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GMM ESTIMATION OF FISCAL POLICY INTERDEPENDENCE ACROSS STATES

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1. Introduction

Estimates of state and local fiscal behavior exhibit three empirical regularities: states' budgetary decisions are influenced by the tax and expenditure policies of their neighbors,[1] demographic variations affect spending patterns, and federal grants have a larger than expected impact on state spending. Theoretical results on these issues have long been established in the literature; however, there is continued interest in estimating these models.

The context for the first finding, that state and local governments' behavior exerts an influence on neighboring jurisdictions, is the early theoretical work that assumes the existence of spillovers. An example is Pauly (1973) who suggests that voters in one state might have an interest in welfare payments made to poor residents of different states.[2] More recently, Besley and Case (1995) make a convincing argument that tax-setting involves comparisons across jurisdictions for both voters and incumbent politicians. Case, Rosen, and Hines (1993) (CRH) were the first to incorporate neighbor effects into an empirical model of state spending by specifying a first-order autoregressive spatial model with first-order autoregressive disturbances. Using panel data on the states from 1970 through 1985, they find that the level of spending in neighboring states exerts a strong positive influence on a state's per-capita expenditures. Subsequently, many empirical studies have found evidence of spatial effects in models of spending and of tax setting, see for example, Hernandez-Murillo (2004) (HM) and Edmiston and Turnbull (2003).

[1] The term neighbor does not necessarily refer to geographic closeness. Two states are defined as neighbors if they have similar economic or demographic characteristics.
[2] See also Gramlich and Laren (1984).

The issues of demographic influences and the effects of grants-in-aid arise in the context of the median voter model. In these models, expenditures of state and local governments approximate the choices of the utility-maximizing median voter. These choices will depend on, among other things, factors that determine consumer preferences. Empirical models have typically used demographic variables reflecting the age, education and ethnic distributions of the population to capture variations in preferences. Studies with these types of variables include Poterba (1997), Merrifield (2000), Sjoquist, Walker and Wallace (2004). While the studies obviously vary, the results generally indicate that demographic variables exert a substantial influence on spending patterns.

The effect of grants-in-aid on spending has long presented an empirical puzzle. A straightforward median voter model would yield the result that the median voter should respond to a change in the amount of grants-in-aid as she would to a change in individual income or transfers. Yet empirical models estimate the effect of grants to be much larger than the effect of income.[3] Both theoretical and econometric explanations for this 'flypaper' effect exist. Roemer and Silvestre (2002) find that extensions of the single-policy-variable, median voter model to one with multiple policies and party competition result in computed equilibria that are consistent with estimates in the empirical literature. Knight (2002) suggests that because political processes determine both federal grants and state spending, these variables are correlated with unobservable components of voters' preferences, hence grants will be endogenous in a spending equation. He finds that the endogeneity-corrected estimates do not display the flypaper effect found in the uncorrected estimates.

[3] See Hines and Thaler (1995) for a review of this empirical literature.

In this paper, we revisit the classic study of CRH with new data and new methodology to provide updated empirical evidence on each of these three issues. While our results suggest that neighbor effects remain significant and that federal grants continue to exert a large influence on spending decisions, we find mixed evidence on the influence of demographic variables. Most interestingly, we find dramatic evidence of structural change in the demand for state public expenditures in the 1990's.

Our estimation strategy differs from previous authors who model neighbor effects as parametric first order processes. CRH specify a first-order autoregressive spatial process to capture the direct effect of neighbor state spending and use a first-order autoregressive process for the disturbances. As was common practice at the time, they estimated their model with maximum likelihood. HM uses the same parametric spatial model for tax-setting, but employs a computationally simpler and more robust estimator; he estimates the model with a generalized method of moments estimator suggested by Kelejian and Prucha (1998). Our estimation strategy follows what has become a more conventional empirical practice; that is, we specify a fully parametric conditional mean function displaying first-order spatial dependence, but we combine this with semi-parametric estimation of the moment conditions' covariance. Thus, we allow for more general cross-sectional dependence in the model's unobservables. Our inferences are therefore robust to more general spatial error structures than the first-order model imposed by CRH or HM.

Our estimation results indicate that the structure of demand for state and local public goods changed dramatically in the years following the Tax Reform Act of 1986. It is particularly interesting to observe the diminished impact of the demographic variables in the later years. For example, in the years from 1977 through 1988, a one percentage point increase in the portion of the population that is black was associated with a $147 increase in real per capita public expenditures, whereas in the later period, 1989 – 1999, the same change in the proportion black lowered expenditures by $20.57. Similar, though less dramatic, changes were found for the population proportion that is school-aged and the population proportion that is greater than 65 years old. These results suggest that preferences for public expenditures have undergone substantive change in recent years, or perhaps that the demographic variables are no longer adequate to capture the variation in preferences across states.

Our results also suggest that the estimated impact of neighboring states' expenditures is very sensitive to whether a proxy for tax price is included in the model. When a measure of tax price is included, the copy-cat effect falls by more than half, although it remains statistically significantly different from zero.

Finally, we find that the effect of federal grants-in-aid is quite large. We reject the null that grants are exogenous in only two of six models. For that case, the estimation allows for the endogeneity of grants, but the estimated coefficient is still larger than standard theory would predict.

In the next section, we outline the empirical model. We then describe the estimation strategy and data. A discussion of the results follows.

2. The Model

Much empirical work on the determinants of the demand for public goods has been based on the theoretical underpinning of median – voter models; see Bergstrom and Goodman

(1973) for the classic treatment.[4] In these models, expenditures approximate the choices of the utility-maximizing median voter and so depend on income and other revenue sources such as government grants, the tax prices of public goods, and factors that determine consumer preferences.

We will not review the standard median voter model here, instead we focus on two areas where our empirical work differs from what was done in previous studies. The median voter model yields a demand equation for public goods that depends on the tax price of those goods. Yet some empirical work, including CRH, does not include a measure for this variable.[5] Part of the difficulty in including this variable involves finding a reasonable measure for it. Once a measure is found, another difficulty arises because we would anticipate that prices and quantities are jointly determined, hence the variable is endogenous.

Despite these difficulties, we prefer to include a measure of tax price and adopt a strategy outlined in Tresch (2002).[6] He suggests that an approximation for the tax price of public goods can be obtained in a median voter model that assumes that governments use property taxes to collect revenues. If one further recognizes that most state and local governments must balance their budgets, then the tax price of public goods is proportional to the supply price of the public goods. More assumptions are necessary to

[4] Other theoretical frameworks have been discussed, see for example, Brennan and Buchanan (1977) or Hettich and Winer (1988).

[5] CRH acknowledge the issue, of course, saying that such a measure is likely to be endogenous; the estimation method they use would require that outside instruments be found to allow for the endogeneity of price. They refer to their equation as a reduced form and add that their results were not substantially affected by the inclusion of a proxy.

[6] A reader suggested the ratio of median household income to total state income as an alternative measure of tax price. Due to the unavailability of median income in non Census years, we did not pursue this strategy.

find a measure of the supply price. If we suppose: that there are constant returns to scale in the production of public goods, that the price of capital does not vary across states, and third and most heroically, that the production of public goods is least cost efficient, one obtains the result that the supply price is proportional to the marginal cost of producing public goods. Because we have assumed no variation in capital prices across states, the supply price is then proportional to the wage of government workers.

This tax price measure is likely to be correlated with the error term not only due to the joint determination of prices and quantities, but also because the wage variable is a proxy. Hence, we have concerns about measurement error. Finding appropriate outside instruments would no doubt be quite challenging. Fortunately, the estimation strategy that we adopt yields instruments that turn out to work reasonably well. This is described in the following section.

The second area where our model differs from previous work is in the specification of the spatial structure. CRH provide a very strong argument for enriching the empirical specification of earlier models to allow for the impact of expenditures of neighboring states, noting at the time of their study that while an extensive theoretical literature pointed to the existence of spillovers, little empirical testing had occurred. Their empirical approach adopts the Cliff-Ord (1981) type of first-order spatial model that is widely used in the regional and environmental literatures. This model assumes that the econometric investigator knows the pattern of interrelations among the units of observation. Thus, the expenditures of state i in time period t depend on a weighted average of its neighbors' expenditures, and the weights are assumed to be known *a priori*. Because random shocks to expenditures seem likely to be correlated in neighbor states, CRH include a first order spatial error process that uses the same arbitrary weight structure.

We are persuaded by the arguments that states look beyond their own boundaries when determining the levels of provision of public goods and services. It also seems entirely plausible to expect that random shocks do not stop at state borders, but instead exhibit some cross-sectional dependence. It is possible to retain these attractive model features, yet increase somewhat the generality of the model. In the vast majority of empirical studies, researchers make strong assumptions about functional form and the choice of explanatory variables to express the conditional mean of the dependent variable. However, it has become much less common to specify parametric forms for a model's stochastic structure. This is because the consequences of misspecification are severe. Inconsistent estimates of a model's variance/covariance matrix will yield invalid inferences. The widespread use of robust covariance matrix estimators (HAC estimators) suggests that researchers prefer to remain agnostic about the form of the unobservable error terms.

Based on the above discussion, the empirical model estimated here can be written:

$$y_{it} = f(x_{it}, y_{-i,t}, tp_{it}, \theta) + u_{it},$$

where the dependent variable is per capita expenditures of state and local governments in state i at time t, x_{it} includes income, grants, demographic variables, population density, $y_{-i,t}$ is a vector representing expenditures of neighboring states, tp_{it} is the tax price of public goods, θ is a vector of parameters, and u_{it} is a random disturbance with the property $E(u_{it}u_{jt}) \neq 0$.

3. Estimation

Our empirical model for a panel of states can be written as

$$
(1) \qquad \begin{aligned} y_t &= X_t\beta + tp_t\alpha + W_n y_t \rho + f_t + h_t + u_t, \\ u_t &= D_n \varepsilon_t, \qquad\qquad t = 1,2,...T, \end{aligned}
$$

where y_t is the vector of observations on per capita expenditures of the 48 contiguous states, X_t contains observations on *k* variables (state demographics and federal grants) that are treated as exogenous, tp_t is the tax price measured by the wage of state workers, f_t is the vector of state (fixed) effects and h_t is the year effect. The known weight matrix W_n has zeros on the diagonal and row- and column-sums normalized to be one.[7] We are interested in estimates of the unknown parameters $(\beta', \alpha, \rho)' = \theta$, a $(k+2)\times 1$ vector. The fixed state effects and year effects are eliminated prior to estimation.

We allow the structural disturbances to be correlated across states but, unlike CRH or HM, we do not specify a first-order spatial error structure. Instead, we assume only that the $n \times n$ matrix D_n is nonsingular and the $n \times 1$ error vector ε_t is independent and identically distributed with covariance matrix I_n. These assumptions imply that each state's structural disturbance is a weighted sum of all state's disturbances; however, the weights are unknown. With the functional form of D_n unspecified, we cannot estimate the $n \times n$ disturbance covariance matrix since *n* (the number of states) is greater than *T* (the number of years) and thus cannot use a fully efficient estimator (such as maximum likelihood or a best IV estimator). Instead, we estimate θ by using an efficient GMM method and construct a robust covariance estimator for inference.

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[7] See Kelejian and Prucha (1998) for a more complete discussion of the characteristics of the W matrix.

Kelejian and Prucha (1998) and Lee (2003) suggest a generalized spatial two-stage-least-squares (GS2SLS) procedure for estimating the parameter vector θ in a cross-sectional setting (T = 1) when the error is a first-order spatially correlated process with $D_n = (I_n - \rho W_n)^{-1}$. HM (2004) applies Kelejian and Prucha's GS2SLS method to a panel data model (T > 1) to estimate states' tax setting behavior. The limiting properties of the estimator in this context are thus based on the assumption that both n and T approach infinity.

Like HM, we also allow both the time and cross-section units' dimensions to grow without bound. Our model specification, however, differs in two important respects from the standard first-order spatial autocorrelation model used by previous authors. We allow the error process to follow a more general spatial process, defining D_n from $E(u_t u_t') = \Omega_n = D_n D_n'$. We also allow a subset of the explanatory variables to be endogenous. Therefore, we modify the GS2SLS estimation method to account for these differences in model structure.

We estimate θ by an efficient GMM method that identifies θ by the moment conditions, $E(H_t' u_t) = 0$, where H_t is the instrument matrix suggested by Kelejian and Prucha.[8] The matrix H_t is defined as the p linearly independent columns of

[8] Although Lee (2003) shows that Kelejian and Prucha's instruments do not provide the most efficient IV estimator in the 1st order spatial autoregressive model with 1st order spatial autoregressive disturbances, Monte Carlo results of Das, Kelejian and Prucha (2001) suggest that these instruments approximate closely the optimal instruments. Hernandez-Murillo (2004) also chooses to use the instrument set suggested by Kelejian and Prucha (1998).

$(X_t, WX_t, W^2 X_t)$.[9] Note that the GMM estimator is based on *p* sample moments that are averaged over both *n* and *T*, $\sum_{t=1}^{T}\sum_{i=1}^{n} H_{ti}u_{ti} / nT$.[10]

To define the resulting GMM estimator, it is convenient to write model (1) stacked by observation.

$$
\begin{aligned}
&y = (X, TP, (I_T \otimes W_n)y)\,\theta + u = Z\theta + u, \\
&u = (I_T \otimes D_n)\varepsilon
\end{aligned} \tag{2}
$$

The efficient GMM estimator can then be written as,

$$\hat{\theta} = (Z'HS^{-1}H'Z)^{-1}Z'HS^{-1}H'y,$$

where $S = \lim_{n\to\infty, T\to\infty} \sum_{t=1}^{T}\left(\frac{H_t' D_n D_n' H_t}{nT}\right) = \lim_{T\to\infty}\sum_{t=1}^{T}\frac{Q_{AA,t}}{T} = \overline{Q}_{AA}$. The estimator's limiting distribution is,

$$\sqrt{nT}(\hat{\theta} - \theta) \xrightarrow{d} N(0, V)$$

where $V = (\overline{Q}_{ZH} S^{-1} \overline{Q}_{HZ})^{-1}$ and $\operatorname*{plim}_{n\to\infty, T\to\infty}\sum_{t=1}^{T}\frac{Z_t' H_t}{nT} = \lim_{T\to\infty} Q_{ZH,t}/T = \overline{Q}_{ZH}$.

We estimate the covariance matrix in the obvious way by replacing the probability limits with their sample counterparts and replacing $D_n D_n'$ with $\sum_{t=1}^{T} \hat{u}_t \hat{u}_t' / T$, where $\hat{u}_t = y_t - Z_t\hat{\theta}$.[11]

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[9] See McGarvey and Walker (2004) for sufficient conditions for consistency and asymptotic normality of the GMM parameter estimator in this model.

[10] Driskoll and Kraay's (1998) consistent covariance estimator for spatially dependent panel data is also based on sample moment conditions where the asymptotics allow both n and T to approach infinity.

[11] This covariance estimator is of the general form suggested by Driscoll and Kraay (1998) for spatially dependent errors in panel data models.

4. Data

The variable of interest is annual expenditures by state and local governments (net of expenditures for insurance, interest, and state-run liquor and utility firms) over the period 1977 – 1999. These data come from the U.S. Bureau of the Census, Government Finance Series, General Revenue Tables. In order to obtain real, per capita expenditure figures, we use population figures from the Bureau of the Census and a state deflator from the Bureau of Economic Analysis.

We use real per capita income, obtained from U.S. Census publications; the square of this variable is also included. The grants variable represents total federal grants to state and local governments; it is also computed in real per capita terms. The income and grants variables measure state and local governments' resources. Population density is measured as the ratio of state population to land area in square miles.

The preference variables are the proportion of the states' populations that are over 65 years of age, the proportion that is school-age (between 6 and 17), and the proportion that is African-American. These variables are also obtained from U.S. Department of Census publications.

The variable measuring wages to state and local government workers is constructed using data from the Bureau of Labor Statistics. We sum payments to workers at the state and local levels, then divide by the number of full-time equivalent workers. The computed variable represents average monthly wages for full-time workers measured in thousands of dollars.

Table 1 contains summary statistics for all data for the two subperiods used in estimation. The data indicate a striking increase over time in expenditures by state and local governments. Average expenditures for the 1989 to 1999 period were more than 40 percent higher in real terms than average expenditures in the earlier period. Per capita income rose by about 28 percent, an increase that seems small only because of the much larger change in public expenditures. Average wages to government workers increased more slowly, exhibiting about a 14 percent rise between the two time periods.

Demographic variables changed by relatively small amounts over the period under consideration. The largest change was in the proportion of the population that is over 65 years of age. That variable increased by about 10 percent. Population growth in the entire U.S. from 1977 to 1999 was about 24 percent, much higher than the average increase in population density of about eight percent.

5. Results

We investigate three possible break points for structural change in the demand for state and local spending. The first two dates are motivated by the potential effect of the Tax Reform Act (TRA) of 1986, with one split occurring the year the Act was passed and the second split occurring after the two year phase-in period. A third break point allows the demand for state and local spending to change in the face of the budget crises faced by most states in the early 1990's. The evidence of structural change in demand is quite strong for each of the three break points; the point estimates change dramatically after each sample split. Because the *qualitative* results are unaffected by the choice of break point, we report results only for the 1987/1988 split.[12]

Another choice that might affect the estimates is the definition of a state's neighbor. Our model (like CRH's) allows a state's per capita spending to depend on the levels of its neighbors' and summarizes the value of this copy-cat effect in the parameter

[12] The results using the other two break points are available from the authors upon request.

ρ. To gauge the effect of how we define a state's neighbor, we estimate the model over each sample period using two weight matrices suggested by CRH.[13] The qualitative results (except for the estimated copy-cat effect and the effect of the tax price in the early period) are relatively invariant to the choice of weight matrix, so that we report only the results using the preferred weight matrix of CRH. This weight matrix defines neighboring states as those with similar proportions of blacks in their populations.

We present estimates from two models of per capita state and local spending. The first is our preferred model that includes the wage of government workers as a proxy for the tax-price.[14] The second omits the wage variable entirely and is included to provide a closer comparison with the model estimated by CRH.[15] We test for the possible endogeneity of federal grants in both models using a Hausman test under the maintained hypothesis that $(X_t^*, WX_t^*, W^2X_t^*)$ is a valid instrument set, where X_t^* is the original matrix of exogenous variables with grants omitted. The results from these tests are summarized in Table 2.

In the model without wages, for the early sample period, we soundly reject the null that federal grants are exogenous with a t-statistic of -2.73. Our estimated measure of

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[13] These specifications of the weight matrix, *W*, define neighboring states as those with similar demographic measures and are defined as

$$\{W : w_{ii} = 0;\;\; w_{ij} = \frac{1/\left|\overline{dem_i} - \overline{dem_j}\right|}{\sum_j 1/\left|\overline{dem_i} - \overline{dem_j}\right|},\; i, j = 1, 2, ..., n\}.$$

The variable $\overline{dem_i}$ is the sample-period-mean of per capita income of state *i* for one weight matrix and the sample-period-mean proportion of black in the population of state *i* for the other weight matrix.

[14] All reported estimation results were obtained using GAUSS.

[15] CRH restrict the error to follow a first order spatial process and estimate the model by maximum likelihood using data from 1970 to 1985. Because our data begin in 1977, we cannot replicate CHR's results exactly; however when we estimate their model specification from 1977 to 1985 by maximum likelihood using Matlab routines provided by LeSage, our results are very close to those of CRH.

the inconsistency from ignoring the endogeneity of grants is -.22 which supports Knight's (2002) conjecture that accounting for the endogeneity reduces the estimated flypaper effect. The quality of our instruments for grants over this period is reflected in the relative precision of this estimate. Over the later period, however, the instruments are not as highly correlated with grants. Although the point estimate of the inconsistency is .29, the standard error increases substantially, with a resulting test statistic of 1.44. Now we conclude that ignoring the endogeneity of grants imparts an *upward* bias to the estimated flypaper effect at the 7.5% significance level.

Over both sample periods, the estimated magnitudes of the inconsistency in the model that includes wage are much smaller than in the model with wages excluded. During the early period, the estimated bias is .10 with standard error, .14, and over the later period the estimated bias is only -.06 with a much larger standard error of .49. The imprecise estimates using the 1990's data again reflects the poorer quality of the instruments. Since both the test statistics and the estimated biases are small, we choose to treat federal grants as exogenous in this specification.

For completeness, Table 2 also reports the Hausman test results for the combined sample. In both models, we fail to reject the null hypothesis that federal grants are exogenous but now the estimated magnitude of the bias from ignoring possible endogeneity is much greater in the model including a tax price. The point estimate in the model with wages is -.21 and in the model without wages is -.06. Interestingly, if one ignores the structural change in the demand for state spending and estimates the model using the combined sample, one finds some support for Knight's prediction that accounting for the endogeneity of federal grants will reduce the estimated flypaper effect.

5.1 Overview of Results

Table 3 presents the results from GMM estimation of per capita state and local spending from 1977-1988, from 1989-1999, and from the combined sample. Based on the Hausman test results, we treat federal grants as exogenous in estimation of the model including the wage variable. We treat federal grants as endogenous in estimation of the model without the wage over the two subsamples but as exogenous in estimation over the combined sample.

For the early period, the coefficient estimates indicate that demographic variables are important in explaining the variation in expenditures. For example, the estimates indicate that a one percentage point increase in the proportion of the population that is black will increase per capita expenditures by about $147. The estimates indicate that the same increase in the school-age population will cause state and local expenditures to rise by $55. Both school age and black population coefficients have small estimated standard errors. The effect of the over-65 population is less precisely measured.

The estimated effects of income in our results differ from those found by CRH. Our results suggest that if per capita income is below about $18,200, increases in this variable are associated with declining public expenditures. However, above that level, the marginal effect of income is positive. The income variables are highly significant by the usual standards.

The estimated coefficient on federal grants suggests that a $1 increase in grants will result in an increase in spending at the state and local levels of $0.91. The estimated coefficient on the tax price variable is both negative and significantly different from zero, suggesting that if government workers' average wages rise by $1000 per month, public expenditures per capita will decline by $443.

The estimates of ρ, the copy-cat parameter, is only 0.14, much smaller than the effect found by CRH, although it is statistically significantly different from zero. We examine the implications of this estimate more closely below.

For the most part, the estimates for the period 1977 through 1988 seem satisfactory and in line with previous studies. The validity of the IV approach we take is supported by our 'first stage' regression of the tax price on the instruments. The R^2 for this regression is 0.549, with a large F-statistic on the set of instruments. We also compute the squared correlation coefficient between predicted and actual public expenditures and find that it is 0.675, so that our model fits the data fairly well.

The later period shows substantial evidence of change. Possibly most interesting is that the demographic variables appear to no longer exert a strong influence on state and local spending patterns. The estimated coefficients all diminish in value and have much larger standard errors. The most dramatic change is in the effect of proportion black on per capita public expenditures. The estimated coefficients suggest that the same one percentage point increase in a state's proportion of blacks that, in the first period, leads to an *increase in per capita spending of $147*, in the second period, leads to a *$20 decline in spending*.

To illustrate the potential magnitude of this structural change over the range of our sample, we compare the predicted per capita spending of two states whose only difference is in the proportion of blacks in their respective populations. In the early period, our model predicts that a state with 35% blacks (the sample maximum) spends

$6,484 per capita and a state with only .23% blacks (the sample minimum) spends $1,311 per capita. In the later period, our model predicts per capita spending in the state with 35% blacks is $3,021 and is $3,760 in the state with .23% blacks.[16] The 35 percentage point difference in racial composition that earlier predicted a 400% difference in spending, now predicts virtually no difference.

The measured effect of per capita income is now closer to that found in earlier studies. Increasing income is now associated with rising expenditures, but at a decreasing rate. Increases in per capita income levels above approximately $20,500 are now associated with decreases in government expenditures. The estimated flypaper effect is now very close to one and recall that this is not a result of ignoring the possible endogeneity of grants.

The estimate of ρ, the copy cat parameter, more than doubles from its level in the earlier period, but, at 0.31, it remains substantially lower than the effect of 0.701 found in CRH. The effect is statistically significantly different from zero.

For the later years, the estimated impact of tax price is positive and statistically significant. The estimates suggest that a $1000 increase in wages for state and local government workers causes an increase in per capita spending of about $618. There are several possible explanations for this result.[17] It is worth noting that the instruments, though still significant, do not do so well in explaining the variation in the tax price for these years; the R^2 for the instrument equation is only 0.282. The overall fit of the model

[16] The predictions use the state- and time-averaged values of the explanatory variables for each sample period.

[17] One such explanation is that, in the early period, it is the demand for per capita expenditures that responds to neighboring states' spending but that, in the later period, it is the supply of per capita expenditures that responds to a neighboring states' spending. Under these assumptions, it is possible that our estimation method identifies a demand curve in the early period and a supply curve in the later period.

has also declined, the squared correlation coefficient between the predicted and actual expenditures is now 0.503.

Finally, we present estimates from a model that restricts the parameter values to be constant over the entire period, 1977-1999. These results are less satisfactory in several ways. The effect of income is not precisely measured, nor are the effects of two of the three demographic variables. The estimate of ρ is close to that obtained in the first period while the effect of government wages is similar to that found in the second period, despite the fact that the R^2 on the instrument equation is 0.552.

5.2 Copy-cat Effect

One interpretation of the parameter ρ (used by CRH to interpret their point estimate) is the impact effect on a state's spending of a one dollar increase in the state's neighbors' spending.[18] Using this interpretation, our estimates suggest that a hundred dollar per capita increase in a state's neighbors' expenditures would lead to an increase in per capita state spending of \$13.66 in the early period and about \$31.04 in the later period. Although the magnitude of this copy-cat effect is certainly not negligible, it is much less than the estimated \$70 response to a \$100 increase in neighbors' spending found by CRH.

Interpreting the value of ρ in this way is useful as a summary statistic; however, it masks the fact that the impact on state *i* of an increase in neighbor *j*'s spending depends on the 'degree of neighborliness' or on the economic distance between states *i* and *j*. An alternative measure that allows the magnitude of the effect to differ across states is ρw_{ij},

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[18] This interpretation follows from the row-normalization of the weight matrix,
$$\sum_j \partial y_i / \partial y_j = \rho\left(\sum_j w_{ij}\right) = \rho.$$

the increase in state *i*'s per capita spending in response to a one dollar per capita spending increase of state *j* (state *i*'s closest neighbor). Since the weights are between zero and one, a state with only one or two very close neighbors will have a much greater response to those neighbor's spending changes than a state with many equally important neighbors.

Figure l plots the distribution of the largest copy-cat effect for each state in the second sample period when $\hat{\rho} = .31$. These effects are measured as the increase in per capita spending of each state in response to a \$1,000 increase in per capita spending of the state's closest neighbor. We see from this distribution that only a small proportion of states would increase spending by more than \$200 per capita when their closest neighbor's spending goes up by \$1,000 per capita. Most states' response is less than \$100. The results in Figure 1 suggest that using the value of ρ alone to gauge the magnitude of the impact of neighbors' spending substantially over-states the importance of the copy-cat effect.

To illustrate why the value of ρ tends to over-state the economic significance of neighbors' spending on state expenditures, we consider two examples. The first is Maine, whose population proportion black is an outlier that is very small relative to the U.S. mean. Maine's nearest neighbor (in terms of the demographic characteristic proportion of black residents) is Vermont with weight, $w_{19,44} = .631$. Given the value of $\hat{\rho}$, we predict that Maine's spending increases by only about \$200 when Vermont's spending goes up by \$1,000. Maine's next closest neighbor is New Hampshire but because New Hampshire's weight is only .025, the effect of this neighbor's \$1,000 spending increase is only about \$8!

The second example is Georgia, a state whose population proportion black is closer to the U.S. population median value. Georgia's closest neighbor in terms of our distance measure is Maryland with weight, $w_{9,18} = .157$. Such a small maximum weight implies that the effect of a $1,000 spending increase by any of Georgia's neighbors cannot exceed $5! Thus, in both of these cases, it is true that if *all states* increase spending by $1,000, we predict that Georgia and Maine will increase their spending by about $300. However, if only Georgia's and Maine's *closest neighbors* increase spending by $1,000, we would predict a much smaller impact on these states' expenditures.

6. Summary and Conclusion

In this paper, we provide new empirical estimates in three areas of interest in the literature on state and local public expenditures, specifically, the effects of preference variables, the copy-cat effect and the flypaper effect. Using panel data on states from 1977 to 1999 to estimate the determinants of total expenditures by state and local governments, we find strong evidence of structural change during the period that substantially alters previous findings in two out of the three issues described above. While the estimated impact of federal grants remains close to one, the demographic variables have only very small and largely statistically insignificant impacts on spending after the structural break. Furthermore, estimates of the copy cat effect are very sensitive to whether a proxy for the tax price of public goods is included in the model.

An overview of the results suggests that the impact of the structural change occasioned by the Tax Reform Act of 1986 is more than just a change in the magnitudes of the estimated coefficients of the model. Rather it appears that the conventional model simply does not perform as well in the later years. The results for the early period seem very much in line with previous work, the one exception being the change in shape of the quadratic function in income. Briefly, we find evidence that the age and race distributions of state populations have a strong impact on spending decisions; there is a substantial flypaper effect,; and the copy-cat effect is small, but statistically significant.

In contrast, for the 1989-1999 period, most of these findings are overturned. The demographic variables essentially have no impact on spending decisions, the tax price has a positive effect on spending, and allowing for the potential endogeneity of grants does not decrease the estimated flypaper effect. We also show that these results for the demographic variables do not arise from the inclusion of a bad proxy for tax price; the same results hold when that variable is omitted.

In addition to the finding of structural change, the paper makes several contributions to the methodology used in estimating and interpreting spatial models. First, we illustrated that with panel data, it is possible to modify Kelejian and Prucha's (1997) generalized spatial two-stage least squares estimator to allow for spatial error correlation without specifying a parametric structure for the error terms. Thus we maintain the assumption of a first order spatial dependence model in the conditional mean of expenditures, but we allow for general spatial error correlation. This estimation strategy follows current empirical practice by combining estimates of a fully parametric conditional mean function with robust standard error estimates.

Second, the Kelejian and Prucha estimation strategy yields more instruments than are needed to account for the endogeneity of other states' expenditures. This is advantageous because it affords instruments to use to account for the endogeneity of the tax price and federal grants, which has not often been done in previous empirical work. Unlike Knight, we find that allowing for the endogeneity of federal grants does not resolve the flypaper effect. In our case, tests indicate that the null hypothesis of exogeneity could not be rejected in four of six models. In the two cases where there is some evidence of endogeneity, using instruments lowers the estimated effect of grants in one model, but actually increases it in the other.

Third, we contribute to the interpretation of spatial models by providing a more detailed examination of the impact of neighboring states' expenditures on own expenditure. While the conventional interpretation of the copy-cat parameter (ρ) as the impact of a unit increase in the weighted average of neighbors' expenditures on own expenditures is correct, it does not answer the question of how a state responds to an increase in spending by its nearest neighbor or neighbors. Nor does this interpretation show the large variation in the response of states to changes in their nearest neighbors' spending. Our interpretation illustrates how much more information on the impact of neighbors can be gained from the estimated spatial dependence.

Overall, our contributions are twofold: first, we believe that the methodology employed here could be useful in many other panel applications where spatial relationships are important. Second, our findings of structural change raise questions about the viability of the conventional model of state and local spending. While plausible explanations of our results that stay within the framework of that model could perhaps be found, it seems possible that it is time for a new model and a new framework.

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Table 1: Descriptive Statistics

Variables	1977-1988		1989-1999	
	Mean	*Std. Dev.*	*Mean*	*Std. Dev.*
Real Per Capita Expenditures∗	2756.343	582.1427	3929.515	688.35
Grants∗	1674.995	.4787504	1548.403	.2204495
Income	17643.85	3862.408	22648.3	3662.601
Pop. Percentage > 65	11.57698	1.875572	12.79176	1.746675
Pop. Percentage 6-17	20.33423	1.940884	19.22558	7.421776
Pop. Percentage Black	9.508559	9.221721	10.21095	9.477844
Pop. Density	160.617	227.2625	173.7339	238.6191
Wages	2229.188	444.141	2549.902	410.5912
Population	4.779784	4.889813	5.364095	5.705286

∗ Grants, expenditures and income are in real dollars per capita terms. The wage variable represents real average monthly wages for full time workers. State population is in millions.

Table 2: Hausman Tests of Exogeneity of Federal Grants

Regression: $y = X^*\theta_1 + TP\theta_2 + (I_T \otimes W_n)\hat{y}\,\theta_3 + Grants\,\theta_4 + Grants\,\hat{\alpha}$

	With Wage Included			Without Wage		
	t-statistic $\hat{\alpha}/se(\hat{\alpha})$	Bias $\hat{\alpha}$	Grants coefficient	t-statistic $\hat{\alpha}/se(\hat{\alpha})$	Bias $\hat{\alpha}$	Grants coefficient
1977-1988	.74	.10 (.14)	.85	-2.73	-.22 (.08)	.84
1989-1999	-.13	-.06 (.49)	1.18	1.44	.29 (.20)	1.18
1977-1999	-.81	-.21 (.26)	.68	-.55	-.06 (.10)	.68

Table 3: Estimation Results

1977-1988				
Variable Name	**Coefficient**	**Std. error**	**Coefficient**	**Std. error**
Grants	0.908*	0.075	0.567*	0.041
Percap inc	-22.522*	1.702	-13.762 *	1.940
Inc-sq	0.062*	0.003	0.042*	0.004
Percent 65+	7.085	11.929	-14.740	10.127
Percent School	54.847*	7.332	67.324*	9.434
Percent Black	147.032*	28.119	126.540*	22.956
Pop. Density	0.106	0.768	-2.232*	0.515
Wages	-442.767*	194.043	-	-
ρ	0.137*	0.068	0.567*	0.041
1989-1999				
Variable Name	**Coefficient**	**Std. error**	**Coefficient**	**Std. error**
Grants	1.072*	0.134	1.506*	0.149
Percap inc	10.041*	2.107	6.336	3.753
Inc-sq	-0.024*	0.003	-0.019*	0.005
Percent 65+	-3.562	14.280	-34.605*	14.613
Percent School	-0.382	0.411	-0.053	0.523
Percent Black	-20.569	13.453	-15.154	11.099
Pop. Density	-1.338*	0.710	.198	0.755
Wages	617.682*	272.724	-	-
ρ	0.310*	0.072	0.828*	0.139
1977-1999				
Variable Name	**Coefficient**	**Std. error**	**Coefficient**	**Std. error**
Grants	0.397*	0.097	0.679*	0.053
Percap inc	-3.976	2.360	1.978	2.294
Inc-sq	0.012*	0.005	0.007	0.004
Percent 65+	2.716	9.654	45.252*	9.556
Percent School	-0.769	0.872	-0.451	0.938
Percent Black	47.180*	12.180	60.788*	10.854
Pop. Density	-0.777	0.624	1.529*	0.533
Wages	779.996*	192.238	-	-
ρ	0.138*	0.053	0.031	0.034

∗ denotes statistically significant at 5 % level or less. Note that per capita income is scaled in 100s of dollars for estimation. Time and fixed effects have been removed prior to estimation.

Figure 1: Effect of Closest Neighbor's Spending Change