NBER WORKING PAPER SERIES

THE REVENUES-EXPENDITURES NEXUS: EVIDENCE FROM LOCAL GOVERNMENT DATA

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Working Paper No. 2180

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
March 1987

We are grateful to Maria San Segundo for assistance with the computations and to Research Services at Princeton University for help in acquiring the data. We thank Gary Chamberlain, James Poterba, and members of Princeton's Industrial Relations Seminar for useful comments. This research was supported in part by NSF Grants SES-8419238 and SES-8410249. The research reported here is part of the NBER's research program in Taxation. Any opinions expressed are those of the authors and not those of the National Bureau of Economic Research.

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ABSTRACT

This paper examines the intertemporal linkages between local government expenditures and revenues. In the terminology that has become standard in the literature on vector autoregression analysis, the issue is whether revenues Granger-cause expenditures, or expenditures Granger-cause revenues. The main results that emerge from an analysis of fiscal data from 171 municipal governments over the period 1972-1980 are that: 1) one or two years are sufficient to summarize the relevant dynamic interrelationships; 2) there are important intertemporal linkages between expenditures, taxes and grants; and 3) past revenues help predict current expenditures, but past expenditures do not alter the future path of revenues. This last finding is contrary to results that have emerged from previous analyses of federal fiscal data, and hence suggests the need for additional research on the differences in the processes generating local and federal decisions.

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I. Introduction

The significance of intertemporal linkages between government expenditures and revenues has been discussed both by economists and political scientists. As von Furstenberg, Green and Jeong [1985, 1986] observe, three hypotheses have been advanced:

1. Revenues change concurrently with expenditures. Such a pattern would result if each year the citizens of a jurisdiction (or their representatives) simultaneously select taxes and expenditures using the standard calculus for weighing marginal benefits and costs. Theoretical models generating such behavior are Lindahl's [1958] model of benefit taxation, or the well-known median voter rule. (Black [1948].)

2. Taxes change before spending. To see how this sequence might emerge, consider a government controlled by individuals who want to expand its size beyond that desired by the citizenry. (Niskanen [1976].) In the presence of statutory or constitutional rules prohibiting deficits, how can public sector managers increase spending? According to this story, the answer is that they must wait for revenues to increase, and then increase expenditures.[1] A state senator from New Jersey put it this way: "It is axiomatic that government spending will rise to meet and eventually exceed available revenues."[2] However, nonsynchronous changes in expenditures and revenues need not be associated with any "failure" in the political process. For example, taxes might change before spending if a community decides to save for anticipated future expenditures by raising taxes prior to the time those expenditures are made.

3. Spending changes before taxes. According to this story, some special event creates a "need" for an increase in expenditures.[3] Rather than cut other expenditues, public sector managers convince voters that the only

way to balance the budget is through increased taxes. Buchanan [1960] notes that this view has a long pedigree; its proponents included members of the nineteenth century "Italian school" of public finance. But just as in the case of hypothesis 2, this pattern can be rationalized by an intertemporal decision-making model.

Each of the 3 hypotheses has a straightforward implication in terms of the time series properties of expenditures and revenues. Under the second hypothesis, for example, one would expect to find that past levels of revenues help predict current expenditure levels. In the same way, according to the third hypothesis, past expenditures help predict current revenues. In the terminology that has become standard in the literature on vector autoregression (VAR) analysis, the issue is whether revenues Granger-cause expenditures, or expenditures Granger-cause revenues. We emphasize that in adopting this terminology, it is not our intention to take sides in the debate over whether vector autoregression results reveal anything about "causality" in a philosophically meaningful sense. However, for the sake of readability, we will henceforth refrain from putting quotation marks around that word.

In two important recent papers, von Furstenberg, Green and Jeong [1985], [1986] (hereafter FGJ) used VAR's to analyze expenditure and revenue data of the federal government. Their 1985 paper examined state and local spending as well. FGJ's basic finding was that taxes did not cause aggregate spending, but there was some weak support for the reverse sequence, that spending helps predict taxes.

As FGJ [1986] note, federal fiscal data may be inappropriate for testing the various political economy hypotheses listed above: "...[O]nly that part of any change in fiscal magnitudes which is not accepted as part of cyclical or price-level stabilization designs can be expected to hold messages for the other side of the budget." Thus, VAR's "would be biased against finding support in past data for the...proposition that spending can be pulled along by prior tax action, unless changes in the average aggregate tax rates were adjusted for movements in cyclical factors and inflation" (p. 181).

FGJ's solution to this problem is to adjust the cyclically sensitive time series for concurrent cyclical effects by regressing each one on the GNP gap and the inflation rate, and then using the residuals in subsequent VAR analyses.[4] It's hard to think of a much better way to deal with this problem. Yet one wonders about its adequacy, particularly in light of the well-known difficulties in measuring the timing and severity of the business cycle. Just how does one measure potential GNP; is this a better measure than the deviation of the actual from the permanent rate of unemployment; etc.? In short, the fact that the federal government carries on its stabilization function concurrently with its other fiscal activities will tend to confound attempts to link intertemporal patterns in fiscal variables to various views of the budget process.

In contrast, state and local governments are not in the business of counter-cyclical policy. In their 1985 paper, FGJ examine fiscal data on the state and local public sector as a whole. Here the problem is whether it is appropriate to aggregate all state and local governments into one unit. After all, the various governments differ with respect to the functions they perform, their budgetary processes, the political environments in which the operate, etc.

In this paper we apply VAR techniques to data from individual local governments to study the revenues-expenditures nexus. Hence, neither stabilization issues nor aggregation problems impede interpretation of the results. To anticipate our main conclusion, we find that past revenues help to predict current expenditures, but past expenditures do not alter the future path of revenues.

Typically, VAR methods are applied to relatively long time series. No comparable fiscal data for individual communities are available. We have assembled a nine year panel with information on 171 municipal governments. VAR techniques can be applied to such data sets, but it requires confronting some interesting econometric problems. These are discussed in Section II. The results are presented in Section III, and Section IV contains a summary and conclusions.

II. Econometric Issues[5]

We begin by considering causality tests in their usual time series context. The issue is to determine the causal relationship between the detrended variables x and y, on which the investigator has a large number of observations. The variable x is said to not (Granger) cause the variable y if:

$$(2.1)\quad E\{y_t | y_{t-1}, y_{t-2}, \ldots, y_1, x_{t-1}, x_{t-2}, \ldots, x_1\} = E\{y_t | y_{t-1}, y_{t-2}, \ldots, y_1\}$$

where $E\{\cdot|\cdot\}$ denotes a linear projection. Intuitively, if one's prediction of y_t, given the history of y, cannot be improved by including the history of x, then x does not cause y.[6]

Essentially, the procedure is to estimate a regression of the form:

$$(2.2)\quad y_t = \alpha_0 + \sum_{\ell=1}^{m} \alpha_\ell y_{t-\ell} + \sum_{k=1}^{n} \delta_k x_{t-k} + u_t$$

where the α's and δ's are parameters and the lag lengths m and n are sufficient to ensure that u_t is a white noise error. While it is not essential that m equal n, we follow typical practice by assuming that they are identical. The test of whether x causes y is simply a test of the joint hypothesis that $\delta_1=\delta_2=\ldots=\delta_m$ are all equal to zero. This can be done by using standard F-tests; a good example is FGJ's study of fiscal data.

To perform the test, there must be enough observations on x and y to obtain consistent estimates of the parameters in equation (2.2). Panel data generally do not have the requisite number of observations. Instead, there often are a great number of cross-sectional units, but only a few years worth of data on each unit. To estimate any parameters, investigators typically pool data from different units, a procedure which imposes the constraint that the underlying structure is the same for each cross-sectional unit.

Given this , why not simply stack all the time series-cross section observations together and use them to estimate equation (2.2)? The main pitfall of such a procedure is that it ignores the possibility that each unit has an "individual effect"--which translates in practice to its own intercept. The individual effect summarizes the influence of unobserved variables which have a persistent effect on the dependent variable. For example, a community's expenditures each period might be affected by its geographical location or its "political make-up." To the extent that the other right hand side variables are correlated with the individual effect, its omission results in inconsistent estimates. Although there are standard methods for estimating individual effects, they are not appropriate for our problem.

To see why, assume that there are N cross-sectional units observed over T periods. Let i index the cross-sectional observations and t the time periods. Assume further the existence of an individual effect (f_i) for the i^{th} cross-sectional unit. The model is:

$$(2.3)\quad y_{it} = \alpha_0 + \sum_{\ell=1}^{m} \alpha_\ell y_{it-\ell} + \sum_{\ell=1}^{m} \delta_\ell x_{it-\ell} + f_i + u_{it} \qquad \begin{array}{l} i = 1,\ldots,N \\ t = m+1,\ldots,T \end{array}$$

If the x's and y's require detrending, equation (2.3) can be augmented with a time trend. Estimation and identification are not complicated by this addition; hence, for simplicity, it is suppressed below.

A standard method of estimating the individual effect is to first difference the data to eliminate f_i and then use ordinary or generalized least squares to estimate the differenced equation:

$$(2.4)\quad y_{it} - y_{it-1} = \sum_{\ell=1}^{m} \alpha_\ell (y_{it-\ell} - y_{it-\ell-1}) + \sum_{\ell=1}^{m} \delta_\ell (x_{it-\ell} - x_{it-\ell-1}) + (u_{it} - u_{it-1}) \qquad \begin{array}{l} i=1,\ldots,N \\ t=(m+2),\ldots,T \end{array}$$

A quick examination of equation (2.4) indicates the flaw with this approach in the current context: because y_{it-1} depends on u_{it-1}, the error term $(u_{it} - u_{it})$ is correlated with the regressor $(y_{it-1} - y_{it-2})$.

The fact that differencing can induce a simultaneity problem is well known from the conventional literature on time series analysis and has been explored in a panel data context. (See, e.g., Chamberlain [1983].) The usual solution is to employ an instrumental variables estimator. Here, too, this turns out to appropriate, but it is implemented in a different fashion than is typical. This is because, as we note below, the variables which are legitimate candidates for use as instrumental variables change over time.[7]

The most straightforward way to motivate an estimation procedure for the system (2.4) is to discuss its identification. The criterion we use for identification is that there must be a sufficient number of instrumental variables to allow estimation of the equation in question. This leads directly to an instrumental variables estimator which has a generalized least squares (GLS) interpretation.

We begin by assuming that, as usual, the error term, u_{it}, is uncorrelated with all past values of y and x, and the individual effect:

(2.5) $E\{y_{is}u_{it}\} = E\{x_{is}u_{it}\} = E\{f_i u_{it}\} = 0, \; s < t.$

The orthogonality conditions (2.5) can be used to identify the parameters of (2.4), since the disturbance term v_{it} (= $u_{it} - u_{it-1}$) will be uncorrelated with y_{it-s} and x_{it-s} for $s \geq 2$. The equation for each time period t has 2m right-hand side variables. To identify the parameters, there must be at least this many instrumental variables. The 2(t-2) variables $[y_{it-2},\ldots y_{i1}, x_{it-2},\ldots x_{i1}]$ are available as instrumental variables to estimate the equation for time period t. Thus, to have at least as many instrumental variables as right-hand side variables, it must be true that $2(t-2) \geq 2m$, or $t \geq m+2$.[8]

Given our assumed lag structure, it is impossible to estimate the equations (2.4) for time periods before t = m+2. Thus, these equations are ignored. Clearly, the decision about which equations to "ignore" depends crucially on assumptions concerning lag length. If we make an incorrect assumption and truncate the lag distribution, the parameter estimates will be inconsistent. This creates a potential identification problem when the lag length is unknown. We do not present a general treatment of the problem, and instead use only the straightforward restriction implicitly imposed above: that if the largest lag length is m, then the number of time periods T is greater than m+2.

These considerations suggest the following estimation procedure. Make some assumption on the lag length, m. Think of (2.4) as a system of (T-m-1) equations with constraints across equations, namely, the α's and δ's are the same in each equation. (This assumption will be relaxed below.) An efficient estimator can be formed in three steps: i) Estimate the equations for each time period using 2SLS. Unlike the standard case, however, the list of instrumental variables is not the same for each equation, because, as noted above, the list of variables uncorrelated with the errors changes each period. ii) Using the residuals and the matrix of instruments, estimate the joint covariance of the error terms. iii) Estimate all the parameters simultaneously using generalized least squares on the stacked equations.

Holtz-Eakin, Newey and Rosen [1985] provide explicit formulas. They also show that in this model linear constraints can be tested in the conventional way, i.e., by noting that the difference in the constrained and unconstrained sum of squared residuals has a χ^2 distribution. In the current context, linear constraints are associated with three particularly interesting questions:

1. Are the parameters stationary over time? Equation (2.4), like virtually all work analyzing panel data, assumes that the parameters are constant not only across different units, but also over time. Similarly, each individual effect is time invariant. A more general specification is to allow all of the parameters to depend on the time period:

$$(2.6)\quad y_{it} = \alpha_{0t} + \sum_{\ell=1}^{m} \alpha_{\ell t} y_{it-\ell} + \sum_{\ell=1}^{m} \delta_{\ell t} x_{it-\ell} + \psi_t f_i + u_{it} \quad [t=(m+1),\ldots T],$$

where ψ_t is the parameter multiplying the individual effect.[9] Because of the ψ_t's, one cannot simply difference away the individual effect. However, following Chamberlain [1983], we can multiply the equation for time period t by (ψ_{t+1}/ψ_t), and then subtract it from the equation for period t+1:

$$(2.7)\quad y_{it} = a_t + \sum_{\ell=1}^{m+1} c_{\ell t} y_{it-\ell} + \sum_{\ell=1}^{m+1} d_{\ell t} x_{it-\ell} + v_{it} \quad [t=(m+2),\ldots,T]$$

where: $r_t = (\psi_t/\psi_{t-1})$

$$a_t = a_{0t} - r_t\alpha_{0t-1}$$

$$c_{1t} = r_t + \alpha_{1t}$$

$$c_{\ell t} = \alpha_{\ell t} - r_t\alpha_{\ell-1,t-1} \qquad [\ell=2,\ldots,m] \qquad (2.7a)$$

$$c_{m+1,t} = -r_t\alpha_{m,t-1}$$

$$d_{1t} = \delta_{1t}$$

$$d_{\ell t} = \delta_{\ell t} - r_t\delta_{\ell-1,t-1} \qquad [\ell=2,\ldots,m]$$

$$d_{m+1,t} = -r_t\delta_{m,t-1}$$

$$v_{it} = u_{it} - r_t u_{i,t-1}$$

Observe that in each of the equations in (2.7) there are 2(m+1) right hand side variables other than the constant, or a total of (2m+3). To identify the parameters of (2.7) an equal number of instrumental variables is required. Since the instrumental variables vector is

$$[1, y_{it-2}, \ldots, y_{i1}, x_{it-2}, \ldots, x_{i1}],$$

it is now required that $t \geq m+3$ to have a sufficient number of instrumental variables for the equation for time period t. Thus, as one would suspect, allowing for time varying parameters makes identification more difficult. Nevertheless, the basic estimating procedure discussed above can still be employed. Specifically, one can: i) choose a relatively large value of m to be sure to avoid truncating the lag structure inappropriately (we discuss below how to find the "best" value of m); ii) estimate the model with and without parameter stationarity; and iii) compare the sums of squared residuals.

2. What is the correct lag length, m? Denote by $\tilde{m}$ the relatively large

value of m used for initial estimation of the model. Re-estimate the system (2.4) or (2.7) (whichever is appropriate) with $m = (\tilde{m}-1)$. If the increase in the sum of squared residuals is "large," then $m = \tilde{m}$ is accepted. If the increase is "small," then try $m = (\tilde{m}-2)$. Continue testing successively smaller lag lengths until one is rejected by the data, or $m = 0$.

3. Does x cause y? In the model with stationary coefficients, (equation (2.4)), this is simply a test of the joint hypothesis $\delta_1 = \delta_2 = \ldots = \delta_m = 0$. In the model with nonstationary coefficients the same procedure can be applied to the $d_{\ell t}$ of equation (2.7). As (2.7a) makes clear, all the δ's being zero implies that all the d's are zero.

III. Investigating the Revenues-Expenditures Nexus

A. Preliminaries: The Role of Grants

In this section we employ the techniques discussed in Section II to investigate the characteristics of the VAR's for local governments' revenues and expenditures. An important source of funds for localities is grants from state and federal governments. Both theoretical considerations and earlier econometric work suggest that grants from other levels of government affect communities' fiscal decisions differently than own source revenues. (See Inman [1979].) We therefore include lagged grants as right hand side variables.

The inclusion of grants in our analysis not only facilitates examination of our main concern, the revenues-expenditures nexus, but also allows us to gain insights into some other controversial aspects of local public finance. In previous econometric investigations of local government expenditures, an important empirical regularity is the "flypaper effect": a dollar increase in exogenous grant monies stimulates local spending more than a dollar increase in local income. (See Inman [1979].) One interpretation of this result turns on the hypothesis that local bureaucrats, for a variety of reasons, seek to increase the amount of public spending beyond the level desired by the representative voter. Assuming further that the bureaucrats have better information than voters on the magnitude of outside grants, the bureaucrats may "trick" the voters into supporting larger expenditures than they might otherwise permit.

As far as we know, all evidence on the flypaper effect comes from cross-sectional analysis of local governments. The dynamic and stochastic properties of local revenue, grant, and spending streams have not been explored in a panel context. A straightforward dynamic reinterpretation of the flypaper effect is that grant monies cause (in the sense discussed above) local expenditure.

In discussions of intergovernmental grants, it is common to distinguish between lump sum grants and matching grants. Investigators such as Craig and Inman [1982], who examine the contemporaneous relationship between grants and spending, have correctly noted that since matching grants have price effects, they cannot simply be combined together with lump sum grants to form a single "grants variable." However, in the context of causality testing this distinction is a non-issue. To be sure, in the presence of matching rates, innovations in grants and expenditures may be correlated. Nevertheless, the existence of matching rates puts no restrictions on the way in which current expenditures respond to past innovations.[10]

B. Data

Our data are drawn from the Annual Survey of Governments between 1973 and 1980 and the Census of Governments conducted in 1972 and 1977. A random sample of municipal government records was selected from the data tape for 1979 (the year with the least coverage) and these same government records were selected for the remaining eight years when possible.[11] There is usable information on 171 municipal governments over a period of nine (fiscal) years.

In each year, the record for each government essentially presents the budget identity--including revenues from a variety of sources, expenditures by program and type (current, capital, etc.), debt transactions, grant receipts (by source), and grant transfers (of minimal importance at the municipal level.) These dollar amounts were converted to per capita real dollars using a regional price index with December 1977 = 100. All variables are entered as natural logarithms.

The data contained virtually no information on the economic and demographic characteristics of the communities. Such variables typically play an important role in regression analyses of local government spending. (See Inman [979].) However, to the extent that economic and demographic characteristics can be regarded as "individual effects," this absence of information will cause no problems. In essence, the statistical procedure discussed in Section II eliminates these effects via differencing. In addition, every equation contains a dummy variable for each year. The system of dummies will capture any underlying trend in the data, as well as important macroeconomic influences common to all jurisdictions in a given year.

The results presented here use total local current expenditures, total local revenues, and total grants received. Less aggregative work focusing on specific revenue and expenditure categories is presented in Holtz-Eakin [1986].

C. Estimation and Testing

Our focus is on the dynamic interrelationships between three variables: expenditures, revenues, and grants. First, we estimate a model in which expenditures appear on the left hand side, and on the right hand side are its own lags and lags of the other two variables. Next we do the same thing for revenues.[12] The results are used to investigate issues of parameter stationarity, lag length, and causation.

Expenditures. We begin by estimating an equation with 2 lags of each of the right hand side variables; in terms of our earlier notation, m=2.[13] The quasi-differenced version, then, has three lags. Given that in our data T=9, m=2 implies that we can estimate parameters for only the last five years in the data set; i.e., t=1976,...,1980. When the equations for these years are estimated jointly using the three-stage procedure described above, the minimized value of the χ^2 test statistic, which we denote Q, is equal to 1.99, and has 30 degrees of freedom.[14] (For convenience, this result and others to follow are summarized in Table 1.) Now, inferences about causality will be incorrect if the lag distribution is incorrectly truncated and/or parameter stationarity is incorrectly imposed. In order to avoid these (type II) errors, we choose 10% significance levels for the tests on lag length and parameter stationarity, rather than the conventional 5% or 1% levels. Because the value of the χ^2_{30} at the 10% level is 40.26, we can easily accept m=2.

When we examined the coefficients of this specification, we noticed that most of them were quite small relative to their standard errors. To see if we could sharpen the results by putting more structure on the model, we imposed the condition that the coefficients on the individual effects be stationary, i.e., that r_t from (2.7a) be unity for all t. The value of Q from this restriction is 3.54. Therefore, the value of the appropriate test statistic, denoted L, is 3.54 (restricted Q) minus 1.99 (unrestricted Q), or 1.55. There are nine degrees of freedom because there are four restrictions with $r_t = 1$, and five restrictions on the parameters for

Table 1

Expenditures Equation

	Q	L	Degrees of Freedom	χ^{2*}
i) m=2	1.99	-	30	40.26
ii) stationary fixed effects (given i)	3.54	1.55	9	14.68
iii) all parameters stationary (given ii)	46.48	42.94	30	40.26
iv) m=1 (given ii)	19.03	15.49	18	25.99
v) m=0 (given ii)	108.44	89.41	18	25.99
vi) exclude revenues (given iv)	39.41	20.38	6	12.6
vii) exclude grants (given iv)	32.72	13.69	6	12.6

*For lines i through v, χ^2 is evaluated at the 0.10 significance level; for lines vi and vii, at the 0.05 significance level.

the third lag of each variable. The associated critical value of χ^2_9 at a .10 significance level is 14.68; hence, the model with stationary individual effects passes the test by a wide margin. (See line ii of Table 1.)

Are all the parameters similarly stationary over time? When we impose this constraint, the associated value of Q is 46.48. In this case, then, L = 46.48 - 3.54, or 42.94, and has 30 degrees of freedom. (There are 30 degrees of freedom because the six lag parameters for each of 1975 through 1979 are constrained equal to their 1980 values.) The critical value of the χ^2_{30} distribution at the 0.10 level is 40.26. We therefore reject the hypothesis that all coefficients are stationary across time.

We next investigate results relating to lag length (conditional on the assumption that r_t=1). The first question is whether the data will permit us to shorten the lag length from two to one. When we impose m=1, the value of Q is 19.03. Comparing this to the value of Q in line ii of Table 1, we find that L = 15.49, and has 18 degrees of freedom. (There are 18 degrees of freedom because we are restricting three lags in each of six years.) The critical value of the χ^2_{18} distribution at the 0.10 level is 25.99. We can accept the restriction that one lag in each variable adequately characterizes the data.

The fact that m=1 passes the test gives rise to the thought that an even more parsimonious specification, m=0, might do so as well. When we estimate the expenditures equation with no lags at all, the value of Q jumps to 108.44; the associated value of L is 89.41 (108.44 - 19.03). The data clearly reject this hypothesis by a wide margin. (See line v of Table 1.)

Conditional on m=1 and stationary fixed effects, we next turn to causality issues. As noted above, to test whether revenues cause expenditures, we simply estimate the expenditures equation excluding revenues, and evaluate the increase in the minimum χ^2 test statistic. The value of Q when revenues are excluded is 39.41; the value of L is 39.41 minus 19.03, or 20.38, and it has six degrees of freedom. (There are six degrees of freedom because the coefficient on the lagged value of revenues in each year 1975-1980 is restricted to equal zero.) The critical value of the χ^2_6 distribution at the 0.05 level is 12.6; hence, the data reject by a wide margin the notion that revenues do not cause expenditures.

When we estimate the expenditures equation excluding grants, we find that Q = 32.72, L = 13.69, and the hypothesis of non-causality is again rejected, although by a smaller margin.

To summarize: We find that community expenditures can be described by a dynamic process which has only one year lags. The individual effects are stationary across time periods, but the other parameters (taken as a group) are not. Further, one can reject the hypothesis that revenues do not cause expenditures.

Revenues. The procedures for analyzing revenues are very similar to those for expenditures, which were just described in detail. We therefore briefly summarize the results which are reported in Table 2: (a) A lag length of two is at least sufficient to characterize the data (see line i). (b) Given m=2, one cannot reject the hypothesis that all the parameters are stationary across time periods (see lines ii and iii). (c) One can reject the hypothesis that m=1 (see line iv). (d) One cannot reject the hypothesis that expenditures do not cause revenues (line v), but one can reject the hypothesis that grants do not cause revenues (line vi).

Parameter Estimates. We next turn to an examination of the parameter

Table 2

Revenues Equation

	Q	L	Degrees of Freedom	χ^2 *
i) m=2	1.24	-	30	40.26
ii) stationary fixed effects (given i)	3.49	2.25	9	14.68
iii) all parameters stationary (given ii)	24.13	20.64	30	40.26
iv) m=1 (given iii)	47.23	23.10	3	6.25
v) exclude expenditures (given iii)	25.43	1.3	2	5.99
vi) exclude expenditures and grants	47.37	23.24	4	9.49

*For lines i through iv, χ^2 is evaluated at the 0.10 significance level; for lines v and vi at the 0.05 significance level.

estimates of the expenditures and revenues equations.[15] In Table 3, for most cases we report the lag coefficients of the most parsimonious specification of each equation that is consistent with the data, based on the discussions surrounding Tables 1 and 2. Table 3 suggests the following thoughts:

1. While the processes generating expenditures and revenues share the important characteristic of a stationary individual effect, they differ with respect to lag length and whether the lag parameters change over time. More coefficients are reported for expenditures than for revenues, because only for the latter are all the parameters stationary over time.

2. In general, parameter stationarity can be rejected for one of two reasons. Either the estimates are qualitatively "close" but are precisely estimated, or the parameters differ greatly in magnitude even if they are individually estimated without much precision. The former seems roughly to be the case in the expenditures equation.

3. As noted earlier, the data suggest that expenditures do not cause revenues. For the sake of completeness, however, we have reported the coefficients of lagged expenditures in the revenues equation.[16] It is interesting to note that in addition to being statistically insignificant, they are small in magnitude compared to most of the other coefficients in the revenues equation. <u>From either perspective, past expenditures are not an "important" determinant of revenues.</u>

4. Analysis of the dynamic behavior of the system as a whole is complicated by the non-stationarity of the estimated coefficients. For example, examination of steady state multipliers is not meaningful when the coefficients are changing over time. Is our equation mispecified, or does the process generating expenditures vary from period to period? At this

Table 3

Parameter Estimates*

Expenditures						Revenues		
1980			1977			1980		
E_{t-1}	0.0981	(.786)	E_{t-1}	0.170	(.185)	E_{t-1}	0.054	(.202)
E_{t-2}			E_{t-2}			E_{t-2}	0.054	(.051)
R_{t-1}	0.988	(.616)	R_{t-1}	0.628	(.208)	R_{t-1}	0.543	(.179)
R_{t-2}			R_{t-2}			R_{t-2}	-0.019	(.0245)
G_{t-1}	-0.2122	(.546)	G_{t-1}	-0.207	(.0809)	G_{t-1}	-0.164	(.0640)
G_{t-2}			G_{t-2}			G_{t-2}	-0.101	(.0245)
1979			1976					
E_{t-1}	-0.160	(.727)	E_{t-1}	0.262	(.117)			
E_{t-2}			E_{t-2}					
R_{t-1}	1.13	(.659)	R_{t-1}	0.684	(.166)			
R_{t-2}			R_{t-2}					
G_{t-1}	-0.211	(.175)	G_{t-1}	-0.137	(.0443)			
G_{t-2}			G_{t-2}					
1978			1975					
E_{t-1}	0.201	(.220)	E_{t-1}	0.151	(.335)			
E_{t-2}			E_{t-2}					
R_{t-1}	0.642	(.219)	R_{t-1}	0.662	(.288)			
R_{t-2}			R_{t-2}					
G_{t-1}	-0.145	(.0576)	G_{t-1}	-0.176	(.141)			
G_{t-2}			G_{t-2}					

*Numbers in parentheses are standard errors.

point we simply do not know. However, this finding does create concerns about analyses that _impose_ stationarity without ever testing it.

IV. Summary and Conclusions

A number of results have emerged from our analysis of local expenditures and revenues data: 1.) The figures from the lag truncation tests suggest that lags of one or two years are sufficient to summarize the dynamic interrelationships in local public finance. In other words, a sufficient information set to characterize the correlations in the data is just one or two years. 2.) There are important intertemporal linkages among expenditures, taxes, and grants. The existence of such linkages suggests that hypothesis number 1 in the introduction--revenues change only concurrently with expenditures--is incorrect. This finding casts doubt upon the interpretation of standard regressions that examine only contemporaneous relationships among fiscal variables. 3.) The results from the stationarity tests suggest that it is dangerous, as is common, to assume that all parameter estimates from panel data do not change over time. 4.) Past revenues help to predict current expenditures, but past expenditures do not alter the future path of revenues.

The last of these findings is particularly interesting in the context of von Furstenberg, Green and Jeong's results. FGJ [1986] find that taxes do not Granger-cause spending, and view this as evidence against "...the tactical supply-side proposition, that spending can be pulled along by prior tax action" (p. 181). If one is willing to impose this interpretation upon the VAR results, then our examination of disaggregated local government data suggests that this "tactical supply-side proposition" _is_ consistent with historical experience. Why do the results for local and federal government differ? Is it because of the difficulties inherent to abstracting from the federal government's attempts to pursue countercyclical fiscal policies? Or are there fundamental differences in the processes generating local and federal decisions? These are important topics for future research.

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Footnotes

[1]This leaves open the question of why citizens do not require their representatives to rebate any tax revenues in excess of optimal expenditures. There are a number of theories which attempt to explain non-responsiveness of this kind; such models tend to emphasize the costs that voters must incur to obtain the relevant information and oust non-responsive incumbents. See, e.g., Atkinson and Stiglitz [1980, Chap. 10].

[2]New York Times, New Jersey Weekly, March 17, 1985, p. 22.

[3]Most state and local governments face balanced budget rules, although it is not clear they serve as an effective limit on current expenditure. See Inman [1983].

[4]More specifically, first differences in the variables are employed, and a dummy variable is included to account for anomalous fiscal behavior in the year 1975.

[5]See Holtz-Eakin, Newey and Rosen [1985] for additional details on identification, estimation, and inference in this class of model.

[6]The discussion generalizes easily to the case where there is more than one right hand side variable.

[7]One should also note that heteroskedasticity is likely to be a problem in the panel context--different units may be expected to have error terms with unequal variances. Efficient estimation and correct formulae for standard errors require that heteroskedasticity be taken into account.

[8]A sufficient condition for identification is that in the limit the cross-product matrix between the instruments and the right hand side variables be nonsingular.

[9]A special case which may be of particular interest occurs when the α's and δ's are time invariant, but ψ_t is not.

[10]Alternatively, matching rates do not change the "reduced form" relationship, which is all that matters for Granger-causality. However, the matching rates are embedded in the coefficients, so they would have to be taken into account if one attempted to recover the coefficients of the underlying "structural model."

[11]To remain in the sample, communities had to report positive school expenditures.

[12]For the sake of completeness, we also estimated a model with grants on the left hand side. These results are available upon request.

[13]We begin at m=2 in order to estimate the covariance matrix necessary to test for this and other (including larger) lag lengths. However, it is obvious from the test statistic that the restriction m=2 is consistent with the data. This also turns out to be true for the revenues equation.

[14]The calculation of degrees of freedom is as follows: For 1980, we have available 7 years of data for each variable (1972-1978). Adding a constant gives 22 instrumental variables. This number falls to 19 in 1979, 16 in 1978, and so forth. Thus, the total number of instrumental variables is 22 + 19 + 16 + 13 + 10 = 80. For each year we estimate 10 parameters; for a total of 50. The degrees of freedom is simply the difference: 80-50.

[15]To conserve space, the coefficients of the unrestricted equations are not reported; these are available upon request, as are the results for the grants equation.

[16]When expenditures are excluded from the equation, the other coefficients barely change. The coefficients on R_{t-1}, R_{t-2}, G_{t-1} and G_{t-2}, respectively, a are 0.567 (.122), -0.00677 (.0216), -0.156 (.0575), and -0.0833 (.018).