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Abstract

A growing literature documents the existence of strategic political reactions to public expenditure between rival jurisdictions. These interactions can potentially create a downward expenditure spiral ('race to the bottom') or a rising expenditure spiral ('race to the top'). However, in the course of identifying the existence of such interactions and ascertaining their underlying triggers, the empirical evidence has produced markedly heterogeneous findings. Most of this heterogeneity can be traced back to study design and institutional differences. This paper contributes to the literature by applying meta-regression analysis to quantify the magnitude of strategic inter-jurisdictional expenditure interactions, controlling for study and institutional characteristics. We find several robust results beyond confirming that jurisdictions do engage in strategic expenditure interactions, namely that strategic interactions: (i) are weakening over time, (ii) are stronger among municipalities than among higher levels of government, and (iii) appear to be more influenced from tax competition than yardstick competition, with capital controls and fiscal decentralization shaping the magnitude of fiscal interactions.

JEL-Code: H100, H500.

Keywords: inter-jurisdictional competition, yardstick competition, meta-regression, expenditure competition.

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1. Introduction

Governments often interact with each other. For example, if one government increases the supply of public schools, this is likely to impact on school supply decisions in neighboring jurisdictions. The political economy literature refers to such phenomena as 'strategic interactions', denoting policies in one jurisdiction that are correlated with those from neighboring or benchmark jurisdictions. Interactions can take the form of competition or cooperation between rival jurisdictions in the provision of publicly provided goods and services (Volden, 2005). Moreover, interactions in public sector activity between jurisdictions can occur at the same level (*e.g.* municipalities horizontally competing with each other) or between different levels (*e.g.* States competing with Federal governments).

Strategic interactions between rival jurisdictions are important because they shape public spending decisions (Berry, 2008; Besley and Rosen, 1998; Fiva and Rattso, 2006) and because they affect public policy adoption (Berry and Baybeck, 2005; Murillo and Martínez-Gallardo, 2007). One of the most common consequences of such interactions within a democracy is the shaping of public expenditures to suit the median voter's preferences in a given jurisdiction. Government strategic interactions are likely to be heterogeneous across countries depending upon the limits of fiscal capacity,[1] the contours of the political agency control, the informal rules of political culture and the effectiveness of electoral institutions.

To date it remains unclear what underpins such interactions and why some jurisdictions are more predisposed to interactions than others. The theoretical literature offers conflicting predictions regarding the existence and consequences of inter-government political interactions influencing public sector activity, and ultimately public expenditures. The direction of the reaction to a rival government's expenditure policy is theoretically ambiguous (Eggert, 2001; Brueckner, 2003). Intervening variables such as the design of

[1] By fiscal capacity we mean a class of factors such as the tax base, tax rates, the softness of the jurisdiction's budget constraint, and the availability of debt financing.

political incentives and institutional constraints are said to be important in shaping both the direction and magnitude of inter-government interactions (Oates, 1999; Besley, 2006; Rom, 2006; Simeon, 2006). For example, classical examples of inter-governmental expenditure competition include the so-called 'race to the bottom' models which predict a downward bias in public expenditure resulting from inter-jurisdictional competition (Oates, 1972). In these models, rival jurisdictions are assumed to exhibit strong political and fiscal incentives to compete for mobile factors. Taxes and subsidies are then used to attract high-income households, while also trying to discourage low-income households and welfare dependents.[2] In a simple model where no other variables are accounted for, such inter-jurisdictional competition is said to erode redistributive expenditure, especially the provision of important public or publically provided private goods. Consequently, public expenditure levels are said to be sub-optimal.

In contrast, when yardstick competition models are used to describe fiscal competition, it is the mechanisms of political agency that occupy the center stage. Here the emergence of a 'race to the bottom' is far from obvious. In these models, mobility is not the mechanism that gives rise to interdependence. Instead, interactions take place by comparison of some salient dimensions of public sector activity which are political rewarded in electoral contests. Strategic interactions in public sector activity can arise from yardstick competition mechanisms when the fear of electoral punishment induces incumbent governments to compete with their rivals at the same (horizontal competition) or different (vertical competition) level (Salmon, 1987; Besley and Case, 1995; Breton, 1996; Costa-Font and Rico, 2006). In such settings, median voter type models predict that when faced with re-election, governments might be reluctant to lower salient dimensions of the quality of social services (*e.g.*, hospital waiting times and school staff). The latter is especially the case if the

[2] 'Desirable' factors contribute to regional growth and development and to revenues for the jurisdiction, while 'undesirable' factors add to social welfare expenditures (Craw, 2010).

median voter uses them more than proportionally, or if the views of the median voter are highly supportive of certain sources of expenditure such as health care (Costa-Font and Pons, 2007) and education. The latter two examples of so-called 'merit goods' are important because they tend to explain the largest sources of expenditure growth in advanced economies. Basinger and Hallerberg (2004) point out that the pressure on public expenditure to converge with rival jurisdictions is moderated by 'domestic costs to reform'.

Given the above considerations, it is theoretically unclear what the effects of inter-government competition will be. Indeed, the political economy literature postulates and maps out a wide range of possibilities: jurisdictions can engage in increased spending designed to attract (*e.g.*, through schooling and recreation); they can engage in reduced spending intended to repel (*e.g.*, through welfare payments); and they can engage in free riding, enjoying spillovers from the spending of others (*e.g.*, from security and crime prevention).[3] Thus it is possible that interactions can: engender a race-to-the-bottom, say through welfare competition that results in too little spending on public inputs; engender a 'race-to-the-top' where competition for desirable factors results in too much spending on public inputs; or have no net effect on spending at all. The theoretical ambiguity increases when considering strategic interactions within multi-tiered governments, where competition occurs between the same levels of government, as well as between governments at different levels too (Besley and Rosen, 1998; Wilson, 1999).[4]

Theoretical ambiguity is matched by scattered and inconclusive empirical evidence, especially with respect to the size of public expenditure interactions, as well as the institutional and economic determinants underpinning them. Berry (2008, p. 817) correctly

[3] Moreover, they can conceivably engage in all processes simultaneously, in terms of different expenditure categories.

[4] Ambiguity emerges also with regard to regulatory competition. For example, inter-jurisdictional competition can result in a 'race to the bottom' in environmental regulations resulting in sub-optimal regulation that makes all states worse off, or it can lead to a 'race to the top' (Konisky, 2007).

argues that: "… the evidence of wasteful tax competition is largely anecdotal; the field has produced no systematic evidence of a fiscal race to the bottom …".

In this paper we assess the conflicting evidence on inter-jurisdictional strategic interactions (sometimes known as welfare spending competition). When study findings exhibit conflicting conclusions there is no clear policy advice forthcoming, unless some technique is used to weigh the evidence. We apply the statistical tools of meta-regression analysis (MRA) to analyze the existing evidence base. An assessment of the empirical literature is presently missing. Such an assessment can be developed using MRA, which we use to analyze statistically several dimensions of the literature, some of which have not been explored in the primary literature.

Our main contribution to the literature is to document that there is actually sufficient evidence from which to conclude that jurisdictions do play expenditure games and engage in strategic expenditure interactions. Whether expenditure interactions take place or not is decidedly relevant to the design of federal jurisdictions, as interactions provide an additional source of pressure for governments to improve their efficiency. We also show that it is possible to identify the causes (determinants) of inter-government competition: larger interactions appear to emerge from tax competition rather than yardstick competition, which is consistent with our previous meta-analysis which documents tax competition interactions at both the nation and municipality level (Costa-Font *et al*., 2014).

Through MRA, we take stock of the existing evidence and address four sets of issues. First, we explore whether strategic interactions or competition between rival jurisdictions does indeed affect government behavior and shape expenditure levels. Second, we examine whether expenditure interactions are more likely to occur in homogenous communities (*e.g.* local authorities within states) than at higher levels of aggregation of the government unit (*e.g.* at the state or national level). We then extend our analysis to differences between

countries. Third, we seek to identify the institutional and regulatory factors that shape between-country differences. Specifically, we investigate whether expenditure interactions arise from tax competition and/or yardstick competition. Both give raise to different mechanisms: a reaction to the threat of factor mobility or to the threat of electoral defeat of the jurisdictional incumbent (Salmon, 1987; Besley and Case, 1995; Breton, 1996). Finally, we wish to explain the large degree of heterogeneity in reported results between studies. Why do studies report different results?

MRA is particularly well suited to drawing inferences from a literature that reports diverse estimates and where there is heterogeneity in the institutional settings and econometric models adopted.[5] Our innovation is to apply MRA to explore dimensions that were not considered by the primary studies. We take advantage of the between-study heterogeneity to provide further insights into inter-jurisdiction competition and to explore additional dimensions. We do this by collecting information on the degree of capital controls, voter turnout, and the degree of decentralization of the jurisdictions investigated by primary studies. That is, we assess the evidence base by drawing upon data from within the studies themselves (such as reported estimates of strategic interactions), as well as information that was not considered by the authors, such as the degree of political participation and regulation of capital mobility that applied at the time the samples were taken. This enables us to model both the heterogeneity within the primary studies themselves (through their chosen econometric model), plus the heterogeneity in the samples used by different studies that was not previously modeled by the studies.

[5] There have been numerous applications of MRA, including Doucouliagos and Paldam (2008) on the growth effects of aid, Efendic, Pugh and Adnett (2011) on institutions and economic performance and Feld and Heckemeyer (2011) on FDI and taxation.

2. From Primary Analysis to Meta-Analysis

The fiscal interactions literature has progressed through several waves of research. The earlier empirical literature focused on welfare migration caused by competition for mobile resources.[6] The subsequent and much larger empirical literature has focused on neighborhood effects by directly estimating fiscal reaction functions. Some of these studies investigate tax competition, while others focus on expenditure competition. The literature then moved on to estimate reaction functions between jurisdictions at different levels (vertical interactions). Expenditure interactions are modeled as reaction functions of the ith jurisdiction's expenditure choices (E_{it}) in year t, depending on the choices of the jth neighboring jurisdiction at time t, plus other variables that also explain the jurisdiction's expenditure:

$$E_{it} = \gamma_0 + \gamma_1 \sum_{j \neq i} \omega_{jt} E_{jt} + \gamma_2 \sum_{v \neq i} \omega_{vt} E_{vt} + \mathbf{x}_{it}\boldsymbol{\beta} + \alpha_i + \delta_t + \mu_{it}, \quad (1)$$

where $\gamma_0, \gamma_1, \gamma_2, \boldsymbol{\beta}$ are parameters to be estimated, E_{jt} refers to the fiscal choices of the j neighboring jurisdiction at time t (horizontal expenditure competition), E_{vt} refers to the fiscal choices of the *v* higher level governments at time t (vertical expenditure competition)[7], ω_{jt} and ω_{vt} are the associated 'spatial weights' that account for the influence of rival jurisdictions, $\mathbf{x}$ is a vector of other variables that affect a jurisdiction's expenditure, and α_i and δ_t are region and time fixed effects, respectively.

In estimating Eq. (1) researchers need to make several choices. First, there is the choice of the expenditure to be modeled: total expenditure or a specific component, such as health or education. A second choice involves the appropriate weight to assign to rival

[6] There have also been some important recent papers on this theme, *e.g.*, Schaltegger, Somogyi and Sturm (2011).

[7] Actually, in contrast to the tax competition literature, very few spending/welfare competition studies include the vertical competition term.

jurisdictions. Most studies weigh geographical contiguity positively in the spatial weights matrix (ω), though studies attach different weights to geographical neighbors (*e.g.,* Case 1993 and Case, Rosen and Hines 1993), or use some alternative weighing scheme, such as cross state news media influences (Edmark, 2007) or migration patterns (Figlio, Kolpin and Reid, 1999). The third choice variable is the most efficient estimator. The main concern with Eq. (1) is the simultaneity problem created by strategic interactions between competing governments. This is a type of "spatial lag" that can create an omitted variable bias under OLS estimation. Alternatives to OLS are the Maximum Likelihood estimator, IV, and spatial GMM (Saavedra, 2000; Edmark, 2007; Kelejian and Prucha, 1998 and 1999).

Estimation of Eq. (1) generates estimates of strategic interactions (γ_1, γ_2). Our meta-analysis involves identifying studies that report such estimates and coding these estimates, as well as other characteristics of the studies. When suitably converted, these estimates can be made comparable between studies and then included in the MRA: they form the 'meta-data' for our meta-analysis. The MRA model involves regressing estimates of inter-jurisdictional expenditure interactions against a constant and a set of variables that can explain the heterogeneity in estimates, such as data, specification and estimation differences in research design:

$$r_{ij} = \beta_0 + \beta_k \mathbf{Z}_{jk} + v_{ij} \qquad \text{(j=1, 2, …L)}, \tag{2}$$

where r_{ij} are c*omparable* estimates of the ith inter-jurisdictional expenditure interactions from study j, *i.e.* the transformations of estimates of γ_1 for horizontal inter-governmental expenditure competition from Eq. (1), v_{ij} is the random error term, and $\mathbf{Z}_{jk}$ are moderator variables used to explain the large within and between study heterogeneity routinely found in economics research (Stanley and Jarrell, 1989). The logic of Eq. (2) is that reported estimates will vary as a result of sampling error and a set of variables used to reflect features of the data

used and the way in which the studies were conducted (Roberts and Stanley, 2005; Doucouliagos and Ulubasoglu, 2008). That is, estimates of interactions will vary because of genuine heterogeneity in strategic interactions and also because of heterogeneity in research design. In particular, Eq. (2) enables us to quantify the impact of misspecification and omitted variable biases in the primary literature.

In this paper, estimation of the MRA model, Eq. (2), is carried out using unrestricted weighted least squares (WLS), with standard errors adjusted for data clustering. Eq. (2) uses multiple estimates per study. Multiple estimates reported within a single study might not be statistically independent of each other, violating one of the OLS assumptions. Hence, we adjust the standard errors for data clustering, using each study in our meta-dataset as a distinct cluster (estimates are assumed to be clustered within studies).[8] Estimates of strategic interactions and their partial correlation transformations will have different variances (heteroscedasticity). This is evident from the funnel plot, Figure 1 discussed below. WLS corrects this heteroscedasticity. We use inverse variance as the weights, assigning larger weight to estimates with greater precision.

3. The Meta-Data

The data for the meta-regression analysis needs to satisfy three criteria: they should be comprehensive, comparable and representative. We followed the MAER-NET guidelines in constructing the database and conducting the MRA (see Stanley *et al.* 2013).

3.1 Study selection

We carried out a comprehensive search for all empirical studies that reported comparable estimates of inter-government expenditure competition. The search for studies involved

[8] Alternative approaches include the use of a linear hierarchical model or panel data techniques. We report such estimates as part of robustness tests.

numerous keywords and search engines, as well as checking all references cited within prior studies. We searched both published material (books and journal papers), as well as the so-called "Grey literature" (unpublished working papers, conference papers and dissertations). We searched for all studies published in either English or French. The search was terminated in December 2012. We excluded several studies that did not provide sufficient information from which we could calculate comparable effect sizes (in our case the partial correlations discussed below). This search process identified 33 studies of horizontal expenditure competition that report 369 estimates and 3 studies of vertical expenditure competition that report 20 estimates. Due to the small number of studies and estimates, we do not consider vertical expenditure competition in the rest of this paper.[9] The studies included in our meta-dataset cover only developed countries, predominantly the USA, Sweden, the UK, and Switzerland (see Table 1, column 1).

[9] This poses no problems for our MRA of horizontal expenditure competition, as the inclusion of vertical expenditure competition in the primary econometric study does not to appear to make any noteworthy difference on the estimates of horizontal expenditure competition.

As noted above, primary studies estimate some version of Eq. (1). In doing so, they differ in the way in which the dependent and explanatory variables are measured. Moreover, there are often differences in the scale of measurement used. Hence, the regression coefficients from Eq. (1) are not directly comparable across all studies (and estimates) included in the dataset. Following Doucouliagos and Ulubasoglu (2008), we converted the study results into partial correlations. Our focus in this paper is purely on the direct effect of the neighboring governments' choice variable. Hence, we abstract from the various spatial impacts (LeSage and Fischer, 2008).[10]

The partial correlations measure the degree of correlation between expenditures made by rival jurisdictions, controlling for the effects of variables that are unrelated to strategic interactions. Partial correlations are interpreted in the same manner as simple correlations. They are a unit-less measure of the strength and direction of jurisdictional interaction: the larger the partial correlation, the stronger the strategic interaction. While the partial correlation is a correlation, the underlying econometric models it is derived from are deemed to be causal models by their authors. Hence, to the extent that estimates of inter-jurisdictional competition are causal (they measure the effect of the spending decisions of other jurisdictions), then the partial correlations can also be interpreted as a causal measure. A more conservative approach is to treat them as correlations without any causal inference.

While there are 369 reported estimates from 33 studies, in the empirical analysis we use 341 observations from 32 studies (see Table 1). Some observations are lost because (i) we are unable to match some estimates with data on capital controls, voter turnout and tax revenue

[10] Studies rarely report enough information from which these other effects can be calculated and included in a meta-analysis. Hence, by necessity, the MRA can only be conducted on the direct effect of expenditure competition between rival jurisdictions.

decentralization (see section 4 below) and (ii) we removed 7 observations which were clear outliers.[11]

The partial correlations for expenditure competition are illustrated in Figure 1, in the form of a funnel plot. The funnel plot illustrates the distribution of the partial correlations. Specifically, it traces the association between partial correlations and their associated precision, measured here as the inverse of the associated standard error (Stanley and Doucouliagos, 2010; Stanley and Doucouliagos, 2012). The funnel plot reveals two important pieces of information. First, it illustrates that the greater majority of estimated partial correlations are positive, indicating positive strategic interactions. There is, however, a fairly wide range of results reported in the literature. MRA can be used to explain this heterogeneity (see section 5 below). Second, it highlights the position of the central tendency of the results. The more precise estimates are closer together and tend to converge towards what might be considered to be the 'real' underlying effect. In our dataset, the weighted average of all partial correlation is +.07, suggesting a small positive degree of inter-jurisdiction expenditure interaction.

3.2 Data comparability

It is essential that the estimates included in the meta-dataset are comparable so that they can be included in the MRA. Our data consists of all estimates of public spending strategic interactions in published and unpublished studies. We are agnostic with regards to the literature, and prefer not to eliminate studies based on whether they take certain approaches: instead we control for methodological differences via meta-regression. We confirmed data comparability by running three tests. First, we tested whether partial correlations differed significantly between published and unpublished studies. Second, we

[11] We used standardized residuals to identify outliers. Removing these outliers is also justified statistically on the grounds that it improves the MRA model diagnostics.

tested whether study results differ according to the quality of the journal in which they were reported. We used the 2009 Social Science Citation Index Journal Impact Factors as proxies for study quality, assigning a zero weight to unpublished studies and to any journal that is not indexed in the SSCI. Third, we regressed the precision of the estimated partial correlations against the same Impact Factors. The results of these tests are reported in Table 2, columns 1, 2 and 3, respectively.[12] We found no significant difference in partial correlations between published and unpublished studies (column 1) and no difference on the basis of the quality of the journal as measured by journal Impact Factors (columns 2 and 3). In contrast, we show below that the partial correlations do vary as a result of measurement, data, specification, and estimator differences.

Our dataset purposefully includes estimates from different dimensions: estimates at different jurisdiction levels, in different countries, at different time periods, and for different types of expenditures. Expenditure on health, security, and infrastructure expenditures can be regarded as three different types of policy choices; internal redistributive, external threat, and internal developmental. Fiscal interactions might vary along these dimensions, *e.g.*, redistributive spending may result in race-to-the-bottom while public spending on development might yield efficient competitive processes and positive effects. The benefit of pooling these estimates together is that it enables us to formally test whether there are significant differences between fiscal interactions along these dimensions. The key advantage of MRA is that it can deal with such heterogeneity. MRA enables the identification of multiple dimensions of heterogeneity. That is, instead of *a priori* assuming that strategic interactions differ along these dimensions, we statistically test whether they do differ.

[12] For these estimates we include all observations except the outliers discussed above.

3.3 Publication selection bias

Publication selection bias occurs when authors and/or journals have a preference for statistically significant results or a preference for results consistent with a certain theory (Doucouliagos and Paldam, 2008; Stanley and Doucouliagos, 2012). In such cases, authors do not report all of the results they uncover. Rather, they select results that are consistent with their priors, or results which they believe have a stronger chance of being published. The effect of this process is that certain findings are suppressed while others are over-represented. Consequently, inferences drawn from an empirical literature maybe biased.[13] Typically, the bias is in favor of rejecting the null hypothesis of a zero effect. Hence, publication selection bias will tend to inflate the magnitude of an empirical effect.

Stanley (2005, 2008) advocates a simple, though powerful, test for publication bias – the funnel asymmetry precision effect size test (FAT-PET). This involves regressing the partial correlations against a constant and the partial correlation's standard error (SE_{ij}). The logic of this test is that estimates should not be correlated with their standard errors if a literature is free of publication selection bias (Egger *et al.*, 1997; Stanley 2005, 2008). If researchers search for estimates that are statistically significant, then they will re-estimate their models until the relationship between *r* and *SE* achieves some acceptable standard of statistical significance (*e.g.* a t-statistic of 1.96). This process will generate a correlation between the partial correlations with their standard errors (Stanley, 2008).[14] The FAT-PET results are reported in Table 2, column 4. We find little evidence of a statistically significant association between the partial correlations and their standard errors: the coefficient on *SE* is

[13] This bias affects all reviews of the evidence base, be they systematic reviews of the evidence, qualitative literature reviews, or formal statistically based assessments like MRA.

[14] Stanley and Doucouliagos (2012) argue that publication selection bias is analogous to sample selection biases and that it can be modeled as a Heckman-type regression: the MRA with a selection bias adjustment replaces the inverse Mills ratio term with the effect size's standard error.

weakly statistically significant (*p*-value = 0.078). Below we test for publication bias controlling for various other factors and find that *SE* is not statistically significant (see Table 3). This is a rather heartening finding given evidence reported elsewhere that there is a large degree of selection bias in economics (see the papers in Roberts and Stanley, 2005).

4. Regulation, political participation and decentralization

Heterogeneity in reported estimates may arise from genuine empirical differences in the underlying government reaction functions (*e.g.* differences in the expenditure function, countries and time period analyzed), or it can arise from differences in the specification of Eq. (1) (*e.g.* differences in the weights used, control variables and estimator). We use the MRA model in Eq. (2) to quantify both the effects of misspecification and genuine differences in strategic interactions.

One advantage of MRA is that it is able to explore associations that might not have been considered by the primary studies. We hypothesize that strategic interactions will be moderated by regulation, political participation, and the structure of federations.

4.1 Capital mobility

By impacting upon revenues, tax competition can drive and shape inter-jurisdictional expenditure spillovers. Tax driven strategic interactions require capital mobility: the more mobile capital is, the more likely that jurisdictions will engage in tax competition. For federations such as the EU, we posit that easing capital controls increases capital mobility, stimulating tax competition which in turn generates expenditure interactions. If this is the case, the partial correlation of expenditure choices between rival jurisdictions will be

positively related to the degree of regulation in capital controls.[15] A similar process is postulated for jurisdictions within single country federations (*e.g.*, the USA), particularly when jurisdictions have a balanced budget requirement. This results in a causal relationship between capital mobility and tax competition and ultimately expenditure competition.

We explore this association by including the variable *CapitalControl* in the MRA. If *CapitalControl* has a positive coefficient in the MRA, then this is consistent with the notion that expenditure interactions are driven by tax competition. We use data from the Fraser Institute on International Capital Market Controls.[16] This series is available only at the national level. Nevertheless, the national data should serve as a reasonably good proxy for capital controls at the sub-national government level, as capital controls are often imposed at the national level but their effect is felt throughout the economy and state and federal regulations often move together.[17]

A positive relationship between the partial correlation of expenditure choices between rival jurisdictions and the degree of capital control regulations can also emerge when countries loosen capital controls and also devolve authority and financial resources to lower level jurisdictions. In this scenario, jurisdictions are given more to spend from the Federal government and they have greater choice in regulations and taxes they can impose, and public expenditures they can make. Consequently, in our MRA we control for the degree of

[15] Some authors have found that that capital mobility might have no effect on tax rates and that it might even increase them (Lockwood and Makris, 2006 and Lai 2010 and references therein). Our hypothesis is that capital controls will shape the *magnitude* of fiscal interactions. The resulting impact on the direction of tax rates – driving tax rates down or pushing them up – is an entirely different issue.

[16] This is the series 4E, International Capital Market Controls. The maximum value of the series is 10, which denotes the most liberal regime, free of all international capital market controls.

[17] For example, analysis of US sub-national labor market regulation data for the 1981 to 2010 period (Bueno, Ashby and McMahon, 2012) shows that labor market regulations are highly correlated in the majority of regions, with most correlations exceeding 0.70.

tax revenue decentralization and we also control for whether the primary estimates controlled for grants received from the Federal government.

4.2 Political participation

Our second constructed variable relates to political participation.[18] According to yardstick competition theory, voters make inter-jurisdictional comparisons as an attempt to overcome agency problems (Besley and Case, 1995; Wilson and Gordon, 2003). If this process holds, then there should be a positive correlation between inter-jurisdiction interactions and the degree of political competition. As a measure of this process, we considered the Polity series on political competition (Polity2). However, this series displayed no variation for the countries included in our dataset. Instead, we use data on voter turnout, *VoterTurnout*. This is the total number of votes cast (both valid and invalid) divided by the number of names on the voters' register. We constructed this series using data from the International Institute for Democracy and Electoral Assistance and from various national and sub-national electoral bodies. We matched as closely as possible the level at which expenditure competition is occurring with voter turnout. Thus, for competition between nations we used voter turnout in national elections, while for competition at the municipality level we used voter turnout at municipal elections. We were able to do this for 86% of the observations. Where it was not possible to match the level of disaggregation we used the next level of voter turnout, *e.g.*, we use turnout at the state level as a proxy for municipal elections and national elections as a proxy for state level turnout.

The median voter might very well be different at low levels of voter turnout than at higher levels. Arguably, the median voter at low levels of voter turnout will have a higher

[18] Political participation is a different phenomenon to political competition. We would prefer to have data on political competition, such as the electoral margin of the top two candidates in each set of elections, but this information is unavailable for many of the samples used in our meta-study.

socio-economic background and is more likely to be industry friendly. In contrast, the median voter at higher levels of voter turnout might have a lower socio-economic background, with a stronger preference to increase public expenditure and potentially be less industry friendly. If this is the case, then all else equal, voter turnout will be positively correlated with expenditure competition: there will then be a positive correlation between voter turnout and the partial correlation between the expenditure choices of rival jurisdictions.

4.3 Fiscal decentralization

A third variable is *Fiscal decentralization.* Fiscal interactions are driven by decision makers with autonomy over benefits and/or taxes. Hence, we expect that fiscal interactions will be shaped by the degree of decentralization. However, it is unclear whether fiscal decentralization has a positive or negative effect on the partial correlation of expenditure choices between rival jurisdictions. Some authors such as Keen and Marchand (1997) predict that fiscal interactions will result in excessive public spending in productive investments and insufficient spending on public consumption goods. Others, however, argue that there are factors operating that will lead to insufficient investment. The net effect on public spending on investment is thus unclear, as is the net effect on total investment. Tax decentralization is an important driver behind these spending biases.[19] In order to explore this effect we use the series on tax revenue decentralization constructed by Stegarescu (2005).[20] A positive (negative) coefficient in the MRA indicates that fiscal decentralization increases (decreases) fiscal interactions.

[19] Kappeler, Solé-Ollé, Stephan and Välilä (2013) point out that sub-national jurisdictions are responsible for most public infrastructure. Kappeler and Välilä (2008) and Kappeler, Solé-Ollé, Stephan and Välilä (2013) find that decentralization increases spending on infrastructure and public goods but not on redistributive outlays.

[20] Higher values indicate a greater degree of decentralization. Other series are available, such as IMF's Government Finance Statistics and the Fiscal Empowerment series by Boex and Simatupang (2008). However, these data series tend to overstate the degree of decentralization (Stegarescu, 2005).

Descriptive statistics for these three variables are reported in Table 1, columns 2, 3, and 4. Capital controls, voter turnout and federal structure vary over time and between countries. We take advantage of this variation to explore the effects of regulation, political participation and federal structure on strategic interactions. As noted above, while various predictions are possible, our own expectations are that tax competition should increase as capital controls ease, while yardstick competition should increase as voter turnout rises. If the effects of capital controls on strategic interactions are greater (lower) than the effects of voter turnout, then we can conclude that tax competition (yardstick competition) is more prominent.

5. MRA Results

Various versions of the MRA model, Eq.(2), were estimated and the key results are presented in Table 3. We restricted the MRA to 27 variables that capture the main differences in the data, specification and estimation of Eq. (1).[21] Column 1 lists the moderator variables and their means and standard deviations. The first of these is *Standard error* which is included to test and correct for publication selection bias (Stanley, 2008). This offers a multiple regression version of the FAT-PET reported in Table 2, Column 4. Next we include the three variables (discussed in section 4 above) constructed using data collected from sources external to the primary studies; *CapitalControl*, *VoterTurnout*, and *FiscalDecentral* (capital market controls, voter turnout, and the degree of federalism, respectively).

Seven variables are included to capture data differences. Differences in the measurement of the dependent variable are reflected in three binary variables, *Health*, *Security*, and *Infrastructure*, which take the value of 1 if the measure of competition is based on spending on health, security, or infrastructure, respectively. The base for this model is

[21] As part of robustness checks, we also expanded the MRA model to include other variables in the MRA, but these were not statistically significant in explaining observed heterogeneity; see Table 5 below.

total expenditure and all other types of expenditure. *State* and *Nation* are binary variables that allow us to test how the level of government affects the magnitude of expenditure competition, with municipality as the base. *Panel* is a binary variable taking the value of 1 if panel data are used, with cross-sectional data as the base.[22] *AverageYear* is the average year of the sample used. This variable is included to test whether the degree of expenditure competition varies over time. This variable is normalized at the mean of the sample, 1991. A negative (positive) sign on this variable would indicate that fiscal spending interactions are becoming weaker (stronger) over time. It is also possible that this variable might be picking up improvements in the quality of estimates over time or better quality data over time. Note that capital controls have loosened in the EU over time. Given the discussion above this should then result in time trend in data (at least for estimates relating to this region). However, we condition the MRA for changes in capital controls. Hence, the *AverageYear* variable is picking up factors unrelated to capital controls.

Five variables relate to estimation differences. *Time effects* is a binary variable for those studies that use panel data and control for fixed time period effects.[23] The existence of strategic interactions means that expenditure policies are endogenous and determined jointly by competing policy makers. The variables *IV* and *ML* are binary variables that capture any difference in estimates that address this endogeneity by instrumenting spending competition and the use of maximum likelihood estimation, respectively. *OtherNonOLS* is a binary variable for studies that use other estimators. Most studies use contiguity or distance to assign weights to rival jurisdictions: the further away a jurisdiction is, the less weight it is assigned. *NoWeight* is a binary variable taking the value of 1 for studies that take a simple average of other regions.

[22] Essentially these variables capture some of the differences between partial and general equilibrium effects.
[23] In unreported regressions we also considered fixed jurisdiction effects. This variable was never statistically significant in the MRA.

The variables *Sweden*, *UK* and *AllOthers* capture any national differences, with the USA as the base. Since the MRA also includes *CapitalControl*, *VoterTurnout* and *FiscalDecentral*, the three country dummies are picking up any remaining unobservable differences between countries that might affect estimates of government expenditure strategic interactions.

Model specification differences are reflected in the seven binary variables that capture the effect of including specific controls in the primary studies. *Grants*, *Income*, *Population*, *Unemployment*, *Politics*, *Neighborlag*, and *Neighborchar* are all binary variables that control whether the primary study includes grants, income, population, unemployment, the politics of the ruling party as control variables, the use of a lagged measure of the neighbor's benefit instead of a contemporaneous value,[24]and the characteristics of neighboring jurisdictions, rather than just controlling of the characteristics of the own jurisdiction, respectively.

Column 2 presents estimates of the general MRA with all 27 variables when OLS is used, with standard errors adjusted for the clustering of observations within studies. These 27 variables quantify the main differences between studies in the measures, data, specification, and estimation. Column 3 presents the same model estimated using WLS, using 'optimal weights', *i.e.*, each estimate is weighted by its inverse variance. This assigns greater weight to estimates that are reported with greater precision. Column 4 presents the results attained through a general-to-specific modeling strategy, sequentially removing any variable that was not statistically significant at least at the 10% level. The reason for estimating this model is that MRA variables are often highly collinear and the general-to-specific model reveals the underlying associations with greater clarity (see Stanley and Doucouliagos, 2012).[25] Most of the observations included in our dataset relate to sub-national fiscal interactions, while 9%

[24] This is often adopted to get around the endogeneity between the own and neighboring jurisdiction's expenditures.

[25] A Wald test was conducted to confirm that the omitted variables were redundant: *p*-value is 0.15.

relate to national fiscal interactions. In column 5 we present the results of just sub-national fiscal interactions. Columns 2 to 5 use a WLS fixed effects MRA. This is also known as unrestricted WLS (Stanley and Doucouliagos, 2013, 2015). In contrast, column 6 reports the results from a random effects MRA. Our preferred estimates are presented in columns 3 and 4. The OLS and random effects results are presented for the sake of robustness. The main problem with OLS is that it ignores the heteroscedasticity in meta-analysis data. The precision of estimates of strategic interactions varies across studies and this needs to be accommodated via weighted least squares. Column 6 uses WLS but with random effects weights. Stanley and Doucouliagos (2012) caution against the use of this estimator in the case of observational data as the random effects may not be independent of the underlying heterogeneity in the meta-data, resulting in biased estimates. Monte Carlo simulations indicate that unrestricted WLS is preferable to random effects (Stanley and Doucouliagos, 2013, 2015).

The MRA models reported in Table 3 explain over 40% of the variation in partial correlations. This is actually a fairly large proportion of the variation, given that it highly likely that there will be much random variation in estimates of strategic interactions. As already noted, the preferred models are reported in columns 3 and 4. Both of these models pass various diagnostic tests.[26] The constant in these MRA models measures the degree of expenditure competition (as measured by partial correlations) for studies using US data on total expenditures at the municipality level, using cross-sectional data for 1991, estimated by OLS, without any of the controls listed in the table, using distance to weigh neighbors' spending *and* setting the three institutional data variables to zero. The MRA variables are then interpreted relative to this constant. A statistically significant negative (positive)

[26] For example, the MRA model reported in Column 4 passes the RESET test (p-value = 0.60) and the linktest (p-value = 0.27).

coefficient in the MRA indicates that the variable reduces (increases) the size of the fiscal interaction.

Several robust results emerge from Table 3. First, *Standard error* is not statistically significant when unrestricted WLS is used. This is broadly in line with the findings from Table 2 (column 4) where there was only weak statistical evidence of publication selection in this literature. There is evidence of publication bias when OLS and the random effects estimator is used, but as noted above we do not put much credence to the results from these models.[27]

Second, we find that relaxing capital controls (higher values of *CapitalControl*) increases interdependence between jurisdictions. This finding can be explained by the flow on effect of tax competition on expenditure competition. Competing for mobile capital through the relaxation of capital controls effectively results in linking expenditure decisions between jurisdictions. Keeping the tax base and federal grants constant, a change in the tax rate alters revenues and hence expenditure. This flows on to the expenditure decisions of rival jurisdictions.

Decentralization (higher values of *FiscalDecentral*), on the other hand, has the opposite effect of relaxing capital controls: decentralization reduces welfare and spending competition. This means that when jurisdictions have greater tax revenue autonomy they are less likely to engage in strategic spending interactions with rival jurisdictions. In other words, the less autonomy they have in raising their own revenue, the more likely they are to use spending as a policy instrument. Hence, a negative coefficient in the MRA is consistent with the view that fiscal decentralization is more likely to result in increased tax competition than it is in expenditure yardstick competition. When taken together, the results from *CapitalControl* and

[27] We also interacted *SE* with the journal Impact Factors. Adding this variable to the general-to-specific model (Column 4 of Table 3) produces a positive coefficient, 0.47, suggesting that higher ranked journals tend to report estimates that are more likely to arise from publication selection biased. However, this interaction variable is not statistically significant (*t*-statistic = 1.67, *p*-value = 0.105).

FiscalDecentral suggest that the effects of relaxing capital controls are weaker when fiscal decentralization is greater.

Voter turnout appears to have no effect on expenditure competition.[28] We interpret this to mean that political participation does not moderate strategic spending interactions. This suggests that yardstick competition effects may affect the magnitude of public spending interactions, at least in the countries covered by our data. Hence, we conclude from the MRA results that expenditure interactions appear to be driven by tax competition rather than yardstick competition.

[28] One possible explanation for this is the ability of jurisdictions to manipulate who is actually eligible to vote. This can also arise when jurisdictions are very heterogeneous with respect to the median voter.

A third finding is that data make a significant difference. The negative coefficient on *Security* indicates that inter-jurisdictional expenditure competition is smaller with respect to security. This is consistent with the notion that jurisdictions might free ride on spending decisions made by others. The level of the jurisdiction is also important. The *State* and *Nation* variables both have negative coefficients in the MRA. The use of state or nation level data results in smaller expenditure competition effects, compared to the base (municipality data). That is, strategic interactions appear to be weaker amongst rival states and nations than they are at lower level jurisdictions. *Panel* has a positive coefficient indicating that studies that use panel data report stronger strategic interactions than studies that use cross-sectional data. *AverageYear* has a negative coefficient indicating that expenditure competition is time variant and has been declining over time. The country dummy variables are not statistically significant in the preferred WLS models. This means that there are no country differences in fiscal interactions conditional on capital controls and fiscal decentralization differences.

Our fourth finding is that, surprisingly, the estimation variables are not important in explaining the variation in the reported results. The one possible exception is that the inclusion of time fixed effects results in smaller strategic interactions. The use of an IV or a maximum likelihood estimator does not appear to make a noticeable difference to reported results once other differences in data and research designed are controlled for. Not using any weights at all (treating all jurisdictions equally) also makes no difference to the reported estimates of expenditure competition, once other dimensions of research design are taken into account.

Fifth, econometric specification also matters. The inclusion of grants, the politics of the party in office, and population in an econometric model all result in larger expenditure

interactions. In contrast, unemployment, the neighbor's spending levels lagged and characteristics of neighboring jurisdictions, all result in smaller effects.

The MRA coefficients can be used to estimate the degree of expenditure interactions arising from inter-jurisdictional competition. We report such estimates in Table 4, for the US and for all countries combined, for total spending and spending on security, and at the municipal and state jurisdiction levels. In forming such estimates we use country specific data on capital controls and fiscal decentralization. We assume that the inter-jurisdiction competition occurs at either the municipality or the county level and that a well-constructed model will include controls for grants, the politics of the party, population, and unemployment.[29] The average of all countries combined (Column 2 of the first row of Table 4) shows a positive partial correlation between rival jurisdictions spending. This suggests the existence of a *negative* spending externality. A positive partial correlation means that the ith jurisdiction will reduce its spending when other jurisdictions decrease theirs. That is, the rival jurisdictions decisions have an adverse effect on the ith jurisdiction. However, it also means that when other jurisdictions increase their spending, it becomes easier for the ith jurisdiction to increase spending. The point estimates of interactions for the USA (the second row of Table 4) are slightly lower than for all countries combined. However, the 95% confidence intervals overlap significantly so that there is no practical difference in the size of the interactions. Table 4 also confirms that interactions at the municipal/county level are significantly larger than those at the state/provincial level, with little overlap in the confidence intervals. In all cases, there is no evidence of fiscal interactions with regarding to spending on security: the point estimates vary from positive to negative but the confidence intervals always include zero.

[29] We also assume that panel data are used and that model is estimated using either IV, or maximum likelihood or OLS with a lag value in the rival jurisdictions spending, with time effects included.

Cohen (1988) provides well-known guidelines for the practical significance of a correlation: 0.1 for a small effect, 0.3 is medium, and anything larger than 0.5 is a large effect. Doucouliagos (2011) provides similar guidelines for the practical significance of a partial correlation: 0.07 is a small effect, 0.17 is a moderate effect and 0.33 is a large effect. Hence, we can conclude that fiscal spending interactions at the municipality jurisdiction level are moderate in size and of some practical importance.

Robustness

Table 5 reports the results of various robustness checks on the general-to-specific MRA model reported in Column 4 of Table 3, which is reproduced in row 1 for comparison. Note, however, that we add back in *VoterTurnout* to this specific model. Table 5 reports the coefficients on the *CapitalControl*, *VoterTurnout*, and *FiscalDecentral* variables. First, instead of adjusting standard errors for data clustering, we re-estimated the MRA model using a multilevel mixed effects model estimated using REML. The key difference is that voter turnout now emerges with a statistically significant negative coefficient, suggesting that greater political participation reduces strategic interactions between jurisdictions. Row 3 reports the results of re-estimating the MRA without adjusting standard errors for data clustering. Row 4 reports the results using Robust regression, which is less sensitive to the effects of outliers (recall however that we have already removed several outliers from the data). The MRA was also re-estimated with the addition of more control variables (see the notes to Table 5). These results are reported in row 5. Finally, row 6 reports the results of adding study fixed effects to the MRA. This model focuses on the within study differences in reported results and is not comparable to the other models. With the exception of row 6, the results are robust to these alternative approaches.

6. Conclusion

The design of federal and decentralized jurisdictions is said to improve government efficiency when jurisdictions interact. Interactions can arise from tax competition to attract tax revenues, and through political competition to maximize political support, by setting public expenditure to the preferences of voters among competing constituencies. This paper applies meta-regression analysis to investigate the magnitude of strategic expenditure interactions between jurisdictions, namely the extent to which expenditure in one jurisdiction is influenced by rival jurisdictions' actions. MRA is particularly helpful in revealing the causes of inter-jurisdictional interactions when the extant evidence is diverse and conflicting because of study specific heterogeneity and institutional differences. We draw several robust conclusions from the analysis.

The evidence points strongly to the existence of inter-jurisdictional expenditure interdependence among lower level jurisdictions (municipalities and counties). However, compared to the municipality level, horizontal expenditure competition is weaker when the jurisdiction is a State. That is, the strategic spending interactions are more pronounced at the municipal and county levels, and less so at the State level. However, there is some heterogeneity in our results, as we find an absence of interactions with respect to spending on security. More importantly for the design of institutions, we find that expenditure interactions are becoming weaker over time. This might be driven by rising political costs of inter-jurisdictional rivalry, as per Basinger and Hallerberg (2004). This finding is also consistent with expressive voting (Brennan and Lomasky, 1993). Specifically, if voting behavior is becoming increasingly driven by expressive considerations, then over time this will weaken the correlation between the spending decisions of rival jurisdictions. Alternatively, this could reflect higher quality and more accurate estimates arising from the availability of better datasets over time. However, our reading of the literature is that the

quality of data used has not materially changed over time.[30] We also find that much of the observed variation in strategic interactions can be explained by differences in the specification of the fiscal interactions econometrics model.

Analysis of the effects of regulation, political participation and decentralization indicates that capital controls and fiscal decentralization play an instrumental role in shaping expenditure interactions. Relaxing capital controls strengthens strategic expenditure interactions whereas granting jurisdictions greater tax revenue autonomy decreases strategic interactions. Capital controls, however, appear to be more important. Evaluated at sample means, we find that the elasticity of strategic interactions with respect to capital controls is 3.97, while the elasticity of strategic interactions with respect to decentralization is -1.36. Hence, a one percentage reduction in capital controls has nearly three times the effect on strategic interactions as does an equivalent percentage change in decentralization. This is consistent with the view that expenditure interactions are indeed induced by tax competition between rival jurisdictions. While some of the robustness results suggest otherwise (recall Table 5), our main results (Table 3) indicate that political participation is not an important factor. This suggests that yardstick competition driven by political incentives alone is insufficient to drive expenditure interactions, at least in the countries and time periods included in our study and when yardstick competition is proxied by voter turnout. This is an important finding suggesting that regional political interactions are not as important to the magnitude of strategic interactions and race to the bottom trends as some studies have suggested. Potential explanations include the imperfect nature of the electoral mechanisms in place to reward and penalize incumbents that overspend and the limited transparency of political markets for the median voter to be able to fully judge the relative efficiency of one government to another.

[30] Another possibility is that the declining effect might be a statistical artefact caused by the early literature publishing exaggerated results, with the subsequent studies 'correcting' the evidence base.

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Appendix A: List of Studies Included in the MRA
(NOT FOR PUBLICATION)

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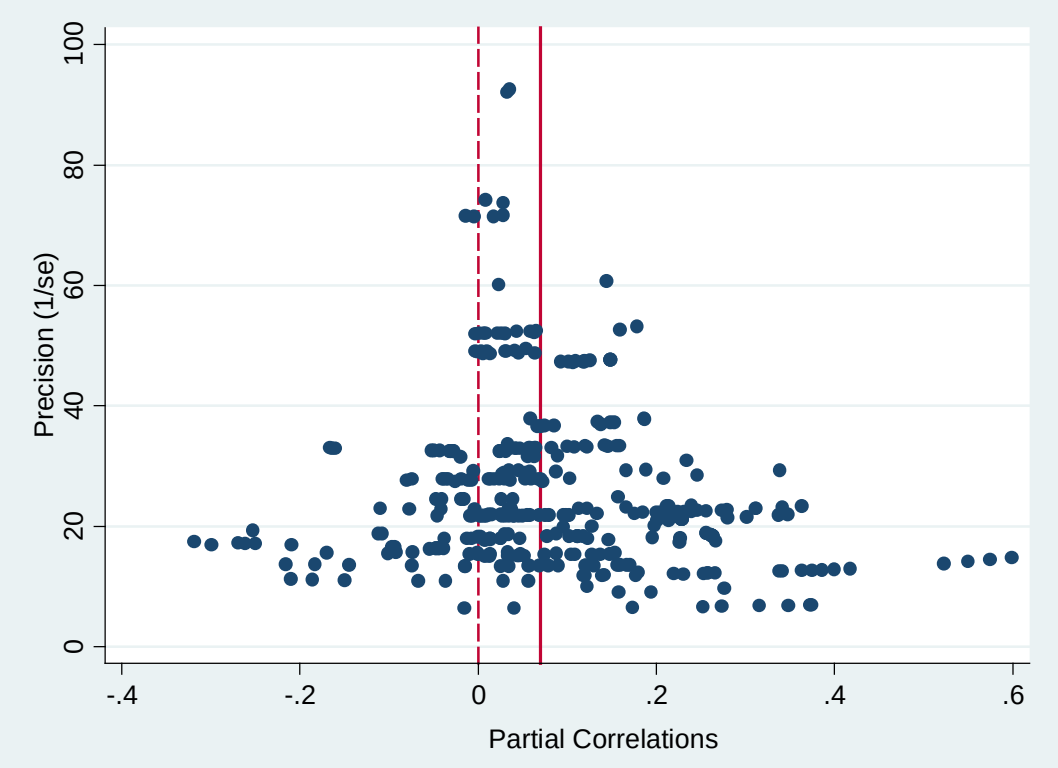
Figures and Tables

Figure 1: Funnel Plot for Estimates of Horizontal Inter-Jurisdiction Expenditure Interactions



Notes: The solid line denotes the position of weighted average partial correlation (+.07). The dash line denotes the position of a zero partial correlation. The vertical axis measures precision calculated as the inverse of the standard error of the partial correlation.

Table 1: Country Composition of Estimates of Inter-Jurisdictional Expenditure Interactions

Country	Number of studies [estimates] (1)	Tax revenue decentralization (2)	Capital controls (3)	Voter turnout (4)
USA	15 [156]	38.10 (6.75)	8.12 (0.75)	54.04 (8.19)
Sweden	6 [68]	44.07 (1.05)	7.91 (1.71)	81.58 (3.26)
UK	5 [30]	6.33 (3.06)	9.17 (0.45)	44.37 (3.35)
Switzerland	2 [21]	55.67 (0.56)	9.53 (0.14)	44.70 (0.84)
All studies	341 [32]	32.08 (15.02)	7.92 (1.22)	62.38 (14.87)

Note: Cells in columns 2 to 4 report averages with standard deviations in brackets. Section 4 discusses these variables.

Table 2: Meta-data Comparability and Publication Selection Bias Tests

	(1)	(2)	(3)	FAT-PET (4)
Constant	.058 (2.39)	.056 (2.72)	29.339 (6.24)	.026 (1.09)
Published	.011 (0.37)	-	-	-
Impact factor	-	.009 (0.65)	-3.164 (-1.46)	-
Standard error	-	-	-	1.245 (1.82)
Adjusted R^2	.001	.005	.063	.042

Notes: The number of observations is 355 estimates from 33 studies. The dependent variable in columns 1, 2 and 4 is the partial correlation. The dependent variable in column 3 is the precision of the partial correlation. Columns 1, 2 and 4 are estimated using unrestricted WLS, with inverse variance weights. Column 3 is estimated using OLS. Brackets report t-statistics using standard errors corrected for the clustering of observations within studies. Column 4 reports results of the FAT-PET model. Published is a binary variable with a value of 1 for studies that are published and 0 otherwise. Impact factor is the SSCI journal Impact Factor.

Table 3: Meta-Regression Analysis of Inter-Jurisdictional Expenditure Competition

(Dependent variable is partial correlations)

Variable	Mean (S.D.) (1)	OLS (2)	Unrestricted WLS (3)	General to Specific WLS (4)	Without nation estimates WLS (5)	Random effects WLS (6)
Constant		-.23 (1.22)	-.29 (1.61)	**-.21 (1.94)**	**-.21 (1.92)**	-.28 (1.50)
Standard error	.05 (.03)	**1.41 (1.81)**	1.35 (1.41)	-	-	**1.60 (3.23)**
Regulation, political participation and decentralization						
CapitalControl	7.98 (1.15)	**.08 (4.28)**	**.06 (2.34)**	**.04 (5.00)**	**.04 (4.95)**	**.07 (6.46)**
VoterTurnout	62.30(14.50)	-.01 (0.78)	.01 (0.57)	-	-	-.01 (0.22)
FiscalDecentral	30.80(15.75)	**-.01 (2.07)**	-.01 (1.47)	**-.01 (3.68)**	**-.01 (3.70)**	**-.01 (3.29)**
Data differences						
Health	.09 (.28)	-.01 (0.35)	.02 (1.34)	-	-	-.01 (0.03)
Security	.06 (.25)	**-.20 (4.34)**	**-.16 (4.48)**	**-.16 (3.94)**	**-.16 (3.70)**	**-.19 (6.40)**
Infrastructure	.08 (.27)	-.01 (0.30)	.01 (0.42)	-	-	.01 (0.03)
State	.40 (.49)	**-.14 (2.90)**	**-.15 (2.80)**	**-.10 (3.49)**	**-.10 (3.41)**	**-.15 (4.23)**
Nation	.09 (.29)	**-.25 (1.87)**	**-.40 (3.08)**	**-.19 (3.68)**	-	**-.30 (3.08)**
Panel	.75 (.43)	**.17 (1.90)**	.14 (1.54)	**.14 (2.05)**	**.14 (2.06)**	**.16 (5.16)**
AverageYear	2.67 (6.94)	-.01 (1.36)	**-.01 (2.01)**	**-.01 (2.78)**	**-.01 (2.71)**	**-.01 (2.31)**
Estimator differences						
Time effects	.56 (.50)	-.07 (1.56)	**-.06 (2.59)**	-.06 (1.65)	-.06 (1.66)	**-.06 (2.54)**
IV	.38 (.49)	.01 (0.54)	**-.03 (1.85)**	-	-	.01 (0.23**)**
ML	.26 (.44)	.02 (0.48)	-.02 (0.55)	-	-	.01 (0.25)
OtherNonOLS	.07 (.26)	-.03 (1.09)	**-.05 (1.79)**	-	-	-.03 (1.05)
NoWeight	.10 (.30)	-.04 (1.27)	-.04 (1.21)	-	-	-.02 (0.86)
Country differences						
Sweden	.18 (.38)	-.05 (0.41)	-.13 (0.90)	-	-	-.09 (0.84)
UK	.08 (.28)	**-.35 (2.07)**	-.21 (0.89)	-	-	**-.31 (2.87)**

AllOthers	.17 (.38)	-.21 (1.43)	-.18 (1.10)	-	-	**-.21 (2.20)**
			Specification differences			
Grants	.59 (.49)	.13 (3.63)	.06 (1.69)	**.07 (3.09)**	**.07 (3.06)**	**.12 (4.58)**
Income	.68 (.47)	-.04 (0.91)	-.05 (1.18)	-	-	**-.05 (1.77)**
Neighborlag	.12 (.32)	-.01 (0.25)	**-.03 (2.17)**	**-.02 (1.79)**	**-.02 (1.82)**	-.01 (0.59)
Neighborchar	.15 (.36)	**-.05 (1.86)**	-.04 (1.65)	**-.05 (1.95)**	**-.05 (1.95)**	**-.04 (1.69)**
Politics	.49 (.50)	**.15 (5.15)**	**.13 (3.88)**	**.09 (5.80)**	**.10 (5.54)**	**.15 (7.55)**
Population	.79 (.41)	.05 (1.32)	**.07 (2.65)**	**.06 (1.96)**	**.06 (1.97)**	**.05 (1.91)**
Unemployment	.54 (.50)	**-.10 (2.61)**	**-.11 (3.02)**	**-.09 (3.65)**	**-.09 (3.66)**	**-.10 (4.51)**
F-test		327.32 [0.00]	529.04 [0.00]	38.59 [0.00]	39.66 [0.00]	11.30 [0.00]
Adjusted R^2		.47	.42	.42	.43	.52

Notes: The number of observations is 341 estimates from 32 studies for columns 2 to 4 and 6, and 309 estimates from 30 studies in column 5. Cell entries in bold denote statistical significance at least at the 10% level. Cell entries in brackets in columns 2 to 5 report absolute values of t-statistics derived using standard errors adjusted for data clustering. Column 2 uses OLS, while columns 3 to 5 use unrestricted weighted least squares using inverse variance weights. Column 6 reports results of a random effects MRA estimated using Restricted Maximum Likelihood.

Table 4: Summary of MRA Estimates of Spatial Interactions, USA and All Countries combined

	Municipality/County jurisdiction		**State jurisdiction**	
	Security spending (1)	Other spending (2)	Security spending (3)	Other spending (4)
All Countries	.04 (-.07 to .14)	.20 (.13 to .27)	-.06 (-.17 to .04)	.09 (.03 to .16)
USA	.02 (-.09 to .12)	.17 (.10 to .25)	-.09 (-.19 to .02)	.07 (.01 to .14)

Note: MRA predictions using unrestricted WLS regression coefficients reported in column 4 of Table 3. 95% confidence intervals reported in brackets.

Table 5: Robustness of MRA

	Capital controls	Voter turnout	Decentralization
Main results (1)	**.04 (5.00)**	-.01 (-0.27)	**-.01 (-3.68)**
Multilevel (2)	**.05 (5.94)**	**-.01 (-2.07)**	**-.01 (-2.72)**
Without clustering (3)	**.04 (6.08)**	-.01 (-0.39)	**-.01 (-5.73)**
Robust regression (4)	**.05 (5.42)**	**-.01 (-1.23)**	**-.01 (-3.12)**
With additional controls (5)	**.04 (3.17)**	-.01 (-0.55)	**-.01 (-1.75)**
Fixed effects (6)	.05 (1.08)	.01 (0.14)	**-.03 (-4.19)**

Notes: The first row reports the results of the model reported in Table 3, Column 4, with the addition of the *VoterTurnout* variable. The second row uses a multilevel mixed effects model estimated using restricted maximum likelihood. The third row uses WLS but does not adjust standard errors for data clustering. The fourth row uses Robust regression. The fifth row adds additional controls variables: welfare spending, education spending, a dummy for jurisdiction fixed effects, GMM estimator, non-contiguity weights, Switzerland, and vertical expenditure competition. The sixth row adds study fixed effects.