Preliminary
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USING MARKET VALUATION TO ASSESS THE IMPORTANCE AND EFFICIENCY OF PUBLIC SCHOOL SPENDING

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Abstract

In this paper we take a "market-based" approach to examine whether increased expenditures improve perceived school quality and whether the current level of public school provision is inefficient. We find evidence that, on average, school districts are not wasting taxpayers' education dollars. Rather, if anything, we find that education may be underfunded. As a result, increased competition has the potential to increase school spending levels. We also find evidence that school districts spend less efficiently in areas in which school districts face less competition from other public schools and in areas in which residents are less educated (leading either to less mobility from a lack of resources or to less efficient education production through peer effects).

I. Introduction

An enduring question in education policy is whether spending additional resources on schools improves student outcomes. Some researchers point to evidence that schooling inputs, such as lower pupil-teacher ratios, longer school terms, and more qualified teachers, improve test scores and wages (see Card and Krueger (1992), Angrist and Lavy (1999), Achilles and Finn (1990), and Krueger (1999)). Others point to research that suggests that improvements in school inputs do not result in higher student achievement (see Betts (1995), Hanushek (1986), Hanushek, Rivkin, and Taylor (1996)). Many of these researchers also argue that teachers unions and bloated bureaucracies inhibit improved schooling inputs from generating better outputs resulting in inefficiency in public school provision (Chubb and Moe, 1990; Hoxby, 1996). The typical approach in the existing literature is to estimate directly the relationship between schooling inputs and student outputs, such as test scores or eventual wages. However, the importance of test scores to later adult outcomes is unclear, and it is rare to be able to link schooling inputs to later outcomes (such as eventual educational attainment and/or wages) because of the cost and difficulty in implementing long longitudinal surveys and the unreliability of asking retrospective information on schooling inputs in surveys of labor market outcomes.

An alternative approach to analyzing whether additional expenditures result in better student outcomes is to estimate whether the market appears to value the spending by examining the relationship between school spending and property values. In the United States, the provision of elementary and secondary education is largely determined at the local level.[1] Therefore, if individuals make residential choices based on their preferences for schooling, then property values should reflect how schooling in a particular area is valued by potential residents.[2] Loosely speaking, if an additional dollar spent by a district

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[1] In the U.S. only 7 percent of public primary and secondary school revenues are provided by the federal government, while states and local governments contribute approximately 45 percent each (*Digest of Education Statistics*, 1996). The balance is funded by private sources.

[2] For evidence that households with children choose where to live based in part on school quality, see Barrow (1999) and Black (1999).

improves school outcomes, then one should find that additional expenditures lead to higher property values. In contrast, if the additional spending does not result in better schools, then one should not find that additional school spending increases property values.

An advantage of this approach is that it permits an assessment of the value of school spending using a more contemporaneous measure of (at least the perception of) schooling outputs. In addition, one can assess whether schools receiving the additional revenue are inefficient in their provision of education by testing whether a one dollar increase in expenditures generates a (properly discounted) one dollar increase in housing values. On the one hand, as established by Tiebout (1956), if individuals choose their residence based on the provision of public goods, then the optimal level of such public goods should be provided.[3] Therefore, by this argument, the provision of schooling in the United States may well be efficient because schools compete with one another through the residential housing market. This form of competition insures both allocative and productive efficiency as parents who would like a different kind or bundle of schooling can choose a different neighborhood, as can those who do not believe that the quality of schooling merits their tax dollars (i.e., the district is inefficient).

On the other hand, there are several obstacles that may prevent the majority of school districts from achieving the optimal level of schooling. First, because it is costly to move, parents may not be perfectly mobile or have perfect information about localities and the provision of education. In particular, low-income families may not be able to choose to move out of the inner-city in order to reside in a district providing their preferred level of education. In addition, a Tiebout equilibrium requires that parents have a choice of different kinds of communities. Over the past 20 years many states have centralized education finance to improve the equity in the provision of schooling (Kenny and Schmidt, 1994). To the extent that these policies have led to more equal provision of schooling across districts within state, parents have less ability to choose

[3] Tiebout (1956) relies on several assumptions including full mobility, a large number of communities, unrestricted employment opportunities, and an optimal community size.

their preferred provision of schooling today than they did 20 years ago.

In this paper we adopt a "market-based" approach to examine both whether additional spending on schools appears to increase school outputs and whether public schools are inefficient. Because most of the debate surrounding school quality in the U.S. is implicitly about whether the current spending is too high,[4] we focus on testing whether the current level of public school provision is inefficiently high. We consider efficiency in the situation in which local school spending continues to be (primarily) financed by a property tax as is the case in the majority of school districts in the U.S. In this case, housing choices may be distorted by the property tax. Nevertheless, if families have the option of choosing to send their children to the school of their choice, communities may still achieve the second-best schooling optimum. Therefore, we test for whether the current level of school spending is efficient conditional on the inefficiency induced by the property tax.[5]

We find evidence that, on average, additional school spending leads to increased property values suggesting that additional expenditures do improve student outcomes. In addition, we find that, on average, public school districts likely spend increases in state aid for education efficiently. However, to the extent there is inefficiency, it is likely that school districts under-spend on education; that is, a $1 increase in state aid results in more than a $1 dollar increase (properly discounted) in local housing values. We also find

[4] For example, many who argue that the public provision of schooling in the U.S. is likely inefficient note that during the 1963-64 school year the government spent $2,609 per pupil (in 1995 dollars), compared to $6,459 per pupil spent during 1995-96, an increase of over 147 percent (*Digest of Education Statistics*, 1996); without comparable increases in student achievement. Further, many argue that despite the fact that the U.S. ranks fourth and third in spending per pupil for primary and secondary education, respectively, (*Education at a Glance: OECD Indicators, 1998*), it ranks much lower in standardized tests of mathematics and science knowledge, particularly at the secondary level (*Pursuing Excellence: A Study of U.S. Fourth-Grade Mathematics and Science Achievement in International Context, 1998; Pursuing Excellence: A Study of U.S. Twelfth-Grade Mathematics and Science Achievement in International Context, 1998*).

[5]Alternatively one could consider the case where under a full school choice program, either property taxes are not used to finance schooling or the school property tax rates are fully-equalized thereby severing the link between the local property taxes raised and the schooling provided. In this case, if families have the option of choosing to send their children to any school, their housing choices will not be distorted by the financing of that education. See, for example, Epple, Zelenitz, and Visscher (1978).

some evidence that efficiency varies with the likely mobility of a district's residents. We find that districts with a higher proportion of householders without a high school degree are less efficient likely resulting either from a lack of mobility (from a lack of resources) or lower quality "peers" within the schools. Our results also provide some evidence that districts in which there is more competition (as defined by a Herfindahl-Hirschman index) spend education funding more efficiently than those with less competition and that unionized school districts likely spend money more efficiently than non-unionized districts.

The rest of the paper is organized as follows: section II outlines the theoretical model upon which our estimation strategy is based, section III explains our empirical strategy, section IV describes our data, section V contains our results and section VI concludes.

II. A (Highly) Stylized Model of Public Good Valuation

A. Basic Model

Our "market based" approached for examining efficiency in public schooling is based on Brueckner's bid-rent model of property value determination (1979, 1982, 1983).[6] The result derived from this model is that efficient public good provision occurs at the level of public good provision that maximizes aggregate property value. We note that the model relies on some strong assumptions, and we address violations of these assumptions below.

The bid-rent model of the housing market is a short-run model in which the housing stock of a community is fixed. In the long-run one might expect that the stock of available housing might change in response to local public good provision; however, in the short-run it is reasonable to expect that the housing stock remains relatively fixed. Consumers are assumed to have identical preferences, and their utility depends on the level of schooling provided, *E*; other local public goods, *G*; housing units, *H*; and the

[6] Other attempts to test for efficiency in the public provision of schooling include Barlow (1970) and Bergstrom et al. (1988).

numeraire good, *B*. All residents in a community consume the same level of the public goods, *E* and *G*.[7] Costless mobility is assumed such that consumers with the same level of income must achieve the same utility level.[8] As a result, house prices (rents) adjust to insure that residents are indifferent between different houses.[9] Formally this means that a resident with income *I* achieves utility *h(I)* and her consumption bundle must satisfy,

$$U(E,G,H,B)=h(I). \tag{1}$$

This equality is guaranteed since if a consumer could achieve a higher utility elsewhere, she would move. As a result, such disparities are arbitraged away. A resident's budget constraint can be written as $B+R=I$, where *R* represents the rent paid for housing and the price of *B* is normalized to 1. Then, *R* must satisfy

$$U(E,G,H,I-R)=h(I). \tag{2}$$

This equation determines the bid-rent function,

$$R=R(E,G,H;I). \tag{3}$$

This function specifies the rent necessary to equalize an individual's utility across differing residences. Differentiating equation (3) with respect to *E* gives

$$R_E(E,G,H;I)=\frac{U_E(E,G,H,I-R)}{U_B(E,G,H,I-R)} \tag{4}$$

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[7] We assume there are no externalities across communities. Boskin (1972) argues that due to such externalities, mobility may not lead to the optimal provision of public goods if local governments compete through their tax and expenditure policies. In particular, public goods that redistribute among constituents may be under-provided and those that increase the value of property over-provided. Although mobility will lead to allocative inefficiencies, it will continue to induce productive efficiency.

[8] We could also allow for heterogenous preferences in which case residents with the same preferences and income would have to achieve the same level of utility.

[9] One can think of "rent" as either the price of owning a house or of renting because in equilibrium an individual should be indifferent between owning and renting a property.

where subscripts denote partial derivatives. Equation (4) shows that the required change in the rent resulting from a change in *E* depends on the marginal rate of substitution between education and the numeraire good, *B*. Similarly, the required change in the rent resulting from a change in *G* depends on the marginal rate of substitution between the other public goods and *B*.

Next, assume that local revenues for schooling are financed exclusively by a residential property tax rate, t_E, and the other public goods by a residential property tax, t_G. The property tax rates are applied to both land and improvements to ensure that the choice of housing-factor inputs is not distorted. Letting δ be the discount rate, the value of an individual house *i* is,

$$P_i = \frac{R - \left(t_E + t_G\right) \cdot P_i}{\delta} \tag{5}$$

which rearranged gives,

$$P_i = \frac{R\left(E, G, H_i; I_i\right)}{\delta + t_E + t_G}. \tag{6}$$

The aggregate value of housing property is defined as the sum of the individual property values within a community,

$$P = \sum_{i=1}^{N} P_i = \sum_{i=1}^{N} \frac{R\left(E, G, H_i; I_i\right)}{\delta + t_E + t_G}, \tag{7}$$

where *N* is the number of houses in the community.

Assuming that the state contributes an amount, *S*, to local school districts (the local community fully finances the other public goods, *G*, for simplicity), the community budget constraint is,

$$\left(t_E + t_G\right) \cdot P = CE\left(E, N\right) - S + CG\left(G, N\right), \tag{8}$$

where *CE* and *CG* are the (convex) cost functions for *E* and *G*. The fact that cost is a function of the community size, N, reflects potential congestion costs.[10] Combining equations (7) and (8) gives,

$$P = \frac{1}{\delta}\left[\sum_{i=1}^{N} R\left(E, G, H_i; I_i\right) - CE\left(E, N\right) + S - CG\left(G, N\right)\right]. \tag{9}$$

Aggregate property values are a function of the aggregate rents, state aid, the discount rate, and the production costs of education and the other public goods.[11] Differentiating equation (9) with respect to the state aid and assuming that changes in state money for education have no effect on the provision of other public goods, *i.e.* $\partial G/\partial S = 0$, yields

$$\frac{\partial P}{\partial S} = \frac{1}{\delta}\left[\sum_{i=1}^{N} R_E\left(E, G, H_i; I_i\right) - CE_E\left(E, N\right)\right]\frac{\partial E}{\partial S} + \frac{1}{\delta} \tag{10}$$

where, as shown in equation (4), R_E is equal to the marginal rate of substitution between education and the numeraire good, *B*. As a result, equation (10) establishes that $\partial P/\partial S = 1/\delta$ when the "Samuelson condition" for the optimal provision of education is satisfied, i.e., the sum of the marginal rates of substitution between education and the numeraire equals the marginal cost of providing education. (A similar condition holds for all other public goods as well.) Assuming that *R* is a strictly concave function of *E* and *G* and that *CE* is convex in *E* and *G*, it follows that *P* in equation (9) is strictly concave in *E* and *G*. As a result, aggregate property value reaches a global maximum at values of *E* and *G* where the Samuelson condition holds, ceteris paribus. Thus, one can determine whether education is under-provided or over-provided relative to the property value maximizing level as $\partial P/\partial S \gtreqless 1/\delta$. Because the null hypothesis of the coefficient of interest, that on state school aid, depends on assumptions about the discount rate, we use 7.33 percent which is the

[10] Note that for simplicity in notation we have allowed the community size in terms of congestion costs to equal the number of housing units. In fact, one might think total population or number of school-aged children, in the case of education, is a more appropriate measure for potential congestion.

[11] We discuss adding business property to the model below.

average real 30-year conventional mortgage rate from 1980-1990.[12]

B. Discussion of Assumptions and Omissions

The result above derives from a number of rather strong assumptions and omissions that may not hold in reality. For example, the model assumes that households can move without cost and that the provision of public goods other than education are unaffected by changes in state funds for public schooling. In addition, the theoretical model omits the role of commercial property. In this section we discuss the likely effects of each of these assumptions on interpretations of the results that follow.

1. Costless Mobility

Costless mobility insures that equation (1) holds, that is, households with the same income achieve the same utility level because if a household could achieve a higher utility elsewhere, it would move. We have attempted to incorporate costs to moving into the model in two ways. First, we considered the case of a household facing the following consideration: remain in community 1 or move to community 2. If there is only one period, then the individual on the margin of moving will be indifferent between moving and staying. We represent this by amending equation (2) as,

$$U\left(E_1, G_1, H_1, I-R_1\right) = U\left(E_2, G_2, H_2, I-R_2-k\right) = h(I). \tag{2'}$$

where E_j, G_j, H_j, and R_j represent the amenities and rent in community j (where j=1,2) and k represents the fixed cost of moving. In this case, equation (9) still holds (i.e., one would test for efficiency by testing whether $\partial P/\partial S = 1/\delta$) because the moving costs do not change the marginal cost and benefit of education.

More generally, however, one may be concerned that households in poor neighborhoods have fewer

[12] The nominal mortgage rate is from the Federal Home Mortgage Corporation (obtained from the Federal Reserve Board of Governors – http://www.bog.frb.fed.us/releases/h15/data/a/cm.txt). We use the personal consumption expenditure price index to calculate the real mortgage rate.

options (which could be thought of as extremely high mobility costs). In this case, as in all districts, as long as there are some households moving into and out of the district (i.e., there is some market determining prices), then the market price for those houses should reflect the valuation that potential homeowners put on the amenities in the district. And, in districts in which households have less mobility (such as low-income districts), we might expect that education (and other public goods) is not provided efficiently.

2. $\partial G/\partial S = 0$

A second assumption is that state spending on education does not affect the provision of other public goods. From the standpoint of basic economic theory this assumption seems unrealistic since funding is fungible and if a district receives aid from the state, the equivalent to an increase in income, then the share devoted to education should be equal to the marginal propensity to spend out of income (which would be about 5-10 cents on the dollar) (Hines and Thaler, 1995). If we modify equation (10) to relax the assumption that $\partial G/\partial S = 0$, we have,

$$\frac{\partial P}{\partial S} = \frac{1}{\delta}\left[\sum_{i=1}^{N} R_E\left(E,G,H_i;I_i\right) - CE_E\left(E,N\right)\right]\frac{\partial E}{\partial S} + \frac{1}{\delta}\left[\sum_{i=1}^{N} R_G\left(E,G,H_i;I_i\right) - CE_G\left(E,N\right)\right]\frac{\partial G}{\partial S} + \frac{1}{\delta} \quad (10')$$

where R_G is equal to the marginal rate of substitution between other public goods and the numeraire good, *B*. In this case, if $\partial G/\partial S \neq 0$, $\partial P/\partial S$ may not equal $1/\delta$ even if education is provided efficiently because the other public goods are not provided efficiently. Of course, if one is willing to assume that other public goods are provided efficiently, then equation (10′) becomes equation (10) again because the Samuelson condition is met for the other public goods so that the second piece of equation (10′) equals zero even when $\partial G/\partial S \neq 0$.

For three reasons we believe that we can continue to learn something from our exercise even if $\partial G/\partial S \neq 0$. First, other public goods may well be efficiently provided, as noted above, in which case equation (10′)

becomes equation (10) again. Second, the empirical literature attempting to measure the effect of an increase in grants to states and local governments tends to find that spending increases by much more than 5-10 cents on the dollar, an anomaly that has been dubbed the "fly-paper" effect, because government funding tends to "stick where it hits." (Hines and Thaler, 1995; Rubinfeld, 1987). Thus, although in theory we might expect $\partial G/\partial S \neq 0$, in practice the assumption that it is zero (or very close to zero) may be relatively innocuous.

Third, the ambiguity in interpretation in our exercise arises if according to our test, districts appear to be spending efficiently but in reality are not. This could only arise if the first term (representing the marginal rate of substitution between education and *B* net of marginal cost) is completely offset by the second term (representing the marginal rate of substitution between the other public good and *B* net of marginal cost). As a theoretical possibility, this could only occur if, on average, districts over-provide education and under-provide other public goods (or vice versa). While we have been unable to find any empirical evidence on this issue, it seems much more likely that if districts over-provide one public good they likely over-provide the others as well. In this case, our estimated coefficients will be upward or downward biased such that we will reject efficiency although the size of the inefficiency will be exaggerated. A second possibility is that education is provided efficiently (such that the first term does equal zero), but that other public goods are not provided efficiently. (Note that even if $\partial G/\partial S$ is small, a large inefficiency in the provision of other public goods may cause us to find evidence of inefficiency in education where there is none.) Again, while this is possible, our review of the literature suggests that there is little evidence of systematic over-provision of any public goods (See Brueckner, 1979; Deller, 1990; and Taylor, 1995).

3. Business Property

The model specified above omits business property from consideration. Consider the case in which we assume that the value of business property only enters the model through the budget constraint and both residential and business property are taxed. Again assuming $\partial G/\partial S = 0$, equation (10) becomes:

$$\frac{\partial P}{\partial S} = \frac{1}{\delta}\left[\sum_{i=1}^{N} R_E\left(E,G,H_i;I_i\right) - \alpha C E_E\left(E,N\right)\right]\frac{\partial E}{\partial S} + \frac{\alpha}{\delta} \qquad (10'')$$

where α is the residential share of total property value.[13] Thus, at the efficient level of education provision, the first part of equation (10″) no longer equals zero: in fact, it is now positive. This positive effect on $\partial P/\partial S$ is partially offset by the effect of α on the second term, but the net effect is indeterminate. What we can say is that this problem is minimized in heavily residential areas where α is close to one. In addition, it is still clear that $\partial P/\partial S < 0$ implies that too much is being spent on public education.

Empirically, we do not include business property in our estimation because data are not available for most states. However, when we include it (along with other types of property, such as personal property) for the states for which we have the data, it has not changed our empirical results substantively. As a result, we conclude that our treatment of commercial property is unlikely to have a large effect on our conclusions about education spending.

III. Empirical Framework

Based on the theoretical framework, we estimate the following reduced-form equation,

$$P_{jst} = \alpha_0 + X_{jst}\alpha_1 + H_{jst}\alpha_2 + W_{cst}\alpha_3 + \beta S_{jst} + \xi_j + \varepsilon_{jst} \qquad (11)$$

where P_{jst} is the natural logarithm of the aggregate house value per pupil in school district *j*, in state *s*, and year *t*, X_{jst} are a set of demographic characteristics about the district's population, H_{jst} are characteristics of the housing stock of the district, W_{cst} are county characteristics, and S_{jst} is the logarithm of state revenue for education spending. ξ_j is a district fixed-effect, and ε_{jst} is a normally distributed random error term. The estimate of β represents the efficiency of state expenditures on education.

[13] In 1986, approximately 61 percent of gross assessed valuation was due to residential (nonfarm) property (*1987 Census of Governments*).

By far the most difficult empirical challenge to overcome is to control for all characteristics that are correlated with state schooling revenue and housing values as omitted factors may bias the results.[14] In the 1970's many states relied on categorical aid to fund education. During this time, state revenue was primarily determined by a flat grant per pupil or by a flat grant in which the pupil count was weighted by the characteristics of the students in each school district (such as grade level, special education needs, and transportation). Between the 1970's and the 1990's many states changed their formulas in order to equalize education funding across rich and poor districts. Many of the formulas now incorporate the wealth of the district (the total assessed valuation per pupil), such that property-rich districts receive less state aid while property poor districts receive more.[15] As a result, while the relationship between district state funding per pupil and district assessed valuation per pupil was relatively flat in 1980, the relationship has gotten more negatively sloped in 1990 for states that have made "equalizing" changes to their state financing formulas.[16]

Figures 1a-1k depict predicted basic state aid per pupil in 1980 and 1990 versus aggregate house value per pupil in 1980 by state.[17] Each graph also includes regression fitted values from regressing predicted basic state aid per pupil in each year on aggregate house value per pupil in 1980. Colorado and Connecticut, in Figures 1a and 1b, are good examples of states in which state school aid was made more

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[14] See Rubinfeld (1987) for a discussion of the empirical challenges of testing the Tiebout model.

[15] This is a very broad generalization. See Card and Payne (1997), Evans, Murray, and Schwab (1997), Hoxby (1998), and Murray, Evans, and Schwab (1998) for more details on state financing plans.

[16] We have attempted to model which states change their financing formulas and which states adopt more or less equalizing formulas. We did so by aggregating our data to the state level and estimating binary and multinominal logit models. While we found some evidence that the average state household income and/or property values (either the levels or distribution) may partially determine state behavior, the evidence was neither overwhelming nor systematic. A more in-depth political economy approach would be quite useful for this literature.

[17] In these figures, both 1980 and 1990 state aid are predicted from the state school financing formulas using the characteristics of the districts in 1980.

equalizing.[18] In 1980, the slope of the regression fitted line is relatively flat while in 1990 the slope of the regression fitted line is more negative indicating that property poor districts are getting more aid per pupil than wealthier districts. While Indiana (Figure 1c) adopted a more generous school financing formula in 1990 as evidenced by the upward shift in the regression fitted line in 1990, the state did not adopt a formula that increased the degree of equalization between 1980 and 1990. This is evidenced by the slope of the regression fitted lines remaining relatively similar. Indiana's school financing formulas generate some equalization, however, as can be seen by the negative relationship between predicted state aid per pupil and aggregate house value per pupil. (Also see Card and Payne (1997).)

Because moving toward greater equalization is likely to result in districts with declining property values receiving greater increases in state aid per pupil, we expect that the coefficient estimate on state aid will be negatively biased. We attempt to address these potential problems in three ways. First, we control for a variety of district and county characteristics that may be correlated with education provision and district property values. For example, we include demographic characteristics of the district as well as the county crime rates. These measures vary over time as well. Second, we focus on estimates that contain school district fixed-effects (which we implement through first-differenced equations). This allows us to parcel out any time-invariant features of districts that may be correlated with state education revenues and house values (such as distance to employment centers, climate, and relatively stable characteristics about the student and local populations).

Third, we instrument for changes in state education revenue with changes in the state school financing formulas. We do so by first calculating the amount of aid that each district should have received

[18] Some states additionally use measures of income to calculate local school district wealth. Massachusetts appears to have a positive relationship between predicted state aid per pupil and school district wealth when wealth is measured as aggregate house value per pupil. (See Figure 1e.) If predicted state aid is plotted instead versus average household income in the district, the relationship between aid and wealth is negative.

based on the state's financing formulas in effect in 1978-79 and 1990-91.[19] We calculate both the "basic" state aid and the "total" state aid which includes other components such as aid for special and vocational education for each year. Because many states have a "variable grant" portion of their state financing formulas in which each district's state aid depends in part on its actual spending,[20] we instrument for state revenue received using predicted state revenue based on 1980 district characteristics so that the change will not depend on district behavior.[21] We refer to this instrument as the "predicted state aid." The disadvantage of the predicted state aid instrumental variables strategy is that it relies on the assumption that the change in district housing values and state aid do not depend on their values in 1980. To the extent that changes from 1980 to 1990 are correlated with the 1980 values, the instrumental variables results may be inconsistent. To assess these results, we also construct instrumental variables by calculating average state aid to districts in each year using all school districts in our data except for those in the state of interest. We refer to these instrumental variables as "synthetically predicted state aid." Because nearly identical data are used to predict state aid, the changes in actual state aid are identified by changes in the state financing formulas. We aggregate the predictions to the state level and use changes in the mean predicted state aid as the instrument. The advantage of this strategy is that it more clearly isolates the changes in state financing formulas as the identifying variation; the disadvantage is that the instrument only varies at the state level reducing the power

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[19] We predict the amount of state aid that each district receives according to the formulas as described in the *Public School Finance Programs* series from 1978 and 1990. In some states we also supplemented these descriptions of the formulas with additional information from the states or from the state statute when necessary. We used total assessed valuations by district nationwide in 1980 and 1990 (which we collected) to determine the amount of state aid.

[20] According to Card and Payne (1997), approximately 40 percent of states had a variable grant component either in 1975-76 or in 1990-91.

[21] See Hoxby (1998) for a discussion of the incentives built into the formulas for districts to change their behavior regarding educational expenditures.

of the exercise.[22]

We also note that our strategy is to include an array of district and county characteristics to proxy for the district's underlying cost function for education, rather than control directly for education costs. We do so for two reasons. First, education costs (as opposed to expenditures) are inherently more difficult to observe, and second, if we use education expenditure as a proxy for local education cost, we do not have a strategy for addressing the endogeneity of local education expenditure in addition to state aid. Many state school financing formulas calculate a total amount of school financing "need" for each district based on pupil counts in the district. One common feature of this portion of the formulas is to assign different weights to different types of pupils in generating a total pupil count. More specifically, the formulas give more weight to pupils that are more costly to educate, such as students with special education needs. As a consequence, this feature of the formulas will induce some positive correlation between local education costs and state aid. Our IV estimates may be downward biased to the extent to which we have not properly proxied for local education costs.

Finally, we estimate equation (10) using a log-log specification although equation (9) is written in levels.[23] We do so for two reasons. First, the parameter estimates using the log-log specification are much

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[22] We present standard errors for the IV estimates using the synthetically predicted state aid that account for the fact that these instruments are grouped to the state level (Moulton, 1986).

[23] Because we use log values of basic and total school financing aid in our estimation, we drop 856 districts with zero or negative aid in these categories. States with financing formulas that do not provide an aid floor with a strictly positive minimum and states in which the finance formulas include a provision for recapture of funds from school districts may have districts receiving zero or negative state school aid. While a district may not qualify to receive state school finance aid (or the maximum state school aid) for failing to meet various requirements such as a minimum tax effort, the districts receiving negative or zero aid are most likely to fail to qualify for any state aid because they are too wealthy. A typical example would be a school district for which the minimum required tax rate for participating in the state financing program would raise money in excess of 100 percent of what the state determines is the district's total financial need. We have investigated the effect of excluding these wealthy districts on our results by estimating the equation in levels. The results (in levels) are similar with and without these districts (see Appendix Table I).

more robust to changes in the sample than are the estimates using levels.[24] Second, the log-log specification generates estimates of proportionate changes, rather than absolute changes, and therefore accounts for scale. In order to translate the null hypotheses for an efficiently spending district (derived above) into logarithms, we note that with a log-log specification, the coefficient $\beta = (\partial P/\partial S)(\bar{s}/\bar{p})$ – the elasticity of housing values with respect to state education aid. Therefore, we simply multiply the expected coefficient in levels by the inverse of the ratio of the means of the variables of interest. Assuming a discount rate of 7.33% the null hypothesis is that $\beta = 0.278$.

IV. Data

For most of our empirical analysis we use data from the 1980 and 1990 decennial census school district data files, the 1977 and 1987 *Census of Governments*, and the *USA Counties 1996* CD-ROM. In order to generate our instrumental variables, we have also merged these data with data we collected on the total assessed valuation (adjusted to market value both by the statutory assessment ratio and assessment-to-sales ratios where possible) by school district from 1980 and 1990.[25] The unit of observation is a school district.

We limit the sample to elementary, secondary, and unified school districts that did not change between 1980 and 1990 (that is, they did not merge or split apart). We exclude districts in California because we could not obtain property value data with which to model the school financing formulas and Hawaii because it has a state level school district and does not have an explicit school financing formula to use in

[24] For example, the point estimates using a levels specification are sensitive to whether or not New Jersey and Connecticut (two wealthy states with high state spending on education) are included in the sample, whereas the estimates using the log-log specification are not.

[25] Note, however, that the dependent variable of aggregate residential house values per pupil in our regressions is from the 1980 and 1990 decennial censuses.

constructing our instrument.[26] We also exclude school districts with zero enrollment in either 1980 or 1990, and those for whom we are missing data on our instruments (about 3162 observations), actual state aid to districts (about 154 observations), aggregate property values (about 257 observations), and are not identified in the census data as an elementary, unified, or high school district (about 6 observations). The final analysis sample includes 11928 observations.

Means of selected school district characteristics in 1980 and 1990 are presented in Table 1. On average, housing values increased by approximately 20 percent between 1980 and 1990.[27] State aid per pupil also increased from 1980 to 1990, by 49 percent; however, this increase came not only from increases in total state aid but also from falling enrollment over the time period.

V. Empirical Analysis

A. Ordinary Least Squares Estimation (OLS)

In Table 2 we present ordinary least squares (OLS) estimates of the relationship between the change in logarithm of property value per pupil and the change in logarithm of state aid for education per pupil. The results from a simple bivariate regression are presented in column (1); the remaining two columns sequentially add district and then county characteristics. We estimate an extremely small positive, but statistically insignificant, relationship between housing values and state aid per pupil in column (1).

The estimates in column (2) include the first-differences for a quadratic in average household income, percentage of the population with at least 16 years of education, percentage of the population that is unemployed, percentage of housing units that are owner occupied, the percentage of housing units that are vacant, the percentage of occupied housing units that were built more than 10 years ago, the percentage of

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[26] The District of Columbia is not included because it is not a state and therefore does not have state school financing.

[27] Aggregate house value for a school district excludes the value of rental housing property.

the population that moved into their house less than 10 years ago, the percentage of the population over 55 years of age, the percentage of children enrolled in private schools, the log of the total housing units in the district, and the log of the public school district enrollment. In column (3) we also add the FBI's serious crime index, the percentage of voters that voted for the republican candidate in the most recent presidential election, the percentage of voters that voted for the democratic candidate in the most recent presidential election, the percentage of the county employees that are union members, and the percentage of workers employed in manufacturing.[28] We expect that the coefficient estimates on changes in state aid per pupil will increase when the additional covariates are included.

As expected, in column (2) we estimate that a 1 percent increase in state aid will increase property values by 0.041 percent, an effect that is statistically significant. The effect decreases to 0.036 percent in column (3) when we add the county variables. Note, however, that these estimates are significantly different from 0.278, the value we would expect if districts were spending efficiently. As a result, based on the OLS results, one would conclude that although increased expenditures appear to increase school quality, school districts are inefficiently spending state school financing dollars since a $1 increase in their state education aid does not generate a (properly discounted) $1 increase in housing values.

B. Instrumental Variables (IV) Estimation

1. Overall Efficiency

Because it is likely that the OLS coefficient estimates on state aid per pupil are negatively biased because most states have moved to "more equalizing" state financing formulas, we instrument for changes in state aid with predicted changes in state aid using an instrument that holds the school districts'

[28] We also include dummy variables indicating whether there are missing values for the percentage of households that moved into their house less than 10 years ago, the percentage of children enrolled in private schools, the FBI crime index, the percentage of county workers who are organized, and the percentage of employment in manufacturing.

characteristics constant at their 1980 levels ("predicted state aid") and using an instrument constructed with a synthetic sample of school districts outside of the state ("synthetically predicted state aid"). The first-stage estimates of the relationship between observed changes in state aid per pupil and the instruments are presented in Table 3. The top panel shows both types of instruments when we predict state "basic" aid, while the bottom panel shows both instruments when we predict "total" state aid. The "predicted state aid" instruments in column (1) are correlated with observed changes in state aid per pupil. A one percent increase in predicted aid is associated with approximately a 0.05 percent increase in actual state aid, controlling for district and county characteristics. Using the synthetically produced instruments instead, the magnitudes of the coefficients are larger although the t-ratios are slightly lower. The p-value on the synthetically predicted state basic aid (the top panel in cell (2)) is 0.11 and that on the total aid (in cell (4)) is 0.12. In general, the synthetically generated instrument estimates are less precise because of the aggregation to the state level.

The IV estimates are presented in Table 4. The table structure is similar to Table 3: the estimates in column (1) use the predicted state aid and those in column (2) use the synthetically predicted state aid (the top panel uses the basic aid; the lower panel uses total aid). We continue to control for the district and county characteristics described in the text and shown in column (3) of Table 2. For all estimates, we report the p-values for the test of the null hypothesis that districts are spending education money efficiently. Specifically, we test that the coefficient on state aid equals 0.278 (corresponding to a discount rate of 0.0733).

The magnitudes of the IV estimates are remarkably similar across the two types of instruments and the two calculations of state aid. A one percent increase in state aid increases aggregate housing values by about 0.4 percent. Using the predicted state aid (column (1)) we conclude that states are likely under-spending. We do not reject the null hypothesis that the districts are spending efficiently using the synthetically predicted instruments in column (2) because of their relatively large standard errors. Because the two types of instruments generate similar coefficient estimates and yet the synthetically generated instruments have the disadvantage of only varying at the state level, we continue the analysis using only the predicted state aid instruments.

Based on the results presented in Table 4, it appears that additional state spending on schooling increases school quality, as reflected in property values. In addition it appears that, on average, school districts under-spend. That is, school districts could increase total property values by increasing spending on public education because they are at the point where the marginal benefit of the additional dollar spent exceeds its cost.[29] This interpretation, however, depends heavily on the assumed discount rate. Another way to evaluate the precision of the finding is to consider the range of discount rates over which one would reject the null hypothesis that school districts spend inefficiently by over-spending. Given the point estimates in Table 4 of about 0.4, if the true discount rate is lower than 4 percent, school districts over-spend and if the true discount rate is more than 4 percent, school districts under-spend. Thus, the finding that school districts do not spend inefficiently by over-spending would hold for a fairly wide range of discount rates (those greater than 4%).

A natural question is whether increases in state aid truly translate into increases in education expenditures at the district level and therefore into changes in school inputs and outputs. To provide some evidence on how the additional state aid is spent, we study the relationship between state aid and district total expenditures on education, the district pupil-teacher ratios (in imperfect measure of class size), and the district's high school dropout rate (measured as the percentage of individuals aged 16-19 who are not enrolled in school and do not have a high school diploma). Ideally, we would use a measure of high school dropout rates that was lagged the changes in state aid by several years (to account for the fact that changes in student outcomes are both cumulative). However, we only have a contemporaneous measure.

We estimate instrumental variables models identical to those in Table 4 except for the dependent

[29] We contrast a log-log specification with a levels specification in Appendix Table I. Using a discount rate of 7.33%, we do not reject the hypothesis that school spending is efficient, on average.

variable. The results are below, in Table 5.

Table 5
IV First-Differenced Estimates of the Effect of
Educational Expenditures on School Inputs and Outcomes

	Dependent Variable		
	Change in Log Total Expenditures per Pupil	Change in Log Pupil-Teacher Ratio	Change in Log Percent High School Dropouts
	(1)	(2)	(3)
Change Log in state aid per pupil	0.234 (0.058)	-0.496 (0.076)	-0.020 (0.209)
Number of Observations	11928	11652	10058

Notes: Standard errors are in parentheses. See text or column (3) of Table 2 for other covariates.[30] The equations are weighted by the log of student enrollment in 1980. The mean of the dependent variable in column (1) is 0.337; the mean of the dependent variable in column (2) is -0.220; and the mean of the dependent variable in column (3) is -0.223. The instrument is the predicted basic state aid.

The results suggest that a 1 percent increase in state aid increases district total expenditures by about 0.23 percent. This result translated into levels suggests that a $1 dollar increase in state aid increases education expenditures by 15 cents, a result that is consistent with a "fly-paper" effect (Hines and Thaler, 1995). We also find that a 1 percent increase in state aid reduces the district pupil-teacher ratio by about 0.5 percent, an effect that is statistically significant suggesting that districts do indeed attempt to improve school inputs with the additional funding. Further, we find some evidence that increases in state aid translate into better student outcomes, although the result is imprecisely estimated and not statistically significant.

2. Differential Efficiency

[30] We have estimated all three equation also controlling for the change in log aggregate property values per pupil as an independent variable in order to isolate more effectively the effect of the state spending formulas with nearly identical results.

The previous section tested for whether there is inefficiency in school spending, on average, in the U.S. This aggregate estimate, however, may mask important differences across the country. A natural implication of the theoretical framework is that because households with greater income have more schooling options (as they can afford to consider a wider range of school districts in which to reside and can more easily afford private schools), school spending in wealthier areas should be more efficient. Similarly, characteristics of school districts (e.g., the degree of competition or unionization) may affect the efficiency of school spending. Therefore, in this last section we consider whether the degree of inefficiency in the public schools is related to household and district characteristics.[31] We focus on the relative sizes of the estimates for different groupings, rather than the point estimates in isolation, because the main purpose of the exercise is to test further the notion of "residential mobility" and Tiebout sorting and to better understand the inner-workings of the estimation strategy and the components that underlie the overall estimate.

We begin by asking whether public schools are more efficient in wealthier districts where residents might plausibly have more "choice" because moving costs are not prohibitive or because private schooling is a more affordable option. To do so we divide districts (based on their characteristics in 1980) into those with an average household income less that $26,000 (approximately 20 percent of our (weighted) sample), and those with income greater than $54,000 (about 20 percent of the sample).[32] We also test for whether efficiency is greater in districts that have a sizable proportion of householders without a high school diploma. We define districts as low education districts if more than 53 percent of householders do not have a high

[31] In Tables 6a and 6b we restrict the effects of the other covariates to be the same across the districts and only interact state education revenues with the demographic or district characteristic in question. We also interact the instruments with these categories. We have attempted to relate efficiency to state characteristics (such as the degree to which school funding is centralized), however the results were quite noisy.

[32] Because the definitions for many of the categorizations in Tables 6a and 6b are inherently arbitrary, we adopted the rule of defining the categories based on the 20th and 80th percentiles (when weighted). One exception is the categorization of districts into "competitive" or "not competitive" using the county Herfindahl-Hirschman Index, as discussed below. The results on district average household income, education and level of competition are not particularly sensitive to these cut-off choices.

school diploma (about 20 percent of the sample). High education districts are districts in which fewer than 19 percent of householders do not have a high school diploma (also about 20 percent of the sample). The efficiency of schooling provision may differ by the education level of the district for two reasons. First, residents in predominantly lower education communities may have fewer schooling options because of a lack of income; the idea being that education may reflect permanent income better than average household income. Second, the presence of peer effects which make the production of schooling in highly educated communities more efficient than that in less educated communities (Bénabou, 1993).

In the upper-panel of Table 6a we estimate whether wealthier school districts are more efficient than poorer school districts. The results in both columns (1) and (2) suggest that most districts could increase property values by increasing spending on public schools and that the degree of inefficiency is worst among wealthier districts, although none of the coefficient estimates are statistically different from one another. The estimates reported in the bottom panel allow for variation in efficiency by the education level of the community. Here we find that districts in low education communities (those in which we think residents may be less mobile and have fewer schooling options and/or have poorer quality peers) seem to be less efficient than those in high education communities. Using the benchmarks discussed above, it appears that low education districts are over-spending on schools while high education districts seem to be spending about the optimal amount. The difference in the results by income and education may be explained by the fact that education provides a better proxy for the long-term socio-economic status of the residents than does income in a particular year.

In Table 6b we conduct a similar exercise for district characteristics. In the top panel of Table 6b we test for differential efficiency by the level of public school competition, as measured by the Herfindahl-Hirschman Index (HHI). Researchers argue that a HHI based on the concentration of enrollment in a geographic area reflects the market power of public schools in the area and therefore the degree of "choice" that parents may have (Borland and Howsen, 1992; Hoxby, 1994). Thus, we would expect that districts with less market power would be more efficient than those in less competitive areas. The HHI ranges from 0 to 1.[33] Districts in areas with only a few large school districts will have values close to 1 as the districts monopolize student enrollments; districts with lower values face more competitive pressure. We base our HHI on the concentration of public school enrollments in the county. The Federal Trade Commission (FTC) guidelines for horizontal mergers define markets with HHIs below 0.10 as unconcentrated, HHIs from 0.10 to 0.18 as moderately concentrated, and HHIs above 0.18 as highly concentrated. Using these guidelines, 69 percent of school districts are in highly concentrated markets. However, the FTC guidelines were not written for school districts, which must exist in all counties, and will therefore generate markets that are more concentrated than the typical product market. As a result, we use a more moderate definition of concentration and divide the districts into those that are somewhat competitive (HHI<0.14, approximately 26% of our sample), those that are monopolistic (HHI>0.46, approximately 25% of the sample), and those in between.[34]

The results in the top panel of Table 6b show that the coefficient on state aid is negative among districts that face little competition from other public schools suggesting that these districts over-spend. In addition, the estimates suggest that the spending practices of school districts in not-competitive counties are significantly different from the practices of those that face more competition. As a result, it appears that increased competition increases district efficiency.

In the middle panel of Table 6b we differentiate schools by the percentage of households in the district that live in urban areas. We define rural areas as those in which all of the households live in rural

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[33] The HHI is defined for each market as the sum of the squares of the market shares of all participants. In this case, we define market share as the proportion of county public school enrollment in each district and sum the squares of these proportions for each county, i.e., HHI = $\sum_{j=1}^{J} S_{jc}^2$ where S_{jc} is district j's share of county c's total enrollment.

[34] That said, if we use the FTC guidelines for defining degree of competition, our results are qualitatively similar.

areas, both farm and non-farm (45% of the weighted sample); urban areas as those in which all of the households live inside urbanized areas (11% of the weighted sample); and "not-urban/not-rural" areas which comprise the balance. We suspect that districts rural areas will be less efficient than those in urban areas because they face little competition from other public schools and because many of them may not reach efficient scale.[35] The results suggest that school district efficiency does, indeed, vary by the urbanicity of the school district as rural districts do not appear to be efficient. Note, however, that the coefficient estimate suggests that school districts in urban areas do operate efficiently.[36]

We also examine the effect of school unionization on efficiency. The *a priori* effect of teachers' unions on "productivity" is unclear (Eberts, 1984; Eberts and Stone, 1987; Hoxby, 1996). On the one hand, unions may increase schooling efficiency because they have a better understanding of the educational production process or by giving their members "voice" to communicate grievances with management (Freeman and Medoff, 1984). On the other hand, the unions may predominantly exact "rents" from the school district and thereby decrease efficiency. For this exercise we define those districts where at least 50 percent of the teachers are organized and there is at least one collective bargaining agreement in effect at the time as "unionized." Approximately 45 percent of our sample school districts were unionized in 1980. The results are in the bottom panel of the table. We find little evidence that unionized districts spend beyond the optimal level thereby wasting the education dollars they spend.[37] Rather, we find that unionized districts may

[35] That said, some urban residents will also have few choices as in some urban centers (e.g., Chicago, Los Angeles, New York) the entire city is represented by one school district.

[36] The results by the education level and the Herfindahl-Hirshmann Index of the district cannot solely be explained by urbanicity. When we estimate the results by education level and by the competitive pressure felt by the district for only urban school districts, the qualitative results (that districts in less-educated communities and those facing little competition are inefficient) remain. Similarly, the results by the Herfindahl-Hirshmann Index do not appear to be explained by the size of the county since if we control for the number of square kilometers in the county the results are nearly identical.

[37] These results partly reflect regional differences as most of the low-education districts are in the South which has traditionally had a lower tolerance for unions.

under-spend (particularly relative to non-union districts).[38]

Finally, we estimate whether the effects of state aid on school inputs and outputs differ by some of the district characteristics we have examined. If school districts in more highly educated communities and those districts that face more public school competition are more efficient, then one should observe significant differences in how the state aid is translated into schooling inputs and outputs. In Table 7 we estimate the effect of state aid on the change in the logarithm of the district pupil-teacher ratio and of the district high school drop out rate by selected district characteristics. By-and-large, the results do suggest that districts that were observed to be more efficient according to the effects on property values are more effective at translating an increase in state aid into lower pupil-teacher ratios than districts that were observed to be less efficient. The results on the effect of state aid on school outcomes (high school dropout rates) are much less conclusive, again because of relatively large standard errors. Nonetheless, we interpret these results as providing some evidence that the changes in state aid are affecting school quality.

The results in Tables 6a and 6b suggest that school districts in areas with less educated residents spend less efficiently than school districts with highly educated residents and that schools districts that face a lot of competition (as measured by a Herfindahl-Hirschman Index) are more efficient than districts that face little competition. We also find that unionized school districts are more efficient than non-unionized school districts. The results suggesting that increased competition can improve the performance of public schools are consistent with Hoxby (1994), and (indirectly) consistent with Urquiola (1999) who finds that in areas

[38] We have also tested for differences by the percentage of residents over the age of 55 and found that areas with the highest concentration of older residents (about 32%) were significantly more likely to under-spend relative to areas with less than 14% older residents. In contrast, we do not find significant differences by percentage of children who attend private schools and percentage of homeowners in the district. When we test for differences by the type of school district (elementary, unified, or high school), we find no statistically significant differences among the types. Finally, when we group districts by size we cannot reject that spending efficiency is similar between large and small districts, largely because the estimates are quite imprecise.

with greater choice of school districts student-teacher ratios are lower.[39]

V. Conclusion

In this paper we take a "market-based" approach to examine the effect of school expenditures on school quality and whether public schools are inefficient. We find evidence that, on average, it appears additional school funding increases school quality, as reflected in property values. In addition, we find that, on average, school districts are likely spending their state funding efficiently, or at least they are not over-funded. We also find that school districts spend less efficiently in areas in which school districts face less competition from other public schools and in areas in which residents are less educated (leading either to less mobility from a lack of resources or to less efficient education production through peer effects). One interpretation of these results is that increased competition has the potential to increase school efficiency.

Some care must be taken in interpreting these findings. First, the judgements about school efficiency result from a model with strong assumptions. While we do not believe that violations of these assumptions would have a large impact on our qualitative findings, they must be kept in mind. Second, based on our methodology, it is unclear whether increased efficiency would generate higher or lower levels of education spending. For example, while we find evidence that some districts over-spend on education, our analysis cannot reveal the source of the inefficiency and therefore we cannot determine whether increased competition would lead to increases or decreases in education spending. Competition may lead districts to decrease the amount of education provided and thus decrease spending. Alternatively, competition may lead districts to increase their productivity with little effect on the total spending. Finally, we note that the competition we

[39] Although it might appear that these characteristics reflect very different sources of mobility, peer effects, and/or competitive pressure, they actually appear to tell a fairly consistent story. Low-education districts, which have a higher proportion minority are lower income, are also districts that face less competition from other public school districts. For example, only 7 percent of low-education districts face high levels of competition from other public schools compared to 47 percent of high-education districts. (See Appendix Tables IIa and IIb for means by district characteristic.) Additionally, only 8 percent of low-education districts are unionized compared to 63 percent of high-education districts.

observe that improves efficiency likely has the consequence of increasing stratification which may decrease social welfare (Fernández and Rogerson, 1996, 1998; Bénabou, 1993). As such, policymakers interested in increasing competitive pressure on schools should attempt to do so with policy mechanisms that do not also have the consequence of increasing stratification.

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