Document de travail de l'OFCE

GOVERNMENTS UNDER INFLUENCE : COUNTRY INTERACTIONS IN DISCRETIONARY FISCAL POLICY

N° 2010-25

OCTOBRE 2010

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Governments under influence: Country interactions in discretionary fiscal policy

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Abstract
We investigate the interactions between countries of the discretionary component of national fiscal policies (i.e. the cyclically- and interest-adjusted part of fiscal policy), therefore observing and investigating the part of public spending and tax receipts on which governments keep full discretion. Our sample covers 18 OECD countries, during the 1974-2008 period. First, we build a measure of such discretionary fiscal policy, considered as the residual component of a VAR model, and compute the measure for the full sample. Drawing on this new dataset, the second step provides estimates of discretionary fiscal policy interactions between countries of the sample. Our results highlight the existence of interactions between neighboring countries' public decisions, where neighborhood is defined by economic leadership as well as geography. We also find evidence of an opportunistic behavior of OECD countries' governments for the discretionary public spending. Finally, the disciplining device of the European Union fiscal framework is shown to be ineffective.

Keywords: Fiscal policy; discretion; interactions; VAR; spatial econometrics

JEL Classification: E62, H60, H87

Introduction

Are national fiscal policies under external influence? A positive answer to this question could help design and promote good practices in fiscal policy design. Even more so if such external influence does not arise through (potentially) binding institutions – such as the Stability and Growth Pact in the European Union – or through peer pressure. On the contrary, if countries influence one another simply by adopting the same kind of behavior, then good practices could spread by the pure virtue of imitation.

The idea that public policies might diffuse from one country to another is not new: it has been developed, e.g. in the literature dedicated to yardstick competition (see Besley and Case, 1995), tax competition (see Wilson, 1999), coordination issues (see Oudiz and Sachs, 1984, for a seminal contribution), capital account policies (see Steiner, 2010), as well as in political science (see Gilardi, 2010, for a recent contribution). Though binding fiscal rules and peer pressure have already been studied extensively so far (see, e.g., Hallerberg et al., 2009, for the European case), it remains to be investigated, as far as tax and fiscal policies are concerned, whether other means for such diffusion, *i.e.* imitation, are possible. However, it first has to be proven that a country's tax or fiscal policy influences other countries'.

Our aim in this paper is to discover if such other influences exist. Defining a discretionary policy is therefore a prerequisite to studying behavioral interactions between governments: only the part of public spending and/or tax receipts on which governments fully retain control can be used strategically in response to other governments' behaviors.

Before dealing with measurement issues, it remains to be explained how discretionary fiscal policies might look like. As is well known, there is only one publicized yearly budget by country. Nevertheless, supplements to the budget are always possible at any time, stemming from changes in, say, welfare benefits, public employees' wages

following a new collective agreement, or new tax exemptions. These unexpected supplements at the time of adoption of the yearly budget are common practice, and can use the legal possibility (sometimes obligation) of mid-year budget review but, in many countries, they can be proposed to the legislature at any moment (see OECD, 2004). For instance in the UK, the end-of-the-year budget is substantially revised each year in comparison with the Pre-Budget Report which serves as a basis for preparing the UK yearly budget. Among the reasons explaining the revisions, one can find the effects of forecasting changes, which are not directly attributable to policy decisions, but also effects of discretionary changes, which are. Moreover, no OECD country provides a limit to these supplementary budgets, though the practice is generally to limit their size (OECD, 2004). Mid-year budget, plus the possibility of supplementary budgets, when they include discretionary changes, may affect the fiscal policy outcome of the current fiscal year, but only marginally. However, their unexpected essence is what counts in the end: expected changes are incorporated in private expectations by agents who can smooth their revenues and profits accordingly; this is not possible with unexpected changes which may therefore have a dynamic real effect on private consumption and investment until the new policy measures have become common knowledge. Hence, discretionary policy measures have no neutral macroeconomic effect, even in a rational expectations setting.

Assessing these measures is not straightforward, however, and various attempts have been made in the literature to extract or reveal discretionary fiscal measures (Beetsma and Giuliodori, 2008, 2010a; Blanchard and Perotti, 2002; Mountford and Uhlig, 2009; Ramey and Shapiro, 1998; Romer and Romer, 2009, 2010)[1]. We will briefly review these methods in the next section. It can nevertheless be stated that, except the studies by Beetsma and Giuliodori (2008, 2010a), none questions the issue of external influences on the adoption of the new policy measures and they generally focus on one country, while our scope is much broader.

[1] See Beetsma and Giuliodori (2010b) for a review on discretionary fiscal policy, where they pay attention to estimates in the open economy. Exchange rates and current accounts are out of the scope of this contribution.

In order to deal with external influence on the design of discretionary fiscal policy, we provide two contributions to the literature. First, we define and provide, as a first step, a measure of discretionary fiscal policy for 18 OECD countries, throughout the 1974-2008 period. Two measures are computed: a discretionary (*i.e.* a cyclically- and interest-adjusted) measure of public spending and a discretionary (*i.e.* a cyclically-adjusted) measure of tax receipts. Second, we measure country interactions in discretionary tax or fiscal policy. As such, we consider several weight matrices - to check if influences among countries are driven by pure chance or by a systematic pattern - and different political variables that could not be driven away by the first step and may explain why countries imitate each other. As such, our approach provides a new way to look at the problem, by cross-breeding two methodologies established in their respective fields and never combined before, despite the potential fecundity of this combination.

The literature on fiscal policy, its determinants and consequences is abundant. There are broadly two strands of literature: a macro-founded literature and a micro-founded literature. On the macroeconomic side, only a few papers have addressed the question of the measure and determinants of reciprocal influences in discretionary fiscal policy. Giuliodori and Beetsma (2008) analyze the interdependence of fiscal policies, and in particular deficits, among European Union countries using an empirical analysis based on real-time fiscal data. They find some evidence of fiscal policy interdependence, with the fiscal plans of the large countries affecting the fiscal plans of the small countries, but not vice versa. However, they restrict attention to fiscal plans, *i.e.* measures announced *ex ante* by European Union governments where they (have to) internalize how they will abide by the rules of the Stability and Growth Pact[2]. Though fiscal plans to address European recommendations of fiscal disciplining are included in discretionary fiscal policies, the latter cannot be restricted to them: governments might modify their fiscal policy during a year without justifying it on grounds of fiscal obedience to a European rule: *ex post* data are necessary to reveal such a modification. Moreover, the scope for

[2] Giuliodori and Beetsma (2008) use OECD forecasts in order to escape political use of fiscal forecasts by governments. However, given that OECD figures come from governmental institutions, OECD forecasts are blurred by political matters, as Giuliodori and Beetsma (2008) acknowledge: economic forecasts can be expected to abide *ex ante* by the rules of the Stability and Growth Pact.

discretionary fiscal interactions may go beyond the EU area. Beetsma and Giuliodori (2010a) made also use of real-time fiscal data in the OECD, but they paid much attention to the changes in domestic fiscal plans which were driven by changes in cyclical conditions, not to external interactions.

Concerning discretionary policy, Agnello and Cimadomo (2009) look at the revenue side of the government budget of the European Union countries, to investigate if discretionary measures have been implemented in reaction to economic fluctuations. They establish that legislated changes in taxes and social security contributions have responded in a strongly pro-cyclical way to the business cycle. However, not only their measure of discretionary fiscal policy differs from ours (and looks at the revenue side only, while we use a more encompassing measure), but they consider EU countries, in contrast to our larger sample of OECD countries.

Neely and Rapach (2009) analyze co-movements in four measures of budget surpluses for 18 OECD countries for 1980–2008 with a dynamic latent factor model. They show that the world factor in national budget surpluses declines substantially in the 1980s and then rises throughout much of the 1990s to a peak in 2000, before declining again. This world factor explains a substantial portion of the variability in budget surpluses across countries they exhibit. Though they document a common trend, their modeling strategy does not allow them to check if the common trend is not in fact driven by some of their sample countries' influence. Moreover, it does not allow them to work on the determinants of the interrelations they exhibit. Here, not only do we work on discretionary fiscal policy (and not on aggregates that may be subject to other influences, such as the generalization of the welfare state, along the line of the case made by Lindbeck, 2008), but we deepen the analysis by uncovering the origins of the reciprocal national influences.

There is a second strand of literature, which is micro-founded and that focuses on spending interactions among countries or states (Case et al., 1993; Figlio et al., 1999; Baicker, 2001; Redoano, 2003, 2007). For instance, Case et al. (1993) estimate the effect

of one State's spending on that of its neighbors using a spatial lag model. Authors find that States' per capita expenditures are positively and significantly correlated with their neighbors' spending. These results are confirmed by Figlio et al. (1999), who check the existence of spillovers in welfare spending. Baicker (2001) also finds that each dollar of State spending causes spending in neighboring states to increase by 37 to 88 cents. Finally, Redoano (2003) estimates reaction functions for taxes, public expenditures, both aggregated and disaggregated, using a dataset including EU countries for the period 1985–1995. She finds that governments behave strategically with respect to those expenditures that are more directly comparable, such as expenditures in education: An increase by one dollar spent in education by the neighbors increases the same expenditure in a country by over 40 cents. Nevertheless, to our knowledge, all these papers use broad measures for spending and gross fiscal data. We believe that to properly investigate interactions between countries, we need to isolate the sole part which is in the hand of policy makers. This is the discretionary part of public spending and tax receipts which are fully under control by governments.

Our results show that interactions do exist among our sample countries. Interestingly, these interactions are all the more important that countries are close (closeness being defined by relative per capita GDP or by geographic distance). Another important result is that political cycles are influential, as we find evidence of an opportunistic behavior of OECD countries' governments for discretionary public spending. Finally, the disciplining device of the EU fiscal framework is shown to be ineffective.

We proceed in two steps. In section 2, we detail the data on which we rely, then define and compute our measure of discretionary fiscal policy. In section 3, we provide estimates of interactions among OECD countries, while Section 4 tests for the determinants of these interactions. Finally, section 5 contains our conclusions and provides hints for further research.

1. Measuring discretion in fiscal policy

It is well-known that macro-fiscal data are blurred by many influences that make it difficult to extract their discretionary part. The latter part, however, is important to gauge fiscal policy's design and effectiveness because it is the sole part that is in the hands of policymakers. Consequently, this is the part for which policymakers can be made accountable. In the following, we will concentrate on policy design rather than effectiveness.

In order to proceed with extractions of discretionary fiscal stances, adjustments to interactions with other policies (from central banks and foreign policymakers' decisions) have to be implemented, and other adjustments to business-related cyclical variations are also required. In the end, it is thus possible to relate discretionary fiscal policy in one country to its counterpart in another country and to ask whether a causal relationship might appear, whether discretionary interactions (if they do exist) change with political closeness, with geographical borders, with good and bad times, etc., and how these interactions occur: between public expenditures and between tax policy.

Three different approaches for measuring the discretionary part of fiscal policy have already been followed. First, Romer and Romer (2009, 2010) made use of their (1989) narrative approach on monetary policy for tax policy issues[3]: they gathered information on episodes of new discretionary tax changes that successive US governments implemented every year, distinguishing these changes according to four different sets of motivation: financing a new spending program, reducing past deficits, implementing a countercyclical policy or raising economic growth in the long run. Then, they assessed the influence of some "shocks" to public spending and the economy. Though the approach is very appealing as it sticks to "real-time" and concrete discretionary fiscal episodes, it remains that gathering the same kind and quality of information for 18 countries is a task loaded with methodological issues. Moreover, in an international setting, it might well be in the end that identified discretionary fiscal shocks using the

[3] The seminal paper was Ramey and Shapiro (1998).

narrative approach, which would mainly consist in fixing a dummy variable rather than estimated pure tax shocks expressed in percentage points of GDP as in Romer and Romer (2009, 2010), might not be able to trace back interactions with delayed effects of other large fiscal shocks.

Second, Mountford and Uhlig (2009) identify fiscal shocks in the case of the US, using VAR with sign restrictions on the dynamics of the fiscal variables, and imposing orthogonality to a business cycle shock and a monetary policy shock.[4] In the first case, the fiscal shock is meant to be clearly disconnected to automatic stabilizers whereas, in the second case, the fiscal shock is separated from monetary policy interferences. Though the identifying assumptions "are close to minimal" according to the authors, the identification procedure is not immune from prerequisites which somewhat blur the relevance of an empirical characterization of discretionary fiscal policy. As for the identification of the business cycle shock, the assumption that it requires a co-movement of consumption, GDP and non-residential investment for four quarters following the shock may not perfectly characterize automatic stabilizers. The latter should start playing *either* if a shock on consumption *or* a shock on non-residential investment occurred, not because both variables co-move. Thus, identifying the date of the discretionary policy can be difficult. Let us take an example. As reported in figure 1, neither the negative US real GDP growth rate of 2008Q1, nor the consecutive third and fourth quarters of 2008 of negative US real GDP growth rate can be labeled "business cycle shocks" according to Mountford and Uhlig (2009). As a consequence, if we were to apply Mountford and Uhlig (2009)'s identification procedure beyond 2000, the end of their sample, US fiscal shocks during 2008 would not be orthogonal to the business cycle: discretionary fiscal policy and the automatic stabilizers could not be easily separated.

Insert Figure 1

Third, Blanchard and Perotti (2002) used Structural VARs to extract the discretionary part of fiscal policy in a dynamic and a-theoretical model in which they assume that GDP

[4] Canova and Pappa (2007) identify fiscal shocks, at the regional level in the case of the US.

reacts sluggishly to fiscal policy shocks. In contrast with Mounford and Uhlig (2009), Blanchard and Perotti (2002) cannot deal with lags between the announcement and implementation of changes in fiscal policy. However, their identification of fiscal policy shocks depends on computed elasticities of pairs of the dependent variables which do not require any type of restrictions. Because the method can be made automatically systematic, it can be applied to many countries. Recent applications to France (Biau and Girard, 2005), Ireland (Benetrix and Lane, 2009), Italy (Giordano et al., 2007), the UK (Creel et al., 2009), and the Euro area taken as a whole (Burriel et al., 2010) testify for this property. Using this methodology, we provide a comparative assessment for 18 OECD countries.

In the following, we describe the method to obtain adjusted fiscal data that characterize the discretionary part of gross fiscal data. We obtain discretionary public spending and tax receipts data for 18 OECD countries between 1974 and 2008. Gross data were taken from the OECD database. Public spending data were not free of net interest: our choice was dictated by missing net-of-interest spending data in a few countries. Anyway, spending data were finally adjusted for long-run interest rates (except in Greece where only short-run interest rates were available for the entire period). The list of countries is the following: Australia, Austria, Belgium, Canada, Germany, Denmark, Spain, Finland, France, United Kingdom, Greece, Ireland, Italy, Japan, Netherlands, Norway, Sweden and the United States.[5]

For each country, a single VAR model has been estimated. Let g, τ, π, r and y denote respectively the values of government spending, tax revenues, consumer inflation, the long-term interest rate and GDP growth. Public finance variables are expressed in percent of GDP; all variables are expressed in percentage. Let $\boldsymbol{Y_t}$ and $\boldsymbol{U_t}$ denote the vector of endogenous variables and of the reduced-form residuals of the VAR, respectively. The reduced form VAR can be written:

[5] Ideally, one would use quarterly data. However, quarterly public finance data are, more often than not, interpolations of yearly data (see also Giuliodori and Beetsma, 2010a, for complementary justifications). Though the limitation has to be kept in mind, making use of quarterly data would clearly blur the information we want to reveal.

$$\boldsymbol{Y_t = A(L)Y_{t-1} + U_t}$$

where $\boldsymbol{Y}_t = [g_t, \tau_t, y_t, \pi_t, r_t]'$ and $\boldsymbol{U_t} = [u_{g,t}, u_{\tau,t}, u_{y,t}, u_{\pi,t}, u_{r,t}]'$. ***A(L)*** is the L-year lag operator. With respect to the usual tests (Akaike information criterion, Schwartz information criterion), the optimal lag has been set equal to 3 for all countries. Except in Greece, Italy and the United Kingdom, where Johansen cointegration tests point to the existence of one cointegration equation (at 1 %) for each country, the remaining 15 countries do not show a significant cointegration relation. For these 15 countries, a VECM is not required; for the first three countries, and for the sake of comparison with the remaining 15, we decided not to implement a VECM experiment. We thus first performed a canonical VAR model.

However, the residuals of this VAR model are uninformative on the response of endogenous variables to shocks: they are only estimation errors. Hence, in order to extract the discretionary part of fiscal data – public spending and tax receipts-, one has to isolate the structural part of the respective canonical residual. Thus, while the canonical residual of, say, the tax receipt collects information on all the unexpected movements of that variable, the corresponding structural residual is obtained by eliminating all the instantaneous feedback mechanisms triggered by evolutions of the other endogenous variables.

The identification methodology consists in isolating structural residuals according to the following procedure. Following Blanchard and Perotti (2002), we begin with by writing the reduced form canonical residuals of the two fiscal policy variables as linear combinations of the structural and automatic components:

$$\begin{aligned} u_{g,t} &= \alpha_{g,y} u_{y,t} + \alpha_{g,\pi} u_{\pi,t} + \alpha_{g,r} u_{r,t} + e_{g,t} \\ u_{\tau,t} &= \alpha_{\tau,y} u_{y,t} + \alpha_{\tau,\pi} u_{\pi,t} + \alpha_{\tau,r} u_{r,t} + e_{\tau,t} \end{aligned} \qquad (1)$$

where $e_{g,t}$ and $e_{\tau,t}$ are the structural shocks to the two fiscal policy variables. The first three terms on the RHS of each equation in block (1) capture the automatic responses of fiscal policy to a change in GDP growth, in inflation and in the interest rate (the

elasticities, denoted by α). The last term captures the structural policy component which will be interpreted as the discretionary component.

Elasticities ($\alpha_{\tau,y}, \alpha_{g,y}, \alpha_{g,\pi}, \alpha_{\tau,\pi}, \alpha_{g,r}, \alpha_{\tau,r}$) are computed as the estimation of the log change of the variable – the canonical residual on public spending or tax receipts - on the contemporary log change of either GDP, inflation, or the interest rate[6].

After this step, canonical residuals are corrected for GDP growth (the automatic stabilisers), inflation and interest rate variations, in order to extract the respective discretionary parts of spending and tax variables. We can consequently define the cyclically-adjusted (*CA*) public spending and tax receipts as their respective canonical residuals net of the effects of the other contemporaneous endogenous variables, hence:

$$\begin{aligned} u_{g,t}^{CA} &\equiv u_{g,t} - (\alpha_{g,y} u_{y,t} + \alpha_{g,\pi} u_{\pi,t} + \alpha_{g,r} u_{r,t}) = e_{g,t} \\ u_{\tau,t}^{CA} &\equiv u_{\tau,t} - (\alpha_{\tau,y} u_{y,t} + \alpha_{\tau,\pi} u_{\pi,t} + \alpha_{\tau,r} u_{r,t}) = e_{\tau,t} \end{aligned} \qquad (2)$$

As a consequence, without any theoretical priors, estimations errors of the canonical VAR are adjusted for changes in the macroeconomic environment. The ensuing structural component can be interpreted as discretionary because it is neither related to the other endogenous variables nor to their unexpected variations.

Though the method owes to Blanchard and Perotti (2002), the present elaboration does not completely endorse their identification strategy. Beyond the introduction of automatic responses to macroeconomic shocks in the adjusted residual of public spending and tax receipts, Blanchard and Perotti (2002) adjusted public spending (tax receipts) for the instantaneous interaction with tax receipts (public spending). Nevertheless, their identification methodology required to fix to zero one of these two potential interactions:

[6] Two other computation methodologies could have been implemented. First, taking all the taxes into account (from income to social contributions), one could compute a weighted-average of tax elasticities where weights would depend on the respective contribution of taxes to tax revenues. Second, like in Blanchard and Perotti, (2002), overall tax elasticity to GDP could be a weighted average of the product of the elasticity of each tax to its own base and the elasticity of its tax base to GDP. The methodology which was preferred in this paper is the simplest to be performed uniformly for a large sample of countries.

identifying one structural residual – e.g. the public spending one - as the canonical residual – on public spending -, it is possible to regress the second canonical residual – on tax receipts – on the first one, and identifying the new residual as the second structural residual – on tax receipts -. Such an assumption however requires taking step in the debate between two competing theories: the "Spend & Tax" and the "Tax & Spend" public finance frameworks (see Musgrave, 1966), depending on which variable must be constrained by the other when designing policy action. Within the VAR model, a first case arises where public spending is left free to affect taxes but not the opposite. In a second case, tax receipts are left free to affect public spending but not the opposite. Because there are no *a priori* reasons to consider that one of these frameworks fits better than the other for all countries at any time, we do not follow this step in the identification of structural components.

Thus we escape an unresolved discussion on the best fiscal framework for a sample of 18 countries over 40 years. Nevertheless, we do not escape the strategic issue completely: though we fix all the *contemporaneous* interactions to zero, we only do so at the macroeconomic stage of the empirical procedure; in the spatial stage of it, the analysis of the strategic interactions across tax or fiscal policies is performed.

It must also be acknowledged that the method introduces some new elements in comparison with Blanchard and Perotti (2002). Since we include the inflation and the interest rates (as in Perotti, 2004), we can adjust fiscal variables for a wide array of macroeconomic shocks. Under the assumption that long-term interest rates are related to short-term rates according to the yield curve, the adjustment of fiscal data can be interpreted as involving correction for changes in monetary policy. And, for what concerns the adjustment for foreign fiscal policies, the spatial econometric methods we implement below is a novelty in the literature on discretionary components of fiscal policy.

The values of elasticities are reported in Table 1. Expected signs are the following. Public spending (on GDP) should decrease contemporaneously with GDP, and should increase

contemporaneously with long term interest rates (provided public debt is mostly financed at floating rates), but the sign of the elasticity of public spending (on GDP) towards inflation depends on the indexation of public spending on actual inflation. The sign of the elasticity of tax receipts (on GDP) towards GDP depends on the tax structure: the more progressive the tax system, the larger (and positive) the elasticity; if taxes are lump-sum or exemptions are numerous, the elasticity can be negative: higher growth means lower tax receipts in proportion of GDP. Tax receipts (on GDP) should respond positively, provided net interests are charged with taxes; however, the sign of the related elasticity could as well be indeterminate, as a higher long term interest rate also reduces the values of bonds and stocks; consequently, reduced wealth can lead to lower tax receipts. Finally, the elasticity of tax receipts (on GDP) towards inflation depends on the indexation procedure of tax brackets.

Insert Table 1

Reported elasticities give a very diverse picture for the different countries involved under review. It is noteworthy that the sole elasticity which sign is clearly determinate, $\alpha_{g,y}$, is always negative, as expected, and statistically significant for most countries. Other elasticities testify for variations between countries in terms of degrees of indexation and tax structures. The contemporaneous impact of long term interest rates on public spending is often not statistically significant. This is not surprising as one can expect a relative inertia of total public spending *vis-à-vis* a short term change in the nominal long term interest rate.

Estimated discretionary parts of public spending and tax receipts are presented in the appendix for the 18 countries of the sample. As a matter of comparison, we report in figure 2 the government revenue shocks estimated by Mountford and Uhlig (2009), exogenous tax changes computed by Romer and Romer (2010), and our estimated discretionary part of tax receipts, all three in the case of the US. At first sight, our method gives frequent tax shocks, like Mountford and Uhlig (2009). A closer look shows that the three methods point to a discretionary increase in taxes in 1981, 1987 and 1991, and to a

decrease in 1982 and 1988. All in all, our method gives consistent results with other methods.

Insert Figure 2

Given the results obtained in this first step, we now turn to the investigation of the potential spatial interactions between the sample countries.

2. Assessing fiscal policy interactions between OECD countries

Traditionally, empirical models of public policies relate public spending or tax receipts to variables reflecting socio-economic and political characteristics of the country (or region). This amounts to assuming that fiscal policies are only influenced by observed national features. Under a linear specification, such a view leads one to estimate the following model:

$$u_{k,t}^{CA,i} = X_{i,t}\beta + \lambda_i + \lambda_t + \varepsilon_{i,t} \qquad \text{with } k = g, \tau \qquad (3)$$

where $u_{k,t}^{CA,i}$ are the per capita discretionary spending (or tax receipts) of the $i = 1, N$ countries, X is the set of exogenous national socio-economic characteristics, λ_i is a country fixed effect, λ_t is a period fixed effect and ε, a vector of i.i.d error terms.

However, strategic interactions among governments have been one of the central issues in theoretical public finance for the two last decades at least (see the survey by Wilson, 1999). As a consequence, spatial econometric techniques have recently been used to estimate inter-governmental interaction models (see Brueckner, 2003; Revelli, 2005). Building on the spatial econometric method developed by Anselin (1988), the model in equation (3) is then augmented to include the spatially lagged dependent variable:

$$u_{k,t}^{CA,i} = \rho W u_{k,t}^{CA,j} + X_{i,t}\beta + \lambda_i + \lambda_t + \varepsilon_{i,t} \qquad \text{with } i \neq j \qquad (4)$$

In this spatially lagged model, W is a weight matrix that assigns "neighbors" to each country; the spatial lagged variable WY is a weighted average of all other countries' fiscal policy, ρ being the spatial autoregressive coefficient, which gives the sign and the intensity of the impact of "neighboring" fiscal policies on one country's public decision.

A negative ρ will imply that expenditure spillovers explain the spatial correlation between countries decisions (Brueckner, 2003; Revelli, 2005). On the contrary, when ρ is positive, a form of imitation (mimicking, *i.e.* fiscal competition or yardstick competition) explains the observed interaction. For example, in the presence of tax base mobility, the fiscal policy of a government may affect the budget constraints of other governments, through capital migration (Wilson, 1999). This is the fiscal competition for mobile resources assumption. In the yardstick competition theory, information on the fiscal policy of the neighbor governments acts as a yardstick for the electorate in any given country. As a result, any country's citizens will compare the performance of their own policymakers to the neighboring ones. This encourages mimicking behavior from governments, as they do not want to be stigmatized (Salmon, 1987; Besley and Case, 1995).

Finally, a spatial auto-correlation pattern may simply reflect common shocks affecting public policy or the omission of variables (such as country characteristics) that are spatially dependent (Manski, 1993). In this case, we have a spatial autoregressive process in the error term, or a spatial error model:

$$u_{k,t}^{CA,i} = X_{i,t}\beta + \lambda_i + \lambda_t + \varepsilon_{i,t} \text{ and } \varepsilon_{i,t} = \gamma W \varepsilon j,t + m_{i,t} \quad (5)$$

where γ is the spatial correlation coefficient, W a weight matrix and m a vector of i.i.d. error terms.

Turning to estimation techniques, Anselin (1988) shows that, due to an endogeneity bias, OLS estimators are inconsistent when estimating spatial lag and spatial error models. However, instrumental variables (IV) or maximum likelihood (ML) estimation methods lead to consistent estimators (Brueckner, 2003; Elhorst, 2003).

The weight matrix, denoted *W*, defines the structure of the interaction, the "neighborhood" among countries. Since an *a priori* definition of interaction may arbitrarily influence the estimations results, we will test the robustness of our fiscal policy interaction model by using five different criteria, *i.e.* five different weight matrices.

First, to test whether our results are not an artifact of the statistical procedure in which the neighborhood variable picks up the effect of any random set of countries, we build an intentionally absurd weighting scheme.

Traditionally, to test fiscal competition, yardstick competition or spillover effects in which neighborhood is a central feature, most empirical papers use weight matrices based on geographical distance or simple contiguity. Following the relevant empirical literature, we have chosen a common geographical definition of neighborhood based on the Euclidean distance between countries (d_{ij}).[7] This scheme is given by the weight matrix W^{DIST} and imposes a smooth distance decay with weights w_{ij}, given by $1/d_{ij}$ when *i* is different from *j* (otherwise $w_{ij} = 0$).

A third set of matrix is based on economic criteria[8]. We consider the case where countries follow an economic leader, the latter being defined by her GDP per capita. The matrix W^{GDPL} assigns higher weights to countries *j* with higher GDP per capita: $w_{ij} = GDP_j / \sum_j GDP_j$ if *i* is different from *j*, and 0 otherwise. We are thus able to assess size effects. We clearly set the leader country. Matrix USLEADER gives a coefficient equal to 1 if country *j* is the United States and 0 otherwise. Trade flows between countries may also be a source of mimicking. As for W^{TRADE}, country *i* is closer to country *j* than to country *k* if the share of trade with *j* is higher than the share of trade with *k* in the total trade of country *i*.

[7] Geodesic distances are calculated following the great circle formula, which uses the geographic coordinates of the capital cities (CEPII data base).

[8] See Case et al. (1993) and Baicker (2005) for a discussion on these matrices, defining similarities between countries in terms of income, population, etc.

It is conventional in the empirical spatial literature that all these weight matrices are standardized so that the elements of each row sum to 1. Besides, we also include in our model some control variables reflecting the impact of differences in socio-economic and political factors grouped in the vector X in (3). Following the empirical literature, we include some explanatory variables that might affect fiscal policies. We expect no important impact of these variables, in validation of the first step of the empirical procedure. It has to be noted that the economic resource variables such as GDP per capita, which can be used as a measure of country income, have been removed from this step in the empirical procedure since it was already included in the first one (see above).

Our data set includes the above 18 OECD countries, considered over a period of 34 years (1975-2008). Descriptive statistics are shown in Table 2 in appendix.

The first data set of control variables, as is traditional in the literature, is composed of socio-demographic variables, such as unemployment rate, population density, and shares of under 14 (young people) and over 65 year-old in the population (old people). All these variables are available from the AMECO database (European Commission, Economic and Financial affairs). They are expected to exhibit a positive sign as they might reflect higher needs of the population they designate. The variable (old people) is designed to capture the political demand for social services by the older members of the electorate. This segment of the population constitutes an interest group with growing political power, and the variable (old people) is expected to be positively related to the size of the government.

A second group of control variables includes political data collected from the Database of Political Institutions (DPI, see Beck et al., 2001). *Left* is a dummy variable for the country partisan affiliation, which takes the value 1 if the chief executive of country i in year t belongs to a left-wing party, and 0 otherwise. We also introduce dummies for the electoral cycle. *Election year (t)* is a dummy variable, which takes the value 1 if there is a legislative election in year t. *Election year t-1* (resp. $t+1$) is a dummy variable, which

takes the value 1 the year before (resp. after) the legislative election, and zero otherwise. If there is a trend for an opportunistic political business cycle during the legislative legislature, we will observe higher discretionary public spending and lower tax receipts the year before the election or the election year.

As 13 countries of our sample are members of the European Union, the last group of explanatory variables deals with EU features. We introduce three dummies respectively for EU membership, for Eurozone membership and for the respect of the Stability and Growth Pact (SGP). EU membership and Eurozone membership take the value 1 if the country *i* in year *t* belongs respectively to the EU and to the Eurozone, zero otherwise. SGP takes the value 1 if the EU country *i*'s deficit does not respect the SGP in year *t,* and zero otherwise. A negative (positive) sign for this parameter will appear if the SGP is a constraint or a disciplining device for the manipulation of discretionary spending (taxes) in EU countries experimenting high deficits. Conversely, a positive (negative) sign will emphasize the ineffectiveness of the disciplining device for discretionary spending (taxes).

Our estimation strategy is as follows. We first estimate (3) using OLS without taking into account the potential influence of the fiscal policy set by other countries. Because serial correlation in panel data models biases the standard errors and causes the results to be less efficient, we performed the Wooldridge test (2002) to identify potential serial correlation in the idiosyncratic error term. This test does not detect the presence of such correlation. We then run the appropriate spatial tests based on the Lagrange multiplier tests in their robust version, which can detect the presence of spatial lag dependence and spatial error dependence (see Anselin et al., 1996). We also use them in their robust version which means that the robust LM-lag tests for lag dependency in presence of missing error and the robust LM-ERR tests for error dependence in presence of missing lag. If the LM test for spatial lag is more significant than the LM test for spatial error, and the robust LM test for spatial lag is significant but the robust LM test for spatial error is

not, then the appropriate model is the spatial lag model (Anselin and Florax, 1995).[9] We find spatial lag dependency for all the weighting schemes we consider.[10]

We then estimate the full model (4), the Y variable being our first step u^{CA} residuals, taking into account the influence of the other countries' fiscal policies (weighted spending decisions or tax receipts) using the maximum likelihood (ML) method.[11] As macroeconomic shocks that could be common to all countries have already been taken into account in the first step of the empirical procedure, we do not need to include time dummies. Estimation results for discretionary spending decisions and tax receipts are shown in Tables 3a and 3b.

Insert Table 3a

Insert Table 3b

According to estimates reported in tables 4a and 4b, we find both a significant and positive sign for the coefficient associated with the "neighboring" OECD countries' decisions in discretionary public expenditures and tax receipts, except for the absurd matrix. The estimation results confirm the existence of fiscal policy interactions for all weighting schemes, either based on geographical proximity or on economic leadership. This implies that geographically close countries tend to imitate each other, when they set their discretionary fiscal policy. Countries also mimic their main trade partners and

[9] Conversely, if the LM test for spatial error is more significant than the LM test for spatial lag and the robust LM test for spatial error is significant but the robust LM test for spatial lag is not, then the appropriate specification is the spatial error model.

[10] LM tests estimation results are not shown in this paper, but are available upon request from the authors.

[11] Furthermore, the normality of the residuals (the dependent variables) cannot be rejected, as the Shapiro-Wilk reveals (not shown here for space convenience but available on request). Non-normality could include overall skewness, overall tail weight differing from normal, individual outliers. Shapiro-Wilk test collapses all that onto one dimension by quantifying the straightness of a normal probability plot. The departure from a normal distribution is not statistically significant using this test.

economic leaders in the OECD like the US[12]. However, there is no interaction using the absurd alphabetical matrix. This outcome confirms that our results are not an artifact of the statistical procedure nor the effect of common shocks to countries of our sample but come from mimicking between countries.

Result 1: There are some discretionary fiscal policy interactions between OECD countries. Countries tend to imitate their geographical neighbours, their trade partners and the economic leader of the area when they set their discretionary public spending or tax receipts.

Let us now turn to the estimation results associated with the other explanatory variables. Although no parameter associated with the socio-economic or political explanatory variables is significant for tax receipts (a logical result, and a further proof of the fact that our measure of discretionary fiscal policy really measures discretion), two important results for public spending can be put to the fore.

First, dummies associated with legislative election years indicate an opportunistic use (meaning, an increase) of discretionary public spending during the election year. This gives strong evidence of a political budget cycle for discretionary public expenditures. This result contradicts Brender and Drazen (2005, 2007), who find a political deficit cycle in a large cross-section of 74 to 106 countries, a result which is driven by the experience of “new democracies”. In contrast, our sample contains "mature" democracries. Though we rehabilitate opportunistic cycles in older democracies, it has thus to be recalled that our measure of fiscal policy draws on discretionary fiscal policy, whereas Brender and Drazen (2005, 2007) use an overall deficit, *i.e.* the difference between receipts and expenditures. Moreover, our measure does not allow to differentiate between measures that may be more “visible” for voters than others, and thus measures

[12] We also check if other countries are mimicked by all other countries. We find that interactions in discretionary tax receipts are significant at 5% with Germany, Ireland and Netherlands as leader. Results are available upon request.

that may be more or less effective, from the incumbent's point of view (see Drazen and Eslava, 2010).

Result 2: There is evidence of an opportunistic behavior of OECD countries' governments for the discretionary public spending.

Second, we find evidence of ideological effects on the discretionary spending decisions, as the coefficient of the partisan affiliation (*Left*) for the chief executive is significant and negative. Left-wing chief executives seem to set lower discretionary public expenditures than right-wing chief executives. As shown by the coefficient on the *COAL*LEFT* variable, this result is not driven by the fact that Left-wing governments are more often than their Right-wing counterpart, members of a ruling coalition.

Empirics on the relationship between partisan politics and public finance, drawing on a panel of advanced economies, have generally come to mixed results (see Cusack, 1999, for a survey). Stated briefly, some showed that left-wing governments were more favorably inclined to high deficits or spending (see Cusack, 1997), though their influence was a small one in comparison with right-wing governments (see Blais et al., 1993, 1996), whereas some argued that such an influence was contingent on macroeconomic conditions (Carlsen, 1997; Cusack, 1999) and has decreased over time with the larger openness of advanced economies (Cusack, 1999). In a recent paper dedicated to public budgeting in France, Baumgartner et al. (2009) show that since 1981, no matter which political type of government has been in power, limited growth in spending has been the rule. Because right-wing governments had always been in power between 1958 and 1981, Baumgartner et al. (2009) show that though differences are small, right-wing governments have been the highest spenders.

Hence, our result is not completely surprising. Contrary to the above-mentioned literature, it stems from the use of discretionary fiscal measures and thus does not hinge

on spending measures like unemployment benefits which are supposedly of greater matter for left-wing governments than for right-wing ones, supposedly weighing the costs of inflation higher. As a matter of fact, a complementary interpretation of the negative coefficient associated with the (Left) variable, could be that left-wing governments are more concerned than right-wing ones by the policy consequences of discretionary fiscal policy. On the contrary, right-wing governments could be more prone to use such a policy if they think it is associated with political (electoral) gains, along the lines of Gilardi's results (2010) with unemployment benefit policy. Moreover, our result is not surprising if a strategic use of debt exists, à la Persson and Svensson (1989), a hypothesis that can be consistent with this result and could not have been exhibited before. Overall public spending might well be raised by incumbents, hence feeding debt growth, if they fear they will not be reelected, hence newly elected governments have no choice but to reduce spending whatever the economic conditions: discretionary public spending must fall. Our result would fit this analysis provided the incumbent would come from the Right, whereas the newly elected government would come from the Left.

Result 3: Left-wing chief executives set lower discretionary public spending.

The remaining explanatory variables based on socio-demographic features (unemployment rate, young people, old people, and population densities) and on European characteristics (EU and Eurozone memberships dummies) exhibit the expected sign though they never appear as significant. Once again, this confirms that only the discretionary components of fiscal policy are present in our dependent variables (*i.e.*, the first step has purged the fiscal data and our measure correctly estimates discretionary impulses). The only significant explanatory variable is the SGP dummy, which sign is positive in the discretionary public spending equation. The sign is consistently negative in the discretionary tax revenues equation, though it is not statistically significant. The coefficient on the SGP dummy shows that the EU fiscal framework is not effective in prompting a corrective effect when public deficits in the EU are above the SGP threshold. The disciplining device of the SGP is not working. This result complements Fatás and

Mihov (2003), who show that once countries are under the 3% threshold the incentives to go further (towards the final objective of close to balance or surplus budgets) are much weaker and countries reduce their efforts.

3. Further results

In this section, we first investigate the source of interdependence in national discretionary fiscal policies (4.1) and second, we check whether the belonging to the EU matters (4.2).

3.1. How to interpret the existence of interactions between OECD fiscal policies?

We here investigate more deeply the source of interdependence in national discretionary fiscal policies. As stressed by Redoano (2007), the coefficient associated with the interaction term is not negative, so our results are not supportive of spillovers caused by free riding behaviors. However, the positive interaction coefficient may come from fiscal competition, yardstick competition, a common trend or may be the effect of similar exogenous shocks.

First we can easily give up the last two explanations as the absurd matrix shows no sign of interactions. Second, to distinguish between fiscal competition and yardstick competition, we have to check if the spatial effect is stronger in election year. Yardstick competition occurs when citizens compare fiscal and spending decisions made by their incumbent with those of their neighbours (Salmon, 1987; Besley and Case, 1995). In this context, policy-makers will be particularly concerned about neighbour incumbents in election periods, and strategic interactions might be stronger at such times. A straightforward way of testing this hypothesis is to use election cycle variables (Redoano, 2007; Solé-Ollé, 2003). We interact the weighted average discretionary policy of neighbours ($Y_{j,t}$) with an election dummy (D) and estimate two interaction coefficients,

one for the year of election (ρ_1) and one for the rest of the period(ρ_2). We thus need to use a two-regime spatial lag model:

$$u_{k,t}^{CA,i} = \rho_1 D W u_{k,t}^{CA,j} + \rho_2 (1-D) W u_{k,t}^{CA,j} + X_{i,t}\beta + \lambda_i + \lambda_t + \varepsilon_{i,t} \quad (6)$$

In line with Elhorst and Fréret (2009), we use a ML estimator to deal with this specific model. Results are reported in Table 4 in appendix. We do not find a significant difference in interactions in discretionary spending. However, highly significant differences in the spatial interaction coefficient are found for the discretionary tax receipts. OECD countries mimic discretionary revenues of the economic leaders of the area (all GDP leaders or only US). The yardstick competition hypothesis cannot be rejected for discretionary tax receipts.

3.2. Does the EU matter?

Since 13 countries of our sample are EU members, we estimate equation (4) on a subsample of European countries. The estimation results, shown in Table 5 in appendix, confirm the robustness of the results found in tables 4a and 4b, while the political cycle coefficients appear stronger for the European countries than for the whole sample. The coefficient of the SGP dummy is also very robust to different specifications regarding the weight matrices and the different samples.

Another way to test if the EU matters in the interdependence of fiscal policy decisions is to allow two interaction coefficients whether the country *i* belongs or not to the EU in the full sample. We then estimate equation (6) on the full sample using as a dummy D for EU membership. In our dataset, 8 countries were EU members while at the end of the period, 13 out of 18 are members of the EU. On the one hand, EU membership may cause more interdependent decisions between countries, as countries are constrained in the policy tools available to compete with other European countries. As stated by Redoano (2007): “they move in a similar competitive and institutional environment and are subject to

similar budget and political constraints, moreover policy makers have more occasions to meet and discuss formally or informally their plans". On the other hand, EU non members may engage in a more competitive behavior than EU countries, especially if they want to convince EU states to accept their future membership. Table 6 (in appendix) shows that the degree of interaction does not differ whether the country belongs or not to the EU. This result is obtained for both measures of discretionary policy.

4. Conclusion

In this paper, we investigate the relationships between the discretionary components of fiscal policies, for a sample of 18 OECD countries, during the 1974-2008 period. In a first step, we build two indicators of discretionary fiscal policy, considered as the residual components of a VAR model: one for public spending, and one for tax receipts.

The second step provides estimates of discretionary fiscal policy interactions between these OECD countries using spatial econometrics. Our results confirm the existence of interactions between neighboring countries' public decisions, where neighborhood is defined by economic proximity as well as by geography. We also find evidence of an opportunistic behavior of OECD countries' governments for the discretionary public spending, even stronger for the sub-sample of European countries. Moreover, left-wing chief executives seem to set lower discretionary public expenditures than right-wing chief executives, which could reveal the presence of a strategic use of deficits by right-wing incumbents. Finally, the disciplining device of the European Union fiscal framework is shown to be ineffective. Future research may be to investigate the link between public spending and tax receipts.

Acknowledgements

The authors acknowledge financial assistance from the French National Research Agency (grant FRAL022). They have benefited from comments by Cristina Badarau-Semenescu, Lars Borge, Martial Foucault, Jean-Luc Gaffard, Marcel Gérard, Jurgen von Hagen, Hakim Hammadou, Jérôme Héricourt, Grégory Levieuge, Ioana Moldovan, Edoardo di Porto, Laurence Rioux, Blandine Zimmer, participants in the Public Economics Day (Paris 1), the European Public Choice Society 2010 meeting, the 44th Canadian Economics Association conference, Journées AFSE 2010, the 27th symposium on Money Banking and Finance Bordeaux 2010, AFSE Annual Conference 2010, and seminar participants in Bonn. The usual disclaimer applies.

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Appendix

Insert Table 2
Insert Table 4
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Tables

Table 1: Elasticities

	$\alpha_{g,y}$	$\alpha_{g,r}$	$\alpha_{g,\pi}$	$\alpha_{\tau,y}$	$\alpha_{\tau,r}$	$\alpha_{\tau,\pi}$
Australia	-0.03	0.05*	0.0001	0.004	0.03*	0.002
Austria	-0.04	-0.03*	-0.02	-0.03	0.005	0.002
Belgium	-0.04	-0.005	0.003	-0.02	-0.03*	-0.003
Canada	-0.04	-0.004	0.00003	-0.004	0.05	0.004
Germany	-0.03	-0.03*	-0.009	-0.003	-0.02*	-0.004
Denmark	-0.02*	0.02*	-0.004	-0.005	-0.02*	0.001
Spain	-0.08	0.04*	-0.03*	-0.03*	0.0006	-0.04*
Finland	-0.03	-0.03*	-0.03	-0.01*	0.10	0.0003
France	-0.02	-0.007	-0.002	-0.008	0.002	-0.002
UK	-0.05	0.09	0.008	-0.03*	0.08	0.02*
Greece	-0.09	0.02*	-0.002	-0.07	0.004	-0.005
Ireland	-0.04	0.12*	-0.007	-0.04	0.03*	-0.007
Italy	-0.06	-0.0003	-0.02*	-0.02	0.002	-0.03
Japan	-0.02*	-0.02*	-0.004	0.008	0.03*	0.003
Nld.	-0.04	0.009	0.0005	-0.02	-0.02*	0.004
Norway	-0.04	0.04*	-0.01*	0.02	0.06	0.008
Sweden	-0.01*	0.04*	-0.002	-0.007	0.002	-0.009
USA	-0.04	-0.04*	-0.01*	0.0005	0.06	0.02*

Sources: OECD, authors' calculations.
*: not statistically different from 0.

Table 2: Descriptive statistics

	Sources	Mean	Std Dev.	Min	Max
Unemployment rate	AMECO	7.05	3.46	1.10	19.50
Young people	AMECO	19.88	3.25	13.74	30.90
Old people	AMECO	13.99	2.35	7.91	20.03
Population density	AMECO	83.03	78.96	1.15	265.50
Election year	DPI				
Left	DPI				
EU membership	European Commission				
Eurozone membership	European Commission				
SGP	European Commission and AMECO				
AMECO : database of the European Commission, Economic and Financial affairs					
DPI : Database of Political Institutions, World Bank					

Table 3a. Estimation results-discretionary spending

Dependent variable	Spending				
Weight matrix	WABSURD	WDIST	WGDP LEADER	US LEADER	Wtrade
W*Y	0.004	0.28***	0.17***	0.06*	0.20***
	(0.09)	(3.99)	(2.62)	(1.75)	(3.66)
Unemployment rate	0.02	0.01	0.01	0.02	0.01
	(1.39)	(1.29)	(1.29)	(1.39)	(1.26)
Young people	0.02	0.02	0.02	0.02	0.02
	(1.15)	(1.21)	(1.31)	(1.22)	(1.33)
Old people	0.02	0.01	0.01	0.01	0.02
	(0.63)	(0.64)	(0.67)	(0.64)	(0.65)
Population density	0.0002	0.0001	0.0001	0.0002	0.0002
	(0.32)	(0.30)	(0.31)	(0.33)	(0.34)
Election year (t)	0.19**	0.19**	0.19**	0.19**	0.19**
	(2.48)	(2.55)	(2.56)	(2.51)	(2.55)
Election year (t+1)	-0.04	-0.03	-0.04	-0.04	-0.04
	(-0.63)	(-0.55)	(-0.65)	(-0.66)	(-0.58)
Election year (t-1)	0.11	0.12*	0.11	0.11	0.12*
	(1.55)	(1.73)	(1.52)	(1.51)	(1.73)
Left	-0.16**	-0.14*	-0.15**	-0.15**	-0.14*
	(-2.04)	(-1.89)	(-1.97)	(-2.00)	(-1.82)
COAL LEFT	0.04	0.04	0.05	0.05	0.03
	(0.48)	(0.38)	(0.48)	(0.49)	(0.33)
EU membership	0.003	0.0004	0.01	0.006	-0.006
	(0.03)	(0.004)	(0.12)	(0.05)	(-0.06)
Eurozone membership	0.05	0.07	0.04	0.05	0.06
	(0.55)	(0.73)	(0.49)	(0.55)	(0.74)
SGP	0.21**	0.18*	0.20**	0.20**	0.20**
	(2.14)	(1.93)	(2.10)	(2.14)	(2.11)
Log likelihood	-641.7	-634.3	-637.9	-639.6	-634.7

Notes: 612 observations. Spatial fixed effects are included. T-Student in parentheses
** significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level*

Table 3b: Estimation results-discretionary tax receipts

Dependent variable	Tax receipts				
Weight matrix	WABSURD	WDIST	WGDP LEADER	US LEADER	Wtrade
W*Y	0.05	0.36***	0.24***	0.07*	0.22***
	(1.29)	(5.44)	(4.03)	(1.82)	(4.12)
Unemployment rate	-0.001	-0.003	-0.003	-0.001	-0.001
	(-0.12)	(-0.28)	(-0.26)	(-0.09)	(-0.11)
Young people	-0.008	-0.003	-0.003	-0.006	-0.003
	(-0.43)	(-0.21)	(-0.19)	(-0.31)	(-0.21)
Old people	-0.000	-0.0001	0.0008	0.001	-0.0003
	(-0.00)	(-0.004)	(0.03)	(0.06)	(-0.01)
Population density	-0.0001	-0.0001	-0.0001	-0.0001	-0.0001
	(-0.25)	(-0.28)	(-0.20)	(-0.22)	(-0.23)
Election year (t)	-0.10	-0.11	-0.10	-0.09	-0.10
	(-1.34)	(-1.54)	(-1.38)	(-1.29)	(-1.40)
Election year (t+1)	-0.03	-0.02	-0.01	-0.02	-0.01
	(-0.39)	(-0.31)	(-0.26)	(-0.29)	(-0.23)
Election year (t-1)	0.04	0.05	0.05	0.05	0.05
	(0.63)	(0.73)	(0.76)	(0.71)	(0.73)
Left	-0.05	-0.04	-0.05	-0.06	-0.05
	(-0.73)	(-0.62)	(-0.76)	(-0.75)	(-0.70)
COAL LEFT	-0.009	-0.03	-0.02	-0.008	-0.01
	(-0.09)	(-0.32)	(-0.18)	(-0.08)	(-0.16)
EU membership	-0.07	-0.05	-0.08	-0.07	-0.07
	(-0.71)	(-0.56)	(-0.76)	(-0.76)	(-0.66)
Eurozone membership	0.10	0.08	0.10	0.11	0.11
	(1.13)	(0.92)	(1.12)	(1.16)	(1.18)
SGP	-0.14	-0.10	-0.12	-0.14	-0.13
	(-1.59)	(-1.08)	(-1.31)	(-1.57)	(-1.41)
Log likelihood	-633.6	-622.5	-627.9	-633.3	-627.1

Notes: 612 observations. Spatial fixed effects are included. T-Student in parentheses
** significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level*

Table 4: Source of the interactions in the OECD

Dependent variable	Spending				Tax receipts			
Weight matrix	WD	WGDP LEADER	US LEADER	Wtrade	WD	WGDP LEADER	US LEADER	Wtrade
ELECT*W*Y	0.30***	0.25***	0.16***	0.29***	0.43***	0.48***	0.24***	0.35***
	(3.21)	(3.27)	(3.20)	(4.35)	(4.97)	(7.15)	(5.37)	(5.48)
(1-ELECT)*W*Y	0.29*	0.28**	0.14*	0.20*	0.21	0.04	-0.03	0.10
	(1.80)	*(2.35)*	(1.75)	*(1.84)*	*(1.36)*	(0.37)	(-0.46)	(0.95)
Unemployment rate	0.01	0.01	0.02	0.01	-0.003	-0.005	-0.004	-0.002
	(1.28)	*(1.23)*	*(1.35)*	*(1.21)*	*(-0.31)*	(-0.48)	(-0.37)	(-0.19)
Young people	0.02	0.02	0.03	0.02	-0.003	-0.002	-0.004	-0.004
	(1.21)	*(1.39)*	*(1.43)*	*(1.37)*	*(-0.20)*	(-0.10)	(-0.24)	(-0.20)
Old people	0.01	0.02	0.02	0.01	0.0006	0.0007	0.0007	-0.002
	(0.64)	*(0.70)*	*(0.68)*	*(0.64)*	*(0.02)*	(0.03)	*(0.027)*	(-0.07)
Population density	0.0001	0.0001	0.0002	0.0002	-0.0001	-0.0008)	-0.0003	-0.0001
	(0.30)	*(0.31)*	*(0.38)*	*(0.34)*	*(-0.32)*	(-0.14)	*(-0.06)*	(-0.29)
Election year (t)	0.19**	0.20***	0.20**	0.19***	-0.11	-0.09	-0.09	-0.10
	(2.55)	*(2.60)*	*(2.56)*	*(2.58)*	*(-1.49)*	*(-1.26)*	*(-1.17)*	(-1.34)
Election year (t+1)	-0.?03	-0.04	-0.04	-0.04	-0.02	-0.009	-0.009	-0.01
	(-0.54)	*(-0.65)*	*(-0.64)*	*(-0.56)*	*(-0.33)*	*(-0.13)*	*(-0.13)*	*(-0.18)*
Election year (t-1)	0.12*	0.11	0.11	0.13*	0.05	0.06	0.06	0.05
	(1.74)	*(1.51)*	*(1.49)*	*(1.81)*	*(0.72)*	*(0.84)*	*(0.83)*	*(0.76)*
Left	-0.14*	-0.16*	-0.15*	-0.13*	-0.04	-0.04	-0.04	-0.04
	(-1.88)	*(-1.94)*	*(-1.93)*	*(-1.75)*	*(-0.60)*	*(-0.57)*	*(-0.60)*	*(-0.61)*
COAL LEFT	*0.04*	*0.05*	*0.04*	*0.03*	*-0.03*	*-0.03*	*-0.01*	*-0.02*
	(0.38)	*(0.47)*	*(0.43)*	*(0.28)*	*(-0.34)*	*(-0.28)*	*(-0.13)*	*(-0.27)*
EU membership	0.0005	0.02	0.003	-0.006	-0.05	-0.07	-0.07	-0.06
	(0.005)	*(0.17)*	*(0.03)*	*(-0.05)*	*(-0.57)*	*(-0.69)*	*(-0.71)*	(-0.60)
Eurozone membershi	0.06	0.04	0.05	0.07	0.08	0.08	0.08	0.10
	(0.74)	*(0.46)*	*(0.60)*	*(0.81)*	*(0.90)*	*(0.95)*	*(0.96)*	*(1.16)*
SGP	0.18*	0.19**	0.19**	0.19**	-0.09	-0.11	-0.14	-0.12
	(1.92)	*(2.07)*	*(2.01)*	*(2.06)*	*(-1.07)*	*(-1.18)*	*(-1.51)*	*(-1.38)*
Wald test rho	*0.007*	*-0.04*	*0.02*	*0.08*	*0.22*	*0.44****	*0.27****	0.25*
	(0.03)	*(-0.25)*	*(0.26)*	*(0.63)*	*(1.15)*	*(3.25)*	*(3.23)*	(1.84)
Log likelihood	-633.9	-636.1	-637.7	-632.6	-621.6	-622.2	-627.9	-623.7

Notes: 612 observations. Spatial fixed effects are included.

** significant at the 10% level; ** significant at the 5% level; *** significant at the 1% leve*

Table 5: Estimation results for EU-13

Dependent variable	Spending					Tax receipts				
Weight matrix	WABSURD	WDIST	WGDP LEADER	Germany LEADER	WTRADE	WABSURD	WDIST	WGDP LEADER	Germany LEADER	WTRADE
W*Y	0.003	0.28***	0.26***	0.04	0.25***	0.01	0.35***	0.30***	0.09*	0.32***
	(0.06)	(3.71)	(3.78)	(0.94)	(3.32)	(0.36)	(4.93)	(4.43)	(1.82)	(4.53)
Unemployment rate	0.02	0.01	0.01	0.01	0.01	0.01	0.008	0.009	0.01	0.01
	(1.18)	(1.05)	(0.92)	(1.13)	(1.04)	(1.02)	(0.55)	(0.61)	(0.99)	(0.80)
Young people	0.0001	0.001	0.003	0.001	0.002	-0.02	-0.01	-0.01	-0.02	-0.02
	(0.004)	(0.06)	(0.15)	(0.07)	(0.11)	(-0.96)	(-0.77)	(-0.80)	(-0.93)	(-0.90)
Old people	-0.06	-0.06	-0.06	-0.06	-0.06	-0.02	-0.01	-0.02	-0.02	-0.03
	(-1.45)	(-1.39)	(-1.43)	(1.46)	(-1.41)	(-0.53)	(-0.39)	(-0.57)	(-0.57)	(-0.64)
Population density	0.003	0.002	0.002	0.002	0.002	-0.0005)	-0.0002	-0.0009	-0.0005	-0.0008
	(0.57)	(0.54)	(0.49)	(0.56)	(0.49)	-0.10	(-0.05)	(-0.18)	(-0.10)	(-0.17)
Election year (t)	0.34***	0.34***	0.36***	0.34***	0.35***	(-0.12	-0.12	-0.12	-0.12	-0.12
	(3.80)	(3.94)	(4.11)	(3.87)	(3.96)	(-1.37)	(-1.37)	(-1.39)	(-1.37)	(-1.39)
Election year (t+1)	-0.02	-0.02	-0.004	-0.01	-0.01	-0.02	-0.01	-0.01	-0.01	-0.01
	(-0.22)	(-0.21)	(-0.05)	(-0.18)	(-0.14)	(-0.19)	(-0.12)	(-0.13)	(-0.14)	(-0.14)
Election year (t-1)	0.12	0.13	0.14*	0.12	0.13	0.04	0.06	0.06	0.05	0.05
	(1.41)	(1.61)	(1.65)	(1.43)	(1.63)	(0.51)	(0.71)	(0.70)	(0.56)	(0.65)
Left	-0.06	-0.05	-0.05	-0.05	-0.04	-0.11	-0.11	-0.11	-0.12	-0.10
	(-0.56)	(-0.46)	(-0.46)	(-0.54)	(-0.44)	(-1.11)	(-1.07)	(-1.09)	(-1.16)	(-1.05)
LEFT COAL	-0.09	-0.10	-0.11	-0.10	-0.11	0.01	-0.01	-0.002	0.008	-0.01
	(-0.79)	(-0.84)	(-0.90)	(-0.86)	(-0.95)	(0.11)	(-0.10)	(-0.02)	(0.06)	(-0.11)
EU membership	0.03	0.03	0.04	0.02	0.02	-0.14	-0.10	-0.11	-0.14	-0.13
	(0.22)	(0.26)	(0.28)	(0.18)	(0.16)	(-1.01)	(-0.74)	(-0.86)	(-1.04)	(-0.93)
Eurozone membership	0.17	0.16	0.17	0.18	0.18	0.15	0.09	0.13	0.15	0.14
	(1.31)	(1.25)	(1.31)	(1.38)	(1.37)	(1.11)	(0.73)	(1.04)	(1.16)	(1.13)
SGP	0.23**	0.20**	0.22**	0.22**	0.21**	-0.17	-0.11	-0.10	-0.16	-0.12
	(2.24)	(1.98)	(2.12)	(2.14)	(2.07)	(-1.57)	(-1.13)	(-1.00)	(-1.55)	(-1.18)
Log likelihood	-466.9	-460.2	-461.1	-466.2	-461.2	-472.6	-462.9	-465.3	-470.3	-462.9

Notes: 442 observations. Spatial fixed effects are included.

** significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level*

Table 6: Interactions in the OECD (Joining the EU)

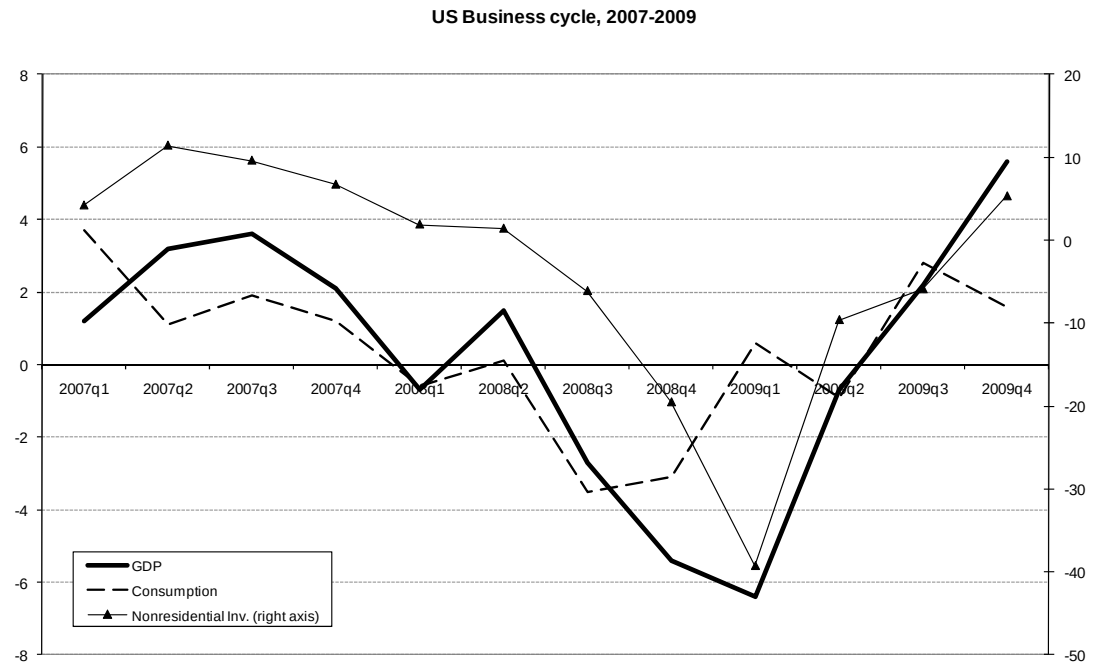
Dependent variable	Spending				Tax receipts			
Weight matrix	WD	WGDP LEADER	US LEADER	WTRADE	WD	WGDP LEADER	US LEADER	WTRADE
EU*W*Y	0.40***	0.25**	0.16***	0.28***	0.30**	0.28***	0.21***	0.17**
	(3.09)	(2.41)	(2.79)	(3.85)	(2.45)	(3.13)	(3.59)	(2.22)
(1-EU)*W*Y	0.23**	0.27***	0.14***	0.23***	0.40***	0.41***	0.12**	0.37***
	(2.42)	(3.12)	(2.87)	(3.00)	(4.65)	(5.44)	(2.46)	(5.17)
Unemployment rate	0.01	0.01	0.01	0.01	-0.003	-0.004	-0.002	-0.002
	(1.19)	(1.24)	(1.34)	(1.19)	(-0.29)	(-0.39)	(-0.24)	(-0.16)
Young people	0.02	0.02	0.02	0.02	-0.004	-0.001	-0.003	-0.003
	(1.22)	(1.38)	(1.30)	(1.39)	(-0.23)	(-0.08)	(-0.20)	(-0.17)
Old people	0.02	0.02	0.02	0.02	0.00004	0.001	0.001	0.0009
	(0.65)	(0.69)	(0.67)	(0.67)	(0.001)	(0.06)	(0.06)	(0.03)
Population density	0.0001	0.0001	0.0002	0.0002	-0.0002	-0.0001	-0.0001	-0.0001
	(0.30)	(0.31)	(0.34)	(0.36)	(-0.31)	(-0.20)	(-0.21)	(-0.27)
Election year (t)	0.20***	0.20***	0.19**	0.19***	-0.11	-0.10	(-0.10	-0.10
	(2.60)	(2.60)	(2.54)	(2.58)	(-1.52)	(-1.41)	(-1.36)	(-1.41)
Election year (t+1)	-0.03)	-0.05	-0.05	-0.04	-0.02	-0.01	-0.02	-0.01
	(-0.52)	(-0.65)	(-0.70)	(-0.56)	(-0.33)	(-0.22)	(-0.30)	(-0.24)
Election year (t-1)	0.12*	0.10	0.10	0.13*	0.05	0.06	0.05	0.05
	(1.77)	(1.51)	(1.47)	(1.79)	(0.73)	(0.84)	(0.70)	(0.77)
Left	-0.14*	-0.15*	-0.15*	-0.13*	-0.04	-0.05	-0.05	-0.05
	(-1.87)	(-1.94)	(-1.93)	(-1.75)	(-0.63)	(-0.68)	(-0.71)	(-0.68)
COAL LEFT	0.03	0.05	0.05	0.03	-0.03	-0.03	-0.008	-0.03
	(0.37)	(0.48)	(0.50)	(0.28)	(-0.35)	(-0.31)	(-0.08)	(-0.30)
EU membership	0.005	0.02	0.01	-0.007	-0.06	-0.07	-0.08	-0.06
	(0.05)	(0.17)	(0.11)	(-0.07)	(-0.56)	(-0.72)	(-0.78)	(-0.63)
Eurozone membership	0.06	0.04	0.05	0.07	0.08	0.09	0.10	0.09
	(0.70)	(0.45)	(0.54)	(0.77)	(0.86)	(1.01)	(1.15)	(1.06)
SGP	0.18**	0.19**	0.20**	0.19**	-0.09	-0.10	-0.14	-0.12
	(1.98)	(2.05)	(2.13)	(2.10)	(-1.03)	(-1.14)	(-1.52)	(-1.33)
Wald test rho	0.16	-0.02	0.02	0.05	-0.09	-0.13	0.09	-0.20*
	(0.96)	(-0.13)	(0.27)	(0.48)	(-0.60)	(-1.00)	(1.12)	(-1.84)
Log likelihood	-633.5	-636.1	-636.8	-632.7	-622.1	-624.2	-630.0	-623.6

Notes: 612 observations. Spatial fixed effects are included. T-Student in parentheses

** significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level*

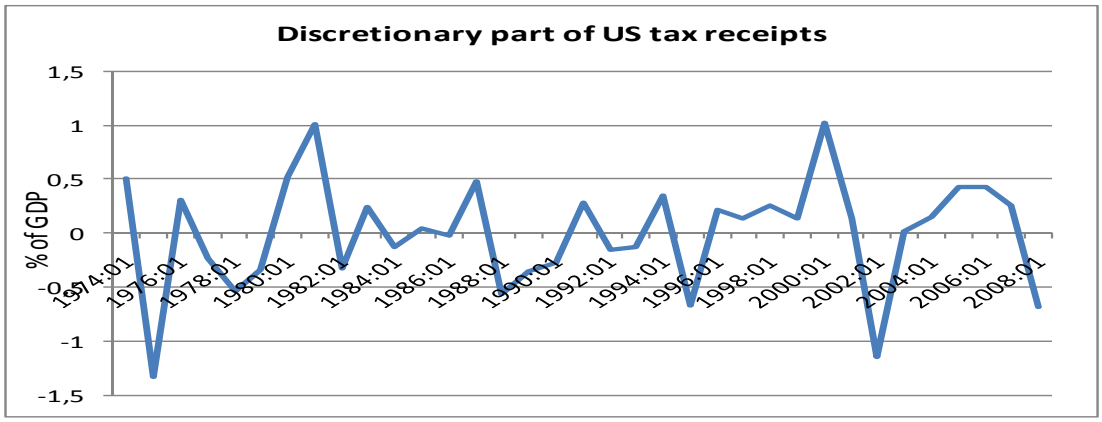
Figures

Figure 1.



Source: US Bureau of Economic Analysis.

Figure 2. Different measures of discretionary tax changes



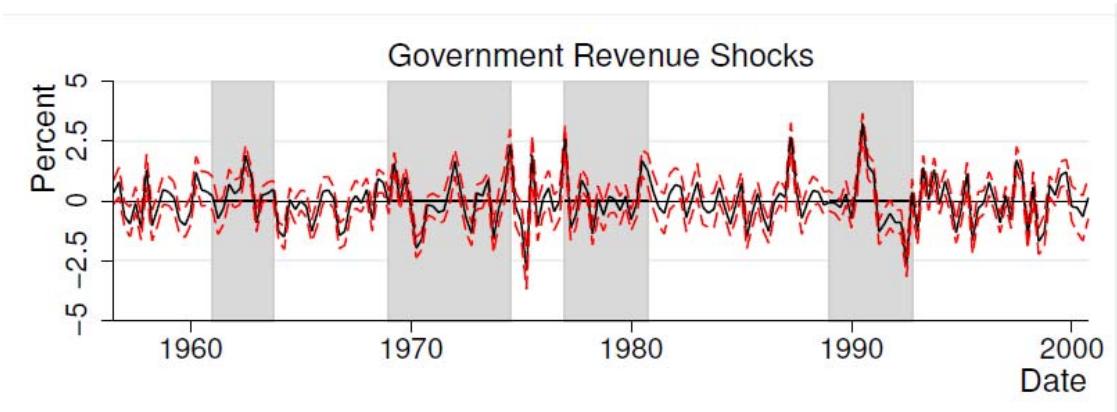
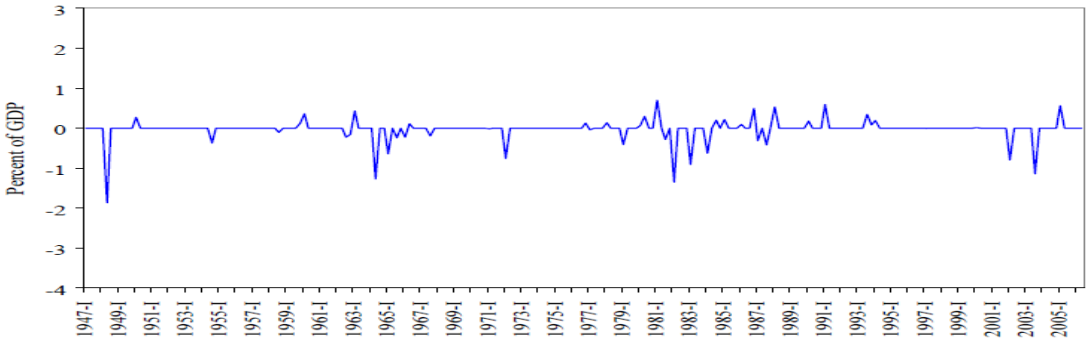


Figure 1
New Measure of Exogenous Tax Changes

a. All Exogenous Tax Changes



Sources: top figure: authors' estimations; middle figure: excerpt from figure 1 in Mountford and Uhlig (2009); bottom figure: excerpt from figure 1 in Romer and Romer (2010).