) associates different political systems with different capacities to produce policy change. Tsebelis' theory distinguishes between institutional and partisan veto players, where the former is more prevalent in presidential systems (e.g., the president and the chambers), while the latter typically plays a larger role in parliamentary systems (e.g., the role of parties in the government). The potential for policy change is decreasing

[5] The empirical strategy in the present paper is similar to the strategy in Acemoglu et al. (2009), who utilize oil price shocks to investigate the income effects on health expenditures in Economic Sub Regions (ESRs) in the U.S.. Also Persson and Tabellini ($2000_b$) and Persson (2002) make an effort to exploit oil prices to identify comparative effects of shocks, but their strategy is incapable of distinguishing between the effects of the oil price innovations and other, correlated shocks.

[6] Exceptions are the member countries of the OPEC cartel, and the U.S., of which all certainly are in a position to affect the price of oil. See Wirl (2009) for a review of the potential political determinants of the price of oil, and for an analysis of the role of OPEC in particular. Most OPEC memebers a considered nondemocratic and are hence excluded from the current sample throughout. However, all the main results go through also when excluding the remaining OPEC-members which occasionally are classified as democratic, as well as excluding the U.S..

in the number of veto players.[7] To investigate whether the main result of the paper is driven by veto player mechanisms, or some other mechanism that is systematically correlated with the form of government, two measures of the political constraints on the executive and the prevalence of coalition governments, respectively, are successively included in the regressions. In addition, a measure of the democratic quality is included, as constitutional rules are likely to be of greater importance in more advanced democracies.

The inclusion of alternative institutional measures in the regressions provides some support for the mechanisms of institutional and partisan veto players, as suggested by the theory of Tsebelis; more political constraints are, if anything, associated with weaker effects of the windfalls on the government expenditures. The electoral system, however, does not seem to matter.[8] Importantly, including these variables does not shut down or even weaken the separate effect of presidentialism, which is still strong and statistically significant. This is an indication that there might be more to the comparative politics of the form of government—in relation to policy dynamics—than can be explained by the veto player theory. One alternative, simple, and intuitive mechanism, suggested by Torvik (2009, p.247), is that while: "presidentialism may be more of a 'one man show' that can be captured by special interests, parliamentary regimes with their continuous vote of confidence and broader representation in the making of policy, may be better suited to putting proceeds from resources into productive use." It is not clear, in the current context, what is more 'productive'—spending or saving the windfall revenues—but Torvik's argument about the differences between presidentialism and parliamentarism is nonetheless somewhat broader and more encompassing than what is captured by the veto player framework of Tsebelis. The results in the present analysis, and the remark by Torvik (2009), clearly indicate that several of the political mechanisms associated with the effects of the form of government are not well understood and require further investigation. The proposed methodology, employing 'natural experiments' to investigate the comparative effects of the form of government, and the results from the analysis in the current paper, are contributions in the greater project of expanding our knowledge of the economic effects of political institutions in general, and of the form of government in particular.

In addition to investigating the role of veto players, several additional robustness checks are performed. The main results appear robust to different model specifications and operationalizations of the oil windfalls, to the intensity of oil production and the general level of economic development, to IV- and GMM estimation methods, and to symmetry issues (in particular, positive and negative realizations of the oil windfalls are shown to have similar effects).

Besides constituting a stepping stone for theorizing, the present results may have even more important and far-reaching economic implications. There is now a growing literature, initiated by the contribution of Ramey and Ramey (1995), which documents negative effects of output

[7] Note that an additional mechanism in Tsebelis' theory is that the ideological distance between the veto players matters; the larger the distance, the more likely is new legislation to be biased towards the status quo.

[8] The pattern that coalition governments are more common under proportional electoral rules (Persson et al. $2004_c$), implies that coalition government should be less frequent in presidential systems because of the positive correlation between presidentialism and majoritarian electoral rules (see, e.g., Müller, 2008)

volatility on economic growth. In addition, as emphasized by van der Ploeg and Poelhekke ($2009_{a,b}$), among others, fiscal policy volatility is an important source of volatility in output. The results presented in the current paper hence suggest that the form of government may be decisive for the long term effects of exogenous budget shocks. The present results may thus shed some light on why natural resource abundance, and in particular oil, appear to harm growth if the form of government is presidential, but not if it is parliamentary (Andersen and Aslaksen, 2008): Government revenue volatility appear to generate stronger expenditure volatility when the form of government is presidential.

# 2 Data

*2.1 Main variables*

Data on the total government revenues, $TGREV_{it}$, and expenditures, $TGEXP_{it}$, are mainly borrowed from a study by Brender and Drazen (2005). Both measures are defined as ratios to GDP. Because the identifying windfall variable (to be defined in Section 2.3) is likely to affect the contemporaneous level of GDP, the time differenced fiscal policy variables ($\Delta TGREV_{it}$ and $\Delta TGEXP_{it}$) are defined in terms of GDP in time $t-1$ rather than in time $t$, to reduce endogeneity in these variables. Second, the study relies on data on the level of oil production, $OIL\_PROD_{it}$, and on oil prices (measured in 2000 USD), $OIL\_PRICE_t$, which are collected from the WDI and the World Bank's Adjusted Net Savings dataset (sometimes referred to as the genuine savings dataset). The analysis also makes use of data on the size of the proven petroleum reserves, $OIL\_RESERVES_{it}$, provided by the Energy Information Administration (EIA). Third, countries are assigned constitutional classifications in accordance with the literature. Using the definitions of Persson and Tabellini (2003), as well as data from the World Bank's Dataset on Political Institutions (DPI), countries are assigned indicator variables dependent on the specific constitutional features they are recognized by. In particular, years in which a government is not subject to a confidence requirement are coded as $PRES_{it} = 1$, otherwise $PRES_{it} = 0$. In addition to the indicators for the form of government, the analysis also employs an indicator variable for the electoral system and a measure of the political constraints on the executive. Years in which the lower house was elected exclusively through plurality rule in the most recent elections are coded as $MAJ_{it} = 1$, whereas years in which the lower house was elected by proportional rules are coded as $MAJ_{it} = 0$. The extent of political constraint on the executive is captured by the variable $POLCONIII_{it}$, which is borrowed from Henisz (2000, 2002). Finally, a battery of control variables that for various reasons have been suggested to affect the government revenues and expenditures is being employed.[9] The vector of control variables include the real per capita income (in logs), $LGDPCAP_{it}$, measures of demographic

[9] See Persson and Tabellini (2003) for a survey of the literature, and Sanz and Velazquez (2007) for the importance of demographics.

composition, $PROP\_1564_{it}$ and $PROP\_65_{it}$, and a multidimensional index of global integration, $KOF_{it}$. The former three controls are taken from the WDI database, while the latter is borrowed from Dreher (2006) (updated in Dreher et al., 2008).

All observations must satisfy a democracy requirement to be allowed into the sample. The standard threshold level in the literature (see, e.g., Pevehouse, 2002) is given by $POLITY\_2_{it} \geq 6$, where the $POLITY\_2_{it}$ variable is borrowed from the Polity IV data set. Accordingly, only years in which the countries receive a score of six and above on this index are classified as democratic. This threshold is restrictive, which prevents the influence of non-democratic policies or processes of democratization.[10] However, as a robustness check, also even more restrictive thresholds are being employed.[11] Given the data at hand, inference is based on up to 63 countries and some 1100 observations, implying on average of between 15 and 20 annual observations per country.

### *2.2 Sample*

Table 1 provides an overview over the countries in the sample, and list their respective constitutional classifications.[12] In addition, the table indicates the maximum value of oil production in GDP, $OIL\_GDP_{it}$, in one single year for all of the countries. All countries in Table 1 are considered democratic in at least three consecutive years within the sample period, according to the democracy rule discussed in the previous section.

There are several things to note from Table 1. First, a majority (40) of the countries in the sample derives, or have derived, positive revenues from oil production within the sample period. Thus, the sample also includes countries without oil production. Although these countries do not contribute directly to the identification of the windfall effects, they improve the identification of any additional effects on fiscal policy of the oil price innovations, which is potentially crucial for the identification of the windfall effects working via the government revenues of the oil producers. Second, a significant fraction (about 30 percent) 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. The oil producers are evenly distributed across regime types. Third, employing the stronger democracy threshold of $POLITY\_2_{it} \geq 9$ does not reduce the sample size by much, and there is still a considerable amount of variation in the oil revenues among the countries. The

[10] Several studies find that oil affects democratization (Ross 2001, Tsui 2010; Aslaksen 2007, Gassebner et al. 2009). In addition, oil has been found to affect the level of corruption (Ades and Di Tella 1999, Sala-I-Martin and Subramanian 2003, Aslaksen 2007). The latter findings, however, are robust only among the weak democracies and the non-democratic countries, and should hence not be a major concern in the current study, in which the focus is on democracies only.

[11] In particular, the main results are robust to employing the threshold $POLITY\_2_{it} \geq 9$ (table is not shown, but can be made available upon request) and to the exclusion of specific countries which have switched from autocracy to democracy during the sample period (Turkey, Argentina, Peru, Chile).

[12] Note that Papua New Guinea was considered an extreme outlier and was removed from the sample, thorughout: The relative importance of oil in GDP in Papua New Guinea is more than twice as high as in any other country in the parliamentary sample (followed by Norway) and amounts to some 25 percent of GDP in the sample period. All of the main results remain however also when including Papua New Guinea.

final column in Table 1 indicates which observations drop out of the sample when employing the stricter democracy threshold. Finally, there is only one registered lasting constitutional reform (Cyprus reformed its electoral system in 1981 from majoritarian to proportional), which reflects an institutional inertia that sometimes is being referred to as an "iron law" by the political scientists.[13] The lack of constitutional reforms is the main reason why standard panel data evidence on constitutional effects is so hard, if not impossible, to achieve.

Table 2 provides some key descriptive statistics, for the whole sample and across regime types. The general insights from the literature on the economic effects of constitutions are confirmed. On average, the level of government expenditures is higher in countries with a parliamentary form of government and/or a proportional electoral rule; and a larger fraction of the government budget is allocated towards social security and welfare spending ($CGSSW_{it}$) among the parliamentary countries. Hence, the sample appears to be representative with respect to the existing literature on constitutional effects (see, e.g., Persson and Tabellini, 2003). 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 $W./O.$) which is important for making panel inference in specifications that include country specific trends. Moreover, the average value of oil production in GDP, $OIL\_GDP_{it}$, is about 2 percent, which indicates that oil production on average constitutes a significant fraction of the economy. Oil price fluctuations would hence be expected to cause significant fluctuations in the oil production revenues among the oil producers. Finally, virtually all of the variation in the $WINDFALL_{it}$ variable, which will constitute the main identifying variable in the analysis, is within-country variation. Because oil production intensity differs substantially between countries, but not so much within countries over time, the main within-country variation in the $WINDFALL_{it}$ variable is caused by innovations in the price of oil, which is subject to substantial fluctuations over time.

# 3 Identification and empirical results

*3.1 Oil windfalls and government revenues*

The main identifying assumption of the analysis is that the governments of oil producing countries receive some positive amount of revenues from the oil production sector.[14] Panel data on petroleum tax schedules for a broad cross-section of countries are not readily available, hence the size of the effect of an oil windfall on the total government revenues in GDP must be

[13] Note that France conducted a brief experiment with proportional electoral rules in the period 1985-1986. Moreover, 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 main effects in the paper.

[14] There are few, if any, examples of countries in which there is oil production and where the government does not receive any revenues from this sector. Even among the small oil producers these revenues can be significant. One example is Denmark, who reports (see: http://www.ens.dk/sw51664.asp) that the sum of taxes and fees from the activities in the North sea amounted to about DKK 31 billion in 2006, and just a little less (DKK 7 billion) in 2005, which amounts to 2-3 percent of the total government revenues.

estimated. An oil windfall in country $i$ in time $t$ is defined in the following way,

$$WINDFALL_{it} = \Delta OIL\_PRICE_t \times OIL\_INTENSITY_i, \tag{1}$$

where $\Delta OIL\_PRICE_t$ is the innovation in the real world market price of oil (in 2000 USD) from time $t-1$ to $t$, and $OIL\_INTENSITY_i$ is a measure of the economic importance of oil in country $i$. Because oil price shocks appear to be permanent, the $WINDFALL_{it}$ variable has the interpretation of an unanticipated permanent income shock, relative to GDP, in a country $i$ in time $t$.[15]

Ideally, the $WINDFALL_{it}$ measure would employ an exogenous and time-invariant measure of the oil intensity in each country $i$. However, in reality (i.e., in the data) such a variable does not exist, hence the analysis relies on several operationalizations which are time varying. In the regressions, $OIL\_INTENSITY_i$ is proxied for using different measures of the relative importance of oil production in GDP. Because oil production may be endogenous to factors that simultaneously affect fiscal policy decisions, it will be central to the analysis to demonstrate that the main results are robust to different operationalizations of the $OIL\_INTENSITY_i$ variable. The baseline operationalization is to proxy $OIL\_INTENSITY_i$ by $OILPROD\_GDP_{it-1} = OIL\_PROD_{it-1}/GDP_{it-1}$, where $OIL\_PROD_{it-1}$ is the oil production (in tons) in country $i$ in $t-1$, and $GDP_{it-1}$ is the gross domestic product in country $i$ in $t-1$. The proxy variable $OILPROD\_GDP_{it-1}$ is predetermined to reduce policy endogeneity in the oil intensity variable. However, lagging the variable by one year might not be sufficient to avoid endogeneity, hence also deeper lags of the oil production volume (five- and ten years), relative to the GDP in $t-1$, will be employed as robustness checks on the baseline operationalization. Finally, the analysis makes use of the level of proven oil reserves (relative to GDP in $t-1$) as a proxy for $OIL\_INTENSITY_i$. The level of proven oil reserves in a country is positively correlated with the level of oil production in a country, but is not likely to be endogenous to current volatility (or sources of current volatility) in the government revenues.

The effect of oil windfalls on the government revenues is estimated using the following empirical model

$$\Delta TGREV_{it} = \alpha \Delta TGREV_{it-1} + \sum_{j=0}^{2} \gamma_j WINDFALL_{it-j} + \mathbf{X}'_{it}\boldsymbol{\beta} + \eta_i + \delta Z_t + u_{it}. \tag{2}$$

In equation (2), $\mathbf{X}_{it}$ is a vector of first differenced control variables, $\eta_i$ has the interpretation of country specific trends in the government revenues, $Z$ is a vector of time dummies, and, finally, $u_{it}$ is assumed to be pure white noise and hence that $E(u_{it}) = 0$ for all $i$, $t$.

The country specific trend term, $\eta_i$, is potentially crucial for the identification of the $\gamma$'s.

[15] Acemoglu et al. (2009) show that augmented Dickey-Fuller unit-root tests fail to reject the null hypothesis that log oil prices follow a unit root. This is also the case for the time period of the current analyses. This evidence suggests that the present empirical strategy will be informative about the effects of permanent (rather than transitory) changes in income on government revenues and, consequently, expenditures.

Given the existing evidence that countries indeed have followed different paths with regard to the size of government, it is important not to confound these trends with the effects of the oil windfalls.[16] In particular, by excluding the $\eta_i$'s from model (2) one would run the risk of confounding the effects of oil windfall shocks with the country- or constitution specific fiscal policy trends.[17]

The price of oil affects the economy and fiscal policy via several additional, and often more economically important channels than the effects that work via the government revenues of the oil producers. For example, the price of oil often correlates with international business cycles, and has direct and indirect effects for both the production costs of public goods and services, as well as for the demand of the same goods and services. Moreover, the price of oil affects the general profitability of private production and thus the tax bases of countries. Hence, the $Z_t$'s are included to capture the general economic and political effects of oil price innovations and international business cycles.

Finally, the identification rests on the inclusion of a relevant set of controls. In the present context, the relevant controls are taken to be the standard and most robust set of determinants of the size of government in the constitutions literature. Because the model is in first differences, so are the included controls. Hence all variables are defined as the changes from time $t-1$ to time $t$, denoted by the first-difference operator $\Delta$. The baseline set of controls are (the change in) GDP per capita (in logs) ($\Delta LGDP\_CAP_{it}$)—which is a measure of idiosyncratic, country specific business cycles—, the degree of global integration ($\Delta KOF_{it}$), and a set of demographic variables ($\Delta PROP\_1564_{it}$ and $\Delta PROP\_65_{it}$). In addition, since the windfall variable has the interpretation of an interaction term, each (time varying) element in this term are always included among the control variables.

Table 3 displays the results from employing model (2) to estimate the contemporaneous and lagged effects of the oil windfalls on the changes in the government revenues, in the full sample and in the main subsamples of parliamentary and presidential systems, respectively. The regressions in Table 3 account for two, specific nonlinearities in the effect of oil windfalls on the government revenues. First, because one might worry that oil windfalls have different effects dependent on the oil intensity of the countries—which might be of particular importance in the present context because the mean oil intensity in the presidential subsample is significantly higher than the mean oil intensity among the parliamentary countries—all regressions are estimated both on the full subsamples, as well as on subsamples which are constrained to include only country-years with a net contribution from the oil sector of at least 0.5 percent in GDP. These latter subsamples are referred to as "Oil rich" in the table. Second, the regressions take into account that the level of

[16] See, e.g., Persson and Tabellini (2003) for evidence on the heterogeneity in fiscal policy trends across countries and constitutional categories.

[17] Person and Tabellini (2003) find that the fiscal policy responses to exogenous shocks are stronger in parliamentary than in presidential systems. Their study does however not include country-specific trends, hence the estimates of the fiscal effects of common shocks are likely to be biased. In particular, their main finding may reflect the steeper long-term trend in the size of government in parliamentary systems, rather than responses to economic shocks.

economic development matters, which may be a concern because the mean GDP is significantly lower in the subsample of presidential systems compared with the subsample of parliamentary systems. Hence, the country-years are also separated with respect to the level of development, and country-years are categorized as "Developed" in the table if the level of real GDP per capita exceeds 4000 USD (denoted in constant 2000 USD). As indicated by the estimates in Table 3, however, none of these two concerns appear to be of significant importance; the aggregate effect of the windfalls—aggregated over the three consecutive periods implied by the lag structure—indicates that the windfalls exert a positive effect on the total government revenues across both regime types, and that the effects do not appear to significantly depend on neither the oil intensity nor the level of economic development. Moreover, the aggregate effects are similar across the two regime types, and lie in the range 0.3-0.4. Assuming that the estimates indeed reflect tax revenues from the petroleum sector, the estimates thus imply an effective marginal tax rate somewhere in the range of 30-40 percent. Thus, the estimates suggest that an oil windfall of a value of ten percent in GDP is expected to boost the government revenues by some three to four percent in GDP. The magnitude of the estimates appear sensible; oil production, being intrinsically immobile, is usually taxed quite heavily, and when adjusting for risk- and production costs an effective marginal tax rate of 30-40 appears a plausible estimate.

It appears from the estimates in Table 3 that the second lag of the windfalls are associated with *negative* effects for the government revenues. Although these effects are not statistically significant at any conventional level, they appear somewhat puzzling. One available and straightforward interpretation, however, is tax substitution. When governments experience a positive (negative) shock to the government revenues, an optimal response may be to cut down on (increase) other and potentially more costly sources of taxation. If policy is associated with some level of inertia, such effects would typically appear with lags, and this type of dynamics can hence be a potential explanation for why we observe a negative lagged effect of windfalls on the government revenues.[18]

The model appears to perform quite well as it seems to be capable of delivering credible and consistent estimates of the effects of oil windfalls on the government revenues of oil producing countries. There are, however, several potential challenges which will be addressed successively. First, as briefly discussed above, the operationalization of the variable $OIL\_INTENSITY_i$ is not straight forward. Conceptually, $OIL\_INTENSITY_i$ is a time-independent and purely exogenous variable. However, such a variable rarely exists. For example, the level of oil production in a country $i$ is at any point in time $t$ likely to be a function of the intensity of past and present petroleum activity in that country, which in turn is associated with historical and present political factors. Hence, instead of relying on just one single operationalization of the $OIL\_INTENSITY_i$ variable, several different approaches will be employed. The regression estimates displayed in Table A1a in the Appendix make use of three alternative operationalizations of $OIL\_INTENSITY_i$. In the table, "Five years" refers to an operationalization where

[18] Note that this pattern of tax substitution is consistent with standard macroeconomic models of optimal intertemporal allocation of wealth shocks, such as for example Barro's (1979) tax smoothing hypothesis.

$OIL\_INTENSITY_i$ is proxied by $OILPROD\_GDP_{it-5}$ ($= OIL\_PROD_{it-5}/GDP_{it-1}$), "Ten years" refers to the operationalization $OILPROD\_GDP_{it-10}$ ($= OIL\_PROD_{it-10}/GDP_{it-1}$), and, finally, "Reserves" refers to the operationalization $OILRESERVES\_GDP_{it-1}$ ($= OIL\_RESERVES_{it} \times 10^7/GDP_{it-1}$). The latter operationalization (which reads 'the amount of oil reserves, in tens of tons, per predetermined unit of GDP measured in constant 2000 USD') has no straightforward, intuitive interpretation, except that the level of proven oil reserves is assumed to be a proxy for the level of oil production in a given country in a given year, and hence the measure is a proxy for the oil intensity in that country. What matters in the current context, is whether this proxy, together with the other alternative operationalizations, delivers effects that are similar to those in the baseline regressions in Table 3. Comparing the results in Table A1a and Table 3 (columns (1), (4), and (7)) provides an indication that the results in Table 3 are robust to the alternative operationalizations, with the exception of the ten year lagged variable in the parliamentary sample which does not appear to exert a positive effect on the government revenues. A potential worry is, however, that several of the estimates in the sample of parliamentary systems are insignificant at the conventional levels. One interpretation is that the alternative $OIL\_INTENSITY_{it}$ operationalizations are more noisy than the baseline operationalization, and a way of 'removing' noise from the estimations is to focus attention on a more homogenous sample. In Table A1b, the estimates are based on the subsamples of "Oil rich" country-years (as in Table 3 columns (2), (5) and (8)). As expected, the estimates in Table A1b are more precise. Moreover, the overall explanatory power is also higher, with an R-squared (within) in the parliamentary sample in the range of 0.3 to 0.4, compared with 0.08 in Table A1a. Taken together, the evidence suggests that oil windfalls exert positive effects on the government revenues, but—not unexpectedly—more so in the 'Oil rich' country-years. Importantly, comparing across the two different forms of government the overall effects of oil windfalls for the government revenues appear comparable in magnitude.[19]

### *3.1 Comparative politics*

The previous section documents evidence that the oil windfalls have significant and positive effects for the government revenues. This section investigates and compares the effects of the shifts in the government revenues caused by the windfall shocks on the government expenditures across the two forms of government. The structural relationship of interest is,

$$\Delta TGEXP_{it} = \alpha' \Delta TGEXP_{it-1} + \sum_{j=0}^{2} \gamma_j' \Delta TGREV_{it-j} + \mathbf{X}_{it}' \boldsymbol{\beta}' + \eta_i' + \delta' Z_t + u_{it}, \qquad (3)$$

[19] One additional worry when estimating dynamic panel data models—such as model (1)—is that the estimates could be biased due to the systematic correlation between the lagged regressand and the error term. The Nickell bias (Nickell, 1981) is however less severe the longer is the panel (see, e.g., Wooldridge, 2002). As the average panel length in the current analysis is between 15 and 20, the fixed effects estimator is expected to perform quite well. As a check on the potential severity of this source of bias, the Table 3 regressions were also estimated employing the standard Difference and System GMM estimators (see, e.g., Bond, 2002). The GMM estimates (which are available upon request) were highly consistent with the OLS-FE estimates.

where the parameters have similar interpretations as in (2). The baseline empirical strategy thus implies that not only the $\gamma_j'$'s, but also all of the other parameters are allowed to vary across the regime types.[20] Clearly, estimating (3) with OLS would result in highly significant estimates of the $\gamma_j'$'s as $\Delta TGREV_{it}$ and $\Delta TGEXP_{it}$ are strongly correlated;[21] however, the results would obviously be severely biased due to endogeneity in $\Delta TGREV_{it}$. The empirical strategy of the present paper is, as discussed in the introduction, to deal with this problem of endogeneity by employing the proposed windfall proxy to instrument for the changes in the government revenues, $\Delta TGREV_{it}$. In particular, $\Delta TGREV_{it}$ is instrumented for using model (2) in the first-stage regressions.[22]

The results from the 2SLS IV-regressions are displayed in Table 4, where columns (1)-(3) report the results for the full sample, while columns (4)-(6) and (7)-(9) display the results for the parliamentary- and presidential regime types, respectively. Given the validity of the IV-estimates, which will be discussed in more detail below, the estimates in Table 4 suggest that $(i)$ shocks in the government revenues have positive, causal effects for the government expenditures, but $(ii)$ the effects appear to critically depend on the form of government. While the effects are strong and significant (at the one percent level in two out of three subsamples) in the presidential sample, the estimates are nonsignificant and unstable in the parliamentary sample (and subsamples).

The aggregate effects of the three lags of government revenue shocks in the presidential sample, and subsamples, are in the range of 0.5-0.8, implying that a positive shock in the government revenues in $t$, of, say, 5 percent in GDP, implies an increase in the government expenditures over the subsequent two years (from $t$ to $t+2$) of some 2.5 to 4 percent in GDP. The evidence reported in Table 4 hence suggests that the short term pass-through of government revenue shocks to the government expenditures is insignificant if the form of government is parliamentary, while it is highly significant, both economically and statistically, when the form of government is presidential.

The first stage F-tests, reported below the parameter estimates in the table, indicate that the current and lagged $WINDFALL_{it}$ variables are generally strong predictors of variation in the government revenues. The F-statistics are weaker in the parliamentary sample, which is not surprising given that there is less variation in the oil windfall variables in this sample. The F-statistics is comparatively weaker in columns (4) and (6), which may be explained by the evidence in Table 3 that these parameter estimates are characterized by more noise (i.e., higher standard deviations) in the parameter estimates.

Considering the Kleibergen-Paap rk Wald-, Anderson-Rubin Wald-, and the Hansen J statistics of the regressions on the full sample (columns (1) to (3)), the instruments appear generally

[20] This assumption will be relaxed in a reduced-form approach in Section 3.3, where all parameters but the institutional will be estimated in one single regression and hence will be constrained to take on the same values across both forms of government.

[21] The pairwise correlation coefficent between the two is 0.80, significant at the one percent level of significance.

[22] Note that in the first-stages of the IV-regressions, three lags of the windfall variable are being employed, instead of two, as in Table 3. Employing three lags is necessary to achieve a good identification of the second lag of $\Delta TGREV$. The main results of interest do however not critically depend the inclusion or exclusion of single lags of the $WINDFALL$ variables.

strong and exogenous.[23] There is, however, some indication that the instruments may be weak, in particular in the parliamentary subsample. In columns (4) and (6), both the Kleibergen-Paap rk Wald F-statistic and the Hansen J-statistic indicate weak and potentially endogenous instruments, while, on the other hand, the Anderson-Rubin Wald statistic rejects the joint hypothesis that the coefficients of the endogenous regressors are jointly equal to zero in the structural equation in two out of the three samples (the exception is the 'Developed' sample, in Column (6)). In the sample of parliamentary 'Oil rich' countries, Column (5), both the Kleibergen-Paap rk Wald statistic and the Hansen J statistic indicate that the instruments perform well, and the Anderson-Rubin Wald F statistic suggests that the endogenous regressors are statistically significant (with a p-value of 0.07). Thus, there is some indication of instrument weakness, but the overall evidence supports the validity of the instruments, and hence that the estimates can be trusted.

### *3.3 Additional sensitivity checks*

The form of government may correlate with other constitutional features, such as the electoral rule.[24] Moreover, also other institutional features correlate with the form of government, such as the formal political constraints on the executive, veto players (Tsebelis, 1995, 1999, 2002),[25] and the overall institutional quality. Table A2 in the Appendix displays the results from employing a reduced-form approach, regressing the government expenditures on the windfall variables (and their separate, constituting terms) in interaction with the different institutional correlates, to check whether the main results of the paper are likely to be driven by the form of government, or by some other institutional correlates. The windfall-presidential interaction terms are positive throughout, while the other institutional correlates are for the most part statistically insignificant (and unstable). In the full specification (Column (6)), the constitutional interactions (i.e., the form of government and the electoral rule) are the only ones which are statistically significant, and presidentialism appear much more important for the fiscal dynamic effect of the exogenous

[23] The Stock-Yogo (2005) critical values are not available in the baseline specification with three endogenous regressors (in the tables, $\Delta TGREV$, $\Delta TGREV\_1$, $\Delta TGREV\_2$) and four excluded instruments (in the tables, $WINDFALL$, $WINDFALL\_1$, $WINDFALL\_2$, $WINDFALL\_3$), thus the extent of the weakness in the identification, as suggested by the Kleibergen-Paap rk F-statistics, can not easily be assessed. However, in the more restrictive, but less demanding specification employing only two lags of the endogenous regressor ($\Delta TGREV$, $\Delta TGREV\_1$), the Stock-Yogo critical values are available, and the F-statistics are well above the critical value for 10% maximal IV relative bias in both the full sample and the sample of presidential countries, whereas among the parliamentary samples this is only the case within the "oil rich" subsample (table not shown, but available upon request). The Stock-Yogo critical values are for Cragg-Donald F statistic and i.i.d. errors, as reported by Stata, version 10.1.

[24] In the current sample, the pairwise correlation between presidentialism and majoritarian electoral rule is -0.13, significant at the one percent level.

[25] Several papers have investigated the interaction between institutions, different veto player measures, and the responsiveness to economic shocks (e.g., Roubini and Sachs, 1989; Alesina and Drazen,1991). Using data on U.S. state government, Poterba (1994) shows that divided governments—as opposed to governments consisting of just one single party—tend to block fiscal adjustments. Auerbach (2008) present recent evidence on the effect of the U.S. federal budget rules.

oil windfalls than having a majoritarian electoral rule. Also, the magnitude of the effects appear consistent with the previous results, as implied by the estimates in Tables 3 and 4.

An additional question is whether the windfall effects are symmetric, that is whether positive and negative realizations of oil price shocks have similar effects for the fiscal dynamics. Table A3 reports the results from running separate regressions on positive ($> 0$) and negative ($< 0$) shocks, respectively, on the current and future government revenues, for the whole sample as well as for both forms of governments separately. To achieve a consistent identification of the separate effects of positive and negative shocks, the regression samples are constrained to sequences of at least two consecutive positive and negative shocks, respectively. In particular, inference is based on 143 sequences of positive shocks and 235 sequences of negative shocks. Since the sample is constrained to sequences of two consecutive shocks that are qualitatively similar, so is the regression model, which now employs only one lag (instead of two, as in the baseline model).[26] The results displayed in Table A4 indicate that the effects of oil price shocks for the government revenues are fairly symmetric; thus, positive shocks are expected to have positive effects, while negative shocks have negative effects on the current and future government revenues.

# 4 Final remarks and avenues for future research

The main finding of the present paper—that government revenue shocks have strong effects for the government expenditures if the form of government is presidential, but not if it is parliamentary—has several potential implications. First, it constitutes a guide for further theorizing on the dynamic fiscal effects of the form of government. Hopefully, future models will be able to rationalize and investigate in more detail the legislative mechanisms that can be consistent with the empirical patterns. Second, the results might shed some light on the literature linking political institutions with fiscal policy volatility, and in turn growth. The present results suggest that fiscal policy might be more volatile in the presence of shocks to the government revenues when the form of government is presidential. This result is consistent with the regularity that fiscal policy is more volatile when the form of government is presidential (Fatás and Mihov, 2005), which may have effects for the long-term capacity for economic growth.

# 5 Acknowledgments

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[26] Constraining the sample even further, to three rather than two consecutive positive (/negative) shocks would reduce the sample sizes by too much to making meaningful statistical inference feasible.

comments and suggestions.

# 6 Tables

Table 1
Sample: Countries, constitutions, oil production intensity, and level of democracy

| Country | *PRES* | *MAJ* | Dem. years | Maximum value of oil depletion in GDP (*OIL_GDP*) in one year >0% | >1% | >5% | Strong Dem. |
|---|---|---|---|---|---|---|---|
| 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(*PRES*=0, *MAJ*=0)=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 |
| Thailand | 0 | 1 | 1991-2001 | Yes | | | Yes |
| UK | 0 | 1 | 1970-2001 | Yes | Yes | Yes | Yes |
| [n(*PRES*=0, *MAJ*=1)=14] | | | | [=9] | [=6] | [=2] | [=11] |

(Table continues on next page)

Table 1 (continued)

| Country | *PRES* | *MAJ* | 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(*PRES*=1, *MAJ*=0)=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(*PRES*=1, *MAJ*=1)=9] | | | | [=5] | [=1] | [=1] | [=4] |

Note: Constitutional classification: Countries/years in which lower house legislative elections rely exclusively on plurality rules are coded as *MAJ*=1 (otherwise, *MAJ*=0). Countries in which the executive is relying on the confidence of the legislature are coded *PRES*= 1 (otherwise, *PRES*=0). * Country has undergone electoral reform. ** Country has undergone reform in the form of government. *** OPEC member. "Dem. years" means years in which the country has received a *POLITY_2* score >=6. "Strong Dem" means a *POLITY_2* score >=9.

Table 2
Descriptive statistics

| | All countries | | | | Parl. Form of gov. | | Pres. Form of gov. | |
|---|---|---|---|---|---|---|---|---|
| | | Std. Dev. | | | | | | |
| Variable | Mean | Overall | W./O. | N/n/T | Mean | N/n/T | Mean | N/n/T |
| *TGEXP* | 28.0 | 11.7 | 0.5 | 1211/63/19 | 32.2 | 503/26/19 | 18.9 | 231/16/14 |
| *TGREV* | 25.5 | 10.7 | 0.4 | 1208/63/19 | 29.1 | 506/26/19 | 17.3 | 232/16/15 |
| *TGBAL* | -2.5 | 3.9 | 0.7 | 1203/63/19 | -3.1 | 503/26/19 | -1.6 | 231/16/14 |
| *CGSSW* | 9.0 | 6.3 | 0.3 | 777/45/17 | 12.2 | 321/17/19 | 4.4 | 127/11/12 |
| *GDP_CAP* | 10.2 | 9.3 | 0.3 | 1564/67/23 | 13.2 | 621/28/22 | 5.7 | 302/16/19 |
| *OIL_GDP* | 1.9 | 5.4 | 0.4 | 1599/67/24 | 1.1 | 621/28/22 | 7.6 | 302/16/19 |
| *WINDFALL* | 0.000 | 0.019 | 0.994 | 1344/66/21 | 0.007* | 540/27/20 | 0.041* | 256/16/16 |

Note: W./O. is the ratio of the within group std. dev. to the overall std. dev.. Asterisks (*) indicate that the values refer to overall standard deviations, rather than the mean.

Table 3
The effect of oil windfalls on the government revenues.

| Regime type | All regime types | | | Parl. form of government | | | Pres. form of government | | |
|---|---|---|---|---|---|---|---|---|---|
| Sample | All | Oil rich | Devel. | All | Oil rich | Devel. | All | Oil rich | Devel. |
| Estimation method | OLS-FE | OLS-FE | OLS-FE | OLS-FE | OLS-FE | OLS-FE | OLS-FE | OLS-FE | OLS-FE |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| *WINDFALL* | 0.18*** | 0.18*** | 0.20*** | -0.12 | 0.06 | -0.16 | 0.16*** | 0.13** | 0.19*** |
| | (0.05) | (0.05) | (0.02) | (0.11) | (0.08) | (0.11) | (0.04) | (0.05) | (0.03) |
| *WINDFALL-1* | 0.12*** | 0.15** | 0.10** | 0.39** | 0.35** | 0.41** | 0.16*** | 0.20*** | 0.11*** |
| | (0.04) | (0.05) | (0.04) | (0.17) | (0.14) | (0.18) | (0.03) | (0.06) | (0.03) |
| *WINDFALL-2* | -0.04 | -0.06 | 0.02 | 0.00 | -0.09 | -0.05 | -0.02 | -0.08 | 0.03 |
| | (0.04) | (0.06) | (0.04) | (0.16) | (0.13) | (0.15) | (0.05) | (0.07) | (0.03) |
| *OILPROD_GDP-1* | 81.44* | 78.75* | 171.29** | 126.74 | -5.92 | 140.16 | 118.34** | 104.35* | 227.02*** |
| | (41.00) | (42.69) | (65.90) | (102.16) | (29.81) | (104.55) | (50.09) | (51.86) | (31.93) |
| *OILPROD_GDP-2* | -56.24*** | -45.80*** | -196.73*** | -116.94 | -63.13 | -143.46 | -45.73*** | -45.64*** | -161.73** |
| | (22.11) | (12.19) | (68.82) | (117.62) | (40.54) | (121.06) | (9.47) | (8.49) | (51.45) |
| *OILPROD_GDP-3* | -7.93 | -8.23 | 44.36** | -4.15 | -60.89* | 8.22 | -26.11 | -13.04 | -5.39 |
| | (12.95) | (21.48) | (18.61) | (31.02) | (29.74) | (31.57) | (30.89) | (44.29) | (38.43) |
| Δ*OILP* | 4.0E-3 | 4.0E-3 | 4.2E-3 | 5.2E-3* | -2.3E-3 | 5.5E-3 | 6.4E-3 | 1.6E-2 | 1.1E-2 |
| | (2.7E-3) | (5.6E-3) | (3.5E-3) | (3.0E-3) | (4.6E-3) | (4.0E-3) | (5.8E-3) | (1.5E-2) | (0.8E-2) |
| Δ*OILP-1* | 1.6E-3 | -3.2E-3 | -0.6E-3 | 1.4E-3 | -6.3E-3 | 0.8E-3 | 0.2E-3 | -3.6E-3 | 2.7E-3 |
| | (2.4E-3) | (2.9E-3) | (3.3E-3) | (2.7E-3) | (4.2E-3) | (3.6E-3) | (2.9E-3) | (5.3E-3) | (5.7E-3) |
| Δ*OILP-2* | 1.8E-3 | -2.6E-3 | 2.1E-3 | 3.3E-3 | -4.0E-3* | 4.5E-3 | -2.5E-3 | 1.4E-3 | 1.8E-3 |
| | (2.3E-3) | (2.6E-3) | (2.7E-3) | (2.3E-3) | (1.7E-3) | (3.2E-3) | (4.2E-3) | (5.4E-3) | (6.4E-3) |
| Δ*LGDP_CAP* | 0.28*** | 0.20*** | 0.24*** | 0.24*** | 0.15** | 0.21*** | 0.27*** | 0.21*** | 0.19*** |
| | (0.04) | (0.05) | (0.06) | (0.05) | (0.06) | (0.05) | (0.03) | (0.03) | (0.04) |
| Δ*KOF* | 1.1E-3 | 0.9E-3 | 0.1E-4 | 2.5E-4 | 1.8E-4 | 5.0E-4 | 2.0E-3* | 2.1E-3 | 1.6E-3 |
| | (0.7E-3) | (0.9E-3) | (5.9E-4) | (6.0E-4) | (7.0E-4) | (6.0E-4) | (1.0E-3) | (1.4E-3) | (1.8E-3) |
| Δ*PROP_1564* | -1.5E-2 | 1.3E-2 | -1.3E-2 | -1.3E-2 | 1.1E-2* | 1.2E-2 | -1.4E-2* | -1.2E-2 | -1.1E-2 |
| | (1.1E-2) | (0.8E-2) | (1.3E-2) | (1.4E-2) | (0.5E-2) | (1.5E-2) | (0.7E-2) | (1.4E-2) | (1.4E-2) |
| Δ*PROP_65* | 0.5E-3 | 2.3E-3 | 3.0E-3 | 8.4E-3 | 1.3E-2 | 3.6E-3 | 0.2E-2 | 1.9E-2 | 0.3E-3 |
| | (9.5E-3) | (2.1E-3) | (8.0E-3) | (9.0E-3) | (2.1E-2) | (9.0E-3) | (2.5E-2) | (4.9E-2) | (5.8E-3) |
| Δ*TGREV-1* | -0.15* | -0.26* | 0.04 | 0.04 | 0.09 | 0.08 | -0.40*** | -0.40*** | -0.22 |
| | (0.09) | (0.14) | (0.07) | (0.07) | (0.07) | (0.08) | (0.09) | (0.12) | (0.15) |
| R-squared | 0.10 | 0.4 | 0.09 | 0.08 | 0.43 | 0.09 | 0.43 | 0.54 | 0.66 |
| Observations | 1087 | 361 | 686 | 673 | 189 | 546 | 409 | 172 | 140 |
| Countries | 63 | 27 | 35 | 35 | 14 | 26 | 30 | 14 | 9 |

Note: *-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, clustered at the country level. Dependent variable is Δ*TGREV* . All regressions include year fixed effects and country specific trends. The samples of "Oil rich" country-years are country-years in which the value of oil production weakly exceeds 0.5 percent of GDP (*OIL_GDP*>=.5). The samples of "Devel." country-years are country-years in which the level of GDP per capita exceeds 4000 US$ (measured in constant US$, base year 2000). R-squared refers to the within-variation.

Table 4
Government revenues and expenditures across regime types: IV(2SLS)-estimates

| Regime type | All regime types | | | Parl. form of government | | | Pres. form of government | | |
|---|---|---|---|---|---|---|---|---|---|
| Oil intensity and level of development | All | Oil rich | Developed | All | Oil rich | Developed | All | Oil rich | Developed |
| Estimation method | 2SLS | 2SLS | 2SLS | 2SLS | 2SLS | 2SLS | 2SLS | 2SLS | 2SLS |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| $\Delta TGREV$ | -0.34** | -0.06 | -0.89*** | -0.17 | 0.00 | 0.22 | -0.11 | -0.37* | -0.37*** |
| | (0.15) | (0.24) | (0.24) | (1.10) | (0.21) | (0.27) | (0.19) | (0.20) | (0.11) |
| $\Delta TGREV\text{-}1$ | 0.49*** | 0.43*** | 0.53** | -2.94 | -0.36 | -0.52 | 0.45*** | 0.23 | 0.67*** |
| | (0.16) | (0.05) | (0.24) | (3.43) | (0.28) | (0.65) | (0.07) | (0.18) | (0.11) |
| $\Delta TGREV\text{-}2$ | 0.21 | 0.18** | 0.83*** | 3.60 | 0.43 | -1.05 | 0.34*** | 0.02 | 1.04*** |
| | (0.22) | (0.08) | (0.41) | (3.90) | (0.56) | (0.70) | (0.12) | (0.22) | (0.29) |
| F($\Delta TGREV$) [p] | 40.11 [0.00] | 26.59 [0.00] | 98.52 [0.00] | 5.60 [0.00] | 4.94 [0.01] | 9.57 [0.00] | 16.21 [0.00] | 20.21 [0.00] | 67.47 [0.00] |
| F($\Delta TGREV\text{-}1$) [p] | 65.04 [0.00] | 46.80 [0.00] | 92.50 [0.00] | 1.90 [0.13] | 6.22 [0.00] | 1.67 [0.19] | 31.96 [0.00] | 81.65 [0.00] | 119.9 [0.00] |
| F($\Delta TGREV\text{-}2$) [p] | 48.27 [0.00] | 53.59 [0.00] | 23.06 [0.00] | 2.68 [0.05] | 12.48 [0.00] | 4.02 [0.01] | 73.98 [0.00] | 34.06 [0.00] | 23.07 [0.00] |
| Kleibergen-Paap rk Wald (p) | 0.00 | 0.01 | 0.00 | 0.67 | 0.00 | 0.08 | 0.00 | 0.01 | 0.00 |
| Kleibergen-Paap rk Wald (F) | 4.18 | 1.81 | 12.79 | 0.17 | 1.90 | 1.08 | 6.23 | 1.40 | 16.04 |
| Anderson-Rubin Wald F (p) | 0.01 | 0.00 | 0.00 | 0.00 | 0.07 | 0.15 | 0.00 | 0.01 | 0.00 |
| Hansen J (p) | 0.31 | 0.11 | 0.50 | 0.23 | 0.74 | 0.06 | 0.16 | 0.07 | 0.27 |
| Observations | 1039 | 351 | 658 | 642 | 187 | 522 | 394 | 164 | 136 |
| Countries | 63 | 27 | 35 | 35 | 14 | 26 | 30 | 14 | 9 |

Note: *-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, clustered at the country level. The dependent variable is $\Delta TGEXP$. Contermporaneous and two lags of first-differenced government revenues, $\Delta TGREV$, are instrumented using the contemporaneous- and three lags of the *WINDFALL* variable (in addition to the baseline set of controls, a full set of country- and year indicators, and each component of the *WINDFALL* variable with the associated lags). The F-test refer to the test of the joint significance of the four *WINDFALL* intruments in the 1. stage regerssions, with the corresponding *p*-values in brackets. The Kleibergen-Paap LM- and Wald-statistics refer to the LM and Wald versions of the Kleibergen-Paap (2006) rk statistics, which test the null hypothesis of underidentification. The Anderson-Rubin F-statistics refer to the Anderson-Rubin (1949) Wald F-statistics, which tests the null hypothesis that the coefficients of the endogenous regressors in the structural equation are jointly equal to zero and that the overidentifying restrictions are also valid. Finally, the Hansen J statistic refer to the test of the null hypothesis of instrument validity. The abbreviations (p) and (F) refer to p-values and F-statistics, respectively.

# Appendix

*A.1 Operators and variable definitions*

**Operators and indicies**

The first difference operator $\Delta(.)$ indicates changes in the respective variable from time $t-1$ to time $t$. All variables are country-year observations, and are indexed according to their contextual time dimensions in the main text.

**Institutional 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).

$DEM$: Interpolated version of the Polity- and Gastil indexes. 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 indexes for civil liberties and political rights provided by the Freedom House, "Annual Survey of Freedom Country Ratings".

$POLCONIII$: The $POLCONIII$ variable, from the POLCON database 2006 (Henisz, 2002). The variable ranges between zero and one, and represents an estimate of (Henisz, 2002, p.363): "...the feasibility of policy change (the extent to which a change in the preferences of any one actor may lead to a change in government policy)...". For more detailed information on this variable, see Henisz (2002).

**Fiscal policy variables:**

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

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

**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.

$OIL\_PROD$: The quantity of oil production, measured in tons. Source: World Bank Adjusted Net Savings Dataset.

$OIL\_RESERVES$: Oil reserves in million metric tons, recalculated from barrels using the conversion calculator provided by the Energy Information Administration (EIA). Source: The EIA: http://www.eia.doe.gov/emeu/international/contents.html.

$OIL\_PRICE$: The world market price of crude oil per metric ton, base year 2000. Source: World Bank Adjusted Net Savings Dataset.

$WINDFALL$: Defined in Section 2.3.

**Economic and demographic control variables:**

$GDP$: The level of (real) GDP, measured in constant U.S. dollars, base year 2000. Source: The World Banks World Development Indicators database.

$LGDPCAP$: The natural logarithm of real GDP per capita in constant U.S. dollars, base year 2000. Source: The World Banks World Development Indicators database.

$PROP\_1564$: The percentage of a country's population between 15 and 64 years old in the total population. Source: The World Banks World Development Indicators database.

$PROP\_65$: The percentage of the population over the age of 65 in the total population. Source: The World Banks World Development Indicators database.

$KOF$: A composite index of globalization, composed of data on economic globalization (e.g., trade, FDI, import barriers, etc.), social globalization (e.g., personal contact, information flows, and cultural proximity), and political globalization (e.g, the number of embassies in the country, participation in the U.N. Security Council Missions, or membership in international organizations. Source: Dreher (2006) and Dreher et al. (2008).

*A.2 Tables*

Table A1a
Alternative operationalizations of *OIL_INTENSITY* in the Table 3-regressions. Full sample

| Regime type | All regime types | | | Parliamentary form of government | | | Presidential form of government | | |
|---|---|---|---|---|---|---|---|---|---|
| *WINDFALL* -specification | Five years | Ten years | Reserves | Five years | Ten years | Reserves | Five years | Ten years | Reserves |
| Estimation method | OLS-FE | OLS-FE | OLS-FE | OLS-FE | OLS-FE | OLS-FE | OLS-FE | OLS-FE | OLS-FE |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| *WINDFALL* | 0.09*** | 0.10* | 4.19*** | -0.14 | -1.16 | -9.22* | 0.09*** | 0.10 | 4.26*** |
| | (0.02) | (0.05) | (1.22) | (0.17) | (0.91) | (4.85) | (0.02) | (0.06) | (1.48) |
| *WINDFALL-1* | 0.16*** | 0.12*** | 7.91*** | 0.25 | 0.40 | 12.94*** | 0.23*** | 0.13*** | 8.97*** |
| | (0.06) | (0.04) | (1.59) | (0.32) | (0.58) | (4.11) | (0.06) | (0.03) | (1.35) |
| *WINDFALL-2* | 0 | 0.09*** | 1.31 | -0.01 | -0.17 | 2.40 | 0.04 | 0.13*** | 3.04*** |
| | (0.05) | (0.03) | (1.36) | (0.22) | (0.21) | (6.61) | (0.05) | (0.03) | (1.25) |
| R-squared | 0.09 | 0.09 | 0.09 | 0.08 | 0.09 | 0.07 | 0.39 | 0.41 | 0.42 |
| Observations | 1032 | 855 | 936 | 631 | 503 | 581 | 398 | 349 | 354 |
| Countries | 63 | 62 | 65 | 35 | 34 | 37 | 30 | 29 | 29 |

Note: *-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, clustered at the country level. Dependent variable is $\Delta TGREV$ . "*WINDFALL* -specification": "Five years" and "Ten years" means that the *WINDFALL* -variable is constructed using the five- and ten year lags of oil production volumes, respectively. "Reserves" means that the WINDFALL-variable is instead constructed using the size of the countries' proven reserves. All regressions are fixed-effects regressions, including a one-year lag of the dependent variable, full sets of country- and year indicators, the baseline set of control variables, and each of the components of the respective WINDFALL-variables (with lags). R-squared reports on the within-variation.

Table A1b
Alternative operationalizations of *OIL_INTENSITY* in the Table 3-regressions. "Oil rich" sample

| Regime type | All regime types | | | Parliamentary form of government | | | Presidential form of government | | |
|---|---|---|---|---|---|---|---|---|---|
| *WINDFALL* -specification | Five years | Ten years | Reserves | Five years | Ten years | Reserves | Five years | Ten years | Reserves |
| Estimation method | OLS-FE | OLS-FE | OLS-FE | OLS-FE | OLS-FE | OLS-FE | OLS-FE | OLS-FE | OLS-FE |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| *WINDFALL* | 0.08*** | 0.06 | 3.50** | 0.16 | 0.21 | -4.64* | 0.05 | 0.00 | 2.31 |
| | (0.03) | (0.06) | (1.46) | (0.14) | (0.38) | (2.42) | (0.04) | (0.09) | (1.94) |
| *WINDFALL-1* | 0.21*** | 0.14*** | 9.24*** | 0.50* | 0.21 | 10.39*** | 0.26*** | 0.17*** | 10.32*** |
| | (0.07) | (0.04) | (1.64) | (0.25) | (0.31) | (3.36) | (0.08) | (0.06) | (2.04) |
| *WINDFALL-2* | -0.01 | 0.10*** | 1.53 | -0.05 | 0.04 | -3.17 | 0.00 | 0.10*** | 2.38 |
| | (0.06) | (0.02) | (1.57) | (0.17) | (0.24) | (3.40) | (0.06) | (0.03) | (1.39) |
| R-squared | 0.34 | 0.33 | 0.33 | 0.40 | 0.36 | 0.29 | 0.48 | 0.47 | 0.52 |
| Observations | 347 | 304 | 342 | 181 | 158 | 194 | 166 | 146 | 148 |
| Countries | 26 | 25 | 27 | 13 | 13 | 15 | 14 | 13 | 13 |

Note: *-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, clustered at the country level. Dependent variable is ΔTGREV. "WINDFALL-specification": "Five years" and "Ten years" means that the WINDFALL-variable is constructed using the five- and ten year lags of oil production volumes, respectively. "Reserves" means that the WINDFALL-variable is instead constructed using the size of the countries' proven reserves. All regressions are fixed-effects regressions, including a one-year lag of the dependent variable, full sets of country- and year indicators, the baseline set of control variables, and each of the components of the respective WINDFALL-variables (with lags). R-squared reports on the within-variation.

Table A2
Oil windfalls and government expenditures: Institutional correlates

| Estimation method | OLS-FE | OLS-FE | OLS-FE | OLS-FE | OLS-FE | OLS-FE |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Dependent variable | Δ*TGEXP* | Δ*TGEXP* | Δ*TGEXP* | Δ*TGEXP* | Δ*TGEXP* | Δ*TGEXP* |
| *WINDFALL* | -0.08** | -0.59** | -0.66** | -0.30 | -0.59** | -0.56 |
| | (0.03) | (0.17) | (0.25) | (0.34) | (0.19) | (0.43) |
| *WINDFALL-1* | 0.09** | -0.18 | -0.24* | -0.08 | -0.28 | 0.05 |
| | (0.03) | (0.15) | (0.14) | (0.53) | (0.18) | (0.46) |
| *WINDFALL-2* | 0.07** | 0.00 | 0.10 | -0.89 | 0.19 | 0.27 |
| | (0.03) | (0.12) | (0.19) | (0.58) | (0.14) | (0.86) |
| *WINDFALL* x *PRES* | | 0.55** | 0.61** | 0.48** | 0.53** | 0.59** |
| | | (0.18) | (0.25) | (0.18) | (0.18) | (0.29) |
| *WINDFALL* x *PRES-1* | | 0.28* | 0.34** | 0.22 | 0.27 | 0.33* |
| | | (0.15) | (0.15) | (0.18) | (0.17) | (0.17) |
| *WINDFALL* x *PRES-2* | | 0.09 | -0.02 | 0.15 | 0.08 | 0.06 |
| | | (0.12) | (0.19) | (0.18) | (0.13) | (0.22) |
| *WINDFALL* x *MAJ* | | | 0.09 | | | 0.18 |
| | | | (0.37) | | | (0.36) |
| *WINDFALL* x *MAJ-1* | | | 0.08 | | | 0.09 |
| | | | (0.21) | | | (0.24) |
| *WINDFALL* x *MAJ-2* | | | -0.04 | | | -0.08 |
| | | | (0.28) | | | (0.28) |
| *WINDFALL* x *POLITY_2* | | | | -0.03 | | -0.01 |
| | | | | (0.03) | | (0.03) |
| *WINDFALL* x *POLITY_2-1* | | | | -0.01 | | -0.03 |
| | | | | (0.05) | | (0.04) |
| *WINDFALL* x *POLITY_2-2* | | | | 0.09* | | 0.01 |
| | | | | (0.05) | | (0.07) |
| *WINDFALL* x *POLCONIII* | | | | | -0.04 | -0.09 |
| | | | | | (0.17) | (0.14) |
| *WINDFALL* x *POLCONIII-1* | | | | | 0.16 | -0.15 |
| | | | | | (0.21) | (0.40) |
| *WINDFALL* x *POLCONIII-2* | | | | | -0.24** | -0.46 |
| | | | | | (0.11) | (0.27) |
| R-squared | 0.072 | 0.081 | 0.088 | 0.084 | 0.090 | 0.093 |
| Observations | 1093 | 1078 | 1049 | 1060 | 1058 | 1028 |
| Countries | 63 | 63 | 62 | 62 | 63 | 61 |

Note: *-Significant at the 10 percent level; **-significant at the 5 percent level. The numbers in parentheses are robust standard errors, clustered at the country level. All regressions are fixed-effects regressions, including full sets of country- and year indicators, the baseline set of control variables, and each of the components in the WINDFALL-variable (OILPROD_GDP and ΔOILP) with lags and interacted with the respective institutional variables (results not displayed). R-squared refers to the within R-squared.

Table A3
Symmetry in the effect of the *WINDFALL*- variables in the Table 3 regressions

| Regime type | All regime types | | | | Parl. form of gov. | | Pres. form of gov. | |
|---|---|---|---|---|---|---|---|---|
| *WINDFALL* -variables | All | All | >0 | <0 | >0 | <0 | >0 | <0 |
| Estimation method | OLS-FE | OLS-FE | OLS-FE | OLS-FE | OLS-FE | OLS-FE | OLS-FE | OLS-FE |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *WINDFALL* | 0.18** | 0.19** | 0.58** | 0.43** | -0.41** | -0.19 | 0.78** | 0.42** |
| | (0.05) | (0.04) | (0.06) | (0.08) | (0.20) | (0.24) | (0.09) | (0.12) |
| *WINDFALL-1* | 0.12** | 0.13** | -0.12 | 0.37** | 0.58* | 1.68** | -0.20* | 0.39** |
| | (0.04) | (0.04) | (0.10) | (0.08) | (0.31) | (0.59) | (0.10) | (0.14) |
| *WINDFALL-2* | -0.04 | | | | | | | |
| | (0.04) | | | | | | | |
| R-squared | 0.10 | 0.09 | 0.55 | 0.28 | 0.39 | 0.32 | 0.93 | 0.69 |
| Observations | 1087 | 1113 | 143 | 235 | 99 | 174 | 43 | 61 |
| Countries | 63 | 63 | 41 | 40 | 27 | 27 | 14 | 13 |

Note: *-Significant at the 10 percent level; **-significant at the 5 percent level. The numbers in parentheses are robust standard errors, clustered at the country level. Dependent variable is $\Delta TGREV$. "*WINDFALL* -variables": "All" means that inference is based on both positive and negative observations of the *WINDFALL* -variable, ">0" means that inference is based exclusively on observations with two or more consequtive lags of positive realizations of the *WINDFALL* -variable, while "<0" means that only observations with at least two consequtive lags of negative realizations of the *WINDFALL* -variable are included. All regressions are fixed-effects regressions, including a one-year lag of the dependent variable, full sets of country- and year indicators, the baseline set of control variables, and each of the components of the respective WINDFALL-variables (with lags). R-squared reports on the within-variation.

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