MONEY, THE MONETARY BASE, AND NOMINAL GNP

William E. Cullison

The relationship between money and nominal GNP has been generally stable, financial innovations notwithstanding, although the relationship between the monetary base and nominal GNP has been slightly more predictable.

Recently, a number of influential policymakers have argued that innovations in the means of making payment have changed past relationships between the money supply and aggregate income (see, for example, Morris [18], Solomon [20], and Wallich [21]). These policymakers have asserted that financial innovations such as NOW accounts, money market mutual funds, customer repurchase agreements, and deposit-sweeping arrangements obscure the relationship between a narrowly defined monetary aggregate such as M1[1] and money balances held for transactions purposes. The apparent plausibility of this view has spawned the corollary notion that M1 should be replaced as a primary intermediate target[2] for monetary policy. Frank Morris, President of the Federal Reserve Bank of Boston, for example, advocates total liquid assets (L) as an "intermediate goal" [18, p. 9]. Others have simply advocated that the FOMC be flexible in choosing which aggregate to target.

The first purpose of this paper is to investigate whether financial innovations have indeed obscured the relationship between narrow monetary aggregates and nominal income. On the basis of the empirical evidence, the article shows that contrary to popular opinion, financial innovations did not have a substantive effect on the relationship between M1 and GNP over the period examined, 1959 to 1981 (except possibly for the three-year period from 1975 to 1978).

This is, of course, not to say that financial innovations might not change the historical relationship of M1 to GNP at some future date. But even if that relationship changed, there is another money/income relationship, namely that between the monetary base[3] and GNP, that is thought to be relatively immune to financial innovations (see Meltzer [17] for a succinct explanation of this assertion). Therefore, a second purpose of this paper is to examine the monetary base to see whether it has potential as an intermediate target for monetary policy. Several recent studies have tended to dismiss the monetary base as an intermediate target on the grounds that M1 has borne a closer empirical relationship to GNP over the years than has the monetary base. This article reexamines the evidence and concludes that the base actually bore a slightly more predictable relationship to GNP than did M1.

Narrowly Defined Monetary Aggregates As Targets for Monetary Policy Milton Friedman has argued that ". . . the monetary authority should guide itself by magnitudes that it can control, not by ones that it cannot control" [7, p. 486]. Broad aggregates like total liquid assets are not (in practice) controllable through the reserve base, whereas narrowly defined aggregates can be controlled through the monetary authority's control over bank reserves, the basis for monetary expansions and contractions.[4]

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[1] M1 is currently defined to include currency and coin, demand deposits, traveler's checks, and NOW accounts. This sum was named M1B in 1981. For simplification, whenever M1 is referred to in this article, the current definition will be relevant.

[2] Under current operating procedures, nonborrowed reserves are used as the operating target. M1, therefore, is called an intermediate target—i.e., intermediate between nonborrowed reserves and nominal GNP.

[3] The monetary base is defined as the sum of reserves held at the Federal Reserve and currency and coin outside the Federal Reserve System and the Treasury. It is adjusted for changes in reserve requirements. In all subsequent discussion of the monetary base in this article, the figure referred to will be the monetary base as constructed by the Federal Reserve Bank of St. Louis.

[4] See Goodfriend [10] for an analysis of this issue. Contemporaneous reserve accounting, of course, is a necessary precondition for direct control of money through the reserve base.

Broad monetary aggregates are (in practice) controllable, if at all, only through measures designed to affect interest rates. As a result, they are subject to considerably larger targeting errors than are narrowly defined monetary aggregates. Also, in attempting to stabilize a broad aggregate by reacting to changes in the demand for credit, the monetary authority may actually destabilize the economy. This perverse outcome may come about because the monetary authority may misperceive the lag between a policy action and the subsequent impact of that action on the economy. Friedman [7] has noted that, for this reason, past Federal Reserve actions designed to stabilize the economy have nearly always proved to be destabilizing.

The argument that financial innovations can cause loss of control of monetary aggregates is not new. On the contrary, it represents a resurrection of the well-known Gurley-Shaw thesis [12] that was discussed widely in the economics literature in the late 1950s. This thesis held that near-monies such as deposit liabilities of savings and loan associations, savings banks, and other financial intermediaries—which were outside the jurisdiction of the Federal Reserve System—rendered monetary policy per se useless as an anti-inflationary weapon. In particular, Gurley and Shaw argued that the Federal Reserve could not stop inflation because it could not control nonbank financial intermediaries and thus could not limit the creation of near-monies that were regarded as effective substitutes for M1. Accordingly, the issue in the fifties was, as it is today, whether monetary control is feasible in a financial system that can produce an endless array of money substitutes, i.e., whether an easily controllable monetary aggregate such as M1 (or the monetary base) could be used to control the entire credit superstructure and therefore total spending.

Both the Gurley and Shaw thesis and the current financial innovations argument can be tested empirically. Both propositions imply that the relationship between money and nominal GNP is extremely variable and unpredictable. Equivalently, the financial innovation theses imply that the income velocity of money, far from being stable, is a will-of-the-wisp. (By definition, MV ≡ GNP, where M is a monetary aggregate and V is the income velocity of that monetary aggregate.)

The simplest and most straightforward way of examining the relationship between money and GNP is to regress the percentage change in GNP on the percentage change in the monetary aggregate (see Friedman and Meiselman [8]). This method is employed below. Before presenting the model, however, a word of caution is in order. Results from single-equation regression models are always subject to potential statistical difficulties. Even so, the evidence reported below is sufficient to demonstrate that (1) the relationship between M1 and GNP has been generally stable except for one three-year period, and (2) the monetary base has also borne a stable relationship to GNP. The analysis will proceed by first examining the relationship of M1 to GNP and, subsequently the relationship of the monetary base to GNP.

The Relationship of M1 to GNP: The Empirical Evidence Countless analyses of GNP and M1 have been undertaken for different reasons since Friedman and Meiselman. One recent analysis performed by Richard Davis [5] is shown below. Davis used a single-equation model of the form,

$$g_t = \alpha + \sum_{i=0}^{4} \beta_{t-i} \cdot m_{t-i},$$

where g is the percentage change in nominal GNP and m is the percentage change in M1. This model is employed in the present article.[5]

Parameter estimates in a model such as this will be influenced by the state of the economy at the end of the estimation period. As Friedman concluded on the basis of an extensive historical study, ". . . income velocity tends to rise during cyclical expansions when real income is rising and to fall during cyclical contractions when real income is falling" [6, p. 329]. Consequently, in order to minimize possible bias from that source, the regression coefficients for this study were always estimated over a period from one quarter before the peak of one business cycle (as defined by the NBER) to one quarter before the peak of another.[6]

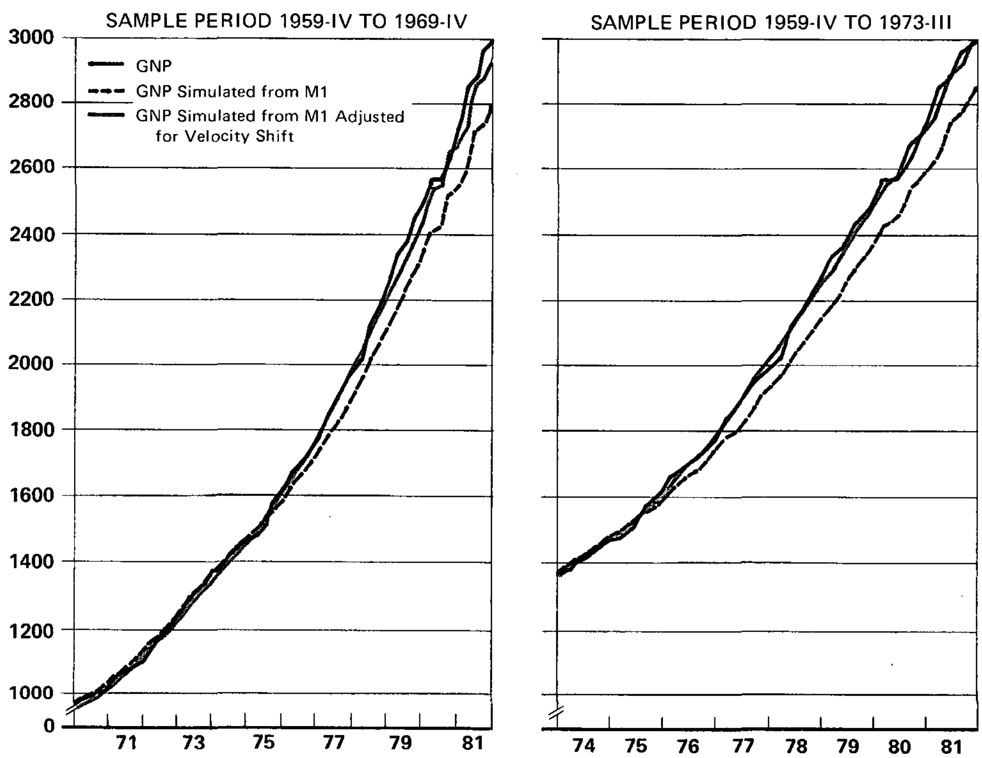
The results of the regressions estimated (with quarterly data) from 1959-IV to 1969-IV, 1959-IV to 1973-III, and 1959-IV to 1979-IV are shown in Table I. In-sample results by themselves, while of some interest, cannot give much information about the long-run stability of M1 velocity. Thus, the equations were simulated dynamically from the fourth

[5] The Davis equation was used as a model for the analysis because he concluded that M1 was more closely related to GNP than was the monetary base. This article subsequently examines that question, as noted before, and his form of the equation will be used to evaluate his conclusion. The equation was estimated with unconstrained lags.

[6] See Cullison [4] for an example of the pitfalls that can be associated with disregarding Friedman's advice.

Chart 1

GNP AND OUT-OF-SAMPLE SIMULATIONS OF GNP FROM M1 AND M1 ADJUSTED FOR THE 1975 – 1978 VELOCITY SHIFT



quarter of 1959 through fourth quarter of 1981.[7] Chart 1 shows the resulting out-of-sample forecasts (from 1959-69 and 1959-73 data) plotted against actual GNP. As is apparent, the equations predicted nominal GNP fairly accurately until the second quarter of 1975, when velocity growth rose as the economy moved out of the recession. The simulation began to track the changes in the actual GNP again in 1978,[8] although simulated GNP was at a lower level.

After an ad hoc adjustment was made to simulated GNP to account for the 1975-78 velocity shift, the forecasts came back on track. The adjustment involved adding 0.5 percent per quarter to the percentage change in nominal GNP over the period from 1975-II to 1978-II. Chart 1 also shows the out-of-sample simulation of GNP forecasted from M1 with that adjustment, and Table II reports the forecast errors in the out-of-sample period. As the table shows, the simulations adjusted for the velocity shift missed actual fourth quarter 1981 GNP by only $34.1 billion (1.1 percent) in the simulation from the parameters estimated from 1959-69 data and only $2.3 billion (0.08 percent) in the simulation based upon 1959-73 data. Considering that these were dynamic simulations with the only actual GNP data entering the forecasts coming in 1959-IV (the beginning of the simulation), the closeness of the forecasts to actual GNP in the post-sample period is striking.

During 1981, nationwide NOW account ownership was authorized and NOW accounts, a component of M1, grew rapidly. At the same time, the economy experienced an immense increase in outstanding shares of money market mutual funds. Money market mutual fund shares, while checkable (under certain restrictions), are not included in M1. Could the relationship of predicted to actual GNP have remained so close in 1981 if financial innovations had obscured the relationship of M1 to GNP?

[7] In the dynamic simulations, the regression equation predicts the percentage change in nominal GNP. Actual GNP in the beginning period is used as the base and never again enters the simulation.

[8] Thanks are due Stephen Hale for pointing this out to me.

During 1981, the Federal Reserve paid close attention to a monetary aggregate denoted shift-adjusted M1B—i.e., M1 adjusted to remove any shifts from time and savings deposits into NOW accounts. That aggregate was also tested in the simulations of GNP in 1981. The NOW-shift adjusted simulation gave considerably poorer results than did the simulation based upon actual M1.[9] This result was somewhat puzzling, for it implied that the NOW-shift adjustment was faulty and that M1 (then M1B) continued to be the appropriate measure of transactions accounts. One explanation may be that money market mutual funds were absorbing funds designed for savings whereas NOW accounts were absorbing transactions balances. The 1981 experience does illustrate that financial innovations have made monetary targeting more difficult.

These simulation results, in any event, are not consistent with an unstable and unpredictable relationship between M1 and GNP. Therefore, econometric money demand equations that are unstable and unpredictable may well be misspecified. (See also Hafer and Hein [13] and Hetzel [14] who reach similar conclusions using different methodology.) Marvin Goodfriend [11] has shown that there are

[9] The root mean squared error for 1981 of GNP simulated from M1 was $44 billion compared to $76.5 billion for GNP simulated from M1 adjusted for the shift into NOW accounts. Additionally, the geometric averages of the quarterly percentage changes in actual GNP and the GNP simulations for 1981 were 9.69 percent for actual GNP, and 10.2 percent for GNP simulated from M1, but only 7.5 percent for GNP simulated from "shift-adjusted M1B."

Table I

RESULTS OF REGRESSIONS OF GNP ON THE ST. LOUIS MONETARY BASE AND M1*

(All variables are represented as quarter-by-quarter percentage changes. All lags are unconstrained.)

Regression Equation, $g = \alpha + \sum_{i=0}^{4} (\beta_{t-i}) \cdot (M_{t-i})$

Sample Period, 1959-IV to	α	β_t	β_{t-1}	β_{t-2}	β_{t-3}	β_{t-4}	R^2†	dw	SEE
(M is equal to M1)									
1. 1969-IV	.86 (3.78)	0.554 (2.77)	−0.350 (−1.42)	0.664 (2.70)	0.213 (0.86)	−0.19 (−0.91)	0.36	1.30	0.00656
2. 1973-III	.8 (3.39)	0.571 (3.09)	−0.208 (−0.90)	0.44 (1.88)	0.289 (1.23)	−0.108 (−0.56)	0.364	1.93	0.0072
3. 1979-IV	.74 (3.34)	0.525 (3.14)	0.002 (0.01)	0.403 (1.96)	0.313 (1.50)	−0.192 (−1.11)	0.371	1.96	0.0078
(M is equal to the monetary base)									
4. 1969-IV	.7 (2.56)	0.253 (0.83)	0.103 (0.29)	0.663 (1.79)	0.110 (0.30)	−0.269 (−0.89)	0.23	1.60	0.0072
5. 1973-III	.7 (2.43)	0.445 (1.64)	−0.047 (−0.15)	0.470 (1.47)	0.380 (1.19)	−0.325 (−1.21)	0.28	1.88	0.0076
6. 1979-IV	.6 (2.41)	0.400 (1.58)	0.126 (0.44)	0.276 (0.96)	0.547 (1.91)	−0.400 (−1.61)	0.29	1.80	0.0083

Regression Equation, $g = \alpha + \sum_{i=1}^{2} (\beta_{t-i}) \cdot (BASE_{t-i})$

(Form preferred for analyzing relationship between monetary base and GNP)

Sample Period, 1959-IV to	α	β_{t-1}	β_{t-2}	R^2†	dw	SEE
7. 1969-IV	.76 (3.02)	0.226 (0.80)	0.630 (2.23)	0.27	1.54	0.0070
8. 1973-III	.78 (3.113)	0.226 (0.85)	0.621 (2.34)	0.27	1.90	0.0077
9. 1979-IV	.76 (3.02)	0.368 (1.49)	0.506 (2.06)	0.26	1.89	0.0085

* Numbers in parentheses represent "t" statistics.

† Corrected for degrees of freedom.

Table II

OUT-OF-PERIOD FORECASTING ERRORS FOR QUARTERLY GNP FROM DYNAMIC SIMULATIONS ENDING IN 1981-IV**

Date Forecast Began	Actual 1981-IV GNP Less Estimated GNP	Number of Out-of-Sample Quarters	Root Mean Squared Error
	$ billions		$ billions
1970-I			
M1 (Eq. 1)*	176.7	48	119.9
M1 Adjusted†	34.1	48	29.3
Monetary Base (Eq. 4)*	57.3	48	31.2
Monetary Base (Eq. 7)*	43.8	48	35.0
Trend Alone	−446.3	48	156.0
1973-IV			
M1 (Eq. 2)*	142.0	33	98.5
M1 Adjusted†	−2.3	33	23.0
Monetary Base (Eq. 5)*	47.1	33	36.9
Monetary Base (Eq. 8)*	45.6	33	41.2
Trend Alone	−374.8	33	147.4
1980-I			
M1 (Eq. 3)*	−37.8	8	34.7
Monetary Base (Eq. 9)*	−21.5	8	32.9
Trend Alone	−158.5	8	41.6

* Equation numbers refer to regression equations in Table I from which simulations were made.

** All dynamic simulations began on 1959-IV. Forecast errors, however, include only those errors that began after the estimation period.

† Adjustment on the M1 simulations adds 0.5 percent (2 percent annual rate) per quarter to the simulated change in GNP over the 1975-II to 1978-II time period.

sound theoretical reasons to believe that conventional money demand equations are indeed misspecified.

There remains, however, the troublesome fact that the rate of growth of income velocity of M1 apparently did increase in the 1975-78 period. One can adjust for such shifts on an ex post basis, but if an aggregate is to be an appropriate target for monetary policy, such shifts should be predictable ex ante. Fortunately, there is another narrowly defined monetary aggregate whose relationship to nominal GNP did not shift through the fourth quarter of 1981—one that is amenable to control by the monetary authority. That variable is the monetary base.

The Relationship of the Monetary Base to GNP

Three Federal Reserve articles have recently considered the monetary base as a policy target (John Carlson [2], Richard Davis [5], and Carl Gambs [9]). Their conclusions were generally unfavorable toward the base, although all agreed that the base could be better controlled than other monetary aggregates, even under current institutional arrangements. All three articles concluded that the empirical evidence weighed against using the monetary base as a policy target because it was not as closely related to nominal GNP as was M1. In addition, the studies enumerated several theoretical reservations against targeting the monetary base. This article will focus primarily on the empirical arguments against the base, although the theoretical reservations voiced in the articles will be discussed.

The Davis and Gambs studies (which contained the empirical work) reach the conclusion that the base is less closely related to aggregate demand by comparing the correlation coefficients of regressions of nominal GNP on money with those of GNP on the monetary base. There are slight variations in techniques, but each used current and lagged values of the monetary variables to estimate his single-equation model. As is shown in Table I, this article's regressions of GNP on the base and on M1 were consistent with the result found by Davis and Gambs, namely that the multiple correlation coefficients of the M1/GNP regressions were higher than the multiple correlation coefficients of the base/GNP regressions. Both papers, however, gave insufficient attention to a very important criterion, forecast performance in out-of-sample simulations. That omission is illustrated in Chart 2.

Chart 2 shows actual GNP and two dynamic simulations of GNP in out-of-sample periods. One is based upon a regression of GNP on M1 (without adjustment for the 1975-78 velocity shift). The other is based upon a regression of GNP on the monetary base. The chart shows out-of-period forecasts from parameters estimated from 1959-IV to 1969-IV, and from 1959-IV to 1973-III. GNP simulated from M1 began to go off track in the second quarter of 1975, but GNP simulated from the monetary base continued to track nominal GNP through the fourth quarter of 1981. This result contradicts Davis and Gambs's conclusion that the monetary base is less closely related to GNP than is M1. Note that this contradiction occurs even though the multiple correlation coefficients were consistently higher for the regressions of GNP on M1 than for the regressions of GNP on the base.

Chart 2

GNP AND OUT-OF-SAMPLE SIMULATIONS OF GNP FROM M1 AND THE MONETARY BASE

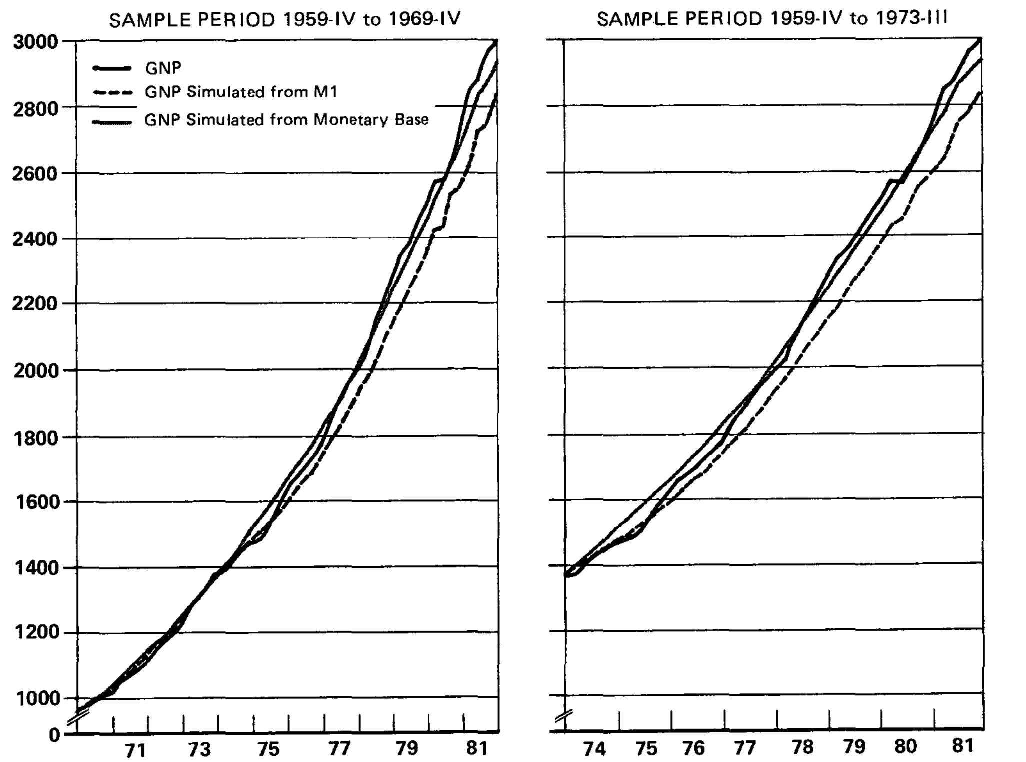


Table II provides measures of the dynamic tracking ability of the out-of-sample simulations. As the table shows, GNP simulated from the monetary base always ended up closer to actual fourth quarter 1981 GNP than GNP simulated from M1 (not adjusted for the velocity shift). The root mean squared error (RMSE),[10] a measure of overall forecasting error reported in Table II, also shows smaller errors for the simulations derived from the monetary base.

The monetary base/GNP equation was specified to conform to Davis's analysis. Having no further need of this specification, the monetary base/GNP relationship was reestimated using a different lag structure. This preferred equation regresses the percentage change in GNP on percentage changes in the monetary base over the two previous quarters. The results are reported in Table I, and simulations from them are evaluated in Table II.

[10] The RMSE is defined as the square root of the sum of the squared forecasting errors divided by the number of forecasted periods. The squaring procedure not only prevents negative errors from offsetting positive errors in the summing up, but it also penalizes large errors more than proportionately.

This particular lag structure was chosen because it avoids current period relations between the base and GNP, and it is relatively uncomplicated. Avoidance of contemporary relationships between GNP and the base is predicated upon the assumption that changes in the monetary base affect GNP only after a lapse of time. As can be seen from Table I, in the preferred lag form the bulk of the effects of changes in the base on GNP take place with a two-quarter lag.

The out-of-sample forecasting errors (in percentages)of the simulations of GNP from the preferred monetary base equation, from M1, and from M1 adjusted for the velocity shift are shown in Chart 3. (The relative accuracy of the forecasts is more apparent from percentage errors than from levels.) GNP simulated from M1 adjusted for the velocity shift outperformed GNP simulated from the monetary base during the 1973-76 period, although they were virtually identical before and after. The simulation from the monetary base, however, substantially outperformed the simulation from unadjusted M1. Thus, the simulation from the base did not predict the 1973-75 recession, while the simulation from M1 did not pick up the changing trend in income velocity in the 1975-78 period.

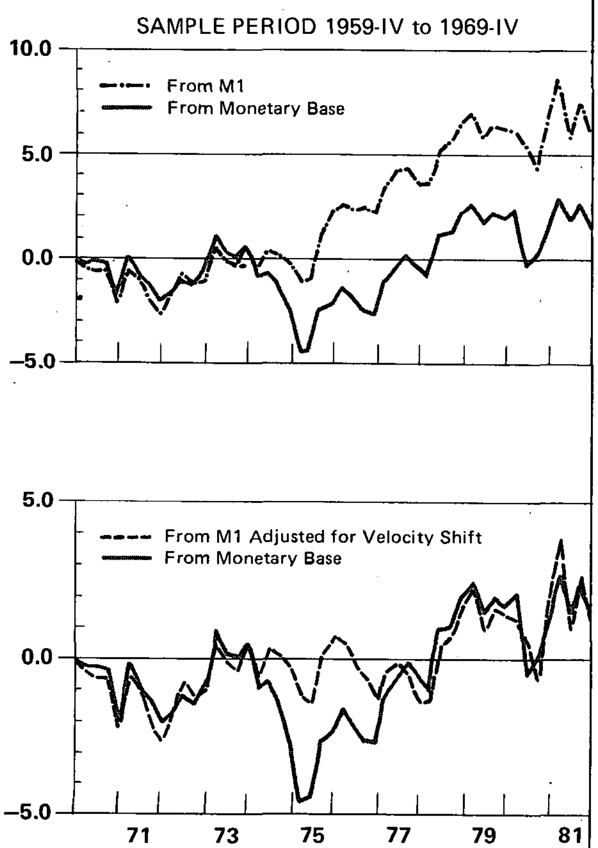
The failure of the base to predict the 1973-75 recession represents a shortcoming in its feasibility as a target for monetary policy. Note, however, that the base came back on track after the recession with no ad hoc adjustment, and it did not mispredict the other out-of-sample recessions. The explanation may lie in the character of the 1973-75 recession, which began with the oil embargo and was influenced throughout by energy supply effects. If, as many economists believe, the base is less influenced by feedback from GNP than is M1,[11] M1 might be expected to show the effects of the 1973-75 recession more closely than would the monetary base.[12] The failure of the base to predict the 1973-75 recession, however, should provide a caution to anyone relying solely upon it as a forecasting tool.[13]

Chart 4 shows actual GNP plotted against simulations of GNP from the preferred monetary base equation and from GNP's own trend. The chart clearly shows that GNP simulated from the trend extrapolation is subject to substantially higher forecast error than GNP simulated from the monetary aggregates. Table II, which reports the out-of-period forecasting statistics, confirms this visual observation.

Are Monetary Aggregates Endogenous or Exogenous? The empirical relationship between the monetary base and GNP has been dismissed by some analysts on the ground that the base is endogenous to GNP (i.e., that the base responds to changes in GNP rather than vice versa). This contention is difficult to resolve. Like the money supply, the monetary base has passed statistical causality tests that indicate that the monetary aggregates add some-

Chart 3

PERCENTAGE ERRORS IN PREDICTING GNP FROM M1 AND THE MONETARY BASE



[11] This paper's regression results reported in Table I indicate that the contemporaneous relationship between M1 and GNP was more pronounced than the relationship between the base and GNP. The result is consistent with, but no proof of, the explanation advanced above.

[12] If adjustment of currency holdings is more costly than adjustment of demand deposits, the result can also be consistent with a transaction cost analysis of the demand for money that distinguishes between transitory and permanent input variable changes (see, for example, Goodfriend [11]). Such analysis would seem to be able to rationalize why M1 should track transitory changes in income better than the monetary base.

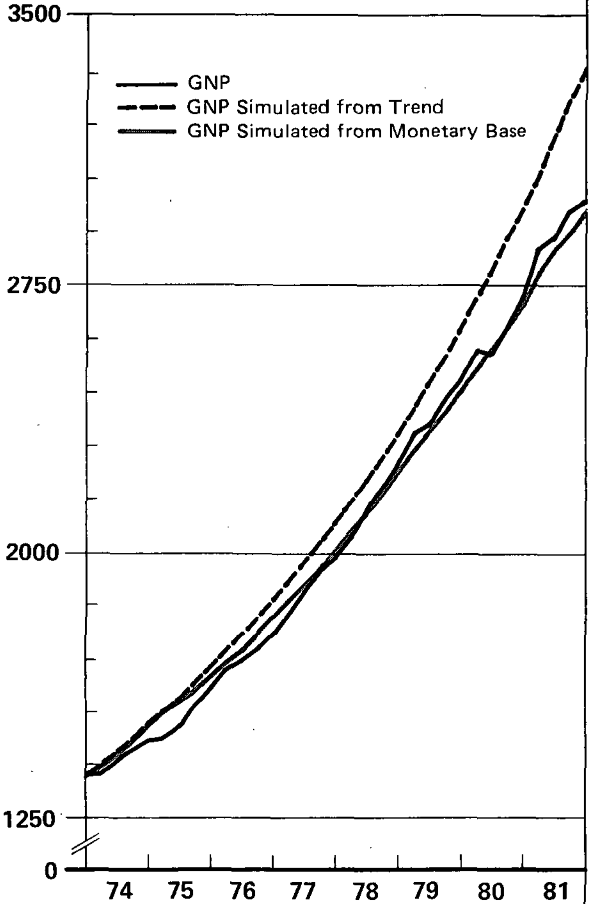
Finally, the explanation might be advanced that inherent technical problems related to the composition of the base caused the base to mispredict the 1973-75 recession. It is indeed true that a trend toward a larger proportion of currency in the base seemed to begin somewhere around 1973. As a result, currency increased from 65.8 percent of the base in the fourth quarter of 1973 to 68.8 percent by the third quarter of 1976, a rate of increase of approximately 0.4 percent per quarter. This trend in the currency composition of the base has continued since that time, however, although at a slower 0.2 percent per quarter rate. Given, however, that the predictions from the base came back on track of their own accord even though the composition of the base was continuing to change; and given that the parameters of the equations from which the simulations were made were estimated during a period in which there was very little change in the composition of the base (0.06 percent per quarter), compositional changes seem an unlikely explanation of the miss in the 1973-76 period.

[13] In comparison to the forecasters reported by Stephen McNees [15] in his article evaluating forecast performance over the 1976-II to 1980-III period, however, the simple base equation estimated over the 1959-IV to 1973-III period (Equation 8 in Table I) performed respectably. In terms of one-quarter forecast horizons, the average absolute error from the base equation turned out to be 3.27 percent, measured at a compound annual rate, which was lower than nine of the sixteen average forecast errors reported by McNees. Using the base equation to forecast two quarters out (this could be done by assuming that the rate of growth of the base in time period t−1 was the same as in t−2, which would be known), the average absolute error turned out to be 2.4 percent, which was as low as that of any forecaster reported in McNees's article.

Chart 4

GNP AND OUT-OF-SAMPLE SIMULATIONS OF GNP FROM TREND AND THE MONETARY BASE

SAMPLE PERIOD 1959-IV TO 1973-III



thing to predicting GNP whereas GNP adds little or nothing to predicting the monetary aggregates. The power of these tests is somewhat limited, however, for there is a contemporaneous relationship between both aggregates and GNP (although Table I shows the contemporaneous M1/GNP relationship to be more pronounced).

To illustrate this problem, suppose the Federal Reserve System were using an interest rate target while nominal GNP and hence demands for liquidity were rising rapidly. In this case, interest rates would be under upward pressure, so the System would provide reserves to keep short-term rates down. Increases in nominal GNP would thus be correlated with concurrent increases in the monetary base and M1. A monetarist would argue that these contemporaneous changes in the monetary aggregates would have feedback effects on GNP that would show up a few months later.

The endogeneity argument provides an additional reason to prefer a base/GNP regression that avoids the contemporaneous relationship. It must be acknowledged, however, that avoidance of the contemporaneous relationship does not answer the endogeneity charge, for the one- and two-quarter lags could be providing a proxy for concurrent changes.

Some simple tests were run to show that the fit of the regression of GNP on GNP lagged one, two, and three periods was substantially improved by adding the base lagged one and two periods, and that the fit of a regression of the monetary base on the monetary base lagged one period was not significantly improved by adding GNP lagged one and two periods. The results of these regressions are reported in the appendix, along with measures of their out-of-sample forecasting accuracy. These results are all consistent with a causal relationship running from the monetary aggregates to GNP. Other studies have come to similar conclusions (see Cagan [3], Hetzel [14], Mehra [16], and Sims [19]). Because of the contemporaneous relationship between GNP and the monetary aggregates, however, the direction of causation cannot be conclusively demonstrated by analyses such as these.

Conceptual Reservations to Using the Base As a Target for Monetary Policy The conceptual reservations to targeting the monetary base, mentioned earlier, are related to the definition of the monetary base. The base is defined as the sum of (1) currency outside the Federal Reserve System and the Treasury and (2) bank deposits at the Federal Reserve. Since currency accounts for over 70 percent of the base, many economists argue that currency changes would be given disproportionate weight if the monetary base were the target for monetary policy. This is particularly true since a dollar of bank reserves can support multiple dollars of money and credit.

The argument continues that if the Federal Reserve System were to react to changes in the demand for currency by making offsetting changes in bank reserves, the resulting effects on the economy would be destabilizing. As a result, targeting total (or nonborrowed reserves) and excluding currency has often been suggested as an alternative to targeting the monetary base.

Advocates of targeting the base answer that currency is included in the monetary base because it, along with bank reserves, is a balance sheet item (liability) for the Federal Reserve System. Thus, changes in the total base measure changes in the asset side of the Federal Reserve balance sheet and, hence, measure Federal Reserve open market actions. Therefore, the argument goes, no distinction should be made between the components of the base.

To test this proposition, regressions were run of GNP on currency and on total reserves (adjusted for reserve requirement changes) from 1959-IV to 1973-III, and the results were simulated dynamically through the fourth quarter of 1981. Both simulations, shown in Chart 5, went off track. Moreover, both of the component simulations underpredict nominal GNP; the differences were not offsetting. This result implies that the monetary base as a whole is more closely related to GNP than is its components. And that result, if correct, would seem to contradict the conceptual argument advanced against the base at the beginning of this section (i.e., that currency changes are given disproportionate weight by the base).

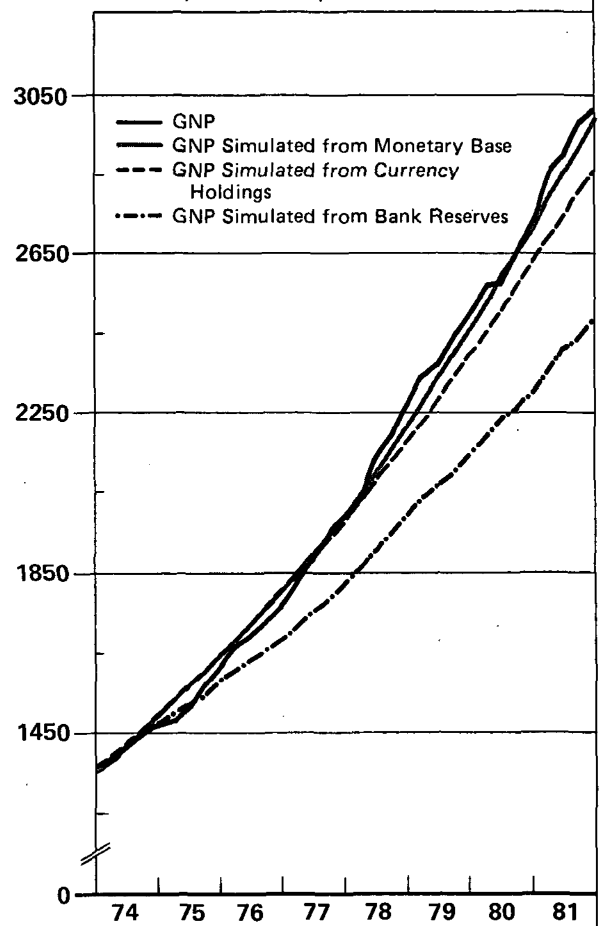
Phillip Cagan [3] recently provided another analysis of the currency issue. He also thought that currency was a questionable indicator of economic activity. He argued that reserves and checkable deposits are highly correlated and both provide the same information about the economy, implying that the reserve portion of the monetary base was the more important indicator of the effects of money on GNP. Using a modified Granger-Sims test, he found that "... when concurrent values are omitted, neither set of growth rates [of checkable deposits or the monetary base] can be shown by this test to add significant information not contained in the other" [3, p. 29]. His test pertained to in-sample data over the period from 1953-III to 1974-IV.

Using the methods outlined previously in this article, the percentage change in GNP was regressed on lagged values of the percentage change in checkable deposits over the 1959-IV to 1973-III period. The simulations therefrom are shown in Chart 6 compared to actual GNP and GNP simulated from the monetary base. The out-of-sample simulation from checkable deposits did not track nominal GNP at all well.[14]

Chart 5

GNP AND OUT-OF-SAMPLE SIMULATIONS OF GNP FROM THE MONETARY BASE, CURRENCY HOLDINGS, AND BANK RESERVES

(Bank Reserves Adjusted for Changes in Reserve Requirements by St. Louis Method)



These last results combine with simulations from currency and reserves to favor those who recognize no distinction between the components of the monetary base. This conclusion deserves further testing, however.

Conclusion This article presents statistical results demonstrating that the trend in income velocity of the monetary base remained remarkably constant from 1959 to 1981 and that the trend in income velocity of M1 also remained remarkably constant except for the three-year period from 1975 to 1978.

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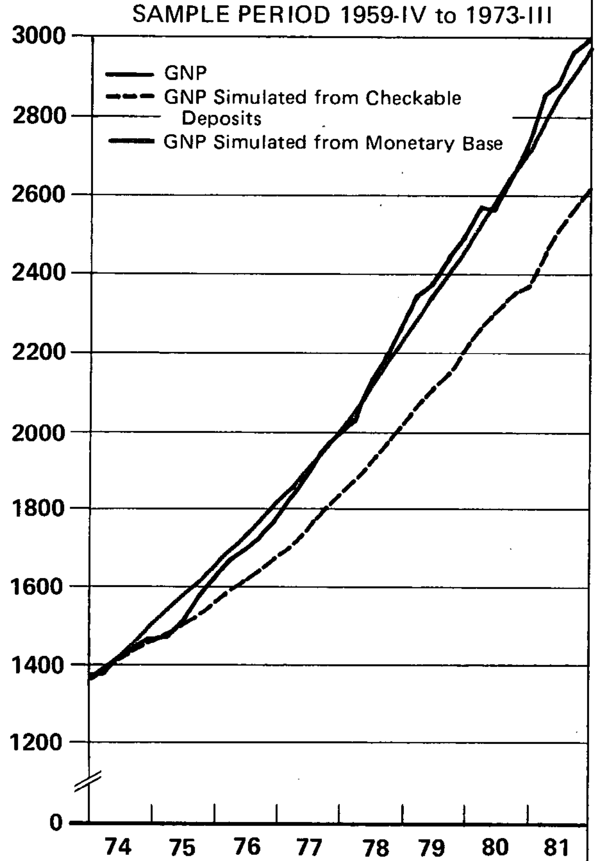
[14] The equation form specified for Chart 6 was similar to that for the "preferred base" simulation (Equations 6, 7, and 8 in Table I), having checkable deposits lagged one and two quarters. An alternative specification was also tried, using checkable deposits with five lagged quarters, but the simulation results were not appreciably different from those illustrated in the chart.

Chart 6

GNP AND OUT-OF-SAMPLE SIMULATIONS OF GNP FROM THE MONETARY BASE AND FROM CHECKABLE DEPOSITS

SAMPLE PERIOD 1959-IV to 1973-III



These results imply (1) that the demand for money (M1) has been generally stable since 1959 but that (2) the monetary base has borne a slightly closer and more predictable relationship to the long-run trend in GNP than has M1.[15]

As noted at the outset, the argument has often been made that financial innovations such as retail repurchase arrangements, money market mutual funds, Eurodollars, and NOW accounts have obscured past relationships of monetary aggregates to nominal income. And if financial innovations have indeed rendered money/income relationships meaningless, so the argument goes, then narrow monetary aggregates should be scrapped as targets for monetary policy. The analysis in this article suggests, however, that (except possibly for M1 during 1975-78) the much heralded financial innovations had no substantial impact upon the relationships between the narrow monetary aggregates and nominal GNP through the fourth quarter of 1981.

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APPENDIX

ILLUSTRATION OF NECESSARY (BUT NOT SUFFICIENT) CONDITIONS FOR THE MONETARY BASE TO BE EXOGENOUS TO NOMINAL GNP

(sample period from 1959-IV to 1973-III)

Form of Equation	Multiple Correlation Coefficients (uncorrected for degrees of freedom)	Root Mean Squared Errors (forecast period from 1973-IV to 1981-IV, 33 observations)
1. $g = f[\beta(-1), \beta(-2), g(-1), g(-2), g(-3)]$	0.310	$ 39.87
2. $g = f[g(-1), g(-2), g(-3)]$	0.142	146.45
3. $\beta = f[g(-1), g(-2), \beta(-1)]$	0.59	3.71
4. $\beta = f[g, g(-1), g(-2), \beta(-1)]$	0.61	3.25
5. $\beta = f[\beta(-1)]$	0.58	4.21

Definitions: *g = percentage change in nominal GNP, β = percentage change in St. Louis monetary base. Equations are linear* regressions with unconstrained lags, estimated by ordinary least squares.

F test for significance of differences in multiple correlation coefficients from Kmenta, Elements of Econometrics (New York: Macmillan and Company, 1971), p. 371.

For Eq. 1 versus Eq. 2, $F(2,50) = 6.09$*

For Eq. 3 versus Eq. 5, $F(2,52) = 0.63$**

For Eq. 4 versus Eq. 5, $F(3,51) = 1.31$**

* Significantly different at 0.01.

** Not significantly different at 0.10.

DETERMINANTS OF INDIVIDUAL TAX-EXEMPT BOND YIELDS: A SURVEY OF THE EVIDENCE

Timothy Q. Cook

The distinctive feature of the market for tax-exempt bonds is its heterogeneity. The features of specific tax-exempt bonds, the characteristics of the issuers, the scope of the market for the bonds, and the conditions under which the bonds initially are sold all vary greatly across individual bond issues.[1] As a result, the yields of different tax-exempt bonds issued at a given point in time cover a wide range. A substantial amount of recent research has attempted to explain the effects of different tax-exempt bond characteristics on a bond's yield.[2]

The purpose of this article is to describe in some detail characteristics of tax-exempt bonds and the tax-exempt bond market and to survey the evidence on cause and effect relationships between variations in these characteristics and variations in the yields of individual tax-exempt bonds. Table I identifies the factors that have been analyzed as determinants of the yields on individual tax-exempt bonds.

The first group of characteristics are those related to the *issues* themselves. These are call provisions, coupon setting practices, and issue size. Most tax-exempt bonds have call provisions that permit the issuer to redeem the bond prior to its maturity. Newly issued tax-exempt bonds frequently have coupons that differ substantially from their yield-to-maturity. As a result, these bonds sell at prices above or below their par value. Finally, the size of a tax-exempt issue can range anywhere from less than $1 million to over $200 million.[3]

The second source of diversity in the tax-exempt bond market relates to the *issuers* of the bonds. Most tax-exempt bonds are classified either as "general obligation" or as "revenue" bonds. General obligation bonds are issued by state and local governments and are secured by the taxing power of the issuing government. Revenue bonds, in contrast, are usually secured solely by the revenues of the project they are issued to finance. Typically, revenue bonds are sold by an authority, commission, special district, or other government entity created for the specific purpose. A major focus of studies of tax-exempt yields has been on the risk of issuer default as a determinant of yields. Default risk of general obligation bonds depends on economic and fiscal conditions of the government issuing the bonds while default risk of

Table I

EXPLANATORY VARIABLES USED IN REGRESSION STUDIES OF THE DETERMINANTS OF THE YIELDS ON INDIVIDUAL MUNICIPAL BONDS*

Issue (Bond) Characteristics

1. call dummy variable
2. other call provision variables
3. coupon variables

Issuer Characteristics

4. rating category dummy variables
5. issue purpose dummy variables
6. other measures of default risk

Marketing (Underwriter Competition)

7. number of bids
8. dispersion of bids
9. negotiated dummy variable
10. bank eligibility dummy variable

Regional Market Conditions

11. pledging variables
12. other demand variables
13. supply variables

Other

14. revenue bond versus general obligation
15. size of issue
16. other

* Excluded are variables to pick up national market conditions at the time of issue.

[1] Sources of information on tax-exempt bonds and the tax-exempt bond market are Lamb and Rappaport [37], Peterson [43], Public Securities Association [44], Rabinowitz [45], and Robinson [47].

[2] There have been at least 25 regression studies of the determinants of individual tax-exempt bond yields. These studies are enumerated and described briefly in the references at the end of this article. A preliminary version of one of these studies by Broaddus and Cook [8] was done for the Federal Reserve System's Ad Hoc Subcommittee on Full Insurance of Government Deposits [28]. The data collected for that study is used extensively in this article.

[3] In this brief overview issue size and revenue bond versus general obligation status are discussed in the issue and issuer categories, respectively. However, alternative theories as to how size and revenue bond status affect yields relate them to other categories as well. Consequently, later in the article they are discussed separately as shown in Table I.

revenue bonds depends primarily on the income generated by the project financed by the bonds.

The *marketing* process by which new issues of tax-exempt bonds are sold is a third broad area considered as a source of variation in individual tax-exempt bond yields. Some small issues are sold directly to local banks. Other issues are generally sold through the process of underwriting and syndication, in which a group of dealers join together in a "syndicate" to purchase an entire issue of bonds from a governmental unit for the purpose of reselling them in the retail market at a slightly higher price. Commercial banks are prohibited by law from participating in the underwriting of some types of revenue bonds.

Bonds are purchased from the government unit by dealers either through negotiation or through competitive bidding. In the case of negotiation the issuer chooses a syndicate with which it negotiates the purchase price of the bonds. In the case of competitive bidding the issuer solicits bids from syndicates and sells the bonds to the highest bidder. The number of bids received varies greatly across issues.

Two features of the underwriting process are worth noting. First, prior to offering a price to the issuer for bonds, the underwriting syndicate canvasses potential buyers to get a firm idea of the price the latter will offer for the bonds. Because of the great diversity of tax-exempt issues, this process is widely believed to help identify those willing to pay the highest price—i.e., receive the lowest yield—for the bonds. Many studies have specified this information as a determinant of tax-exempt yields.

The second important characteristic of the marketing process is the method by which the winning bid is chosen. In most cases the winning bid is determined on the basis of the syndicate offering the lowest net interest cost (NIC) to the issuer. NIC is defined as

$$\frac{\text{total interest payments} + \text{discount (or} - \text{premium)}}{\text{bond year dollars}}$$

where bond year dollars is simply the amount of bonds outstanding over the time they are outstanding. A drawback of NIC is that it ignores the time value of money. Hence, payments made by the government in early years count the same as payments in later years even though the present discounted value of the later payments is much lower.

Although most bidding is done on the basis of NIC, some bonds are awarded on the basis of the lowest true interest cost (TIC). TIC is the single discount rate that equates the present value of all future payments by the issuer to the price received from the syndicate. Conceptually it is a far superior measure of yield than NIC. Both NIC and TIC are calculated from the *issuer's* point of view, so they reflect not only the payments to the bond holders, but also the payment to the underwriter (called the "underwriter's spread").

A third yield concept is the *reoffering yield to maturity* earned by the investor who purchases a tax-exempt bond from the underwriter.[4] It is the discount rate that equates the present value of the stream of payments received by the investor to the price he pays the dealer for the bond.

The fourth broad area that has received some attention as a source of variation in individual tax-exempt bond yields is *regional market conditions* where the issue is sold. For reasons that will be explained in detail below, the argument has been made that the demand for at least some tax-exempt issues is largely regional in character. If true, the yields on these tax-exempt bonds may be influenced by regional supply and demand factors.

The methodology employed by all the studies of individual tax-exempt bond yields discussed in this article is multiple regression analysis, which regresses the dependent variable, a measure of tax-exempt yield, on various subsets of the independent or explanatory variables listed in Table I. Some of the variables listed have been included in virtually all of the studies while others have been included in only a small number. The references at the end of this article indicate the set of explanatory variables in each of the studies surveyed, using the same format as in Table I.

While the basic approach followed by all the studies is similar, an important difference among them is the choice of the yield to be used as the dependent variable. Municipal bonds are generally sold in serial issues that include securities of several maturities. Most studies employ as the dependent variable a composite measure of the yields on all of the bonds in the serial such as net interest cost (NIC) or true interest cost (TIC), both of which are measures of issuer cost. The studies then attempt to specify independent variables that are representative of the entire serial issue.

This use of a composite yield measure has a number of disadvantages. First, treating the entire serial as the analytical unit makes it difficult and in some cases impossible to analyze the effect on yields

[4] In the rest of the article this is called the reoffering yield, yield-to-maturity or, simply, yield.

of particular bond (as opposed to issue) characteristics. For example, serial issues often include short-term bonds that are not callable and some longer term ones that are. It is therefore very difficult to measure differences in call provisions across entire serial issues. Second, this approach precludes analysis of the effect of a particular factor on bond yields of differing maturity. A third disadvantage of using aggregate yield measures such as NIC or TIC is that these variables include *both* the compensation to the underwriter and the return to the investor. Hence, in some cases it is difficult to interpret whether an estimated regression coefficient reflects the behavior of one or both of these groups.

As a result of the disadvantages of using NIC and TIC, a small number of studies have used instead reoffering yields as the dependent variable in tax-exempt bond yield regressions. Broaddus and Cook [8] estimated separate equations for four separate maturities: 5 years, 10 years, 15 years, and 20 years. Where appropriate, the independent variables were defined differently for each maturity.

In addition to the variables shown in Table I, virtually all of the studies include one or more independent variables to capture the effect of current "national market conditions" on a bond yield. If the dependent variable is the reoffering yield for a specific maturity, then the national market condition variable chosen is always the reoffer yield on high-grade general obligation bonds for that maturity. If the dependent variable is NIC or TIC, capturing national market conditions is much more complicated because the dependent variable is affected not only by the level of general yields in the market but also by the average *maturity* of the serial issue and the current *slope* of the yield curve.[5] Different studies have specified one, two, or three variables to capture these three effects.[6]

A final background comment on the regression studies is that conceptually they are cross-section studies that attempt to measure how variations in characteristics *across* tax-exempt issues influence their yields. In practice, however, the studies use data gathered over periods from three months or less to ten years or more. Lengthy data periods create two potential problems. First, the effect of a given value of an explanatory variable may vary over time due to changes in national economic conditions. Second, inflation over the data period may cause nominal values of variables to change, which makes it important to specify whether a theory calls for an explanatory variable to be measured in nominal or real terms.

The remainder of this article is divided into five sections corresponding to the five categories shown in Table I. In each section the relevant characteristics of tax-exempt bonds are described. Next the theories linking these characteristics to bond yields are outlined. Finally, the results of the empirical studies are presented.

I.

ISSUE CHARACTERISTICS

Coupons

Coupon Setting Practices When state and local governments solicit bids on new issues, they set constraints on the coupons (i.e., annual interest payment) that the bonds can carry when they are resold by underwriters to the public. Occasionally, the bonds are required to carry coupons at each maturity equal to their reoffering yield to maturity. More commonly, however, underwriters are allowed a great deal of flexibility in setting coupons. As a result of this flexibility, and for reasons that will be described immediately below, the winning bids on tax-exempt issues often specify coupons for at least some maturities that differ considerably from their yield to maturity. Specifically, new short- and intermediate-term bonds often carry a coupon above their yield to maturity while long-term issues often, but less frequently, carry a coupon below their yield to maturity. When a bond has a coupon above its yield to maturity it is sold at a "premium," i.e., at a price above its par value. Conversely, when a bond has a coupon below its yield it is sold at a "discount."

Table II shows the different coupon-setting practices on the winning bids for ten tax-exempt issues. As the table illustrates, sometimes the coupon is set well above the yield to maturity at the 5-year maturity, about equal to the yield to maturity at the 15-year maturity, and well below the yield to maturity at the 20-year maturity. This pattern is shown in issues 3 and 6 and to a lesser extent in issues 4 and 10. In other cases such as issues 1 and 9 the

[5] The yield curve in the tax-exempt market is generally upward sloping. Hence, for a given level of market interest rates the NIC of a serial issue will be higher the greater the average maturity. Similarly, for a given average maturity the NIC will be higher the steeper the slope of the yield curve. The problems and possible pitfalls of using national market conditions variables in NIC and TIC regressions are discussed in Broaddus and Cook [8].

[6] For example, see Hendershott and Kidwell [15] who include variables for market yield, average maturity of issue, and slope of the yield curve.

Table II

COUPONS AND YIELDS TO MATURITY OF SELECTED TAX-EXEMPT ISSUES

(Percent)

Bond issue	5-year maturity			10-year maturity			15-year maturity			20-year maturity		
	Yield	Coupon	Difference	Yield	Coupon	Difference	Yield	Coupon	Difference	Yield	Coupon	Difference
1. Plano, Texas (3-14-77)	4.15	6.50	2.35	4.80	6.50	1.70	5.35	5.35	0	5.90	5.90	0
2. Columbia, Missouri (3-30-77)	4.20	5.25	1.05	4.70	5.25	.55	5.20	5.25	.05	5.65	5.25	−.40
3. Anderson Co., Tennessee (3-31-77)	4.00	7.00	3.00	4.75	6.00	1.25	5.25	5.25	0	6.00	4.00	−2.00
4. State of Hawaii (4-5-77)	4.10	5.50	1.40	4.75	4.75	0	5.20	5.20	0	5.75	4.50	−1.25
5. Nebraska Public Power District (4-5-77)	4.30	4.30	0	5.00	5.00	0	5.50	5.50	0	5.80	5.80	0
6. Miami, Florida (4-14-77)	4.00	7.00	3.00	4.70	4.70	0	5.20	5.20	0	5.80	3.50	−2.30
7. Carroll Co., Maryland (4-20-77)	3.75	4.50	.75	4.30	4.30	0	4.80	4.80	0	5.00	5.00	0
8. New Jersey Housing Finance (4-28-77)	4.25	4.25	0	5.10	5.10	0	5.60	5.60	0	5.95	5.95	0
9. Alaska Municipal Bond Bank (4-27-77)	4.50	6.50	2.00	5.20	5.20	0	5.90	5.90	0	6.10	6.10	0
10. Mecklenberg Co., North Carolina (5-3-77)	3.70	6.00	2.30	4.20	6.00	1.80	4.75	4.75	0	5.25	4.00	−1.25

coupon is set well above the yield to maturity at the early maturities but does not fall below the reoffering yield at the longer maturities. A third group of issues, such as issues 5 and 8, have coupons equal to yields throughout the entire range of maturities. Coupons are never set below yield to maturity at the shorter maturities or above it at the 20-year maturity.

Table III shows the spread between coupon and yield to maturity at the shorter maturities for all the issues included in the Broaddus-Cook study [8].[7] At the 5-year maturity, 745 out of 885 issues had coupons above their yield. Of these, the difference between coupon and yield was greater than one percentage point for 392 bonds and greater than two percentage points for 146 bonds. The number of bonds with large premiums dropped sharply at the 10-year maturity and was negligible at the 15-year maturity.

Table IV shows the distribution of issues with coupons below yield to maturity at the 15- and 20-year maturities. At the 20-year maturity, 318 of the issues in the sample had coupons below their yield. Of these, however, only 33 had coupons more than one percentage point below yield. At the 15-year maturity, 181 were sold at a discount; but in all cases the difference between yield and coupon was quite small.

Explanation for and Possible Costs of Coupon-Setting Practices The coupon setting practices illustrated above occur because, as noted earlier, winning bids on most serial municipal bonds are determined on the basis of net interest cost (NIC). NIC bidding creates incentive for underwriters to set high coupons at the shorter maturities and low coupons at the longer maturities:[8]

> Because investors place time value on coupons, coupons on early maturities can be sold by the

[7] Data from the study by Broaddus and Cook [8] is used for description throughout this section. Bonds used in the study were almost all those for which reoffering yields were reported in **Moody's Bond Survey** from March 1977 through the end of 1978.

[8] This explanation is given in more detail in Robinson [47] and Hopewell and Kaufman [32, 33]. Robinson (Appendix B) and Public Securities Association [44, Appendix] give examples.

Table III

PREMIUM BONDS

Distribution of Bonds by Spread Between Coupon and Yield to Maturity

(number of bonds)

Spread: (percentage points)	5-year maturity	10-year maturity	15-year maturity
Equal to 0 (or negative)	140	306	622
0 < and ≦ 0.5	123	332	207
0.5 < and ≦ 1.0	230	129	11
1.0 < and ≦ 1.5	142	50	9
1.5 < and ≦ 2.0	104	51	2
2.0 < and ≦ 2.5	95	26	0
2.5 < and ≦ 3.0	36	3	0
3.0 < and ≦ 3.5	6	2	0
3.5 < and ≦ 4.0	6	2	0
4.0 < and ≦ 4.5	2	0	0
greater than 4.5	1	0	0
Total	885	901	851

Source: Broaddus and Cook [8].

> underwriters at higher prices than coupons on later maturities summing to the same dollar amount. Yet, under NIC, these coupons do not cost the underwriter any extra. Thus, to maximize their revenues from the sale of the bonds, the underwriters are encouraged to place the highest coupons on the earliest maturities. The placing of large coupons on the early maturities is referred to as frontloading. To obtain a low NIC, compensating low coupons are placed on the most distant maturities [32, p. 534].

There are two possible limitations to the ability of underwriters to set coupons that differ from a bond's yield to maturity. The first is that investors may purchase bonds with such coupons only if compensated by a higher yield than on an otherwise similar par bond. At some point this could offset the advantage to an underwriter in achieving a low NIC of setting high coupons at shorter maturities and low coupons at longer maturities. The second limitation may be imposed by issuers who specify constraints on the type of coupon they will accept.

Hopewell and Kaufman [32, 33] identified two possible costs to municipal governments of awarding bonds sold competitively to underwriters on the basis of NIC bidding as opposed to true interest cost (TIC) bidding. The first cost is that the lowest NIC bid may not be the same as the lowest TIC bid. In other words the government may accept the wrong bid. The second potential cost of using NIC bidding is that, for reasons to be discussed below, investors may only purchase bonds with coupons that are above or below their yield to maturity at a lower price than bonds that have coupons equal to their yields and that are otherwise equal in all respects. If this were the case then all bids (in terms of the prices offered by underwriters) under NIC bidding would be lower than under TIC bidding because under NIC bidding the bonds would be worth less to the ultimate investors.[9]

The question remains as to why investors might demand a higher yield to maturity for bonds that carry "high" or "low" coupons. For bonds with low coupons there is a powerful reason for this relating to the taxation on income earned on discount bonds versus income earned on bonds selling at par. Par bonds are not subject to any Federal taxes because all the income is tax-exempt interest income. However, investors in low coupon discount bonds generally have to pay capital gains taxes at maturity on the difference between the par value and purchase price of the bond.[10] Consequently, a higher yield is required on a discount bond in order to earn the same after-tax yield as a par bond.

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[9] Hence, the lowest TIC bid under NIC bidding may be higher than the lowest TIC bid that would occur under TIC bidding. This is distinct from the first possible cost of NIC bidding, which is that the issuer may not select the lowest TIC bid.

[10] The question of whether the discount on new tax-exempt issues is subject to capital gains tax is very confusing. According to a 1973 publication of the Securities Industry Association [26, p. 9]:

> Issue discount is recognized as being interest in substance and as "interest" on a tax exempt bond such **issue** discount is tax exempt. . . . This is true only where the bond is issued at a discount and does not apply where bonds which were originally sold at par or at a premium are subsequently re-offered at a discount. Where an issue of serial bonds is purchased from the issuer by a dealer at a single unallocated price of not less than their total par value (face amount) and some of them are re-offered by the dealer at a discount, they are not **issued** at a discount.

This statement clearly implies that if an underwriter purchases an entire serial issue of bonds at a single price not less than their total par value and some of the bonds are reoffered by the dealer at a discount, the capital gain is not tax-exempt. However, a just-published article ("The Tax Treatment of Municipal Discount Bonds: Correction of a Fallacy" by Ronald C. Braswell, Walter J. Reinhart, and James R. Hasselback, **Financial Management,** Spring 1982) states unequivocally, citing IRS Revenue Ruling 73-112, that if an investor buys a new discount bond from an underwriter, the discount is treated as tax-free interest income regardless of the circumstances under which the underwriter acquired the bond from the issuer. In any case the regression results discussed in this section present convincing evidence that at least in the 1977-78 period investors viewed new issue discount bonds as subject to capital gains tax.

Table IV

DISCOUNT BONDS

Distribution of Bonds by Spread Between Yield to Maturity and Coupon

(number of bonds)

Spread: (percentage points)	15-year maturity	20-year maturity
Equal to 0 (or negative)	670	317
0 < and ≦ 0.5	181	272
0.5 < and ≦ 1.0	0	13
1.0 < and ≦ 1.5	0	16
1.5 < and ≦ 2.0	0	11
2.0 < and ≦ 2.5	0	5
greater than 2.5	0	1
Total	851	635

Source: Broaddus and Cook [8].

Hopewell and Kaufman argued that even after adjusting for tax differences investors may require a higher yield on discount bonds than on par bonds for two additional reasons [33, pp. 284-285]. First, since low coupon bonds have longer duration than par bonds of comparable maturity investors might, to the extent that liquidity premiums increase with duration, demand a higher yield on low coupon bonds.[11] Second, discount bonds may be less marketable than par bonds and consequently investors might demand a higher yield. The major argument Hopewell and Kaufman presented for the effect of high coupons on yields is that high coupon bonds may subject investors to greater reinvestment risk [32, p. 535].

Empirically, Hopewell and Kaufman concluded that both high and low coupons raise yields, although the effect of high coupons was much smaller.[12] They also concluded that in most cases, capital gains tax liability by itself is insufficient to explain the magnitude of the additional yield required on discount bonds.

Regression Results Of the regression studies on the determinants of tax-exempt yield only the Broaddus-Cook study attempted to estimate the effect of high and low coupons on yields to maturity. The study calculated a data series for high coupon bonds by taking the difference between coupon and yield and a similar series for low coupon bonds by taking the difference between yield and coupon. (In both cases the values were set equal to zero if they were negative.) These series were then entered as explanatory variables in the four maturity regressions. The regression results strongly supported the view that low coupons raise yields to maturity. At the 20-year and 15-year maturities the estimated effects were 19 and 36 basis points, respectively, for each percentage point difference between yield and coupon. These estimates support Hopewell and Kaufman's contention that the effect of low coupons on tax-exempt bond yields was greater than could be explained by the capital gains tax factor alone.[13]

A possible explanation for the larger estimated effect on yields of selling discount bonds at the 15-year maturity than at the 20-year is that it reflects the greater role of commercial bank behavior at the 15-year maturity. According to Hobby [31], bank holdings of tax-exempt bonds fall off sharply after 10- to 15-year maturity range. Since commercial banks have a higher capital gains tax rate than individuals, who are the second largest group of investors in tax-exempt securities, one might expect the marginal investor at the 15-year maturity to have a higher capital gains tax rate than the marginal investor at the 20-year maturity. This would push up the coefficient of the low coupon variable in the 15-year regression relative to the coefficient in the 20-year regression.

The evidence from the regressions for an effect of *high* coupons on yields to maturity was very weak. The coefficient in the regression for the 5-year maturity was less than 2 basis points and was significant only at the 10 percent level. The coefficients of the high coupon variables in the 10- and 15-year maturity regressions were not significant.

These results indicate that discount bonds constitute by far the greatest cost to governments in terms of the additional yield required to induce investors to buy them. The small number of large discounts in the 1977-78 period indicate that most governments have realized this and have put constraints on the use of low coupons in NIC bidding.

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[11] Duration is a weighted average of times in the future when payments are to be received.

[12] Hopewell and Kaufman did not use the regression technique common to the studies surveyed in this article. Hence, their results are not discussed in detail here. See [33].

[13] In order to make this judgment the estimated coefficients of the low coupon variables were compared to hypothetical coefficients calculated under the assumption that only the capital gains effect was at work. The calculation used the 48 percent capital gains tax rate of the commercial banking sector, which is the largest investor in the tax-exempt market and which has the highest capital gains rate. Hence, the hypothetical coefficients were the maximum expected if capital gains taxes were the only force pushing up the yield on discount bonds.

Call Provisions

The Extent and Nature of Call Provisions on Tax-Exempt Bonds Most tax-exempt bonds have call provisions that permit the issuer to redeem them *prior* to maturity under certain conditions. For example, in the Broaddus-Cook data sample 58.5 percent of the general obligation issues and all of the revenue bond issues were callable.

Call provisions vary substantially across tax-exempt issues. To illustrate, the provisions of ten issues are shown in Table V. The call provisions typically include the date of first call, the price the issuer must pay per $100 at the time of first call, and the changes in call price between first call and maturity. It is also generally indicated whether the bonds in a serial issue are callable "inversely," i.e., in reverse order of maturity.

On the right-hand side of Table V specific characteristics of the call provisions are extracted from the summary statements of the issuer. The number of years to first call is specified for the whole package, rather than for individual maturities, so that it is the same for all maturities. The table illustrates that while the most common number of years to first call is ten, there is a great deal of variation.

The summary data on years to first call for the issues used in the Broaddus-Cook regressions is shown in Table VI. These data show that although a particular serial issue may be "callable," some of the bonds that comprise the issue may in fact mature before the first call date. Only 0.7 percent of the callable issues at the 5-year maturity had an initial call date prior to maturity. This figure rises to 10.6 percent for the 10-year maturity, 88.1 percent for the 15-year maturity, and 98.2 percent for the 20-year maturity.

The examples shown in Table V also illustrate the variation in call price provisions across different bond issues. These provisions typically indicate the call price at the time of first call and specify how it declines to par in between the time of first call and maturity. The provisions are specified for the issue as a whole but the call price at a given point may or may not be the same for bonds of different maturity with a given issue. Call price schedules generally fall into two categories. Some issues specify a specific price covering all maturities for each point in time. For instance, the call price of issue 8 is $104 per $100 at the time of first call and then drops sharply to $100 four years later. Other issues specify a formula. For instance issue 2 specifies the call price for any maturity as 100 plus ¼ percent for each 12 months or fraction thereof between date of redemption and date of maturity. In this instance the price at time of first call would be $101.25 for the 15-year maturity and $102.50 for the 20-year maturity. Some issues that specify formulas indicate an upper limit for the call price. For instance, the cap for any maturity in issue 2 is $102.50. Issue 1 has the same formula as issue 2, but without a cap. The examples shown in Table V also illustrate that in some instances the call price drops quite slowly while in others it drops sharply to 100.

Table VII shows the call price at the first call date for the bonds in the 15-year maturity Broaddus-Cook regression. The table, which includes only those issues in the regression that are callable prior to maturity, shows that while a price of $103 at the time of first call is most common, call prices fall over a wide range including $100.

The Predicted Effects of Call Provisions on Tax-Exempt Bond Yields Callable tax-exempt bonds may have higher yields to maturity than otherwise similar non-callable bonds because they expose the investor to the risk of having to invest his money at a lower interest rate between the time of call and maturity. In compensation for this risk, investors may demand a higher "promised" yield to maturity on callable than on non-callable bonds.

The effect of call risk on the yield of a bond should depend on two broad factors. The first is the expected pattern of interest rate movement over the life of the bond. Ceteris paribus, the lower interest rates are expected to fall relative to current rates between the first call date and maturity, the greater the probability that the issuer will find it profitable to call the issue.[14] The second factor consists of the call provisions specific to that bond. The longer the years to first call for a bond of a given maturity the lower the exposure to call risk. Consequently, one would expect the effect of call risk to vary inversely with the number of years to first call. Also, the higher the call price the lower the probability that the market price of the bond will rise enough for the issuer to find the call option attractive. Consequently, one would expect increases in the call price to decrease the risk of call and thereby decrease the impact of callability on a bond's yield.

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[14] The risk of interest rates falling to a level where the tax-exempt bond will be called may be viewed as a function of the expected change in interest rates and/or as a function of the expected variation in rates. The only regression study [8] that attempted to capture this effect used a proxy for the expected change in rates.

Table V

CALL PROVISIONS OF SELECTED TAX-EXEMPT ISSUES

Bond issue	Call provisions specified by issuer	Years to first call	Call Price at Time of First Call (Per $100)				Call Price 3 Years After Time of First Call		Call Price 6 Years After Time of First Call
			S-year maturity	IO-year maturity	15-year, maturity	20-year maturity	15-year maturity	20-year maturity	20-year maturity
1. Montebello Comm. Redevel. Agency (5-10-77)	Callable as a whole or in port inversely and by lot within a maturity from any available funds on any interest date beginning April 15, 1984 at 100 plus ¼ percent for each year or fraction thereof between date of redemption and date of maturity.	7	not callable	100.75	102	103.25	101.25	102.50	101.75
2. Philadelphia, Pennsylvania (5-24-77)	Callable as a whole on any date, or in part inversely and by lot within a maturity on any interest date, beginning Sept. 15, 1987 at 100 plus ¼ percent for each 12 months or fraction thereof between date of redemption and date of maturity, premium not to exceed 2½ percent.	10	not callable	not callable	1.25	2.50	100.50	101.75	101
3. State of California (6-7-77)	Callable beginning June 1, 1992 or any interest date thereafter at 100.	15	not callable	not callable	not callable	100		100	
4. Birmingham, Alabama (7-26-77)	Callable as a whole, or in part inversely on any interest date beginning Aug. 1, 1987 thru Feb. 1: 1989, 103½; 1991, 103; 1993, 102½; 1995, 102; 1997, 101½; 1999, 101; thereafter 100.	10	not callable	not callable	103.50	103.50	103	103	102
5. Kentucky Hous. Corp. (8-18-77)	Callable as a whole or in part at any time beginning July 1, 1987 thru June 30: 1988, 103; 1989, 102; 1990, 101; thereafter 100.	10	not callable	not callable	103	103	100	100	100
6. Hennepin Co., Minnesota (9-13-77)	Callable as a whole or in part inversely on any interest date beginning Jan. 1, 1987 at 100.	9	not callable	100	100	100	100	100	100
7. Mera, Arizona Utility (10-3-77)	Callable as a whole or in part inversely on any interest date beginning July 1, 1983 at 100 plus ½ percent for each year between date of redemption and date of maturity, premium not to exceed 3 percent.	6	not callable	102	103	103	103	103	103
8. Anchorage Alaska Electric (11-8-77)	Callable as a whole or in part inversely and by lot within a maturity on any interest date beginning May 1, 1988 to Nov. 1: 1989, 104; 1990, 103; 1991, 102; 1992, 101; thereafter 100.	10	not callable	not callable	104	104	102	102	100

Source: Moody's Municipal and Government (News Reports), 1977; Moody's Bond Survey, 1977,

Table VI

DISTRIBUTION OF BONDS BY YEARS TO FIRST CALL

(number of bonds)

	5-year regression	10-year regression	15-year regression	20-year regression
Total number of observations	860	876	829	620
Total number of issues not callable	270	265	243	166
Number of years-to-first call				
1 ≦ and < 5	4	4	3	3
5 ≦ and < 10	59	61	52	32
10 ≦ and < 15	451	470	461	351
15 ≦ and < 20	68	68	62	60
Greater than or equal to 20	8	8	8	8
(memo item: equal to 10)	359	376	371	283

* Bonds below the solid line are not callable because call date is not prior to maturity.

Source: Broaddus and Cook [8].

Before reporting the regression results on the effect of call provisions on tax-exempt bond yields, several complications should be mentioned. First, as discussed above, a given tax-exempt issue is a conglomeration of bonds among which call risk may vary greatly. Hence, like the analysis of coupon effects, the analysis of call provisions must focus on individual bonds, not serial issues. Second, while the protection offered by years to first call is fully captured by one number, the call price at a given point, such as the first call date, is only a rough proxy for the price over the whole period between years to first call and maturity. Third, in general, the difference between the call price and the price of the bond represents the gap that has to be overcome by falling interest rates before the market price of the bond rises to its call price. The larger the gap, the greater the decline in interest rates necessary to make call profitable to the issuer and, hence, the lower the risk of call. The size of this gap, however, depends not only on the call price but also on the *initial price* of the bond, which is frequently not par. Consider the case of a discount bond selling at $95 with a call price of $103. In this case interest rates have to fall enough to raise the market price of the bond from $95 to $103. The point here is that coupon effects complicate the analysis of call risk.

Table VII

CALL PRICE AT TIME OF FIRST CALL FOR BONDS IN 15-YEAR REGRESSION*

Call price	Number of issues
Equals 100	112
100 < and ≦ 101	25
101 < and ≦ 102	104
102 < and ≦ 103	246
103 < and ≦ 104	20
greater than 104	9
Total	516

* Includes only bonds callable in less than 15 years.

Source: Broaddus and Cook [8].

A final complication in analyzing the effect of call provisions on tax-exempt yields is that they are *interdependent*. That is, call risk for a particular issue depends jointly on interest rate expectations, years to first call, call price, and initial price of the bond. Consider the case where interest rates are at a cyclical low and are expected to rise in the future. Then investors may have little fear that callable bonds will be called. In such a period variations in call price or years to first call across bonds of a given maturity may have little effect on yields.

Regression Results The approach used in all but one of the eighteen regression studies that attempted to capture the effect of call provisions on tax-exempt bond yields was to include either a dummy variable set equal to one for callable issues or a variable for the number of years to first call.[15] The predicted sign of the call dummy variable coefficient is positive while the predicted sign of the years to first call coefficient is negative. These studies have generally had poor results in estimating the effect of call provisions. Only in four studies [15, 19, 20, 22] did all reported regressions have statistically significant call variable coefficients with the predicted sign.[16] Of those studies reporting significant coefficients for the call dummy variable, the coefficients with one exception ranged from 11 to 35 basis points.[17]

The unexpectedly poor results of these studies stem from the complications discussed above. First, the dummy variable technique forces the effect of call risk to be constant over the whole period covered by a study's data sample. It is clear, however, from the reasoning above and from evidence in the corporate bond market (see Yawitz and Marshall [51] for example) that this should not be the case. Second, 12 of the 17 studies used the whole issue as the unit of investigation and NIC or TIC as the dependent variable. As noted above, this approach cannot accurately estimate call effects, because a given callable serial issue in fact is a conglomeration of bonds, only some of which are callable. And even among callable bonds within a given serial issue the effect on yield of a given set of call provisions may vary.

The Broaddus-Cook study attempted to deal with these problems by analyzing the effects of call risk on yields to maturity at three different maturities: 10, 15, and 20 years. In each case rather than use a simple call dummy the study used an interest rate expectations proxy that allowed the effect of call risk to vary over time as expectations varied. This proxy was the spread between the 20- and 7-year U. S. government bond yields.[18] The assumption underlying the use of this proxy is that in 1977 and 1978 changes in the U. S. bond yield curve were determined by changes in interest rate expectations.[19] This proxy was set equal to zero if years to first call of a particular issue were greater than the maturity of the bond in question.

The study made the effect of years to first call and call price on a bond's yield dependent on the interest rate expectations proxy. The specific call price variable used was the difference between the call price of a bond at the time of first call and its price at the time of first call calculated using the initial reoffering yield of the bond. (The gap between these two prices is a proxy for the amount rates have to fall below the initial yield before the bond's market price at first call rises to its call price.)

The study found a highly significant relationship between interest rate expectations and the yield on callable tax-exempt bonds at the 15- and 20-year maturity. The coefficient of the 10-year maturity was only significant at the 10 percent level. The study also found a significant negative relationship between years to first call and the effect of call risk on yield. The coefficient of the call price variable did not have the expected sign in any regressions and it was concluded that the call price at the first call date is simply a poor proxy for the call price over the whole span between years to first call and maturity. The estimated call effect for a 20-year bond with 5 years to first call ranged from 8 to 22 basis points over the 1977-78 period. For a bond with 10 years call protection, the effect was only 3 to 9 basis points. These effects are fairly small compared to those reported in corporate bond studies. However, the 1977-78 period was one of low tax-exempt rates relative to the previous three years and one might consequently expect call risk to be relatively small in this period.

II.
ISSUER CHARACTERISTICS: DEFAULT RISK

The Expected Effect of Default Risk on Tax-Exempt Bond Yields

Yields on tax-exempt bonds are calculated using the *promised* interest payments of the bonds. Default risk refers to the possibility that an issuer of a bond may not make these payments or may not make them

[15] An exception within this group is Kidwell [18] who tested for other call features. However, years to first call is the only call feature that had a significant coefficient with the predicted sign in any of his regressions.

[16] In seven studies [4, 11, 12, 17, 18, 23, 24] the results were mixed and in six studies [3, 6, 7, 14, 16, 21] there were no statistically significant call variable coefficients.

[17] Specifically, references [4, 15, 19, 20] reported coefficients of .346, .148, .110, and .137, respectively. Reference [24] reported a coefficient of .198 for general obligation issues and a coefficient of .995 for all issues.

[18] An implicit assumption is that the adjustment to changing interest rate expectations is made solely through the yield on callable bonds. This in turn assumes that state and local governments do not alter years to first call and call prices to offset the effect of changing interest rate expectations on callable bond yields. Both Kidwell [36] and Broaddus and Cook [8] present evidence that supports this assumption.

[19] The specific variable used was (call dummy) $(1/e^{SPR})$ where the call dummy indicates whether or not the issue is callable and SPR is the spread between 20- and 7-year U. S. government bond yields. This functional form has the feature that as the spread gets very high the effect of call risk approaches zero.

on time. Default risk may affect the yield to maturity on a bond in two ways. First, a higher promised yield to maturity is necessary to achieve a given *expected* yield. Second, the investor may demand a *higher* expected yield on a risky bond, relative to the yield on a risk-free bond in compensation for the risk involved.[20]

Default Risk Explanatory Variables Used in Tax-Exempt Yield Regression Studies

Cross-Section Variables In discussing the explanatory variables designed to capture the effect of default risk on tax-exempt yields, it is useful to distinguish between "cross-section" and "time-series" explanatory variables. While tax-exempt bond yield studies are conceptually cross-section studies, they frequently use data that cover a long period of time. As a result, the effect of default risk on yield may not be totally captured by cross-section variables. The cross-section and time-series variables used in the various studies are summarized in Table VIII.

Regression studies of the determinants of tax-exempt bond yields have used one of two approaches in attempting to capture the effect of default risk at a given time on yields. A small number of studies, interested primarily in default risk, have specified the economic and fiscal characteristics of the issuer believed to influence risk premiums and entered them directly into yield regressions. The much more common procedure is to instead include dummy variables that are presumed to be related in some way to the underlying economic and financial characteristics of the issuers. As shown in Table VIII, by far the most widespread practice in this regard is to enter dummy variables for the various rating categories employed by one or both of the two major rating agencies, Moody's and Standard and Poor's. It has been argued in a number of articles that the rating category dummy variables do not adequately capture cross-section variations in default risk. In particular, the argument has been made that within a given rating category, default risk of revenue bonds varies systematically by *purpose* of issue, i.e., whether the bond is issued to finance universities, hospitals, schools, etc., (for example, see [6, 22]). Issue purpose dummy variables have been included to capture this effect. A third cross-section dummy variable tested in a small number of studies has been the location of the issuer.

Time-Series Variables Default risk variables that change over time have been entered as explanatory variables in tax-exempt bond yield regressions primarily to allow for the possibility that the relationship between the default risk premium on a bond and its rating category might change over time. This might happen if economic and financial conditions of an issuer change but the rating agencies do not change, or are slow to change, their ratings. Or, risk

[20] In studies of yield relationships these two effects are generally combined and called "default risk premiums." Strictly speaking the first is a "default" premium to compensate for the expected loss of holding a risky bond, while the second is a "risk" premium to provide additional compensation for holding a risky asset. See Lawler [38].

Table VIII

DEFAULT RISK EXPLANATORY VARIABLES USED IN TAX-EXEMPT BOND YIELD REGRESSIONS

	Cross Section Variables	Time Series Variables
direct measures	1. economic and fiscal characteristics of issuer [9, 10, 13, 24]	
indirect measures	1. rating category dummy variables [1, 2, 3, 4, 5, 6, 7, 8, 11, 12, 14, 15, 16, 17, 18, 19, 20, 21, 22, 23, 24, 25]	1. rating category dummy variables weighted by spread between Moody's low- and high-rated bond yield series at time of issue [8]
	2. issue purpose dummy variables [4,* 6, 7, 14, 16,* 19,* 20,* 22, 23*]	2. percentage change in real GNP [5, 19, 20, 21]
	3. location dummy variables [8, 10]	3. annual dummy variables [6]
		4. Proposition 13-related dummy variables and time trends [2]

* In these studies the individual purposes were grouped into aggregate "high-" and "low-" risk purpose categories.

premiums might change over time for reasons not directly related to the condition of the issuer. In an attempt to deal with these possibilities, one study [8] used a variant of the dummy variable method in which the dummy variables were weighted by yield spread series from Moody's. For instance, the Aa dummy variable was weighted by the spread between Moody's Aa and Aaa yield series. This forces the risk premium on all Aa-rated bonds in the sample to conform with the risk premium implied by Moody's yield series at the date of issue. As shown in Table VIII, other approaches to this problem have been to include the percentage change in real GNP or simple annual dummy variables.

Regression Results

Cross-Section Variables In two articles, Browne and Syron [9, 10] tested for the effect of numerous economic and financial characteristics of big cities on the yields of their general obligation bonds. They found that a city's unemployment rate, its volume of short-term debt per capita, its ratio of pension benefits to assets, and its location were significant determinants of the yield on its bonds. The characteristics used by Hastie [13] to capture the effect of default risk on the yields of general obligation bond yields of local governments included default history, the ratio of overall debt to true property values, and a measure of economic diversification.

Virtually all the studies that used the indirect dummy variable approach found the rating category of the issuer to be the most important determinant of variations in yields across tax-exempt bonds. For instance, the Broaddus-Cook study included dummy variables for bonds rated Aa, A1, A, Baa1, and Baa. (The omitted category was Aaa-rated bonds, so the coefficients of the dummy variables are interpreted as the increased yield relative to the going Aaa-rated bond necessary to sell the bond.) Over the sample period covered by the study (1977-1978) the estimated risk premiums for bonds at the 10-year maturity with these five ratings were 12, 33, 37, 55, and 75 basis points, respectively.

The studies that included issue-purpose dummy variables generally concluded that issue purpose for revenue bonds was associated with variations in default risk premiums among revenue bonds having the same rating. Of particular interest here are the studies of Bierwag, Hopewell, and Kaufman [6] and Sorenson [22] both of which included over ten issue-purpose dummy variables. Both studies found that within a given revenue bond rating category, hospital, university, and housing had above average default risk premiums while non-university schools, roads, and utilities had below average risk premiums. Sorenson reasoned that the important difference between the two groups of revenue bonds was that the first group of issuers compete with other suppliers of similar products or services and, as a result, their "future revenue flows are subjected to the uncertainties of future market share."[21]

Time-Series Variables In all those studies using it, the percentage change in real GNP had a significant and negative effect on a bond's yield relative to the going Aaa yield. This is consistent with the widely observed phenomenon that risk premiums on bonds of a given rating category tend to widen in periods of economic weakness. Similarly, the Broaddus-Cook regressions were modestly improved with the rating category dummy variables weighted by Moody's yield spread at the time of issue. Beebe [2], using time trends and dummy variables that took on a value of one after certain points in time, found that the effect of Proposition 13 on the risk premiums of California municipal bonds varied according to the type of bond (general obligation, revenue, tax allocation, or lease-purchase).

Default Risk and Maturity An area of concern related to default risk is the relationship of default risk premiums to term-to-maturity. One view of this relationship is that default risk premiums demanded by investors on the bonds of an issuer with a given rating category may increase with term-to-maturity because of the greater uncertainty associated with promised payments further out into the future. An alternative view is that—at least for low-rated issues—risk premiums may be larger the shorter the term-to-maturity of bonds of a given-rated issuer because of a "crisis-at-maturity." The rationale for the crisis-at-maturity effect on risk premiums for lower grade bonds is that "for these grades, the probability of default may increase as the final redemption date grows nearer and the company is unable to improve its financial condition" [49, p. 166].

Van Horne [49] surveyed the evidence from three studies on the relation between risk premiums and maturity in the corporate bond market and found that evidence from two of the three supported the notion that the lower the grade of the bond, the higher short-term default risk premiums are in relation to long-term risk premiums, consistent with the

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[21] If this argument is valid, the puzzle remains as to why these differences are not captured by the rating agencies.

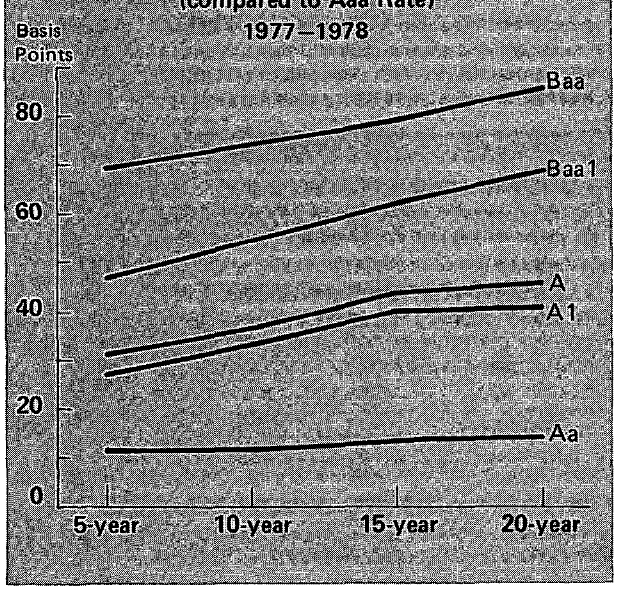
notion of a crisis-at-maturity. He emphasized that the relationship between risk structure and maturity can change over time, especially during economic downturns when crisis-at-maturity may grow in importance.

There is relatively little evidence on the relationship between risk premiums and maturity in the tax-exempt market. Looking at data for the 1940s and 1950s, Robinson [47] concluded that the differential between the yields of Baa-rated and Aaa-rated bonds widened as maturity lengthens. This finding is not consistent with crisis-at-maturity in the tax-exempt market in the years studied by Robinson.

To estimate the relationship between risk and maturity, the Broaddus-Cook study ran regressions using data only for general obligation issues that offered bonds at each of the 5-, 10-, 15-, and 20-year maturities. Also for the purposes of this exercise the default risk dummy variables alone were used so that the coefficients across rating categories and maturities could be easily compared. The coefficients for the five categories of lower than Aaa-rated bonds are graphed in the accompanying chart across the four maturities for which regressions were estimated. In every case the coefficients of a particular rating category increase with maturity. In particular, risk premiums rise at least as much from the 5- to 10-year maturity for the low-grade bonds as they do for the high-grade bonds. Also, the slopes of the term-to-maturity risk premium curves generally are *steeper* the lower the rating category. (Only from Baa1- to Baa-rated is there a slight drop in the slope.)

Chart 1

THE RELATIONSHIP BETWEEN DEFAULT RISK PREMIUMS AND YIELD TO MATURITY ON FIVE GRADES OF TAX-EXEMPT BONDS (compared to Aaa Rate) 1977–1978



While there was no recession during the study's sample period, risk premiums did move over a wide range especially at the beginning and end of the period. As a second test of the crisis-at-maturity effect, the sample was divided up into two subsets, one for the relatively "high risk" period and one for the "low risk" period, and regressions were run for both subperiods.[22] This second test also provided no support for the existence of a crisis-at-maturity for the lower rated issues. There was very little difference in the slopes of the term-to-maturity curves for the five risk categories over the two subperiods. Both were very close to the slopes for the total sample regressions shown in the chart.

In summary, the available evidence from the tax-exempt market is that the relationship between risk premiums and maturity is positive over the whole range of maturities. Hence, there is no evidence of a crisis-at-maturity effect in the tax-exempt market. However, the evidence is by no means overwhelming since the only regression study to address this issue did not include a recession. Also, the relationship between risk premiums and maturities of less than five years was not studied.

The Effect of New York City Default on Big City Tax-Exempt Bond Risk Premiums A final question related to default risk that has received some attention from a small number of the studies is whether or not the financial crisis of New York City in 1975 had an effect on the risk premiums, and hence on the yields, of bonds issued by similar Northern industrial cities. This question arose when the crisis spawned numerous reports (for example [42]) that the concurrent unfavorable publicity was pushing up the borrowing costs of other Northern industrial cities.

Three of the studies addressed this question by doing regressions with data from the period following the crisis. Browne and Syron [9] found that an equation with economic and financial conditions as explanatory variables understated the yields of four

[22] Specifically, the "high-risk" period covered all time periods when Moody's (Baa-Aaa) spread was greater than or equal to 78 basis points and the "low-risk" period was when the spread was less than 78 basis points. The cut-off point of 78 basis points was chosen arbitrarily to divide the sample roughly in half.

Northern industrial cities in 1976 by at least 90 basis points each. They leaned towards the conclusion that the market demanded a premium on the yields of these cities' securities because of the intense publicity following the New York crisis. Browne and Syron [10] conducted a follow-up study using 1978 data and an implication of this study was that the unexplained risk premium in the yields of the bonds of Northern industrial cities had declined considerably, if not disappeared.

Broaddus and Cook [8], using data from 1977-78, attempted to analyze the question by incorporating a dummy variable set equal to one for the issues of six large Northern industrial cities: Boston, Chicago, Cleveland, Detroit, Philadelphia, and Pittsburgh. The dummy variable had a highly significant coefficient of about 60 basis points. They also ran regressions with separate dummy variables for the issues of each of the six cities individually and in each case reported positive and significant coefficients. Broaddus and Cook attempted to gain insight into the cause of this result by comparing Moody's ratings at the end of 1981 for the 30 Northern industrial city issues in their sample to the ratings at the time of issue, reasoning that if there were a change in the economic and financial conditions of these cities not reflected in Moody's ratings at the time of issue, one would expect that eventually Moody's would lower their ratings. In fact, the ratings of 15 of the 30 issues were lowered subsequent to their issue date, none were raised and 15 were unchanged. The ratings of the cities of Boston, Chicago, Cleveland, Detroit, and Pittsburgh were all lowered, although the ratings of some of their associated districts were unchanged. On balance, it was felt that the only conclusion that could be drawn from these results was that much, if not most, of the "unexplained" risk premiums on issues of Northern industrial cities in the study reflected the relatively slow reaction of Moody's, as compared with investors, to deteriorating conditions in those cities.

Finally, a study by Kidwell and Trzcinka [20] using data from the summer of 1975 concluded upon analyzing the residuals of their regressions that these results "provide marginal support that the New York City crisis by itself led to higher borrowing costs for other municipalities (Detroit, Philadelphia, Cleveland)." In summary, there is some evidence from the regression studies that the New York financial crisis had a temporary effect on the yields of securities of similar Northern industrial cities. However, there is no evidence of a lasting effect.

III.
UNDERWRITER COMPETITION

Underwriter Conditions

Almost all of the regression studies consider a third potential source of variation in tax-exempt bond yields, namely the marketing conditions of the bond. As noted earlier, most tax-exempt issues are sold initially by the issuer to an underwriter who in turn sells them to the public. Bonds can be sold by issuers to underwriters via competitive bidding among numerous underwriting syndicates or through negotiation with only one syndicate. Almost all general obligation bonds are required by law to be sold via competitive bidding, while revenue bonds are sold both by competitive bidding and through negotiation.

If a bond is sold competitively it may receive as many as ten bids or as few as one. Table IX shows the number of bids received by the 793 competitively sold issues in the sample used by Broaddus and Cook [8]. The median number of bids received was four. Table IX also shows the number of bids received by rating category, illustrating that the lower rated issues tend to receive fewer bids. The *dispersion* of bids received by an issue as measured by the variance or range of bids also varies greatly. An issue, for example, that receives four bids may have these bids

Table IX

NUMBER OF BIDS ON COMPETITIVE ISSUES IN SAMPLE

(Moody's Ratings)

Number of Bids	Number of Issues	Aaa	Aa	A1	A	Baa1	Baa
1	17	0	4	2	4	6	1
2	113	12	22	16	34	17	12
3	184	39	38	37	39	17	14
4	136	22	27	24	37	15	11
5	113	17	29	29	23	8	7
6	70	7	27	12	14	7	3
7	74	10	28	11	21	3	1
8	31	3	8	7	10	3	0
9	25	5	15	1	3	1	0
10	11	3	8	0	0	0	0
greater than 10	19	6	9	3	1	0	0
Total	793	124	215	142	186	77	49

Source: Broaddus and Cook [8].

all come in close to the winning bid or the bids may be scattered over a wide range. Table X shows the range of bids for all the issues in the sample that received four bids.

Another underwriter competition characteristic is the issue's eligibility to be underwritten by banks. Prior to 1968, the Glass-Steagall Act of 1933 prevented banks from underwriting *all* revenue bonds. Hence, revenue bonds could be used as a proxy for bank eligibility to underwrite a given issue. The Housing and Urban Development Act of 1968, however, permitted banks to underwrite municipal revenue bonds issued to finance housing, university, or dormitory projects. Further, the Comptroller of the Currency may rule that a municipal revenue bond is in effect a general obligation bond eligible to be underwritten by national banks if the bond is backed by the full faith and credit of the issuer. According to the Public Securities Association [44], 40 percent of the revenue bonds issued in 1979 were eligible for bank underwriting.

This discussion points to four underwriter conditions that vary across issues: (1) method of sale, i.e., competitive bidding versus negotiation; (2) the number of bids received by competitive issues; (3) the dispersion of bids; and (4) bank underwriter eligibility. Various studies have included one or more of these variables as explanatory variables in tax-exempt yield regressions.

Before proceeding with a discussion of why underwriter conditions are thought to influence tax-exempt yields, it is useful to recall that many regression studies have used measures such as NIC or TIC which represent the total cost to an *issuer* of selling bonds, including both the underwriter spread plus the yield earned by the investor. This is a complication in interpreting the coefficients of the underwriter competition variables because there are a priori reasons, confirmed by empirical evidence (for example, see Kessel [17]), to expect that these variables affect not only yields to maturity but also underwriter spreads. Hence the coefficients of these variables may reflect the behavior of the underwriter or the investor or both. For that reason, when possible, the focus in this section is generally on those studies that used reoffering yields to maturity.

Table X

DISTRIBUTION BY RANGE OF BIDS OF ISSUES RECEIVING FOUR BIDS

Range of bids	Number of issues
0 < and ≦ .05	23
.05 < and ≦ .10	36
.10 < and ≦ .15	31
.15 < and ≦ .20	22
.20 < and ≦ .25	4
.25 < and ≦ .30	4
.30 < and ≦ .35	5
.35 < and ≦ .40	4
greater than .40	7
Total	136

Source: Broaddus and Cook [8].

Regression Results and Search Theory as a Possible Explanation

Kessel's Paper and the Number of Bids as a "Determinant" of the Reoffer Yield on Competitively Sold Bonds In early 1971, Kessel [17] presented an argument explaining why underwriter competition affects reoffering yields to maturity. He also presented evidence from a regression model in support of his argument. His major empirical finding was that the number of bids on competitively sold issues is negatively related to reoffering yields. This result has held up remarkably well in subsequent studies, and, until recently, his rationale for this relationship was widely accepted. For this reason, it is useful to begin with a discussion of Kessel's theory. The criticisms of his explanation will be discussed below.

Kessel employed George Stigler's thesis regarding search and the economic value of information to argue that an increase in the number of underwriters bidding on an issue would reduce the reoffering yield at which the issue could be sold to investors.[23] Specifically, Kessel hypothesized that information regarding potential buyers of a new municipal issue varied across underwriters. Each underwriter knew some prospective buyers not identified by other underwriters. On this basis, Kessel suggested that the number of bids was a proxy for the extent to which prospective final buyers of an issue had been identified and informed about an issue: the larger the number of bids, the greater the search and therefore the lower the reoffer yield.

[23] An earlier study by West [50] concluded that large issues receiving one and, to a lesser extent, two bids have higher reoffering yields because of monopsony in the underwriting and distribution of these securities. However, West also concluded that the number of independent buyers necessary to assure most of the benefits of competition is quite small and issuers, in any case, can take precautions to protect themselves from monopsonistic behavior by underwriters.

The magnitude of the effect of the number of bids on reoffering yields implied by Kessel's regression results is substantial. His coefficient for the natural logarithm of bids was —.14, which implies that issues receiving five and ten bids carry reoffer yields 23 and 32 basis points lower, respectively, than an issue receiving only one bid. In all subsequent studies in which the number of bids has been tested it has been found to be a significant determinant of reoffering yields, even though these studies have covered widely varying time periods [5, 8, 11, 12, 20].[24] The reported coefficients have been equal to or greater than Kessel's.

Dispersion of Bids In an attempt to extend the Kessel thesis, Benson [4] argued that the number of bids captures only part of the total effect of underwriter search on municipal bond yields. Specifically, he argued that the *intensity* of underwriter search varied across issues receiving the same number of bids due to variations in underwriter expectations of the benefits and costs of search. Benson assumed that the intensity of search varied inversely with the dispersion of bids. On the grounds that more intense search should uncover buyers willing to accept lower yields, Benson hypothesized a positive correlation between municipal yields and the variance of bids. His findings supported his hypothesis in the case of general obligation bonds, but not in the case of revenue bonds.[25] One other study [8] included a measure of the dispersion of bids—the range of bids—as an explanatory variable. The coefficient had the correct (i.e., positive) sign and was highly significant in each of the full sample regressions.

Negotiated versus Competitive A third underwriter competition variable tested in a limited number of studies including both competitively sold and negotiated issues is a dummy variable denoting the bond's sale through negotiation. Of the three studies reporting regressions with a negotiated dummy variable [2, 8, 14], only Broaddus and Cook [8] found a significant relationship. The coefficients of the negotiated dummy variable ranged between 11 and 15 basis points in the full sample regressions.

Joehnk and Kidwell [34] analyzed a sample of 730 paired competitive and negotiated bonds issued between 1970 and 1976 and found that the mean 10-year reoffering yield for negotiated issues exceeded the corresponding mean yield for competitive issues by 23 and 27 basis points, respectively, in the case of general obligation and revenue bonds.[26] They found this difference in yields consistent with the hypothesis that monopoly powers may exist with issues sold through negotiation. An alternative explanation following the search thesis, is that underwriters that do not go through the competitive bidding process might conduct a less thorough search for buyers than competitive underwriters. (A third interpretation will be given below.)

Bank Eligibility A fourth underwriter competition variable that has been tested in a small number of studies of revenue bond yields is bank eligibility to underwrite the bond. Cagan [11, 12] found bank eligibility to have a negative effect on reoffering yields of negotiated revenue issues and used Kessel's search theory to explain this result.[27] Bierwag, Hopewell, and Kaufman [6], however, argued that bank eligibility is correlated with issue purpose among revenue bonds and that Cagan's result largely reflected the absence of issue purpose variables in his regression. When they introduced issue purpose dummy variables into Cagan's regressions, the coefficient of bank eligibility was no longer significant. In an earlier study, Hopewell and Kaufman [16] reported a negative coefficient significant at the 15 percent level for bank eligibility.

The Attack on Search Theory

While search theory is perhaps an intuitively plausible explanation of the correlation between underwriter competition variables and reoffer yields, its application is vulnerable to criticism because it fails to explain *why* some issues receive more bids than others. Critics argue that on this point the search theory explanation for the correlation between underwriter competition variables and reoffer yields falls

[24] Not discussed here are those studies that used NIC or TIC as the dependent variable. All 13 of these studies that included the number of bids also reported a significant coefficient. These studies are listed in the references.

[25] Benson's dependent variable was TIC.

[26] Sorenson [23] argued that the effect of negotiation on yields differs according to the riskiness of the bond. Specifically, Sorenson estimated that negotiation actually reduced the NIC of lower rated issues.

[27] The literature on the effect of bank eligibility on revenue bond yields is voluminous and this article was unable to deal with the issue in any depth. Major combatants in the debate are Bierwag, Hopewell, Kaufman, and Leonard [6, 7] and Mussa [40, 41] on the side that bank eligibility does not lower yields and Cagan [11, 12] and Silber [48] on the side that bank eligibility does reduce yields.

apart. The criticism has two parts. First, it is argued that the number of bids is related to investment quality [41]:

> . . . underwriters will undertake costly search and marketing activities only if they are adequately compensated for the costs incurred in such activities. For some bond issues, the search and marketing costs will be low. These will typically be issues of well known borrowers with impeccable credit, particularly general obligation bonds issued by states and localities with high credit ratings. On intuitive grounds, one might expect that such issues would attract a large number of bids because the cost of marketing is low. For such issues, the potential bidder does not need to engage in a costly search for potential customers in advance of making his bid. As one of many bidders his costs must be low because his chance of winning is also low and he runs the risk of failing to recover these costs if he is a losing bidder. In contrast, issues by less well known or less credit worthy borrowers are likely to attract fewer bids because the costs of ascertaining a reasonable bid for such issues is greater than for issues with a ready market.

In support of the contention that the number of bids is related to investment quality, the critics of search theory point to the information shown earlier in Table IX that higher rated issues, on average, receive more bids.

The second part of the criticism is that variables included in tax-exempt yield regressions to capture variations in default risk are crude measures that fail to differentiate between issues of significantly different quality. As shown in Section II, the standard way to capture default risk in tax-exempt yield regressions is to use dummy variables corresponding to the rating categories of the rating agencies. These categories can cover a fairly wide ground in terms of basis points. The critics of search theory argue that the number of bids is in fact correlated with differences in "intrinsic quality" across bonds not captured by the rating category dummy variables.

Mussa [41] asserted that evidence in support of the argument that the coefficient of bids reflects quality differences is the larger estimated effect of bids on tax-exempt yields during periods (such as 1973) when there are large disparities among yields. Broaddus and Cook ran regressions for two subperiods when the spread between Moody's Baa and Aaa yield series was relatively high and when it was relatively low.[28] The average coefficient for the natural logarithm of bids in the high-risk period was .1885 while the average coefficient in the low-risk period was .1005. These results, reported in detail in [8], are consistent with Mussa's contention.[29]

The above argument is essentially that the number of bids is correlated with a "missing variable" related to default risk. A second and equally plausible "missing variable" argument is that number of bids is correlated with marketability of an issue.[30] There are great differences in the marketability of tax-exempt issues. Some have well developed secondary markets, while others have virtually no secondary markets. The bid-ask dealer spread on tax-exempt bonds with poor secondary markets is huge (e.g., as much as $5 per hundred), which means that an investor needing to sell such a bond prior to maturity has to take a large loss. It is reasonable to assume that investors demand a higher yield in compensation for this lack of marketability. Likewise, it is also very plausible that the less marketable an issue the more search—and hence the more cost—has to be done by underwriters before making a bid. If this is the case, then the number of bids would be highly and positively correlated with marketability. Since the tax-exempt yield regressions specify at best very crude explanatory variables to capture the effect of marketability, the number of bids may be capturing this effect.[31]

One might argue that proponents of the "missing variables" explanation should specify appropriate explanatory variables to capture the effects they claim are correlated with the underwriter competition variables. This would be difficult to do, however. There are no available direct measures of marketability (i.e., bid-ask spreads), especially for new issues, and the whole reason the rating category dummy variables have been used in regression studies is that the alternative of specifying numerous economic and financial characteristics of the issuer would be extremely cumbersome. In any case, both the search theory and missing variables explanations for the relationship between underwriter competition and reoffering yields are essentially ex post explanations for an unexpected result. The reader has to choose between the two on the basis of which is most plausible.

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28 These regressions were described in Section II.

29 However, proponents of search theory might argue that this difference in coefficients is also consistent with search theory. See [8].

30 Actually Mussa also seems to imply this. Hastie [13] uses number of bids as a proxy for marketability.

31 The argument here is for number of bids. However, it could also be applied to the negotiated dummy variable and the dispersion of bids. In particular, it has been reported that negotiated sales are often used by "lesser known" issuers [44]. If so, negotiated issues would in general have poorer marketability than competitive issues.

IV.
REGIONAL MARKET CONDITIONS

The Argument for an Effect of Regional Market Conditions on Tax-Exempt Bond Yields

A small number of studies have considered regional market conditions as a determinant of tax-exempt yields. (For reasons explained below, in all cases the region focused on is the state.) A priori, one would expect arbitrage by investors to eliminate all but very temporary differentials between the yields to maturity on comparable bonds issued in different regions. The essence of the argument that arbitrage may not eliminate all interregional yield differentials is that investors inside and outside a region are subject to different costs, taxes, and other considerations that create a gap between the observed yield to maturity on a region's bonds and the true yield earned by investors inside versus outside the region.[32] Specifically, there are three factors which may affect in-state investors differently from out-of-state investors: information costs, differential taxes, and commercial bank pledging requirements.

Information Costs The first and most widely cited reason why regional market conditions may affect tax-exempt bond yields is information costs. As noted earlier, many municipal bond issues are relatively small and are handled by local or regional underwriters that sell primarily in local or regional markets. The cost to an investor of obtaining information about, say, a local sewer bond issued in a different state might be considerable. Similarly, the cost to an underwriter (and hence to an issuer) of searching for and identifying distant buyers for the bond might also be considerable. If these costs are significant, then the yields on bonds in a region could deviate from the "going" yields on similar bonds outside the region without triggering interregional arbitrage.

Taxes The second reason why in-state investors are willing to accept lower yields on in-state bonds is that income from a municipal bond is typically exempt from state and local income taxes within the state of issue but not in other states. As a result the true after-tax yield on a bond with a given before-tax yield is different for in-state and out-of-state investors. This creates the incentive for investors to buy tax-exempt bonds issued within their own state. The tax rates applicable to individual investors in each state vary over a great range from 0 to over 15 percent.[33] In Virginia, for example, an individual investor earning 7.6 percent on an in-state issue would require over 8 percent on an out-of-state issue to get the same after-tax yield. Hence, if the out-of-region yield were 8 percent, yields in Virginia would have to fall below 7.6 percent before Virginia investors would be induced to buy non-Virginia bonds. Conversely, if the yield on Virginia bonds were 7.6 percent the yield on out-of-region bonds would have to rise above 8 percent before Virginia investors would be induced to buy non-Virginia bonds. This creates a range of 40 basis points over which Virginia bond yields could move in response to regional market conditions without inducing interregional arbitrage.

Pledging Requirements A third factor that may permit regional market conditions to affect individual bond yields in a given state is the effect of state and local "pledging" requirements at commercial banks against state and local deposits in that state. Some states require banks to hold securities equal to 100 percent or more of the value of their deposit liabilities to the state and its political subdivisions. Other states have less stringent requirements, and still others have no requirements or very low requirements. Those states that impose such requirements invariably accept as eligible collateral U. S. government and agency securities and securities issued by the state in question and its political subdivisions. Most states, however, *do not* accept out-of-state municipal securities as eligible collateral [28]. Hence, banks in states with high pledging requirements must purchase substantial amounts of in-state issues or Federal securities if they wish to acquire public deposits.[34] Consequently, banks may be willing to accept a lower yield on in-state bonds than on out-of-state bonds in order to gain the return associated with attracting public deposits.[35]

In summary, information costs, differential state income tax treatment of in-state and out-of-state municipal bond interest, and differential treatment of in-state and out-of-state municipal bonds for pledging

32 The theory behind the existence of "regional market segmentation" in the tax-exempt bond market has not been rigorously formulated. This explanation is from [8].

33 See [27].

34 Studies that have examined the effects of pledging requirements on bank behavior are [1, 28, 29, 46]. Most recently, Ratti [46] found that the demand by banks for state and local securities is greater as a result of the presence of pledging requirements.

35 The pledging requirements in the various states as of 1979 are summarized in [28].

purposes may have created a range over which a given region's yields move in response to regional supply and demand factors without inducing arbitrage activity. This phenomenon has often been referred to as "market segmentation"—a term that explains little and that conjures up the image of investors too lazy, ignorant, or irrational to arbitrage away interest rate differentials across regions. The point here is that information costs, state income taxes, and pledging regulations may prevent this arbitrage by creating gaps between the true after-tax yields earned by investors inside and outside a region on that region's bonds.

Regression Results

Before proceeding with the results of the studies that have incorporated regional variables, a few preliminary comments are necessary. First, as noted above, in all these studies the "regional" market used was the state. This is because the pledging and tax arguments relate specifically to the state and because "regional" data are available only on a state basis. Second, the argument has been made that, to the extent that regional conditions influence tax-exempt yields, the effect should be inversely related to the size of the issue [15]. The essence of this argument is that unit information costs decline as issue size rises because the relatively fixed costs of acquiring information about an issue in another region are spread over more dollars and the effect on yield is smaller. Third, the argument has also been made that the effect of regional variables—especially those related to the pledging effect—might vary inversely with maturity [8]. This is because banks which are the major holder of tax-exempt bonds, purchase primarily short- and intermediate-term bonds with maturities generally not exceeding 15 years.

Demand Variables: The Pledging Effect In theory, if the pledging requirements, state income taxes, and information costs create a situation in which yields in a region can move over a range without triggering interregional arbitrage, then anything affecting the demand for or supply of bonds in a given region might affect yields in that region relative to the "national" yield. In practice, the only demand variable that has been tested in the tax-exempt yield regressions is bank demand related to pledging requirements. Two studies have examined this effect on tax-exempt yields. The first [1] which was part of a larger study of public deposit insurance for the Advisory Commission on Intergovernmental Relations (ACIR), included dummy variables constructed to measure the pledging effect. The pledging dummies were based on a classification of states into "high-pledge," "moderate-pledge," and "low-pledge" categories. The results of the analysis suggested that pledging requirements reduced NIC of general obligation bonds in the high-pledge states by 5 to 20 basis points relative to those in the low-pledge states. Further, the effect appeared to be more consistently significant in the latter part of the 1966-1974 period covered due to the apparent substitution of municipal for Treasury and agency securities as collateral for public deposits by banks in the late 1960s and early 1970s.

The dummy variable included to measure the pledging effect in the ACIR study differentiated among states only on the basis of the character of their pledging requirements. It took no account of differences in the proportion of short-term assets held by state and local government units solely in the form of bank deposits. However, both the stringency of pledging requirements and the relative share of government funds held in bank deposits are relevant. For this reason Broaddus and Cook [8] used the percentage of total deposits in a state subject to pledging requirements. This pledging variable worked well in the full sample and general obligation bond regressions. Its coefficients had the expected sign and were highly significant at the 5-, 10-, and 15-year maturities. The coefficient was much smaller and was not significant in the 20-year regressions. This pattern was not unexpected since banks are less important participants at the long end of the market. The pledging variable was not significant in any of the revenue bond equations. This result may reflect the ineligibility of revenue bonds as collateral in some states with high pledging requirements.

The Broaddus-Cook study also tested for the effect of size by adding a multiplicative term of the pledging variable times the logarithm of the size of the issue. This multiplicative term had the expected sign and was significant at the 5-, 10-, and 15-year maturities in the full sample and general obligation equations, where the basic pledging effect exists. The results suggest that at the 10-year maturity, for example, relatively high pledging requirements reduce reoffer yields on the order of 30 basis points for small issues to 10 basis points or less for issues exceeding $200 million.

Supply Variables Three studies have tested the effects of regional supply variables on tax-exempt bond yields and all three found them to be significant on yields. Hendershott and Kidwell [15] used the standard regression model (with NIC as dependent variable) to estimate regional supply effects on yields of bonds issued in Indiana between 1970 and 1974. Their supply variable was the recent volume of new municipal securities issued by Indiana government units relative to the recent volume of new issues in the national market. They also included the supply variable multiplied by the logarithm of issue size. The supply variable coefficient was positive and the coefficient on the multiplicative term was negative. Both were significant at the one percent level. They concluded that a regional supply effect existed, but that the effect was inversely related to issue size.

Both the ACIR [1] and the Broaddus-Cook [8] studies included as a supply variable the ratio of the currently outstanding stock of state and local bonds to state personal income.[36] In the ACIR study this variable was significant and had the expected positive sign in a majority of the regressions. The coefficients in the Broaddus-Cook regressions also had the expected positive sign and were highly significant in all of the full sample equations. Since this variable measures the outstanding stock of regional bonds rather than the flow of new issues, these results imply that an increase in the supply of regional issues has a permanent effect on the yields of new regional issues as long as the increase is reflected in a rise in the ratio of the stock of regional issues to regional income. This implication differs somewhat from the results of Hendershott and Kidwell's analysis, which was inconclusive on this point.

V.
OTHER VARIABLES EMPLOYED IN TAX-EXEMPT YIELD REGRESSIONS

This section will discuss a small number of variables that do not fit easily into the above four categories. Two of these—issue size and general obligation versus revenue bond status—have been used in numerous studies, yet the expected effect on yields has either generally not been discussed, or remains a matter of controversy. Also, there are a couple of recent articles that have argued for the existence of market segmentation by type of bond that will be discussed briefly at the end of the section.

[36] Actually, in the ACIR study this variable was interpreted as both a regional supply variable and a default risk variable [1, p. 520].

Size of Issue[37]

Theories of the Effect of Issue Size on Reoffering Yield Issue size has been included as an explanatory variable in about three-fourths of the tax-exempt yield regression studies (see references). However, only a handful of these studies have attempted to articulate the expected relationship between issue size and tax-exempt yields and even these few studies offer diverse hypotheses of the relationship. Tanner [25] argued that supply effects would drive up yields on large issues because the demand curve for any particular issue is downward sloping. Benson, Kidwell, Koch, and Rogowski [5] agreed that these supply effects exist, but they argued that the size of issue is also a proxy for marketability. They argued that marketability increases with issue size and investors will accept a lower yield in return for greater marketability. Hence, the marketability effect of size on yield is the opposite of the supply effect. They postulated that the marketability effect initially would dominate, but that at some issue size the supply effect becomes dominant. Hence, "the expected relationship between yield and size may be U-shaped."[38]

Problem in Interpreting the Coefficient of Issue Size Before summarizing the regression results of the effect of issue size on yields two problems that are encountered in interpreting the coefficients of size should be discussed. First, size of issue is generally viewed as being a determinant of underwriter spread which is a component of NIC and TIC. Consequently, in those tax-exempt yield regressions that use NIC or TIC as the dependent variable, the coefficient of issue size may reflect the behavior of underwriters or the behavior of investors.[39]

[37] This discussion is concerned exclusively with the direct effect of issue size on yield. Size may also effect yields through its interaction with regional market variables. See Section IV.

[38] A third rather tenuous hypothesis was offered by Kessel [17] who found an unexpected negative relationship between size and yield, and argued that the relationship reflected a weakness in the use of number of bids as a proxy for underwriter search. Specifically, he argued that, ceteris paribus, larger underwriters do more search than smaller underwriters. Also larger underwriters tend to bid on large issues. Hence, issue size captures the additional search on a given issue as opposed to a smaller one with the same number of bids.

[39] To complicate things further, issue size is also used as an explanatory variable in equations attempting to explain the number of bids received by a competitively sold issue. Hence, issue size is thought to affect underwriter spread both directly and indirectly through its effect on the number of bids. (See Kessel [17].)

The second problem in interpreting the coefficient of issue size is that, in all but one of the regression studies, size is measured in *nominal* terms. This is relevant because the theories of the relationship between size and yield are implicitly theories of the effect of size relative to the size of other current new issues. If a data sample covers an extended period of time, size measured in nominal terms may be incompatible with theories of how size is supposed to affect yield. This problem is aggravated by the fact that, due to inflation in the 1960s and 1970s, issue size has an upward trend in the period covered by virtually all the regression studies. The longer the data period covered by a regression, the greater is the risk that the coefficient of size is picking up some spurious correlation between the trend in size and in the dependent variable. An example of this is Kessel's study which used data covering a nine-year period. Over this period there was a significant downward trend in the spread between lower rated and Aaa-rated bonds. Kessel's regression shows a negative and highly significant coefficient on size but this may simply reflect correlation between the upward trend in size and the downward trend in risk premiums over the period.[40]

Regression Results The regression results for issue size show a division depending on whether the dependent variable was (1) reoffering yield or (2) NIC or TIC. Of the nine studies that found a significant relationship between size and NIC or TIC, all but one found a simple positive relationship [2, 3, 4, 6, 7, 15, 19, 24]. The one exception was the ACIR study [1], which found a negative relationship for small issues and a positive one for large issues. Conversely, of the four that found a significant relationship between size and reoffering yield, two [17, 21] found a simple negative relationship and none found a simple positive relationship.[41]

Unfortunately, the two studies that found a significant negative relationship between issue size and reoffering yields used data series covering nine years and measured size in current dollars. Thus, they are subject to the criticisms discussed above. Benson, Kidwell, Koch, and Rogowski [5] corrected for the trend in size by measuring it in price-deflated (i.e., "real") dollars. They tested a quadratic specification for issue size and concluded that increased size reduced reoffering yield up to a certain point ($26 million in 1972 dollars) after which further increases in size raised reoffering yields. As noted, they attributed this pattern to a combination of marketability and supply effects. Broaddus and Cook [8], who used data covering a period of two years, tested various forms of issue size and found that the quadratic specification worked best.

In summary, even though issue size has been used as an explanatory variable in 22 regression studies of tax-exempt yields, there is no generally accepted theory of how issue size should affect reoffering yields. A common and perhaps intuitively plausible belief is that issue size is a proxy for marketability and that consequently it should have a negative effect on reoffering yields. However, none of the regression studies have provided any evidence of the link between issue size and bid-ask spreads. Studies using reoffering yield as the dependent variable found a negative relationship between size and yield but these studies are subject to the criticism that they measure size in nominal dollars over an extended period of time. One study dealt with this problem by measuring size in constant dollars and found increases in issue size exert a downward impact on yields up to a point and an upward effect thereafter.

Revenue versus General Obligation Bonds

Almost all of the studies using data for both general obligation and revenue bonds have included a revenue bond dummy variable to capture any systematic difference between the yields on general obligation and revenue bonds not captured by the other explanatory variables. In all cases, the coefficient of the revenue bond dummy variable has been positive and significant.

Explanations for the Positive Relationship Between Revenue Dummy Variable and Yields Virtually all the studies that have included a revenue bond dummy variable fail to specify the predicted effect of this variable on tax-exempt yields. Consequently, the explanations are basically ad hoc attempts to explain a statistically significant result. Three explanations have been offered for the positive coefficient of the revenue bond dummy variable. Hopewell and Kaufman [16] argued that the positive effect of revenue status on yields reflects a perception on the part of investors that revenue bonds carry a higher default risk than general obligation bonds of equal rating. Broaddus and Cook argued that an equally plausible

[40] Tanner [25] pointed out this problem with Kessel's study.

[41] This would seem to imply that the net effect of issue size on underwriter spread is positive. However, the behavioral interpretation of the result is not clear because issue size is thought to affect underwriter spread both directly and indirectly through its effect on the number of bids (see footnote 39).

explanation is that the revenue bond coefficient reflects the relatively poorer marketability of revenue bonds. Revenue bonds generally have poorer secondary markets and higher bid-ask spreads than comparable (in size and rating) general obligation bonds.[42] Investors would therefore be expected to demand a somewhat higher yield on revenue bonds in compensation for the greater loss experienced if those bonds are sold prior to maturity. Since none of the tax-exempt yield regressions include a variable that directly captures the effect of marketability on yield, the revenue bond dummy variable may capture some of this effect. Finally, Silber [48] has suggested that the revenue bond dummy variable picks up the effect on yields of bank ineligibility to underwrite most revenue bonds.

Regression Results Ten of the regression studies included a revenue bond dummy variable and all of these reported a positive and statistically significant coefficient. Six used NIC or TIC as the dependent variable and four used reoffering yields (see references). There is no systematic difference in the magnitude of the coefficient between the two sets of studies.

Kessel [17] estimated that revenue status raised reoffer yields approximately 8.5 basis points. Although Kessel offered no explicit rationale for the inclusion of the revenue bond dummy variable in his specification, Silber argued that the coefficient captures a direct effect of bank ineligibility on reoffer yields. This interpretation is possible since Kessel used pre-1968 data, when virtually all revenue bonds were ineligible.

The coefficients of the revenue bond dummy variable in the Broaddus-Cook regressions, all of which are significant, indicate that revenue status increases yields from about 6 basis points at the 5-year maturity to about 10 basis points at the longer maturities. These results are quite close to Kessel's estimate. The coefficients of the revenue bond dummy variable in the other studies were generally higher, in some cases over 20 basis points [15, 19, 20, 21].

In summary, it seems clear that the yields on revenue issues are systematically higher than comparably rated general obligation bonds. However, it is not clear whether this reflects (1) default risk not captured by the rating category dummy variables, (2) poorer marketability of revenue bonds, or (3) bank ineligibility to underwrite most revenue bonds. Ultimately, the only way to answer this question is to include as explanatory variables more explicit measures of default risk, marketability, and bank eligibility.[43]

Segmentation by Class of Bond

Benson, Kidwell, Koch, and Rogowski [5] argued that because of regulation and liquidity needs, banks purchase primarily high-quality tax-exempt bonds and that, consequently, changes in commercial bank demand for tax-exempt bonds should influence the relationship between the yields on high- and low-rated bonds. To test this theory they ran the standard tax-exempt yield regression model with the ratio of bank net purchases of tax-exempt securities to total net issues as an additional explanatory variable. The regression supported their hypothesis that increases in commercial bank demand for tax-exempts increase the differential between the yield on low-rated bonds and the yield on high-rated bonds.

Using the same type of reasoning, Kidwell and Koch [19] argued that the markets for general obligation and revenue bonds might be segmented. To test their hypothesis they included, in addition to the revenue bond dummy variable, a number of terms constructed by multiplying the revenue bond dummy variable by other variables. They concluded that the differential between the yields on revenue bonds and those on high-grade general obligation bonds increases as commercial banks increase their purchases of net tax-exempt securities and as the supply of revenue bonds increases relative to the total supply of new tax-exempt bonds. They also concluded that the spread between revenue and general obligation yields varies inversely with the GNP growth rate.

Summary and Conclusions

This article has surveyed the evidence from 25 regression studies on the determinants of individual tax-exempt bond yields. There is general agreement among the studies as to why coupon, call provision, and default risk variables should affect tax-exempt bond yields. The variables used in the regressions fit the underlying rationales fairly well, although the complexity of call price schedules has made it difficult, if not impossible, to devise an accurate proxy

[42] This statement was confirmed by discussions with underwriters. Also, some indirect evidence on this point comes from a 1973 Municipal Finance Officers Association Survey [39], which reported that a new revenue issue has an average marketing cost per $1,000 about twice as large as that of a new general obligation issue ($3.84 versus $1.98).

[43] In the case of bank eligibility that has in some cases been done. See Section III.

to capture call price effects. The regression results for these variables are fairly good. Many studies, however, have had difficulty estimating the effect of call risk for the reasons discussed earlier.

The relatively few studies that have included regional market conditions variables have found them to have the predicted effect on tax-exempt yields. The basic idea underlying the inclusion of these variables is that because investors inside and outside a region face different taxes, costs, and regulations, yields within a region can move over a limited range in response to regional market conditions without inducing interregional arbitrage. This theory, while plausible, has not been given a rigorous theoretical formulation. Hence, the specific choice of variables used to test it has been somewhat arbitrary. The same limitation applies to testing for the effects of segmentation by class of bond. Also, the rationale for this type of segmentation seems to be weaker because it does not rely on differential taxes and information costs.

The regression results for the underwriter competition variables, especially the number of bids, are remarkably consistent across studies. There is strong disagreement, however, on the ability of search theory to explain the correlation between these variables and tax-exempt yields. In particular, a number of recent studies (especially [40, 41]) have argued that the underwriter competition variables are picking up differences in intrinsic quality and marketability not captured by other variables in the regression. The basic problem is that the search theory explanation in its present state does not clearly link the underwriter competition variables to aggregate underwriter search.

The problem with the issue size and revenue bond dummy variables is that they are not clearly related to the theories that have been used to justify their inclusion in tax-exempt yield regressions. As noted, the appropriate solution to this problem would be to include variables that are directly related to the underlying theory. Most important from this standpoint would be a variable that accurately reflects marketability. The logical choice is the bid-ask spread in the secondary market.[44] In the absence of an accurate measure of marketability, the interpretation of the coefficients of issue size and the revenue bond dummy variable (and, for that matter, the underwriter competition variables) will remain clouded.

In conclusion, the regression studies surveyed in this article provide much information on the determinants of individual tax-exempt bond yields. In a couple of instances further evidence might be obtained through the inclusion or more careful specification of a variable. More generally, however, it seems unlikely that additional regression analysis will shed new light on the determinants of tax-exempt bond yields. Rather, the need is for a more rigorous and clearer articulation of the theories underlying the variables employed. Most important in this regard is a theory convincingly linking the underwriter competition variables, especially the number of bids, to aggregate search. Also, in the case of market segmentation by region (or by class of bond) it would be desirable to have a more rigorous theory of how heterogeneous investors confronted with different taxes, costs, and regulations can lead to a situation in which regional market conditions affect the relative yields on tax-exempt bonds.

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[44] There is no bid-ask spread for new issues but a reasonable proxy would be the bid-ask spread on other outstanding bonds of the issuer or the bid-ask spread of the new issue after it begins to trade in the secondary market. The difficulty here is that there is no comprehensive published data on bid-ask spreads in the tax-exempt market, so this data would have to be gathered through the dealer community.

References with Tax-Exempt Bond Yield Regressions

Article	Central Focus and Conclusion	Data	Regressions Reported	Dependent Variable	Issue Characteristics			Issuer Characteristics			Regional Market Conditions			Underwriter Conditions				Other		
					call dummy variable	other call variables	coupon variables	rating category dummy variables	purpose of issue dummy variables	other default risk variables	pledging variables	other demand variables	supply variables	number of bids	dispersion of bids	negotiated dummy variable	bank eligibility dummy variable	revenue bond dummy variable	size of issue	other
1. Advisory Committee on Intergovernmental Relations. **The Impact of *Increased Insurance on Public Deposits*.** Study prepared for the Committee on Banking, Housing and Urban Affairs, United States Senate. Washington: Government Printing Office, 1977.	**The Pledging Effect:** Bonds from states with high pledging requirements sell *at lower yields than bonds from states* with low pledging requirements. Hence, interest costs on bond issues from high pledging states would increase if these pledging requirements were substantially reduced or supplemented by full insurance on public deposits.	21,437 general obligation issues; *1966-74*	For each year: 1. bonds less *than $5 million* 2. bonds greater than $5 million	NIC				×			×	×	×						×	
2. Beebe, Jack H. "The Effect of Proposition 13 on California Municipal Debt." **Economic Review, Federal Reserve Bank of San Francisco (Winter 1979), pp. 25-38.**	**Default Risk:** The effect of Proposition 13 on California municipal bond yields varied according to the type of bond (GO, revenue, tax-allocation, or lease-purchase).	367 California issues; 1977-78	1. GO 2. revenue 3. tax allocation 4. lease-purchase	NIC				×		×				×		×			×	
3. Benson, Earl D. "Municipal Bond Interest Cost, Issue Purpose and Proposition 13," **Governmental Finance, September 1980, pp. 15-19.**	**Revenue versus GO bonds:** The Proposition 13-inspired tax revolt and the New York City crisis have altered the way investors view the basic GO-revenue relationship.	2904 issues sold competitively; 1976-79	1. inclusive 2. utility 3. school 4. college 5. hospital 6. various purposes	NIC	×			×						×	×			×	×	
4. ———. "The Search for Information by Underwriters and Its Impact on Municipal Interest Cost." **The Journal of Finance, 34 (September 1979), pp. 871-885.**	**Underwriter Competition:** Municipal interest cost is significantly related not only to the number of underwriter bids received but also to the dispersion of bids, which is used as a proxy for the intensity of underwriter search.	319 issues sold competitively; June-August 1973	1. inclusive 2. GO 3. revenue	TIC	×			×	×					×	×			×	×	
5. Benson, Earl D., David S. Kidwell, Timothy W. Koch, and Robert J. Rogowski. "Systematic Variation in Yield Spreads for Tax-Exempt General Obligation Bonds." **Journal of Finance and Quantitative Analysis (December 1981), pp. 685-702.**	**Default Risk and Segmentation by Investment Grade:** Yield spreads between tax-exempt general obligation bonds of different bond ratings vary systematically with changes in economic activity and commercial bank investment activity.	10,165 general obligation bonds sold competitively; 1966-75	1. inclusive 2. Aa 3. A 4. Baa 5. noninvestment	10-year reoffering yield				×		×				×					×	×
6. Bierwag, G. O., Michael H. Hopewell, George G. Kaufman. **Bank Eligibility to Underwrite Municipal Revenue Issues and the Cost of Borrowing: An Empirical Analysis. New York: Securities Industry Association, 1979.**	**Underwriter Competition:** There is no convincing statistical evidence that permitting banks to underwrite municipal revenue bonds has resulted in any cost savings to issuers.	815 revenue issues; 1973-76	1. negotiated 2. competitive	NIC		×		×	×	×				×			×		×	
7. Bierwag, G. O., Michael H. Hopewell; George G. Kaufman, and Paul A. Leonard. **Commercial Bank Eligibility to Underwrite Municipal Revenue Issues and the Cost of Borrowing: Additional Evidence for 1979. Eugene, Oregon: Center for Capital Market Research, University of Oregon, 1980.**	**Underwriter Competition:** same as above.	396 revenue issues; 1979	1. negotiated 2. competitive	NIC		×		×	×					×			×		×	
8. Broaddus, Alfred and Timothy Cook. "An Analysis of the Determinants of the Yields on Individual Municipal Securities." Federal Reserve Bank of Richmond. Unpublished paper presented at 1981 meeting of Western Finance Association.	**Coupon and Call Effects, Regional Market Conditions, and Maturity Effects:** Analysis of many determinants of tax-exempt bond yields requires the use of reoffering yield as dependent variable; call provisions and coupons affect tax-exempt bond yields; regional market conditions influence tax-exempt bond yields; the impact of many determinants of tax-exempt bond yields varies systematically across maturity.	914 issues, 4 maturities; 1977-78	1. inclusive 2. general obligation 3. revenue 4. competitive revenue	5-, 10-, 15-, and 20-year *reoffering* yields and NIC	×	×	×	×		×	×		×	×	×	×		×	×	
9. Browne, Lynn E. and Richard F. Syron. "Big City Bonds After New York." **New England Economic Review, Federal Reserve Bank of Boston (July/August 1977), pp. 3-15.**	**Default Risk:** A city's unemployment rate, ratio of pension benefits to assets, and short-term debt per capita are significant determinants of big city default risk premiums; the bonds of four Northeastern cities had large "unexplained" risk premiums in 1976.	29 general obligation bonds of large cities; 1976	inclusive	8-year secondary market yield						×										
10. Browne, Lynn E. and Richard F. Syron. "Municipal Market Since the New York City Crisis." **New England Economic Review, Federal Reserve Bank of Boston (July/August 1979), pp. 11-26.**	**Default Risk:** A city's unemployment rate, short-term debt per capita, and region are significant determinants of big city default risk premiums.	29 general obligation bonds of large cities; 1978	inclusive	6-year secondary market yield						×										
11. Cagan, Phillip. **The Interest Saving to States and Municipalities from Bank Eligibility to Underwrite All Nonindustrial Municipal Bonds. Washington, D. C.: Dealer Bank Association, 1978.**	**Underwriter Competition:** Bank eligibility reduces reoffering yields on municipal bonds.	503 issues; 1973-76	1. competitive 2. competitive revenue 3. negotiated revenue	15-year reoffering yield		×		×						×			×		×	
12. ———. **The Interest Saving to States and Municipalities from Bank Eligibility to Underwrite All Nonindustrial Municipal Bonds: A Supplementary Analysis. Washington, D. C.: Dealer Bank Association, 1979.**	**Underwriter Competition:** same as above.	835 issues; 1973-78	1. competitive 2. competitive revenue 3. negotiated revenue	15-year reoffering yield		×		×						×			×		×	
13. Hastie, K. Larry. "Determinants of Municipal Bond Yields," **Journal of Financial and Quantitative Analysis (June 1972), pp. 1729-1748.**	**Default Risk and Marketability:** Municipal bond yields are a function of the bond's default risk and marketability. Relative significance of various factors depends on whether banks or individuals are dominating the market.	644 noncallable general obligation bonds; 1957, 1960, 1963, 1965, 1967	1. 1957 2. 1960 3. 1963 4. 1965 5. 1967	secondary market yield between 15 and 25 years						×										×

References with Tax-Exempt Bond Yield Regressions (cont.)

Article	Central Focus and Conclusion	Data	Regressions Reported	Dependent Variable	Issue Characteristics: call dummy variable	Issue Characteristics: other call variables	Issue Characteristics: coupon variables	Issuer Characteristics: rating category dummy variables	Issuer Characteristics: purpose of issue dummy variables	Issuer Characteristics: other default risk variables	Regional Market Conditions: pledging variables	Regional Market Conditions: other demand variables	Regional Market Conditions: supply variables	Underwriter Conditions: number of bids	Underwriter Conditions: dispersion of bids	Underwriter Conditions: negotiated dummy variable	Underwriter Conditions: bank eligibility dummy variable	Other: revenue bond dummy variable	Other: size of issue	Other: other
14. Heins, A. James. "The Interest Rate Differential Between Revenue Bonds and General Obligation Bonds: A Regression Model." **National Tax Journal**, 15 (December 1962), 399-405.	**Revenue versus GO Bonds:** Revenue bonds have higher yields than GO bonds of equal rating.	471 issues; 1956-59	1. GO 2. revenue	NIC	x			x	x							x			x	
15. Hendershott, Patric H., and David S. Kidwell. "The Impact of Relative Security Supplies: A Test with Data from a Regional Tax-Exempt Bond Market." **Journal of Money, Credit and Banking**, 10 (August 1978), 337-47.	**Regional Market Conditions:** An increase in the relative supply of small-size tax-exempt bonds in a regional market increases both regional borrowing costs and costs of particular bond issues relative to those nationwide.	389 issues sold competitively in Indiana; 1970-74	inclusive	NIC	x			x					x	x				x	x	
16. Hopewell, Michael H., and George G. Kaufman. "Commercial Bank Bidding on Municipal Revenue Bonds: New Evidence." **The Journal of Finance**, 32 (December 1977), 1647-1656.	**Underwriter Competition:** Revenue bonds carry higher interest costs primarily because market participants believe them to carry greater risks than GO bonds comparable in other characteristics. Permitting banks to bid on these bonds would not increase the number of bids on the average	340 issues sold competitively; June-August 1973	inclusive	TIC	x			x	x					x			x	x	x	
17. Kessel, Reuben. "A Study of the Effects of Competition on the Tax-Exempt Bond Market." **Journal of Political Economy**, 79 (July/August 1971), 706-738.	**Underwriter Competition:** Reoffering yields decline as the number of bids increase, because bids constitute search by issuers for those who most prize the bonds they have to sell; bank eligibility decreases reoffering yields indirectly by increasing the number of bids.	6503 issues sold competitively; 1959-67	1. inclusive 2. GO 3. revenue	20-year reoffering yield		x		x						x				x	x	x
18. Kidwell, David S. "The Exante Cost of Call Provisions on State and Local Government Bonds." **Journal of Economics and Business**, 30 (Fall 1977), 73-78.	**Call Provisions:** The shorter the call deferment period, the higher the exante interest cost of a general obligation issue.	340 issues sold competitively; summer of 1973	1. GO 2. revenue 3. callable 4. callable GO 5. callable revenue	TIC	x	x		x						x				x	x	
19. Kidwell, David S. and Timothy W. Koch. "The Behavior of the Interest Rate Differentials Between Tax-Exempt Revenue and General Obligation Bonds: A Test of Risk Preferences and Market Segmentation." **The Journal of Finance**, 37 (March 1982), 73-85.	**Revenue versus GO Bonds:** The yield differential between revenue bonds and similar general obligation bonds varies contracyclically with the level of economic activity. Significant segmentation exists between the markets for general obligation and revenue bonds.	20,466 issues sold competitively; 1966-75	inclusive	NIC	x			x	x					x				x	x	x
20. Kidwell, David S., and Charles A. Trzcinka. "Municipal Bond Pricing and the New York City Fiscal Crisis." University of Tennessee Working Paper, 1981.	**Default Risk:** The New York City fiscal crisis by itself did not lead to fundamental or long-lasting changes in investor risk perceptions, resulting in higher interest rates in the municipal bond market. However, there is evidence consistent with the hypothesis that New York raised the borrowing costs of large, economically depressed northern cities.	2309 issues of $1 million or more sold competitively; 1974-75	1. inclusive 2. June-July 1975 3. July-August 1975	10-year reoffering yield	x	x		x	x	x				x				x	x	
21. Rogowski, Robert J. "Underwriting Competition and Issuer Borrowing Costs in the Municipal Revenue Bond Market." **Journal of Bank Research** (Winter 1980), pp. 212-220.	**Underwriter Competition:** Eligibility of commercial banks to underwrite college revenue issues increases underwriting competition and reduces issuer borrowing cost.	621 college issues sold competitively; 1966-74	1. inclusive 2. revenue	20-year reoffering yield		x		x		x							x	x	x	
22. Sorenson, Eric H. "Bond Ratings Versus Market Risk Premiums." **The Journal of Portfolio Management** (Spring 1980), pp. 64-69.	**Default Risk:** Risk premiums on revenue bonds of the same credit rating differ depending on the purpose of the issue.	504 revenue issues; 1971-76	inclusive	NIC		x		x	x					x					x	x
23. ———. "Negotiated Municipal Underwritings: Implications for Efficiency." **Journal of Money, Credit and Banking**, 11 (August 1979), pp. 366-370.	**Underwriter Competition:** If an issue is subject to high demand uncertainty, negotiation can generate a cost savings relative to using competitive bidding.	504 revenue issues greater than $1 million; 1971-76	1. Baa and lower 2. A and higher	NIC		x		x	x					x					x	x
24. Swenson, "The Cyclical Behavior of the Net Interest Cost Between General Obligation Bonds and Revenue Bonds." **National Tax Journal** (March 1974), pp. 123-140.	**Revenue versus General Bonds:** Revenue bonds have a higher NIC than comparable general obligation bonds. No evidence was found of cyclical behavior in the difference between the NIC of revenue and general obligation bonds.	1942 issues of $750,000 million or greater issued in California, Indiana and Texas; 1955-70	1. inclusive 2. GO 3. revenue	NIC	x			x		x				x					x	
25. Tanner, J. Ernest. "The Determinants of Interest Cost on New Municipal Bonds: A Reevaluation." **Journal of Business**, 48 (January 1975), pp. 74-80.	**Regression Procedure:** Estimating "cross-section" tax-exempt bond yield regressions with data covering a long period of time may cause statistical problems.	67 issues sold in three 2-day periods	1. October 27-28, 1971 2. December 22-23, 1971 3. February 2-3, 1972	NIC				x						x					x	

Note: Excluded from the "Other" column are all variables used to capture national market conditions. Variables included here are:

[5, 19] variables to capture segmentation by class of bond;

[13] proxies for marketability;

[17] outstanding bonds of issuer;

[22, 23] first time issued.

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