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ABSTRACT

Empirical evidence suggesting that a considerable amount of horizontal strategic interaction exists amongst governments is important in light of recent devolutionary trends of many important public programs. The empirical approach in these studies typically relies on estimating reaction functions in a uni-dimensional policy framework, where a nonzero slope estimate is interpreted as evidence in support of strategic interactions. While this framework is a useful representation within certain contexts, it is potentially too restrictive; for example, in models of resource competition, localities may use multiple instruments in their recruiting pursuits, leading to potential strategic interactions *across* policy instruments. In this study, we first develop a simple theoretic construct that includes resource competition in a world of three-dimensional policy choice. The model suggests that while a zero-sloped reaction function may exist for any particular policy, this does not necessarily imply the absence of strategic interactions. We examine the implications of the model empirically using US state-level panel data over the period 1977-1994. The results suggest that important cross-policy strategic interactions exist, lending support in favor of the multi-dimensional framework, and indicate that uni-dimensional frameworks may present lower bound estimates of the degree of strategic interaction.

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I. Introduction

Throughout its history the United States has utilized a federal system to promote greater efficiency and innovation in government practice. Federalism is endorsed because it provides freedom for states and localities to choose their optimal bundle of public goods and services. Indeed, this freedom of choice has allowed states and localities to take on the role of laboratories of democracy, whereby unique policies can be implemented on a much smaller scale than necessary at the federal level. In describing the novel aspects associated with the New Deal, Franklin Roosevelt highlighted this advantage: "*Practically all the things we've done in the federal government are like things Al Smith did as governor in New York*."[1]

While these and other advantages of federalism have been widely touted by supporters of local control over many important public programs, it is not clear that all levels of government have incentives to establish economically efficient policies. For instance, Oates and Schwab (1988) and Wilson (1996) show in a second-best world in which initial distortions already are present that locally determined policies are likely to be suboptimal. This potential suboptimality is highlighted in the "race to the bottom" literature, which suggests that competition for firms and jobs will cause localities to "race to the bottom" in their provisioning of public goods and services (e.g., Zodrow and Mieszkowski, 1986; Wildasin, 1989). In particular, a major fear is that upon one locality relaxing its environmental standards, other jurisdictions would follow, leading to a domino effect whereby local environmental quality is sacrificed ubiquitously. This potential race to the bottom was one of the motivating factors behind the creation of the Environmental Protection Agency in 1968, which resulted in a dominating federal presence in environmental policy.

[1] See Schlesinger (1960, p. 520).

Understanding whether such a dynamic process unfolds in practice is invaluable since budgetary difficulties around the world have led to a heavy reliance on local fiscal decisions. The rich literature to date has taken a first step in this direction by examining horizontal interaction of public policies.[2] The general intuition underlying the theoretical constructs of these strategic interaction models is quite straightforward: since 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 is significantly different from 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; Brueckner and Saavedra, 2001; Fredriksson and Millimet, 2002a; 2002b; Revelli, 2002).[3]

While this particular framework is a useful representation within certain contexts, the possibility of reaching false inferences may not be trivial. 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

[2] For thoughtful reviews see Wilson (1996) and Brueckner (2003). Brueckner (2003) splices the studies into two groups: i) spillover models, which includes yardstick competition models, and ii) resource flow models.

[3] 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). The only exceptions to the uni-dimensionality approach appear to be Heyndels and Vuchelen (1998) who study tax rates and public expenditures in Belgian municipalities, as well as Besley and Rosen (1998), Hayashi and Boadway (2001), and Esteller-Moré and Solé-Ollé (2001) who study the vertical relationship between national (federal) and local (state) government taxation.

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 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 homes.[4]

These anecdotes highlight the fact that competition may occur across *several* policy dimensions, and in particular tax-, environmental-, and infrastructure policies may be used simultaneously. Accordingly, 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 competitors' environmental concessions via tax breaks or promises of expanded infrastructure. In this case, an empirical examination focusing on relative pollution control standards would indicate an orthogonal horizontal relationship between environmental policies and entirely miss the fact that lower effective tax rates are potentially inducing lower provisioning of other types of public goods and services.

In this study, we revisit the issue of horizontal strategic policymaking by developing a simple theoretical model that includes multi-dimensional policies. Motivated by the concerns of voters over environmental quality, public goods, and the attraction of mobile capital, states may act strategically when determining three interrelated policies: (i) state-level taxation, (ii)

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

infrastructure spending, and (iii) pollution control standards.[5] Via this extension, we are able to provide a much richer model of strategic policymaking as we are able to investigate both *intra*- and *inter*-policy strategic reaction functions.

We test the empirical implications of the model by utilizing US state-level panel data over the period 1977-1994. Our empirical results suggest that important own- and cross-policy interactions 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). Overall, our results suggest that uni-dimensional frameworks may present lower bound estimates of the degree of strategic interaction.

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. Section 5 concludes.

II. The Model

We seek to develop a simple model of multi-dimensional strategic interaction between n states (indexed by i, $i = 1,\ldots,n$), building on models of capital competition discussed in Oates and Schwab (1988, 1991, 1996), Brueckner (2003), and Oates (2001). The representative agent's preferences in state i are given by

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

[5] 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).

where c_i is consumption, Q_i is the level of environmental quality, P_i is the amount provided of a "pure" local public good, and $\widetilde{X}_i$ is a vector of state characteristics except income, which contribute to determining preferences. Utility is increasing and concave in c_i and P_i, and decreasing and convex in Q_i. We make no assumptions about the cross-partials.

Following Oates and Schwab (1988), we assume that each identical jurisdiction has firms producing a private good, Z, for the national and international markets with price equal to one. As in Fredriksson et al. (2002), production requires inputs of mobile capital, K, immobile labor, L, waste emissions, Ψ, and infrastructure, S. The production technology of the consumption good exhibits constant returns to scale, is concave and increasing in all inputs, and twice continuously differentiable, $Z_i = F(K_i, L_i, \Psi_i, S_i)$, which can be rewritten as $Z_i = L_i f(k_i, \psi_i, s_i)$, where $k_i = K_i / L_i$ is the capital-labor ratio, $\psi_i = \Psi_i / L_i$, is the emissions-labor ratio, and $s_i = S_i / L_i$ is the infrastructure-labor ratio. Since the number of workers is fixed, ψ determines aggregate emissions for each state. Environmental quality is simply a function of the emissions-labor ratio within a state, $Q(\psi_i)$.

The marginal products of capital, emissions, infrastructure spending, and labor equal $\partial f / \partial k$, $\partial f / \partial \psi$, $\partial f / \partial s$, and $(f - k\partial f / \partial k - \psi \partial f / \partial \psi - s \partial f / \partial s)$, respectively, where $\partial^2 f / \partial k^2 < 0, \partial^2 f / \partial \psi^2 < 0$, $\partial^2 f / \partial s^2 < 0$. We assume $\partial^2 f / \partial k \partial \psi > 0$, $\partial^2 f / \partial k \partial s = \partial^2 f / \partial s \partial k > 0$, and $\partial^2 f / \partial s \partial \psi = 0$.

Each state government finances the provision of the "pure" public good and the infrastructure good by a capital tax t_i is levied on the capital stock located within state i. Assuming prices of both the pure public good and the infrastructure good are equal to unity, the local government's budget constraint equals $K_i t_i = P_i + S_i$. We assume that state i allocates a share γ_i of all local government revenues to the infrastructure good, such that $S_i = K_i t_i \gamma_i$. The remaining share is used for the pure public consumption good, such that $P_i = K_i t_i (1-\gamma_i)$. State i chooses t_i and γ_i.

The capital stock is perfectly mobile between states, but is available in a fixed supply economy-wide, such that $\sum_{i=1}^{n} k_i = n\bar{k}$, where $\bar{k}$ is the average capital-labor ratio nationally. Capital responds to changes in the environmental policy, the capital tax, and the infrastructure level, which affect its endogenous rate of return, r. The rate of return on capital is equalized across jurisdictions, and the equilibrium condition for the capital stock is given by

$$\partial f / \partial k + t_i \gamma_i \partial f / \partial s - t_i = r, \qquad i = 1,\ldots,n \qquad (2)$$

where the capital stock's indirect effect on output via the level of infrastructure spending depends on the capital tax, $\partial s_i / \partial k_i = t_i \gamma_i$. It follows that the capital stock, k_i, and the rate of return, r, can be expressed as functions of all environmental policies, the capital tax, and infrastructure spending share levels, such that

$$k_i = H(\psi_i, \psi_{-i}, t_i, t_{-i}, \gamma_i, \gamma_{-i}), \qquad (3)$$

$$r = G(\psi, t, \gamma), \qquad (4)$$

where ψ, t, and γ are the vectors of environmental policies, capital tax rates, and infrastructure spending share levels, respectively.

Each individual supplies one unit of labor. The wage equals the sum of the marginal product of labor plus the additional output arising from the increase in allowable emissions, $\psi_i \partial f / \partial \psi$. Hence, the wage rate is given by $w(k_i, \psi_i, s_i) = f - k_i \partial f / \partial k - s_i \partial f / \partial s$. Assuming equal ownership of capital, individual consumption is determined by the wage rate plus capital income, $r\bar{k}$, i.e. the individual's budget constraint is given by $c = w + r\bar{k}$.

Given budget restrictions, preferences in (1) can now be restated as

$$U[w(k_i, \psi_i, s_i) + r\bar{k}, Q(\psi_i), K_i t_i (1-\gamma_i); \widetilde{X}_i] =$$

$$U[w(H(\psi_i, \psi_{-i}, t_i, t_{-i}, \gamma_i, \gamma_{-i}), \psi_i, s_i) + G(\psi, t, \gamma)\bar{k}, Q(\psi_i), L_i H(\psi_i, \psi_{-i}, t_i, t_{-i}, \gamma_i, \gamma_{-i}) t_i (1-\gamma_i); \widetilde{X}_i] \equiv$$

$$V(\psi_i, \psi_{-i}, t_i, t_{-i}, \gamma_i, \gamma_{-i}; X_i) \tag{5}$$

where X_i represents all attributes in $\widetilde{X}_i$, as well as income. From (5) it follows that the objective function of state i depends on its own and other states' environmental, capital tax, and infrastructure policies. State i maximizes (5) by setting $\partial V / \partial \psi_i = 0$, $\partial V / \partial t_i = 0$, and $\partial V / \partial \gamma_i = 0$. These first-order conditions implicitly define the equilibrium values of ψ_i, t_i, and γ_i as functions of the values in neighboring states, ψ_{-i}, t_{-i}, and γ_{-i}. Applying the implicit function theorem to the first-order conditions, treating ψ_{-i}, t_{-i}, and γ_{-i} as parameters, yields the following set of reaction functions:

$$\partial \psi_i / \partial \psi_{-i},\ \partial \psi_i / \partial t_{-i},\ \partial \psi_i / \partial \gamma_{-i}, \partial t_i / \partial \psi_{-i}, \partial t_i / \partial t_{-i},\ \partial t_i / \partial \gamma_{-i}, \partial \gamma_i / \partial \psi_{-i}, \partial \gamma_i / \partial t_{-i}, \partial \gamma_i / \partial \gamma_{-i}, \tag{6}$$

which can be shown to be ambiguous in sign due to the presence of ambiguous cross-partials. We assume stability and interior conditions are met. With the exception of knife-edge cases and when no capital competition occurs between states, the vector of reaction functions can take either sign (see also Brueckner and Saavedra, 2001).

The goal of our empirical analysis is to test if the reaction functions have slopes significantly different from zero. As the positions of the reaction functions may depend on the underlying characteristics of each jurisdiction, it is necessary to control for such state-specific attributes in the empirical work below. However, our attention is focused on estimation of the slopes of the reaction functions, rather than on the relative positions of the reaction functions.

III. Empirical Specification

To test whether horizontal strategic interaction occurs across policies, our empirical analysis proceeds by analyzing the temporal and spatial patterns of state-level pollution abatement compliance expenditures, tax rates, and infrastructure investment.[6] To begin the empirical inquiry, consider the "traditional" approach to estimating reaction functions in a uni-dimensional framework:

$$Y_{it} = \phi\Sigma_{48}\, \omega_{ijt} Y_{jt} + x_{it}\beta + \eta_{it} \qquad i = 1\ldots48;\, j \neq i \qquad (7)$$

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 stringency). 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

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

approach allows, say, state *j*'s tax rates to influence state *i*'s pollution regulatory stringency (inter-policy interactions).

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 (7) separately for each policy instrument.[7] A 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, two important issues in the estimation of the multi-dimensional strategic interaction model merit discussion. First, in choosing weights, ω, we follow the procedures of Fredriksson and Millimet (2002a; 2002b) 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 temporal variability in the weights, 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 (7). 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

[7] In the actual estimation, the set of right-hand side control variables is identical for each policy instrument. As a result, joint estimation of the system of policy measures is identical to estimation one equation at a time.

autocorrelation). To circumvent these potential problems, we follow two distinct approaches. First, we instrument for neighboring policies via a two-stage least squares (TSLS) regression approach. While other viable procedures are available (e.g., Brueckner and Saavedra, 2001), it is important to recall that instrumental variables (IV) estimation remains 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. Thus, following Figlio et al. (1999) and Fredriksson and Millimet (2002a; 2002b), 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.[8]

Several diagnostic tests are conducted to assess the reliability and efficiency of the TSLS estimates. First, since the number of instruments exceeds the number of endogenous regressors, we present the results of Sargan overidentification tests for the validity of the instruments. Second, we show the results from Durbin-Wu-Hausman (DWH) tests for exogeneity. In addition, since it is well-known that TSLS estimates based on weak instruments are biased toward the OLS estimates (e.g., Bound et al., 1995), we conduct several further tests. First, as argued in Bound et al. (1995) and Shea (1997), we compute the partial R^2s for the instruments in the first-stage regressions, as well as conduct F-tests for the joint significance of the instrument sets. Second, we conduct the test proposed in Hall et al. (1996) for instrument relevance. The

[8] 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.

test examines whether the smallest sample canonical correlation between the instrument set and the vector of endogenous variables is significantly different from zero. Finally, we compute Stock and Staigner's (1997) measure of the maximum squared bias of the TSLS estimates relative to the OLS estimates ($B_{\max}$ in their notation; equation (3.6), p. 566).

Our second approach to handling the endogeneity issues proceeds by replacing the contemporaneous vector of neighboring policies with its lagged counterpart (see, e.g., Smith, 1997; Fredriksson and Millimet, 2002b). This particular approach 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. Moreover, this approach has the added benefit of flexibility in that it allows lags in strategic interaction, perhaps due to lags in the flow of information across states. 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.

Data Description

A test for the presence of strategic policymaking in a multi-dimensional world requires data across several state policy items. 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; Fredriksson et al., 2002; Keller and Levinson, 2002) for other purposes. Our second policy variable measures governmental state expenditures and is defined as "total general expenditures."[9] The data are reported annually by state in the *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 index, 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 based on the distribution of industries across states. 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.[10]

Besides these major policy variables, we utilize several control variables in the estimation of the augmented (7). In choosing our control variables, we were careful to follow the previous literature 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

[9] 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. States differ significantly in size, and there may be large economies of scale in the provision of public (consumption) goods. Moreover, public good consumption is not assumed to attract capital. Thus, the test of strategic interaction in the public expenditure dimension is implicitly a test of whether the funds are used for goods that raise the productivity of capital. Also, given that we can observe S and not γ_i, since $S_{\mathrm{i}} = \gamma_i t_i K_i$, controlling for t and K, variation in γ_i maps into variation in S_i. Our empirical treatment therefore uses data for S, the level of infrastructure investment.

[10] The index of relative abatement expenditures has also been used in Millimet and Slottje (2002), Keller and Levinson (2002), Fredriksson et al. (2002), and Fredriksson and Millimet (2002a; 2002b).

(http://www.bea.doc.gov). Descriptive information pertaining to each of the variables can be found in Table 1.[11]

IV. 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 results, several points should be emphasized. First, estimated coefficients on the policy instrument regressors in Tables 2 and 3 should be interpreted as elasticities since we model the regressand and policy regressors in natural logarithmic form. Second, results from tests of joint significance of the parameters of interest presented in Tables 2 and 3 suggest that there is considerable evidence supporting the notion of strategic interaction between and within state-level policies across space. Third, our instrument sets pass the Sargan overidentification test in every case, as well as the Hall et al. (1996) test for instrument relevance at the $p < 0.01$ level of significance.[12] Fourth, F-tests reject the null that the instruments are jointly insignificant in the first-stage regressions in every case at the $p < 0.01$ level of significance, and the partial R^2s tend to be quite high.[13] Fifth, the maximum squared bias of the TSLS estimates relative to the OLS estimates is always approximately 0.03,

[11] Most importantly, there is considerable variation in the three policy measures within states over time. For environmental policy, the average state experienced a 7% increase in its value of the stringency index from 1977 to 1994, with a minimum of a 67% decrease and a maximum of a 163% increase. States experienced an 8% increase on average in tax effort over the sample period, with a minimum of a 41% decrease and a maximum of a 48% increase. Finally, the average state experienced a 71% increase in real government expenditures over the sample period, with a minimum of a 23% decrease and a maximum of a 173% increase.

[12] The minimum canonical correlation between the instrument set and vector of endogenous regressors is at least 0.40 in every specification.

[13] In the models with environmental policy as the dependent variable, the F-tests are 5.28, 37.60, and 75.40 for the three first-stage equations (neighboring environmental policy, neighboring tax policy, and neighboring spending) using equal weights. Shea's (1997) partial R^2s are 0.02, 0.15, and 0.19, respectively. The F-tests are 6.56, 24.79, and 74.25 (2.96, 24.65, and 78.44) for the three first-stage equations using income (population) weights; the partial R^2s are 0.03, 0.14, and 0.22 (0.00, 0.01, and 0.03), respectively. In the models with tax and spending policy as the dependent variables, the F-tests are 5.84, 113.05, and 88.70 for the first-stage equations using equal weights. Shea's (1997) partial R^2s are 0.01, 0.05, and 0.07, respectively. The F-tests are 5.57, 116.00, and 85.60 (8.36, 84.95, and

suggesting the reliability of the TSLS estimates. Finally, the DWH tests consistently reject the null of exogeneity at the $p < 0.01$ level of significance.

In terms of the point estimates, the results suggest a fair amount 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), and we reject the null of no cross-policy strategic behavior in the determination of environmental stringency at the $p < 0.10$ level in all three weighting schemes. 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. In the two-year lag model (Table 3), neighboring tax policy has a statistically significant effect on own environmental policy, while in the five-year lag model both neighboring tax and spending policy have a statistically significant impact on own environmental policy. The null of no cross-policy effects is rejected at the $p <$ 0.01 level in all three weighting schemes in both lag models. While many possible explanations exist, these results are consonant with states appealing to firms by conceding lax environmental standards as a reaction to neighboring states enhancing their public good provision and tax relief.

Moving to the tax effort specifications, we find sporadic evidence in favor of the multi-dimensional approach. The contemporaneous models reveal no inter-policy results, but there is evidence that a statistically significant interaction exists between neighboring governmental expenditures and own tax rates in the two-year lag models. Under two of the three weighting schemes, the coefficient estimate is significant at the $p < 0.01$ level ($p < 0.10$ in the third model),

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89.69) for the three first-stage equations using income (population) weights; Shea's (1997) partial R^2s are 0.01, 0.06, and 0.05 (0.04, 0.18, and 0.25), respectively.

suggesting 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% tax rate decrease. In the five-year lag model this result is not evident, yet there is a statistically significant relationship between neighboring abatement levels and own tax rates at the $p < 0.01$ level of significance. Interestingly, the coefficient estimates do not accord well with the preceding results that are largely consistent with a basket of incentive tradeoffs; in this case parameter estimates indicate that increases in neighboring environmental stringency 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 a weak influence of neighboring environmental stringency on own government expenditures, and a strong effect of neighboring tax effort. The null of no contemporaneous cross-policy effects is rejected at the $p < 0.10$ level in all three weighting schemes. While the contemporaneous relationship between neighboring tax effort and own government expenditures is contrary to our conjecture of a basket of incentive tradeoffs, the results from the lagged models are in line with this hypothesis, indicating a highly significant, negative relationship between neighboring tax effort and own government expenditures. The magnitude of the effect is in the range of a 2.5% decrease in expenditures for each 10% increase in neighboring tax effort. The null of no cross-policy effects is rejected at the $p < 0.01$ level in all six lagged specifications.

Prior to discussing the own-policy reaction functions, it is worth pointing out that the matrix of cross-policy effects is not symmetric. For example, while neighboring government expenditures has a contemporaneous effect on own environmental policy, neighboring environmental policy does not have a robust, statistically significant effect on own government expenditures. Similarly, neighboring tax effort has a contemporaneous influence on own spending levels, but neighboring spending does not affect own tax effort. Likewise, in the two-year lag models, neighboring tax effort influences own environmental policy, but neighboring environmental policy does not impact own tax effort. The source behind such asymmetries is beyond the scope of the current paper, but suggests either a ranking of the importance of policies, or may reflect differences in the observability of neighboring policies. For example, neighboring tax effort has a statistically significant effect in virtually every specification estimated, regardless of dependent variable, lag specification, or weighting scheme. This may reflect the importance of tax policy in luring mobile capital, or may reflect the fact that changes in tax effort tend to be highly publicized.

Proceeding to the intra-policy effects, we find that the behavioral patterns found in the between-policy interactions spill over 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 have a strong, positive effect on own tax and spending rates, respectively. In the tax (spending) specifications using the *Equal* weighting schemes, a 10% increase in neighboring tax effort (spending) increases own tax effort (spending) by 9.3% (12.0%). These results also hold by and large when we consider the lagged results in Table 3, although the coefficient magnitudes decrease in value over time. In addition, we also find evidence – consonant with Fredriksson and Millimet (2002b) – of within-policy strategic behavior for environmental policy using the two-year lag model. Overall, these results are largely consonant with the growing literature on uni-dimensional policies.

Sensitivity analysis

Following Fredriksson and Millimet (2002b), we perform two sets of sensitivity analyses to assess the robustness of the results discussed above.[14] 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.[15] In the interest of brevity, empirical results are not presented, but we make them available 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 significant, are 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 (7)

$$Y_{it} = \phi_{0p} I_{it} \Sigma_{48}\, \omega_{ijt} Y_{jtp} + \phi_{1p}(1-I_{it}) \Sigma_{48}\, \omega_{ijt} Y_{jtp} + x_{it}\beta + \eta_{it}; \qquad i = 1\ldots48; j \neq i \qquad (7')$$

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.[16]

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[14] We also attempted a third set of robustness tests related to our measure of tax policy. We replaced the measure of tax effort with (i) total state corporate tax revenue, (ii) total state tax revenue (from all sources), and (iii) total local property tax revenue collected within each state. These results are contained in Tables 4 – 9. While use of these variables supports the conclusion of own- and cross-policy strategic behavior, many of the TSLS diagnostic tests suggest that the results are not overly reliable.

[15] 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.

[16] 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 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.

Again, in the interest of brevity, empirical estimates are not presented, but a noteworthy result is that when comparing ϕ_{0p} and ϕ_{1p}, we rarely reject the null of equality, 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 FDI.

V. Concluding Comments

Whether, and to what extent, strategic interaction of public policies is prevalent amongst governments merits serious consideration. Since many 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 largely considers strategic interaction in a uni-dimensional framework. Our findings are consistent with the notion that reaction functions *between* policies have a nonzero slope. For example, we find that states respond to 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. 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
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

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	0.71	0.06	0.79	1.30	0.26
	(0.10)	(0.25)	(0.87)	(1.60)	(1.60)	(0.57)	(1.53)	(1.76)	(1.81)
ln(Neighboring Tax Effort)	-1.26	-0.83	-2.52	0.93	0.86	0.47	0.98	1.13	0.29
	(-2.54)	(-1.67)	(-1.30)	(2.61)	(2.67)	(5.56)	(2.20)	(2.09)	(2.46)
ln(Neighboring Government Expenditure)	-1.23	-1.21	-1.96	0.29	0.38	-0.03	1.20	1.52	0.76
	(-3.21)	(-3.55)	(-1.75)	(1.09)	(1.29)	(-0.38)	(3.62)	(3.11)	(7.77)
Population	1.75E-08	1.15E-08	1.24E-07	8.74E-09	9.96E-09	-6.36E-09	3.78E-08	5.44E-08	3.49E-08
	(0.71)	(0.47)	(0.86)	(0.66)	(0.70)	(-1.00)	(2.29)	(2.29)	(4.00)
Population Density	-5.21E-04	5.29E-04	-2.45E-03	8.02E-04	5.19E-04	3.47E-04	1.58E-03	1.18E-03	5.76E-04
	(-0.38)	(0.39)	(-0.71)	(1.29)	(0.90)	(1.11)	(2.03)	(1.22)	(1.34)
% Urban	1.65	2.03	0.18	-1.69	-1.82	-1.00	-1.50	-2.60	-0.89
	(1.77)	(2.23)	(0.08)	(-3.04)	(-2.78)	(-4.70)	(-2.16)	(-2.38)	(-3.05)
Unemployment Rate	0.02	0.02	-0.01	-0.01	-0.01	3.10E-03	-0.01	-0.03	-3.12E-03
	(1.96)	(2.11)	(-0.15)	(-1.05)	(-1.12)	(1.33)	(-1.36)	(-1.62)	(-0.97)
Per Capita Income	-1.29E-05	5.21E-05	8.90E-04	-2.09E-04	-2.59E-04	-8.08E-05	-2.00E-04	-4.34E-04	-6.60E-05
	(-0.04)	(0.17)	(1.17)	(-1.11)	(-1.25)	(-1.13)	(-0.85)	(-1.25)	(-0.67)
(Per Capita Income)^2	-1.31E-09	-4.95E-09	-8.07E-08	1.25E-08	1.57E-08	4.04E-09	1.37E-08	2.95E-08	3.42E-09
	(-0.05)	(-0.21)	(-1.17)	(0.91)	(1.05)	(0.74)	(0.80)	(1.18)	(0.45)
(Per Capita Income)^3	5.16E-14	1.40E-13	2.04E-12	-3.29E-13	-4.02E-13	-1.19E-13	-2.76E-13	-6.58E-13	4.70E-15
	(0.08)	(0.24)	(1.18)	(-0.96)	(-1.08)	(-0.87)	(-0.64)	(-1.06)	(0.02)
% Elderly	2.54	2.74	-0.97	0.05	-0.07	0.26	-2.03	-2.45	-1.60
	(1.42)	(1.53)	(-0.24)	(0.06)	(-0.09)	(0.58)	(-2.23)	(-1.90)	(-2.57)
% Young	1.64	1.27	0.62	-0.26	-0.5	1.13	-2.21	-3.48	-0.82
	(1.12)	(0.87)	(0.33)	(-0.29)	(-0.47)	(0.30)	(-1.96)	(-1.96)	(-1.96)
State Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Joint Significance: All Neighboring Policies	[p=0.00]	[p=0.00]	[p=0.08]	[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=0.10]	[p=0.21]	[p=0.26]	[p=0.80]	[p=0.03]	[p=0.10]	[p=0.04]
Sargan's Overidentification Test	χ^2 = 5.29	χ^2 = 3.99	χ^2 = 5.80	χ^2 = 2.71	χ^2 = 0.80	χ^2 = 4.05	χ^2 = 5.44	χ^2 = 1.62	χ^2 = 6.02
	[p=0.15]	[p=0.26]	[p=0.12]	[p=0.44]	[p=0.85]	[p=0.26]	[p=0.14]	[p=0.66]	[p=0.11]
Hall et al. (1996) Test for Instrument Relevance	ρ = 0.43	ρ = 0.43	ρ = 0.43	ρ = 0.41	ρ = 0.42	ρ = 0.40	ρ = 0.41	ρ = 0.42	ρ = 0.40
	[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]
Staiger-Stock (1997) Measure of Maximum Relative Bias	B_{max}=0.03	B_{max}=0.03	B_{max}=0.03	B_{max}=0.03	B_{max}=0.03	B_{max}=0.03	B_{max}=0.03	B_{max}=0.03	B_{max}=0.03
Durbin-Wu-Hausman Test for Exogeneity	χ^2 = 38.48	χ^2 = 45.84	χ^2 = 36.19	χ^2 = 9.72	χ^2 = 13.42	χ^2 = 0.87	χ^2 = 63.14	χ^2 = 78.08	χ^2 = 56.62
	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.02]	[p=0.00]	[p=0.83]	[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. 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	0.17	0.17	0.22	-0.01	-0.01	0.01	0.03	0.03	0.03
Abatement)	(2.15)	(2.15)	(2.48)	(-0.27)	(-0.45)	(0.65)	(1.20)	(1.21)	(1.41)
ln(Neighboring	-0.84	-0.84	-0.61	0.34	0.34	0.37	-0.16	-0.18	-0.25
Tax Effort)	(-3.82)	(-3.98)	(-3.64)	(6.05)	(6.18)	(8.61)	(-2.54)	(-3.05)	(-5.36)
ln(Neighboring	0.06	0.01	0.13	-0.14	-0.13	-0.07	0.26	0.25	0.18
Government Expenditure)	(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	-0.05	-0.07	-0.24	-0.11	-0.11	-0.07	-4.32E-03	-3.15E-03	0.01
Abatement)	(-0.59)	(-0.72)	(-2.29)	(-5.32)	(-5.19)	(-2.93)	(-0.19)	(-0.14)	(0.35)
ln(Neighboring	-1.07	-0.95	-0.95	0.14	0.14	0.19	-0.23	-0.24	-0.26
Tax Effort)	(-4.30)	(-4.03)	(-4.68)	(2.51)	(2.51)	(4.00)	(-3.61)	(-3.98)	(-5.02)
ln(Neighboring	-0.54	-0.39	-0.41	-0.02	-0.03	-0.01	0.12	0.10	0.04
Government Expenditure)	(-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 2. Number of observations is 768 in the two-year lag specifications, 624 in the five-year specifications. T-statistics in parentheses.

Table 4. Sensitivity Analysis I: State Corporate Tax Receipts.

Var./Depdt. Var.	ln(Own Abatement)			ln(Own Taxes)			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)	1.08	0.24	-0.32	0.33	-0.60	-0.28	-0.44	-0.59	0.22
	(1.72)	(0.38)	(-0.19)	(0.59)	(-0.73)	(-0.63)	(-2.38)	(-1.63)	(1.47)
ln(Neighboring Taxes)	0.71	-0.01	0.37	-0.67	0.65	0.90	0.30	0.66	-0.28
	(1.01)	(-0.01)	-0.29	(-1.08)	(0.71)	(1.74)	(1.49)	(1.59)	(-1.77)
ln(Neighboring Government Expenditure)	-1.03	-0.96	-0.95	-0.35	-1.68	-1.49	0.22	-0.26	0.88
	(-2.01)	(-1.82)	(-0.72)	(-0.54)	(-1.49)	(-2.85)	(0.91)	(0.55)	(5.39)
State Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Joint Significance: All Neighboring Policies	[p=0.01]	[p=0.00]	[p=0.02]	[p=0.02]	[p=0.07]	[p=0.02]	[p=0.00]	[p=0.00]	[p=0.00]
Joint Significance: Neighboring Cross-Policies	[p=0.10]	[p=0.01]	[p=0.06]	[p=0.40]	[p=0.21]	[p=0.02]	[p=0.06]	[p=0.24]	[p=0.17]
Sargan's Overidentification Test	χ^2 = 8.63	χ^2 = 6.72	χ^2 = 14.34	χ^2 = 11.75	χ^2 = 8.55	χ^2 = 3.12	χ^2 = 14.94	χ^2 = 9.10	χ^2 = 8.55
	[p=0.03]	[p=0.08]	[p=0.00]	[p=0.01]	[p=0.04]	[p=0.37]	[p=0.00]	[p=0.03]	[p=0.04]
Hall et al. (1996) Test for Instrument Relevance	ρ = 0.34	ρ = 0.32	ρ = 0.21	ρ = 0.32	ρ = 0.31	ρ = 0.17	ρ = 0.32	ρ = 0.31	ρ = 0.17
	[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]
Staiger-Stock (1997) Measure of Maximum Relative Bias	B_{max}=0.05	B_{max}=0.05	B_{max}=0.12	B_{max}=0.05	B_{max}=0.05	B_{max}=0.13	B_{max}=0.05	B_{max}=0.05	B_{max}=0.13
Durbin-Wu-Hausman Test for Exogeneity	χ^2 = 38.24	χ^2 = 45.84	χ^2 = 27.52	χ^2 = 1.98	χ^2 = 13.42	χ^2 = 6.86	χ^2 = 35.61	χ^2 = 46.11	χ^2 = 59.25
	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.58]	[p=0.00]	[p=0.08]	[p=0.00]	[p=0.00]	[p=0.00]
Observations	864	864	864	864	864	864	864	864	864

NOTES: See Table 2.

Table 5. Sensitivity Analysis I: State Corporate Tax Receipts.

Var./Depdt. Var.	ln(Own Abatement)			ln(Own Taxes)			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	0.19	0.19	0.26	0.12	0.13	-0.04	0.03	0.03	0.05
Abatement)	(2.49)	(2.47)	(2.98)	(1.38)	(1.44)	(-0.40)	(1.38)	(1.42)	(2.11)
ln(Neighboring	0.11	0.09	0.05	-0.36	-0.33	-0.25	0.00	-2.90E-04	0.01
Taxes)	(1.29)	(1.02)	(0.62)	(-3.51)	(-3.27)	(-2.66)	(0.06)	(-0.01)	(0.43)
ln(Neighboring	0.29	0.22	0.28	-0.84	-0.68	-0.81	0.31	0.31	0.25
Government Expenditure)	(1.53)	(1.22)	(1.67)	(-3.90)	(-3.29)	(-4.15)	(5.99)	(6.29)	(5.27)
Joint Significance: All Neighboring Policies	[p=0.01]	[p=0.02]	[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=0.06]	[p=0.15]	[p=0.12]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.38]	[p=0.36]	[p=0.10]
Five-Year Lag:									
ln(Neighboring	-0.05	-0.07	-0.20	0.22	0.24	0.09	1.35E-03	1.02E-03	0.02
Abatement)	(-0.53)	(-0.70)	(-1.88)	(2.17)	(2.38)	(0.78)	(0.06)	(0.04)	(0.58)
ln(Neighboring	-0.02	-0.01	-0.01	-0.07	-0.05	-0.05	0.09	0.09	0.10
Tax Effort)	(-0.21)	(-0.13)	(-0.14)	(-0.64)	(-0.45)	(-0.54)	(3.57)	(3.65)	(4.37)
ln(Neighboring	-0.02	8.76E-04	0.02	-0.95	-0.88	-1.08	0.11	0.09	0.04
Government Expenditure)	(-0.09)	(3.73E-03)	(0.10)	(-3.44)	(-3.36)	(-4.42)	(1.82)	(1.55)	(0.73)
Joint Significance: All Neighboring Policies	[p=0.95]	[p=0.92]	[p=0.31]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]
Joint Significance: Neighboring Cross-Policies	[p=0.96]	[p=0.99]	[p=0.99]	[p=0.00]	[p=0.00]	[p=0.01]	[p=0.00]	[p=0.00]	[p=0.00]

NOTES: See Table 3.

Table 6. Sensitivity Analysis II: State Total Tax Receipts.

Var./Depdt. Var.	ln(Own Abatement)			ln(Own Taxes)			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)	2.41	1.70	-0.25	-0.70	-0.34	0.38	-0.26	-0.03	0.24
	(2.26)	(1.77)	(-0.32)	(-3.65)	(-1.64)	(1.28)	(-2.05)	(-0.17)	(1.34)
ln(Neighboring Taxes)	4.66	3.97	1.15	0.88	-0.04	-2.93	-0.08	-0.42	-1.31
	(2.08)	(1.82)	(0.98)	(1.45)	(-0.07)	(-1.96)	(-0.21)	(-0.90)	(-1.48)
ln(Neighboring Government Expenditure)	-4.33	-3.84	-1.65	-0.57	0.43	3.11	0.62	1.02	1.78
	(-2,36)	(-2.35)	(-1.46)	(-0.92)	(0.71)	(2.36)	(1.50)	(2.08)	(2.28)
State Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Joint Significance: All Neighboring Policies	[p=0.02]	[p=0.00]	[p=0.01]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]
Joint Significance: Neighboring Cross-Policies	[p=0.05]	[p=0.01]	[p=0.04]	[p=0.00]	[p=0.01]	[p=0.06]	[p=0.08]	[p=0.42]	[p=0.28]
Sargan's Overidentification Test	$\chi^2 = 0.59$	$\chi^2 = 0.63$	$\chi^2 = 13.59$	$\chi^2 = 21.62$	$\chi^2 = 53.50$	$\chi^2 = 7.60$	$\chi^2 = 24.90$	$\chi^2 = 30.95$	$\chi^2 = 7.33$
	[p=0.90]	[p=0.89]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.06]	[p=0.00]	[p=0.00]	[p=0.06]
Hall et al. (1996) Test for Instrument Relevance	$\rho = 0.37$	$\rho = 0.38$	$\rho = 0.37$	$\rho = 0.43$	$\rho = 0.43$	$\rho = 0.45$	$\rho = 0.43$	$\rho = 0.43$	$\rho = 0.45$
	[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]
Staiger-Stock (1997) Measure of Maximum Relative Bias	B_{max}=0.04	B_{max}=0.04	B_{max}=0.04	B_{max}=0.03	B_{max}=0.03	B_{max}=0.03	B_{max}=0.03	B_{max}=0.03	B_{max}=0.03
Durbin-Wu-Hausman Test for Exogeneity	$\chi^2 = 47.00$	$\chi^2 = 50.23$	$\chi^2 = 26.96$	$\chi^2 = 34.64$	$\chi^2 = 24.49$	$\chi^2 = 50.14$	$\chi^2 = 30.02$	$\chi^2 = 35.46$	$\chi^2 = 56.75$
	[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]
Observations	864	864	864	864	864	864	864	864	864

NOTES: See Table 2.

Table 7. Sensitivity Analysis II: State Total Tax Receipts.

Var./Depdt. Var.	ln(Own Abatement)			ln(Own Taxes)			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	0.20	0.20	0.26	3.83E-03	4.80E-03	-0.01	0.03	0.03	0.05
Abatement)	(2.55)	(2.58)	(3.00)	(0.14)	(0.17)	(-0.41)	(1.49)	(1.55)	(2.13)
ln(Neighboring	0.48	0.48	0.26	-0.12	-0.12	-0.11	0.11	0.10	0.08
Taxes)	(1.78)	(1.86)	(1.05)	(-1.22)	(-1.34)	(-1.26)	(1.41)	(1.41)	(1.17)
ln(Neighboring	-0.04	-0.13	0.09	0.19	0.22	0.18	0.23	0.22	0.19
Government Expenditure)	(-0.16)	(-0.49)	(1.35)	(1.85)	(2.29)	(1.96)	(2.78)	(2.90)	(2.51)
Joint Significance: All Neighboring Policies	[p=0.00]	[p=0.01]	[p=0.00]	[p=0.30]	[p=0.11]	[p=0.25]	[p=0.00]	[p=0.00]	[p=0.00]
Joint Significance: Neighboring Cross-Policies	[p=0.03]	[p=0.05]	[p=0.08]	[p=0.17]	[p=0.07]	[p=0.14]	[p=0.14]	[p=0.13]	[p=0.06]
Five-Year Lag:									
ln(Neighboring	-0.03	-0.05	-0.20	0.01	0.02	0.04	4.53E-04	3.99E-04	0.02
Abatement)	(-0.37)	(-0.52)	(-1.86)	(0.29)	(0.59)	(1.07)	(0.02)	(0.02)	(0.76)
ln(Neighboring	0.65	0.64	0.47	9.41E-04	-0.04	0.03	0.17	0.12	0.09
Taxes)	(2.15)	(2.22)	(1.75)	(0.01)	(-0.38)	(0.36)	(2.26)	(1.72)	(1.29)
ln(Neighboring	-0.59	-0.56	-0.39	0.17	0.22	0.04	0.08	0.09	0.07
Government Expenditure)	(-1.78)	(-1.76)	(-1.30)	(1.52)	(2.12)	(0.40)	(0.92)	(1.16)	(0.97)
Joint Significance: All Neighboring Policies	[p=0.18]	[p=0.14]	[p=0.09]	[p=0.13]	[p=0.03]	[p=0.50]	[p=0.00]	[p=0.00]	[p=0.01]
Joint Significance: Neighboring Cross-Policies	[p=0.10]	[p=0.09]	[p=0.22]	[p=0.29]	[p=0.08]	[p=0.51]	[p=0.08]	[p=0.00]	[p=0.33]

NOTES: See Table 3.

Table 8. Sensitivity Analysis III: Total Local Property Tax Receipts.

Var./Depdt. Var.	ln(Own Abatement)			ln(Own Taxes)			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)	1.89	-1.32	-1.51	-0.32	-0.23	-1.28	-0.33	-0.29	0.42
	(1.90)	(-0.64)	(-1.83)	(-1.50)	(-0.97)	(-2.42)	(-2.16)	(-1.65)	(1.73)
ln(Neighboring Taxes)	-2.24	2.27	0.96	0.35	0.37	-0.47	-0.10	-0.28	0.28
	(-1.63)	(0.80)	(1.94)	(1.65)	(1.61	(-1.27)	(-0.70)	(-1.65)	(1.65)
ln(Neighboring Government Expenditure)	2.25	-3.79	-1.36	0.31	0.48	1.41	0.62	0.74	0.35
	(1.23)	(-1.07)	(-2.67)	(1.48)	(2.64)	(3.33)	(4.16)	(5.48)	(1.81)
State Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Joint Significance: All Neighboring Policies	[p=0.04]	[p=0.07]	[p=0.02]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]
Joint Significance: Neighboring Cross-Policies	[p=0.10]	[p=0.12]	[p=0.03]	[p=0.18]	[p=0.03]	[p=0.00]	[p=0.08]	[p=0.18]	[p=0.21]
Sargan's Overidentification Test	$\chi^2 = 2.36$	$\chi^2 = 1.86$	$\chi^2 = 5.28$	$\chi^2 = 23.34$	$\chi^2 = 21.94$	$\chi^2 = 5.51$	$\chi^2 = 22.21$	$\chi^2 = 22.56$	$\chi^2 = 8.84$
	[p=0.50]	[p=0.60]	[p=0.15]	[p=0.00]	[p=0.00]	[p=0.14]	[p=0.00]	[p=0.00]	[p=0.03]
Hall et al. (1996) Test for Instrument Relevance	$\rho = 0.18$	$\rho = 0.19$	$\rho = 0.27$	$\rho = 0.23$	$\rho = 0.23$	$\rho = 0.30$	$\rho = 0.23$	$\rho = 0.23$	$\rho = 0.30$
	[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]
Staiger-Stock (1997) Measure of Maximum Relative Bias	B_{max}=0.16	B_{max}=0.14	B_{max}=0.08	B_{max}=0.09	B_{max}=0.09	B_{max}=0.06	B_{max}=0.09	B_{max}=0.09	B_{max}=0.06
Durbin-Wu-Hausman Test for Exogeneity	$\chi^2 = 41.03$	$\chi^2 = 45.28$	$\chi^2 = 31.93$	$\chi^2 = 19.54$	$\chi^2 = 31.50$	$\chi^2 = 48.35$	$\chi^2 = 33.14$	$\chi^2 = 39.00$	$\chi^2 = 59.55$
	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.83]	[p=0.00]	[p=0.00]	[p=0.00]
Observations	864	864	864	864	864	864	864	864	864

NOTES: See Table 2.

Table 9. Sensitivity Analysis III: Total Local Property Tax Receipts.

Var./Depdt. Var.	ln(Own Abatement)			ln(Own Taxes)			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	0.17	0.17	0.25	-0.03	-0.04	3.25E-03	0.03	0.03	0.05
Abatement)	(2.23)	(2.23)	(2.88)	(-0.90)	(-1.23)	(0.09)	(1.24)	(1.30)	(1.99)
ln(Neighboring	-0.21	-0.19	-0.17	0.21	0.23	0.13	-0.04	-0.04	-0.06
Taxes)	(-1.96)	(-1.82)	(-1.90)	(4.68)	(5.18)	(3.27)	(-1.50)	(-1.21)	(-2.48)
ln(Neighboring	0.41	0.32	0.38	0.36	0.40	0.32	0.33	0.32	0.28
Government Expenditure)	(2.29)	(1.92)	(2.34)	(4.66)	(5.61)	(4.60)	(6.46)	(6.79)	(6.14)
Joint Significance: All Neighboring Policies	[p=0.00]	[p=0.01]	[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=0.02]	[p=0.04]	[p=0.02]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.12]	[p=0.17]	[p=0.01]
Five-Year Lag:									
ln(Neighboring	-0.06	-0.08	-0.21	-0.12	-0.14	-0.07	-0.01	-0.01	0.02
Abatement)	(-0.69)	(-0.87)	(-1.93)	(-3.33)	(-3.70)	(-1.55)	(-0.32)	(-0.34)	(0.67)
ln(Neighboring	-0.29	-0.25	-0.21	-0.10	-0.07	-0.14	-0.07	-0.08	0.09
Taxes)	(-2.25)	(-2.03)	(-2.03)	(-1.92)	(-1.40)	(-3.38)	(-2.32)	(-2.45)	(-3.44)
ln(Neighboring	-0.07	-0.04	0.02	0.52	0.55	0.39	0.21	0.19	0.15
Government Expenditure)	(-0.34)	(-0.18)	(0.10)	(5.91)	(6.92)	(4.93)	(3.92)	(3.78)	(3.11)
Joint Significance: All Neighboring Policies	[p=0.15]	[p=0.20]	[p=0.05]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]
Joint Significance: Neighboring Cross-Policies	[p=0.08]	[p=0.13]	[p=0.13]	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.07]	[p=0.05]	[p=0.00]

NOTES: See Table 3.