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The Role of Federal Taxation in the Supply of Municipal Bonds: Evidence from Municipal Governments

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THE ROLE OF FEDERAL TAXATION IN THE SUPPLY OF MUNICIPAL BONDS: EVIDENCE FROM MUNICIPAL GOVERNMENTS

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ABSTRACT

This paper considers ways in which federal tax policy affects municipal asset and debt holdings. The tax treatment of municipal bonds and income creates an arbitrage opportunity for communities to issue tax-exempt debt and invest in financial assets. I present evidence that suggests the rules in effect prior to 1986 to prevent this activity were not effective. I then develop and estimate a model of municipal bond supply. I find a semi-elasticity of 1.23 of long-term debt with respect to the spread between the after-tax rate of return and the municipal borrowing rate.

OVER 50 years ago, the American economist Henry Simons argued that the federal tax exemption for municipal bond interest income was "a flaw of major importance" (1938, p. 172). Since then, economists have argued the merits of this tax expenditure; politicians for the most part have supported the subsidy, arguing that it is an important component of federal support to state and local governments. Additionally, most state and local politicians have argued that there is a Constitutional right to the tax exemption. This latter argument was decisively rejected in 1988 when the United States Supreme Court ruled that state and local governments had no Constitutional right to issue bonds free from federal taxation. For a brief period, municipal bond prices plunged before Congressional leaders assured traders that Congress had no intention of taxing traditional municipal bonds.[1]

This incident illustrates the sensitivity of municipal bond prices to federal taxation. It reflects in main the important influence of federal taxation on the demand for municipal bonds. There is an extensive literature on the influence of federal taxation on the demand for municipal bonds, and Poterba (1989) provides a good introduction to that literature. This paper considers the role of federal taxation in affecting the supply of municipal bonds. There are two important ways in which federal taxation affects supply. First, the tax-exempt nature of most municipal debt creates opportunities for financial arbitrage as municipal governments can borrow at tax-exempt rates and re-invest at taxable rates, thereby earning an arbitrage spread. While this activity is illegal—and has been for over 20 years—enforcement is difficult and opportunities for evasion persist. Empirical evidence presented below suggests that prior to the Tax Reform Act of 1986 (TRA86), this activity occurred at the municipal level and corroborates evidence presented in Metcalf (1990a) for arbitrage activity at the state level.

Federal tax policy can affect the supply of debt on a second dimension by altering incentives to finance local spending by private versus public borrowing. Viewed in this light, the supply of municipal bonds increases with the spread between the after-tax rate of return on saving for individuals in a community and the municipal borrowing rate. Federal tax policy then has two effects: first, exempting municipal bond income from federal taxation reduces the municipal interest rate and second, taxing the return to savings of residents within a community affects the desired supply of that community's public debt.

In the next section, I review the various ways in which federal tax policy can affect the decision to issue tax-exempt debt at the state and local level. Two factors lead to increased levels of financial assets (and in one case debt also) in a community, while a third factor leads to an increase in debt. With respect to this third

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factor, I present a model of debt finance from which debt supply equations can be derived. In the second section I provide estimates of the parameters of the debt supply model from a panel data set of 185 cities and towns in the United States over an 11-year period from 1980 through 1988. The policy implications of these results are discussed in a concluding section.

1. Factors Affecting the Supply of Municipal Bonds[2]

The tax-exempt nature of municipal debt creates a direct and obvious opportunity for abritrage activity by state and local governments. A municipal government can borrow at the tax-exempt rate (r_m) and invest the proceeds in taxable securities (earning r). Since municipal governments are not subject to federal taxation, the yield spread $r - r_m > 0$ is a pure tax arbitrage gain. Since 1969 this activity has been illegal. Federal regulations have been written, rewritten, and amended on a steady basis since 1972. As noted in Metcalf (1989), it has been difficult to write regulations which eliminate the arbitrage opportunity yet do not create undue burdens and limitations on the financial activities of state and local governments.[3] The major difficulty in enforcing regulations follows from the concept of "replacement." Consider a community which traditionally has paid for capital projects out of general tax revenues but which in a particular year decides to issue a tax-exempt bond for a capital project. Rather than reducing tax collections by the amount of the bond, the community can invest the extra tax revenues in taxable securities. From an accounting perspective, the bond proceeds are being spent on a capital project and no arbitrage is occurring. From an economic perspective, the community has engaged in tax arbitrage. The difficulty is that the bond proceeds have "replaced" the tax revenues which are now available for investment. While the IRS rules explicitly prohibit replacement as a means of evading the arbitrage rules, an accounting system with a multitude of fund accounts creates significant difficulties in linking specific debt proceeds to specific investments.

This difficulty will exist so long as communities both hold assets and issue debt. In FY 1988, cities and towns held $139.8 billion in non-insurance financial assets and had $139.4 billion in long-term debt outstanding, excluding public debt for private purposes. Of this $65.7 billion is long-term full faith and credit debt. Even if governments act in completely good faith to avoid engaging in tax arbitrage, the very fact that financial assets are held while debt is outstanding means that arbitrage profits are being made.[4]

Tax reform (TRA86) created two significant checks on arbitrage. First, volume caps on revenue bonds limit the amount of debt that may be issued and therefore limit arbitrage activity with revenue debt. However, no limits are placed on full faith and credit debt.[5] Second, more stringent arbitrage rules were enacted. However, the experience with previous arbitrage rules should make one cautious in assuming that the new rules will be more effective than previous ones. This view is buttressed by the evidence presented in Metcalf (1990a), where 40 state governments are followed over a seven-year period. In that paper, I find a significant marginal effect of changes in the yield spread ($r - r_m$) on the levels of non-insurance financial assets held by the state government, suggesting that arbitrage rules are not binding at the state level. Later in this paper, I consider whether the rules bind for city governments over a nine-year period prior to TRA86.

A second form of arbitrage which leads to increased asset holdings (though not debt levels) is saving arbitrage. Communities can raise taxes and invest the proceeds in taxable securities. The interest from the investment is then returned in the future in the form of lower taxes. In effect, the community does the saving for the individual earning an additional return of $r - (1 - \tau)r$ or $r\tau$ where τ is the marginal tax rate on interest income to residents in the community. The greater this "interest tax wedge," the greater the incentives to engage in this type of "saving" arbitrage. There are two factors which

reduce the likelihood of communities engaging in saving arbitrage. First, for residents to be willing to forego income today for future income, there must be some link between current fiscal activity and future activity in light of individual mobility. A necessary condition for the arbitrage to work is that there be complete capitalization of the future tax savings into property values (at the local level). Second, the agency problems which might lead to fiscal managers increasing spending rather than reducing future taxes must somehow be overcome. It is not surprising that neither Gordon and Slemrod (1986) nor Metcalf (1990a) found evidence for saving arbitrage in their data sets. When testing for tax arbitrage, I will also consider the possibility of saving arbitrage.

In the introduction, I argued that the supply of municipal debt should be related to the spread between the after-tax rate of return available to residents of the community and the tax-exempt rate that the community must pay on debt it issues. To show this, I present a very simple model in a two-period framework. Consider a community which is choosing to finance public expenditures through a combination of borrowing and taxes. To simplify, assume that the community is made up of N homogeneous individuals with identical preferences modeled by the utility function $U(C_1, C_2, G)$, where C_1 is consumption in the first period and C_2 consumption in the second period. All government spending occurs in the first period (G). Taxes are raised in a lump sum fashion in either period (T_1 or T_2) so that the individual budget constraint is given by

$$C_1 + \frac{C_2}{1+\rho} = Y - T_1 - \frac{T_2}{1+\rho}, \tag{1}$$

where ρ is the taxpayer's after-tax rate of return on saving or borrowing. The government faces the budget constraint that all borrowing must be repaid out of second period taxes:

$$G = B + NT_1 \tag{2}$$

$$(1 + r_m)B = NT_2 \tag{3}$$

where r_m is the municipal borrowing rate. This rate is endogenous and equals the risk free net of tax return that investors can get elsewhere (ν) plus a risk premium. The risk premium depends on the ratio of debt outstanding to some measure of the ability to repay debt in the future (V) along with other attributes (M) of a community which affect its ability to repay debt (e.g. broadness of tax base, scale economies in the production of goods and services):

$$r_m = \nu + \phi(B/V) + \xi(M). \tag{4}$$

Since individuals are identical, the community's choice comes down to maximizing $U(C_1, C_2, G)$ subject to (1)–(4) over the arguments C_1, C_2, G, B, T_1, and T_2. Assuming that there is positive taxation in both periods and positive borrowing in the first period, we can combine the first order conditions for taxes and borrowing to obtain the supply function for municipal bonds in implicit form:[6]

$$1 + \nu + \phi(B/V) + \xi(M) + (B/V)\phi' = 1 + \rho. \tag{5}$$

The marginal cost of an increment of borrowing is the direct cost plus the indirect cost of raising the cost of any additional borrowing that the community might wish to engage in; this is set equal to the marginal benefit which is the decrease in private borrowing required to finance a given level of public spending through taxes. (Private borrowing can take the form of private dissaving). Rewriting (5) slightly emphasizes the fact that the supply of bonds is dependent on the yield spread between the after-tax rate of return available to residents in the community and the municipal borrowing rate:

$$\rho - r_m = (B/V)\phi'(B/V). \tag{6}$$

The after-tax yield spread is a function of the ratio of debt to the measure of the ability to repay. Assuming convexity of the function ϕ, this function can be inverted to yield the debt to tax base measure (B/V) as a function of the yield spread, $\rho - r_m$.[7]

The key role that federal tax policy plays on the supply side is in the after-tax yield spread and more particularly the specification of ρ. I assume that ρ equals $(1 - \tau)r$, where r is the before-tax return to saving (borrowing) by residents of the community and τ is the marginal tax rate on saving (borrowing) by residents. If residents are saving at the margin, public spending can be financed by public borrowing at rate r_m or by drawing down private saving with cost $(1 - \tau)r$. If residents are borrowing at the margin, the cost of private borrowing depends on the borrowing rate as well as the degree of deductibility for interest costs. Prior to the Tax Reform Act of 1986 (TRA86) all interest costs could be deducted, so that the appropriate tax rate would be $p\tau$ where p is an indicator variable equaling 1 when the taxpayer itemizes on her federal tax return and 0 otherwise. After 1986, most interest payments became non-deductible, with the important exception of mortgage and home equity interest. The deduction on home equity loans allowed many homeowners to repackage their consumer debt and continue to receive the deduction. Thus, one should not assume that ρ jumped to r after 1986 for all taxpayers.[8]

Inverting equation (6) above leads to the estimating equation:

$$b_{it} = \beta_0 + \beta_1(\rho - r_m)_{it} + \theta_i + \xi_t + \epsilon_{it}. \quad (7)$$

To control for unobservable "taste" variables specific to each state, I allow for the possibility of an individual (city) effect (θ_i), and to control for cyclical or macroeconomic influences common to all states, I add year dummies (ξ_t). I assume that the error term, ϵ, is independent and identically distributed with mean zero.

This formulation assumes that actual debt levels represent desired debt levels in each year. An alternative formulation would be a partial adjustment model (analogous to the models in the corporate sector, such as Auerbach's (1985) model). Unlike physical capital, however, where there is a cost of adjustment to accumulate or decumulate capital, there are fewer impediments to changes in financial capital. There are call provisions on municipal bonds, opportunities to buy bonds in the open market, and mechanisms for advance refunding of the debt.

This study differs from previous studies of municipal bond supply (e.g. Asefa, Adams, and Starleaf (1981) and Gordon and Slemrod (1986)) in several ways. First, where previous studies have included a municipal borrowing rate as an explanatory variable, they have not accounted for the endogeneity of the borrowing rate. Second, I have a panel data set which allows me to control for city specific influences on debt and asset levels (individual effects) if appropriate. Finally, I use the NBER TAXSIM tax calculator to compute marginal tax rates for different spread variables. In the next section, I discuss the data and estimation results in more detail.

2. Description of the Data and Empirical Results

In this section, I describe a data set on 185 cities and towns in the United States used in this analysis. Financial data come from the *Annual Survey of Governments* conducted by the U.S. Bureau of the Census. Besides data on revenue and expenditures, the survey contains detailed data on financial assets and outstanding debt, as well as debt issued and retired each year. One advantage of this data set is that the Census Bureau makes considerable efforts to construct data records which are comparable across government units. While these data provide a wealth of information about fiscal decisions in a community, one must exercise some care when using them. In particular, the Government Division of the Bureau of the Census uses different data collection methods for large cities (cities with population greater than 300,000) than for smaller cities and towns. The large cities (so-called "jacket" cities) are given greater scrutiny by Census representatives who compile data directly from official accounts and records of the cities. Data from smaller communities are collected primarily by a mail canvass with some follow-up in the field to verify or question particular sta-

tistics. In the regressions which follow, I have data on 185 cities and towns, of which 42 are "jacket" cities.[9]

One complication arising with local data is the existence of overlapping jurisdictions. Cities may have special districts within or across city limits carrying out activities for city residents or have services provided at the county level. Lacking detailed data with which I could allocate debt from other districts to particular communities, I have not corrected for this problem. Differences in allocation of responsibility may be well modeled as a component of an individual effect, however; in that case, I can control for differences across jurisdictions without explicitly measuring them. I can distinguish between traditional full faith and credit debt and revenue debt issued by the community. During the period of this sample, there was an increasing reliance on revenue debt until 1988 when the restrictions on revenue debt imposed by TRA86 began to bite. Below I analyze both full faith and credit debt and total long-term debt.

I examine data on local governments over an 11-year period (fiscal years 1978 through 1988).[10] These data are supplemented by data from Moody's *Bond Record*. From this source, I obtain generalized credit ratings for General Obligation (G.O.) debt for cities and towns in the data set.[11] I then impute to each community a borrowing cost (r_m) equal to the average borrowing cost for a 30-year G.O. bond of that rating at the beginning of the fiscal year. I use the rate on a 20-year Treasury bond at the beginning of the fiscal year as my measure of r.[12]

My first analysis investigates the extent to which arbitrage activity occurred at the margin for city governments prior to TRA86. As shown in Table 1, non-insurance trust financial asset holdings averaged $466 per capita in constant dollars (1982) over the nine-year period from FY 1978 through FY 1988. The city and towns in this sample held roughly half as many financial assets as did state governments over this period.[13] The before-tax yield spread ($r - r_m$) averaged 238 basis points with a standard deviation of 63 basis points. This provides a measure of the return to the tax arbitrage which should result in higher financial asset (and debt) holdings.

I include a measure of the returns to saving arbitrage which I call the "interest tax wedge" ($r\tau$). For τ, I use the NBER TAXSIM tax calculator to compute the marginal tax rate on interest income for a hypothetical family of four filing a joint tax return and not itemizing. I impute to the family the median household effective buying income (EBI) for that community at the beginning of the fiscal year after adding the average of local tax collections. Effective buying income is a measure of disposable income (net of federal state and local taxes). It is calculated by the Market Statistics division of Bill Communications and is published in the *Annual Survey of Buying Power*. This income concept has been used previously by Holtz-Eakin and Rosen (1988) in a different context. This gives a measure (albeit imperfect) of after-federal-tax income. I then program TAXSIM to determine the before-federal-tax income and marginal tax rate which yields that measure of income.[14] As discussed above, the tax wedge should be positively correlated with asset holdings if saving arbitrage is occurring. I also include long-term debt per capita on the right-hand side. Increasing debt increases the legal amount of financial assets a community can hold. A strong test of arbitrage activity, then, is whether communities increase their holdings of financial assets as the yield spread increases holding debt levels constant. The final variable included in the regression is per capita local tax collections (constant dollars) to capture scale effects in the data set. Cities differ in the amount of services they provide to their residents. Cities providing more services may have larger asset holdings unrelated to arbitrage activity (e.g. sinking funds and bond reserve funds). While I would like to include other fiscal and demographic variables in the analysis, they are simply not available for a time series-cross section analysis such as this one. Including individual effects (fixed effects) is an attempt to capture some of the information

TABLE 1
SUMMARY STATISTICS

Variable	Mean	Standard Deviation	Minimum	Maximum
Non-Insurance Financial Assets (Real, Per capita)	465.92	427.14	4.40	4900.00
Before Tax Yield Spread	2.38	.63	.89	3.67
Interest Tax Wedge	2.38	.73	.75	5.05
Tax Collections Per Capita	296.95	179.48	43.06	1268.65
Long term Debt per $1000 of Income	77.78	68.08	.96	700.41
Full Faith and Credit Debt per $1000 of Income	32.61	26.33	0.0	184.25
Revenue Debt per $1000 of Income	45.17	63.15	0.0	695.47
After tax Yield Spread	-.17	.90	-2.20	1.36
Retail Sales per capita	6692.77	2016.32	1529.77	16516.57
Population (x1000)	245.1	311.3	62.5	3364.2

Summary Statistics are over 185 cities and towns for the fiscal years 1978 through 1986 for the first four variables (1665 observations) and for the fiscal years 1978 through 1988 for the remaining variables (2035 observations).

contained in these data; to the extent that the demographic data are slow-moving over time, there should not be significant bias.

As noted in Metcalf (1990a), the yield spread variable is likely to be negatively correlated with the residual in an asset regression. Unobserved factors which may induce higher holdings of financial assets are likely to be positively correlated with the credit rating of the community for a given level of debt. These include such factors as the strength of the local economy, its tax base diversity and employment levels, among other things. To correct for this, I run Two Stage Least Squares (2SLS) regressions using population and retail sales per capita as in-

strumental variables for the credit rating (and thus r_m). Both variables are reported by the *Annual Survey of Buying Power*. Retail sales should be a valid instrument as it measures economic activity within the community and should be negatively correlated with the borrowing rate. Population is included to control for possible scale economies in the community which may affect borrowing costs. Alternatively, large cities typically have larger debt issues which may be more marketable and hence reduce borrowing costs for communities.

Table 2 presents regression results on asset holdings both for the full data set of 185 cities and the 42 largest cities. All regressions include year dummies. The first regression includes dummy variables for individual effects (a fixed effects regression). Including the fixed effects removes the variation in the data across communities. Even so, the results are striking: after controlling for debt, there is a strong response in asset holdings to changes in the yield spread. The coefficient estimate of 1412.5 suggests that a 10 basis point increase in the yield spread leads to a $141 increase in asset holdings (roughly 30 percent of the mean value of asset holdings in the sample). The estimate implies an elasticity of asset holdings with respect to the yield spread of 7.2 (evaluated at the means) and suggests that the arbitrage rules do little to discourage or limit arbitrage activity.

The interest tax wedge coefficient, a measure of the degree of saving arbitrage, enters negatively and is statistically insignificant. (Remember, it should enter positively if saving arbitrage is occurring). There is little support for saving arbitrage activity in this or other regressions. Hence, I drop this variable in further regressions. The coefficient on the tax collections variable enters negatively and is statistically significant. Cities with greater amounts of tax collection hold fewer financial assets. Finally, the coefficient on the debt variables is .40 and strongly significant. A dollar of additional debt leads to an increase in asset holdings of $.40 due to the reserve funds, temporary holding periods, and other legal mechanisms available for holding assets.

The second regression excludes the interest tax wedge variable. Results are little changed with the coefficient on the yield spread variable falling to 1332. Both these regressions suffer from the problem that cross-section variation in the data has been removed in the fixed effects estimation. Since considerable variation occurs across communities in asset holdings, it would be useful to estimate the regressions without fixed effects. I do this in the third column, where I report results from a random effects estimator. This estimator assumes that the individual effects are drawn from a distribution with mean zero and constant variance. Given this error structure, the random effects estimator is a generalized least squares estimator. While the random effects approach is more efficient in that I'm not estimating city specific intercepts, it may lead to inconsistent parameter estimates if the individual effects are correlated with the yield spread variable. Before reporting the results, it should be noted that I tested for the presence of correlated individual effects. The test statistic, denoted T, is reported at the bottom of the fixed effects regressions in the first two columns. The statistic is a Chi-Square random variable with 4 degrees of freedom in the first regression and 3 in the second. In either test, one rejects the hypothesis that the individual effects are uncorrelated with the exogenous variables. This suggests that the random effects estimates are likely to be biased. Keeping that in mind, let us consider the estimates. The coefficient on the yield spread variable drops to 483.6 while the other coefficient estimates are largely unchanged.[15] The sharp fall in the coefficient estimate from that produced in the fixed effects regression is striking. Even if this smaller estimate is correct (and not subject to bias due to the left out individual effects), the arbitrage response is still very large, with an elasticity (estimated at the means) of 2.47.

The last two regressions limit the sample to the 42 "jacket" cities. The coefficients on the yield spread variables are

TABLE 2
REGRESSIONS WITH REAL PER CAPITA NON-INSURANCE TRUST FINANCIAL ASSETS AS DEPENDENT VARIABLE

Variable	(1)	(2)	(3)	(4)	(5)
Before Tax Yield Spread	1412.49 (419.67)	1332.34 (370.18)	483.60 (189.22)	1054.88 (451.47)	289.82 (217.56)
Interest Tax Wedge	-75.32 (46.80)	-	-	-	-
Tax Collections Per Capita	-.64 (.28)	-.63 (.26)	-.10 (.08)	.15 (.34)	.47 (.18)
Debt Per Capita	.40 (.02)	.40 (.02)	.39 (.01)	.41 (.04)	.39 (.03)
R^2	.688	.701	.424	.757	.611
T	10.16 (.038)	10.40 (.015)	-	9.13 (.028)	-
No. of Observations	1665	1665	1665	378	378
Type of Estimator	FE	FE	RE	FE	RE

This table reports results of instrumental variable regressions of non-insurance asset holdings on the variables in the table plus dummy variables for the years. Standard errors are reported in parentheses. Instruments used are population and retail sales per capita. T is a test of the null hypothesis that individual effects are uncorrelated with exogenous variables. The designation FE stands for fixed effects estimator while RE stands for random effects estimator.

similar to those in the full sample regressions and the debt variable coefficients still precisely estimated as .4. However, the tax collection coefficient now switches sign but is insignificant in the fixed effects regression. Again, the test for correlated individual effects rejects lack of correlation between the individual effects and the

exogenous variables at the 5 percent level.

The regressions in Table 2 suggest the following. First, there is evidence of arbitrage activity even after controlling for debt. Using the fixed effects estimate for the full sample (the second regression), the elasticity of asset holdings with respect to the yield spread (measured at the mean values for the data set) is 6.8. Second, there is no evidence of saving arbitrage. Third, a dollar of additional debt leads to a $.40 increase in asset holdings reflecting the legal opportunities for arbitrage available. Finally, correlated individual effects are significant in the asset regressions.

I now move on to measuring debt supply regressions. My measure of b_{it} is outstanding long-term full faith and credit debt (book value) per $1000 of income in community i at the end of fiscal year t. The average amount of long-term debt outstanding for the 185 cities and towns for the 11-year period is $77.78 per $1,000 of income. The majority of this is revenue debt ($45.17) and the rest full faith and credit debt ($32.61).

The average after-tax yield spread is −.17 basis points across the sample with a standard deviation of 90 basis points. That the average after-tax yield spread is negative is troubling. In part this may reflect an upward bias in my estimates of marginal tax rates. On average in my sample, the computed marginal tax rate is 21 percent. But in part, this reflects the high cost of issuing tax-exempt debt in the middle and latter 1980s. Poterba (1989) reports implied marginal tax rates on tax-exempt bonds below 20 percent for the calendar years 1985 and on. There is also a sharp drop in calendar year 1982, which corresponds to my fiscal year 1983 when the after-tax spread fell sharply (Table 1 in Poterba (1989)).

Table 3 presents regressions with long-term full faith and credit debt outstanding per $1000 of income as the dependent variable. Regressions are presented both for the full data set as well as for the 42 "jacket" cities. Right-hand-side variables include the after-tax spread variable in all regressions and year dummies in the fixed effect and random effect regressions. All regressions include instrumental variables (population and retail sales per capita) for the spread variable as noted above. The first two regressions exploit the cross section variation in the data for the 185 cities. The first regression pools the entire data set and includes year dummies to control for supply shocks common to all cities, while the second regression is a cross section of the average observation for each city (averaged over time). In both regressions, the coefficient on the spread variable is roughly 60 and statistically significant. To the extent that cross section regressions measure long-run relationships, these regressions suggest that there is a large and important response of municipal debt supply to changes in the after-tax yield spread. A 10 basis point difference in yield spread leads to an 18 percent change in full faith and credit debt outstanding measured at the mean value of the debt variable in the sample. Regressions 6 and 7 replicate the first two regressions for the 42 large cities in the sample. The estimated coefficients are the wrong sign and have very large standard errors. This suggests that the smaller sample size may not allow precise estimates or that unobserved characteristics of the larger cities may be important.

Before controlling for individual unobserved characteristics, I present two time series regressions (regressions 3 and 8) which average the data for all cities in any year; this may yield insights about the short-run response of debt supply as tax laws have changed. In both the full data set and the smaller data set, the estimated coefficient is roughly 5.2 and is statistically significant to the full cities regression (regression 3). Note that these regressions have 11 observations. Now a 10 basis point change in the yield spread leads to a change in debt supply of nearly 2 percent.

However, neither the time series or cross section variation controls for any unobserved characteristics in the community. The last set of regressions assumes an individual effect for each city. The fourth and ninth regressions are fixed effects regressions, essentially treating the individual effects as city specific intercepts while the fifth and tenth regressions treat the in-

dividual effects as random draws from a distribution with mean zero and unknown variance. Consider first the estimates using the entire sample (regressions 4 and 5). The fixed effects estimate is 20.9 with a two-sided p value of less than 5 percent. I ran a test for the hypothesis that the individual effects are uncorrelated with the instrumental variables and fail to reject uncorrelated individual effects at the 5 percent level. This is reported in the fourth column of Table 3. Given this result, I estimate a random effects model and obtain a slightly higher coefficient estimate of 26.0 which is significant at the 1 percent level. Using the 42 "jacket" cities only, I also fail to reject uncorrelated individual effects and obtain a coefficient estimate of roughly 78 using the random effects estimator. However, this is a less precisely estimated coefficient than is the random effects estimate from the full sample.

What estimate of a supply response should we use given this array of estimates? The two time series estimates give a sense of the short-run response of debt supply to changes in the yield spread. While one must be cautious in interpreting regressions with 11 observations, it is not surprising that the coefficient estimates are much smaller than those estimated using the cross section variation in the data. But even with only 11 observations, the estimates are fairly precisely measured. The best estimate of the supply response to changes in the after-tax yield spread is probably the random effects estimator using the full data set. The coefficient estimate is statistically significant, and the estimator does not remove much of the cross section variation as does the fixed effects estimator yet does account for the fact that there are unobserved differences across cities. That estimate of 26.0 suggests that a 10 basis point change in the yield spread leads to an 8 percent change in full faith and credit debt based on the mean value of full faith and credit debt in my sample. However, if the random effects estimate from the tenth regression is to be believed, large cities respond with greater change in debt holdings to changes in the after-tax yield spread.[16] Whether this difference (if true) is the result of more sophisticated financial management or of a political and bureaucratic difference between large and small cities is beyond the scope of this paper.

Finally, in Table 4 I run the same regressions as in Table 3 with total long-term debt (full faith and credit and revenue) as the dependent variable. The percentage of debt issued as full faith and credit in the sample has fallen from a peak of 63 percent in 1978 to a low of 45 percent in 1987.[17] If revenue debt is substituting for full faith and credit debt, a natural question is whether the after-tax yield spread affects total debt as it does full faith and credit. The regressions in Table 4 suggest that the answer is yes. As in Table 3, both the fixed effects and random effects estimates are statistically significant in the full sample and there is no evidence in support of correlated fixed effects in either the full or restricted sample. Based on the coefficient estimate of 95.6 from the full sample random effects model, a 10 basis point increase in the after-tax yield spread leads to a 12.3 percent increase in total long-term debt. While the mix of debt may be changing over time, the levels of debt respond to changes in yield spread across communities and time. That the supply of municipal bonds responds to changes in the after-tax yield spread has policy implications that I discuss in the last section.

3. Implications and Conclusions

In summary, the data on municipal governments for the period of the late seventies and most of the eighties suggest the following: first, that the arbitrage rules prior to TRA86 have not been effective in eliminating tax arbitrage at the margin. Second, there is little evidence that saving arbitrage is occurring, whereby residents save through their community to earn a before market rate of return. Finally, there is evidence that federal tax policy influences the desired supply of tax-exempt bonds in communities as well as

TABLE 3
DEBT REGRESSIONS USING FULL FAITH AND CREDIT DEBT

	Coefficient on After Tax Spread (Standard Error)	Type of Estimator	R^2	T (P-Value)	Number of Observations
(1)	59.48 (6.68)	XS	.031	-	2035
(2)	63.67 (19.94)	B	.042	-	185
(3)	5.23 (2.32)	TS	.234	-	11*
(4)	20.90 (9.88)	FE	.783	3.69 (.055)	2035
(5)	25.99 (9.07)	RE	.037	-	2035
(6)	-143.37 (154.67)	XS	.005	-	462
(7)	-34.68 (141.21)	B	.005	-	42
(8)	5.15 (2.91)	TS	.061	-	11**
(9)	79.75 (31.15)	FE	.564	3.44 (.064)	462
(10)	78.13 (43.00)	RE	.011	-	462

This table reports results of instrumental variable regressions of full faith and credit debt on the after tax yield spread and dummy variables for the years. The instruments used are population and retail sales per capita ($1982). T is a test of the null hypothesis that individual effects are uncorrelated with the exogenous variables. It is distributed as a chi square random variable with 1 degree of freedom.

Under the heading "Type of Estimator", the codes stand for the following: XS - cross section over all states and time periods, B - cross section over the cities using the average of observations across time, TS - time series over the 11 years using the average of the observations across cities in any year, FE - fixed effects regressions, and RE - random effects.

* - each observation is an average over the 185 cities

** - each observation is an average over the 42 "jacket" cities

TABLE 4
DEBT REGRESSION USING TOTAL LONG TERM DEBT

	Coefficient on After Tax Spread (Standard Error)	Type of Estimator	R^2	T (P-Value)	Number of Observations
(1)	37.13 (30.42)	FE	.697	.35 (.554)	2035
(2)	95.60 (25.53)	RE	.023	-	2035
(3)	155.78 (76.89)	FE	.634	.08 (.777)	462
(4)	137.49 (36.31)	RE	.096	-	462

This table reports results of instrumental variable regressions of long term debt on the after tax yield spread and dummy variables for the years. See table 3 for list of instruments and other details of regression output.

affecting the demand for those bonds by investors. For full faith and credit debt, I estimate a semi-elasticity of bond supply with respect to the after-tax yield spread of .80. For total long-term debt, the semi-elasticity estimate is 1.23.

In a federal system such as that found in the United States, the justification for a subsidy of the form given by municipal bonds should arise from efficiency or distributional concerns. With respect to efficiency, there may be some concern that there is a suboptimal amount of real investment in the state or locality due to beneficial spillovers on other communities or states. With respect to distributional concerns, there may be efforts to redistribute income among communities or states. The research reported above suggests that the exemption from federal taxation of municipal bond interest income is likely to improve neither of these objectives. Efficiency won't be enhanced if the bonds are simply being used to finance private borrowing and saving at preferential rates.

The municipal bond subsidy also has a perverse distributional effect. With a progressive income tax system, the implicit tax on municipal bonds is typically less than the marginal tax rate on interest income of the holders of the bulk of municipal bonds. Therefore, a substantial fraction of the subsidy is diverted from communities to high tax rate holders of the bonds. This is likely to be a problem even after the substantial flattening of the U.S. rate structure after TRA86. While top marginal tax rates are currently 28 (or 33) percent, the implicit tax rate on municipal bonds has hovered around 17 or 18 percent over the past few years.[18]

Any plan to alter the subsidy to state and local governments through the exemption of municipal bond interest income from federal taxation must take into account the behavioral response of local governments implied by the statistical findings above. Inelastic supply of bonds means that reducing the yield spread toward zero will not change the stock of municipal bonds—for supply reasons. The coefficient estimates reported here suggest that there would be a substantial decrease in the desired supply of tax-exempt bonds if the spread were driven toward zero.

What this paper does not provide is a political economy explanation for the strong support for tax-exempt municipal

bonds despite their efficiency and equity deficiencies. One possible explanation which deserves investigation is that the tax exemption is a form of pre-commitment from the federal government to state and local governments that other subsidies do not provide. The challenge for policymakers is to construct some other subsidy instrument which yields the same level of subsidy protection while reducing the efficiency and equity costs of the existing system.

ENDNOTES

[1]For an analysis of the market's reaction to South Carolina v. Baker, see Poterba (1989). An historical summary of the erosion of the tax exemption of certain types of municipal bonds is given in Davie and Zimmerman (1988).

[2]This section draws on my previous papers, Metcalf (1989, 1990b), as well as an important paper by Gordon and Slemrod (1986).

[3]Of course, arbitrage could be eliminated either by allowing state and local governments to invest only in tax-exempt securities or by eliminating the tax exemption for municipal debt. To date, Congress has not chosen to attempt either of these solutions to the arbitrage problem.

[4]However, some of the holdings are in special federal securities called SLUGs, which are specifically designed below market rate Treasury bonds created to allow state and local governments to bank bond proceeds without earning arbitrage profits. Unfortunately, I am not able to obtain data on municipal holdings of SLUGs in the empirical analysis which *follows*.

[5]Revenue debt is debt backed by the revenues of particular projects (e.g. ticket sales from a sports arena). General obligation debt is debt backed by the full taxing authority of the issuing jurisdiction. This is typically the safest debt issued by a government. Full faith and credit debt (FFC) is a slightly broader category than general obligation debt. It includes G.O. debt but also includes any debt payable from nontax sources but which represent liabilities to the community if the nontax sources are insufficient to cover required debt payments.

[6]To be precise, this is the equilibrium relationship between r_m and B.

[7]Alternatively, (5) can be inverted to yield B/V as a function of $\rho - \nu$ and M. The advantage to this approach is that $\rho - \nu$ is exogenous whereas $\rho - r_m$ is not. However, the other measures that affect the credit quality of debt (M) must be included in the regression explicitly. The credit rating, though, is a useful summary measure of these other factors affecting credit quality and I proceed by using the credit rating explicitly and using an instrumental variable approach to control for the feedback from B/V to r_m.

[8]Even if consumer debt repackaging had not occurred, there would not have been a discontinuous jump in any case as the interest deduction was phased out over time.

[9]For the 11-year period of my sample, I had data for 230 cities and towns. There were 45 communities for which data were either missing or inaccurate, leaving me with 185 communities.

[10]For the arbitrage regressions, I have nine years of data. Data on non-insurance financial assets were not available for fiscal years 1987 and 1988 as of yet.

[11]The ratings I use to impute borrowing costs are ratings on overall city or town credit as opposed to a rating on a specific bond issue.

[12]I use a 20-year rate rather than a 30-year rate to control in part for the callable nature of most municipal debt.

[13]In what follows, financial assets will refer to non-insurance trust financial assets. The insurance trust assets are primarily held for pension funds. As noted in Metcalf (1990a), it is difficult to disentangle arbitrage activity in the pension funds from shifts in the timing of wage payments through over and underfunding. However, one should note that prior to TRA86, it was legal to issue tax-exempt bonds and use the proceeds to purchase non-taxable annuities for pension funds. Hence any evidence in this data set of arbitrage probably understates the true extent of the activity prior to Tax Reform.

[14]This measure ignores important heterogeneity across families in family size, home ownership status, etc. It also underestimates before-tax income by ignoring state tax collections. On the other hand, it also overestimates before-tax income by imputing all local taxes to the resident.

[15]I get very similar estimates from running the regression without random effects. I also considered the time series variation in the data, running regressions on the means of the observations for each of the nine years. However, with so few observations, I get extremely imprecise estimates. Hence I do not report them here.

[16]This is also true in percentage terms. A 10 basis point change in the yield spread is associated with 19 percent change in debt (measured at the mean of the *sample*).

[17]In 1988, the fraction of debt issued as full faith and credit rose by slightly more than one percentage point in my sample.

[18]Note, though, the findings of Feenberg and Poterba in this issue. They find that the subsidy diversion has been substantially reduced since TRA86.

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