DOCUMENT RESUME

ED 231 051 EA 015 712

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TITLE	Michigan School District Response to a Guaranteed Tax Base: A Time-Series Cross-Sectional Analysis. A Rand Note.
INSTITUTION	Rand Corp., Santa Monica, Calif.
SPONS AGENCY	Department of Health and Human Services, Washington, D.C.
REPORT NO	RAND-N-1608-HHS
PUB DATE	Oct 82
GRANT	HHS-016B-7905
NOTE	30p.
AVAILABLE FROM	Publications, Rand Corporation, 1700 Main Street, Santa Monica, CA 90406 ($4.00).
PUB TYPE	Viewpoints (120) -- Reports - Research/Technical (143)
EDRS PRICE	MF01 Plus Postage. PC Not Available from EDRS.
DESCRIPTORS	*Cross Sectional Studies; Data Analysis; Elementary Secondary Education; Estimation (Mathematics); *Finance Reform; Longitudinal Studies; Mathematical Formulas; Mathematical Models; Property Taxes; *School District Spending; School Taxes; State Aid; *State School District Relationship; Tax Allocation; Tax Rates; Time Perspective
IDENTIFIERS	*Guaranteed Tax Base Plan; *Michigan

ABSTRACT

This study analyzes the effectiveness of a "guaranteed tax base" (GTB) as a reform measure designed to reduce the traditional heavy reliance of school financing on local property tax revenues and to help equalize per pupil expenditures across districts. Such measures call for matching locally raised tax dollars with state aid and consequently lowering the price of expenditures to some school districts. Previous studies by Feldstein (1975) and Ladd (1975) estimate school district expenditure equations using cross-sectional data from Massachusetts. Their results indicate a fairly large price effect on expenditures. The author's study of the price effect over a 5-year period of a GTB currently in place in Michigan rejects this approach as inadequate apart from an analysis of variables over time. Variables considered in the Michigan study include the price effect of the GTB on school districts, representative households, and state and federal aid. Results of a pooling of data for all years for all 451 school districts studied indicate that the effect of the GTB plan on expenditures in Michigan has been so small as to be of no policy significance. These results run counter to the earlier findings of Feldstein and Ladd and suggest that their results should perhaps be rejected. (JBM)

The research reported herein was performed pursuant to Grant No. 016B-7905 from the U.S. Department of Health and Human Services, Washington, D.C.

The Rand Publications Series: The Report is the principal publication documenting and transmitting Rand's major research findings and final research results. The Rand Note reports other outputs of sponsored research for general distribution. Publications of The Rand Corporation do not necessarily reflect the opinions or policies of the sponsors of Rand research.

Published by The Rand Corporation

PREFACE

This Note is an outgrowth of work, reported in Park and Carroll (1979), that was funded by the National Institute of Education and the U.S. Department of Health, Education, and Welfare (now the U.S. Department of Health and Human Services). Here the authors provide additional estimates and tests of equations to explain school district expenditure behavior in the State of Michigan. This additional work was supported by the Department of Health and Human Services.

The new work leads the authors to reject some of their earlier estimates but does not change the overall conclusion: The effect of Michigan's guaranteed tax base plan on school district expenditures has been very small.

SUMMARY

Five years of data for 451 school districts in Michigan are pooled to estimate equations explaining school district expenditure behavior upon introduction of a guaranteed tax base (GTB) plan. Hausman's (1978) specification test applied to a complex random-effects model rejects that model, while implicitly rejecting cross-sectional estimates as well. Fixed-effects estimates show that the influence of Michigan's GTB plan on school district expenditure has been so small as to be of no policy significance.

ACKNOWLEDGMENTS

We appreciate the help of Richard Fernandez, Jerry Hausman, Lee Lillard, Willard Manning, Bruce Wetzel, and participants in a Rand work-in-progress seminar; but we absolve them all of any responsibility for errors.

CONTENTS

I. INTRODUCTION

School districts spend widely disparate amounts of money per pupil. The disparities have led the courts and state legislatures to consider reforms in the way schools are financed--reforms designed to reduce the traditional heavy reliance on local property tax revenues and to help equalize expenditures per pupil across districts.

One popular idea for reform is called a "guaranteed tax base" (GTB) plan, because the state in effect guarantees school districts a certain level of assessed property value per pupil. If the actual assessed value in a district falls short of the guaranteed level, the state matches locally raised revenue to make up the difference. During the 1973/74 school year, for example, Michigan guaranteed each district \$38 per pupil for each mill of tax effort up to 22 mills. Under the Michigan plan, a district with \$19,000 of assessed value per pupil and a tax rate of 20 mills would raise \$380 per pupil locally and would receive an additional \$380 per pupil in state matching aid.

The GTB plan changed the form of state aid to school districts in ways that affect the districts' incentives to spend money on education. By matching locally raised tax dollars with state aid, the plan effectively lowered the price of expenditures to some school districts. In theory, we should expect a district faced with a lower price to "buy" more educational expenditure. The effect of any such plan depends on the magnitude of responses by districts to these incentives.

In this report, we use pooled time-series cross-sectional data from 451 school districts, gathered over a five-year period,[1] to estimate the effect of Michigan's GTB plan on expenditures.

Previous studies by Feldstein (1975) and Ladd (1975) estimate school district expenditure equations using cross-sectional data from Massachusetts. Their results indicate a fairly large price effect on expenditures. Feldstein's estimated price elasticities range from -.9 to -1.6, and Ladd's between -.5 and -.7.

[1]Michigan operated some 530 unified districts during this period. Districts excluded from the sample were generally small, rural districts for which census data were not available.

The primary purpose of this Note is to determine whether the Feldstein and Ladd results hold up in another state with a GTB plan in effect. Our conclusion is that they do not. In Michigan, we estimate price elasticities that are so close to zero as to be of no practical use as a policy tool.

Why are Feldstein's and Ladd's estimates so much higher than ours? Our analysis suggests a possible econometric explanation for at least part of the difference. Cross-sectional estimates, like Feldstein's and Ladd's,[1] are biased if the error term is correlated with any of the independent variables. There is no way to check for the bias using cross-sectional data alone, but when pooled time-series cross-sectional data are available, Hausman's (1978) specification test can detect the problem. Applying Hausman's test to Michigan data decisively rejects cross-sectional estimates. Perhaps if Feldstein's and Ladd's cross-sectional estimates could be similarly tested, they would be rejected as well.

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[1]And like similar estimates reported in Park and Carroll (1979).

II. THE DATA

Table 1 lists the variables used in the analysis. The policy variables--those under the control of the school districts, the state, or the federal government--are E, PRICE, STBLK, STCAT, and FEDCAT. We seek to explain school districts' choice of E, operating expenditure per pupil. The natural logarithm of E is the dependent variable in all of our equations. The other, explanatory variables are discussed below.

PRICE TO SCHOOL DISTRICTS

Under a guaranteed tax base plan such as that now in use in Michigan, a school district may be able to "buy" an additional dollar of educational expenditure for less than a dollar of locally raised revenue. In Michigan in 1973/74, for example, the state guaranteed $38,000 of assessed value per student up to a school operating tax rate of 22 mills.[1] Consider a district with an assessed value (AV) less than $38,000 per student. In 1973/74 it faced an increasing block price schedule such as that shown in Fig. 1. If its demand for educational expenditure were D_1, it would buy E_1 dollars per pupil. Up to $836 (22 mills x $38 per mill), each additional dollar of E would cost it AV/38 of locally raised revenue, which the state would match with 1 - AV/38 of matching aid. That is, the PRICE of E to the district is AV/38.

A district with a higher demand D_3 would buy E_3 dollars per pupil. Additional dollars would have to come entirely from local revenue, so PRICE equals 1. However, the state would match the first 22*AV dollars per pupil of local revenue with 22*(38-AV) dollars per pupil. This amount is a lump-sum transfer to the district that does not affect the PRICE of additional E.

[1] In 1974/75, the corresponding values were $39,000 and 25 mills. In 1975/76, the state guaranteed $42,400 for the first 20 mills and $38,250 for the next 7 mills.

Table 1

LIST OF VARIABLES

Variable	See Note: A	B	C	D	Brief Description	Dimension
E	X				Operating expenditure per pupil	Dollars per pupil
TIME				+	1 in first year, ...5 in fifth year	Years
DUM5				-	1 in fifth year; 0 otherwise	Dimensionless
GROWTH				-	Pupils this year/pupils last year	Dimensionless
STBLK	X			+	Unrestricted state aid per pupil	Dollars per pupil
STCAT	X			+	Categorical state aid per pupil	Dollars per pupil
FEDCAT	X			+	Federal aid per pupil	Dollars per pupil
CAT	X			+	Sum of STCAT and FEDCAT	Dollars per pupil
PRICE	X	X		-	Cost to district of one-dollar addition to E	Dollars per dollar
AV		X		+	Assessed value per pupil	$1000 per pupil
PUP/HHLD		X	X	-	Pupils per household	Pupils per household
RESIDENT		X	X	-	Proxy for residential fraction of total assessed value	Dimensionless
DISTVAL		X	X	-	Ratio of median to mean value of owner-occupied housing	Dimensionless
OWNOCC		X	X	-	Ratio of owner-occupied to total dwelling units	Dimensionless
P2		X	X	-	Ratio of median value of owner-occupied housing to AV	$1000/$1000
Y			X	+	Median household income	Dollars per year
PRIVATE			X	-	K-12 private school pupils per capita	Pupils per capita
POVERTY			X	-	Fraction of families in poverty status	Dimensionless
PROF			X	+	Fraction of employed persons 16 or older in professional, technical, or kindred occupations	Dimensionless
MINOR			X	+	Nonwhite fraction of population	Dimensionless
URBAN			X	+	Urban fraction of population	Dimensionless
OLDER			X	-	Fraction of population 55 or older	Dimensionless
STABIL			X	+	Fraction of population 5 or older still in same house as 5 years ago	Dimensionless
POPULAT			X	±	Population	Persons

Notes: A = policy variable.
B = component of price to representative household.
C = Census variables fixed year to year at 1970 values.
D = expected sign.

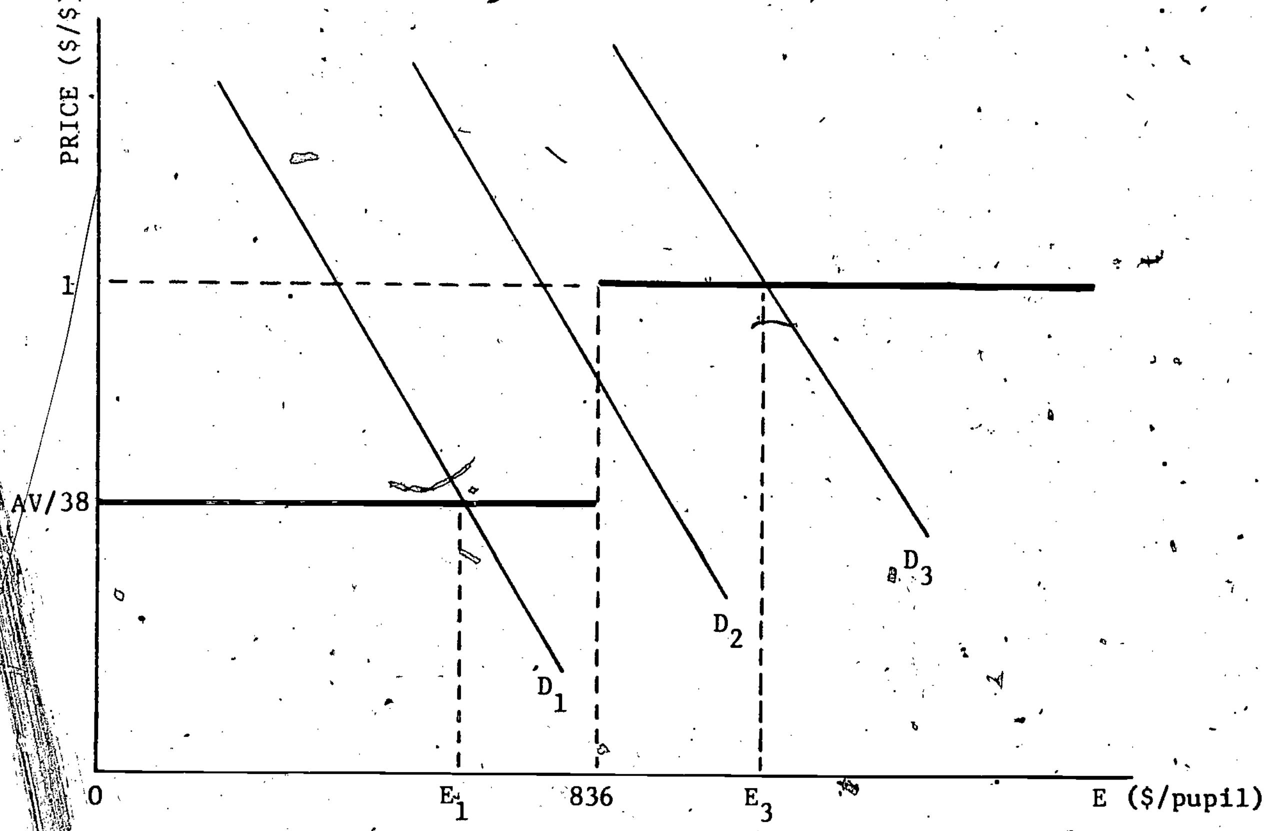

Fig. 1--The PRICE of expenditures per pupil to Michigan school districts, 1973/74

What about districts whose demand curves, like D_2 in Fig. 1, pass through the gap in the price schedule? We can recognize them because their tax rate for school operation ("operating millage," or OM) will exactly equal the limit for state matching aid. Strictly speaking, their PRICE is indeterminate. One more dollar of E would cost them a full dollar of local revenues; one less would save them only AV/38. The PRICE that we would like to use in estimating the demand curve falls somewhere in between, and in principle should be estimated simultaneously with the coefficients of the demand curve. For now, we treat D_2 districts like D_1 districts, setting PRICE equal to AV/GTB. In the estimation section, we find that our conclusions are not affected if instead we exclude them from the sample.

A possible econometric problem is the endogeneity of PRICE. Because a positive random error can push a district to a higher block of the price schedule, PRICE will be positively correlated with the error term. Thus the estimated coefficient of PRICE will be biased in a positive direction; coefficients of other variables can be either positively or negatively biased. We check on the importance of the bias in the estimation section and find that it is too small to affect our conclusions.

Districts with an AV greater than or equal to GTB receive no matching aid; for them, PRICE equals 1 for all values of E. Also, for 1971/72 and 1972/73, when the foundation plan was in effect, PRICE equals 1 for all districts. Finally, transitional guarantees based on 1972/73 revenues were effective for some districts in the first GTB year, 1973/74; PRICE in 1973/74 equals 1 for these districts.

Table 2 shows the number of districts where PRICE is less than 1 in each year. It appears that the variation of PRICE over time and across districts should be sufficient to support estimates of the effect of PRICE on expenditure.

Table 2

NUMBERS OF DISTRICTS IN VARIOUS PRICE CATEGORIES

Category	School Year 1971/72	1972/73	1973/74	1974/75	1975/76
PRICE less than 1	0	0	114	230	284
PRICE indeterminant (OM equals limit for matching aid)	0	0	28	41	14
PRICE equals 1 (Under foundation plan)	451	451	0	0	0
(Transitional guarantees)	0	0	37	0	0
(AV exceeds GTB)	0	0	24	36	57
(OM exceeds limit for matching aid)[a]	0	0	248	144	96

[a] The limit for matching aid was increased each year as follows:

Year	Limit (mills)
1973/74	20
1974/75	25
1975/76	27

PRICE TO THE REPRESENTATIVE HOUSEHOLD (P2 *PRICE)

If school boards are responsive to pressures from residents in their districts, then it is not only PRICE to the district that matters, but also price to the households in the district. Price to the representative household, P_i, will be greater or less than PRICE to the district as the household's assessed valuation (in $1000), V_i, is greater or less than AV:

$$P_i = PRICE * (V_i/AV)$$
$$\equiv PRICE * P_2 .$$

This is algebraically equivalent to

$$P_i = PRICE * (V_i/AV) * (n/n) * (N/N) * (V_r/V_r)$$
$$= PRICE * (N/n) * (V_r/V) * (V_i/(V_r/n)),$$

where N is the number of students,
n is the number of households,
V is the total assessed valuation (=AV *N),
V_r is the assessed valuation of residential property.

If we take the representative household to be the one in the median-valued, owner-occupied house, all the terms in this expression are variables defined in Table 1:

$$P_i = PRICE * PUP/HHLD * RESIDENT * DISTVAL .$$

If the individual components of P_i affect demand only through their effect on P_i, all the terms in this expression should enter the demand equation with the same exponent. This may be too restrictive, however, in light of the fact that some of the terms may pick up taste or wealth effects on demand in addition to pure price effects. Consequently, we began with an unrestrictive specification that allowed separate exponents on the different components of P_i and tested various restrictions on the exponents. The conclusion was that PRICE, P2, and AV should enter the equation separately, but that further disaggregation is not compelled by the data.[1]

STATE AND FEDERAL AID (STBLK, STCAT, FEDCAT, AND DUM5)

STBLK is unrestricted state aid to the district exclusive of marginal matching aid; STCAT is categorical state aid; FEDCAT is federal aid. All three are measured in per-pupil terms.

We can distinguish several forms of state aid:

1. Marginal matching grants under a GTB plan to districts with AV less than GTB and OM less than the matching limit.
2. Inframarginal matching aid under a GTB plan to districts with AV less than GTB and OM greater than the matching limit.
3. Unrestricted nonmatching aid under a transitional guarantee in 1973/74.
4. Unrestricted nonmatching aid under a foundation plan in 1971/72 and 1972/73.
5. Categorical aid.

The first item shows up as a lower PRICE to the district and is not counted again in STBLK. We add together items 2 through 4 to get STBLK. The fifth item is STCAT.

Like PRICE, STBLK is endogenous during the years with a GTB plan in effect; its coefficient will also be biased upward by positive correlation of STBLK with the error term. In the estimation section, we find that this bias is also too small to affect the conclusions.

Michigan's state aid program was not fully funded in 1975/76. To make up the shortfall, each district was "taxed" 4 percent of the sum of its local revenues and state noncategorical aid. The "tax" was deducted from the state's noncategorical aid payments to the district. If the "tax" exceeded those payments, the balance was deducted

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[1] Park and Carroll (1979, pp. 14-16).

from the state's categorical aid payments to the district. The shortfall was not recognized until after districts had established their 1975/76 local property tax rates. Thus, 1975/76 expenditures were planned in response to the aid program's parameters. However, the subsequent reductions in their revenues probably affected the observed values of E. Accordingly, we used the aid program's parameters to compute the 1975/76 values for PRICE, STBLK, and STCAT (gross of the subsequent deductions) for each district and introduced a dummy variable DUM5, equal to 1 in 1975/76 and zero otherwise, to control for the effects of the tax on E.

OTHER VARIABLES

The other variables used in the analysis are self-explanatory. Many of them are calculated using 1970 census figures, and thus are fixed from year to year at 1970 values. This does not introduce much error for those that change only slowly over time--probably all except median household income, Y, and population, POPULAT. Even Y and POPULAT cause no problems to the extent that they are uniformly trended over time for all districts, because the effect of the trend will be picked up by the TIME variable. Nonuniform changes in these variables across districts will bias the estimated coefficients because of errors in variables problems.

Summary statistics for all variables are shown in Table 3.

Table 3

SUMMARY STATISTICS

	School Year												
	1971/72		1972/73		1973/74		1974/75		1975/76				
Variable	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev	Census Variable	Mean	Std Dev
E	791.21	120.35	863.94	133.28	957.53	147.97	1089.80	167.76	1197.40	186.79	P2	.6007	.5585
GROWTH	--	--	1.0073	.0381	1.0005	.0416	1.0000	.0370	1.0013	.0353	PUP/HHLD	.9342	.2884
STBLK	343.58	115.95	364.01	133.43	252.92	225.47	134.47	221.96	102.83	207.60	RESIDENT	.7101	.6864
STCAT	44.83	25.89	55.14	26.65	71.60	25.46	75.50	29.50	64.28	40.25	DISTVAL	.9003	.0588
FEDCAT	31.04	30.58	34.88	32.87	36.07	35.18	45.14	44.17	57.52	47.56	Y	8923	2468
CAT	75.87	45.80	90.02	49.87	107.66	51.48	120.64	61.99	121.80	74.00	OWNOCC	.8126	.0833
PRICE	1.000	0.0	1.0000	0.0	.8241	.2705	.6976	.2758	.7039	.2529	PRIVATE	.0230	.0241
AV	16.863	7.921	18.507	8.966	20.351	10.587	23.042	12.839	25.792	14.850	POVERTY	.0849	.0481
DUM5	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	1.00	0.0	PROF	.1158	.0492
TIME	1.00	0.0	2.00	0.0	3.00	0.0	4.00	0.0	5.00	0.0	MINOR	.0284	.0761
											URBAN	.3541	.3951
											OLDER	.1779	.0558
											STABIL	.5745	.0809
											POPULAT	17667	72088

NOTE: Census variables are fixed at their 1970 values.

III. ESTIMATED EXPENDITURE EQUATIONS

We pool the data for all five years for all 451 school districts to estimate a series of expenditure equations. All variables except TIME, TIME**2, and DUM5 are in natural logarithms.

We specify a model that allows for persistent district-specific effects, owing perhaps to the influence of stable unobserved variables or unobservable taste differences. Our first estimates treat the district effects as random errors in a variance components framework. Then random-effects (RE) estimates would be unbiased and efficient on the assumption that the district-specific errors are independent of the explanatory variables (together with other standard assumptions). We test and reject that assumption using a procedure proposed by Hausman (1978). Implicitly rejected by the same test are the single-year cross-sectional estimates reported in Park and Carroll (1979). If the district effects are correlated with the independent variables, unbiased estimates are still attainable by treating the district effects as fixed rather than random and using a fixed-effects (FE) estimator (Kiefer, 1980). The FE estimates differ very little from the RE estimates. We also run additional FE estimates to check on the effect of the endogeneity of PRICE and STBLK, and on the effect of our treatment of districts whose demand curves pass through the gap in the price schedule. None of the estimates change the basic conclusion that the effect of the GTB plan on expenditure in Michigan has been so small as to be of no policy significance.

RANDOM-EFFECTS ESTIMATES

In an earlier Rand report (1979), we calculated RE estimates based on two commonly specified error structures:

$$v_{it} = w_i + e_{it} \qquad \text{(VARCOMP)}$$

and

$$v_{it} = u_{it} = \rho u_{i,t-1} + e_{it}, \qquad \text{(AUTOCORR)}$$

where districts are indexed by i and years by t. In both cases, we assumed that the errors are uncorrelated across districts, so that when observations are arranged first by district and within district by time, the error covariance matrix has a block diagonal structure:

$$E(vv') = \sigma_v^2 V = \sigma_v^2 \begin{bmatrix} A & & & \text{symmetric} \\ 0 & A & & \\ \vdots & \vdots & \ddots & \\ 0 & 0 & \dots & A \end{bmatrix} \tag{1}$$

For VARCOMP,

$$A = \begin{bmatrix} 1 & & & & \text{symmetric} \\ \gamma & 1 & & & \\ \gamma & \gamma & 1 & & \\ \gamma & \gamma & \gamma & 1 & \\ \gamma & \gamma & \gamma & \gamma & 1 \end{bmatrix}, \ \gamma = \sigma_w^2/\sigma_v^2 ,$$

and for AUTOCORR,

$$A = \begin{bmatrix} 1 & & & & \text{symmetric} \\ \rho & 1 & & & \\ \rho^2 & \rho & 1 & & \\ \rho^3 & \rho^2 & \rho & 1 & \\ \rho^4 & \rho^3 & \rho^2 & \rho & 1 \end{bmatrix}.$$

Upon further investigation, it is apparent that the true error structure is "between" the VARCOMP and AUTOCORR specifications in the following sense. If we use the coefficients b estimated using either model to calculate residuals $\hat{v} = [\hat{v}_1 \dots \hat{v}_{451}] = y - Xb$ (where $\hat{v}_i$ is a 5-vector of yearly residuals for district i), and then estimate the diagonal blocks of the error covariance matrix as

$$\hat{A} = \Sigma_i \hat{v}_i \hat{v}_i' \quad \text{(normalized so that } \mathrm{tr}(\hat{A}) = 5\text{)}, \tag{2}$$

we find that the elements of A decrease away from the diagonal (contrary to the VARCOMP assumption), but that they do not decrease as rapidly as the AUTOCORR assumption implies. We are thus led to specify a mixture of the two error structures, first used by Lillard and Willis (1978):

$$\begin{aligned} v_{it} &= w_i + u_{it} \\ &= w_i + \rho u_{i,t-1} + e_{it}, \end{aligned} \tag{MIXED}$$

with corresponding diagonal blocks of the error covariance matrix

$$A = \begin{bmatrix} 1 & & & & \text{symmetric} \\ \gamma+\rho(1-\gamma) & 1 & & & \\ \gamma+\rho^2(1-\gamma) & \gamma+\rho(1-\gamma) & 1 & & \\ \gamma+\rho^3(1-\gamma) & \gamma+\rho^2(1-\gamma) & \gamma+\rho(1-\gamma) & 1 & \\ \gamma+\rho^4(1-\gamma) & \gamma+\rho^3(1-\gamma) & \gamma+\rho^2(1-\gamma) & \gamma+\rho(1-\gamma) & 1 \end{bmatrix}. \tag{3}$$

We estimate the MIXED model as follows:

1. Calculate ordinary-least-squares estimates $b = (X'X)^{-1}X'y$.
2. Calculate residuals $\hat{v} = y = Xb$.
3. Calculate $\hat{A}$ according to Eq. (2).
4. Find R such that $R'R = V^{-1}$, and calculate transformed variables $y^* = Ry$ and $X^* = RX$.
5. Calculate generalized-least-squares estimates $b = (X^{*\prime}X^*)^{-1} X^{*\prime}y^*$.
6. Iterate on steps 2 through 5 until the elements of $\hat{A}$ change by less than .001 from one step to the next.

7. Calculate $\hat{\rho}$ and $\hat{\gamma}$ by maximum likelihood, using Jöreskog and Sörbom's LISREL program (1978) to fit Eq. (3) to Eq. (2).[1]
8. Use $\hat{\rho}$ and $\hat{\gamma}$ in Eq. (3) to calculate $\hat{A}$.
9. Do steps 4 and 5 to calculate the final value for b_{RE}.

The resulting estimates are shown in Table 4 under the heading "Random Effects." Of particular interest are the estimates of the effects of PRICE and STBLK. These effects are much smaller than those estimated by Feldstein and Ladd. The effect of STBLK is statistically different from zero, but both effects are so small that for practical purposes they might as well be zero.

SPECIFICATION TEST

Maddala (1971, p. 357) was apparently the first to point out that random-effects estimates using pooled data are biased and inconsistent if the individual effects are not independent of the explanatory variables. Hausman (1978) has recently proposed a test of the assumption that $E(w_i|X_i) = 0$.

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[1]See Lillard and Weiss (1979, fn. 3, p. 441) for details; our estimates use their "alternative" procedure. Comparison of the direct estimate of the diagonal blocks of the error covariance matrix

$$\hat{A} = \begin{bmatrix} .980 & & & & \text{symmetric} \\ .865 & .959 & & & \\ .805 & .863 & .987 & & \\ .745 & .800 & .870 & .979 & \\ .745 & .799 & .848 & .911 & 1.096 \end{bmatrix}$$

with the two-parameter fitted estimate

$$\hat{A} = \begin{bmatrix} 1 & & & & \\ .888 & 1 & & & \\ .814 & .888 & 1 & & \\ .766 & .814 & .888 & 1 & \\ .734 & .766 & .814 & .888 & 1 \end{bmatrix}$$

suggests that the MIXED model describes the error structure quite well.

Table 4

ESTIMATED EXPENDITURE EQUATIONS

Variable	Random Effects	Fixed Effects (1)	(2)	(3)	(4)	(5)
CONSTANT	5.578 (23.3)	--	--	--	--	--
TIME	.043 (9.8)	.045 (10.4)	.037 (7.0)	.037 (6.8)	.012 (1.4)	.032 (5.6)
TIME**2	.011 (13.3)	.011 (13.2)	.012 (12.8)	.012 (12.4)	.016 (11.1)	.013 (12.3)
DUM5	-.059 (11.5)	-.060 (11.7)	-.065 (13.2)	-.060 (12.4)	-.063 (13.8)	-.069 (13.0)
GROWTH	-.338 (11.6)	-.340 (11.5)	-.365 (11.6)	-.366 (11.6)	-.373 (11.9	-.377 (11.0)
STBLK	.002 (2.5)	.002 (2.7)	.002 (2.8)	.001 (1.5)	.002 (1.3)	.002 (2.8)
STCAT	.018 (6.8)	.014 (5.3)	.012 (4.2)	.012 (4.2)	.012 (4.1)	.010 (3.3)
FEDCAT	.018 (8.0)	.015 (6.5)	.016 (6.6)	.016 (6.8)	.017 (7.1)	.016 (6.0)
PRICE	-.001 (0.3)	-.004 (0.7)	-.005 (1.0)	-.007 (1.3)	-.018 (3.6)	-.005 (0.9)
SEV	.053 (4.5)	.036 (2.7)	.039 (2.9)	.038 (2.8)	.050 (3.5)	.056 (3.1)
OWNOCC	-.181 (3.6)	--	--	--	--	--
P2	-.068 (4.1)	--	--	--	--	--
Y	.071 (2.4)	--	--	--	--	--
PRIVATE	-.048 (0.2)	--	--	--	--	--
POVERTY	-.045 (2.8)	--	--	--	--	--
PROF	.046 (2.9)	--	--	--	--	--
MINOR	.485 (5.9)	--	--	--	--	--
URBAN	.117 (4.8)	--	--	--	--	--
OLDER	-.016 (0.8)	--	--	--	--	--
STABIL	.012 (0.3)	--	--	--	--	--
POPULAT	-.002 (0.3)	--	--	--	--	--
ρ	.655 (14.3)	.655[b]	--	--	--	--
γ	.674[a]					

NOTES: Dependent variable is the logarithm of operating expenditures per pupil. All independent variables except TIME, TIME**2, and DUM5 are in natural logarithms. Estimated t-statistics are in parentheses. The t-statistics for the fixed-effects estimates are corrected as necessary to account for degrees of freedom used to implicitly estimate district-specific effects. The five separate fixed-effects estimates differ as follows:

(1) Estimated using Kiefer's procedure assuming a known intertemporal covariance matrix.
(2) Estimated using Kiefer's procedure for an unknown intertemporal covariance matrix, as are (3) through (5).
(3) Two-stage estimates using PRICE and STBLK implied by a first-stage regression of tax rate OM on independent variables.
(4) PRICE and STBLK are exogenously given as the minimum and maximum values, respectively, attainable by the district under the GTB plan in effect each year.
(5) Like (2), except excludes districts whose demand curves pass through the gap in the PRICE schedule during at least one GTB year.

[a] Estimated indirectly as $\sigma_w^2/(\sigma_w^2 + \sigma_u^2) = \sigma_w^2/(\sigma_w^2 + \sigma_e^2/(1-\rho^2))$, based on direct estimates of σ_w^2 = .00577 (10.7) and σ_e^2 = .00159 (22.2), t-statistics in parentheses.

[b] Assumed equal to ρ estimated in random effects model.

Hausman's test compares b_{RE} with an alternative estimate that is consistent whether or not the null hypothesis $H_o: E(w_i|X_i) = 0$ is true. If the two estimates are significantly different, the null hypothesis is rejected. The alternative estimator in our case is a fixed-effects estimator, which treats the individual effects w_i in the MIXED error structure as fixed rather than random. If u_{it} were not autocorrelated, the FE estimator would be ordinary least squares on variables transformed to deviations from district means:

$$y^* = My \text{ and } X^* = MX,$$

where

$$M = I_{NT} - Z(Z'Z)^{-1} Z'$$

and

$$Z = I_N \otimes 1_T \quad (1_T \text{ is a column vector of ones}).$$

Then

$$b_{LSDV} = (X^{*\prime}X^*)^{-1} X^{*\prime}y^* .$$

However, as Kiefer (1980, p. 196) points out, when the u_{it} are correlated over time, the correct transformation depends on their covariance matrix. Specifically, the FE transformation is then

$$y^* = BFy \text{ and } X^* = BFX ,$$

where

$$B = I_{NT} - FZ(Z'F'FZ)^{-1} Z'F ,$$

and in our case

$$F = I_N \otimes \begin{bmatrix} \sqrt{1-\rho^2} & 0 & 0 & 0 & 0 \\ -\rho & 1 & 0 & 0 & 0 \\ 0 & -\rho & 1 & 0 & 0 \\ 0 & 0 & -\rho & 1 & 0 \\ 0 & 0 & 0 & -\rho & 1 \end{bmatrix} .$$

For the specification test, we substitute $\hat{\rho} = .655$ (from the RE estimate) in F, and calculate

$$b_{FE} = (X^{*\prime}X^{*})^{-1} X^{*\prime}y^{*} ,$$

the results of which are shown in fixed-effects column (1) in Table 4. Although there is little apparent difference between b_{RE} and b_{FE}, the difference is statistically significant.

The regression form of Hausman's test compares the random-effects transformed regression with the following auxiliary regression: y transformed for a random-effects estimate regressed on both X transformed for a random-effects estimate and X transformed for a fixed-effects estimate. If the additional variance explained by adding the fixed-effects transformed X is significant, the hypothesis that $E(w_i|X_i) = 0$ must be rejected. In our case, the additional variance is significant at beyond the .01 level ($F_{9,2225} = 3.30$). Thus we must reject the RE estimates.[1]

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[1]We ran into a problem when we attempted to apply the matrix form of Hausman's test. The matrix form of the test is based on a direct comparison of b_{RE} and b_{FE}. The test statistic is

$$m = (b_{FE} - b_{RE})'(\Omega(b_{FE}) - \Omega(b_{RE}))^{-1}(b_{FE} - b_{RE}) ,$$

where $\Omega(\cdot)$ are the coefficient covariance matrixes estimated in the transformed regressions. After correcting $\Omega(b_{FE})$ for the degrees of freedom implicitly used to estimate 451 district-specific effects, $\Omega(b_{FE})$ and $\Omega(b_{RE})$ are, in our case, very close together. In fact, the estimated covariance matrix $\Omega(b_{FE}) - \Omega(b_{RE})$ is not positive definite, so the test statistic m is meaningless.

As Hausman points out, this test implicitly rejects cross-sectional estimates as well. For if $E(w_i|X_i)$ is not equal to zero, then neither is $E(w_i + u_{it}|X_i)$, and so cross-sectional estimates are biased. In particular, the cross-sectional estimates reported in Park and Carroll (1979) must be rejected. (They showed PRICE elasticities of up to -.2, much larger than those estimated with pooled data, but still smaller than Feldstein's and Ladd's (cross-sectional) estimates.)

FIXED-EFFECTS ESTIMATES

The FE estimate used in the specification test assumed that the intertemporal covariance matrix F is known, with ρ equal to the value estimated in the RE model. Now that we have rejected the RE model, we must treat the intertemporal covariance matrix as unknown. Kiefer (1980, p. 198) describes a consistent FE estimator when the intertemporal covariance matrix is unknown:

$$b_{FE} = (X'M(I_N \times S^-)MX)^{-1} X'M(I_N \times S^-) My ,$$

where

$$S^- = \begin{bmatrix} S_*^{-1} & 0 \\ 0 & 0 \end{bmatrix}$$

with S_* obtained from S by deleting the first row and column, where

$$S = N^{-1} \sum_{i=1}^{N} M_1(y_i - X_i b_{LSDV})(y_i - X_i b_{LSDV})' M_1 ,$$

with

$$M_1 = I_T - T^{-1} 1_T 1_T' .$$

We use this estimator for the remaining FE estimates in Table 4, columns (2) through (5). The column (2) estimate is for the same specification as column (1); treating the intertemporal covariance matrix as unknown has very little effect on the estimates.

Exogenous PRICE and STABLK

We estimated two equations with exogenous instruments for PRICE and STABLK to see whether our results are seriously affected by their endogeneity. The first is based on a two-stage procedure. In the first stage, we estimated equations to predict tax rate OM as a function of exogenous variables during GTB years. The predicted value of OM implies predicted values for PRICE and STABLK. We used these predicted values instead of actual values to estimate fixed-effects Eq. (3) in Table 4.

In the second equation, we used values of PRICE and STABLK that are exogenously specified for each district by the GTB plan, namely, the minimum value of PRICE that the district could ever get (with a tax rate below the break in the PRICE schedule) and the maximum value of STBLK that it could get (with a tax rate above the break). Both of these values depend only on the GTB plan and the district's assessed value per pupil AV. These results are in Table 4, fixed-effects Eq. (4).

Neither exogenous treatment of PRICE and STBLK disturbs the conclusion that the effects of these two variables are too small to be useful for policy purposes.

Excluding Districts with Indeterminate PRICE

The final estimates check the effect of our treatment of districts whose demand curve passes through the gap in the PRICE schedule (D_2 districts in Fig. 1). We have assigned them their downward-marginal PRICE in all regressions so far (that is, we have treated them like D_1 districts). There are 79 districts with indeterminant PRICE in one or more GTB years. We simply omit them from the regression to obtain estimates of fixed-effects Eq. (5). Again, our conclusion about the small effect of PRICE and STBLK is not affected by omitting these districts.

DISCUSSION

Feldstein's and Ladd's estimates for Massachusetts suggest that state matching and block grants substantially affect educational expenditures. The Michigan data, however, provide no support for their conclusions. The differences between our results and theirs may partially reflect the econometric techniques we employed. Our cross-sectional regressions reported in (1979) are the closest we can come to replicating Feldstein's and Ladd's results.[1] But our cross-sectional estimates are emphatically rejected by Hausman's specification test. Perhaps Feldstein's and Ladd's results, if similarly tested, would be rejected as well.

It may be that Michigan school district decisionmakers or voters, or both, simply do not place sufficient value on educational expenditures. Even if school quality enters their preferences, they may base allocative decisions on some measure of quality that is poorly correlated (at least in their minds) with educational expenditures. An immense volume of research has so far failed to establish a relationship between school spending and student achievement. (See, for example, Averch et al. (1974).) Michigan's school districts may not have tried--or may not have succeeded when they did try--to convince the voters to the contrary. The "price" inherent in the GTB plan would then be irrelevant to their decisions.

Alternatively, school districts or voters may equate the "cost" of education with the local property tax *rate*. As Feldstein notes, the education community in general and the school finance community in particular have confused the school tax rate with the "price" of education. The GTB plan provides every district (below the matching ceiling) with identical marginal tradeoffs between local tax

[1]There we estimate an expenditure elasticity with respect to state block grants of .36, very close to Ladd's estimate and reasonably close to Feldstein's. Estimated price elasticities, however, are no greater than -.2, less than one-fifth of Feldstein's comparable result and roughly one-third of Ladd's.

rates and expenditures. Those may not be the relevant tradeoffs, but they may be what actually motivate budgetary decisions in most districts. There would then be no "price" response, because there would be no perceived "price" differences among districts.

Another possibility is that the lack of mechanisms for shifting local tax revenues among public agencies may dampen price response. The GTB plan may make education a "better buy" in one district than in another, relative to private and other public goods and services. In principle, other public agencies coextensive with the former district (e.g., cities) should reduce their property tax rates while the district increases its rate, the net result being a reallocation of funds from private and other public uses to the school. But what if the other public agencies do not reduce their local property tax rates? Given "too much" spending in the noneducational public sector, the public will opt for "too little" educational spending, at least in the short run.

Finally, it should be noted that our data pertain to the first three years of Michigan's GTB plan. The Massachusetts data examined by Feldstein and by Ladd were for the fifth year of that state's plan. It is possible, of course, that Michigan districts will exhibit greater price responsiveness in the future.

In sum, the Michigan data provide no evidence that state matching or block grants do much to stimulate school district expenditures. For whatever reason, school districts in Michigan have not (as yet) responded appreciably to changes in the implicit price of school expenditures.

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