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Summary

Recent studies suggest a considerable amount of horizontal strategic interaction amongst governments exists. The empirical approach in these studies typically relies on estimating reaction functions in a uni-dimensional policy framework, where a nonzero slope estimate suggests strategic interactions exist. While this framework may be useful within certain contexts, it is *potentially* too restrictive; for example, in models of resource competition, locales may use multiple instruments to attract agents, leading to strategic interaction across policy instruments. In this study, we develop a theoretic construct that includes yardstick competition in a world of multi-dimensional policies to show that while a zero-sloped reaction function may exist for any particular policy, this does not necessarily imply the absence of strategic interactions. We empirically examine the implications of the model using US state-level panel data over the period 1977-1994. Empirical results suggest important cross-policy strategic interactions exist, lending support in favor of the multi-dimensional framework.

Keywords: Political economy, resource competition, strategic policymaking, yardstick competition

JEL: C33, H7, R1, R3, Q28

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1. Introduction

Determining the optimal institutional arrangements to carry out tasks of allocation, distribution, and stabilization remains of great policy relevance. Perhaps adding a sense of urgency to the matter is the mounting budgetary difficulties of federal and state governments in the United States and of decentralized governments in a number of countries (Oates, 1991). While research over the past several decades has provided a considerably better understanding of the various processes at work, a recent important line of inquiry examining the horizontal interaction of public policies has garnered much attention.[1] The general intuition underlying the theoretical constructs of the strategic interaction models is straightforward: given that local economies are spatially linked, under certain realistic assumptions governments may interact strategically when setting policies. Although the various theoretical models and the accompanying empirical literature at times are motivated quite differently, the resulting empirical goals within the literature are composed quite similarly: estimate reaction functions in a uni-dimensional policy framework and test whether the slope estimate is zero. A finding of a nonzero (zero) slope estimate is conjectured to be evidence that strategic interactions exist (do not exist) (see, e.g., Case et al., 1993; Besley and Case, 1995a; Murdoch et al., 1997; Brueckner, 1998; Heyndels and Vuchelen, 1998; Fredriksson and Millimet, 2001a; 2001b; Revelli, 2001; Brueckner and Saavedra, 2001).[2]

While this particular framework is a useful representation within certain contexts, the possibility of reaching false inferences may not be trivial. For example, consider the case of local competition for a new plant, where extravagant baskets of incentives are not unusual in the world of smokestack chasing: in the 1993 Mercedes sports utility vehicle plant bidding war, Alabama out-dueled 34 other states with an incentive package that totaled \$300 million, of which infrastructure development, job training, tax concessions, and other perks were included. Similar deals were struck in Tennessee, where the state offered an incentive package for a Nissan automobile manufacturing plant that totaled approximately \$11,000 per created job; five years

[1] For thoughtful reviews see Wilson (1996) and Brueckner (2001). Brueckner (2001) splices the studies into two groups: i) spillover models, which includes yardstick competition models and ii) resource flow models. Our study represents a hybrid approach.

[2] The present paper is related to the literature on welfare benefit competition (e.g., Figlio et al., 1999; Brueckner, 2000a; and Saavedra, 2000), to the theoretical literature on tax competition (e.g., Zodrow and Mieszkowski, 1986; Wilson, 1986; 1987; Wildasin, 1988; Bucovetsky and Wilson, 1991; Edwards and Keen, 1996; and Brueckner 2000b), and to the theoretical literature on capital competition using environmental policy (e.g., Oates and Schwab, 1988; Markusen et al., 1995; and Ulph, 2000).

later in 1987 Tennessee offered Saturn a package more than double Nissan's package in terms of dollars per created job: $26,000 per job. Both Nissan and Saturn gladly accepted the offers and chose the Volunteer state as their new home.[3]

These anecdotes highlight the fact that competition may occur across *several* policy dimensions. As such, concluding from a zero-sloped reaction function for any specific policy that strategic policymaking is absent risks a Type II error. For example, whereas California may not have the wherewithal to concede certain environmental requests, it may counteract environmental concessions in Nevada via tax breaks or promises of expanded infrastructure.

In this study, we revisit the issue of horizontal strategic policymaking by developing a simple theoretical model that includes yardstick competition and multi-dimensional policies. Motivated by the concerns of voters over environmental quality and the attraction of mobile capital, states may act strategically when determining three interrelated policies: (i) state-level tax rates, (ii) infrastructure spending, and (iii) pollution control standards.[4] Via this simple extension, we are able to provide a much richer model of strategic policymaking, as we are able to investigate intra- and inter-policy strategic reaction functions.

We empirically test the major implications of the model by making use of US state-level panel data over the period 1977-1994. The empirical results suggest that important own- and cross-policy interactions do exist. For example, states respond to higher levels of governmental expenditure levels in neighboring states by lowering their own pollution standards. Furthermore, within policy types, we find positively sloped tax and expenditure reaction functions, consistent with previous efforts (e.g., Besley and Case, 1995a; Brueckner and Saavedra, 2001).

In addition to estimating the slopes of the various reaction functions, we also empirically test the underlying assumptions of the theoretical model. Important results from auxillary regression models suggest that both capital competition and yardstick competition models have a degree of predictive power: capital location decisions are influenced by tax and pollution policies (as in Keller and Levinson, 2001; Fredriksson et al., 2001), and neighboring tax rates affect gubernatorial voting patterns, consonant with Besley and Case (1995a).

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[3] This story along with several others can be found at: http://www.geocities.com/capitolhill/2817/govern.htm

[4] For discussions of the effect of public spending, see Duffy-Deno and Eberts (1991), Carlino and Voith (1992), Garcia-Milaand and McGuire (1992), Morrison and Schwartz (1996), Dalenberg and Partridge (1997), and Chandra and Thompson (2000).

The remainder of our paper proceeds as follows. Section 2 briefly describes the underlying theoretical construct. Section 3 presents the empirical model and our data. Section 4 contains the empirical results and Section 5 concludes.

2. The Model

2.1 Setup

In our theoretical structure we seek to develop a simple model combining yardstick competition with multi-dimensional strategic interaction between *n* states (indexed by *i*, *i*=1,…,*n*). Within the first strand of literature, it is important to recognize that at the crux of Besley and Case's (1995a) model is an information-theoretic framework: by making comparisons with neighboring states, voters determine whether state politicians are efficient or whether they are engaged in significant rent-seeking activities. In this study, rather than extend their sophisticated set-up, we make use of Brueckner's (2001) simplified approach to yardstick competition. Moreover, we extend his model by incorporating mobile capital, thereby adding a "resource-flow" aspect as well.

We assume that voters' preferences are given by

$$U(c_i, Q_i; \widetilde{X}_i), \tag{1}$$

where c_i is consumption, Q_i is the level of environmental quality, and $\widetilde{X}_i$ is a vector of state characteristics, except income in state *i*.

Environmental quality in state *i*, Q_i, depends on local pollution, $Q_i = Q(P_i)$, where $Q'<0$, and $Q''>0$, and the pollution level, P_i, depends on local abatement efforts such that $P_i = P(a_i)$, where $P'<0$, and $P''>0$. Abatement expenditures are the sum of all resources used for pollution abatement, including resources wasted due to rent-seeking activities and inefficient regulations. State *i* produces a private good using mobile capital and immobile labor. Publicly provided infrastructure is assumed to raise output, and infrastructure is financed by a capital tax, t_i. The constant returns to scale production function is therefore given by $f(k_i, a_i, s_i)$, where k_i is the capital-labor ratio, a_i is the abatement level, and s_i is the infrastructure-labor ratio level in jurisdiction *i*, where $f_{k_i} > 0$, $f_{a_i} < 0$, and $f_{s_i} > 0$. We make no assumptions on the signs of the cross-partials.

Foreign capital moves freely between the *n* jurisdictions in order to equalize returns, such that

$$f_{k_i}(k_i, a_i, s_i) - t_i = r, \qquad i = 1,\ldots,n, \tag{2}$$

$$\sum_{i=1}^{n} k_i = n\bar{k}, \tag{3}$$

where r is the endogenous return net of taxes, and $\bar{k}$ is the economy-wide level of the capital-labor ratio. Without loss of generality, to fix this notion, we assume that capital flows are from foreign investors and thus capital income is of no concern to domestic residents. Consumption c_i is thus determined by the wage level, which equals $w(k_i, a_i, s_i)$, where $w_{k_i} > 0$, $w_{a_i} < 0$, and $w_{s_i} > 0$. Disregarding wasteful activities, the level of infrastructure is given by the amount of tax revenues raised, such that $s_i = t_i k_i$.

2.2 Rent-seeking and Government Inefficiencies

Within this framework, an obvious consideration is the behavior of voters who are cognizant of rent-seeking possibilities among public officials. We assume that whereas politicians are aware of the true extent of wasted resources, voters are unaware of the amount of resources lost due to rent-seeking and inefficiencies in pollution abatement (environmental policies), taxation, and infrastructure investment. Although unobserved, the inefficiencies are assumed judged by voters through inter-jurisdictional comparisons. Accordingly, voters may be less likely to re-elect the government officials if:

(i) the pollution level relative to abatement expenditures is high, compared to neighboring states; or

(ii) the level of FDI is low relative to infrastructure expenditures, compared to neighboring states; or

(iii) infrastructure investments are low relative to tax payments, compared to neighboring states.

If the ratios discussed in (i)-(iii) do not conform to the levels in neighboring jurisdictions, voters conclude that the home state's politician is engaging in rent-seeking behavior. We should note, however, that voters may be content to re-elect the politician despite an unsatisfactory performance if simultaneously neighbors show increases in pollution, decreases in FDI, and/or decreases in infrastructure investment.

Formally, suppose the inter-jurisdictional comparisons by voters yield minimum acceptable ratios of (i) environmental quality/abatement costs, (ii) capital stock/infrastructure investment, and (iii) infrastructure investment/tax payments. The incumbent politician must therefore meet or exceed these minimum acceptable levels in all three dimensions. Since each minimum ratio required for re-election (denoted by "~") is determined by comparisons, it depends on the ratio in other jurisdictions

$$\left(\frac{\tilde{Q}}{a}\right)_i \geq \Psi_1\left[\left(\frac{Q}{a}\right)_{-i}\right], \tag{4.1}$$

$$\left(\frac{\tilde{k}}{s}\right)_i \geq \Psi_2\left[\left(\frac{k}{s}\right)_{-i}\right], \tag{4.2}$$

$$\left(\frac{\tilde{s}}{t}\right)_i \geq \Psi_3\left[\left(\frac{s}{t}\right)_{-i}\right], \tag{4.3}$$

where $\Psi_1^{'} > 0$, $\Psi_2^{'} > 0$, $\Psi_3^{'} > 0$, and $-i$ indicates all remaining states other than state i. Note that an increase in any of the ratios in state j, $j \neq i$, forces the policymaker in state i to raise the corresponding ratio.

Assuming that no politician has an incentive to exceed the minimum acceptable ratios, (4.1), (4.2), and (4.3) imply

$$Q_i = a_i \Psi_1\left[\left(\frac{Q}{a}\right)_{-i}\right], \tag{5.1}$$

$$k_i = s_i \Psi_2\left[\left(\frac{k}{s}\right)_{-i}\right], \tag{5.2}$$

$$s_i = t_i \Psi_3\left[\left(\frac{s}{t}\right)_{-i}\right]. \tag{5.3}$$

Combining (5.2) and (5.3) implies that the capital-labor ratio in jurisdiction i will partially be determined by comparisons with neighboring jurisdictions, such that

$$k_i = t_i \Psi_2\left[\left(\frac{k}{s}\right)_{-i}\right] \Psi_3\left[\left(\frac{s}{t}\right)_{-i}\right]. \tag{6}$$

Since $c_i = w(k_i, a_i, s_i)$, it follows that we may rewrite (1) as

$$U\left(w\left(t_i\Psi_2\left[\left(\frac{k}{s}\right)_{-i}\right]\Psi_3\left[\left(\frac{s}{t}\right)_{-i}\right],a_i,t_i\Psi_3\left[\left(\frac{s}{t}\right)_{-i}\right]\right),a_i\Psi_1\left[\left(\frac{Q}{a}\right)_{-i}\right];\tilde{X}_i\right)\equiv V(a_i,a_{-i},t_i,t_{-i},s_i,s_{-i};X_i), \tag{7}$$

where X_i includes all state characteristics including income. State i maximizes (7) by setting the first-order conditions equal to zero, such that $\partial V/\partial a_i = 0$, $\partial V/\partial t_i = 0$, and $\partial V/\partial s_i = 0$. Equilibrium policy choices in state i, given by a_i^*, t_i^*, and s_i^*, thus depend on the corresponding policy choices made in neighboring states, as well as home state characteristics. Differentiation of the FOC's yields the following system of reaction functions

$$\begin{aligned}
&\frac{\partial a_i^*}{\partial a_{-i}} = \frac{\partial V_i^2/\partial a_i\partial a_{-i}}{|D|}, \quad \frac{\partial a_i^*}{\partial t_{-i}} = \frac{\partial V_i^2/\partial a_i\partial t_{-i}}{|D|}, \quad \frac{\partial a_i^*}{\partial s_{-i}} = \frac{\partial V_i^2/\partial a_i\partial s_{-i}}{|D|},\\
&\frac{\partial t_i^*}{\partial a_{-i}} = \frac{\partial V_i^2/\partial t_i\partial a_{-i}}{|D|}, \quad \frac{\partial t_i^*}{\partial t_{-i}} = \frac{\partial V_i^2/\partial t_i\partial t_{-i}}{|D|}, \quad \frac{\partial t_i^*}{\partial s_{-i}} = \frac{\partial V_i^2/\partial t_i\partial s_{-i}}{|D|},\\
&\frac{\partial s_i^*}{\partial a_{-i}} = \frac{\partial V_i^2/\partial s_i\partial a_{-i}}{|D|}, \quad \frac{\partial s_i^*}{\partial t_{-i}} = \frac{\partial V_i^2/\partial s_i\partial t_{-i}}{|D|}, \quad \frac{\partial s_i^*}{\partial s_{-i}} = \frac{\partial V_i^2/\partial s_i\partial s_{-i}}{|D|},
\end{aligned} \tag{8}$$

which reflect state i's best response to each of the policy choices of state j, $j \neq i$, and where $|D|$ is the second-order condition of (7) with respect to a_i^*, t_i^*, and s_i^*, respectively, and is required to be negative for a maximum. The diagonal elements in (8) represent the intra-policy reaction functions, while the off-diagonal terms correspond to the various inter-policy reaction functions.

Due to the ambiguous signs of the cross-partials, we are unable to sign the reaction functions in (8). The vectors of reaction functions given by (8) can consequently take either sign, with the exception of knife-edge cases, and when no yardstick competition occurs between states (see Brueckner and Saavedra, 2001). Thus, the goal of the empirical analysis that follows is to test if the various reaction functions have slopes significantly different from zero. Furthermore, the expressions in (8) reveal that the positions of the reaction functions may depend on the underlying characteristics of each jurisdiction. In the empirical work below, while it is necessary to control for such state-specific attributes, we are not concerned with the relative positions of reaction functions, rather our attention is focused solely on estimation of the slopes of the reaction functions.

3. Empirics

3.1 Empirical Specification

To examine the main assertions of the model, our empirical analysis proceeds by analyzing the temporal and spatial patterns of state-level pollution abatement compliance expenditures, tax rates, and infrastructure investment.[5] Specifically, we test whether evidence exists that is consonant with the notion of horizontal strategic interaction. Such analysis provides important insights into the heretofore unanswered question of the presence of inter-policy strategic reaction functions. Moreover, allowing for the possibility of such inter-policy reactions may shed new light on the strength of intra-policy strategic behavior. Finally, while our primary goal in the empirical analysis is the documentation of (or lack thereof) strategic policymaking, we also examine the assumptions underlying the yardstick and resource competition model of the previous section.

To begin the empirical inquiry, consider the "traditional" approach to estimating reaction functions in a uni-dimensional construct:

$$Y_{it} = \phi\Sigma_{48}\, \omega_{ijt}Y_{jt} + x_{it}\beta + \eta_{it}; \qquad i = 1\ldots48; j \neq i \qquad (9)$$

where $Y_{it(jt)}$ is a measure of policy choice in state i (j) at time t, ω_{ijt} is the weight assigned to state j by state i at time t, ϕ is the parameter of interest, as it represents the slope estimate of the reaction function, x_{it} is a vector of state characteristics, and $\eta_{it} = u_t + \alpha_i + e_{it}$, where u_t and α_i are fixed time and state effects, and e_{it} represents idiosyncratic shocks uncorrelated over time, but potentially correlated across states.

To augment this approach and maintain consistency with our theoretical model, we assume an isomorphic weight vector and simply replace Y_{jt} with a policy instrument vector Y_{jtp} and ϕ with a vector of parameters, ϕ_p, where P indexes the three policy instruments mentioned above (taxes, infrastructure spending, and environmental regulations). This regression approach is quite flexible; for example, rather than implicitly assuming orthogonal policies (e.g., restricting neighboring tax rates to influence only own tax rates (intra-policy interaction)), this approach allows, for example, state *j*'s tax rates to influence state *i*'s pollution regulatory stringency (inter-policy interactions).

[5] In the following discussion we assume that abatement expenditures are related to stringency of environmental policies.

If the spirit of competing for resources involves offering a basket of market incentives, then such trade-offs across the individual incentives seem likely; hence it makes sense that there is an inherent marginal rate of substitution across the various instruments. Accordingly, we estimate the augmented (9) separately for each policy instrument. The test for strategic interaction among states therefore requires testing for the statistical and economic significance of ϕ_p.

Before proceeding to a description of the data, we would be remiss not to discuss two important issues in the estimation of the multi-dimensional strategic interaction model. First, in choosing weights, ω, we follow the procedures of Fredriksson and Millimet (2001a; 2001b) and use three straightforward methods. The first approach, deemed Equal weights, assigns a weight of zero to non-contiguous states and equivalent weights to all contiguous states; hence $\sum_j \omega_{ijt} Y_{jt}$ becomes the mean of policies in neighboring states. Our second and third approaches, denoted Income and Population weights, assign weights of zero to non-contiguous states, but weight each contiguous state by its per capita income level or population: $\omega_{ijt} = Z_{jt}/\sum_{j \in J_i} Z_{jt}$, where Z_{jt} is either population or income per capita and J_i is the set of states bordering state *i*. These schemes explicitly allow the weights to vary over time, whereas the Equal weights approach imposes a static weight.

A second major estimation issue relates to the potential endogeneity of the policy vector of other states. In the true spirit of reaction functions, states simultaneously choose their policies, potentially giving rise to concerns about the direction of causation implied in (9). A further specification issue that arises in this framework is the influence of unobservable regional and national shocks that are correlated with the policy decisions of several states (i.e., spatial autocorrelation). To attenuate these potential problems, we follow two distinct approaches. In the first approach, we instrument for neighboring policies via a two-stage least squares regression approach. While other viable procedures are available (e.g., Brueckner and Saavedra, 2001), it is important to recall that instrumental variables (IV) estimation does remain consistent in the presence of spatially correlated error terms (Kelejian and Prucha, 1997; Brueckner, 2001), and offers the advantage of computational ease in light of the multi-dimensional framework. Within a test of strategic policymaking, this is critical since the presence of spatially correlated unobservables could lead one to conclude incorrectly that strategic behavior is evident.

Following Figlio et al. (1999) and Fredriksson and Millimet (2001a; 2001b), we make use of (a subset of) the attributes included in x_{it} for neighboring states as instruments (e.g., population, population density, age composition, and the degree of urbanization) and employ the same weighting scheme for the instruments as we do for neighboring policies.[6] In addition, we include fixed state and time effects in the instrumenting equation. Note that since regression models treating neighboring policies endogenously are over-identified, we provide the results of Lagrange Multiplier (LM) tests for the validity of instruments (Davidson and MacKinnon, 1993, p. 236).

Our second approach to handling these specification issues proceeds by replacing the contemporaneous vector of neighboring policies with its lagged counterpart (see, e.g., Smith, 1997). This particular approach by definition eliminates any concern related to reverse causation since policies enacted in state *i* today should have no direct implications for past policies enacted in neighboring states. This approach has the added benefit of flexibility in that it allows lags in strategic interaction. We allow for two distinct lag processes: i) replace neighboring policies with their lagged values using two year lags and ii) replace neighboring policies with their lagged values using five year lags.

3.2 Data Description

A test for the presence of strategic policymaking in a multi-dimensional world requires data across several state policy items. As aforementioned, to maintain consistency with the spirit of our theoretical inquiry, we focus on three state policies. Our first state-level policy relates to the level of taxation. This particular variable, which is a form of tax effort, is from the Advisory Commission on Intergovernmental Relations and measures the extent to which a state utilizes its available tax bases. It represents a state's actual revenues divided by its estimated capacity to raise revenues based on a model tax code, multiplied by 100. The national average is 100. This variable has been used in a number of previous empirical efforts (e.g., List and Co, 2000; Keller and Levinson, 2001). Our second policy variable measures governmental state expenditures and is defined as "total general expenditures".[7] The data are reported annually by state in the

[6] For example, we use a vector of average neighboring exogenous attributes, weighted equally (by income or population), as instruments for the vector of equally (income, population) weighted average policies in neighboring states.

[7] Note that since the tax variable is not tax revenue, but rather tax effort, there is no issue of government expenditures and tax policy being perfectly co-linear even if states balance their budgets in each period.

Compendium of State Government Finances, and have been used in previous studies of gubernatorial electoral accountability (Besley and Case, 1995b). Our third policy measure is the relative stringency of environmental policies across states. The pollution abatement variable, which is derived in Levinson (2001), measures environmental stringency at the state level as the ratio of *actual* pollution costs per dollar of output to *predicted* pollution costs per dollar of output. A value greater (less) than one indicates that industries in the state spend relatively more (less) per dollar of output on pollution abatement than identical industries located in other states.[8]

Besides these major policy variables, we also make use of several other control variables in the estimation of the augmented (9). In choosing our control variables, we were careful to follow the previous literature (e.g., Fredriksson and Millimet, 2001a; 2001b), and include measures of economic conditions at the state-level, such as per capita income and the rate of unemployment, as well as demographic characteristics, such as age composition (as measured by percentage of young and elderly citizens). Other controls measure the scale of the local economy, and include population and population density. Finally, to provide a control for the heterogeneous populations across space, we include the percentage of urban residents. These state-level data are obtained from the US Bureau of Economic Analysis (http://www.bea.doc.gov). Descriptive information pertaining to each of the variables can be found in Table 1.

4. Empirical Results

Tables 2 and 3 present the empirical results, with Table 2 displaying empirical estimates from the contemporaneous specifications and Table 3 containing estimates from the lagged specifications. Before proceeding to a discussion of the coefficient estimates a few noteworthy items should be briefly discussed. First, p-values in the second to last line in the Tables suggest that Hausman tests reject the exogeneity null at conventional levels in every model except the Population weights specification examining the determinants of tax effort. In addition, our instruments pass the LM test for validity in every case except the model explaining tax effort using the Income weights specification. Second, estimated coefficients on the policy instrument regressors in Tables 2 and 3 should be interpreted as elasticities since we model the regressand

[8] The index of relative abatement expenditures has also been used in Keller and Levinson (2001), Fredriksson et al. (2001), Fredriksson and Millimet (2001a; 2001b), and Millimet and Slottje (2001).

and policy regressors in natural logarithmic form. Finally, results from tests of joint significance presented in Tables 2 and 3 suggest that there is a considerable amount of evidence in favor of the notion that strategic interaction exists between and within state-level policies across space.

While observed in aggregate, strategic interaction results are revealed much more clearly when one considers the actual pattern of individual parameter estimates. Beginning with the contemporaneous specifications in Table 2, we find a fair amount of evidence in favor of inter-policy spatial interaction within each of the three policy instruments. For instance, empirical results in columns 1-3 of Table 2 suggest that neighboring spending levels influence the stringency of pollution regulations at conventional significance levels (and to a lesser extent tax effort influences pollution regulations). Besides their statistical significance, these estimates are also economically significant: in the Equal weights model a 10% increase in neighbors' spending levels is associated with a 12.3% decrease in own relative abatement expenditures. Although this result is not evident in the two-year lag model in Table 3, it is supported in the five-year lag model. While many possible explanations are plausible for such a result, this finding is consonant with a state bargaining with a locating firm by conceding a lax environmental policy as a reaction to neighboring states enhancing their infrastructure.

Moving to the tax effort specifications, we find sporadic evidence in favor of the multi-dimensional approach. Whereas the contemporaneous models reveal no robust inter-policy results, there is evidence that a statistically significant interaction exists between neighboring governmental expenditures and own tax rates in the lagged models. In two of the three two-year lag models the coefficient estimate is significant at the $p < .01$ level ($p < .10$ in the third model), and suggests that increases in state *i*'s neighbors' expenditure levels are associated with tax rate decreases in state *i*. The magnitude of the estimate, however, is small: a 10% increase in neighbors' expenditure levels induces approximately a 1.4% tax rate decrease. In the five-year lag model this result is not evident, yet there appears to be a relationship between neighboring abatement levels and own tax rates. Interestingly, the coefficient estimate does not accord well with the other interaction estimates, which are largely consistent with a basket of incentive tradeoffs, as in this case the parameter estimates indicate that increases in neighboring abatement expenditures are associated with lower tax rates.

We also find evidence in favor of cross-policy interactions within the third policy instrument, government expenditures. In this case, there is no discernible influence of neighboring abatement expenditures affecting own government expenditures, but neighboring tax rates do appear to have an influence. Even though there is sporadic evidence in favor of a direct relationship between neighboring tax effort and own government expenditures (see Table 2), the results are in line with the market basket incentive tradeoff conjecture: results in the lagged models strongly indicate that a higher neighboring tax effort reduces a state's governmental expenditures. The magnitude of the effect is in the range of a 2.5% decrease in expenditures for each 10% increase in neighboring taxes.

These behavioral patterns extend beyond between-policy interactions, as they spillover quite nicely to the within-policy parameter estimates. Considering the contemporaneous specifications in Table 2, we find a fair amount of evidence in favor of intra-policy spatial interaction: neighboring tax and spending rates tend to be positively associated with own tax and spending rates. In the tax (spending) specifications, the effect is significant in two (three) of the three weighting schemes, and the coefficients from the Equal weights model suggest that a 10% increase in neighboring taxes (spending) increases own tax efforts (spending) by 9.3% (12.0%). These general results become even stronger statistically when we consider the lagged results in Table 3. In these models, there are signs of all three of the neighboring policies having an influence on the comparable own-state policies. Overall, these results are largely consonant with the growing literature on uni-dimensional policies.

4.1 Sensitivity analysis

Following Fredriksson and Millimet (2001a), we perform two sets of sensitivity analyses to assess the robustness of the results discussed above. First, we consider three additional weighting schemes: Equal, Income, and Population weights, except defined over regional, as opposed to only contiguous, neighbors. The eight regional assignments are taken from the BEA.[9] In the interest of brevity, empirical results are not presented, but we make them available

[9] The regional assignments are as follows: (i) New England: Maine, New Hampshire, Vermont, Massachusetts, Rhode Island, Connecticut; (ii) Mideast: New York, New Jersey, Pennsylvania, Delaware, Maryland; (iii) Great Lakes: Ohio, Indiana, Illinois, Michigan, Wisconsin; (iv) Plains: Minnesota, Iowa, Missouri, North Dakota, South Dakota, Nebraska, Kansas; (v) Southeast: Georgia, Florida, Virginia, West Virginia, North Carolina, South Carolina, Kentucky, Tennessee, Alabama, Mississippi, Arkansas, Louisiana; (vi) Southwest: Oklahoma, Texas, Arizona, New Mexico; (vii) Rocky Mountain: Montana, Idaho, Wyoming, Colorado, Utah; and, (viii) Far West: Washington, Oregon, California, Nevada.

upon request. We do note that the results suggest a much weaker degree of strategic policymaking at the regional level: in general, estimated elasticities, when statistically significant, are much smaller in absolute value than corresponding estimates reported in Tables 2 and 3.

Second, we allow for the fact that states may react asymmetrically to changes in neighboring policies. In particular, states that have been more (less) successful in the recent past attracting mobile capital may respond differently to changes in neighboring policies. Thus, we estimate the following revised version of (9)

$$Y_{it} = \phi_{0p} I_{it} \Sigma_{48}\, \omega_{ijt} Y_{jtp} + \phi_{1p}(1-I_{it}) \Sigma_{48}\, \omega_{ijt} Y_{jtp} + x_{it}\beta + \eta_{it}; \qquad i = 1\ldots48;\, j \neq i \qquad (9')$$

where I_{it} is an indicator variable, taking a value of one if own FDI exceeds (the weighted average of) neighboring foreign direct investment (FDI) and ϕ_{0p} and ϕ_{1p} are the parameters of interest.[10] Again, in the interest of brevity, empirical estimates are not presented, but a noteworthy result is that tests of the equality of ϕ_{0p} and ϕ_{1p} rarely reject the null, and in the few cases where the null is rejected, the economic difference between the parameters is minimal. Consequently, it does not appear that states respond differentially based on past success in attracting mobile capital.

4.2 Is capital competition or yardstick competition responsible for the observed policy patterns?

The empirical evidence discussed above is suggestive that both intra- and inter-policy interactions occur between state governments. An interesting open issue that remains unresolved is the nature of the underlying mechanism at work. For example, whether capital competition or yardstick competition is inducing the observed patterns is unknown. The literature has shown evidence in favor of both mechanisms: using state-level data on gubernatorial elections, Besley and Case (1995a) present evidence that indicates an incumbent’s future as a governor is critically linked to the level of taxes in neighboring jurisdictions. Revelli (2001) presents similar insights using tax data in English district election results. In a related sense, Brett and Pinske (2000) make use of data on municipal tax rates in British Columbia to show that a jurisdiction’s tax base is inversely related to its own tax rate, suggesting resource competition is important in shaping reaction functions.

[10] We tried two stock measures of FDI: (i) gross value of plant, property, and equipment (PP&E) and (ii) employment in foreign-owned affiliates for total manufacturing. We also used a flow measure of FDI: the number

A natural set of equations associated with our theoretical model allows us to further investigate the issue of capital competition versus yardstick competition using our data. For example, embedded in the model is the intuition behind the spatial allocation of mobile capital, and underlying equations (4.1), (4.2), and (4.3) is the voters' re-election decisions. The performance of the governor in state *i* is judged using comparisons, and only if rent-seeking and waste is sufficiently great such that either of these inequalities do not hold will a governor lose re-election (based on inadequate performance). Given state characteristics, voter strategy in state *i* is thus to re-elect the governor if (4.1), (4.2), and (4.3) all hold, and re-elect the challenger if any of these inequalities are violated. This decision rule gives rise to the strategic interaction between state governors (governments) described by (8).

To provide initial insights into the response coefficients within the model, we estimate two auxiliary regressions. The first regression revolves around estimating the determinants of FDI stocks within the US states. In this regression model, we augment the work of Fredriksson et al. (2001) by including spillover terms in the regressor vector, leading us to estimate a linear fixed effects panel data model of the form

$$FDI_{it} = \theta_p \sum_{48} \omega_{ij} Y_{jtp} + v_{it}\beta + \varphi_{it}; \qquad i = 1\ldots48;\ j \neq i \qquad (10)$$

where FDI_{it} is a continuous measure of inbound foreign direct investment for state *i* at time *t*;[11] ω_{ij} is the weight assigned to state *j* by state *i* (time invariant equal weights are used); θ_p is the parameter vector of interest, where nonzero slope coefficient estimates suggest neighboring states' policies affect state *i*'s resource inflows; v_{it} is a vector of state characteristics that are believed to affect FDI flows, and includes measures of state *i*'s own policies. In addition, the vector v_{it} includes the remaining control variables used in Fredriksson et al. (2001): unemployment and unionization rates, population, industrial energy prices, agricultural land value, and the mean hourly wage of production workers. Finally, $\varphi_{it} = u_t + \alpha_i + e_{it}$; where u_t and α_i are fixed time and state effects, and e_{it} represents idiosyncratic shocks uncorrelated across states and over time.

of new foreign-owned manufacturing plants. Finally, we also used lagged values of these measures to define the indicator variable. All specifications yielded qualitatively similar results.

[11] Our regressands are two continuous measures of FDI: (i) gross value of PP&E and (ii) employment in foreign-owned affiliates for total manufacturing.

Given that a state's environmental policy and spending (GSP) may be endogenous in this FDI model, we follow Fredriksson et al. (2001) and instrument for both. We also follow Fredriksson et al. in that we allow the effect of abatement expenditures to be non-linear. In addition, given the setup of the model presented in the previous section, we also treat tax policy as endogenous and instrument for it as well. The instrument set includes share of legal services in GSP and its quadratic, non-military government employment and its quadratic, per capita income and its quadratic, the interaction between share of legal services and government employment, percentage of elderly population, and percentage of children in the population.[12] Descriptive information of the variables of interest can be found in Table 1.

Empirical results are presented in Table 4. To provide a robustness check, we include estimates for both OLS and fixed effects IV models. We also include estimates from models that include state and time fixed effects as well as estimates from models that replace time effects with a linear time trend. In the IV models, we also include results from overidentification tests of Davidson and MacKinnon (1993), as well as Hausman tests of exogeneity. We first note that in both model types for gross value of plant, property, and equipment (PP&E) and employment in foreign-owned affiliates for total manufacturing we reject the null of exogeneity. In addition, in three of the four model specifications our instruments pass the LM test for validity.

Moving to the coefficient estimates, we find that, overall, there is evidence that one's own policies affect resource flows, especially in the IV fixed effects models. As is evident, there is a degree of significance in each of the three own policy measures. For example, parameter estimates in the first two rows of Table 4 suggest that pollution abatement expenditures are a deterrent to foreign investors over relevant ranges (roughly two-thirds of all observations are on the downward-sloping portion of the U-shaped relationship). This effect is significantly different from zero at the $p < .01$ level and is consonant with the resource competition model. Although there is some evidence in favor of the conjecture that neighboring policies affect one's own resource inflows, the statistical significance of the coefficients is not convincing. The remaining control variables enter the specification in broad agreement with previous studies.

[12] In addition, we should note that we include state and time fixed effects in the instrumenting equation in the specifications where they are included in the FDI equation. The instrument set is identical to that used in Fredriksson et al. (2001) with the addition of the age composition variables. The inclusion of these variables follows from Besley and Case (1995a) who utilize such variables as instruments for tax policy.

Our second auxiliary regression approach is in the spirit of the yardstick competition literature and models the allocation of votes between the incumbent and her rivals as a function of own and neighboring policies. To operationalize our voting construct, we make use of state-level gubernatorial election data from 1977-1994. To gather these data, we began by obtaining Besley and Case's (1995b) data and combining them with an updated data set from List and Sturm (2001). The data are constructed to include dummy variables that indicate whether the current governor faces a binding term limit, his/her party affiliation, the duration in power (of the individual governor as well as her party), and the current President's affiliation. A detailed description of these data and their sources can be found in List and Sturm (2001). We include descriptive information of the variables in Table 1.

We model the determinants of voting patterns by estimating a one-way linear fixed effects panel data model over the 1977-1994 period[13]

$$INC\%_{it} = \Gamma_p \Sigma_{48}\, \omega_{ij} W_{jtp} + q_{it}\beta + \delta_{it}; \qquad i = 1\ldots48;\ j \neq i \qquad (11)$$

where $INC\%_{it}$ is the percentage of the vote received by the incumbent party for state *i* at time *t*; ω_{ij} is the weight assigned to state *j* by state *i* at time *t* (time invariant equal weights are used); Γ_p is the parameter vector of interest, where nonzero slope coefficients suggest neighboring states' policies affect gubernatorial voting patterns in state *i*; W_{jtp} is a vector of neighboring policies, including abatement, tax effort, and FDI (measured by employment); q_{it} is a vector of state characteristics that are believed to affect state-level voting patterns, and includes measures of state *i*'s own policy vector as well as other controls including previous margin of victory, duration in power, national growth of GDP, national inflation rate, and dichotomous regressors for whether a democrat is in power and whether the President is from the incumbent's party. We also interact several of the regressors as added controls (see Table 5).

We use abatement, tax effort, and foreign capital as the three relevant policies in the home and neighboring state in (11) to be consistent with (4.1), (4.2), and (4.3). (4.1) predicts that voters make comparisons based on environmental quality and abatement levels, where environmental quality is a function of abatement. (4.2) suggests that voters make comparisons based on the level of capital stock and government expenditures. Finally, (4.3) implies that voters make comparisons based on governmental expenditures and taxes, where expenditures are

[13] Initially we included fixed time effects but they tended to swamp the parameter estimates so we excluded time effects but include a time trend.

a function of tax policy. Thus, conditional on neighboring levels of abatement, tax effort, and FDI, these policies in the home state should influence election outcomes (and vice versa).

Akin to the issues outlined above, endogeneity of certain regressors could again be problematic. We therefore opt for a fixed effects IV model with the instrument set having similar components as the FDI models, but here we omit per capita income, and include unionization rate, industrial energy prices, and highway mileage.[14] Descriptive information for these additional variables can be found in Table 1.

Table 5 presents the results of several models. The first two columns in Table 5 contain data from all elections, whereas the rightmost two columns contain estimation results for the elections that are not influenced by the binding term limit rules. Before discussing the parameter estimates, we should note that in each case the instruments pass the LM test for validity. Yet, the Hausman exogeneity test does not reject the exogeneity of the three policies in the home state (i.e., abatement, tax effort, and FDI) at conventional levels. Overall, given the great demands placed on the data, it is not surprising that many of the coefficients are insignificantly different from zero. Nevertheless, one stark result is that neighboring tax effort has a consistent positive influence on the incumbent's degree of success. This result is consistent with the yardstick competition model (as well as the results in Besley and Case (1995a)) and suggests that there is a degree of comparative behavior across states. As such, coupling results from both sets of auxiliary regression models suggests that there is evidence implying reaction functions may be driven by both sets of influences—resource competition and yardstick competition.

5. Concluding Comments

Whether strategic interaction of public policies is prevalent amongst governments merits serious consideration. Since many current institutional arrangements in the US are designed to either attenuate or eliminate possibilities of horizontal strategic interaction, it is important to determine if a considerable amount of strategic interaction exists. In this paper we argue that it is not only the within-policy interaction that should be considered, but also the cross-policy reaction functions. The current literature only considers strategic interaction in a uni-dimensional framework. Our findings are consistent with the notion that reaction functions *between* some policies have a nonzero slope. For example, we find that states respond to

[14] We also include state and time fixed effects in the instrumenting equation.

increased governmental expenditure levels of neighbors by lowering their own pollution standards. If these cross-policy interactions are ignored, then the overall level of strategic interaction could be considerably underestimated. Our results also confirm the extant literature in that we observe a good deal of intra-policy horizontal strategic interaction. In our attempt to discriminate between the competing models, we find that both capital competition and yardstick competition models have a degree of predictive power. While these results seem to be a step forward, we by no means consider this study to be the final word on this topic. Much scope remains for fruitful exploration.

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Table 1. Summary Statistics, 1977 - 1994.

Variable	Observations	Mean	Standard Deviation
Abatement (Levinson (2001) index)	864	1.02	0.37
Tax Effort	864	96.06	15.93
Government Expenditures	864	7.89E+06	1.05E+07
FDI: Total Employment	861	3.26E+04	3.64E+04
FDI: Plant, Property, & Equipment	858	2845.96	3872.65
Population	864	4.94E+06	5.13E+06
Population Density	864	164.19	230.30
Urbanization	864	0.67	0.14
Unemployment Rate	864	0.07	0.02
Per Capita Income	864	1.19E+04	2029.67
% Elderly (> 65 years)	864	0.12	0.02
% Kids (5 - 17 years)	864	0.20	0.02
Highway Mileage (miles)	864	8.05E+04	4.84E+04
Unionization Rate	864	0.16	0.07
Average Manufacturing Wage	864	9.13	2.20
Agricultural Land Value	864	880.00	767.42
Industrial Energy Prices	864	5.53	1.68
Legal Services (share of GSP)	864	0.01	0.00
Government Employment (non-military, per 1000)	864	346.46	350.39
Incumbent Party (Percent of Vote)	205	54.64	9.69
National Growth Rate	864	0.03	0.02
National Inflation Rate	864	0.05	0.02
Same (1 = governor of same party as president)	864	0.50	0.50
Democrat (1 = democratic governor)	858	0.61	0.49
Margin of Victory	856	16.83	14.59
Duration in Power	864	8.20	6.71

Table 2. Strategic Interaction over Multiple Policy Instruments Across States, 1977 - 1994.

Var./Depdt. Var.	ln(Own Abatement)			ln(Own Tax Effort)			ln(Own Gov't Expenditure)		
	Equal Weights	Income Weights	Pop. Weights	Equal Weights	Income Weights	Pop. Weights	Equal Weights	Income Weights	Pop. Weights
ln(Neighboring Abatement)	0.05	0.10	1.96	0.66	-0.20	-0.34	0.79	0.03	-0.40
	(0.10)	(0.24)	(0.83)	(1.53)	(-1.69)	(-0.69)	(1.46)	(0.22)	(-0.54)
ln(Neighboring Tax Effort)	-1.26	-0.83	-2.52	0.93	0.28	0.60	0.98	0.77	0.67
	(-2.42)	(-1.59)	(-1.24)	(2.49)	(1.98)	(1.41)	(2.10)	(4.25)	(1.06)
ln(Neighboring Government Expenditure)	-1.23	-1.21	-1.96	0.29	-0.18	0.08	1.20	0.81	1.00
	(-3.07)	(-3.39)	(-1.67)	(1.04)	(-1.85)	(0.34)	(3.45)	(6.51)	(2.75)
Population	1.75E-08	1.15E-08	1.24E-07	8.74E-09	-1.47E-08	-2.86E-08	3.78E-08	1.12E-08	-4.24E-09
	(0.68)	(0.45)	(0.82)	(0.63)	(-2.16)	(-0.90)	(2.19)	(1.27)	(-0.09)
Population Density	-5.21E-04	5.29E-04	-2.45E-03	8.02E-04	1.43E-04	6.16E-04	1.58E-03	1.28E-03	1.23E-03
	(-0.37)	(0.37)	(-0.68)	(1.23)	(0.37)	(-0.81)	(1.94)	(2.56)	(1.09)
% Urban	1.65	2.03	0.18	-1.69	-0.66	-0.64	-1.50	-1.06	-0.31
	(1.69)	(2.13)	(0.08)	(-2.90)	(-2.61)	(-1.30)	(-2.06)	(-3.21)	(-0.43)
Unemployment Rate	0.02	0.02	-0.01	-0.01	0.01	0.01	-0.01	9.80E-04	0.01
	(1.87)	(2.02)	(-0.15)	(-1.00)	(2.39)	(1.13)	(-1.30)	(0.24)	(0.57)
Per Capita Income	-1.29E-05	5.21E-05	8.90E-04	-2.09E-04	9.75E-05	-1.44E-04	-2.00E-04	9.65E-05	-2.11E-04
	(-0.04)	(0.16)	(1.12)	(-1.06)	(1.11)	(-0.86)	(-0.81)	(0.84)	(-0.85)
(Per Capita Income)^2	-1.31E-09	-4.95E-09	-8.07E-08	1.25E-08	-9.39E-09	1.05E-08	1.37E-08	-6.67E-09	1.77E-08
	(-0.05)	(-0.20)	(-1.12)	(0.87)	(-1.45)	(0.69)	(0.76)	(-0.79)	(0.79)
(Per Capita Income)^3	5.16E-14	1.40E-13	2.04E-12	-3.29E-13	2.15E-13	-2.87E-13	-2.76E-13	2.34E-13	-3.61E-13
	(0.08)	(0.23)	(1.12)	(-0.91)	(1.33)	(-0.75)	(-0.61)	(1.11)	(-0.64)
% Elderly	2.54	2.74	-0.97	0.05	0.18	0.68	-2.03	-2.12	-0.81
	(1.36)	(1.46)	(-0.23)	(0.06)	(0.35)	(0.77)	(-2.12)	(-3.27)	(-0.62)
% Young	1.64	1.27	0.62	-0.26	1.38	1.30	-2.21	-1.18	-0.57
	(1.07)	(0.83)	(0.32)	(-0.27)	(3.37)	(3.16)	(-1.87)	(-2.22)	(-0.94)
State Effects Inc.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Effects Inc.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Joint Significance: All Neighboring Policies	[p=0.00]	[p=0.00]	[p=0.11]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]
Joint Significance: Neighboring Cross-Policies	[p=0.01]	[p=0.00]	[p=0.12]	[p=0.24]	[p=0.14]	[p=0.60]	[p=0.04]	[p=0.00]	[p=0.26]
Overidentification Test	[p=0.15]	[p=0.26]	[p=0.12]	[p=0.44]	[p=0.00]	[p=0.09]	[p=0.14]	[p=0.11]	[p=0.22]
Hausman Test for Exogeneity	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.01]	[p=0.04]	[p=0.16]	[p=0.00]	[p=0.00]	[p=0.00]
Observations	864	864	864	864	864	864	864	864	864

NOTES: All regressions estimated via IV-FE. Instrument set includes the weighted average of neighboring values for: population, population density, % urban, % elderly, and % young. In addition, neighboring per capita income is used as an instrument in the abatement equations; neighboring unemployment as an instrument in the tax and expenditure equations. Overidentification test is from Davidson and MacKinnon (1993, p. 236). T-statistics in parentheses.

Table 3. Strategic Interaction over Multiple Policy Instruments with a Lag: Selected Coefficients.

Var./Depdt. Var.	ln(Own Abatement)			ln(Own Tax Effort)			ln(Own Gov't Expenditure)		
	Equal Weights	Income Weights	Pop. Weights	Equal Weights	Income Weights	Pop. Weights	Equal Weights	Income Weights	Pop. Weights
Two-Year Lag:									
ln(Neighboring Abatement)	0.17	0.17	0.22	-0.01	-0.01	0.01	0.03	0.03	0.03
	(2.15)	(2.15)	(2.48)	(-0.27)	(-0.45)	(0.65)	(1.20)	(1.21)	(1.41)
ln(Neighboring Tax Effort)	-0.84	-0.84	-0.61	0.34	0.34	0.37	-0.16	-0.18	-0.25
	(-3.82)	(-3.98)	(-3.64)	(6.05)	(6.18)	(8.61)	(-2.54)	(-3.05)	(-5.36)
ln(Neighboring Government Expenditure)	0.06	0.01	0.13	-0.14	-0.13	-0.07	0.26	0.25	0.18
	(0.30)	(0.05)	(0.80)	(-2.74)	(2.89)	(-1.71)	(4.74)	(5.08)	(3.94)
Joint Significance: All Neighboring Policies	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]
Joint Significance: Neighboring Cross-Policies	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.02]	[p=0.00]	[p=0.19]	[p=0.02]	[p=0.00]	[p=0.00]
Five-Year Lag:									
ln(Neighboring Abatement)	-0.05	-0.07	-0.24	-0.11	-0.11	-0.07	-4.32E-03	-3.15E-03	0.01
	(-0.59)	(-0.72)	(-2.29)	(-5.32)	(-5.19)	(-2.93)	(-0.19)	(-0.14)	(0.35)
ln(Neighboring Tax Effort)	-1.07	-0.95	-0.95	0.14	0.14	0.19	-0.23	-0.24	-0.26
	(-4.30)	(-4.03)	(-4.68)	(2.51)	(2.51)	(4.00)	(-3.61)	(-3.98)	(-5.02)
ln(Neighboring Government Expenditure)	-0.54	-0.39	-0.41	-0.02	-0.03	-0.01	0.12	0.10	0.04
	(-2.21)	(-1.79)	(-1.92)	(-0.41)	(-0.57)	(-0.21)	(1.92)	(1.87)	(0.67)
Joint Significance: All Neighboring Policies	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]
Joint Significance: Neighboring Cross-Policies	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.01]	[p=0.00]	[p=0.00]	[p=0.00]

NOTES: All regressions estimated via OLS-FE, and include the same controls as in Table 4. Number of observations is 768 in the two-year lag specifications, 624 in the five-year specifications. T-statistics in parentheses.

Table 4. Determinants of FDI Stocks Across States, 1977 - 1994.

	Model I				Model II			
	OLS		IV		OLS		IV	
Var./Depdt. Var.	**PP&E**	**Emp.**	**PP&E**	**Emp.**	**PP&E**	**Emp.**	**PP&E**	**Emp.**
Own Abatement	85.41	8.22	-40572.69	-2449.19	-693.16	-34.79	-38887.48	-2419.33
	(0.11)	(0.17)	(-2.55)	(-3.06)	(-0.92)	(-0.71)	(-3.65)	(-4.13)
(Own Abatement)^2	108.42	5.20	19149.18	1043.33	347.66	18.55	17402.47	1000.28
	(0.41)	(0.31)	(2.89)	(3.16)	(1.29)	(1.05)	(-3.85)	(4.01)
Neighboring	-2518.73	-145.24	-912.48	-66.30	-4526.19	-191.97	-2007.02	-44.01
Abatement	(-1.06)	(-0.94)	(-0.11)	(-0.16)	(-1.88)	(-1.20)	(-0.28)	(-0.11)
(Neighboring	1303.96	8.16	4.06	14.00	1940.04	88.82	341.51	1.91
Abatement)^2	(1.20)	(1.17)	(0.00)	(0.07)	(1.76)	(1.22)	(0.10)	(0.01)
Own Government	0.26	0.02	0.23	0.02	0.27	0.02	0.26	0.02
Expenditure	(14.99)	(14.06)	(2.71)	(3.74)	(15.66)	(13.70)	(3.91)	(4.37)
Neighboring	0.23	0.01	0.24	3.90E-03	0.23	0.01	0.20	2.14E-04
Government	(8.05)	(5.05)	(1.76)	(0.54)	(8.17)	(3.76)	(1.91)	(0.04)
Expenditure								
Own Tax Effort	25.00	-0.57	169.84	7.23	22.94	-0.31	150.83	7.86
	(2.94)	(-1.04)	(2.26)	(1.93)	(2.68)	(-0.55)	(2.67)	(2.56)
Neighboring	79.92	3.6	-59.08	-4.35	78.79	3.59	-48.25	-4.73
Tax Effort	(6.47)	(4.53)	(-0.89)	(-1.31)	(6.42)	(4.44)	(-0.94)	(-1.68)
Highway Mileage	0.09	4.41E-03	0.04	1.99E-03	0.09	4.26E-03	0.05	1.91E-03
	(9.00)	(6.56)	(0.93)	(1.00)	(8.67)	(6.06)	(1.37)	(1.05)
Population	0.50	0.03	0.85	0.03	0.38	0.03	0.55	0.03
	(3.86)	(3.58)	(1.42)	(0.97)	(2.95)	(3.32)	(1.21)	(1.12)
Unemployment	-240.48	-16.96	-464.84	-25.18	-113.79	-14.29	-285.2	-22.76
Rate	(-5.65)	(-6.18)	(-2.74)	(-3.01)	(-3.30)	(6.30)	(-2.54)	(-3.75)
Unionization	-96.54	-5.58	-228.99	-13.87	-61.69	-6.4	-202.27	-17.00
Rate	(-2.44)	(-2.19)	(-1.52)	(1.86)	(-1.62)	(-2.57)	(-1.69)	(-2.60)
Wages	603.49	34.36	-154.49	-16.12	502.48	33.52	-150.8	-10.67
	(3.98)	(3.52)	(-0.25)	(-0.52)	(3.52)	(3.57)	(-0.34)	(-0.43)
Agricultural	-0.16	-0.03	0.71	0.05	-0.01	-0.01	0.85	0.06
Land Value	(-0.84)	(-2.43)	(0.68)	(0.94)	(-0.05)	(-0.97)	(1.23)	(1.65)
Industrial	-280.26	-9.24	132.68	9.42	-172.13	5.31	138.06	19.66
Energy Prices	(-3.39)	(-1.73)	(0.42)	(0.59)	(-3.19)	(1.49)	(0.77)	(1.97)
Time Trend					-192.66	-11.03	20.49	5.01
					(-3.84)	(-3.33)	(0.12)	(0.55)
State Effects Inc.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Effects Inc.	Yes	Yes	Yes	Yes	No	No	No	No
Ho: Time Effects	$F_{(17,778)}$=4.49	$F_{(17,781)}$=6.46	$F_{(17,778)}$=0.26	$F_{(17,781)}$=0.49				
are Equal	[p=0.00]	[p=0.00]	[p=1.00]	[p=0.96]				
Adj.-R2	0.90	0.95			0.89	0.95		
Overidentification			[p=0.45]	[p=0.03]			[p=0.91]	[p=0.29]
Test								
Hausman Test			[p=0.00]	[p=0.00]			[p=0.00]	[p=0.00]
for Exogeneity								
Observations	858	861	858	861	858	861	858	861

NOTES: PP&E = Plant, Propoerty, and Equipment. Employment measured in 100s. T-statistics in parentheses. Instrument set includes: share of legal services in GSP and its quadratic, non-military government employment and its quadratic, per capita income and its quadratic, the interaction between share of legal services and government employment, % of elderly population, and % of children in population. Overidentification test from Davidson and Mackinnon (1993, p. 236).

Table 5. Determinants of Percentage of Vote Received by the Incumbent Party, 1977 - 1994.

	All Elections				All Elections Not Affected by Binding Term Limits on the Incumbent			
	Model I		Model II		Model I		Model II	
Variables	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Own Abatement	0.01	-3.46	3.25	14.32	-2.30	-9.89	3.46	15.33
	(0.00)	(-0.25)	(0.88)	(0.59)	(-0.74)	(-0.62)	(0.76)	(0.65)
Neighboring	-5.71	-0.26	-0.32	-5.77	-10.03	-5.91	-4.15	-6.27
Abatement	(-0.77)	(-0.02)	(-0.04)	(-0.17)	(-1.23)	(-0.61)	(-0.42)	(-0.36)
Own Tax Effort	-0.03	-0.78	-0.21	-1.27	-0.70	-0.51	-0.19	-0.70
	(-0.24)	(-1.84)	(-1.33)	(-2.13)	(-0.51)	(-1.38)	(-1.10)	(-1.47)
Neighboring	0.33	0.77	0.70	1.50	0.41	0.68	0.72	1.06
Tax Effort	(1.73)	(2.34)	(3.12)	(2.81)	(2.03)	(2.10)	(2.75)	(2.37)
Own Inbound	-3.28	2.31	1.31	14.68	-5.02	-1.09	0.66	18.35
FDI	(-0.48)	(0.22)	(0.16)	(0.45)	(-0.67)	(-0.11)	(0.07)	(0.70)
Neighboring	-10.34	-7.40	0.93	3.82	-9.06	-11.76	-8.41	-12.33
Inbound FDI	(-0.92)	(-0.49)	(0.08)	(0.17)	(-0.71)	(-0.73)	(-0.58)	(-0.55)
Own Per Capita	2.78	2.24	3.45	6.78	2.79	0.67	2.90	1.66
Income	(1.75)	(0.10)	(2.24)	(0.16)	(1.56)	(0.23)	(1.61)	(0.48)
Neighboring	-1.46	-3.12	-1.74	-2.84	-0.88	0.67	-0.83	-0.24
Per Capita Income	(-0.74)	(-0.11)	(-0.92)	(-0.08)	(-0.40)	(0.18)	(-0.37)	(-0.06)
Democrats in	-2.74	-0.17	-4.23	-5.49	-3.59	2.49E-03	-1.94	-0.81
Power (1 = yes)	(-0.70)	(-0.04)	(-1.11)	(-0.87)	(-0.70)	(0.03)	(-0.37)	(-0.11)
Previous Margin	-0.07	-0.07	1.77	3.08	-0.01	-2.67	1.95	2.22
of Victory	(-1.16)	(-1.01)	(1.99)	(1.04)	(-0.12)	(-0.43)	(1.74)	(0.98)
Duration in	-0.41	-0.45	-0.34	-0.37	-0.19	-0.19	-0.14	-0.20
Power	(-2.26)	(-2.25)	(-1.94)	(-1.49)	(-0.99)	(-0.86)	(-0.71)	(-0.80)
Same Party as	-21.48	-16.80	-24.52	-28.35	-22.21	-22.51	-16.65	-14.45
President (1 = yes)	(-1.94)	(-1.25)	(-2.28)	(-1.62)	(-1.48)	(-1.19)	(-1.08)	(-0.63)
National Growth	-1.14	-0.99	-1.34	-1.65	-1.00	-0.98	-1.07	-1.27
Rate of GDP	(-2.02)	(-1.54)	(-2.47)	(-1.90)	(-1.54)	(-1.32)	(-1.65)	(-1.47)
Same * National	1.64	1.42	1.94	2.35	1.73	1.70	1.64	1.81
Growth	(1.90)	(1.46)	(2.32)	(1.95)	(1.64)	(1.42)	(1.53)	(0.34)
National Inflation	-99.17	-122.39	-85.81	-166.52	-94.60	-142.87	-44.60	-34.48
Rate	(-1.12)	(-1.22)	(-1.00)	(-1.32)	(-0.87)	(-1.02)	(-0.40)	(-0.20)
Same * Inflation	236.24	188.34	265.36	342.75	238.86	254.72	154.59	126.12
	(1.54)	(1.02)	(1.77)	(1.41)	(-1.16)	(0.97)	(0.72)	(0.39)
INTERACTIONS:								
WITH PREVIOUS								
MARGIN OF VICTORY:								
own abatement			-0.20	-1.38			-0.27	-1.06
			(-1.22)	(-1.25)			(-1.33)	(-1.16)
neigh. abatement			-0.36	0.39			-0.41	0.39
			(-1.07)	(0.23)			(-0.77)	(0.26)
own tax effort			4.73E-03	0.01			3.40E-03	0.01
			(1.16)	(0.43)			(0.67)	(0.51)
neigh. tax effort			-0.02	-0.03			-0.02	-0.02
			(-2.35)	(-1.45)			(-1.93)	(-1.42)
own FDI			-0.34	-0.08			-0.34	-0.75
			(-1.54)	(-0.93)			(-1.35)	(-0.93)
neigh. FDI			-0.50	-0.04			-0.95	-0.14
			(-1.79)	(-0.63)			(-0.25)	(-0.23)
Joint Sign.: Own Policies	[p=0.97]	[p=0.33]	[p=0.49]	[p=0.18]	[p=0.75]	[p=0.59]	[p=0.67]	[p=0.41]
Joint Sign: Neighboring Policies	[p=0.25]	[p=0.10]	[p=0.02]	[p=0.03]	[p=0.14]	[p=0.14]	[p=0.06]	[p=0.11]
Joint Significance: All Interactions			[p=0.01]	[p=0.05]			[p=0.26]	[p=0.76]
Overidentification Test		[p=0.18]		[p=0.57]		[p=0.26]		[p=0.32]
Hausman Test		[p=0.18]		[p=0.17]		[p=0.47]		[p=0.54]

NOTES: FDI measured by employment in 100,000s. Per capita income measured in 1000s. T-statistics in parentheses. All regressions include state fixed effects. Instrument set includes the same instruments as in Table 2, omitting per capita income, and including unionization rate, industrial energy prices, and highway mileage.

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