WP/16/13

IMF Working Paper

Estimating Fiscal Multipliers with Correlated Heterogeneity

by Emmanouil Kitsios and Manasa Patnam

INTERNATIONAL MONETARY FUND

 WP/16/13

IMF Working Paper

Research Department

Estimating Fiscal Multipliers with Correlated Heterogeneity*

Prepared by Emmanouil Kitsios[1] and Manasa Patnam[‡]

Authorized for distribution by Steven Phillips

February 2016

Abstract

We estimate the average fiscal multiplier, allowing multipliers to be heterogeneous across countries or over time and correlated with the size of government spending. We demonstrate that this form of nonseparable unobserved heterogeneity is empirically relevant and address it by estimating a correlated random coefficient model. Using a panel dataset of 127 countries over the period 1994-2011, we show that not accounting for omitted heterogeneity produces a significant downward bias in conventional multiplier estimates. We rely on both cross-sectional and time-series variation in spending shocks, exploiting the differential effects of oil price shocks on fuel subsidies, to identify the average government spending multiplier. Our estimates of the average multiplier range between 1.4 and 1.6.

JEL Classification Numbers: E62, H23, C33

Keywords: Fiscal Multipliers, Nonseparable Unobserved Heterogeneity, Oil Price

Authors' E-Mail Addresses: ekitsios@imf.org; manasa.patnam@ensae.fr

---

[1] International Monetary Fund; [‡] CREST (ENSAE).
* We are grateful to Gustavo Adler, Jaebin Ahn, Céline Allard, Jorge Ivan Canales-Kriljenko, Tiago Cavalcanti, Benedict Clements, Giancarlo Corsetti, Mai Dao, Mitali Das, Xavier Debrun, Oliver DeGroot, Bill Dupor, Eric Gautier, Ruy Lama, Nan Li, David Newbery, Michael Plante, Issouf Samake, Sampawende Jules Tapsoba, Anne Villamil, and participants at various seminars for helpful comments.

Contents Page

Tables

Figures

I. Introduction

One of the most contested issues in formulating macroeconomic policy is the size of fiscal multipliers. The desirability of fiscal expansion or contraction often hinges on the magnitude of what is commonly termed the 'fiscal multiplier'. The fiscal multiplier is defined as the change in output for a given change in a fiscal policy instrument, such as total government spending. A correct causal estimate of the fiscal multiplier is, for example, critical to evaluating the short-term effects of fiscal consolidation decisions in times of recession, such as those taken by the governments of many advanced economies in the aftermath of the global financial crisis of 2008.[1]

Yet, the empirical identification of the fiscal policy effects on output remains challenging. A wide range of estimates for the fiscal multiplier at the national level have been proposed in the empirical macroeconomics literature with little consensus amongst them (Ramey, 2011). Government spending multipliers, in particular, have been estimated to be as low as negative and as high as above three. The large divergence in the multiplier estimates obtained in the literature suggests that the effects of government spending on output are heterogeneous, both across countries and over time.[2] In this paper, we offer an additional insight on the nature of this heterogeneity by showing that the fiscal multipliers vary systematically with the level of government spending and that this can, empirically, impede the identification of the fiscal multiplier.

From a theoretical point of view, there are several reasons to expect fiscal multipliers to be correlated with the size of government spending. Larger governments are associated with larger automatic stabilizers, which in turn tend to have a downward effect on the size of fiscal multipliers by containing the impact of discretionary fiscal

[1] See, for example, Blanchard and Leigh (2013) for a discussion on the policy implications of the forecasters' under-estimation of fiscal multipliers at the early stages of the recent financial crisis.

[2] This aspect has been recently brought to attention by several authors who have argued that fiscal multipliers vary systematically with features of the economy or the business cycle that are potentially also correlated with government spending, such as the phase of the business cycle, the exchange rate regime, the degrees of trade openness and government indebtedness, as well as the extent to which the zero lower bound on nominal interest rates is binding. Supportive evidence for the range of these conditional estimates can be found in Auerbach and Gorodnichenko (2012), Baum, Poplawski-Ribeiro, and Weber (2012), Blanchard and Leigh (2013), Christiano, Eichenbaum, and Rebelo (2011), Corsetti, Meier, and Müller (2012), Corsetti and others (2013), Erceg, Lindé, and Erceg (2014), Favero, Giavazzi, and Perego (2011), and Ilzetzki, Mendoza, and Végh (2013).

policy (Coenen and others, 2012). Higher levels of government spending are also associated with greater public indebtedness, which is likely to affect the fiscal multiplier either because of spending reversals anticipated in the future (Corsetti, Meier, and Müller, 2012), or because increases in sovereign risk premia increase funding costs in the private sector, exacerbating in this way the effects of cyclical shocks (Corsetti and others, 2013). A negative relation between the fiscal multiplier and the level of government spending can also arise when the latter is financed via distortionary taxation (Uhlig, 2010). This relation can be time-varying depending, for example, on whether monetary policy is constrained and, therefore, not responsive to increases in government spending (Christiano, Eichenbaum, and Rebelo, 2011; Eggertsson and Krugman, 2012; Woodford, 2011). Similarly, Erceg, Lindé, and Erceg (2014) show that the spending multiplier declines with the level of government spending in a liquidity trap. Parker (2011) argues that if the multiplier declines with the size of government spending, then countries with stronger countercyclical fiscal policy would experience during recessions multipliers that are lower than the average multiplier across countries.

While intuitive and founded on theoretical grounds, the possibility of incorporating non-additive forms of unobserved heterogeneity when estimating the relationship between government spending and output changes has not yet been explored empirically, to the best of our knowledge.

Our contribution in this paper is threefold: First, we argue that, when ignored, correlated heterogeneity may severely bias fiscal multiplier estimates. We provide evidence that multipliers are negatively correlated with the size of government spending in a panel dataset of 127 countries over the period 1994-2011.[3] We show that such omitted negative correlated heterogeneity causes a significant downward bias in OLS estimates of the average multiplier, which persists even if one controls for country-specific and time-varying heterogeneity.

Second, we identify the average government spending multiplier and address potential endogeneity concerns from time-varying omitted variables using a selectivity bias correction method proposed by Garen (1984), as well as a fixed-effect instrumental

[3] In the Appendix (Section VIII.A), we analytically motivate the finding of the negative correlation between the multiplier and the level of government spending by showing that countries with higher fiscal multipliers will optimally choose a lower level of government spending to minimize a given output gap.

variables estimator shown by Murtazashvili and Wooldridge (2008) to be consistent under correlated heterogeneity.[4] Our instrument for both these approaches exploits cross-country differences in fuel subsidies schemes that induce variation in government expenditures when exposed to fluctuations in oil prices. Governments in many countries adopt subsidy policies with respect to fuel pricing to support fuel consumption and insulate consumers against global oil price fluctuations. Our identification makes use of the fact that an upward oil price shock sharply increases government spending in high fuel subsidy countries relative to countries that do not subsidize fuel consumption as much. Since fuel subsidizing policies are endogenously chosen by governments, potentially in response to changing oil prices, we control directly for the effect of the fuel subsidy regime and global oil price changes on output growth. Our estimates for the fiscal multiplier after properly accounting for heterogeneity range between 1.4 and 1.6.

Finally, given that substantial heterogeneity exists, it is useful to assess the range of multiplier values both cross-sectionally and across time. We provide some evidence in this direction by estimating the distribution of the effect of government spending on output growth using quantile regressions. We compute the range of fiscal multipliers for a given country during its periods of recessions (low output growth) and booms (high output growth). Altogether, our estimates show that the effects of government spending on output are substantially heterogeneous. Taking into account the heterogeneity, we estimate the average effect to be positive and significantly above one.

Our approach offers distinct advantages in comparison to the previous literature. Our estimates of fiscal multipliers incorporate flexible forms of heterogeneity in at least two ways. First, our approach does not require the effect of government spending on output to be stationary. In doing so, we differ from the many studies relying on the DSGE or VAR approaches that assume that the effect of government spending to be time-invariant. As pointed out by Parker (2011), such an assumption would have the subtle implication that fiscal policy is as effective in a boom as it is in a recession. The time-invariance assumption is also employed in studies that exploit cross-sectional variation in government spending to secure identification. In addition, these studies

[4] Kraay (2012) uses a first-differenced instrumental variable estimator that is also, potentially, consistent under correlated heterogeneity, to identify the effect of government spending shocks on output growth for a sample of 29 low income countries. The effects are, however, assumed to be homogeneous.

typically assume that the multiplier is homogeneous across countries as it is across time.

Second, we do not impose any restrictions on the correlation structure of the heterogeneous coefficients, allowing them to co-vary with the explanatory variables. Here, we differ from the approach taken by the panel time-series literature that uses estimators which allow for heterogeneity in the slope parameters (see, for example, Pesaran (2006)), but rule out any correlation between the heterogeneous effects and the regressors. In contrast to these approaches, our estimation strategy accommodates time-varying additive and multiplicative forms of unobserved country-specific heterogeneity.

The rest of the chapter is structured as follows. Section II describes our identification and estimation strategy, Section III presents the data, Sections IV and V discuss the results, and Section VII concludes.

II. Empirical identification with heterogeneity

Accounting for correlated heterogeneity has important implications for the interpretation of the fiscal multipliers. To see this point, consider a simple model of government spending where output for country i at time t, Y_{it}, is a function of government expenditure at time t, G_{it}, and where both the coefficient and the intercept are allowed to vary across units:[5]

$$Y_{it} = \alpha + \beta G_{it} + \underbrace{(\alpha_i - \alpha) + (\beta_i - \beta) G_{it} + \epsilon_{it}}_{\text{Composite Error}}. \tag{1}$$

The above formulation shows that if we restrict the partial effects to be homogeneous across units, the composite error term will contain the heterogeneous coefficient and the intercept. The first term of the composite error term reflects omitted heterogeneity that is additive and can be differenced away using panel data. The second term represents multiplicative or nonseparable omitted heterogeneity that cannot be easily differenced away using standard panel data techniques. The average partial effect of

[5] Later, we consider a more general model, $Y_{it} = \beta_{it}(A_i, U_{it}) G_{it} + \alpha_{it}(A_i, U_{it})$, where we allow the coefficient and the intercept to be functions of time-invariant unit specific heterogeneity, A_i, and a time-varying disturbance U_{it}.

government spending is given by:

$$\beta = \mathbb{E}\left[\frac{\partial Y_{it}(G_{it})}{\partial G_{it}}\right] = \mathbb{E}[\beta_i]. \tag{2}$$

In contrast, the average parameter estimate obtained by the mean regression function in equation (1) does not identify a structural parameter, but identifies instead:

$$\frac{\partial \mathbb{E}[Y_{it}|\mathbf{G}]}{\partial G_{it}} = \beta + \underbrace{\left[\frac{\text{Cov}\big(G_{it},(\alpha_i - \alpha)\big) + \text{Cov}\big(G_{it},(\beta_i - \beta)G_{it}\big)}{\text{Var}(G_{it})}\right]}_{\text{Heterogeneity Bias}}. \tag{3}$$

This shows that the OLS estimate of the average partial effect will be biased unless the covariance between government expenditure and the heterogeneous coefficients is jointly zero.[6] The direction and magnitude of the bias depends on the correlation between the level of spending and the fiscal multiplier.

Our approach in this paper is to accommodate both unit and time-varying correlated heterogeneity in the multiplier estimates by modelling the effect of government spending on output using a correlated random coefficient structure. We use the correlated random coefficients (CRC) framework developed by Chamberlain (1992) and Graham and Powell (2012), where the panel dimension of the data helps identify the average partial effect of government spending on output. Our results, using the CRC model and the instrumental variable identification strategy, suggest a significant downward bias in OLS estimates as a result of omitted heterogeneity.

Before laying out our empirical strategy, we conduct a descriptive test to check for heterogeneity in our data. If the true model contains heterogeneous coefficients that are correlated with the values of government expenditure, then the entire path of G_{it}, may have predictive power for β. This suggests that the presence of heterogeneity bias will be signalled by including a full set of interactions of current government spending with its lags and leads in equation (1) or by including some polynomial

[6]A burgeoning literature exploits cross-sectional variation in government spending, mostly across sub-national units, to secure identification though an instrumental variable strategy. See, for example, Acconcia, Corsetti, and Simonelli (2014) and Serrato and Wingender (2010). An IV approach would also require strong conditions to estimate the average partial effect. In a cross-section case, (Heckman, Urzua, and Vytlacil, 2006) show that, in general, an IV strategy cannot identify the average partial effect when the heterogeneous coefficients are correlated with the endogenous variable even when the instrument is separately orthogonal to each. In Section II, we show how additional variation through panel data can help identify the effect.

function $f(\cdot)$ of the mean of these lags and leads (Chamberlain, 1982). Using this approach, we run some descriptive tests by estimating the following equations:

$$Y_{it} = \alpha+\beta G_{it}+(\alpha_i-\alpha)+\boldsymbol{\Omega_m} G_{it}\cdot f(\bar{G}_i-\mu_{\bar{G}})+\epsilon_{it} \tag{4}$$

$$Y_{it} = \alpha+\beta G_{it}+(\alpha_i-\alpha)+\sum_{s=1}^{T}\boldsymbol{\Omega_s} G_{it}\cdot G_{is}+\epsilon_{it}, \tag{5}$$

where Y_{it} is the logarithm of real GDP, G_{it} is the logarithm of total government spending and β_{it} is the elasticity of GDP with respect to total government spending. $\bar{G}_i$ denotes the country specific mean of G_{it} across the time period and $\mu_{\bar{G}}$ is the sample mean of $\bar{G}_i$. Table 3 reports results from this test. The first column presents estimates of the elasticity without the inclusion of polynomials of the mean interactions or the full set of lags and leads interactions. Column 2 includes the polynomials (up to three) of the interaction $G_{it}\cdot f(\bar{G}_i-\mu_{\bar{G}})$. This term is highly statistically significant indicating the presence of correlated heterogeneity. We find the same result when including the interactions of the full set of lags and leads in column 3. The value of the joint F-test statistic rejects the null hypothesis that the coefficients on the interaction variables are jointly equal to zero. Finally, we also run similar descriptive tests using a growth specification and obtain multiplier estimates, rather than output elasticities, where the dependent variable, i.e., the growth rate of real GDP, is regressed on the growth rate of total government spending. Similar to the elasticity specification, we find that both the mean interactions and the interactions with the full set of lags and leads are highly statistically significant, strongly suggesting that the impact multipliers are heterogeneous and correlated with government spending.

A. Identification using panel data

We now, formally, describe our approach to estimating the average partial effect of government spending by allowing the elasticity to vary over countries and to have an arbitrary correlation structure with the level of government spending. The panel structure of our data allows us to control for time-invariant heterogeneity by exploiting within-country variation in Y_{it} and G_{it}. We follow the approach of Chamberlain (1992) and Graham and Powell (2012) and estimate a correlated random coefficients (CRC) model. The approach is based on a generalized within-group transformation that “differences away” the unobserved correlated effects. A crucial assumption, implicit in this strategy, is that all regressors are strictly exogenous. In what follows, we provide a brief description of the identifying conditions and the estimation technique of the CRC model, assuming strict exogeneity of the regressors, and relax this assumption later in the next section. Our description of the approach is based largely on Graham and Powell (2012).

We start by allowing our parameters of interest in equation (1) to be time-varying. More generally, we represent these parameters as *functions* of two unobserved effects, a time-invariant unit specific heterogeneity, A_i, and a time-varying disturbance, U_{it}, and denote these functions as $b_{0it}(A_i, U_{it})$ and $b_{1it}(A_i, U_{it})$. Then, the logarithm of real GDP, Y_{it}, varies according to:[7]

$$Y_{it} = b_{0it}(A_i, U_{it}) + b_{1it}(A_i, U_{it})G_{it}, \tag{6}$$

where, as before, G_{it} is the logarithm of total government spending and b_{1it} is the country-by-period-specific elasticity of GDP with respect to total government spending. The coefficients are allowed to be correlated with the values of government expenditure in the sense that the entire path of G_{it} may have predictive power for $b_{0it}(A_i, U_{it})$ and $b_{1it}(A_i, U_{it})$.

To simplify notation, let $b_{it}(A_i, U_{it}) = (b_{0it}(A_i, U_{it}), b_{1it}(A_i, U_{it}))'$ and $\mathbf{G_{it}} = (1, G_{it})$ (intercept and logarithm of total government spending). Our interest is in identifying the vector of average partial effects $\boldsymbol{\beta} = \mathbb{E}[b_{it}(A_i, U_{it})]$. In compact form, the model can be rewritten as:

$$Y_{it} = \mathbf{G}_{it} b_{it}(A_i, U_{it}). \tag{7}$$

Note that the time-varying random coefficients on all regressors can nonlinearly depend on A_i and/or U_{it}. In addition to the maintained strict exogeneity assumption, the following conditions are required to hold under the CRC model (Graham and Powell, 2012):

Assumption 1. *CRC conditions:*

1.1 $b_{it}(A_i, U_{it}) = b_i^*(A_i, U_{it}) + d_{it}(U_{2,it})$ *for* $t = 1, \ldots, T$ *and* $U_{it} = \left(U_{1,it}, U_{2,it}\right)'$.

1.2 $U_{it} | \mathbf{G_i}, A_i \stackrel{D}{=} U_{is} | \mathbf{G_i}, A_i$ *for* $t = 1, \ldots, T$ *and* $t \neq s$.

[7] In this section, for ease of notation, our model contains only one regressor and a constant. However, in our empirical model we consider a more general specification with additional conditioning covariates.

1.3 $U_{2it}|\mathbf{G_i}, A_i \overset{D}{=} U_{2it}$.

Assumption (1.1) states that the random coefficient consists of a "stationary" and a "nonstationary" component. The stationary part, $b_i^*(A_i, U_{it})$, does not vary over time while the nonstationary part, $d_{it}(U_{2,it})$, allows both the functional form and/or the actual measure of the unobserved time-varying component to vary over time. Assumption (1.2) imposes marginal stationarity of U_{it} given $\mathbf{G_i}$ and A_i which means that the joint distribution of (A_i, U_{it}) given $\mathbf{G_i}$ does not depend on t. This assumption rules out time-varying heteroscedasticity but still allows for serial dependence in U_{it}. Assumption (1.3) requires movements in the time-varying component of the random coefficient, U_{it}, to be idiosyncratic, i.e., independent of $\mathbf{G_i}$ and A_i.

Under these conditions, Graham and Powell (2012) show that the average partial effects, can be obtained in the following way. First, note that Assumptions (1.1)-(1.3) imply that:

$$\begin{aligned}
\mathbb{E}[b_{it}(A_i, U_{it})|\mathbf{G_i}] &= \mathbb{E}[b_i^*(A_i, U_{it})|\mathbf{G_i}] + \mathbb{E}[d_{it}(U_{2,it})|\mathbf{G_i}] &(8)\\
&= \mathbb{E}[b_i^*(A_i, U_{i1})|\mathbf{G_i}] + \mathbb{E}[d_{it}(U_{2,i1})] &(9)\\
&= \boldsymbol{\beta_i}(\mathbf{G_i}) + \boldsymbol{\delta_t}, \ \forall \ t = 1, ..., T. &(10)
\end{aligned}$$

where $\boldsymbol{\delta}$ denotes the vector of aggregate shifts in the random coefficients over time.[8] This essentially, implies that the random coefficient consists of a unit-specific, time-invariant, function of the underlying regressors and a time-varying, aggregate, component that is common to all cross-sectional units. In a nutshell, the identification strategy comprises of exploiting the combined, cross-sectional and time-series, variation present in panel data. This is done by, first, using a within-group estimator to obtain the time-varying aggregate shift parameters, $\boldsymbol{\delta}$. The within-group estimator differences away the time-invariant unobserved heterogeneity, $\boldsymbol{\beta_i}(\mathbf{G_i})$, so as to identify $\boldsymbol{\delta}$ using the remaining differenced out cross-sectional variation. In the second step, we use our estimate of $\boldsymbol{\delta}$ to detrend the vector of outcomes. The unit-specific random coefficients are then obtained as the generalized least squares fits of each unit's detrended time-series of outcomes vis-a-vis the regressors. Finally, the average partial

[8] Equation (32) is equivalent to a common-trends assumption, i.e., the differences in the coefficient values between two time periods are equal, and equal to the difference between the aggregate time trends, regardless of the regressor histories. Formally, consider two regressor histories $\mathbf{G_i}$ and $\mathbf{G_i^\dagger}$: $\mathbb{E}[b_{it}(A_i, U_{it})|\mathbf{G_i}] - \mathbb{E}[b_{is}(A_i, U_{is})|\mathbf{G_i}] = \mathbb{E}[b_{it}(A_i, U_{it})|\mathbf{G_i^\dagger}] - \mathbb{E}[b_{is}(A_i, U_{is})|\mathbf{G_i^\dagger}] = \boldsymbol{\delta_t} - \boldsymbol{\delta_s}$.

effect, $\boldsymbol{\beta}$, is identified by the mean of the, estimated, unit-specific coefficients. Further details of the estimation procedure are provided in Section VIII.B of the appendix.

Overall, the CRC model allows incorporating heterogeneity in a flexible way as it does not restrict the form of the heterogeneity present in the data. Despite this advantage, one shortcoming of the approach is that it relies on the assumption of strict exogeneity of the regressors. As a result, our estimates from this model would be biased if there are time-varying omitted variables that influence both government expenditure and output. Nevertheless, the comparison of OLS and CRC estimates still provides a useful measure of the extent of bias due to heterogeneity. In the next section, we discuss our identification strategy which is robust to heterogeneity and the presence of time-varying omitted variables.

B. Identification using panel data and instrumental variables

As noted above, the CRC model is unable to accomodate the bias due to omitted time-varying unobservables. In order to address this concern together with accounting for unobserved heterogeneity, we rely on instrumental variables in a panel dimension utilizing the framework provided by Murtazashvili and Wooldridge (2008). The authors show that a fixed-effects approach combined with instrumental variables can be used to consistently estimate the average partial effect in the presence of correlated heterogeneity. We start with eliminating α_i from equation (1) by first differencing our variables together with using a fixed-effects estimator. Apart from being able to eliminate the additive heterogeneity, another advantage of using growth indicators, rather than logarithmic values, is that the average partial effect can be interpreted directly as the impact multiplier. In contrast, an elasticity obtained from a log-log specification needs to be multiplied with the average ratio of GDP to government spending to be interpreted as a multiplier. We rewrite the model with variables “detrended” of their individual-specific trends:

$$\ddot{\Delta} Y_{it} = \beta^M \ddot{\Delta}\mathbb{G}_{it} + (\beta_i^M - \beta^M)\ddot{\Delta}\mathbb{G}_{it} + \ddot{\epsilon}_{it}. \tag{11}$$

Here, $\ddot{\Delta} Y_{it}$ is the detrended measure of ΔY_{it} and denotes the annual growth rate in real GDP. $\ddot{\Delta}\mathbb{G}_{it}$ is the detrended measure of $\Delta\mathbb{G}_{it}$ and denotes the annual change in total government spending scaled by the lagged level of real GDP.[9] $\ddot{\epsilon}_{it}$ denotes the time-varying country-specific error term. The government spending multiplier is defined as the change in output brought about by a change in government spending. Thus, in our specification, β_i^M captures the country specific government spending multiplier.

Next, we consider an instrumental variable $\ddot{Z}_{it}$, also detrended, that can be used to predict the endogenous variable $\ddot{\Delta}\mathbb{G}_{it}$, as follows:

$$\ddot{\Delta}\mathbb{G}_{it} = \pi \ddot{Z}_{it} + \ddot{v}_{it}. \tag{12}$$

Before discussing the conditions under which the FE-2SLS is consistent, we provide a detailed description of our chosen instrument.

C. Identifying instrument: Fuel subsidies and oil price shocks

We construct our instrument using variation from the differential effects of an international oil price shock on the government spending across the various fuel subsidy regimes. Fossil fuel subsidies are broadly classified into consumer and producer subsidies. Consumer subsidies for oil products, such as gasoline and diesel, are widespread and are associated, in many cases, with substantial fiscal burden for the country that adopts them. They are typically measured by comparing the final consumer prices in each country to a benchmark price which represents a 'normal sales' price. 'Normal sales' prices for fuels, in turn, depend on factors such as crude oil prices, costs of production, demand forces, the market structure, as well as distribution and transportation costs.

Oil and its products are globally traded, and, therefore, their trade prices do not vary significantly across countries. Significant differences in retail fuel prices primarily arise from the different fuel pricing policies pursued by each country. Retail prices that are above the 'normal sales' price level indicate that the country is taxing domestic fuel consumption. On the contrary, when the retail price of a fuel is lower than its reference price, then the country is considered to subsidize its consumption.

[9] The growth in total government spending is scaled by the lagged level of real GDP rather than by the lagged level of total government spending, i.e., $\Delta\mathbb{G}_{it} = \frac{g_{it} - g_{it-1}}{y_{it-1}}$, where g and y represent the levels of total government spending and real GDP, respectively.

Coady and others (2010) show that both pre-tax and post-tax subsidies constitute a significant proportion of both GDP and government expenditure. They report that the average pre-tax subsidy is 4% ($1.47 billion) while the average post-tax subsidy is around 9% ($3.6 billion). Amongst countries that provide fuel subsidies, the average pre-tax and post-tax subsidies are 7% ($2.6 billion) and 16% ($6.4 billion), respectively. These subsidies can strongly affect government expenditure as can be seen, for instance, in the case of Indonesia whose pre-tax and post-tax subsidies are about 60% and 85% respectively of total government expenditure (Coady and others, 2010).

There are two benchmark prices that are often adopted in cross-country comparisons of fuel pricing policies: crude oil prices and the U.S. average retail prices. The first benchmark is used because crude oil is the primary input in the production of fuels and countries that set retail prices below the price of this primary input, despite subsequent value additions, are considered to heavily subsidize oil products. The second benchmark, i.e., the average U.S. retail price, is used because the U.S. market for fuels is characterized by intense market competition and low taxation.[10] As a result, GIZ (2012) distinguishes between the fuel taxation and subsidy regimes based on the U.S. retail price which comprises of the industry margin, the Value Added Tax and a minimal tax of approximately $0.10 per litre for financing the federal and state road funds. According to GIZ (2012), a taxation level of 10 US cents per litre is sufficient to cover road maintenance costs in most developed and developing countries.

To see how retail prices vary across countries as a result of these subsidies, we plot in Figure 1 the variation in the average gasoline and diesel prices across countries for the year 2010. The graph also plots for reference the international 'Brent' price of oil in 2010. It is evident that there is a wide, cross-sectional, dispersion in the retail prices of gasoline and diesel across countries that depends on the level of fuel subsidies provided, as explained above. What is more important is that we find within-country, time-series variation in retail prices of gasoline and diesel, as fuel subsidy policies of countries are likely to change over time (GIZ, 2012). Table 2 shows the transition matrix with respect to fuel and gas subsidies for our pooled country-year sample observations. Figures highlighted in bold indicate observations that change regimes over time. The percentage of country-year observation switches across regimes are 6% and 10% for gasoline and diesel, respectively. A large part of this total transi-

[10]For many years, the U.S. has adopted a very low fuel taxation policy. To verify that the fuel taxes in the U.S. are the lowest among the industrialized countries, see Tables 8, 9 and 10 of IEA (2013, pp. 297-299).

tion is on behalf of countries that switch from low-to-no subsidy, high-to-low subsidy and low-to-high subsidy for both gasoline and diesel. We have, therefore, substantial variation in the status of subsidy regime even within-country and over time. This variation is crucial for our identification strategy which is discussed in detail in Section II.D below.

Our identification strategy combines the joint effect of an oil price shock and the fuel subsidy regime, whilst implicitly controlling for the direct impact on output growth from each of these two effects. To highlight this joint effect, we examine the temporal distribution of international vis-a-vis domestic oil prices over time. Figure 2 plots the international and domestic prices of oil and gasoline across all subsidy regimes. We also plot the domestic price of gasoline in the U.S., our price benchmark for the low subsidy regime classification. The graph depicts our chosen classification in a clear manner. It shows that the domestic retail price for gasoline in the no subsidy regime remained well above both the international oil price and the U.S. gasoline price throughout the entire period. Similarly, it can be seen that the domestic retail price for gasoline in the high subsidy regime remained well below both the international oil price and the U.S. gasoline price throughout the entire period. The domestic retail price for gasoline in the low subsidy regime (excluding the U.S.) trended between the prices set by the two benchmarks, i.e., the international Brent and the U.S. domestic gasoline prices.

To construct our cross-country and time-varying instrument, we interact a country-specific measure of oil-price shocks, O_{it}, with a variable, S_{it}^{Gas}, that captures the type of gasoline subsidy scheme that is implemented in the country. We define the gasoline subsidy scheme as follows:

- $S_{it}^{Gas} = 2$: The country implements a high subsidy scheme for gasoline when its domestic retail pump price is below the price level of crude oil (Brent).[11]
- $S_{it}^{Gas} = 1$: The country implements a low subsidy scheme for gasoline when its domestic retail pump price is above the price level of crude oil (Brent), but below the average price level of gasoline found in the US.

[11] The ‘pump price’ is the retail price of gasoline. Further details on the data used are provided in Section III.

- $S_{it}^{Gas} = 0$: The country does not implement any subsidy scheme for gasoline when its domestic retail pump price is above the price level of crude oil (Brent), as well as above the average price level of gasoline found in the US.

The variable S_{it}^{Diesel} is constructed in a similar way to the one described above for gasoline. The oil price shock for each country, O_{it}, is measured as the product of the log-change of the crude oil price $\Delta ln(OilPrice)_t$ with the country's average ratio of net oil exports over GDP, θ_i:[12]

$$O_{it} = \Delta ln(OilPrice)_t \cdot \theta_i. \tag{13}$$

We **instrument** for changes in total government spending using the variable:

$$Z_{it} = S_{it-1}^{Gas} \cdot O_{it-1}. \tag{14}$$

We include as regressors the oil price shock, as well as the lagged values of domestic gasoline and diesel subsidy regimes. In this way, we allow for the direct effects of oil price shock and fuel subsidy regimes on output growth. Hence, we include O_{it}, S_{it-1}^{Gas}, S_{it-1}^{Diesel}, as well as year fixed-effects to capture year-on changes in international oil prices.[13]

Vector $\mathbf{X}_{it-1}^{'}$ is used in some specifications to check the robustness of our results to the inclusion of additional regressors, such as change in the value of net imports and past changes in the growth rate of real GDP.

Figure 3 provides an illustration of our identification strategy and the first-stage effect by plotting the change in international (Brent) oil prices and the changes in government spending for all three regimes over the period 1995-2010. This graph clearly shows how the changes in government expenditure fluctuated in accordance with

[12]See, also, Brückner, Chong, and Gradstein (2012) for a similar definition of the oil price shock.

[13]Note that we include the current oil price shock as a control variable while using its first lag as an instrument. This specification is consistent with the findings of Brückner, Chong, and Gradstein (2012) who find that only the first lag of an oil price shock has a significant and positive effect on change in government spending while its impact and lead effects are statistically insignificant. In contrast, they find that current oil price shocks have a significant positive effect on output growth on impact but its lead and lagged effects are insignificant. We show in Section IV that we obtain similar results. Therefore, we exclude further lags of the oil price shock in the second stage not only because they have an insignificant effect on output growth, but also because they weaken the instrument set, as only the first lag of the oil price shock is informative in predicting change in government spending.

the change in international oil prices for the high subsidy regime. We find a sharp increase in government consumption for this regime around the years 2006-2008, when international oil prices were at their peak. This can be explained by the fact that governments belonging to the high subsidy regime had to fund the difference between the wholesale (international) and retail (domestic) prices by sharply increasing their budgetary expenditure relative to other years to stabilize domestic gasoline prices. Interestingly, government consumption under the low- and no-subsidy regimes is much less volatile during the same period, and follows a pattern that is very different from the one observed for the volatility in the international oil price.

We, therefore, have reasons to expect that the oil price shocks positively affect government consumption in high subsidy regimes relative to the low- and no-subsidy regimes. Our first-stage estimates reported in Section IV verify this descriptive analysis.

D. Identifying conditions and estimation

Given the availability of this instrument, the FE-2SLS is consistent under the following conditions (Murtazashvili and Wooldridge, 2008):[14]

Assumption 2. *FE-IV conditions:*

2.1 $\mathbb{E}[\ddot{\epsilon}_{it}|\ddot{Z}_{it}] = 0$.

2.2 $\mathbb{E}[\beta_i^M|\ddot{Z}_{it}] = \mathbb{E}[\beta_i^M] = \beta^M$.

2.3 $Cov(\ddot{\Delta}\mathbb{G}_{it}, \beta_i^M|\ddot{Z}_{it}) = Cov(\ddot{\Delta}\mathbb{G}_{it}, \beta_i^M)$.

Assumption (2.3) implies that the conditional covariances of the random coefficient and the regressor should equal their unconditional covariances. This implies that $\mathbb{E}[(\beta_i^M - \beta^M)\ddot{\Delta}\mathbb{G}_{it}|\ddot{Z}_{it}] = \mathbb{E}[(\beta_i^M - \beta^M)\ddot{\Delta}\mathbb{G}_{it}] = \gamma_t$. Note that we allow for the unconditional covariances to change over time. As a result, $(\beta_i^M - \beta^M)\ddot{\Delta}\mathbb{G}_{it} = \gamma_t + r_{it}$, where r_{it} denotes a random disturbance, and:

$$\ddot{\Delta}Y_{it} = \beta^M \ddot{\Delta}\mathbb{G}_{it} + \gamma_t + (r_{it} + \ddot{\epsilon}_{it}). \tag{15}$$

[14] Murtazashvili and Wooldridge (2008) note that these conditions are most likely to apply when the endogenous explanatory variables are continuous, as in our context.

Thus, our specification explicitly includes time dummies, as required by condition (2.3). Assumption (2.2) is critical to our identification strategy and states that the correlated random coefficient β_i^M is mean independent of all the unit-specific detrended instruments. However, this is a weaker assumption than full independence (of the instrument and coefficient) because it allows β_i^M to be arbitrarily correlated with systematic components of Z_{it}. Therefore, in our context we only require that the idiosyncratic movements in Z_{it} – the oil price shocks – be uncorrelated with β_i^M. This is a reasonable condition that is most likely met in our data since oil price movements are global and not country specific. Finally, Assumption (2.1) requires that the unit-specific detrended instruments be independent of the unobserved error component $\ddot{\epsilon}_{it}$. Our specification includes controls for the chosen fuel subsidy regime and global oil price changes. These variables capture the direct effects of the endogenously chosen fuel subsidizing policy, potentially in response to changing oil prices, on output growth. Our exclusion, therefore, requires that conditional on the choice of the fuel subsidizing policy, the interactive effect of oil price shocks and the subsidy regime has no direct effect on output growth. We devote Section V.B to discuss and ensure the validity of this assumption. In addition to these three assumptions, we also need the standard rank condition to be satisfied. For this, we require that there is still sufficient correlation between the instrument and endogenous regressor after netting out the individual specific trends. We also require that the de-trended instrument contains sufficient variation. As shown in Figure 3 and in the subsequently reported first-stage estimates, both parts of this condition are satisfied in the data.

These conditions imply that $\mathbb{E}[\ddot{\epsilon}_{it}|\ddot{Z}_{it}] = 0$ and $\mathbb{E}[r_{it}|\ddot{Z}_{it}] = 0$, so that the IV fixed-effects method using instruments $\ddot{Z}_{it}$, consistently estimates the average partial effect β^M. We use a two-stage least squares estimator to estimate the parameters, adjusting for heteroskedasticity in the variance estimates.

We also use an alternative estimation strategy provided by (Garen, 1984) that, although somewhat restrictive, provides a useful measure of the extent of bias due to correlated heterogeneity.[15] The parametric 'control-function' approach developed by (Garen, 1984) makes use of the estimated residuals from the first-stage (equation

[15] (Garen, 1984) requires Assumptions (2.1)-(2.2) to hold as before, but adds the additional restrictions $\mathbb{E}[\beta_i^M|\ddot{Z}_{it}] = 0$, $\mathbb{E}[\ddot{\epsilon}_{it}|\ddot{\Delta}G_{it}, \ddot{Z}_{it}] = \lambda_G \ddot{\Delta}G_{it} + \lambda_Z \ddot{Z}_{it}$, and $\mathbb{E}[\beta_i^M|\ddot{\Delta}G_{it}, \ddot{Z}_{it}] = \psi_G \ddot{\Delta}G_{it} + \psi_Z \ddot{Z}_{it}$. Effectively, the combination of all these assumptions imply that $\mathbb{E}[\ddot{\epsilon}_{it}|\ddot{\Delta}G_{it}, \ddot{Z}_{it}] = \lambda_G \ddot{\Delta}G_{it}$ and $\mathbb{E}[\beta_i^M|\ddot{\Delta}G_{it}, \ddot{Z}_{it}] = \psi_G \ddot{\Delta}G_{it}$. Replacing these assumptions into equation (1) produces the convenient formulation of equation (16).

(12)) and plugs them in the structural equation of interest to, effectively, purge out the bias. Specifically, we estimate:

$$\ddot{\Delta} Y_{it} = \beta^M \ddot{\Delta}\mathbb{G}_{it} + \lambda_G \widehat{\ddot{v}}_{it} + \psi_G \ddot{\Delta}\mathbb{G}_{it} \cdot \widehat{\ddot{v}}_{it} + \ddot{e}_{it}. \tag{16}$$

The coefficient $\lambda_G = \text{Cov}(\ddot{e}_{it}, \ddot{v}_{it})/\text{Var}(\ddot{v}_{it})$ provides a measure of the extent of bias due to omitted variables while the coefficient $\psi_G = \text{Cov}(\beta_i^M, \ddot{v}_{it})/\text{Var}(\ddot{v}_{it})$ provides a measure of the extent of bias due to correlated heterogeneity. The control function method is estimated using a weighted least squares that adjusts for the heteroskedasticity due to the inclusion of the estimated first-stage residuals.[16]

III. Data sources

This section provides a description of the data used in our analysis. The data for our main variables of interest, real GDP and total government expenditure, are obtained from the 2013 edition of the World Bank's World Development Indicators Database. The same source provides data on oil imports and exports that we used to calculate each country's average ratio of net oil exports over GDP, θ_i. The database was also used to retrieve data for other macroeconomic variables, such as net imports of goods and services, government revenue, tax revenue over GDP and inflation rates, which were used in our robustness checks.

We constructed our fuel subsidy index using data on domestic pump prices for gasoline and diesel from the German Agency for International Cooperation[17] (GIZ, formerly GTZ) and crude oil (Brent) price data from the BP Statistical Review of World Energy.[18] The GIZ collects information for retail prices of gasoline and diesel in over 170 countries since 1991. The primary source for data on industrialized countries is the German Automobile Club in the EU, whereas data for developing countries are based on locally administered price surveys. The GIZ data reports retail prices using

[16] The variance of the disturbance term in equation (16) is a function of $\ddot{\Delta}\mathbb{G}_{it}$ and $(\ddot{\Delta}\mathbb{G}_{it})^2$. To obtain consistent estimates, we first regress $\ddot{\Delta}\mathbb{G}_{it}$ and $(\ddot{\Delta}\mathbb{G}_{it})^2$ on the squared residuals from equation (16) and use this to estimate the variance matrix of disturbances used in weighted least squares.

[17] Access to the data is provided via the GIZ publication *International Fuel Prices* (www.giz.de/fuelprices) and the World Development Indicators database maintained by the World Bank.

[18] The price data and the BP Statistical Review of World Energy are available at http://www.bp.com/statisticalreview.

nationwide average filling station fuel price statistics, i.e., the 'pump price' for European countries. For all other countries, fuel prices posted at the filling stations in the respective capital cities were used (GIZ, 2012). Given that the GIZ survey on domestic fuel prices is biennial, we imputed the missing values on the retail pump prices using the average retail pump price from the previous and the subsequent year for which the data is available. All prices are expressed in U.S. dollars per litre. We assign countries into three types of fuel subsidy schemes following the categorization described earlier in Section II.C. Similar classification criteria are adopted by GIZ (2012) which further categorizes countries into those that adopt either a high or a low fuel taxation scheme.[19]

The summary statistics for the sample data and variables used are provided in Table 1. All macroeconomic variables are measured in constant 2000 U.S. dollars.

IV. Results

We first report results from the CRC model. The dependent variable for all specifications is the logarithmic value of real GDP which is regressed on the logarithmic value of total government spending. The estimated average partial effects, therefore, yield the average elasticities of GDP with respect to government spending. We use the years 2010, 2011 and 2012 as our sample for estimation.[20] Column 1 of Table 4 presents the elasticity estimates from a fixed-effects OLS specification without intercept or coefficient shifters. The estimated elasticity is low, at 0.073, implying that a 1% increase in government expenditure increases GDP by 0.073%. Column 2 adds intercept shifters and time-varying elasticities. The results remain unchanged with the elasticities ranging between 0.07 and 0.066. Finally, in column 3 we report Chamberlain's (1992) CRC method estimates which include intercept shifters as well as time-varying coefficients. The CRC point estimates are much larger than the FE-OLS estimates, indicating that the OLS estimates are downward biased as a result of correlated het-

[19] More specifically, GIZ (2012) distinguishes between the high and the low fuel taxation categories depending on whether the retail price of gasoline (or diesel) is above the price level of the United States and above the lowest price that can be found among EU countries.

[20] With only two random coefficients and three years of data, our model is overidentified. In principle, one can use all the available time periods to estimate the CRC model. However, adding more time periods than necessary results in the model becoming heavily overidentified and the structural parameter becoming a more complicated function of the underlying reduced form parameters.

erogeneity. The CRC estimate for the elasticity is stable at around 0.38, which implies a multiplier of approximately 1.5 when evaluated at the sample average ratio of GDP to government expenditure.

In what follows, we report results from our fixed-effects instrumental variables strategy. All specifications in Table 5 include country and year fixed-effects. In the first column of Table 5 we present the FE-OLS estimation results for equation (11). The FE-OLS estimate of the government spending multiplier is 0.236, which is statistically significantly different from zero at the five percent level. The second column shows the estimates of the first-stage effects that the suggested instrument has on the change of total government consumption following the specification provided in equation (12). The main conclusion from these estimates is that the interaction term of the country-specific oil price shock and the index for the gasoline subsidy scheme exerts a positive and highly significant effect on changes in total government expenditure. The economic rationale behind the first-stage results was explained in the discussion of Figure 3 in Section II.C.

Using this interaction term as an instrument for the changes in government spending, we present the two-stage least squares (2SLS) estimates of the government spending multiplier in the third column of Table 5. The first-stage F-statistic is 14.1 and, therefore, exceeds the Staiger and Stock (1997) rule-of-thumb threshold of 10 below which instruments are considered weak. The point estimate of the government spending multiplier in our baseline 2SLS specification is 1.45, which is significant at the 5 percent significance level and well above the OLS estimate of column 1. In column 4, we present results from an alternative control function strategy corresponding to equation (16), as developed by (Garen, 1984). Given the linear conditional expectations restrictions, this specification provides a simple test for the direction and magnitude of the heterogeneity bias. As per equation (3) and equation (16), the direction of the heterogeneity bias depends on the impact of time-varying omitted unobservables, λ_G, as well as the extent of correlation between the multiplier heterogeneity and government spending, ψ_G.

Column 4 of Table 5 reports three findings. Firstly, consistent with the CRC and FE-IV results, we find that the control function approach yields estimates for the average multiplier that are much higher than the OLS. Second, the estimated coefficient of the first control function, λ_G, is negative and highly significant. This implies that the omitted variables bias in the conventional FE-OLS estimate is non-negligible. The negative sign suggests that there are time-varying unobservables that, while positively correlated with government spending growth, are negatively correlated with GDP growth. An example of such an unobservable is the counter-cyclical nature of fiscal policy. Time-varying counter-cyclical fiscal policy would imply that periods of higher output growth are likely to coincide with periods of relatively lower government spending and vice versa. As a result, the omission of this latent variable can induce a downward bias in conventional OLS estimates. Third, we find that the coefficient estimate on the selection bias control function, ψ_G, is also negative and highly significant. This means that the negative effect of the time-varying unobservables is larger at higher levels of government spending growth. This confirms our earlier finding that the heterogeneity in the fiscal multiplier estimates is negatively correlated with government spending.

In Table 6 we check the robustness of our baseline estimate of the government spending multiplier by successively including more control variables. All estimates of the multiplier lie in the range between 1.4 and 1.57. Our results are robust to the inclusion of changes in the price of oil (column 1 and columns 3-8), the current and lagged logarithmic values of domestic gasoline and diesel pump prices (columns 2-6), as well as the change in the prices of gasoline and diesel (columns 7 and 8). Further, the estimates of the government spending multiplier remain stable when additional variables such as the annual change in net imports of goods and services scaled by lagged GDP (columns 5-8) and the lagged change in real GDP growth (columns 6 and 8) are included.

Our estimated effects of oil shocks on government expenditure change and output growth are largely consistent with those found in Brückner, Chong, and Gradstein (2012) who estimate the permanent income elasticity of government spending. In their paper, the authors find that only the first lag of oil shock has a significant and positive effect on change in government spending while its impact and lead effects are statistically insignificant. Further, they find that current oil price shocks have a significant positive effect on output growth on impact but its lead and lagged effects are insignificant. Our results differ slightly from their findings. Similar to them, we find that the first lag of the oil price shock has a significant and positive effect on change in government spending but, contrary to their result, we do not find a significant effect of current oil shock on output growth. This could be due to two reasons: first, in our specification we additionally condition on domestic gasoline and diesel prices which may reflect most of the impact; secondly, in comparison to their sample, we analyze a much shorter and different time period.[21] In line with Brückner, Chong, and Gradstein (2012), we use the lagged oil price shock as part of the instrument set but we also condition on the current oil price shock in the second stage since it has a direct effect on output growth. We exclude further lags of the oil price shock in the second stage not only because they have an insignificant effect on output growth, but also because they weaken the instrument set, as only the first lag of the oil price shock is informative in predicting changes in government spending.

The first-stage F-statistic reported in every column of Table 6 remains above 13 and, therefore, our estimations pass the weak instrument test. The estimate of the government spending multiplier lies in the vicinity of 1.5 and remains positive and statistically significant at the 5% level in most specifications (i.e., columns 2-6).

V. Robustness checks

In this section, we assess the sensitivity of our results to various robust inference schemes, as well as to the presence of outliers. We also qualitatively assess the validity of our instrumental variable strategy.

A. Inference and outliers

In our original specification, we use heteroscedasticity and autocorrelation (HAC) robust standard errors for inference and allow for a bandwidth of up to 2 lags for autocorrelation robust inference. In what follows, we use various standard error correction techniques to derive inference. Panel A of Table 7 reports our results for this exercise. All estimates are based on the specification reported in column 5 of Table 6. We present both 90% and 95% confidence intervals for each result. Column 1 clusters standard errors on both country and year identifiers. Our main result remains significant at the 10% level, despite that clustering by country and year increases standard errors by a slight margin. There is probably little to be gained from clustering on these units, given that we include country and year fixed-effects in our specification which tend to absorb the majority of the within-country and within-time heterogeneity. Conditional on fixed-effects, a more serious threat to our inference approach

[21] Brückner, Chong, and Gradstein (2012) analyze data between 1960-2007 compared to our analysis which spans 1992-2010.

comes from arbitrary cross-sectional dependence between unobservables (for example, within regions, continents etc.). We explore this issue below.

In the next two columns, we calculate standard errors accounting for potential cross-sectional dependence. The assumption that the disturbances of a panel model are cross-sectionally independent is often found inappropriate. For example, the presence of trade links, regional integration and other similar factors may induce dependence amongst unobservables across countries at any time period. Such unobservable common factors, although uncorrelated with the explanatory variables, can bias the standard error estimates, thereby invalidating any statistical inference based on them. We, therefore, account for the possibility that cross-sectional dependence may be present to assess the sensitivity of our chosen (HAC robust) inference approach. First, we consider cross-sectional dependence due to spatial correlation. Following Conley (1999) we compute nonparametric estimates of the variance-covariance matrix that allow for contemporaneous spatial correlations between countries whose centroids lie within 1000 kilometers of one another. In addition, nonparametric estimates of country-specific serial correlation are estimated using linear weights that decay to zero after a lag length of 4. This ensures that our inference is adjusted to account for heteroscedasticity, country-specific serial correlation, and cross-sectional spatial correlation. The spatial dependence adjusted standard-errors are reported in column 2. The increase in standard errors is very small and our estimates remain positive and significant at the 5% level, despite that we account for spatial cross-sectional dependence. However, it is possible that the cross-sectional dependence is not entirely spatially driven.

To allow for a more general form of cross-sectional dependence, we calculate Driscoll and Kraay (1998) proposed standard errors, accounting for our panel dimension (Hoechle, 2007), and report these results in column 3. Driscoll and Kraay (1998) propose a technique to compute a nonparametric covariance matrix estimator that produces heteroscedasticity consistent standard errors that are robust to very general forms of spatial and temporal dependence. As before, we find that our inference is robust to accommodating even these general forms of spatial and temporal dependence and our estimates remain significant at the 5% level.

In all our specifications, we report strong first-stage F-statistics that are greater than 15. Nevertheless, in column 4, we account for the possibility that our identification is based on weak instruments and compute confidence intervals (CI's) that are robust to weak instruments as developed by Chernozhukov and Hansen (2008). Their 'dual' inference procedure involves constructing confidence intervals through a linear regression of a transformed dependent variable, $Y - \beta^M \mathbf{X}$, on the set of instruments, $\mathbf{Z}$, and then testing that the coefficients on $\mathbf{Z}$ are equal to 0 using a conventional robust covariance matrix estimator. The resulting confidence intervals are also robust to heteroscedasticity and autocorrelation. Our results indicate that the weak instrument robust confidence intervals are quite close to our conventional confidence bands [0.195, 3.361] compared to the original [0.118, 2.98] at 95%. Although slightly wider, they indicate a shift to the right of the distribution so that the lower bound of the weak instrument robust CI is farther away from zero, compared to the lower bound obtained from conventional CI's. We have, therefore, demonstrated that our usual HAC adjusted inference approach is robust to and stable across various inference correction procedures adopted.

Finally, in Panel B of Table 7 we consider the sensitivity of our estimates to dropping influential observations and outliers. We use two measures of leverage to drop outliers, Cook's Distance and DFFITS (Belsley, Kuh, and Welsch, 1980). Both statistics measure how much an observation influences the model as a whole. While Cook's distance measures the aggregate change in the estimated coefficients when each observation is left out of the estimation, the DFFITS statistic measures the change in the predicted value for each observation when that observation is left out of the regression. Using both techniques, we find that our IV results decrease in magnitude by a small amount, from 1.55 to 1.27 but are still positive, significant and greater than one. For both methods, the first-stage F-statistic reduces slightly but remains above the Staiger and Stock (1997) rule of 10.

Overall, despite the small variability in point estimates due to dropping outliers, our broad set of robustness checks confirms that our results are stable, both in terms of estimation and inference. Our main finding, that the fiscal multiplier is positive, significant, and fairly large is consistent across all robustness checks.

B. Instrument validity

Our identification strategy exploits cross-country differences in fuel subsidies schemes that induce variation in government expenditures when exposed to fluctuations in oil prices. We make use of the fact that an oil price shock sharply increases government spending in high fuel subsidy countries relative to countries that do not subsidize fuel consumption as much. In this way, we expect our instrument to be correlated with total government expenditure. Once we control directly for the effect of changes in retail oil prices, as well as global oil price changes (via the inclusion of year fixed-effects), we should expect no direct effect of our instrument on output.

In this sub-section we present evidence to corroborate the validity of our instrumental variable strategy. We explore three potential threats to identification: (1) impact of fuel subsidy on government revenue; (2) indirect impact of fuel subsidy on inflation and (3) impact of fuel subsidy on tax revenue. In effect, we examine if fuel subsidies are correlated with factors that themselves are correlated with output growth, since this could invalidate our identification strategy. We take up each one of these concerns below.

First, we explore if, empirically, the fuel subsidies are reflected in the revenue, rather than the expenditure, side of the government budget. This would be the case, for example, if the government decides not to make an explicit transfer to the domestic public sector oil companies in order to cover their losses from selling oil products below the normal sales price. Then, these firms would report an accounting loss on their balance sheets, which would result in an equivalent reduction in government revenue (Coady and others, 2010). Most fuel subsidizing countries, however, follow a formula-based fuel pricing mechanism (GIZ, 2012) and will rarely deviate from this to avoid additional administrative costs. Nevertheless, to address this concern, we empirically examine whether our instrument had any impact on government revenue changes. Column 1 of Table 8 reports results from this exercise, where we find an insignificant, close to zero, effect of our instrument on government revenue changes.

Next, we investigate whether the instrument has an indirect effect on inflation by affecting consumer spending. Consumer spending and inflation are potentially affected in high subsidy regimes when fuel subsidies induce income and/or substitution effects that result in consumers purchasing and inflating prices of other commodities. This would cause the instrument to be positively correlated with inflation. Inversely, in low or non subsidy regimes, prices of oil products and transportation costs can act as important drivers of inflation since oil price changes are typically passed on to consumers. This would result in the instrument being negatively correlated with inflation. The total effect of an oil shock on inflation across different subsidy regimes, however, remains ambiguous. Further, whereas the effect of an oil price shock on government expenditure is instantaneous (in subsidy regimes), the full effect of a similar shock on inflation typically emerges only several periods after its occurrence. We should not, therefore, expect an effect of our instrument, the first lag of oil shocks across subsidy regimes, on current inflation. We test and verify this in column 2. The results show that our instrument has no effect on inflation changes.

Finally, we offer evidence to mitigate the concern that government tax revenues are differentially affected across oil subsidy regimes. We explore this issue and report results from regressing our instrument on tax revenue changes in column 3. While we find that it is indeed the case that a high fuel subsidy regime is negatively and significantly correlated with changes in tax revenue, its interaction with oil shocks, our instrument, has no effect on the same. This shows that while low or no fuel subsidizing regimes generate higher tax revenues compared to high fuel subsidizing regimes, this effect is not different when exposed to an oil shock. Since we control for the direct effect of the fuel subsidy regime on output growth and exploit only the interaction effect of the subsidy regime with the oil shock, we believe that our identification is robust to this concern.

Overall, all three robustness checks lend substantial support to our identification strategy and instrument.

VI. Quantile estimates of the fiscal multiplier

In the previous sections, we estimated the magnitude of fiscal multipliers based on the conditional mean relationship between growth rates of output and government spending. Our results indicate the presence of substantial heterogeneity in the multiplier estimates, which when ignored could result in severely downward biased estimates. After properly accounting for heterogeneity, we find a large effect of government spending on the *mean* output growth of countries across time. Given that substantial heterogeneity exists, policy-makers will often be interested in assessing the value of the multiplier at different points in the conditional distributions of output growth. For instance, the implied fiscal multipliers *for a given country* may vary during recessions (periods of low output growth) compared to booms (periods of high output growth). Therefore, it is of interest to provide and assess *heterogeneity* in the estimates of the fiscal multiplier. Quantile regression methods can account for this heterogeneity because the impact of government spending is estimated over the whole distribution of the output growth. In this section, we present estimates on the heterogenous impact of government spending using an instrumental variable quantile regression approach developed by Chernozhukov and Hansen (2005).

In general, in a typical least-squares regression model, we can estimate the fiscal multipliers at every quantile, τ, of the output growth distribution by using the following conditional quantile function:

$$Q_\tau(\Delta Y_{it}) \;=\; \beta^{M,\tau}\Delta\mathbb{G}_{it}+\mathbf{X}'_{it-1}\mathbf{\Theta}(\tau)+\alpha_i^\tau+\gamma_t^\tau, \tag{17}$$

where, α_i^τ denotes the (quantile) conditional fixed-effect and $\mathbf{\Theta}$ is the vector of coefficients associated with the vector of conditioning variables $\mathbf{X}$. However, this ordinary quantile regression estimator is biased in the presence of endogeneity issues as discussed in Section II.B. To tackle this issue, we use the instrumental variable quantile regression (IVQR) method of Chernozhukov and Hansen (2005). The IVQR estimates are obtained from a method that approximately solves the sample analog of moment equations corresponding to:

$$\frac{1}{n}\sum_{i=1}^{n}\Big[1(\Delta Y_{it}\leq\widehat{\beta}^M\Delta\mathbb{G}_{it}+\mathbf{X}'_{it-1}\widehat{\mathbf{\Theta}}+\widehat{\alpha}_i+\widehat{\gamma}_t)-\tau\Big]\Big[\mathbf{X}'_{it-1},\alpha_i,\gamma_t,Z_{it}\Big]'=o_p\left(\frac{1}{\sqrt{n}}\right) \tag{18}$$

The algorithm runs a series of standard quantile regressions of $\Delta Y_{it}-\beta_j^M\Delta\mathbb{G}_{it}$, on the instrument ($Z_{it}$) and the covariates ($\mathbf{X}_{it-1}$, α_i, γ_t), where $\{\beta_j^M\}$ is a grid over β^M. It then takes the value of $\{\beta_j^M\}$ that minimizes the absolute value of the coefficient on the instrument, Z_{it}, as the estimate of β^M ($\widehat{\beta}^M$). Note that the IVQR estimates are robust to outliers and, under some conditions, to the presence of weak instruments.

Figure 4 plots the OLS and IV quantile regression estimates for different quantiles. The dotted line in both panels of the figure marks the (instrumental variable) mean estimate for the fiscal multiplier. The figure shows that for all quantiles, the OLS quantile estimate is much below the IV conditional mean estimate. Furthermore, the OLS estimates lie in the range of [0, 0.25] and are fairly stable across the quantiles, though not significantly different from zero for most of them. The IVQR estimates, on the other hand, indicate sharp variation across quantiles. The effect of government spending on output growth is fairly large for the 20-30th and 75-80th percentiles of output growth densities, lying well above the conditional mean estimate. The estimates for the 35-75th percentiles fluctuate slightly and are in the range of [1.1, 1.8]. Our results suggest that the fiscal impact of government spending is fairly heterogenous across different quantiles of a country's growth distribution with its effect being the highest when the growth rate falls well below or well above its median level.

It is important to note that our IVQR results depict *within-country heterogeneity* in the magnitude of fiscal multiplier and are to be interpreted as conditional IVQR estimates. This is because we allow our country fixed-effect parameter to be indexed by the τ-th quantile of the conditional distribution of output growth. Hence, we allow the estimated fixed-effect parameters to change over the distribution of Δy_{it} as proposed in Harding and Lamarche (2009). We choose this specification for the following two reasons. Firstly, our identification strategy hinges crucially on the inclusion of these fixed-effect parameters so that it is necessary to retain them in our quantile specification to obtain unbiased estimates. Secondly, the impact of country heterogeneity is likely to evolve over time and be very mixed across quantiles of the pooled growth distribution (Harding and Lamarche, 2009). For instance, some countries could make significant progress over the 18 years time period, in terms of growth, overtaking others, thereby gaining position in the cross-country growth ranking.[22] An unconditional IVQR estimate would ignore this possibility, potentially confusing the interpretation of the estimated multiplier in the presence of such movements. Nevertheless, we also estimate heterogeneous impacts that do not condition the fixed-effects on quantiles, treating them as fixed. The unconditional IVQR estimator, developed by Powell (2012) includes the fixed-effects for identification purposes but retains the cross-sectional heterogeneity in output growth to classify its quantiles. Figure 5 plots the resulting fiscal multiplier estimates from the unconditional IVQR. For comparison, we also plot the conditional IVQR estimate that is similar to that shown in Figure 4.[23] As before, we find substantial variation in the impact of government spending on output growth across different quantiles of the *cross-country distribution* of output growth. Fiscal multipliers are large (ranging from 1.5 to 3.5), positive and significant for the 30-60% of the cross-country output growth density. The effect declines throughout the upper part of the distribution, with the impact falling close to zero for the top 20th percentiles.

[22] Although unlikely, note that the conditional IVQR estimates are identified even if some country does not vary in its position in the distribution of growth over time. Mathematically, the quantity is identified as long as government spending and other covariates change over time.

[23] Figure 5 is plotted on a large scale (y-axis) compared to Figure 4. The conditional IVQR estimates are similar in both figures barring some minor differences in the vector of supporting covariates.

Strictly speaking, the unconditional and conditional IVQR are not comparable, since they identify and exploit different sources of heterogeneity. Yet, both approaches can be useful for illustrating the heterogeneity in the impact of government spending. The choice between the two approaches depends on the focus of investigation. While the conditional IVQR approach allows the investigator or policy-maker to investigate heterogeneity in the effects of fiscal policy over time for a single country (for example, across its booms and recessions), the unconditional IVQR approach offers heterogeneous fiscal policy effects across a set of countries (for example, across high and low growth countries).

VII. Conclusions

In this paper, we identify and estimate the effect of government spending on output growth, when these effects are heterogeneous, varying both across countries and over time. We provide theoretical arguments to show that the heterogeneity is systematically related to the level of government spending, such that governments with low spending have relatively higher multipliers. Such a negative association is strongly held up in the data. In the presence of such heterogeneity, we identify the average effect of government spending on output, by exploiting differences in domestic gasoline prices of fuel subsidizing countries that incur changes in government expenditures when exposed to fluctuations in oil prices. Our estimated fiscal multipliers, for a panel of 127 countries over the period 1994-2011, range between 1.4 and 1.6. In addition, we estimate the range of fiscal multipliers across different cross-sectional and within-country quantiles of output growth and find them to be substantially heterogeneous.

Our findings have important implications for measuring and evaluating the effect of fiscal policy. For instance, we find that the effectiveness of a fiscal stimulus is not uniform, but depends crucially on the size of spending (or its growth). Therefore, the evaluation of any expansionary policy must take into account the benefits of providing a stimulus at different levels of spending, rather than incrementally from the mean level. While our model has illustrated one specific channel through which a government's size and its multiplier may be correlated, that of automatic stabilizers, we believe that there may be other mechanisms responsible for the same effect which are worth exploring in the future.

While we have focused on identifying the effects of total government spending, we consider as interesting questions for future research the identification of multipliers by type of expenditure, such as current and capital expenditures, or those arising from government transfers and direct government purchases.

Finally, we have shown how to incorporate heterogeneity in an empirical framework, making use of both cross-sectional and time-series variation. For example, recent literature has debated the relative size of tax cuts and the resulting multiplier, with theory suggesting that the tax multipliers are, like government spending multipliers, highly non-linear (Battaglini and Coate, 2015). Our framework can be used to estimate the effects of other endogenous government policies, such as revenue multipliers, whose estimates vary significantly across studies.

References

Acconcia, Antonio, Giancarlo Corsetti, and Saverio Simonelli, 2014, "Mafia and Public Spending: Evidence on the Fiscal Multiplier from a Quasi-experiment," *American Economic Review*, Vol. 104, No. 7, pp. 2185–2209.

Auerbach, Alan J., and Yuriy Gorodnichenko, 2012, "Measuring the Output Responses to Fiscal Policy," *American Economic Journal: Economic Policy*, Vol. 4, No. 2, pp. 1–27.

Barro, Robert J., 1990, "Government Spending in a Simple Model of Endogeneous Growth," *Journal of Political Economy*, Vol. 98, No. 5, pp. S103–S125.

Battaglini, Marco, and Stephen Coate, 2015, "A Political Economy Theory of Fiscal Policy and Unemployment," *Journal of the European Economic Association, forthcoming*.

Baum, Anja, Marcos Poplawski-Ribeiro, and Anke Weber, 2012, "Fiscal multipliers and the state of the economy," IMF Working Paper No. 12/286, (Washington: International Monetary Fund).

Belsley, David A., Edwin Kuh, and Roy E. Welsch, 1980, *Regression diagnostics: identifying influential data and sources of collinearity* (New York: Wiley).

Blanchard, Olivier J., and Daniel Leigh, 2013, "Growth Forecast Errors and Fiscal Multipliers," *American Economic Review*, Vol. 103, No. 3, pp. 117–20.

Brückner, Markus, Alberto Chong, and Mark Gradstein, 2012, "Estimating the permanent income elasticity of government expenditures: Evidence on Wagner's law based on oil price shocks," *Journal of Public Economics*, Vol. 96, No. 11–12, pp. 1025 – 1035.

Chamberlain, Gary, 1982, "Multivariate Regression Models for Panel Data," *Journal of Econometrics*, Vol. 18, No. 1, pp. 5 – 46.

———, 1992, "Efficiency Bounds for Semiparametric Regression," *Econometrica*, Vol. 60, No. 3, pp. 567–596.

Chernozhukov, Victor, and Christian Hansen, 2005, "An IV Model of Quantile Treatment Effects," *Econometrica*, Vol. 73, No. 1, pp. 245–261.

———, 2008, "The Reduced Form: A Simple Approach to Inference with Weak Instruments," *Economics Letters*, Vol. 100, No. 1, pp. 68 – 71.

Christiano, Lawrence, Martin Eichenbaum, and Sergio Rebelo, 2011, "When is the Government Spending Multiplier Large?" *Journal of Political Economy*, Vol. 119, No. 1, pp. 78–121.

Coady, David, Rolando Ossowski, John M. Piotrowski, Shamsuddin Tareq, Justin Tyson, and Robert Gillingham, 2010, "Petroleum Product Subsidies: Costly, Inequitable, and Rising," IMF Staff Position Note No. 10/05, (Washington: International Monetary Fund).

Coenen, Günter, Christopher J. Erceg, Charles Freedman, Davide Furceri, Michael Kumhof, René Lalonde, Douglas Laxton, Jesper Lindé, Annabelle Mourougane, Dirk Muir, Susanna Mursula, Carlos de Resende, John Roberts, Werner Roeger, Stephen Snudden, Mathias Trabandt, and Jan in't Veld, 2012, "Effects of Fiscal Stimulus in Structural Models," *American Economic Journal: Macroeconomics*, Vol. 4, No. 1, pp. 22–68.

Conley, T.G., 1999, "GMM Estimation with Cross Sectional Dependence," *Journal of Econometrics*, Vol. 92, No. 1, pp. 1 – 45.

Corsetti, Giancarlo, Keith Kuester, André Meier, and Gernot J. Müller, 2013, "Sovereign Risk, Fiscal Policy, and Macroeconomic Stability," *The Economic Journal*, Vol. 123, No. 566, pp. F99–F132.

Corsetti, Giancarlo, André Meier, and Gernot J. Müller, 2012, "What Determines Government Spending Multipliers?" *Economic Policy*, Vol. 27, No. 72, pp. 521–565.

Corsetti, Giancarlo, and Nouriel Roubini, 1996, "Optimal Government Spending and Taxation in Endgenous Growth Models," Working Paper 5851, National Bureau of Economic Research.

Dixit, Avinash, and Luisa Lambertini, 2003, "Interactions of Commitment and Discretion in Monetary and Fiscal Policies," *American Economic Review*, Vol. 93, No. 5, pp. 1522–1542.

Driscoll, John C., and Aart C. Kraay, 1998, "Consistent Covariance Matrix Estimation with Spatially Dependent Panel Data," *The Review of Economics and Statistics*, Vol. 80, No. 4, pp. 549–560.

Eggertsson, Gauti B., and Paul Krugman, 2012, "Debt, Deleveraging, and the Liquidity Trap: A Fisher-Minsky-Koo Approach*," *The Quarterly Journal of Economics*, Vol. 127, No. 3, pp. 1469–1513.

Erceg, Christopher, Jesper Lindé, and Christopher Erceg, 2014, "Is there a Fiscal Free Lunch in a Liquidity Trap?" *Journal of the European Economic Association*, Vol. 12, No. 1, pp. 73–107.

Favero, Carlo, Francesco Giavazzi, and Jacopo Perego, 2011, "Country Heterogeneity and the International Evidence on the Effects of Fiscal Policy," *IMF Economic Review*, Vol. 59, No. 4, pp. 652–682.

Fève, Patrick, Julien Matheron, and Jean-Guillaume Sahuc, 2013, "A Pitfall with Estimated DSGE-Based Government Spending Multipliers," *American Economic Journal: Macroeconomics*, Vol. 5, No. 4, pp. 141–78.

Galí, Jordi, and Roberto Perotti, 2003, "Fiscal Policy and Monetary Integration in Europe," *Economic Policy*, Vol. 18, No. 37, pp. 533–572.

Garen, John, 1984, “The Returns to Schooling: A Selectivity Bias Approach with a Continuous Choice Variable,” *Econometrica*, Vol. 52, No. 5, pp. 1199–1218.

GIZ, 2012, *International Fuel Prices 2010/2011*, Vol. 7th ed. (Bonn and Eschborn, Germany: German International Cooperation).

Graham, Bryan S., and James L. Powell, 2012, “Identification and Estimation of Average Partial Effects in "Irregular" Correlated Random Coefficient Panel Data Models,” *Econometrica*, Vol. 80, No. 5, pp. 2105–2152.

Harding, Matthew, and Carlos Lamarche, 2009, “A Quantile Regression Approach for Estimating Panel Data Models using Instrumental Variables,” *Economics Letters*, Vol. 104, No. 3, pp. 133 – 135.

Heckman, James J., Sergio Urzua, and Edward Vytlacil, 2006, “Understanding Instrumental Variables in Models with Essential Heterogeneity,” *The Review of Economics and Statistics*, Vol. 88, No. 3, pp. 389–432.

Hoechle, Daniel, 2007, “Robust Standard Errors for Panel Regressions with Cross-Sectional Dependence,” *Stata Journal*, Vol. 7, No. 3, pp. 281–312.

IEA, 2013, “Energy Prices and Taxes,” Vol. 2013, No. 1, IEA, Paris.

Ilzetzki, Ethan, Enrique G. Mendoza, and Carlos A. Végh, 2013, “How Big (Small?) are Fiscal Multipliers?” *Journal of Monetary Economics*, Vol. 60, No. 2, pp. 239 – 254.

Kraay, Aart, 2012, “How large is the Government Spending Multiplier? Evidence from World Bank Lending,” *The Quarterly Journal of Economics*, Vol. 127, No. 2, pp. 829–887.

Murtazashvili, Irina, and Jeffrey M. Wooldridge, 2008, “Fixed Effects Instrumental Variables Estimation in Correlated Random Coefficient Panel Data Models,” *Journal of Econometrics*, Vol. 142, No. 1, pp. 539 – 552.

Parker, Jonathan A., 2011, “On Measuring the Effects of Fiscal Policy in Recessions,” *Journal of Economic Literature*, Vol. 49, No. 3, pp. 703–18.

Pesaran, M. Hashem, 2006, “Estimation and Inference in Large Heterogeneous Panels with a Multifactor Error Structure,” *Econometrica*, Vol. 74, No. 4, pp. 967–1012.

Powell, David, 2012, “Unconditional Quantile Regression for Panel Data with Exogenous or Endogenous Regressors,” Working Paper, RAND Corporation Publications Department.

Ramey, Valerie A., 2011, “Identifying Government Spending Shocks: It’s all in the Timing,” *The Quarterly Journal of Economics*, Vol. 126, No. 1, pp. 1–50.

Serrato, Juan Carlos Suárez, and Philippe Wingender, 2010, “Estimating Local Fiscal Multipliers,” *University of California at Berkeley, mimeo*.

Staiger, Douglas, and James H. Stock, 1997, "Instrumental Variables Regression with Weak Instruments," *Econometrica*, Vol. 65, No. 3, pp. 557–586.

Uhlig, Harald, 2010, "Some Fiscal Calculus," *American Economic Review*, Vol. 100, No. 2, pp. 30–34.

Woodford, Michael, 2011, "Simple Analytics of the Government Expenditure Multiplier," *American Economic Journal: Macroeconomics*, Vol. 3, No. 1, pp. 1–35.

VIII. APPENDIX

A. Motivation for heterogeneity

In this section, we motivate the discussion on the potential correlation between the spending multiplier and the level of government expenditure via a stylized model that assumes an active stabilization role on behalf of the government and accounts for productive government services. Optimal government spending, where government spending is an input to private production, has been considered by Barro (1990) and (Corsetti and Roubini, 1996) in the context of endogenous growth models. Our focus here is on the dependence of optimal government spending on the fiscal multiplier. Assume an economy with a unit measure of identical representative agents, each of which maximizes the following utility function:

$$\mathbb{E}_t \sum_{i=0}^{\infty} \rho^i U\left(c_{t+i}\right), \tag{19}$$

where $\mathbb{E}_t$ is the conditional expectation operator based on information available at time t, $\rho \in (0,1)$ is the subjective discount factor, $U(\cdot)$ is a utility function that is strictly increasing in the consumption of the final good c_t , with $U''(\cdot) < 0$.

Each agent maximizes expression (19) subject to the following budget constraint:

$$c_t \leq w_t l_t - \tau_t, \tag{20}$$

where l_t denotes the labor supply of the representative agent, w_t is the real wage and τ_t denotes a lump-sum tax. For simplicity, we assume that the representative agent is endowed with one unit of labor which is supplied inelastically. Government expenditure is fully financed by the lump-sum tax, so that $g_t = \tau_t$ for every period t. The final consumption good, y_t, is produced by a competitive representative firm using the following aggregate production function:

$$y_t = z_t H\left(g_t\right) l_t,$$

where z_t is a technology shock, and $H\left(g_t\right)$ satisfies $H'(\cdot) > 0$ and $H''(\cdot) < 0$. For simplicity, we adopt the functional form $H\left(g_t\right) = g_t^{\varepsilon}$, where ε is a parameter reflecting the productivity of government spending.

Profit maximization suggests that $w_t = z_t g_t^{\varepsilon}$, i.e., the real wage is equated to the marginal productivity of labor. In equilibrium, households and firms optimally choose consumption and production, so that the budget and aggregate resource constraints are binding:

$$c_t = z_t g_t^{\varepsilon} - g_t, \tag{21}$$

where equilibrium output is given by:

$$y_t = z_t g_t^{\varepsilon}. \tag{22}$$

The government spending multiplier β^M is obtained by totally differentiating the above equation and rearranging terms to get:

$$\beta^M \equiv \frac{dy_t}{dg_t} = \varepsilon \cdot \frac{y_{t-1}}{g_{t-1}}. \tag{23}$$

Based on expression (23), we can conclude that:

Result 1: The government spending multiplier is increasing in the productivity of government spending.

We now proceed to characterize optimal spending on behalf of the government based on its output stabilization objective.[24] More specifically, the government chooses g_t to minimize deviations of output y_t from a target level of output y^P, which can be the natural or potential output level:

$$\min_{wrt\{g_t\}} \left(y_t - y^p\right)^2. \tag{24}$$

Substituting (22) into (24), we obtain the following optimal value for government expenditure:

$$g_t^* = \left(\frac{y^p}{z_t}\right)^{\frac{1}{\beta}}, \tag{25}$$

[24]A similar output stabilization problem is explored in Dixit and Lambertini (2003).

where $\beta \equiv \varepsilon$ denotes the elasticity of output with respect to government spending.[25] Note also, using equation (23), that:

$$g_t^* = \left(\frac{y^p}{z_t}\right)^{\frac{y_{t-1}}{\beta^M g_{t-1}}}. \tag{26}$$

Equations (28) and (26) yield our central proposition:

Proposition 1. *Optimal government spending is decreasing in (i) the output elasticity β, (ii) the government spending multiplier β^M, and (iii) the technology shock z_t.*

Hence, governments with higher multipliers, defined either as ratios (i.e., β^M) or elasticities (i.e., β), require relatively lower levels of expenditure to meet a certain target level of output compared to governments with lower multipliers. This induces a negative correlation between the level of government spending and the level of the fiscal multiplier. Additionally, optimal government spending is declining with the magnitude of the stochastic productivity shock (i.e., z_t). This result is consistent with counter-cyclical spending rules, where governments reduce expenditure during periods of high growth and increase spending during recessions (see, for example, Fève, Matheron, and Sahuc (2013) and Galí and Perotti (2003)).

Denoting upper case letters as the logarithmic transformations of each variable, we obtain the log-form of equation (22) as:

$$Y_t = Z_t + \varepsilon G_t. \tag{27}$$

We also obtain an equivalent expression for the optimal government spending rule of equation (25), which in log-form is equal to:[26]

$$G_t^* = \frac{Y^p - Z_t}{\beta}. \tag{28}$$

[25]Fiscal multipliers are often defined as elasticities of output with respect to government spending (i.e., β), as well as ratios of changes in output over changes in government spending (i.e., β^M). Our results are shown to hold under either definition.

[26]To obtain the optimal government spending rule in a log-form, we assume that the government minimizes the log-deviations of output from its potential level, i.e., the minimization problem becomes $\min\limits_{wrt\{G_t\}} \left(Y_t - Y^p\right)^2$.

B. Identification and estimation of the CRC model

Our model, with one regressor and an intercept, is given by:

$$Y_{it} = \mathbf{G}_{it} b_{it}(A_i, U_{it}). \tag{29}$$

As noted earlier, Assumptions (1.1)-(1.3) imply that:

$$\begin{aligned}
\mathbb{E}[b_{it}(A_i, U_{it})|\mathbf{G_i}] &= \mathbb{E}[b_i^*(A_i, U_{it})|\mathbf{G_i}] + \mathbb{E}[d_{it}(U_{2,it})|\mathbf{G_i}] && (30)\\
&= \mathbb{E}[b_i^*(A_i, U_{i1})|\mathbf{G_i}] + \mathbb{E}[d_{it}(U_{2,i1})] && (31)\\
&= \boldsymbol{\beta}_i(\mathbf{G_i}) + \boldsymbol{\delta}_t, \ \forall \ t = 1, ..., T. && (32)
\end{aligned}$$

where $\boldsymbol{\delta}$ denotes the $2(T-1)\times 1$ vector of aggregate shifts in the random coefficients over time. $\boldsymbol{\delta}_1$ is normalized to be equal to zero. Next, we define a matrix of time shifters, $\mathbf{W}$. As described by Graham and Powell (2012), $\mathbf{W}$ is a $T \times 2(T-1)$ block diagonal matrix containing the regressors corresponding to the aggregate time shift coefficients (with the first row containing a vector of zeroes due to the normalization).

Equation (29) can be rewritten as:

$$\mathbb{E}[\mathbf{Y_i}|\mathbf{G_i}] = \mathbf{W_i}\boldsymbol{\delta} + \mathbf{G_i}\boldsymbol{\beta}_i(\mathbf{G_i}). \tag{33}$$

A variance weighted within group transform, which essentially differences away the unobserved heterogeneity, $\boldsymbol{\beta}_i(\mathbf{G_i})$, is obtained using the residual making matrix, $\mathbf{M}_{\mathbf{i}\boldsymbol{\Phi}_i} = \mathbf{I}_T - \mathbf{G_i}\left[\mathbf{G_i}'\boldsymbol{\Phi}_i{}^{-1}\mathbf{G_i}\right]^{-1}\mathbf{G_i}'\boldsymbol{\Phi}_i{}^{-1}$ where $\boldsymbol{\Phi}_i = \mathrm{Var}(\mathbf{Y_i}|\mathbf{G_i})$. Using the fact that $\mathbf{M}_{\mathbf{i}\boldsymbol{\Phi}_i}\mathbf{G_i} = 0$, we get:

$$\begin{aligned}
\mathbb{E}[\tilde{\mathbf{Y}}_\mathbf{i}|\mathbf{G_i}] &= \mathbf{M}_{\mathbf{i}\boldsymbol{\Phi}_i}\mathbf{W_i}\boldsymbol{\delta} + \mathbf{M}_{\mathbf{i}\boldsymbol{\Phi}_i}\mathbf{G_i}\boldsymbol{\beta}_i(\mathbf{G_i}) && (34)\\
&= \tilde{\mathbf{W}}_\mathbf{i}\boldsymbol{\delta}, && (35)
\end{aligned}$$

where $\tilde{\mathbf{Y}}_\mathbf{i} = \mathbf{M}_{\mathbf{i}\boldsymbol{\Phi}_i}\mathbf{Y_i}$ and $\tilde{\mathbf{W}}_\mathbf{i} = \mathbf{M}_{\mathbf{i}\boldsymbol{\Phi}_i}\mathbf{W_i}$.

The pair of moment restrictions that identify the parameter are given by:

$$\mathbb{E}\begin{bmatrix} \mathbf{W_i}'\boldsymbol{\Phi}_i{}^{-1}\mathbf{M}_{\mathbf{i}\boldsymbol{\Phi}_i}(\mathbf{Y_i} - \mathbf{W_i}\boldsymbol{\delta}) \\ {}^{-1}\mathbf{G_i}'\boldsymbol{\Phi}_i{}^{-1}(\mathbf{Y_i} - \mathbf{W_i}\boldsymbol{\delta}) - \boldsymbol{\beta}_i \end{bmatrix} = 0. \tag{36}$$

For estimation, we proceed in two steps:

<u>Estimation step 1</u>: The first step in the estimation procedure consists in using the moment restrictions to identify $\boldsymbol{\delta}$ in the following way:

$$\boldsymbol{\delta} = \mathbb{E}\left[\tilde{\mathbf{W}}_{\mathrm{i}}{}'\boldsymbol{\Phi}_i{}^{-1}\tilde{\mathbf{W}}_{\mathrm{i}}\right]^{-1}\mathbb{E}\left[\tilde{\mathbf{W}}_{\mathrm{i}}{}'\boldsymbol{\Phi}_i{}^{-1}\tilde{\mathbf{Y}}_{\mathrm{i}}\right]. \tag{37}$$

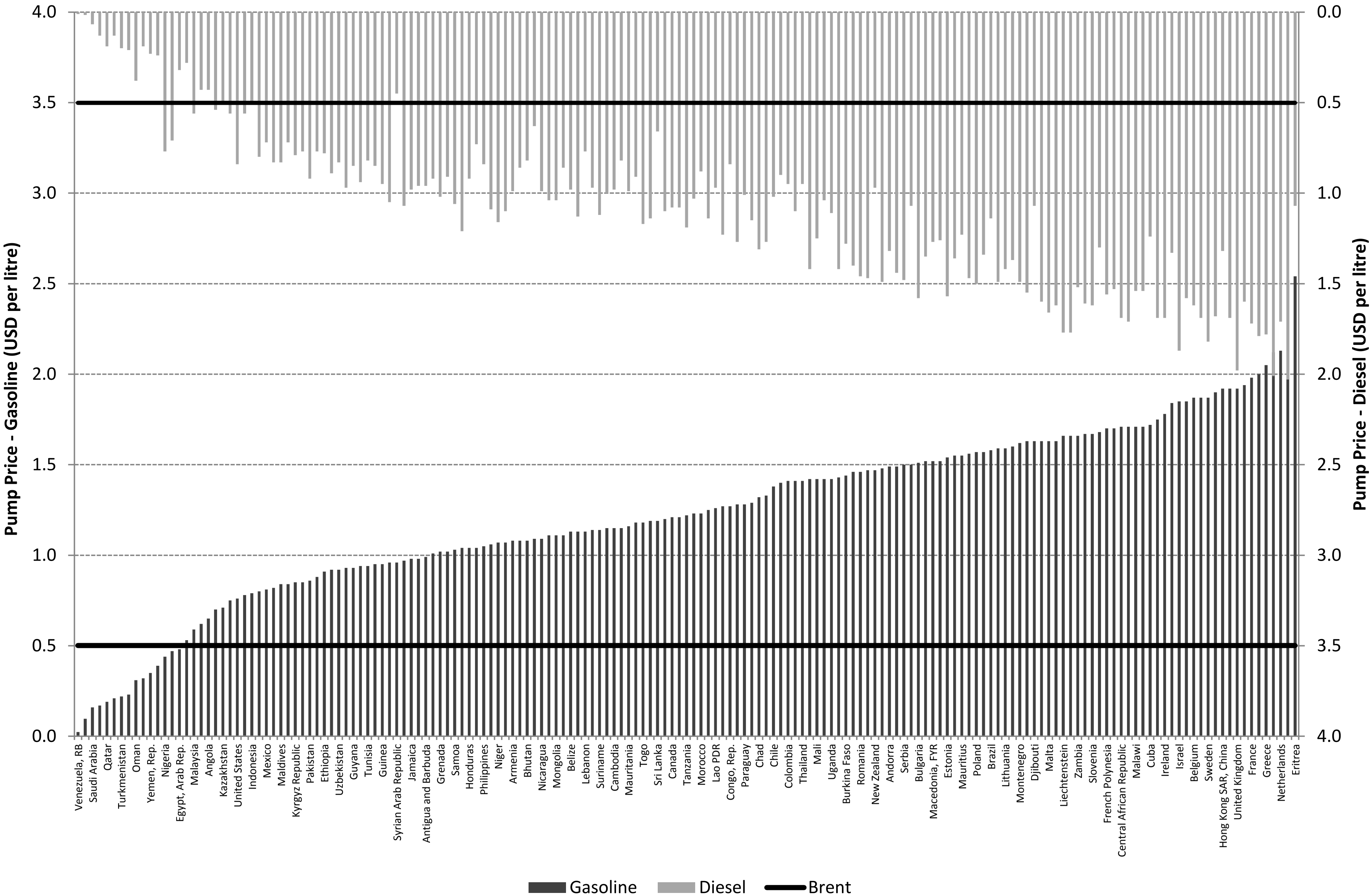
<u>Estimation step 2</u>: In the second step, with an estimate of $\boldsymbol{\delta}$, Chamberlain (1992) shows that the average partial effect, $\boldsymbol{\beta}$, is identified by the (population) mean of the unit-specific generalized least squares fits:

$$\widehat{\boldsymbol{\beta}_i} = \left(\mathbf{G}_{\mathrm{i}}{}'\boldsymbol{\Phi}_i{}^{-1}\mathbf{G}_{\mathrm{i}}\right)^{-1}\mathbf{G}_{\mathrm{i}}{}'\boldsymbol{\Phi}_i{}^{-1}(\mathbf{Y}_{\mathrm{i}} - \mathbf{W}_{\mathrm{i}}\boldsymbol{\delta}). \tag{38}$$

i.e., $\boldsymbol{\beta}$ is given by:

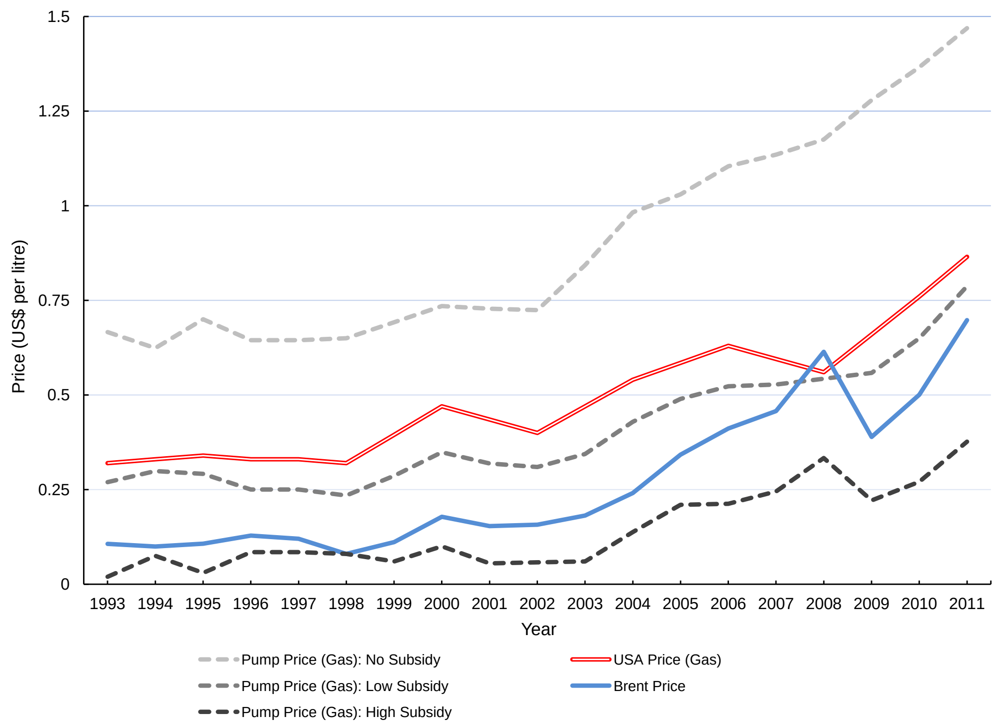
$$\boldsymbol{\beta} = \mathbb{E}\left[\left(\mathbf{G}_{\mathrm{i}}{}'\boldsymbol{\Phi}_i{}^{-1}\mathbf{G}_{\mathrm{i}}\right)^{-1}\mathbf{G}_{\mathrm{i}}{}'\boldsymbol{\Phi}_i{}^{-1}(\mathbf{Y}_{\mathrm{i}} - \mathbf{W}_{\mathrm{i}}\boldsymbol{\delta})\right]. \tag{39}$$

Figure 1. Retail price of gasoline and diesel in 2010.



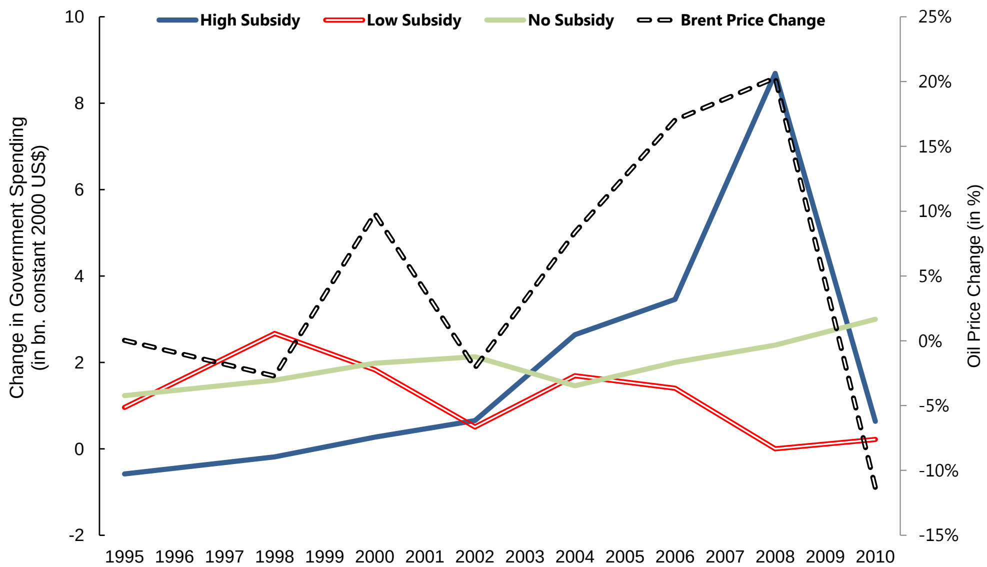
Note: This figure plots the variation in average gasoline and diesel prices across countries for the year 2010. It also plots the international ‘Brent’ price of oil in 2010 for reference.

Figure 2. Times series plot of international and domestic oil prices.



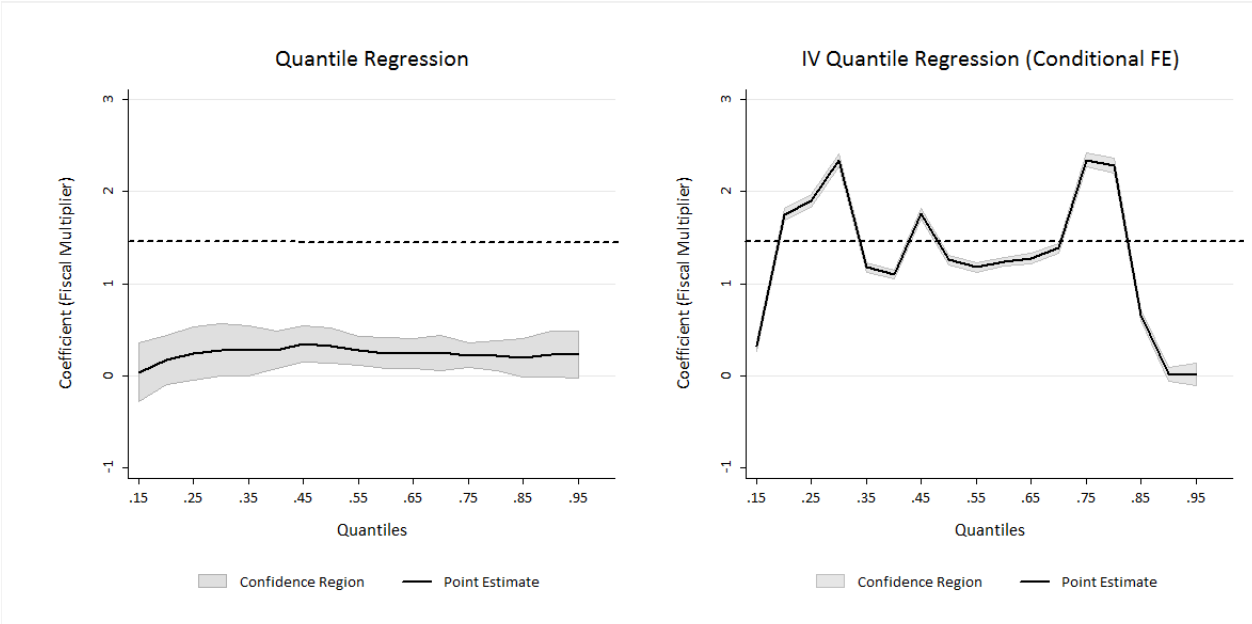
Note: This figure plots the average international and domestic prices of oil and gasoline across all subsidy regimes. It also plots the domestic price of gasoline in the USA and the Brent price, which are our price benchmarks for the low and no subsidy regime classification.

Figure 3. Changes in oil prices and government expenditure by fuel subsidy regime.



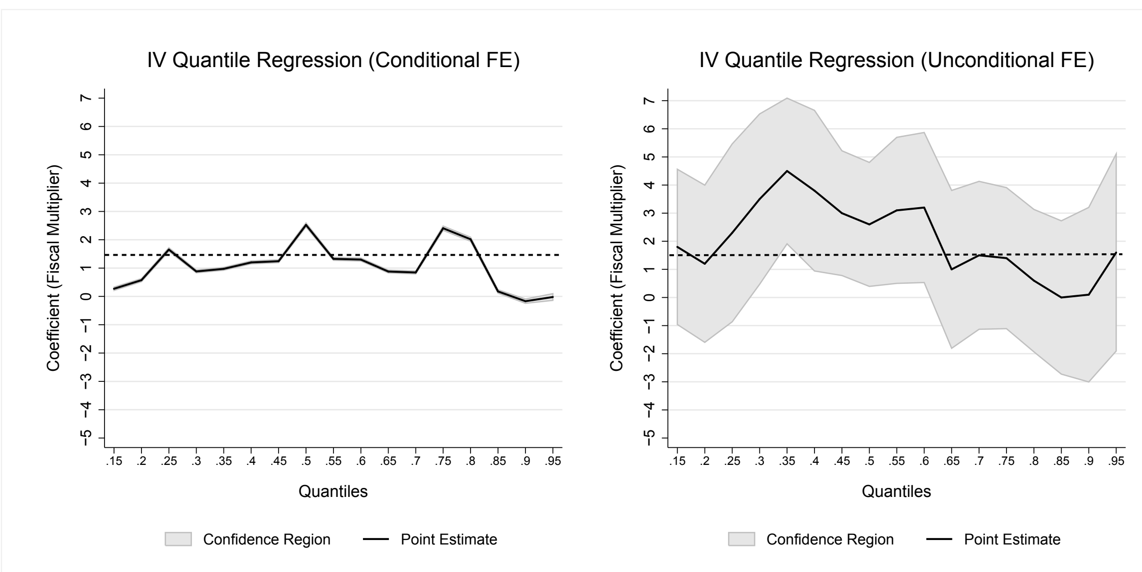
Note: This figure shows the change in international (Brent) oil price (in US$ per litre) and the changes in government spending (in billion constant 2000 US$) over time for every subsidy regime.

Figure 4. Quantile estimates of the fiscal multiplier (OLS and IV).



Note: This figure shows the heterogeneity in the effect of change in government spending on output growth. It plots the OLS and IV quantile regression estimates for different quantiles. The dotted line in both panels of the figure marks the (instrumental variable) mean estimate for the fiscal multiplier.

Figure 5. Quantile estimates of the fiscal multiplier (conditional and unconditional).



Note: This figure shows the heterogeneity in the effect of change in government spending on output growth. It plots the fiscal multiplier estimates from the unconditional IVQR. For comparison, we also plot the conditional IVQR estimate that is similar to that shown in the previous figure. The dotted line in both panels of the figure marks the (instrumental variable) mean estimate for the fiscal multiplier. Figure 5 is plotted on a large scale (y-axis) compared to Figure 4. The conditional IVQR estimates are similar in both figures barring some minor differences in the vector of supporting covariates.

Table 1. Summary Statistics

Variable	Obs.	Mean	Std. Dev.	Min.	Max.	Source
Change in GDP	1875	0.040	0.041	-0.180	0.378	World Bank World Development Indicators
Change in Gov. Expenditure	1875	0.006	0.018	-0.124	0.548	World Bank World Development Indicators
Crude Oil (US$/litre)	1875	0.281	0.181	0.080	0.698	BP Statistical Review of World Energy
U.S. Gasoline (US$/litre)	1875	0.508	0.147	0.320	0.865	World Bank World Development Indicators
U.S. Diesel (US$/litre)	1875	0.547	0.204	0.270	0.945	World Bank World Development Indicators
Domestic Gasoline (US$/litre)	1857	0.855	0.421	0.02	2.53	World Bank World Development Indicators
Domestic Diesel (US$/litre)	1856	0.699	0.424	0.01	2.18	World Bank World Development Indicators
Gasoline Subsidy	1857	0.186	0.466	0.000	2.000	World Bank World Development Indicators
Diesel Subsidy	1856	0.435	0.625	0.000	2.000	World Bank World Development Indicators
Oil Price Shock	1875	0.000	0.042	-0.271	0.312	BP Statistical Review of World Energy
Gasoline Subsidy × Oil Price Shock	1875	0.004	0.039	-0.485	0.373	World Bank (WDI) & BP Statistical Review
Change in Net Imports	1874	0.028	0.063	-0.433	0.704	World Bank World Development Indicators
Change in Gov. Revenue	1056	0.010	0.055	-0.580	0.643	World Bank World Development Indicators
Inflation	1789	9.196	56.578	-9.616	2075.9	World Bank World Development Indicators
Tax Revenue per GDP	1139	17.367	7.120	1.037	61.018	World Bank World Development Indicators

Note: This table reports summary statistics of variables used in our regressions for the full sample of countries over the years 1991-2011. We report the mean, standard deviation and min/max of each variable. All changes are measured as $\Delta x_{it} = \frac{x_{it}-x_{it-1}}{y_{it-1}}$ where x denotes the variable of interest and y is GDP. Oil Price Shock is calculated as the product of the log-change of the crude oil price with the country's average ratio of net oil exports over GDP. Gas Subsidy regime takes the value 0 for countries with no subsidy (retail gasoline price above the U.S. gasoline and Brent prices), 1 for low subsidy (retail gasoline price below the U.S. gasoline price but above the Brent price) and 2 for high subsidy (retail gasoline price below the Brent price).

Table 2. Transition Matrix for Gasoline and Diesel Subsidy

Panel A: **Gasoline Subsidy Regime**

From => To	No-Subsidy	Low-Subsidy	High-Subsidy	Total
No-Subsidy	97.89	**2.01**	**0.1**	100
Low-Subsidy	**14.48**	77.44	**8.08**	100
High-Subsidy	**0**	**11.72**	88.28	100
Total	81.38	13.02	5.6	100

Panel B: **Diesel Subsidy Regime**

From => To	No-Subsidy	Low-Subsidy	High-Subsidy	Total
No-Subsidy	95.13	**4.88**	**0**	100
Low-Subsidy	**13.34**	80.19	**6.47**	100
High-Subsidy	**0.47**	**15.09**	84.43	100
Total	63.51	27.6	8.89	100

Note: This table shows the transition matrix with respect to fuel and gas subsidies for our pooled, 1874 country-year sample observations. Gas (Diesel) Subsidy regime takes the value 0 for countries with no subsidy (retail gasoline (diesel) price above the U.S. gasoline (diesel) and Brent prices), 1 for low subsidy (retail gasoline (diesel) price below the U.S. gasoline (diesel) price but above the Brent price) and 2 for high subsidy (retail gasoline (diesel) price below the Brent price). Figures highlighted in bold indicate observations that change regimes over time. The percentage of country-year observation switch across regimes are 6% and 10% for gasoline and diesel, respectively.

Table 3. Descriptive Tests for Heterogeneity

	Elasticity: Log GDP			Multiplier: Δ GDP		
	(1)	(2)	(3)	(4)	(5)	(6)
Government Expenditure	0.329***	0.376***	0.474	0.403***	0.809***	1.896***
	(0.047)	(0.050)	(0.371)	(0.148)	(0.169)	(0.467)
Joint F-test						
Polynomials of Mean Interactions		4.07***			7.59***	
Lags and Leads Interactions			8.78***			66.62***
Observations	2689	2689	1574	2628	2628	1595
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table reports results from testing for the presence of heterogeneity in an equation regressing GDP on government spending. Columns 1-3 regress log GDP on log of government spending (producing estimates for elasticity) whereas Columns 4-6 regress the growth rate of GDP on the growth rate of government spending (producing estimates for the multiplier). Columns 2 and 5 include polynomials of the interaction of government spending with the mean of the lags and leads of government spending over time. Columns 3 and 6 include a full set of interaction terms of government spending with all lags and leads of government spending across the time period. Standard errors are robust to heteroscedasticity. * indicates significance at 10%; ** at 5%; *** at 1%.

Table 4. Elasticity of GDP to Government Spending: CRC Model Estimates

	Elasticity: Log GDP		
	FE-OLS	FE-OLS	CRC
Log GExp	0.073***	0.070***	0.388***
	(0.024)	(0.024)	(0.090)
Log GExp × $\text{Year}_{2011}^{Dummy}$		0.068***	0.387***
		(0.024)	(0.089)
Log GExp × $\text{Year}_{2012}^{Dummy}$		0.066***	0.387***
		(0.024)	(0.089)
Intercept Shifters	No	Yes	Yes
Observations	222	222	222

Note: This table reports results on the effect of log of total government spending (Log GExp) on log of total GDP (Log GDP). The coefficients represent the elasticity of GDP with respect to government spending. Column 1 reports results from a FE-OLS regression. Column 2 reports results from a FE-OLS regression with the inclusion of intercept shifters and time-varying coefficients. Column 3 reports Chamberlain's (1992) CRC method, estimated using optimal generalized least squares. Standard errors are robust to heteroscedasticity and are clustered at the geographical region level. * indicates significance at 10%; ** at 5%; *** at 1%.

Table 5. Fiscal Multiplier Estimates (1/2)

	OLS	IV		
		1^{st} stage	2^{nd} Stage	Control Function
	(1)	(2)	(3)	(4)
	Δ GDP	Δ GExp	Δ GDP	Δ GDP
Δ GExp	0.236^{**}		1.454^{**}	1.57^{**}
	(0.116)		(0.721)	(0.628)
(Lag) Oil Price Shock x Gas Subsidy		0.035^{***}		
		(0.009)		
(Lag) Gas Subsidy		0.001	0.002	0.001
		(0.001)	(0.004)	(0.003)
(Lag) Diesel Subsidy		0.000	0.000	0.000
		(0.001)	(0.003)	(0.002)
λ_G				-1.118*
				(0.631)
ψ_G				-0.865***
				(0.200)
Joint F-test				
$\lambda_G = 0$ & $\psi_G = 0$				6.11***
Observations	1874	1874	1874	1874
Year fixed effects	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes
First Stage F-stat.		14.145		

Note: This table reports results on the effect of growth in government spending (Δ GExp) on growth in GDP (Δ GDP). Oil Price Shock is calculated as product of the log-change of the crude oil price with the country's average ratio of net oil exports over GDP. Gas Subsidy regime takes the value 0 for countries with no subsidy (retail gasoline price above the U.S. gasoline and Brent prices), 1 for low subsidy (retail gasoline price below the U.S. gasoline price but above the Brent price) and 2 for high subsidy (retail gasoline price below the Brent price). Column 1 reports results from a FE-OLS regression. Column 2 reports the first stage of the IV regression where the dependent variable is growth in government expenditure. Column 3 reports the corresponding second stage; the dependent variable is growth in GDP. Standard errors are robust to heteroscedasticity and autocorrelation (up to 2 lags) and are reported in parentheses. Column 4 reports results from Garen's (1984) selectivity bias correction method. The specification, estimated using weighted least squares, is: $\ddot{\Delta} Y_{it} = \beta^M \ddot{\Delta} G_{it} + \lambda_G \widehat{\ddot{v}}_{it} + \psi_G \ddot{\Delta} G_{it} \cdot \widehat{\ddot{v}}_{it} + \ddot{e}_{it}$, where $\widehat{\ddot{v}}_{it}$ is the estimated residual from the first stage (Column 2). Standard errors are adjusted for the heteroskedasticity using the procedure described in footnote (16). * indicates significance at 10%; ** at 5%; *** at 1%.

Table 6. Fiscal Multiplier Estimates (2/2)

	(1)	(2)	(3)	(4)	(5)	(6)
	Δ GDP	Δ GDP	Δ GDP	Δ GDP	Δ GDP	Δ GDP
Δ GExp	1.404*	1.567**	1.511**	1.418**	1.553**	1.469**
	(0.722)	(0.764)	(0.760)	(0.722)	(0.732)	(0.702)
(Lag) Gas Subsidy	0.002	0.000	0.000	0.001	0.001	0.001
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
(Lag) Diesel Subsidy	0.000	-0.001	-0.001	0.001	0.001	0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Pump Gasoline		-0.007	-0.007			
		(0.016)	(0.016)			
(Lag) Pump Gasoline		-0.005	-0.005	-0.012	-0.010	-0.010
		(0.015)	(0.015)	(0.008)	(0.008)	(0.008)
Pump Diesel		0.019	0.018			
		(0.014)	(0.015)			
(Lag) Pump Diesel		-0.008	-0.007	0.008	0.007	0.008
		(0.014)	(0.014)	(0.007)	(0.007)	(0.007)
Oil Price Shock	-0.029		-0.029	-0.029	-0.026	-0.024
	(0.020)		(0.021)	(0.020)	(0.021)	(0.021)
(Lag) Change in Net Imports					0.043	0.027
					(0.031)	(0.031)
(Lag) Change in GDP						0.084***
						(0.029)
Observations	1874	1854	1854	1874	1867	1865
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
First Stage F-stat.	14.438	13.419	14.055	14.249	14.345	14.253

Note: This table reports results on the effect of growth in government spending (Δ GExp) on growth in GDP (Δ GDP). Oil Price Shock is calculated as product of the log-change of the crude oil price with the country's average ratio of net oil exports over GDP. Gas Subsidy regime takes the value 0 for countries with no subsidy (retail gasoline price above the U.S. gasoline and Brent prices), 1 for low subsidy (retail gasoline price below the U.S. gasoline price but above the Brent price) and 2 for high subsidy (retail gasoline price below the Brent price). Each column in this table accounts for the addition of various control variables that include the following: Gasoline and diesel subsidy regime (Lag), retail prices of gasoline and diesel (current and lagged), change in net imports (Lag) and lagged change in GDP. Standard errors are robust to heteroscedasticity and autocorrelation (up to 2 lags) and are reported in parentheses. * indicates significance at 10%; ** at 5%; *** at 1%.

Table 7. Robustness

Panel A: **Standard Errors**

	Cluster Country/Year (1)	Spatial Adjusted (2)	Driscoll Kraay (3)	Weak IV Robust (4)
Δ GExp	1.553*	1.553**	1.553**	1.553**
90% Confidence Intervals	[0.065 3.04]	[0.346 2.77]	[0.421 2.684]	[0.434 2.961]
95% Confidence Intervals	[-0.224 3.330]	[0.104 3.010]	[0.096 3.009]	[0.195 3.361]
Year fixed effects	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes

Note: This table reports results on the effect of growth in government spending (Δ GExp) on growth in GDP (Δ GDP). Each column in this table computes standard errors for the results in column 5 of Table (6) in different ways. Column 1 clusters standard errors on both country and year identifiers; Column 2 calculates standard errors accounting for potential cross-sectional spatial dependence; Column 3 calculates standard errors accounting for potential cross-sectional dependence of an unknown form; Column 4 reports weak instrument robust confidence intervals. * indicates significance at 10%; ** at 5%; *** at 1%.

Panel B: **Outliers**

	Original Sample (1)	Cook's D Outliers (2)	DFFITS Outliers (3)
Δ GExp	1.553**	1.340*	1.278**
	(0.731)	(0.786)	(0.663)
Year fixed effects	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes
# Observations Dropped		141	57
First Stage F-stat.		12.972	10.966

Note: This table reports results on the effect of growth in government spending (Δ GExp) on growth in GDP (Δ GDP). Each column in this table computes results in column 5 of Table (6) adjusting for outliers. Column 2 drops outliers based on Cook's distance while column 3 drops outliers based on the DFFITS statistic. All specifications control for the following variables: Gasoline and diesel subsidy regimes (Lag), retail prices of gasoline and diesel (current and lagged). * indicates significance at 10%; ** at 5%; *** at 1%.

Table 8. Instrument Validity

	(1) Δ Gov. Revenue	(2) Inflation	(3) Tax revenue (% of GDP)
(Lag) Oil Price Shock x Gas Subsidy	0.046	-0.316	1.476
	(0.060)	(10.117)	(2.237)
(Lag) Gas Subsidy	0.020*	-9.768**	-0.540*
	(0.012)	(4.695)	(0.285)
(Lag) Diesel Subsidy	0.004	-7.453	-0.539**
	(0.007)	(5.424)	(0.228)
Pump Gasoline	0.025	-5.942	-1.423
	(0.022)	(13.023)	(1.036)
(Lag) Pump Gasoline	-0.013	-35.511	1.239
	(0.027)	(36.252)	(1.042)
Pump Diesel	-0.004	1.956	1.562*
	(0.022)	(12.858)	(0.854)
(Lag) Pump Diesel	0.000	0.342	-1.187
	(0.028)	(21.319)	(0.841)
Oil Price Shock	0.175***	6.491	3.085
	(0.042)	(8.115)	(1.953)
Observations	1041	1771	1125
Year fixed effects	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes

Note: This table reports results on the effect of the instrument, oil shock interacted with subsidy regime, on various variables. Oil Price Shock is calculated as a product of the log-change of the crude oil price with the country's average ratio of net oil exports over GDP. Gas Subsidy regime takes the value 0 for countries with no subsidy (retail gasoline price above the U.S. gasoline and Brent prices), 1 for low subsidy (retail gasoline price below the U.S. gasoline price but above the Brent price) and 2 for high subsidy (retail gasoline price below the Brent price). The dependent variable in column 1 is change in government revenue. The dependent variable in column 2 is inflation. The dependent variable in column 3 is total tax revenue collected. * indicates significance at 10%; ** at 5%; *** at 1%.