# Does the United States Spend Enough on Public Schools?

**Patrick J. Bayer*** Duke & NBER

**Peter Q. Blair**† Harvard & NBER

**Kenneth Whaley**‡ University of South Florida §

January 2, 2024

**Abstract**

The United States ranks low among peer countries on the ratio of teacher spending to per capita GDP. Is this (in)efficient? Using a spatial equilibrium model we show that spending on schools is efficient if an increase in school spending funded through local taxes would leave house prices unchanged. By exploiting plausibly exogenous shocks to both school spending and taxes, paired with 25 years of national data on local house prices, we find that an exogenous tax-funded increase in school spending would significantly raise house prices. These findings provide causal evidence that teacher spending in the U.S. is inefficiently low.

*JEL Classification:* I22, I24, and H41

*Patrick Bayer is a Professor at Duke University in the Economics Department and Research Associate at the NBER.

†Peter Blair is an Assistant Professor at Harvard University in the Graduate School of Education, a National Fellow at Stanford University in the Hoover Institution, and a Research Associate at the NBER.

‡Kenneth Whaley is an Assistant Professor at University of South Florida in the Economics Department.

§This paper previously circulated under the title "A National Study of School Spending and House Prices." We received helpful comments from: Rucker Johnson, Thomas Kane, Claudia Persico, Larry Katz, Edward Glaeser, Joseph Gyourko, Kirabo Jackson, Eric Hanushek, Michael Lovenheim, Amy Finkelstein, Hunt Allcott, Isaiah Andrews, Sarah Turner, Jesse Rothstein, Desmond Ang, David Deming, John Friedman, Nathaniel Hendren, William Doerner, Neale Mahoney, Jonathan Meer, Susan Dynarski, Raj Chetty, Jeffrey Smith, Chao Fu, Richard Murnane, Juan Carlos Suarez Serrato, Aimee Chin, Chinhui Juhn, Jonah Rockoff, Mark Shepard, Barbara Biasi, Martin West, Morgan Adderley, Anne Korte, Matthew Weinzierl, and seminar participants at NBER Public Economics Fall Meeting, NBER Summer Institute Urban Economics Meeting, Harvard, University of Pennsylvania, UC Berkeley, UVA, Wisconsin, UCLA, E-con of Education, Cleveland Fed, Clemson, Nebraska, Tulane, Houston, Tufts, New York Fed, University of Chicago, Yale, Columbia, Southern Economic Association, Western Economic Association and The BE-Lab.

Teacher salaries in the U.S. routinely rank near the bottom worldwide as a fraction of GDP per capita. In fact, the US is among a small share of countries in which teachers are paid less than the average GDP per capita (Sandefur, 2018). Such international comparisons and the dueling empirical results on the impact of school spending on student outcomes naturally raise the question of whether the U.S. spends enough on its public school teachers and, more generally, on its public schools (Hanushek, 1989, 2003; Jackson et al., 2016).[1]

Economists naturally approach questions about the optimal provision of publicly provided goods like education through the lens of the Samuelson equation (Samuelson 1954). According to the Samuelson equation, a public good is efficiently provided when its marginal cost equals the sum of the marginal benefits of those who enjoy it. Due to the inherent difficulty in inferring the marginal benefits for the millions of members of society, public goods provision stands as a classic example of a potential market failure in economics — presenting a challenge for policymakers who decide how much to spend on schools and teachers (Poterba, 1996). In fact, in the U.S., public opinion on increasing school spending and increasing teacher salaries is particularly divided. Among survey respondents who are informed about the level of school spending and teacher salaries in their state, the partisan gap is 31 percentage points: 62% of Democrats versus 31% of Republicans believe that school spending should be increased (Houston et al., 2022). Likewise, while 70% of informed Democrats believe that teacher salaries should be increased, only 46% of informed Republicans share that view (Houston et al., 2022).

At its heart, answering the question of whether the current level of public school spending in the United States is optimal/efficient requires a way of measuring and aggregating the marginal benefits and cost of school spending. The primary goal of this paper is to theoretically define and empirically implement a test for the efficiency of public school spending in the U.S. that is based on a credible research design.

[1] The growing consensus, in the literature, as summarized in Jackson and Mackevicius (2021), is that increased school spending has a causal impact in improving student outcomes.

Local house prices provide natural measures of how much households value local public goods, including school quality as shown in Black (1999), Figlio and Lucas (2004), Linden and Rockoff (2008), Imberman and Lovenheim (2016), and the papers surveyed in Black and Machin (2011). Moreover, the housing market provides an attractive context for inferring the marginal benefits of politically contested public goods such as school spending because the partisan gap in attitudes towards home ownership and perceptions of housing affordability is tiny and an order of magnitude smaller than the partisan gap in views of school spending.[2]

We begin by developing a model of local public goods provision and house prices, following the classic literature in public finance (Musgrave, 1939; Tiebout, 1956; Oates, 1969; Barlow, 1970). The theoretical model that we develop, while inspired by Tiebout (1956) and Oates (1969), builds more directly on the key insights in Brueckner (1979, 1982), which show that the application of spatial equilibrium to a broad class of systems of financing public goods yields an intuitive efficiency condition. The level of spending on a publicly provided good is efficient if a marginal increase in spending on the local public good funded through a corresponding increase in local property taxes has no effect on property values. By revealed preference, if increasing local property taxes to provide more of the public good results in increased (decreased) house prices, then the prior level of spending was inefficiently low (high). We generalize Brueckner's framework to a setting with households of heterogeneous preferences and show that this efficiency test is applicable under a very general set of conditions even when the (stronger) assumptions of the Tiebout model fail. The efficiency test is also valid when there is redistribution of property tax revenues across districts.

The intuition informing our model has been implemented in other studies. An especially well-designed study by Cellini et al. (2010), for example, uses school infrastructure

[2]In surveys conducted by the CATO Institute and the Pew Foundation the partisan divide in housing market perceptions was 4-6 percentage points: 90% of Republicans and 86% of Democrats favored home ownership to renting; and 77% of Democrats and 83% of Republicans perceived housing affordability to be declining relative to 2021 (Ekins and Gygi, 2022; Pew Research Center, 2022).

bond elections to test whether the level of capital spending on schools is efficient. A key insight of this study is that the passage of an infrastructure bond referendum by local voters naturally bundles the marginal benefits and costs of capital spending on schools, i.e., the funding for school construction from a successful bond election comes with the implicit future tax obligations required to pay off the associated debt. In this setting, an efficiency test can be implemented by examining the direct impact of the marginal passing of a bond on local property values.

While capital spending is important, it constitutes 8 percent of overall school spending, on average. Most school spending (92 percent) instead falls into the category of "current expenditures," the vast majority of which is spent on the salaries and fringe benefits for teachers and other school personnel.[3] Unlike capital expenditure, current expenditure is typically not financed by bond offerings — ruling out the close bond election research design as a test for the efficiency of current spending.

There are two further challenges to estimating the efficiency of current expenditure on schools. The first challenge is isolating plausibly exogenous variation in both school spending and taxes. In the paper that is most similar to ours, Barrow and Rouse (2004), the authors have an instrument for school spending but not for tax revenue; therefore they credibly "test for whether the current level of school spending is efficient conditional on the inefficiency induced by the property tax." In related work, Dee (2000) and Biasi (2023) estimate the reduced form effects of school finance reforms on house prices. The second challenge faced by other credible research designs commonly used in the capitalization literature, e.g., boundary discontinuity designs and close bond referenda, is that they require extensive amounts of micro data on house prices and local jurisdictions – making it difficulty to construct a panel that spans both a long time period and a broad geography. Consequently, most estimates that leverage credible quasi-experimental research designs focus on a single state or metropolitan area (Black 1999; Bayer et al. 2007; Kane et al. 2006;

[3] For this reason we use "current expenditures" and school spending interchangeably in the paper.

Epple and Ferreyra 2008; Rockoff 2010; Cellini et al. 2010; Neilson and Zimmerman 2014; Chakrabarti and Roy 2015; Martorell et al. 2016). The empirical strategy that we develop successfully tackles both challenges encountered in the literature.

Our implementation of an empirical test for efficiency is guided by the theoretical results from our model. Notably, we construct a 25-year national panel of quality-adjusted local house price indices (HPI), school spending, and taxes paired with a strategy for isolating plausibly exogenous variation in the school spending and local property taxes. To construct instruments for current spending, we organize school districts into quartiles of spending prior to court-mandated school finance reforms (SFRs), and interact pre-reform spending quartiles with time post reform, following the approach in Dee (2000), Jackson et al. (2016), and Lafortune et al. (2018). We use a similar strategy to construct a new set of instruments for local property tax revenues inspired by the insight in Hoxby (2001) and Hoxby and Kuziemko (2004) that school finance reforms in some states affected school districts' incentives to change local property taxes. We construct instruments for local property tax revenue by organizing school districts into pre-reform quartiles of local property tax revenues and interacting pre-reform tax quartiles with time since reform.

To identify separately the impact of school finance reform-induced changes in taxes (school spending) on house prices, we hold the pre-reform spending (tax) quartile fixed and estimate changes in house prices for districts that were in different tax (spending) quartiles. For this identification argument to work, we need to have many districts that differ in their pre-reform spending and pre-reform tax quartiles. In each of the four pre-reform spending quartiles 34% to 64% of school districts are in an off diagonal pre-reform tax quartiles. Further, the share of off-diagonal districts in a state is uncorrelated with the reform year. We also exploit having multiple instruments to test that each instrument satisfies the exclusion restriction. Using a sequence of event study plots we show that our research design yields plausibly exogenous and independent variations in current school spending and local tax revenues that are capitalized into local house price indices.

Implementing our research design, we find a statistically significant house price response to an exogenous increase in current school spending. Holding school spending levels constant, we also find that house prices fall by a statistically significant amount in response to an increase in local property taxes. Combining the estimated impact of school spending and taxes on house prices, we find that, on the margin, a 1% tax-funded increase in current spending on schools would increase house prices by 0.33%, suggesting that current spending on schools in the U.S. is inefficiently low. All of our key results related to house price capitalization and the efficiency test are robust to the inclusion of numerous controls for county $\times$ time trends in demographics, potentially concurrent policy changes, and the subsequent sorting of households across school districts, isolating different sources of variation to test for homogeneity in our local average treatment effects, and to using only the later school finance reforms.

When Tiebout developed his theory of local public good provision in his seminal article Tiebout (1956), it was in response to the consensus view held by Musgrave (1939) and Samuelson (1954) " that no 'market type' solution exists to determine the level of expenditures on public goods. Seemingly, we are faced with the problem of having a rather large portion of our national income allocated in a 'non-optimal' way when compared with the private sector." Our work in this paper draws inspiration from the ambitious research agenda set by Tiebout. Our paper is the first to test whether current expenditure on schools in the U.S. is efficiently provided that exploits plausibly exogenous variation in both taxes and school spending using a nationally sample of school districts. The market-based test that we implement provides a credible answer to an important question of public good provision. Annual public expenditures on K-12 schools in the United States totaled \$640B – or 3.6% of US GDP in 2015 – comparable to Medicaid spending ($\sim$ 2%) and total spending on income assistance programs ($\sim$ 1.2%), yet there has been limited evidence on the question of whether the U.S. is spending enough on schools and teachers (Poterba, 1996). Our paper fills this gap.

The remainder of the paper is organized as follows: Section 1 describes the theoretical model and the efficiency test; Section 2 describes the data; Section 3 outlines the research design; Section 4 presents our main results; Section 5 discusses the results in more detail and presents robustness tests; Section 6 explores heterogeneity in our results by geography and the elasticity of housing supply; and Section 7 concludes the paper with a discussion of why our results may be conservative lower-bound on the extent to which school spending is inefficiently low in the US.

# 1 Testing for Efficiency of School Spending: Theory

The empirical test for the efficiency of school spending that we propose and implement in this paper is rooted in the theoretical public finance literature developed since Samuelson (1954). In this section, we begin with a short discussion of the historical development of the related theory, lay out the theoretical framework and key assumptions that provide the basis for our efficiency test, and close by discussing two subtle issues that are important for empirical implementation.

## 1.1 Background

The Samuelson equation for the efficient provision of public goods is straightforward to understand in theory: the level of a public good should be increased up to the point where the aggregate marginal benefit equals the marginal cost of provision, i.e., $\sum MB_i = MC$. But economists have long pointed out how challenging it might be to satisfy this condition in practice, even for policymakers motivated to do so, given the inherent difficulty of truthfully eliciting each person's marginal benefit.

The central insight of Tiebout (1956) was that the sorting of households across communities gives local governments both the information and incentives needed to provide local public goods efficiently.[4] A major branch of the literature following Tiebout (1956) focused on theoretically grounding an empirical test for the efficient provision of local public goods. While some of the intuition for such a test appeared in the literature as early as Oates (1969), Brueckner (1979) provided the first formal statement of an intuitive test based on property values.[5] In particular, Brueckner (1979) showed an equivalence between the Samuelson condition for efficient public goods provision and the first order condition that results from communities choosing the level of the local public good, financed on the margin through local property taxation, to maximize aggregate property values.

Brueckner's key insight was that the core tenet of spatial equilibrium — that households with identical income and preferences must receive the same indirect utility no matter where they live — was essentially all one needed to derive this equivalence. As a result, his proposed test is not only deep but very general. His framework accommodates heterogeneous housing consumption within communities and tenure choice (rent or own). Households can be heterogeneous in terms of income and preferences, as we show in our extension of Brueckner's framework. Jurisdictions can collect property tax revenues from both businesses and residents, provide multiple public goods, and receive revenue transfers from the state or federal government.

It is important to emphasize that Brueckner's theoretical framework does not make any claims about whether we should expect public goods to be provided efficiently. Instead, it provides the theoretical basis for an empirical test of whether local public goods

[4]Tiebout's original paper was intuitive rather than formal and it launched a large literature in local public finance that sought to better understand its theoretical implications. A major branch of this literature focused on developing the theoretical conditions under which the market force of people "voting with their feet" would lead to the efficient provision of public goods in a system of local governments. So long as households are knowledgeable about (and reacting to) changes in expenditure and revenue patterns, the conceptual basis for efficient school financing relies on households sorting across districts.

[5]This test is sometimes referred to as the Oates test because the idea was suggested informally in a discussion late in Oates (1969). It is important to note that this idea was not the main focus of Oates' paper and, instead, many papers that appear in the literature in the 1970s implemented a different "Oates test" - i.e., whether public goods are positively capitalized into house values conditional on the local tax rate. In this way, Brueckner (1979) was more of a corrective to rather than a natural extension of the literature following Oates.

are efficiently provided in a very general framework, no matter the system of local public finance - i.e., no matter whether the assumptions of the broader Tiebout hypothesis hold.

In what follows, we present a slightly extended version of Brueckner's model with two goals in mind: (i) to provide the key economic intuition behind the test he proposes and (ii) to show that Brueckner's framework can be generalized to allow for heterogeneous preferences.

## 1.2 Theoretical Framework

We begin by dividing households into discrete heterogeneous types on the basis of income $y$ and preferences $\beta$. $\beta$ defines the preferences of each type over the bundle of housing services and neighborhood amenities that vary between communities. More specifically, household utility is defined over numeraire consumption $c$, housing services $h$, and the public good $g$: $u(c,h,g,\beta)$. Households choose from a set $J$ neighborhoods/school districts each of which provides $N_j$ heterogeneous housing units with housing service levels $(h_1^j, ...h_{N_j}^j)$.

The key implication of spatial equilibrium is that households of the same type $(y,\beta)$ must receive the same indirect utility level $u^*=u^*(y,\beta)$. This uniform utility condition is equivalent to $c^*=c^*(h,g,y,\beta)$ such that for households with identical taste and income, the choice of $(h,g)$ determines the consumption level needed to reach indirect utility level $u^*$.

The household's budget constraint is given by $c^*=y-R$, where $R$ is rent. As a result, spatial equilibrium implies the following bid-rent function for household type $(y,\beta)$:

$$R = y - c^*(h, g, y, \beta) \tag{1}$$

For interest rate $r$ and property tax rate $\tau$, we can write house value $V$ as:

$$V = R/(r + \tau) = (y - c^*(h, g, y, \beta))/(r + \tau) \tag{2}$$

Note that equation (2) applies both within and across communities and holds whether households own or rent.[6] Equation (2) uses the uniform utility condition derived from spatial equilibrium to create a tight link between house values across locations. In particular, on the margin, each housing unit's value must reflect the change in the willingness of the household type ($y$,$\beta$) who inhabits it in equilibrium to pay for any marginal change in the attributes of the housing unit or community ($h$,$g$,$\tau$). Thus, as long as each household type ($y$, $\beta$) chooses housing units in multiple communities in equilibrium, the marginal change in the value of any house for a change in ($g$,$\tau$) will reflect the marginal willingness to pay of the household who inhabits it for the associated change - e.g., $MB_i$. And, by summing over all housing units within a community, we recover $\sum MB_i$ in response to a change in ($g$,$\tau$), exactly what is needed to assess the Samuelson equation! [7]

Formally, the problem of a local government choosing the efficient level of the public good is isomorphic to the local government solving the problem of choosing the level of expenditure on the public good $g$ and the local property tax rate $\tau$ that maximizes aggregate property values in the district, subject to the budget constraint $G(g) \leq \tau \sum_i V_i(g,\tau) + B$, where B is a transfer from the state. To show that the local government's first-order conditions reduce to the Samuelson condition for optimal public good provision that equates the sum of the marginal benefits of the expenditure on the public good to the marginal cost, we first write down the government's problem of maximizing aggregate property value subject to its budget constraint:

$$\mathcal{L} = \sum_i V_i(g,\tau) - \lambda \underbrace{\left(\tau \sum_i V_i(g,\tau) + B - G(g)\right)}_{\text{Budget Constraint}} \tag{3}$$

Once we rearrange the local government's first order conditions, i.e., $\frac{\partial \mathcal{L}}{\partial g} = 0$ and $\frac{\partial \mathcal{L}}{\partial g} = 0$,

[6] To keep the presentation simple, we abstract from differences in the tax treatment of owner versus renter occupancy here.

[7] The efficiency condition is similar in spirit to testing whether the marginal value of public funds (MVPF) equals one (Mayshar, 1990; Hendren and Sprung-Keyser, 2020; Finkelstein and Hendren, 2020).

we obtain:

$$\frac{\sum_i \frac{\partial V_i}{\partial g}}{\sum_i \frac{\partial V_i}{\partial \tau}} = \frac{\tau \sum_i \frac{\partial V_i}{\partial g} - G'(g)}{\sum_i V_i + \tau \sum_i \frac{\partial V_i}{\partial \tau}}. \tag{4}$$

We use the expression for the housing value in equation (2) to calculate partial derivatives of aggregate property value with respect to $g$ and $\tau$, i.e., $\frac{\partial V_i}{\partial \tau} = -\frac{V_i}{(r+\tau)}$ and $\frac{\partial V_i}{\partial g} = -\frac{1}{(r+\tau)}\frac{\partial c}{\partial g}$, and to simplify the equation (4) in the following way:

$$\frac{\sum_i \frac{\partial c_i}{\partial g}}{\sum_i V_i} = \frac{-\frac{\tau}{r+\tau}\sum_i \frac{\partial c_i}{\partial g} - G'(g)}{\frac{r}{(r+\tau)}\sum_i V_i} \implies -\sum_i \frac{\partial c_i}{\partial g} = G'(g). \tag{5}$$

According to equation (5), at the optimal level of $g$, the marginal cost of producing the public good equals the sum of the reduction in consumption of the numeraire good across all households which is used to fund the production of the public good. Because we are in spatial equilibrium, the change in consumption of the numeraire good for a unit increase in the public good equals the marginal rate of substitution between the public good and consumption,[8] i.e., $\frac{\partial c}{\partial g} = -\frac{\partial u/\partial g}{\partial u/\partial c}$ and equation (5) reduces to the Samuelson condition for optimal provision of a public good:

$$\boxed{\sum_i \frac{\partial u_i/\partial g}{\partial u_i/\partial c} = G'(g).} \tag{7}$$

The efficiency test follows directly from spatial equilibrium and holds under any system for the provision of school spending, including pure local financing and various hybrid

[8] In spatial equilibrium households of a given type $(y, \beta)$ get the same utility across jurisdictions with different bundles of $(c, g)$, therefore:

$$\frac{du(y,\beta)}{dg} = 0 \implies \frac{\partial u}{\partial g} + \frac{\partial u}{\partial c}\frac{\partial c}{\partial g} = 0 \implies \frac{\partial c}{\partial g} = -\frac{\partial u/\partial g}{\partial u/\partial c} \tag{6}$$

.

systems that include transfers from the state and federal government (i.e., other sources of revenue in the community budget constraint shown above) in addition to a class of financing systems in which there is redistribution of property tax revenues from wealthy districts to poorer districts.[9]

As we turn to empirical implementation, it is important to highlight two key aspects of the test. First, because it is derived from first-order conditions, the efficiency test should be implemented on the margin - i.e., we want to identify the local average treatment effect (LATE) of an increase in school spending financed through local property tax revenues. Importantly, the IV estimator that we propose below has a direct interpretation as a weighted average of LATEs and, in presenting results, we consider a variety of alternative specifications that evaluate the test on different margins - i.e., different LATEs.

Second, as Brueckner (1982) makes clear, because the Samuelson condition requires explicit aggregation across all households within the community, the efficiency test should be based on the impact of local spending and taxation on aggregate (average) property values. In the empirical analysis below, we use a quality-adjusted house price index, which is designed to measure the average rate of house price appreciation in the community, exactly the right theoretical object for implementing the efficiency test.

## 2 Data

The data used in our analysis are drawn from several sources. Average house prices within school district boundaries are measured by the FHFA house price index (HPI), derived from mortgage transactions on single-family properties securitized by Fannie Mae or Freddie Mac.[10] We observe HPI annually from 1990-2015 for nearly 5,400 school

[9] For example, in cases where there is redistribution from rich districts to poor districts, one could rewrite the budget constraint as $G(g) \leq \tau \sum_i V_i(g,\tau) + \tau_r \left(\bar{V}_{tot} - \sum_i V_i(g,\tau)\right) + B$, where $\tau_r$ is an exogenous parameter that measures the effective tax (subsidy) on property wealth above (below) a threshold value $\bar{V}_{tot}$. Working through the first-order conditions, we obtain the same result as in equation (7).

[10] The raw house price data is available through the FHFA: https://www.fhfa.gov/DataTools.

districts, and pair the measure with district-reported finance data from the *F-33 Annual Survey of School System Finances*. The annual survey of school district finances provides aggregate expenditure data along with detailed breakdowns by expense type (current and capital expenditures) and revenue source (federal, state, and local property tax revenues).[11] To construct the historic spending and property tax instruments we use the 1972 district finance data from the Census of Local Governments, and code up the reform timing with the initial passage year following Jackson et al. (2016) and described further in section 3.2.

## 2.1 House Price Index

Following the methodology developed in Case and Shiller (1989), the FHFA HPI is a "constant quality" index, which estimates appreciation using a sample of houses that have been sold or refinanced multiple times.[12] The key advantage of the FHFA HPIs is that they are available at the census tract level for most of the United States over a long sample period, whereas the widely-used Case-Shiller indices are only available at the metropolitan level. Relative to the Case-Shiller indices, the FHFA HPIs differ in that they are based on data for a sample of houses with conforming mortgages, i.e. mortgages below certain cut-off house values and loan-to-value ratios (LTV) and that, in addition to transaction prices, observations from homes that were refinanced are used in constructing the index.[13] In practice, the FHFA and Case-Shiller indices are very highly correlated and these differences in the sample selection create only small differences between the two indices (Leventis, 2008).

[11] All finance variables are deflated to 2015 dollars using CPI inflation conversion factors from Oregon State University. See https://liberalarts.oregonstate.edu/spp/polisci/research/.

[12] The index also employs a weighting procedure that allows for greater sampling variability in the price appreciation for houses that experience a longer time between transactions. As noted in Calhoun (1996), given two identical properties, differential rates of appreciation, change in the neighborhood sociodemographics, and other idiosyncratic deviations from market-level mean appreciation are more liable to arise the longer the time between transactions.

[13] As of 2019, the conforming limit in expensive coastal housing markets is a loan value of $726,525 and the maximum LTV is 97%. The conforming limit is $484,350 in the least expensive housing markets.

To construct our measure for district-level house prices from 1990-2015 we aggregate the underlying house price indicies at the census tract (j) $\times$year (t) level, $\tilde{p}_{j,t}$. Since the first year of data varies for each tract, we must first choose a new base year that is consistent across all tracts in a district. This will parse out within-district noise in the HPI measure due purely to differences in tract base years. We first convert all tract prices to base year 2003, the sample year with maximum data points:

$$\mathbf{P_{j,t}} = \frac{\tilde{p}_{j,t}}{\tilde{p}_{j,03}} \times 100.$$

We then weight the tract indices by the 1990 tract decennial population, $n_{j,90}$ as a share of the 1990 district aggegate population

$$\omega_j = \frac{n_{j,90}}{\sum_{j=1}^{J} n_{j,90}}; \tag{8}$$

where $\sum_{j=1}^{J} \omega_j = 1$. Thus our district-level price outcome is the population weighted tract average in each year

$$\mathbf{P_{d,t}} = \sum_{j=1}^{J} \omega_j \mathbf{P_{j,t}}.$$

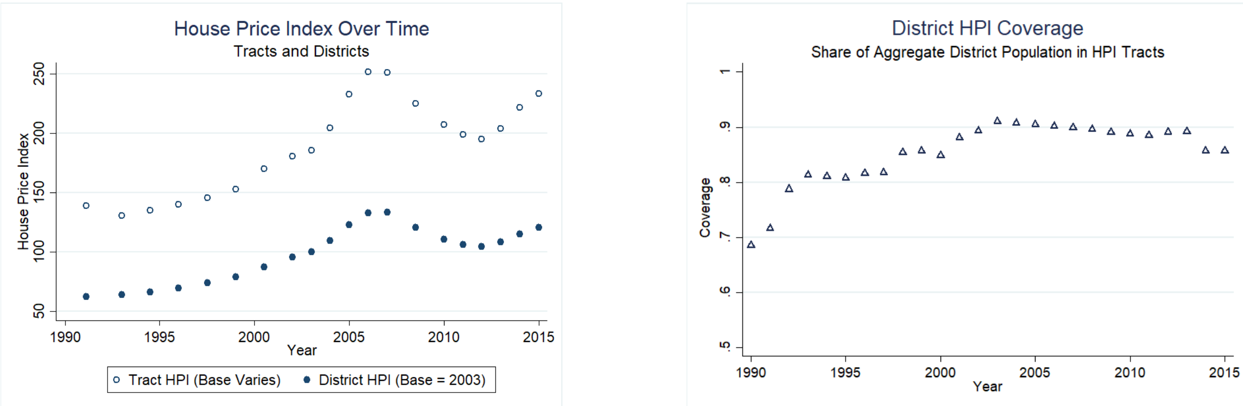
Figure 1 is a binned scatter plot of the mean district price $\mathbf{P_{d,t}}$ and tract raw price $\tilde{p}_{j,t}$ by year over time. Since the index measures within-unit price changes over time, the aggregate district index should follow the trends of the raw tract indices. The difference in levels is representative of differences in base years.

To get the most from the data we do not require the tract panel be fully balanced throughout the sample period. The tradeoff for more information is the potential for bias in the aggregation step if missing tract-level observations create inter-temporal differences in $\mathbf{P_{d,t}}$ unrelated to real price changes. To account for the potential measurement issues that arise from missing data, we construct a measure district *coverage* in the data and use it as a control variable in our empirical analysis. House price coverage in a district

Figure 1: House Price Time Series, 1990-2015

(a) Aggregate House Price Trends

(b) District House Price Coverage




Notes: (Left Panel) Binned scatter plot comparing the mean HPI at the district and census tract level over time. District HPI is the population-weighted average of census tract HPI within the attendance boundary. The level difference between the two measures is attributable to differences in base years. (Right Panel) Binned scatter plot shows the mean HPI coverage each year, which rises during the sample period. Tract population is fixed to 1990, thus increases in data coverage is caused by increased availability of house price data at the census tract level.

is defined as

$$coverage_{d,t} = \frac{\sum_{j=1}^{J} n_{j,90} \times \mathbf{1}(\mathbf{P_{j,t}})}{\sum_{j=1}^{J} n_{j,90}},$$

where $\mathbf{1}(\mathbf{P_{j,t}}) = 1$ if tract HPI is observed in year $t$. The right panel of Figure 1 is a plot of the mean district coverage by year during the sample period. As the tract-level price data improves in later years, district coverage improves to 90% on average.

## 2.2 School District Finances

School finance data are publicly available through the F-33 finance survey maintained originally by the Census of Local Governments for 45 of the lower 48 states. The Census of Local Governments is a massive historical database of public spending on schools and other services like municipal water and waste, public safety, fire departments and housing authorities. The line-item detail of the F-33 survey allows us to fully explore which school spending types matter and the effect of funding schools through local property taxes.

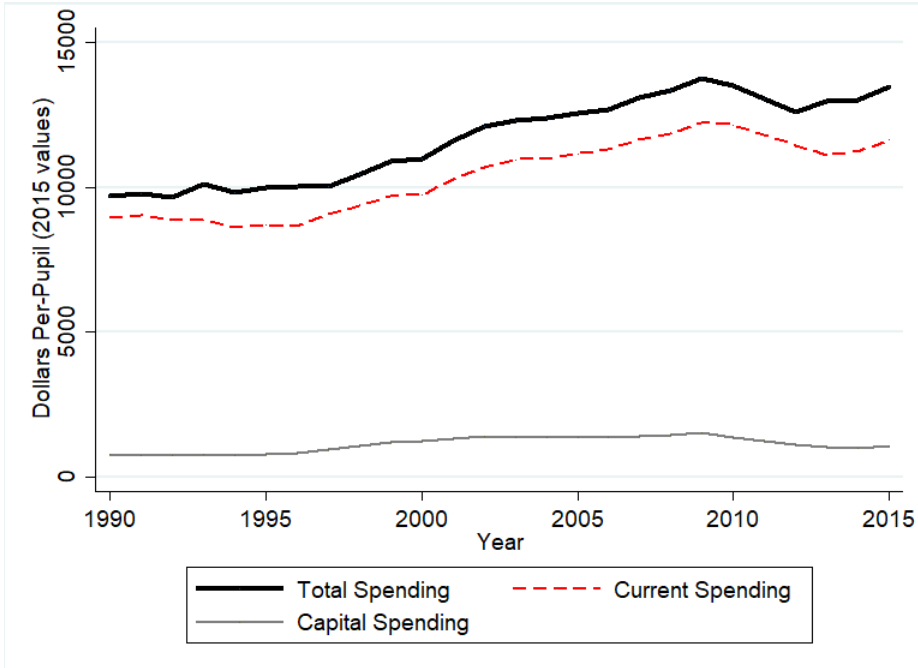
In this paper, we implement an efficiency test for current expenditures. Salaries, wages, and benefits make up the lion's share of these costs, but the category also includes other operational costs (support services and supplies). As shown in Figure 2, current expenditures make up 92% of total district spending and, importantly, are typically funded directly from current tax revenues. The remaining 8% of annual district expenditures are dedicated to capital spending on property, construction, and building rehabilitation.[14] Capital expenditures differ from current expenditures in two key ways. First, capital spending can be quite lumpy due, for example, to large one-time costs of construction. And, second, capital expenditures are often funded through bonds, which are paid back over many years and, therefore, implicitly come with the future tax obligations needed for repayment.

The focus of our study is implementing an efficiency test for current expenditures, since current expenditures represent the lion's share of total school expenditures and, moreover, the close election bond research design can not be readily applied to test for efficiency of current expenditures. Rather, the test that we develop requires a research design that leverages plausibly exogenous variation in both current expenditures and local taxes separately.[15] Local property taxes have long been a contentious source of revenues for school districts. Despite the sheer volume of legislative reforms targeting budgetary reliance on property taxes, the average U.S. school district raised 38% of total revenues via property taxes in 2015. Perhaps indicative of the uneven adoption of finance reforms, the NCES reported in 2017 that the share of revenues from property taxes varied from 17% to 53% by state. For the average district, the remaining funds come from state (48%), federal (14%), and other local sales and millage taxes. For the mean school in the sample property taxes comprise 80% of all local tax revenues collected.

[14]We provide a deep description of school expenditure layers in appendix Table A.2

[15]We discuss how we isolate this exogenous variation in much greater detail in the next section of the paper.

Figure 2: School District Mean Expenditures, 1990-2015




Notes: Time series plot of real per-pupil spending over time for sample school districts. Total expenditures are equal to the sum of capital and current expenditures. All dollar values are deflated to 2015 levels.

## 2.3 Final Dataset

Following Jackson et al. (2016), we include two additional sets of control variables: (i) county level descriptive variables from 1960 such as the poverty rate, minority share, and rural population percentage, interacted with time trends and (ii) the amount of time elapsed since a state adopted or first funded various programs including Head Start, kindergarten, school desegregation, hospital desegregation, and Medicare certification. In all cases, the goal of adding these controls is to ensure that our empirical estimates are robust to possible heterogeneous trends across districts. The final data set consists of nearly 124,000 school district-by-year observations from 40 states and roughly 5,400 U.S. school districts. Summary statistics are presented in Table 1. Given that school districts in the final sample are limited to those with available house price data, we compare the finance data for our sample to the entire school district database in Table 1 and find no statistically significant differences in means.[16]

Table 1: Summary Statistics

| | Full Sample | Quartile 1 | Quartile 4 | Universe |
|---|---|---|---|---|
| **Across School Spending Quartiles** | | | | |
| House Price Index - 1990 | 51.16 | 50.59 | 55.42 | |
| House Price Index - 2015 | 122.07 | 131.75 | 114.42 | |
| *District Finance Variables ($2015)* | | | | |
| Current Spending - 1990 | 8,633.59 | 8,392.56 | 9,955.59 | 8,541.26 |
| Current Spending - 2015 | 11,516.63 | 11,022.47 | 13,158.20 | 11,368.10 |
| Prop. Tax Revenue - 1990 | 3,628.32 | 2,375.74 | 5,217.97 | 3,585.91 |
| Prop. Tax Revenue - 2015 | 4,870.67 | 3,624.46 | 6,445.97 | 4,799.96 |
| **Across Property Tax Quartiles** | | | | |
| House Price Index - 1990 | | 36.74 | 55.41 | |
| House Price Index - 2015 | | 118.32 | 114.37 | |
| *District Finance Variables ($2015)* | | | | |
| Current Spending - 1990 | | 7,505.30 | 9,333.98 | |
| Current Spending - 2015 | | 10,424.77 | 12,485.49 | |
| Prop. Tax Revenue - 1990 | | 2,282.41 | 4,916.72 | |
| Prop. Tax Revenue - 2015 | | 3,301.63 | 6,253.98 | |
| District×Year Observations | 123,978 | 30,092 | 30,539 | 197,579 |

Notes: The house price index is an annual measure of real single-family home values within a district, and equals 100 in the base year (2003). Column 1 is the sample of school districts with sufficient house price data to compute the district-wide price index. Since the sample is constrained by house-price coverage, column 4 displays the school spending data for the entire sample of schools in which spending data is available. Column 2 and column 3 summarize the data for districts categorized as lowest-spend (quartile 1) and highest-spend (quartile 4) based on historical expenditures relative to other districts within the same state. House price indices are inflation adjusted for the real growth calculation.

[16]Two auxiliary data cleaning steps are taken to create the final long panel. Since the district finance data is a survey, we must first exclude districts with plausibly miscoded enrollment, spending and property tax data. To do so we follow the steps in Lafortune et al. (2018) directly. Second, school district finance data is not reported by Census of Governments for the years 1993-1996. As such, we utilize district aggregates computed in a joint effort by the Albert Shanker Institute, Rutgers Graduate School of Education, and the University of Miami College of Education. See https://www.schoolfinancedata.org/download-data/.

# 3 Research Design

In this section, we present the features of the research design that form the basis of our analysis. We begin by describing some of the serious endogeneity issues that arise in attempting to identify the causal impact of current expenditure and local property taxes on housing prices. We then lay out the school finance reform event study design, inspired by the recent studies of Jackson et al. (2016) and Lafortune et al. (2018). Extending this design, we introduce a second set of instruments that exploit another dimension of variation in the way these reforms affected districts based on initial local tax revenue levels. The two sets of instruments are well-suited for identifying the capitalization of both school spending and local property taxes. With the ability to identify the capitalization of both spending and taxes in a single study, we describe the empirical specification for testing the efficiency of local public goods provision.

## 3.1 The Empirical Challenge

Estimating the extent to which school spending is capitalized into property values has long proven to be a challenging problem. Generally speaking, school spending is highly correlated with local resources. This creates an obvious endogeneity problem, as these resources are highly correlated with other local amenities that might impact local housing prices. Even more directly, the level of local school spending is highly correlated with the composition of the community itself, which might affect property values in any number of direct and indirect ways.

Another generic complication that arises when school spending is primarily financed from local sources is that spending increases are directly linked to increases in property taxes and other local sources of tax revenue. In this way, we would expect property values to capitalize the total value of the (highly co-linear) bundle of spending and tax increases. In such a setting, it would not be surprising for OLS estimates of school spending on housing prices to reveal a very small willingness to pay for increases in school spending, as the estimates would capture the combined effect of the spending and tax changes.[17] In fact, as described in Section 1, our efficiency test is premised on the notion that the effect of a marginal change in school spending financed through local taxes should be exactly zero if spending is efficient.

Unfortunately, these kinds of identification problems do not disappear when financing moves to higher levels of government. In this case, a host of different endogeneity issues arise because transfers from the state and federal government are often explicitly tied to a district's property tax base and other local economic conditions. As a result, state and federal funding levels, which often have a redistributive motivation, are often negatively correlated with many factors that directly influence a district's property values.

With these challenges in mind, the main empirical goal of our paper is to estimate the capitalization of school spending and local taxes into property values in a manner that deals directly with this broad array of potential endogeneity problems. To that end, we apply and extend the research design developed by Jackson et al. (2016) to our context. We exploit the timing of court-mandated school finance reforms across states in the U.S. to isolate plausibly exogenous changes in current spending and local property taxes. To fully appreciate the logic of this design, and to understand how it helps to address the numerous endogeneity problems that have made estimating school spending capitalization so difficult, we first provide a brief overview of the wave of court-mandated school finance reforms that swept across the U.S. beginning in the 1970s.

## 3.2 Court-Mandated School Finance Reforms

Unlike many countries which finance education primarily at the national level, the financing of public schools in the U.S. has historically relied heavily on local taxation, primarily

[17] OLS estimates of the specifications shown in Table 5 result in a coefficient on local property taxes that is positive and a coefficient on school spending that is close to zero.

in the form of property taxes. Not surprisingly, such local financing has long generated substantial inequality in spending levels across school districts.

Beginning in the early 1970s in California, citizens of a number of states began challenging this local system for financing public schools on the basis that it violated certain protections provided in their state's constitution. A first wave of rulings, initiated by the *Serrano v. Priest* decision in California in 1971, found that funding public education through local property taxes violated the equal protection clause of the state's constitution, leading to a series of "equity reforms." A second wave of rulings, initiated by the Kentucky State Supreme Court decision in *Rose v. Council for Better for Education* in 1989, was predicated on a constitutional right to the provision of an adequate level of education for children in all parts of the state, leading to a series of "adequacy reforms."[18] In total, the existing school finance regime has been successfully challenged in 25 states since 1971. Figure 3 shows the variation in reforms across space and time, following the coding in Jackson et al. (2016).[19]

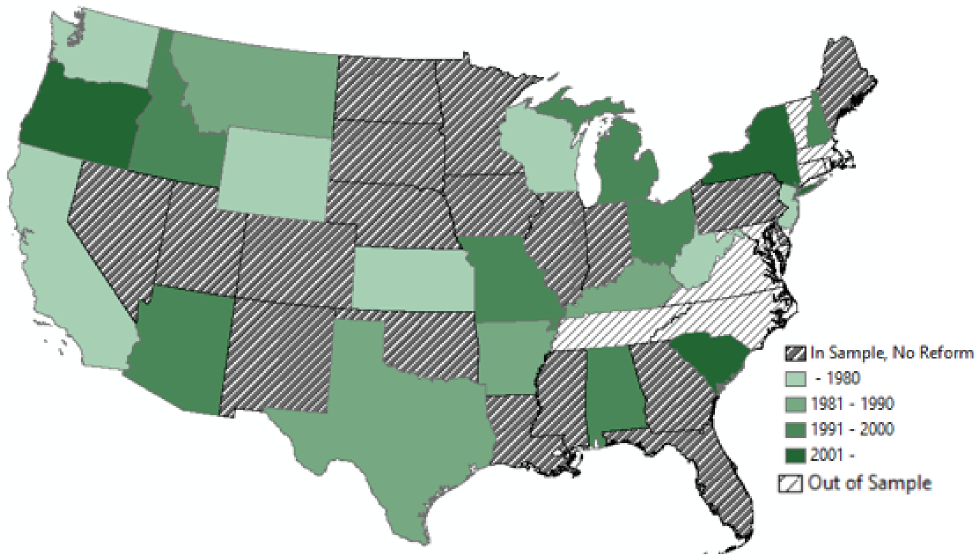
While successful challenges to existing school finance regimes often shared similar legal bases and the general goal of reducing inequality in school spending across students, the implementation of court-mandated school finance reforms varied widely across states, often requiring a lengthy back and forth between the state legislature and the courts until the final implementing legislation was deemed to have met the requirements of the state's constitution.[20] In practice, court-mandated school finance reforms took many forms including (i) block or matching grants from the state to poorer districts, (ii) district power equalizations, which attempted to effectively equalize local tax bases across districts, and (iii) state equalizations, which used state transfers to equal-

[18]See Lafortune et al. (2018) for more discussion of these two waves of reforms.

[19]We make one change relative to Jackson et al. (2016) and code MI as having a reform in 1994 – the year that Michiganders voted to pass a law that increased state funding to schools and reduced property taxes (Loeb and Cullen, 2004). See online appendix.

[20]The famous *Serrano v. Priest* case in California, for example, resulted in three distinct California Supreme Court rulings in 1971, 1976, and 1977, respectively, as well as associated trial court rulings in 1974 and 1983.

Figure 3: Sample States by Reform Status




**Notes**: The above map outlines timing of the first SFR for each state in the sample. States excluded from the sample due to house price data limitations include TN, VA, MA, RI, and NH. States excluded from the sample for school finance data limitations include NC, MD, and CT.

ize per-pupil spending across districts.[21] Each of these approaches embeds some form of redistribution of resources to districts with smaller local tax bases and/or poorer residents but there is considerable heterogeneity in the generosity and form of redistribution across states. As we will see, a recognition of this heterogeneity in the way school finance reforms were implemented across states plays an important role in our research design.

### 3.3 First Stage SFR Event Study

The main idea underlying the school finance reform event study design is that these reforms generated systematic changes in school spending that reduced inequality in spending across districts - e.g., raised spending in previously low spending districts relative to previously high spending districts. To isolate these kinds of SFR-induced shocks to

[21] The impact of various types of school finance reforms on a wide variety of outcomes including school expenditures, tax burdens, and local property values has been studied extensively in the economics literature from both empirical and theoretical perspectives. See, for example, Murray et al. (1998), Hoxby (2001), and Card and Payne (2002).

spending across districts, Jackson et al. (2016) sort school districts by the quartile of per pupil school spending within the state in 1972 and form instruments for per pupil spending levels by interacting these initial spend quartiles with the time since the court first mandated a school finance reform. Beginning in 1972, per-pupil expenditure, salary spending, and property tax revenue at the school district level is continuously available nationwide on an annual basis from the NCDB. Later in the paper, we explore the robustness of our findings to focusing on the post-1990 school finance reforms only, in the spirit of Lafortune et al. (2018), and find similar results to using all reforms.

Specifying the SFR event study as the first stage in our design uncovers dynamic effects on multiple margins of school district finances. Our exact implementation is as follows: we designate event time $T$ as the number of years that have elapsed since a state was first ordered by the courts to change its school finance system, and construct instruments for per pupil school spending in a given year by interacting the 1972 spending quartile with post-reform event time dummies from $T = 0$ to $T = 16$ interacted with the 1972 spending quartiles. The first stage can be expressed as:

$$log(s_{d,t}) = \sum_{T=0}^{T=16} \sum_{Q_s=4}^{Q_s=1} \phi_{Q_s,T}[\mathbb{1}(\mathbb{Q}_{\mathbb{s}}) \times \mathbb{1}(\mathbb{T})] + f_d + \beta X_{d,t} + v_{d,t}, \quad (9)$$

where: $s_{d,t}$ indicates per-pupil current spending of school district $d$ in time period $t$; $f_d$ indicates district fixed effects, $X_{d,t}$ indicates time varying district controls; $\mathbb{1}(\mathbb{Q}_{\mathbb{s}})$ are indicator variables for pre-reform 1972 spending quartile in state; $\mathbb{1}(\mathbb{T})$ are indicator variables for time relative to SFR reform; and $v_{d,t}$ is the error term.

Notice that the Jackson et al. (2016) instruments effectively aggregate the predicted change in spending post-reform across districts within an initial spend quartile in all reform states. Aggregating across districts within a quartile eliminates any idiosyncratic variation across districts that may arise, for example, as districts endogenously respond to local economic conditions in the period before or after the reform. Aggregating across states eliminates any idiosyncratic differences in the way that particular states implemented school finance reforms, isolating only the change in school spending that is predictable based on a district's initial spending level without regards for the particular implementing policy chosen by a given state.

### 3.4 Adding Taxes to the Analysis

Although the spending instruments alone contain abundant variation, i.e., formally there are 4 $\times$T instruments representing the 4 pre-reform quartiles and T post-reform time periods (see Bayer et al. (2020)), we were worried that using this variation alone to estimate the elasticity of house prices with respect to both taxes and spending obscures the nature of the underlying variation that is separately estimating the two elasticities. We address this concern by proposing a new source of variation to instrument for local property taxes where the identification argument is clear.

The fact that school finance reforms triggered changes in both school spending and local tax revenue levels naturally suggests a potential second dimension of instrumental variation that might be used to generate plausibly exogenous variation in local tax revenue. In particular, we construct a second set of instruments based on the interactions of each district's 1972 per-pupil property tax revenue quartile with post-reform event time dummies. In this case, the first stage can be expressed as:

$$log(\tau_{d,t}) = \sum_{T=0}^{T=16} \sum_{Q_\tau=4}^{Q_\tau=1} \rho_{Q_\tau,T}[\mathbb{1}(Q_\tau) \times \mathbb{1}(\mathbb{T})] + f_d + \beta X_{d,t} + v_{d,t}, \tag{10}$$

where $\tau_{d,t}$ indicates per-pupil local tax revenue of school district $d$ in time period $t$ and $\mathbb{1}(Q_\tau)$ are indicators for pre-reform 1972 local tax revenue quartile in the state. To estimate the elasticity of house prices with respect to local property taxes (spending) using school finance reform-induced changes in taxes (school spending), we keep the pre-reform spending (tax) quartile fixed and estimate changes in house prices for districts that were in different tax (spending) quartiles. Our identification strategy works best when we have a large fraction of "off-diagonal" entries, i.e., school districts that differ in their pre-reform spending and pre-reform tax quartiles. To get a sense of the additional variation that the new tax instruments bring to the analysis, Table 2 reports the cross-tabulation of the 1972 spending and local tax revenue quartiles, $Q_s$ and $Q_\tau$.

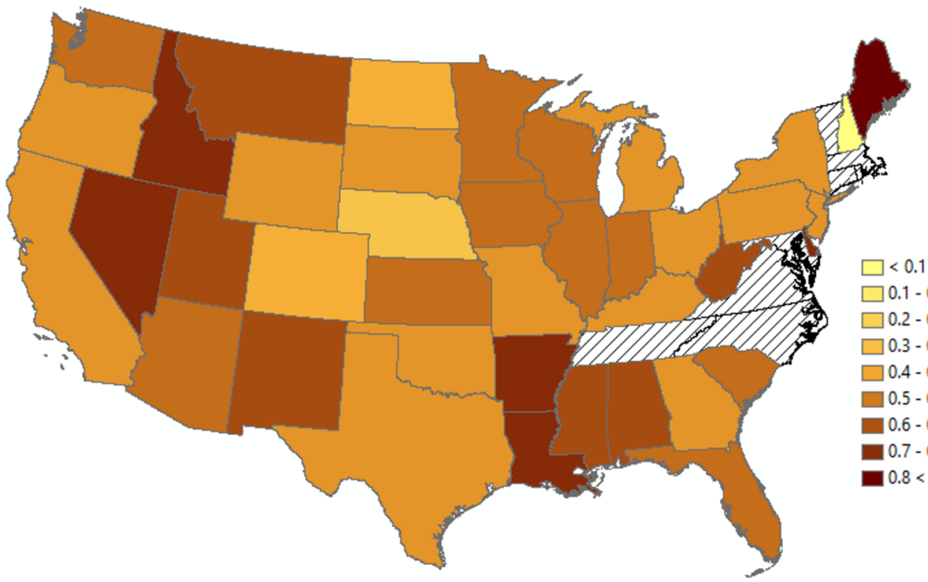
Table 2: Categorization of Districts by Spending and Tax Quartile

| | | 1972 Tax Quartile | | | | |
|---|---|---|---|---|---|---|
| | | **Q1** | **Q2** | **Q3** | **Q4** | **Off Diagonal** |
| 1972 Spending Quartile | **Q1** | **703** | 381 | 157 | 56 | 46% |
| | **Q2** | 287 | **485** | 451 | 127 | 64% |
| | **Q3** | 134 | 317 | **581** | 305 | 57% |
| | **Q4** | 100 | 141 | 216 | **875** | 34% |
| | **Off Diagonal** | 43% | 63% | 59% | 36% | |

**Notes**: The above matrix classifies each district according to position on two historic, state-level distributions: the 1972 distribution of total spending per-pupil and the 1972 distribution property tax revenue per-pupil. On diagonal school districts are classified into the same total spending and property tax revenue quartile. Off diagonal schools illustrate additional variation gained by including both sets of instruments in the main model of the paper.

In each of the four pre-reform spending quartiles, at least 34% of school districts are off-diagonal districts, as shown in Table 2. Off-diagonals are not only prevalent, they are also geographically dispersed. In most states 30% to 60% of school districts are off-diagonals (see Figure 4). The 10 largest and 10 smallest off-diagonal districts by student enrollment in Table 3 are located in a mix of northern, southern, eastern, and western states, some of which are treated by the reforms and some of which are untreated states. Moreover, we do not find a systematic relationship between the reform year and the fraction of off-diagonal districts in a state (see Figure 5), nor do we find any pre-trends in state per capita income in the years prior to a reform (see Figure 6). In our setup, which resembles a Batik-type environment, the two sources of identifying variation, the within-state share of off-diagonal districts and the across-state timing of the reforms do not appear to be correlated, lending credibility to our research design (Goldsmith-Pinkham et al., 2020; Borusyak et al., 2022).

Figure 4: Off-Diagonal Variation By State



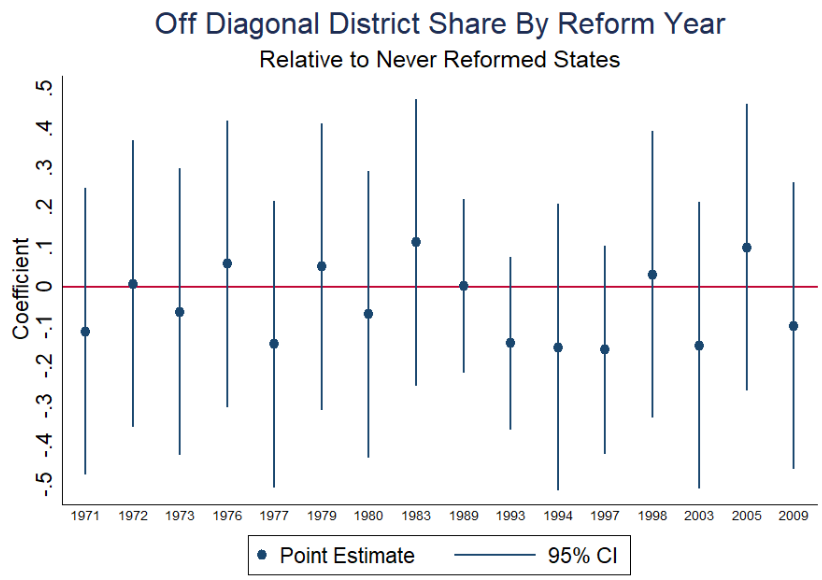

Notes: Illustrated above is the fraction of statewide total enrollment in off-diagonal school districts in 1972. Striped states are out of sample as described in Figure 3.

Table 3: List of the 10 Largest and Smallest Off Diagonal School Districts

| District | Enrollment | MSA | State | Tax Quartile | Spend Quartile | Reform |
|---|---|---|---|---|---|---|
| **20 Largest School Districts** | | | | | | |
| Los Angeles Unified | 646,683 | Los Angeles-Long Beach, CA | CA | 2 | 1 | 1 |
| Clark County | 324,093 | Las Vegas, NV-AZ | NV | 2 | 1 | 0 |
| Houston ISD | 215,225 | Houston, TX | TX | 3 | 2 | 1 |
| Hillsborough | 207,469 | Tampa-St. Petersburg-Clearwater, FL | FL | 3 | 2 | 0 |
| Orange | 191,648 | Orlando, FL | FL | 2 | 1 | 0 |
| Gwinnett County | 173,246 | Atlanta, GA | GA | 4 | 2 | 0 |
| Dallas ISD | 160,253 | Dallas, TX | TX | 4 | 3 | 1 |
| Philadelphia City SD | 134,241 | Philadelphia, PA-NJ | PA | 3 | 4 | 0 |
| San Diego Unified | 129,779 | San Diego, CA | CA | 3 | 2 | 1 |
| Duval | 128,685 | Jacksonville, FL | FL | 2 | 1 | 0 |
| **10 Smallest School Districts** | | | | | | |
| Shasta Union Elementary | 165 | Redding, CA | CA | 1 | 2 | 1 |
| Prairie Lea ISD | 163 | Austin-San Marcos, TX | TX | 2 | 3 | 1 |
| Grass Lake SD 36 | 159 | Chicago, IL | IL | 2 | 1 | 0 |
| Owendale-Gagetown | 153 | Rural Michigan | MI | 3 | 1 | 1 |
| Kenwood | 150 | Santa Rosa, CA | CA | 1 | 2 | 1 |
| Chilhowee R | 148 | Rural Missouri | MO | 3 | 1 | 1 |
| Richards ISD | 144 | Houston, TX | TX | 1 | 3 | 1 |
| Round Valley Joint Elementary | 136 | Rural California | CA | 4 | 3 | 1 |
| Shirland CCSD 134 | 121 | Rockford, IL | IL | 2 | 1 | 0 |
| Meyersville ISD | 115 | Victoria, TX | TX | 4 | 3 | 1 |

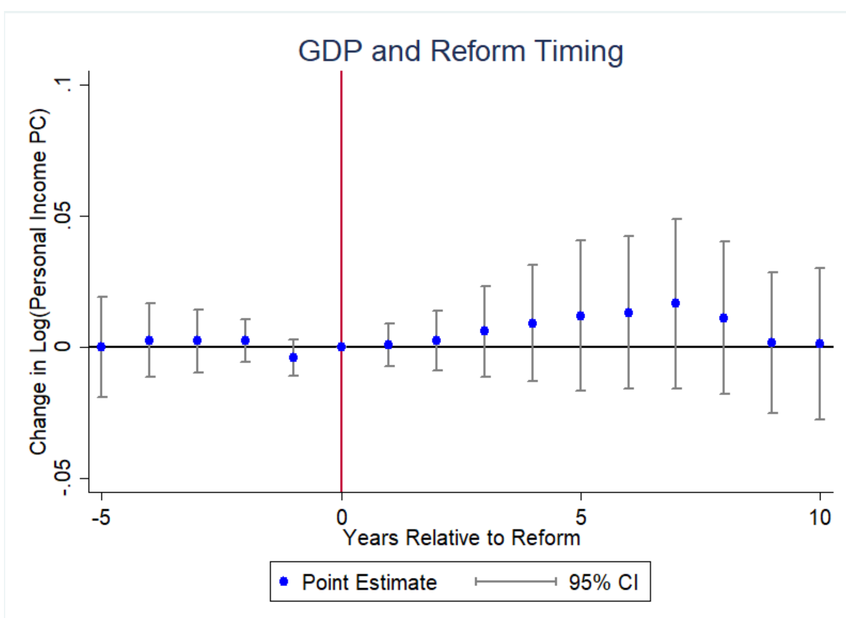
Notes: List of the 20 largest and smallest off-diagonal school districts ordered by enrollment. We also report the MSA where the district is located in addition to its pre-reform tax and spending quartiles and whether it is in a treated or a control state. In appendix Table B.7 we provide examples of on diagonal school districts.

Figure 5: No Correlation Between Off Diagonal Prevalence and Reform Year




Notes: The figure contains coefficients from a state-level regression in which the share of off-diagonal school districts is taken as an outcome of dummy variables for each reform year. The reference group is never reformed states.

Figure 6: State Per Capita Income Does not Predict Reform Timing




Notes: The figure contains coefficients from an event-study model taking state per-capital income as an outcome of reform timing. The reference group includes states that do not undergo school finance reform during the sample period. The estimates illustrate no preceding economic growth prior to state finance reforms. The state per capita income time series come from the St. Louis Fed: https://fred.stlouisfed.org/release/tables?eid=257197&rid=110.

### 3.5 IV Estimation and a Test of Efficiency

It is important to emphasize that the goal of our analysis is not to understand the effects of (particular) school finance reforms but to use them instrumentally, in the truest sense of the word, as major shocks to the ways that schools are financed and funded. These shocks generate plausibly exogenous district variation in spending and local property tax revenues dedicated to schools, allowing us to credibly identify the separate effects of school spending and local tax revenues in a single study. Of interest are the coefficients $\theta$ and $\gamma$ from the baseline specification

$$\begin{aligned} log(p_{d,t}) =& \theta log(s_{d,t}) + \gamma log(\tau_{d,t}) + f_d + \beta X_{d,t} \\ &+ \sum_{T=-4}^{T=-1} \sum_{Q_s=4}^{Q_s=1} \left[ \lambda_{Q_s,T} \mathbb{1}(\mathbb{Q}_s) \times \mathbb{1}(\mathbb{T}) \right] + \sum_{T=-4}^{T=-1} \sum_{Q_\tau=4}^{Q_\tau=1} \left[ \lambda_{Q_\tau,T} \mathbb{1}(Q_\tau) \times \mathbb{1}(\mathbb{T}) \right] + \epsilon_{d,t}, \end{aligned} \tag{11}$$

where $p_{d,t}$ are average district house prices, $s_{d,t}$ is current spending per pupil, and $\tau_{d,t}$ indicates local property tax revenue per pupil. To estimate equation (11), we instrument for both per pupil current spending and property tax revenue with the 1972 spending and property tax quartiles by time since reform instruments. Our model includes the pre-period interactions as exogenous covariates, along with district fixed effects and time-varying district controls.[22]

Equation (11) estimates the capitalization of current spending ($\theta$) and property taxes ($\gamma$), the key parameters needed to implement the test of the efficiency of the local public good provision developed in Section 1. To test for efficiency, we compute the percent change in the house price index, i.e., % $\Delta$ HPI, that arises from a 1% increase in local property tax revenues that are used to increase current expenditure on education. At the

[22] Due to the presence of never-reformed states, our research design does not require binned endpoints for unbiased identification of the event-study instruments (Schmidheiny and Siegloch 2023). We acknowledge, however, that by restricting our event-window to 16 years post-reform any state experiencing reform prior to 1974 will be included with the never-treated group as our data for school district house prices begin in 1990.

mean level of property tax revenue, a 1% increase in property tax corresponds to a 0.45% increase in current spending, hence the formula for the efficiency elasticity is:

$$\%\Delta HPI = \underbrace{(0.45) \times \theta}_{\text{more current spending}} + \underbrace{\gamma}_{\text{higher taxes}} .$$

According to the theory, if $\%\Delta HPI = 0$, current spending is occurring at the optimal level. If instead, $\%\Delta HPI > 0$, then school spending is inefficiently low. Otherwise, if $\%\Delta HPI < 0$, then school spending is inefficiently high. We use the estimated covariance of $\theta$ and $\gamma$ to calculate standard errors and a 95% confidence interval on our efficiency elasticity. It is important to keep in mind that the notion of efficiency here is a private one, in the sense that this measures whether the households living in a school district would receive more value from an additional dollar raised and spent on local public goods. Importantly, broader notions of social efficiency would need to include the benefits of any positive externalities that better-funded schools provide indirectly to others (Agrawal et al., 2022).[23] Any future education spillovers in the labor market or taxes collected by the government due to the higher wages of children attending the better funded schools would not be included here. Hendren and Sprung-Keyser (2020) estimate that many forms of social spending, especially programs that benefit young children, more than pay for themselves in discounted future tax receipts.

### 3.6 Event Study Plots using Spending and Tax Instruments Separately

We plot the event study coefficients on the interacted instruments to trace out time paths of reform induced shocks to current spending. The left-hand panel of Figure 7 highlights the variation in current spending isolated by the spending quartile instruments only. In particular, the figure shows the predicted gap in spending between school districts in the bottom three quartiles of pre-reform spending quartile relative to the quartile that initially

[23] There is an extensive literature on education externalities - see, for example, Moretti (2004).

had the highest level of spending, conditional on district fixed effects.

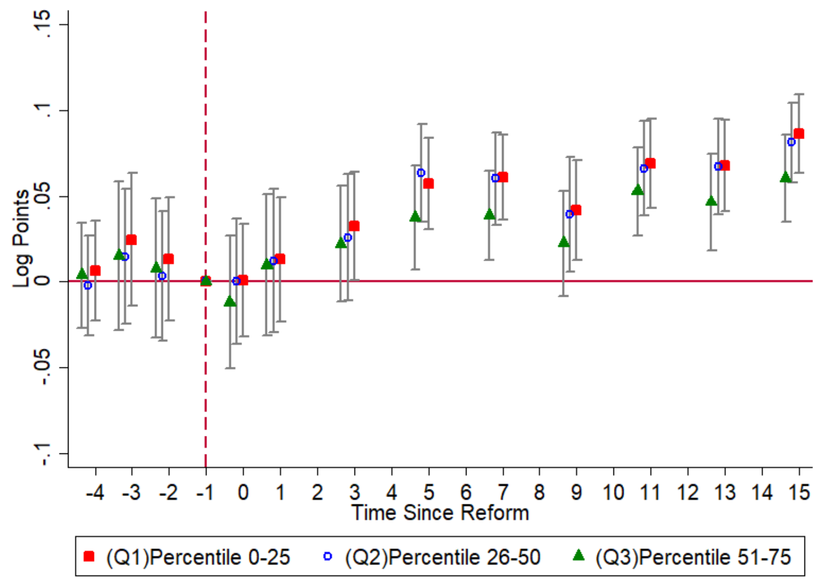
Across all of the states that instituted such reforms, in the fifteen years following a court-ordered reform, current spending increased in districts in the lower versus higher quartiles of the initial spending distribution. Notably, there is a small lag in the full realization of the reforms, reflecting the time it takes for the state legislatures to craft the implementing legislation. Because our interest is not in studying the impact of the SFRs, per se, but rather in using the reforms as an instrument to generate plausibly exogenous variation in school spending, the inclusion of the period between the court ruling and full reform implementation in each state in the post-reform period has little bearing on the analysis, as any delay in implementation by definition contributes little variation in relative spending across districts.

There is also essentially no difference in trends in current expenditures across the four spending quartiles prior to a school finance reform, supporting the assumption that the subsequent changes in spending across the four quartiles in initial spending are effectively shocks to spending levels, uncorrelated with any prior trends in relative spending levels.

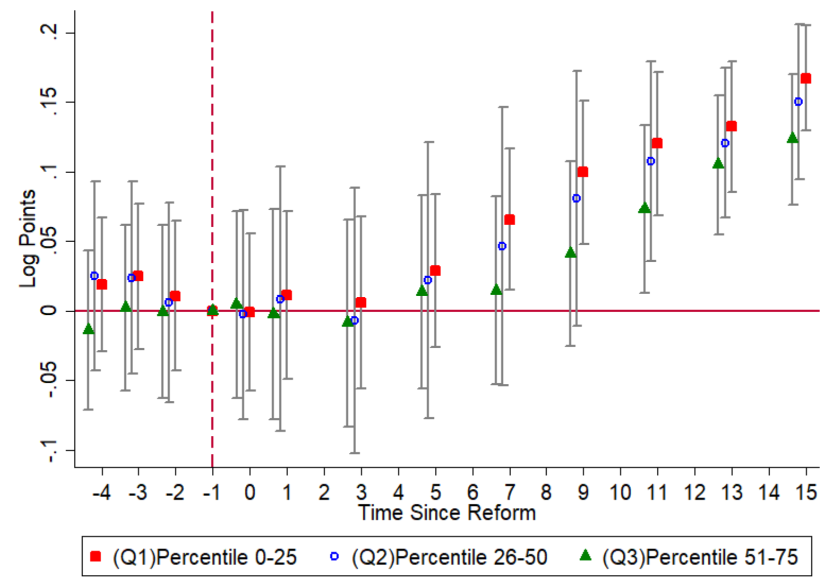
The right-hand panel of Figure 7 plots the reduced form estimates for house prices, analogous to the spending event study figure shown in the left-hand panel. The figure shows that analogous to the salary spending figure, starting a few years after the event date, house prices rose steadily in the initially lower spending quartiles (Q1-Q3) relative to the highest spending quartile (Q4). Like the corresponding changes in school spending, the relative increase in house prices was greatest for districts initially in the lowest spending quartile, with the difference in changes between Q1 and Q4 reaching a magnitude of 16 log points by the end of our 15-year post reform window.

Figure 7: Predicted School Spending and House Prices Using Spending Instruments

(a) Ln(Current Spending/Pupil)



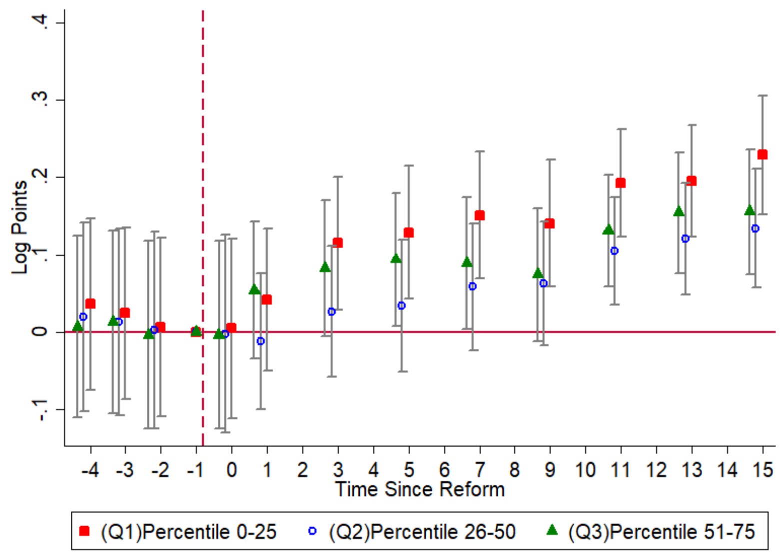

(b) Ln(House Price Index)



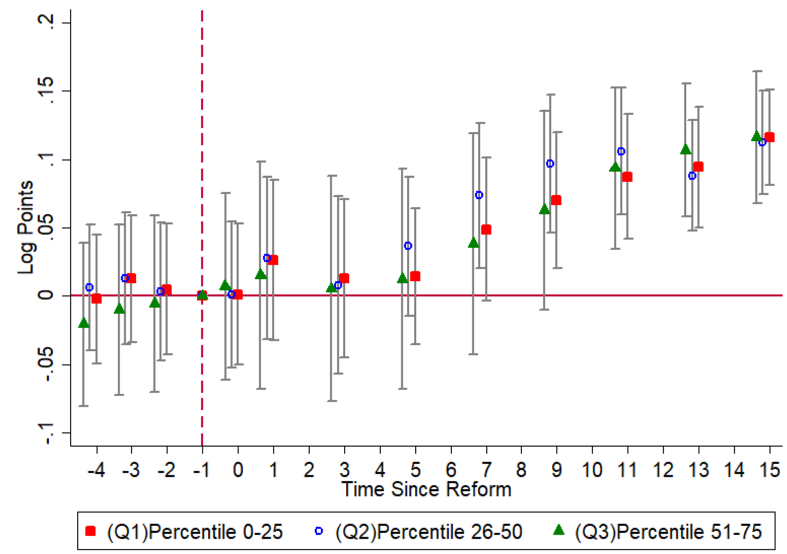

Notes: Each panel contains an event-study graph demonstrating the effect of finance reform timing on the outcomes of interest. Panel (a) is the first-stage, where the outcome is ln(current spending/pupil). Panel (b) is the reduced form, where the outcome is ln(house price index). Of interest are a set of indicator variables that are equal to one for districts in a reform state $T$ years relative to the reform year, interacted with indicators for the the 1972 per-pupil spending quartile. The reference group are school districts in the top quartile of historical total spending along with districts in non-reform states. Additional controls include policy controls for the concurrent rollout of healthcare and social service programs, 1960 county characteristics interacted with linear time trends, along with district and year fixed effects. 95% confidence intervals are depicted. Endpoints ar

Figure 8: Predicted Local Property Tax Revenues and House Prices using Tax Instruments

(a) Ln(Tax Revenue/Pupil)




(b) Ln(House Price Index)




Notes: Each panel contains an event-study graph demonstrating the effect of finance reform timing on the outcomes of interest. Panel (a) is the first-stage, where the outcome is ln(property tax revenue/pupil). Panel (b) is the reduced form, where the outcome is ln(house price index). Of interest are a set of indicator variables that are equal to one for districts in a reform state $T$ years relative to the reform year, interacted with indicators for the the 1972 per-pupil property tax revenue quartile. The reference group are school districts in the top quartile of historical property tax revenues along with districts in non-reform states. See Figure 7 for a complete set of controls. 95% confidence intervals are depicted.

Figure 8 shows the dynamics of local property tax revenues and house prices following a school finance reform, separating districts in this case into quartiles based on their initial local tax revenue per pupil in 1972. As the figure makes clear, local property tax revenue increased in districts with relatively low vs. high initial levels of local tax revenue. In particular, local tax revenue increased especially slowly in $Q_\tau = 2$ districts relative to those in both $Q_\tau = 1$ and $Q_\tau = 3$. The right-hand panel of Figure 8 plots the reduced form estimates for house prices, analogous to the spending event study figure shown in the left-hand panel. This house price event study is based on a district's local tax revenue per pupil in 1972. Interestingly, the house price figure in this case also exhibits some non-monotonicity based on the initial local tax revenue quartile, with $Q_\tau = 2$ districts, which had relatively low property tax increases, experiencing the greatest house price gains in many of the post-reform periods.

# 4 Results

We present three sets of results. In the first set of results, we estimate the elasticity of house prices with respect to current spending by regressing the log of the local house price index on current spending instrumenting for current spending using only the spending instruments. In the second set of results, we estimate the elasticity of house prices with respect to local property taxes by regressing the log of house index on local property taxes, instrumenting for taxes using only the tax instruments. In the third set of results, we jointly estimate the elasticity of house prices with respect to current spending and taxes by regressing the log of house price index on both current spending and local property taxes using both the spending and tax instruments. We use the jointly estimated spending and tax elasticities to calculate the impact of a 1% tax funded increase in current spending on house prices, and to test that our instrumental variables satisfy the exclusion restriction.

## 4.1 The Capitalization of School Spending into House Prices

Table 4 reports the results of regressions of housing prices on school current spending using the school finance reform instruments based on a district's initial spending quartile. In column (1) we include school district and calendar year fixed effects. In column (2) we add time trends interacted with 1960 Census levels of log population, poverty rate, the fraction of non-white residents, and the fraction of residents in rural/non-farm areas, measured at the county level, following Jackson et al. (2016). In column (3), we add a series of policy controls that measure the time since a state adopted or first funded Head Start, Kindergarten, School Desegregation, Hospital Desegregation, and first certified Medicare.[24] The final column adds controls for the coverage of the FHFA house price index, specifically the fraction of the population within a school district that lives in a Census tract for which an FHFA index is available in a given year.

Table 4: The House Price Capitalization of Current Spending

| Outcome: Log(HPI) | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Log(Current Spending) | 1.035*** | 1.235*** | 1.277*** | 1.188*** |
| | (0.243) | (0.246) | (0.234) | (0.228) |
| Observations | 123,554 | 123,554 | 123,554 | 123,554 |
| First-Stage F-stat | 13.98 | 13.86 | 11.16 | 10.93 |
| District FE | ✓ | ✓ | ✓ | ✓ |
| Census Controls | | ✓ | ✓ | ✓ |
| Policy Controls | | | ✓ | ✓ |
| Data Coverage | | | | ✓ |

Notes: Current spending is spending on salaries and all other costs excluding capital investments and construction costs. In all models we instrument for endogenous current spending with the 1972 total spending quartile interacted with event-time shocks from school finance reforms. Policy controls included the timing of state adoption of Head Start, Kindergarten, School Desegregation, Hospital Desegregation, and Medicare certification. Data coverage is calculated as the share of total district population living in a census tract with house price data available and outlined in Section 2. Standard errors are clustered at the district level.

[24] These controls are intended to absorb any changes in house prices that may be due to these other policy changes rather than school finance reforms (Jackson et al., 2016).

The results are qualitatively similar across the four columns, yielding an elasticity of house prices with respect to school salary spending ranging from 1.035 to 1.277. The result in the final column implies that a 1 percent increase in current spending per-pupil leads to a 1.18 percent increase in property values. The magnitude of the point estimates in Table 4 imply that households are willing to pay substantially more for access to better-funded schools. The size of these estimates is consistent with the substantial effects of increased school spending on children's life outcomes documented in Jackson et al. (2016), who find that a 10 percent increase in school spending over 12 years, on average, increases high school graduation rates by 9.5 percent and adult income by 7.3 percent — implied elasticities of 0.95 and 0.73, respectively. Jackson et al. (2016) estimate even larger impacts of a 10 percent increase in school spending on the education and labor market outcomes of children from low-income families — a 11.6 percent increase in high school graduate rates and 9.5 percent increase in adult wages. Taken together with these studies, our work provides revealed-preference evidence that households value the impact of additional school spending on the lives of their children.

## 4.2 The Capitalization of Property Tax Revenues into House Prices

Table 5 presents the results of regressions of housing prices on property tax revenue using only the school finance reform instruments based on a district's initial property tax quartile in 1972. The specifications are analogous to those presented in Table 4, with each column successively including for additional control variables. As expected, local property taxes enter negatively in all of the specifications, implying an elasticity of house prices with respect to property tax revenues ranging from -0.07 to -0.23. The result in the final column implies that a 1 percent increase in per-pupil property tax revenue leads to a 0.23 percent decrease in property values.

Table 5: The House Price Capitalization of Property Tax Revenues

| Outcome: Log(HPI) | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Log(Property Tax Revenue) | -0.0698 | -0.0979* | -0.223*** | -0.225*** |
| | (0.0574) | (0.0497) | (0.0474) | (0.0476) |
| Observations | 123,554 | 123,554 | 123,554 | 123,554 |
| First-Stage F-stat | 12.37 | 11.04 | 10.96 | 10.96 |
| District FE | ✓ | ✓ | ✓ | ✓ |
| Census Controls | | ✓ | ✓ | ✓ |
| Policy Controls | | | ✓ | ✓ |
| Data Coverage | | | | ✓ |

Notes: Property tax revenues are measured at the district level and do not include any other local tax revenues. In all models we instrument for endogenous property-tax revenue with the 1972 tax revenue quartile interacted with event-time shocks from school finance reforms. See Table 4 for a complete description of the various additional controls. Standard errors are clustered at the district level.

### 4.3 The Efficiency Test using both Tax and Spend Instruments

In Table 6 we report the results of our regression of the log of the house price index on the log of current spending per pupil and the log of local property taxes per pupil instrumenting for current spending and property taxes with the spending and tax instruments. We find that the elasticity of house prices with respect to current spending ranges from 1.04 to 1.25 —similar to what we found in Table 4 (1.04 to 1.28) when taxes were omitted from the regressions, and comparable to the elasticity of adult wages and high school graduation rates with respect to school spending in Jackson et al. (2016). The elasticity of house prices with respect to taxes ranges from -0.08 to -0.18 — similar to what we found in Table 5 (-0.07 to -0.23) when spending was omitted.

If our spending and tax instruments violated the exclusion restriction, we would have expected our estimate elasticity of house prices with respect to spending (taxes) when we excluded one of the endogenous regressors, as we did in Table 4 (Table 5), to be substantially different from the results in Table 6 — indicating omitted variable bias. The opposite is true. A strength of our research design and the new tax instruments that we introduce is that we can perform this indirect test of the exclusion restriction of our instrumental variables strategy. The results of this test suggest that both instruments satisfy the exclusion restriction. Moreover, the first stage F-statistics for the spending and the tax instruments are 95 and 47, respectively, suggesting that both sets of instruments also satisfy the relevance condition, obviating any concern that our instruments are weak (Andrews et al., 2019).[25]

Table 6: IV Estimates of Current Spending Efficiency

| Outcome: Log(HPI) | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Log(Current Spending) | 1.044*** | 1.244*** | 1.245*** | 1.172*** |
| | (0.204) | (0.210) | (0.191) | (0.187) |
| Log(Property Tax) | -0.0838* | -0.0995** | -0.183*** | -0.183*** |
| | (0.0506) | (0.0478) | (0.0476) | (0.0469) |
| **Efficiency Test:** | | | | |
| Δ HPI | 0.376*** | 0.448*** | 0.365*** | 0.333*** |
| 95% Confidence Interval | [0.184, 0.567] | [0.240, 0.656] | [0.162, 0.566] | [0.136, 0.531] |
| Observations | 123,554 | 123,554 | 123,554 | 123,554 |
| F-stat: Spending IVs | 396.4 | 115.2 | 89.32 | 95.26 |
| F-stat: Tax IVs | 124.8 | 46.82 | 50.81 | 47.14 |
| District FE | ✓ | ✓ | ✓ | ✓ |
| Census Controls | | ✓ | ✓ | ✓ |
| Policy Controls | | | ✓ | ✓ |
| Data Coverage | | | | ✓ |

Notes: Current spending is spending on salaries and all other costs excluding capital investments and construction costs. Property tax revenues are measured at the district level and do not include any other local tax revenues. In all models, we instrument for endogenous current spending and property tax revenue per-pupil with both initial spending and initial tax quartile in 1972, interacted with the event-time shocks from school finance reforms. Standard errors are clustered at the district level.

The lower panel of Table 6 reports the results of the Brueckner-Oates efficiency test calculated for an increase of 1% in property taxes used to increase current spending. We find that a 1% increase in taxes used to increase current school expenditures would increase house prices by 0.38%-0.45%, which is statistically significant at the 1% level. The results

[25]Moreover, our t-stats on spending and taxes from Table 6 of 6.2 and -3.9, pass the thresholds for valid inference using the corrected procedure in Lee et al. (2022) of 1.98 and -2.17, respectively.

imply that school spending is inefficiently too low. As Hoxby and Kuziemko (2004) point out both theoretically and empirically using school finance reforms in Texas, school finance systems in a number of states create distortions that can lead to inefficiently low spending and a substantial loss in property values.

# 5 Robustness Checks

In this section we examine the sensitivity of our main efficiency results to concerns about: heterogeneity in local average treatment effects that we estimate, the forward-looking nature of house prices with respect to future school spending and taxes, household sorting, an alternative way of clustering our standard errors, including distinct time trends for each pre-reform tax and spend indicator, restricting our sample to a balanced panel of observations, using only the later post-1990 school finance reforms, and specifying our model in terms of the dollar value of housing instead of a house price index. Our results are quantitatively and qualitatively similar when we implement this list of robustness checks.

## 5.1 Identification from Different Local Sources of Variation in the Data

In using variation across both time and the four quartiles of 1972 school spending and local tax revenue levels, the point estimates and efficiency tests reported above implicitly assume that the capitalization of school spending and local property taxes is homogeneous. One potential concern is that this homogeneity assumption may mask variation in efficiency in different types of districts, e.g., housing prices in certain areas are much more sensitive to school spending while those in other areas are more sensitive to the local property tax burden.[26] In Table 7 we test the homogeneity assumption by reporting the results of specifications that estimate different LATEs (Imbens and Angrist, 1994).

[26] We are grateful to John Friedman for fruitful conversations and suggestions on this subtle point.

Table 7: Test of how sensitive our estimates are to different sources of tax and spending variation

| Varying: | Main | Spend Q1 v Q2-4 | Spend Q1-2 v Q3-4 | Spend Q1-3 v Q4 | Tax Q1 v Q2-4 | Tax Q1-2 v Q3-4 | Tax Q1-3 v Q4 |
|---|---|---|---|---|---|---|---|
| Log(Current Spending) | 1.172*** | 1.105*** | 1.118*** | 1.233*** | 1.201*** | 1.206*** | 1.204*** |
| | (0.187) | (0.169) | (0.170) | (0.191) | (0.206) | (0.196) | (0.212) |
| Log(Property Tax) | -0.183*** | -0.223*** | -0.211*** | -0.188*** | -0.203*** | -0.195*** | -0.184*** |
| | (0.0469) | (0.0457) | (0.0444) | (0.0491) | (0.0476) | (0.0447) | (0.0493) |
| **Efficiency Test:** | | | | | | | |
| $\Delta$ HPI | 0.333*** | 0.264*** | 0.281*** | 0.355*** | 0.325*** | 0.335*** | 0.346*** |
| 95% Confidence Interval | [0.136, 0.531] | [0.092, 0.436] | [0.109, 0.454] | [0.149, 0.560] | [0.110, 0.540] | [0.128, 0.543] | [0.132, 0.560] |
| Observations | 123,978 | 123,978 | 123,978 | 123,978 | 123,978 | 123,978 | 123,978 |
| First-Stage (F) Spending | 95.26 | 110.7 | 116.3 | 143.4 | 114.4 | 111.6 | 123.7 |
| First-Stage (F) Taxes | 47.14 | 65.51 | 57.97 | 61.88 | 61.11 | 59.00 | 59.05 |
| Complete Set of Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |

Notes: Current spending is spending on salaries and all other costs excluding capital investments and construction costs. Property tax revenues are measured at the district level and do not include any other local tax revenues. In all models we instrument for endogenous current spending and property tax revenue per-pupil with both initial spending and initial tax quartile in 1972, interacted with the event-time shocks from school finance reforms. standard errors are clustered at the district level.

We restrict the variation used to identify the model by splitting districts into only two (rather than four) groups based on initial quartile of school spending and local tax revenue. In particular, the first column of the table repeats our baseline results (column (4) in Table 6). In columns (2) through (4) we hold the property tax quartiles fixed and explore variation across pre-reform spending quartiles. In columns five through seven we hold spending quartiles fixed and vary the pre-reform tax quartiles. There is little change in the coefficient estimates across the three specifications reported to the right of column one. The point estimates on both current spending and taxes change somewhat as the source of variation shifts to a higher percentile of the initial spending distribution, but these estimates are statistically indistinguishable from one another across the specifications shown in Table 7. Moreover, the efficiency tests are also similar across specifications, with point estimates ranging from 0.264 to 0.355 — and all statistically significant at the 1% level. The results suggest that current expenditure is inefficiently low.

## 5.2 Can Households Anticipate Future Spending Changes?

Another issue that naturally arises in estimating house price regressions is whether house prices might reflect future expectations about trends in school spending in addition to current levels of school spending. While a full-fledged dynamic model is beyond the scope of this paper, one way to see whether these types of forward-looking expectations might have a significant impact on our analysis is to estimate a set of analogous specifications that include leads in the right-hand side variables, especially the spending and tax measures. To that end, the second columns of Table 8 replace all of the right hand side variables (including controls) with their one year ahead leads. Because we are unable to include the final year of the sample in the specification, column (1) re-estimates our baseline specification dropping observations from the year 2015.

Including leads has almost no impact on the coefficients and the resulting Brueckner-Oates test results do not change in the leads specification. Thus, it does not appear that ignoring forward-looking behavior is a first-order concern for our main analysis. If anything, adjusting for the impact of future spending and taxes leads to larger increases in house prices from a tax-funded increase in current spending.

Table 8: Robustness Test for Anticipatory Responses

| Outcome: Log(HPI) | (1) | (2) |
|---|---|---|
| Log(Current Spending) | 1.223*** | 1.441*** |
| | (0.195) | (0.207) |
| Log(Property Tax) | -0.177*** | -0.196*** |
| | (0.0466) | (0.0490) |
| **Efficiency Test:** | | |
| $\Delta$ HPI | 0.36*** | 0.438*** |
| 95% Confidence Interval | [0.157, 0.565] | [0.223, 0.654] |
| Observations | 121,191 | 121,191 |
| First-Stage F-stat: Spending IV | 71.66 | 71.66 |
| First-Stage F-stat: Tax IVs | 61.33 | 61.33 |
| Complete Set of Controls | ✓ | ✓ |
| Independent Variable is a Lead | | ✓ |

Notes: Current spending is spending on salaries and all other costs excluding capital investments and construction costs. Property tax revenues are measured at the district level and do not include any other local tax revenues. In all models we instrument for endogenous current spending and property tax revenue per-pupil with both initial spending and initial tax quartile in 1972, interacted with the event-time shocks from school finance reforms. standard errors are clustered at the district level.

## 5.3 The Direct vs. Indirect Capitalization of School Spending

The increase in house prices that accompanies an exogenous increase in school spending naturally affects who can afford and who is willing to pay to live in a school district. Thus, as an important extension of our main capitalization results, we now investigate the impact of current spending on sorting across districts, focusing on the fraction of children in poverty in a school district as a summary measure of sorting. The US Census Bureau Small Area Income and Poverty Estimates (SAIPE) program produces annual estimates of income and poverty for all states and counties, as well as estimates of school-age children in poverty for all US school districts.[27]

We begin by looking directly at the effects of school spending and property taxes on sorting, by estimating analogous specifications to a number of those reported in Tables 4 and 6 but with the fraction of children in poverty as the dependent variable. The pattern of results shown in Table 9 is remarkably consistent with the house price regressions. The first column reports the results of a specification analogous to the fourth column of Table 4, reporting a 1% increase in current spending is associated with a 0.25% decrease in the school poverty rate. This effect remains largely unchanged when we control for local property taxes in column two, a specification analogous to that reported in the fourth column of Table 6. Our finding that household sorting is driven by current expenditures is consistent with the result that house prices are quite responsive to increases in school spending.

Table 9: Income Sorting Response to Current Spending and Taxes

| Outcome: Local Youth Poverty Rates | (1) | (2) |
|---|---|---|
| Log(Current Spending) | -0.252*** | -0.216*** |
| | (0.0641) | (0.0530) |
| Log(Property Tax) | | 0.00480 |
| | | (0.0108) |
| Observations | 114,038 | 114,038 |
| First-Stage F-stat: Spending IVs | 12.43 | 61.17 |
| First-Stage F-stat: Tax IVs | - | 56.93 |
| Complete Set of Controls | ✓ | ✓ |

Notes: Local youth poverty rates are Small Area Income Poverty Estimates from the US Census bureau and cover all households within the school district boundary. Current spending is spending on salaries and all other costs excluding capital investments and construction costs. Property tax revenues are measured at the district level and do not include any other local tax revenues. In all models we instrument for endogenous current spending and property tax revenue per-pupil with both initial spending and initial tax quartile in 1972, interacted with the event-time shocks from school finance reforms. standard errors are clustered at the district level.

[27]SAIPE school district poverty estimates are available here: https://www.census.gov/programs-surveys/saipe/data/datasets.html

That exogenous increases in school spending decrease the fraction of children in poverty within a district suggests that the house price effects documented above likely combine a direct effect of school spending and an indirect effect that results from the changing socioeconomic composition of the school district. To separate these components, Table 10 repeats the earlier house price specifications reported in Table 6 with additional controls for the fraction of children in poverty in the school district. Because measures of school district socioeconomic composition are only available beginning in 1993, the second column of Table 10 re-estimates our baseline specification from column (4) of Table 6 for a sample that begins in 1993. The coefficient on school spending is significantly greater in this sub-sample perhaps because the early 1990s included an economic recession. The third column of Table 10 controls for the fraction of children in poverty.

Table 10: Indirect Capitalization of Current Spending

| Outcome: Log(HPI) | (1) | (2) | (3) |
|---|---|---|---|
| Log(Current Spending) | 1.172*** | 2.130*** | 1.551*** |
| | (0.187) | (0.309) | (0.231) |
| Log(Property Tax) | -0.183*** | -0.191*** | -0.264*** |
| | (0.0469) | (0.0506) | (0.0398) |
| SAIPE Pct. Poverty, 5-17 yr olds | | | -1.521*** |
| | | | (0.111) |
| **Efficiency Test:** | | | |
| Δ HPI | 0.333*** | 0.746*** | 0.419*** |
| 95% Confidence Interval | [0.136, 0.531] | [0.423, 1.069] | [0.156, 0.652] |
| Observations | 123,978 | 114,048 | 114,038 |
| First-Stage F-stat: Spending IVs | 95.26 | 61.18 | 65.24 |
| First-Stage F-stat: Tax IVs | 47.14 | 56.92 | 51.13 |
| Complete Set of Controls | ✓ | ✓ | ✓ |
| Consistent Sample Years | | ✓ | ✓ |

Notes: Current spending is spending on salaries and all other costs excluding capital investments and construction costs. Property tax revenues are measured at the district level and do not include any other local tax revenues. Local youth poverty rates are Small Area Income Poverty Estimates from the US Census bureau and cover all households within the school district boundary. In all models we instrument for endogenous current spending and property tax revenue per-pupil with both initial spending and initial tax quartile in 1972, interacted with the event-time shocks from school finance reforms. standard errors are clustered at the district level.

Comparing columns (2) and (3) shows that inclusion of controls for demographic and socioeconomic composition, the majority of the capitalization of school salary spending into house prices is a direct effect of spending (73%), while a smaller fraction appears to be due to the sorting that occurs following the spending change (27%).[28] The efficiency test continues to imply that current spending is inefficiently too low, with point estimates of 0.42% efficiency elasticity of current spending and the corresponding p-values remaining below 0.001 levels, which is in the 95% confidence interval of the main results from Table 6, i.e. [0.136, 0.531].

## 5.4 Other Robustness Test

In Table 11, we test whether our main results are robust to clustering more broadly than the district level, using linear time trends in the pre-reform tax and spend quartiles, restricting our sample to a balanced panel and to leveraging only the post-1990 reforms. In the first set of results, we cluster our standard errors at the pre-reform spend $\times$ pre-reform tax $\times$ state $\times$ year level, following a design-based clustering logic (Abadie et al., 2023). In the second set of results, we include linear time trends in the pre-reform tax and spend quartiles. In the third set of results, we use a balanced panel of observations. In the fourth set of results, we test whether our results are the same if we use only the later post-1990 school finance reforms.[29] In each case, we use our fully specified model with all controls including the control variables that we used to account for changes in the income distribution within the school to account for the impact of sorting.

Across the four empirical models, we estimate similar elasticities of house prices with respect to current spending and taxes. Moreover, we find that a 1% increase in local property taxes used to increase current expenditure on education would increase house

[28] Accounting for sorting, we find that household are 38% more sensitive to local property taxes than we otherwise estimated.

[29] We estimate the preferred regression using the reform year coding from Lafortune et al. (2018), utilizing 1990 spending and property tax revenue quartiles to construct instruments as a way of focusing on the post-1990 school finance reforms.

prices by a statistically significant amount that ranges from 0.2% to 0.42%. The range of results from the four additional robustness checks are therefore consistent with our preferred estimate of 0.33% from Table 6, supporting the conclusion that school spending, on average, is inefficiently low.

Table 11: Additional Robustness Test

| Outcome: Log(HPI) | Clustering | Quartile Trends | Balanced Panel | Later Reforms |
|---|---|---|---|---|
| Log(Current Spending) | 1.551*** | 1.182*** | 1.285*** | 1.448*** |
| | (0.222) | (0.185) | (0.200) | (0.148) |
| Log(Property Tax) | -0.264** | -0.319*** | -0.240*** | -0.260*** |
| | (0.0938) | (0.0432) | (0.0384) | (0.0308) |
| **Efficiency Test:** | | | | |
| Δ HPI | 0.419*** | 0.201** | 0.325*** | 0.377*** |
| 95% Confidence Interval | [0.150, 0.688] | [0.011, 0.402] | [0.118, 0.533] | [0.239, 0.515] |
| Observations | 123,978 | 114,038 | 81,234 | 115,372 |
| Complete Set of Controls | ✓ | ✓ | ✓ | ✓ |
| Sorting Controls | ✓ | ✓ | ✓ | ✓ |

Notes: Current spending is spending on salaries and all other costs excluding capital investments and construction costs. Property tax revenues are measured at the district level and do not include any other local tax revenues. Local youth poverty rates are Small Area Income Poverty Estimates from the US Census bureau and cover all households within the school district boundary. In all models, except the "Later Reforms," we instrument for endogenous current spending and property tax revenue per-pupil with both initial spending and initial tax quartile in 1972, interacted with the event-time shocks from school finance reforms.

## 5.5 The Efficiency Test in units of dollars

In our main model we regress the log of the house price index on the log of the spending and the log of taxes. The virtue of the log-log specification is that our estimated parameters are reduced-form elasticities with a natural interpretation. We complement the approach of estimating log-log specifications by estimating a linear-log model in which we convert the house price index into a dollar value amount for one month's rent and regress it on the log of per pupil per capita current expenditure and the log of per pupil local revenues from property taxes — instrumenting current spending and property taxes using the same instrumental variables strategy as before.[30] In this model, a one percent change in spending and taxes generates a unit change in our house price that is proportion to the estimated coefficient.

Estimates for the rent cost response to dollar-valued changes in current spending and taxes are presented in the Appendix (Table B.6.)[31] We find that a 1% increase in local property taxes (equal to $48.71 in 2015) used to fund an increase in current expenditure would increase monthly rents by $2.17. Consider the following conceptual exercise. To close the gap in current spending between the average school district in the first and fourth quartile of school spending in 1990 (2015) would require an increase in current expenditure $1,563 ($2,136) per-pupil, which would require a 68% (64.7%) increase in local property taxes revenue for the quartile 1 districts. Based on the results of our efficiency test, such a policy would increase monthly rental costs by approximately approximately $148 ($140), or 15.6% (13.9%) in 1990 (2015).

# 6 Why Might School Spending be inefficiently low?

According to standard micro-economic theory, a natural reason why school spending could be inefficiently low is that the housing supply is slow to respond to changes in school inputs. We test this explanation by implementing our standard research design and efficiency test in the sample of school districts where housing supply is above and below the national median, according to the housing supply elasticities estimated in Saiz (2010). In Column (1) and (2) of Table 12, we report the results of our test.

[30] In the appendix we detail the process of converting the house price index into a dollar amount.

[31] The columns of Table B.6 vary based on the rent cost conversion using a national, metropolitan area, or local price-to-rent ratio.

Table 12: IV Estimates By Geography

| | < Median Supply Elasticity | ≥ Median Supply Elasticity | Urban | Suburban | Rural |
|---|---|---|---|---|---|
| Log(Current Spending) | 1.950*** | 0.362*** | 1.461*** | 1.234*** | 0.503*** |
| | (0.291) | (0.0912) | (0.374) | (0.169) | (0.108) |
| Log(Property Tax) | -0.151*** | -0.152*** | -0.131* | -0.359*** | -0.218*** |
| | (0.0465) | (0.0486) | (0.0732) | (0.0387) | (0.0316) |
| **Efficiency Test:** | | | | | |
| Δ HPI | 0.707*** | 0.007 | 0.512** | 0.184** | 0.003 |
| Confidence Interval | [0.45, 0.96] | [-0.13, 0.15] | [0.13, 0.89] | [0.01, 0.36] | [-0.12, 0.13] |
| Observations | 51,319 | 56,381 | 11,670 | 48,971 | 53,335 |
| All Controls | ✓ | ✓ | ✓ | ✓ | ✓ |

Notes: Current spending is spending on salaries and all other costs excluding capital investments and construction costs. Property tax revenues are measured at the district level and do not include any other local tax revenues. In all models we instrument for endogenous current spending and property tax revenue per-pupil with both initial spending and initial tax quartile in 1972, interacted with the event-time shocks from school finance reforms. standard errors are clustered at the district level.

Consistent with the theoretical prediction we find evidence that a 1% tax-funded increase in school spending would increase house prices by a statistically significant 0.71% in the sample of school districts where the elasticity of housing supply is below the national median, but have tiny and statistically insignificant impact of 0.01% on house prices in the sample of districts where the elasticity of housing supply is above the national median. Furthermore, consistent with theory, the difference in the efficiency estimates comes from large differences in how current spending is capitalized into house prices. Importantly, we estimate similar elasticities of house prices with respect to property tax revenues in both below-median and above-median districts (-0.151 versus -0.152), which suggests that differences in preferences over taxes, holding the level of school spending fixed are not driving differences in the efficiency estimates across geography. The results of our efficiency test implemented separately for districts in urban, suburban, and rural areas, as reported in Column (3)-(5) of Table 12 reinforce the finding that school spending is inefficiently low in places where housing supply is least responsive (urban and suburban areas) and occurring at the efficient level in places where housing supply is elastic.

# 7 Conclusion

National expenditure on public goods both in the US and abroad constitute a large share of national GDP. Moreover, determining the efficient level of provision is difficult precisely because public goods require the elicitation of individuals' willingness to pay in a context where free-riding is endemic. For these reasons, the efficient provision of public goods is a fundamental question that has has inspired work in the economics profession for nearly a century (Musgrave 1939; Samuelson 1954; Tiebout 1956; Oates 1969; Barlow 1970; Brueckner 1979; Yinger 1982; Bagnoli and Lipman 1992; Barrow and Rouse 2004; Cellini et al. 2010; Nguyen-Hoang and Yinger 2011).

Harkening to a classic literature, we show that even in a model with households with heterogeneous preferences that the theoretical insights of Brueckner (1979) and Brueckner (1982), foreshadowed presciently by Oates (1969), yields an equivalency between the Samuelson (first order) condition for efficient public good provision and utility equalization in spatial equilibrium. This equivalence permits us to use the housing market to test for the efficiency of current expenditure on K-12 education, without relying on the strong assumptions inherent in the Tiebout (1956) model. The theory also provides clear guidance for how to use aggregate data on quality-adjusted local house prices and the natural occurring variation in school spending on salaries and tax revenues used to fund schools to test whether current expenditure on schools is occuring at the efficient level. In addition to providing independent variation in salary spending and local taxation, a key advantage of our empirical design is that the resulting estimates are based on a national sample of school districts rather than a single state or metropolitan area, which is common in the literature given how challenging it is to credibly test for efficiency.

We find that house prices are sharply increasing in current spending and decreasing in local property taxes. Our results indicate that a dollar raised through local taxes and spent on current expenditure has a positive and statistically significant impact on local house prices, which implies that school spending on salaries in the U.S. is inefficiently low. Importantly, our analysis uses identifying variation that arises because of changes in school district spending on personnel following school finance reforms. Thus, while there may be ways for school districts to spend money more efficiently than they currently do, our results provide strong evidence that when given more resources, the additional money that school districts spend on personnel sharply increases house prices, even net of taxes and, moreover, without requiring additional incentives to spend money more efficiently. In this sense, the effect of increased current spending measured in our paper is potentially a lower bound.

The results of our market-based test for current spending in US public schools is consistent with the evidence from the teacher value-add literature and the school finance literature. These two literatures produce credible, consistent evidence of the positive impact of both high value-add teachers (Chetty et al. (2014a,b)) and increased school spending on educational attainment (Loeb and Page, 2000; Card and Payne, 2002; Lafortune and Schönholzer, 2022; Brunner et al., 2020; Baron, 2022; Jackson and Mackevicius, 2021), inter-generational mobility Biasi (2023) and other long-run labor market outcomes (Jackson et al., 2016).

Both a national and international comparison of teacher pay in the U.S. is suggestive of why the efficiency gains from greater teacher pay are potentially so large. Real average wages for teachers in the U.S. have not increased since 1990 – if anything they have slightly decreased from $59,116 in 1990 to $58,136 in 2017 (National Center for Education Statistics 2019a). During this time period, real median income increased from $52,008 to $57,423 and real expenditures per pupil increased from $9,741 to $13,634 (US Census Bureau 2021; National Center for Education Statistics 2019b). Moreover, compared to similarly credentialed workers in the U.S., teachers experience a 22% pay gap, the second largest among 23 peer countries (Hanushek et al., 2019). Another lever available to policymakers to reduce the level of inefficiency in current expenditure is to reduce the restrictions on housing supply.

Finally, it is important to point out that the analysis of the provision of public school spending in this paper on the efficiency of current spending levels is potentially a conservative lower bound because it takes into account only the private returns to households and their children. Agrawal et al. (2022) provides evidence that our efficiency estimates are indeed a lower bound by using our estimates and the estimates in Jackson et al. (2016) to quantify the social value of public funds derived from increased school spending. Any broader social and civic returns to education as well as concerns about the equitable provision of educational opportunities could further raise the value of increased spending on school personnel, especially in relatively poor and low-spending districts (Loeb and Page, 2000; Johnson and Nazaryan, 2019).

# References


ABADIE, A., S. ATHEY, G. W. IMBENS, AND J. M. WOOLDRIDGE (2023): "When Should You Adjust Standard Errors for Clustering?" *The Quarterly Journal of Economics*, 138, 1–35.

AGRAWAL, D. R., W. H. HOYT, AND T. LY (2022): "A New Approach to Evaluating the Welfare Effects of Decentralized Policies," Available at SSRN: https://ssrn.com/abstract=4009872.

ANDREWS, I., J. H. STOCK, AND L. SUN (2019): "Weak Instruments in Instrumental Variables Regression: Theory and Practice," *Annual Review of Economics*, 11, 727–753.

BAGNOLI, M. AND B. L. LIPMAN (1992): "Private provision of public goods can be efficient," *Public Choice*, 74, 59–78.

BARLOW, R. (1970): "Efficiency Aspects of Local School Finance," *Journal of Political Economy*, 78, 1028–1040.

BARON, E. J. (2022): "School Spending and Student Outcomes: Evidence from Revenue Limit Elections in Wisconsin," *American Economic Journal: Economic Policy*, 14, 1–39.

BARROW, L. AND C. E. ROUSE (2004): "Using market valuation to assess public school spending," *Journal of Public Economics*, 88, 1747–1769.

BAYER, P., P. Q. BLAIR, AND K. WHALEY (2020): "The Impact of School Finance Reforms on Local Tax Revenues," *AEA Papers and Proceedings*, 110, 416–418.

BAYER, P., F. FERREIRA, AND R. MCMILLIAN (2007): "A Unified Framework for Measuring Preferences for Schools and Neighborhoods," *Journal of Political Economy*, 115, 588–638.

BESLEY, T. AND H. MUELLER (2012): "Estimating the Peace Dividend: The impact of violence on house prices in Northern Ireland," *American Economic Review*, 102, 810–33.

BIASI, B. (2023): "School Finance Equalization Increases Intergenerational Mobility," *Journal of Labor Economics*, 41, 1–38.

BLACK, S. (1999): "Do Better Schools Matters? Parental Valuation of Elementary Education," *Quarterly Journal of Economics*, 114, 577–599.

BLACK, S. AND S. MACHIN (2011): "Housing Valuations of School Performance," in *Handbook of the Economics of Education*, Elsevier, vol. 3, 485–519.

BORUSYAK, K., P. HULL, AND X. JARAVEL (2022): "Quasi-Experimental Shift-Share Research Designs," *The Review of Economic Studies*, 89, 181–213.

BRUECKNER, J. (1982): "A test for allocative efficiency in the local public sector," *Journal of Public Economics*, 19, 311–331.

BRUECKNER, J. K. (1979): "Property values, local public expenditure and economic efficiency," *Journal of Public Economics*, 11, 223–245.

BRUNNER, E., J. HYMAN, AND A. JU (2020): "School Finance Reforms, Teachers' Unions, and the Allocation of School Resources," *Review of Economics and Statistics*, 102, 473–489.

CALHOUN, C. A. (1996): "OFHEO House Price Indexes: HPI Technical Description," Office of Federal Housing Enterprise Oversight.

CARD, D. AND A. A. PAYNE (2002): "School finance reform, the distribution of school spending, and the distribution of student test scores," *Journal of Public Economics*, 83, 49–82.

CASE, K. E. AND R. J. SHILLER (1989): "The Efficiency of the Market for Single-Family Homes," *The American Economic Review*, 79, 125–137.

CELLINI, S. R., F. FERREIRA, AND J. ROTHSTEIN (2010): "The Value of School Facility Investments: Evidence from a Dynamic Regression Discontinuity Design," *Quarterly Journal of Economics*, 125, 215–261.

Chakrabarti, R. and J. Roy (2015): "Housing markets and residential segregation: Impacts of the Michigan school finance reform on inter-and intra-district sorting," *Journal of Public Economics*, 122, 110–132.

Chetty, R., J. N. Friedman, and J. E. Rockoff (2014a): "Measuring the impacts of teachers I: Evaluating bias in teacher value-added estimates," *American Economic Review*, 104, 2593–2632.

——— (2014b): "Measuring the impacts of teachers II: Teacher value-added and student outcomes in adulthood," *American Economic Review*, 104, 2633–79.

Cook, J. B. (2018): "The effect of charter competition on unionized district revenues and resource allocation," *Journal of Public Economics*, 158, 48–62.

Correia, S. (2018): "IVREGHDFE: Stata module for extended instrumental variable regressions with multiple levels of fixed effects," Statistical Software.

Currie, J., L. Davis, M. Greenstone, and R. Walker (2015): "Environmental health risks and housing values: evidence from 1,600 toxic plant openings and closings," *American Economic Review*, 105, 678–709.

Dee, T. S. (2000): "The Capitalization of Education Finance Reforms," *The Journal of Law and Economics*, 43.

Diamond, R. and T. McQuade (2019): "Who wants affordable housing in their backyard? An equilibrium analysis of low-income property development," *Journal of Political Economy*, 127, 1063–1117.

Ekins, E. and J. Gygi (2022): "New Poll: 87% of Americans Worry about the Cost of Housing, 76% Say Now Is a Bad Time to Buy a Home," https://www.cato.org/blog/new-poll-87-americans-worry-about-cost-housing-76-say-now-bad-time-buy-home, [Accessed: October 20, 2023].

EPPLE, D. AND M. M. FERREYRA (2008): "School finance reform: Assessing general equilibrium effects," *Journal of Public Economics*, 92, 1326–1351.

FIGLIO, D. N. AND M. E. LUCAS (2004): "What's in a grade? School report cards and the housing market," *American Economic Review*, 94, 591–604.

FINKELSTEIN, A. AND N. HENDREN (2020): "Welfare Analysis Meets Causal Inference," *Journal of Economic Perspectives*, 34, 146–167.

GIBBONS, S., S. MACHIN, AND O. SILVA (2013): "Valuing school quality using boundary discontinuities," *Journal of Urban Economics*, 75, 15–28.

GOLDSMITH-PINKHAM, P., I. SORKIN, AND H. SWIFT (2020): "Bartik Instruments: What, When, Why, and How," *American Economic Review*, 110, 2586–2624.

GONZALEZ-NAVARRO, M. AND C. QUINTANA-DOMEQUE (2016): "Paving streets for the poor: Experimental analysis of infrastructure effects," *Review of Economics and Statistics*, 98, 254–267.

HANUSHEK, E. A. (1989): "The Impact of Differential Expenditures on School Performance," *Educational Researcher*, 18, 45–62.

——— (2003): "The Failure of Input-based Schooling Policies," *The Economic Journal*, 113, F64–F98.

HANUSHEK, E. A., M. PIOPIUNIK, AND S. WIEDERHOLD (2019): "The Value of Smarter Teachers International Evidence on Teacher Cognitive Skills and Student Performance," *Journal of Human Resources*, 54, 857–899.

HENDREN, N. AND B. SPRUNG-KEYSER (2020): "A Unified Welfare Analysis of Government Policies," *Quarterly Journal of Economics*, 135, 1209–1318.

HOUSTON, D. M., P. E. PETERSON, AND M. R. WEST (2022): "Partisan Rifts Widen, Perceptions of School Quality Decline: Results of the 2022 Education Next Survey of Public Opinion," *Education Next*, 23.

HOXBY, C. (2001): "All School Finance Equalizations are Not Created Equal," *Quarterly Journal of Economics*, 116, 1189–123.

HOXBY, C. M. AND I. KUZIEMKO (2004): "Robin Hood and His Not-So-Merry Plan: Capitalization and the Self-Destruction of Texas' School Finance Equalization Plan," NBER Working Papers.

IMBENS, G. W. AND J. D. ANGRIST (1994): "Identification and Estimation of Local Average Treatment Effects," *Econometrica*, 62, 467–475.

IMBERMAN, S. A. AND M. F. LOVENHEIM (2016): "Does the market value value-added? Evidence from housing prices after a public release of school and teacher value-added," *Journal of Urban Economics*, 91, 104–121.

JACKSON, C. K., R. C. JOHNSON, AND C. PERSICO (2016): "The effects of school spending on educational and economic outcomes: Evidence from school finance reforms," *Quarterly Journal of Economics*, 131, 157–218.

JACKSON, C. K. AND C. MACKEVICIUS (2021): "The Distribution of School Spending Impacts," Working Paper 28517, National Bureau of Economic Research.

JOHNSON, R. C. AND A. NAZARYAN (2019): *Children of the Dream: Why School Integration Works*, Basic Books and Russell Sage Foundation Press.

KANE, T. J., S. R. CELLINI, AND D. O. STAIGER (2006): "School Quality, Neighborhoods, and Housing Prices," *American Law and Economics Review*, 8, 183–212.

LAFORTUNE, J., J. ROTHSTEIN, AND D. W. SCHANZENBACH (2018): "School Finance Reform and the Distribution of Student Achievement," *American Economic Journal: Applied Economics*, 10, 1–26.

LAFORTUNE, J. AND D. SCHÖNHOLZER (2022): "The impact of school facility investments on students and homeowners: Evidence from los angeles," *American Economic Journal: Applied Economics*, 14, 254–89.

LEE, D. S., J. MCCRARY, M. J. MOREIRA, AND J. PORTER (2022): "Valid t-Ratio Inference for IV," *American Economic Review*, 112, 3260–3290.

LEVENTIS, A. (2008): "Revisiting the differences between the OFHEO and SP/Case-Shiller house price indexes: new explanations," *Research Paper. Office of Federal Housing Enterprise Oversight*.

LINDEN, L. AND J. E. ROCKOFF (2008): "Estimates of the impact of crime risk on property values from Megan's laws," *American Economic Review*, 98, 1103–27.

LOEB, S. AND J. B. CULLEN (2004): "School finance reform in Michigan: Evaluating Proposal A," in *Helping Children Left Behind: State Aid and the Pursuit of Educational Equity*, ed. by J. Yinger, Cambridge: MIT Press, chap. 7, 215–250.

LOEB, S. AND M. PAGE (2000): "Examining The Link Between Teacher Wages And Student Outcomes: The Importance Of Alternative Labor Market Opportunities And Non-Pecuniary Variation," *The Review of Economics and Statistics*, 82, 393–408.

MARTORELL, P., K. STANGE, AND I. MCFARLIN JR (2016): "Investing in schools: capital spending, facility conditions, and student achievement," *Journal of Public Economics*, 140, 13–29.

MAYSHAR, J. (1990): "On measures of excess burden and their application," *Journal of Public Economics*, 43, 263–289.

MORETTI, E. (2004): "Human capital externalities in cities." *Handbook of Regional and Urban Economics*, 4, 2243–2291.

MUEHLENBACHS, L., E. SPILLER, AND C. TIMMINS (2015): "The housing market impacts of shale gas development," *American Economic Review*, 105, 3633–59.

MURRAY, S. E., W. N. EVANS, AND R. M. SCHWAB (1998): "Education-Finance Reform and the Distribution of Education Resources," *American Economic Review*, 88, 789–812.

MUSGRAVE, R. A. (1939): "The Voluntary Exchange Theory of Public Economy," *Quarterly Journal of Economics*, 53, 213–237.

NATIONAL CENTER FOR EDUCATION STATISTICS (2019a): "Estimated average annual salary of teachers in public elementary and secondary schools, by state: Selected years, 1969-70 through 2016-17," https://nces.ed.gov/programs/digest/d19/tables/dt19_236.55.asp.

——— (2019b): "Total and current expenditures per pupil in public elementary and secondary schools: Selected years, 1919-20 through 2016-17," https://nces.ed.gov/programs/digest/d19/tables/dt19_236.55.asp.

NEILSON, C. A. AND S. D. ZIMMERMAN (2014): "The effect of school construction on test scores, school enrollment, and home prices," *Journal of Public Economics*, 120, 18–31.

NGUYEN-HOANG, P. AND J. YINGER (2011): "The Capitalization of School Quality into House Values: A Review," *Journal of Housing Economics*, 20.

OATES, W. E. (1969): "The Effects of Property Taxes and Local Public Spending on Property Values: An Empirical Study of Tax Capitalization and the Tiebout Hypothesis," *Journal of Political Economy*, 77, 957–971.

PEW RESEARCH CENTER (2022): "Views of the national economy, assessments of prices, jobs, housing and the stock market," Available at https://www.pewresearch.org/politics/2022/01/25/views-of-the-national-economy-assessments-of-prices-jobs-housing-and-the-stock-market/.

POTERBA, J. M. (1996): "Government Intervention in the Markets for Education and Health Care: How and Why?" in *Individual and Social Responsibility: Child Care, Education, Medical Care, and Long-Term Care in America*, ed. by Fuchs.

ROCKOFF, J. E. (2010): "Local response to fiscal incentives in heterogeneous communities," *Journal of Urban Economics*, 68, 138–147.

SAIZ, A. (2010): "The Geographic Determinants of Housing Supply," *Quarterly Journal of Economics*, 125, 1253–1296.

SAMUELSON, P. A. (1954): "The Pure Theory of Public Expenditure," *The Review of Economics and Statistics*, 36, 387–389.

SANDEFUR, J. (2018): "Chart of the Week: Teacher Pay around the World: Beyond "Disruption" and "De-skilling"," *Center for Global Development Blog Post*.

SCHMIDHEINY, K. AND S. SIEGLOCH (2023): "On event studies and distributed-lags in two-way fixed effects models: Identification, equivalence, and generalization," *Journal of Applied Econometrics*, 38, 695–713.

TIEBOUT, C. M. (1956): "A Pure Theory of Local Expenditures," *Journal of Political Economy*, 64, 416–424.

US CENSUS BUREAU (2021): "Real Median Household Income in the United States, retrieved from FRED, Federal Reserve Bank of St. Louis," https://fred.stlouisfed.org/series/MEHOINUSA672N.

YINGER, J. (1982): "Capitalization and the Theory of Local Public Finances," *Journal of Political Economy*, 88, 789–812.

# Online Appendix

## Are We Spending Enough on Teachers in the US?

*Patrick Bayer, Peter Blair, and Kenneth Whaley*

# Appendix A: Data and Measurement, Categorization of School District Finances

The data in our sample cover 42 of the lower 48 states from 1990-2015. North Carolina, Maryland, and Nevada do not comprehensively or consistently report district finances during the sample period and are excluded. Property tax revenues in 1972 are not well reported for Massachusetts districts, invalidating the second of instruments for those data. Washington DC is served by one public school district and is also excluded. Reform states (including those with pre-1990 reforms) are coded to match Jackson et al. (2016) (exluding Michigan). Table A.1 lays out the first-year of reforms in our sample. In Section 7 we present our headline results with reform year coding from Lafortune et al. (2018) and find comparable results to the main analysis.

Table A.1: First Year of Finance Reform as Mandated by State Supreme Courts

| State | Reform Year | State | Reform Year |
|---|---|---|---|
| CA | 1971 | MO | 1993 |
| KS | 1972 | AL | 1993 |
| NJ | 1973 | NH | 1993 |
| WI | 1976 | TN | 1993 |
| WA | 1977 | MA | 1993 |
| CT | 1978 | AZ | 1994 |
| WV | 1979 | MI | 1994 |
| WY | 1980 | VT | 1997 |
| AR | 1983 | OH | 1997 |
| MT | 1989 | ID | 1998 |
| TX | 1989 | NY | 2003 |
| KY | 1989 | SC | 2005 |
| | | OR | 2009 |

The treatment window for the IV design begins in the reform year and ends 16 years post-reform. California, Kansas, and New Jersey are coded into the control group as each state experienced the initial reform prior to 1974 thus will not have sample years that fall within the treatment window. For the second component of the IV, school district finances from 1972 are used to estimate the pre-period state spending distribution and categorize districts into spending and property tax revenue quartiles. The Census of Governments finance data is the sole provider of public data describing district finances as early as 1972, and for consistency is the primary data source for this study.[32]

The primary specification of the paper uses both per-pupil spending and property tax revenues from 1972 as instruments for contemporary school finances. Table 2 shows that spending classification in 1972 is not a strong predictor for property tax classification in 1972.

[32]For the school years ending 1991, 1993, and 1994, an overwhelming number of districts do not report finances to the Census of Governments or the National Center for Education Statistics (NCES). Researchers with the Rutgers University School Funding Fairness project have aggregated school-level finances to district-levels available for the missing years 1991, 1993, and 1994. Source: http://www.schoolfundingfairness.org/data-download

## A1. Categorizing District Finances

Table A.2: School Finance Variables

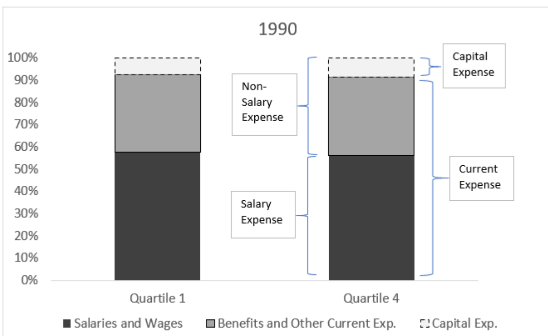
| Variable Name | Definition |
|---|---|
| Total Spending | Total elementary-secondary school district expenditures. |
| Capital Spending | Total expenditures for purchase or maintenance of land, buildings, and other fixed assets. |
| Current Spending | Total spending less capital spending. |
| Salary Spending | Total salaries and wages, not including employee benefit payments. |
| Nonsalary Spending | Total spending less salary spending. |
| Property Tax Revenue | Revenue from property taxes |
| Percent in Poverty | Percent of households in district boundaries living in poverty |

Notes: Table reference for the definitions of relevant school finance variables. Salaries and wages measured in this paper do not include benefit payments.

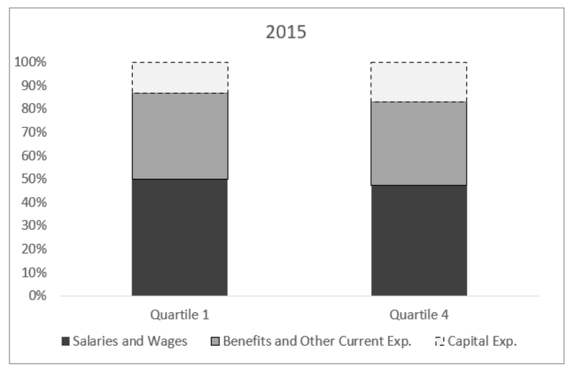
Salaries and wages broadly fall within the broader category of current expenditures, which make up about 92% of total. Other current expenditures include employee benefit payments, spending for educational and student support services, and supplies. The remaining 8% is capital spending on property, construction, and building rehabilitation. Figure A.1 shows the percentage split between current and capital expenditures is remarkably consistent between low and high expenditure districts.

Figure A.1: School District Expenditures Over Time

(a) 1990 Expenditure Classification




(b) 2015 Expenditure Classification




Notes: This diagram shows the allocation of school district expenditures to salary and non-salary expense categories in 1990. Non-salary expenses include both capital and current expenses not including wages and salaries. The composition of spending is remarkably consistent between low-spending (quartile 1) and high-spending (quartile 4) school districts.

We separate salaries and wages from other components of current spending. Employer benefit payments for employee retirement accounts and healthcare (medical, dental, and vision) are not included in our salaries and wages measure. There is available district data for salary spending from 1990-2015. However, NCES did not report detailed salary breakouts until 2000, reporting expenditures for instruction (teachers and assistant teachers), administration (including pupil support services such as counselors), and operations (transportation, food service, maintenance). As a check, we compute the fraction of total salary spending dedicated to the three broad categories for the year 2000 (and 2015): instruction, 74% (73%); administration, 14% (15%); and operations 12% (12%). There are little changes in the salary breakdown over time, with instructional salaries (teachers and teaching assistants) representing the largest share. [33]

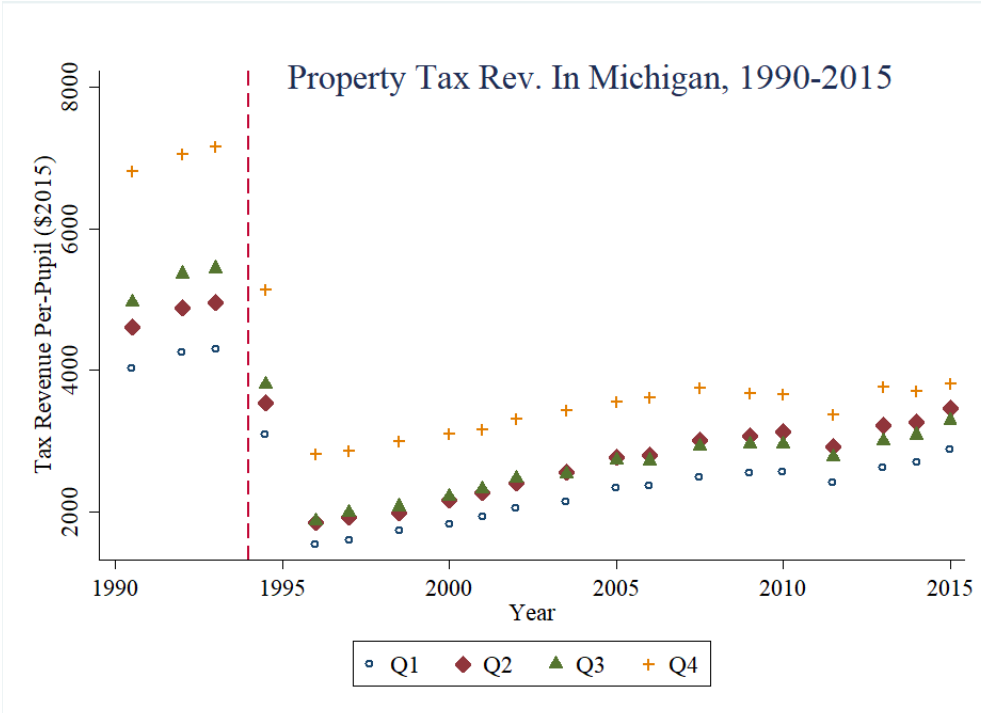
## A3. Property Tax Revenues

On the whole, school districts are largely funded by state governments (47%) and local property taxes (37%), with the remainder coming from the Federal government and other local tax sources. Real property tax revenues per-pupil increased from $3,884 to $5,162 during the sample period but remained within a range of 37%-41% of total spending per-pupil.

Our coding of the Michigan reform date differs from the literature as we use the passage of Proposal A in 1994 as timing of the state reform. The state centralized funding by slashing property tax rates, and hence school revenues from property taxes, while redistributing state revenues in a way that aimed to reduce funding gaps. Figure A.2 shows the decrease in property tax revenues per-pupil. The richest (Q4) districts saw property tax revenues decrease from roughly $7,500 to $3,500 (-$4,000) in the immediate years pre/post 1994, and the poorest (Q1) saw an average decrease from $4,000 to $1,500 (-$2,500). We can compare that to the increase in state taxes for all districts of roughly $2,500 to $7,500 (+$5,000). This implies a net increase of $1,000 per-pupil in Q4 districts and $2,500 per-pupil in Q1 districts generated by the 1994 passages alone. This variation across quartiles in Figure A.2 provides an example of the variation we use to isolate exogenous changes in funding on house prices.

[33] This data is available using from the National Center for Education Statistics. Source:https://nces.ed.gov/ccd/elsi/tablegenerator.aspx

Figure A.2: Coding Michigan Reform Based on Proposal A (1994)




Notes: The 1994 passage of Proposal A in Michigan immediately restructured the funding of schools away from property taxes. Our coding of the reform date in Michigan differs from the literature but follows the implementation of Proposal A.

## A2. Current vs. Capital Spending Over Time

Figure A.3: Spending Over Time by 1972 Spending Quartile


Notes: Time series of per-pupil spending for sample districts from 1990-2015. Each plot shows the trends for total spending, current spending, and capital spending when grouping districts by historic 1972 per-pupil spending quartile. All variables are deflated to 2015 values.

Figure A.4: Spending Over Time by 1972 Tax Quartile


Notes: Time series of per-pupil spending for sample districts from 1990-2015. Each plot shows the trends for total spending, current spending, and capital spending when grouping districts by historic 1972 per-pupil property tax revenue quartile. All variables are deflated to 2015 values.

## A6. House Price Capitalization Literature

There is a broad literature around the house price capitalization of marginal changes to public goods, as well as the internalization of individual and firm behavior within the community. In Table A.3 we describe a small sample of papers studying the house price capitalization of various school quality attributes. An extensive review of this literature is conducted in the Handbook of The Economics of Education, Chapter 10 (Black and Machin 2011). A second strand of literature studies the capitalization of environmental amenities from crime to pollution. Table A.4 summarizes a small sample of the local amenity capitalization literature.

Table A.3: The House Price Capitalization of School Quality

| Article | Setting | Data | Capitalization |
|---|---|---|---|
| Cook (2018) | U.S. - Ohio | Ohio property tax records 1998-2009 | Increased access to charter schools statewide lowered property values and depressed the tax base for traditional public schools. |
| Imberman and Lovenheim (2016) | U.S. - Los Angeles County | 60,000 housing transactions in Los Angeles County from 2009-2011 | Public release of school and teacher value-add ratings have no statistical effect on house prices. There is evidence that test scores are positively capitalized into house prices. |
| Gibbons et al. (2013) | United Kingdom | Price and basic characteristics of all U.K. house sales, linked with schools from 2000-2006. The boundary discontinuity design sample restricts observations to sales within 700 meters of a school zone boundary. | Both school value-add measures and prior student achievement have positive effects on house prices. |
| Black and Machin (2011) | U.S. | - | Handbook chapter reviewing various methodological approaches in the house price capitalization literature prior to 2011. |
| Cellini et al. (2010) | U.S. - California | Sale prices and physical characteristics of California home purchases between 1988-2005. | Willingness to pay for school infrastructure improvements reflected by positive house price capitalization of district capital expenditures. |
| Barrow and Rouse (2004) | U.S. (not including AK, HI, MD, NC, VA, CT, MA, RI, TN | Decennial census home values for 1980 and 1990 aggregated to school district level. | State aid is valued by households on the margin, and on average districts do not overspend on education. Secondary analysis shows potential for overspending in select districts: lower income areas, large districts, and those with low homeownership rates. |
| Figlio and Lucas (2004) | U.S. - Florida | Over 70,000 residential properties near elementary schools in 47 Florida counties. | The release of report cards assigning letter grades to schools significantly increased house prices for schools with an ”A” rating, relative to ”B” and ”C” rated schools. The timing of the report card release explains the sorting of households with high achieving students to ”A” rated schools, above and beyond observed performance of the school on standardized tests. |

Notes: A cross-section of the school quality capitalization studies carried out with robust methods across various localized geographies. For a comprehensive review of the house price capitalization literature see Black and Machin (2011).

Table A.4: The House Price Capitalization of Local Amenities

| Article | Setting | Data | Capitalization |
|---|---|---|---|
| Diamond and McQuade (2019) | U.S. - 15 states | 16 million housing sales within 1.5 miles of 7,098 LIHTC projects, 1987-2012. | LIHTC development increases house prices in low-income areas and reduces prices in upper-income areas. Mechanisms include demographics and crime. |
| Gonzalez-Navarro and Quintana-Domeque (2016) | Mexico - Acayucan | 1,200 dwellings on streets randomly selected to be paved from 2006-2009. | Paving intervention increased home values, boosting household access to credit for the purchase of automobiles, appliances, and home renovations. |
| Muehlenbachs et al. (2015) | U.S. - Pennsylvania | 230,000 property transactions in 36 counties from 1995-2012. | For homes within 2 km of shale development: negative price capitalization when water supply is dependent on groundwater; and small, positive capitalization for homes where water is piped in from outside source. |
| Currie et al. (2015) | US - Texas, New Jersey, Michigan, Florida and Pennsylvania | Housing transactions within 2 miles of a toxic industrial plant opening or closing. 1,600 plants are observed. | The prices of homes within 0.5 and 1 mile bands of a plant opening decrease by 11% relative to those between 1 and 2 miles away. The house price response is stronger in low-income areas. |
| Besley and Mueller (2012) | Ireland - 11 Northern Regions | House price index of 1,000 housing transactions each quarter from 1984-2007. | House prices decrease in response to violent deaths related to paramiltary conflict. The 1993 Peace Process reduced killings substantially. |
| Linden and Rockoff (2008) | U.S. - Charlotte, Mecklenburg County, North Carolina | Tax assessment data for 9,000 home sales within 0.3 miles from the address of a newly register sexual offender. | House prices within a 0.1 mile radius of an offender decrease, reflecting disamenity effects of crime at a localized level. |

Notes: A cross-section of the neighborhood amenity literature carried out with robust methods across various localized geographies. The capitalization of a broad set of amenities studied include crime, infrastructure, environmental pollution, and industrial organization.

# Appendix B. Additional Analysis and Estimation

## B1. IV/2SLS Balanced Panel

Table B.1: IV Estimates : Balanced Panel

| House Price Capitalization of Per-Pupil Expenditures | | |
|---|---|---|
| | (1) | (2) |
| Log(Current Spending) | 1.756*** | 1.285*** |
| | (0.249) | (0.200) |
| Log(Property Tax) | -0.170*** | -0.240*** |
| | (0.0469) | (0.0384) |
| SAIPE Pct. Poverty, 5-17 yr olds | | -1.660*** |
| | | (0.111) |
| **Efficiency: Current Spending** | | |
| Δ HPI | 0.602*** | 0.325*** |
| 95% Confidence Interval | [0.335, 0.869] | [0.118, 0.533] |
| Observations | 81,234 | 81,234 |
| Complete Set of Controls | ✓ | ✓ |

Notes: Current spending is spending on salaries and all other costs excluding capital investments and construction costs. Property tax revenues are measured at the district level and do not include any other local tax revenues. In all models we instrument for endogenous current spending and property tax revenue per-pupil with both initial spending and initial tax quartile in 1972, interacted with the event-time shocks from school finance reforms. Standard errors reported and are clustered at the district level.

## B2. Robustness to using Later Reforms

Table B.2: Capitalization of Current Spending : Post -1990 Reforms

| Outcome: Log(HPI) | (1) | (2) | (3) |
|---|---|---|---|
| Log(Current Spending) | 1.634*** | 1.448*** | 2.211*** |
| | (0.147) | (0.148) | (0.161) |
| Log(Property Tax Revenue) | -0.195** | -0.260*** | -0.312*** |
| | (0.0670) | (0.0308) | (0.0505) |
| **Efficiency: Current Spending** | | | |
| Δ HPI | 0.524*** | 0.377*** | 0.661*** |
| Confidence Interval | [0.301, 0.746] | [0.239, 0.515] | [0.471, 0.850] |
| Observations | 125529 | 115372 | 119791 |
| Complete Set of Controls | ✓ | ✓ | ✓ |
| Poverty Control for Sorting | | ✓ | |
| Dependent Variable is a Lead | | | ✓ |
| Sample Years | 1990-2015 | 1993-2015 | 1991-2015 |

Notes: Standard errors reported and are clustered by district. Current spending is spending on salaries and all other costs excluding capital investments and construction costs. Property tax revenues are measured at the district level and do not include any other local tax revenues. To test whether the main results are driven by event-time coding in the instruments, we estimate the preferred regression using the reform year coding from Lafortune et al. (2018). All models swap utilize 1990 spending and property tax revenue quartiles for the 1972 instruments used in the main analysis of the paper.

Table B.3: IV Estimation with Quartile Specific Time Trends

House Price Capitalization of Per-Pupil Expenditures

| Outcome: Log(HPI) | (1) | (2) | (3) |
|---|---|---|---|
| Log(Current Spending) | 1.175*** | 1.270*** | 1.182*** |
| | (0.191) | (0.189) | (0.185) |
| Log(Property Tax) | -0.335*** | -0.291*** | -0.319*** |
| | (0.0441) | (0.0408) | (0.0432) |
| **Efficiency: Current Spending** | | | |
| Δ HPI ($) | 0.182* | 0.268*** | 0.201** |
| 95% Confidence Interval | [-0.024, 0.388] | [0.072, 0.465] | [0.011, 0.402] |
| N | 114,038 | 114,038 | 114,038 |
| Quartile Trends | Spending | Taxes | Both |
| Complete Set of Controls | ✓ | ✓ | ✓ |

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: Current spending is spending on salaries and all other costs excluding capital investments and construction costs. Property tax revenues are measured at the district level and do not include any other local tax revenues. In all models we instrument for endogenous current spending and property tax revenue per-pupil with both initial spending and initial tax quartile in 1972, interacted with the event-time shocks from school finance reforms. Quartile trends are initial IV quartile interacted with a linear time trend. Standard errors reported and are clustered at the district level.

## B3. Robustness to Multi-Level Clustering

We now test the robustness of our results to an alternative approach to computing our standard errors. Instead of clustering our standard errors at the school district level, we cluster our standard errors at the state-by-year-by-spend quartile-by-tax quartile, since this is the level of variation that we exploit for identification. Column (1) replicates the baseline result, Column (2) replicates the baseline result with the controls for sorting on income and Column (3) replicates the result with forward looking behavior. In all three cases we find that a tax-funded increases in current spending elasticity would increase house prices by a statistically significant amount.

Table B.4: Capitalization of Current Spending

| Outcome: Log(HPI) | (1) | (2) | (3) |
|---|---|---|---|
| Log(Current Spending) | 1.172*** | 1.551*** | 1.441*** |
| | (0.208) | (0.222) | (0.215) |
| Log(Property Tax Revenue) | -0.183* | -0.264** | -0.196* |
| | (0.0914) | (0.0938) | (0.0851) |
| **Efficiency: Current Spending** | | | |
| Δ HPI | 0.333** | 0.419*** | 0.438*** |
| Confidence Interval | [0.084, 0.582] | [0.150, 0.688] | [0.189, 0.687] |
| Observations | 123978 | 114038 | 121191 |
| Complete Set of Controls | ✓ | ✓ | ✓ |
| Poverty Control for Sorting | | ✓ | |
| Dependent Variable is a Lead | | | ✓ |
| Sample Years | 1990-2015 | 1993-2015 | 1991-2015 |

Notes: Standard errors reported and are clustered by $state \times year \times spendq \times taxq$ (Correia 2018). Current spending is spending on salaries and all other costs excluding capital investments and construction costs. In all models we instrument for endogenous current spending with the 1972 total spending quartile interacted with event-time shocks from school finance reforms. Property tax revenues are measured at the district level and do not include any other local tax revenues.

## B4. IV/2SLS Estimation in Dollar Cost of Rent

In our main model we regress the log of the house price index on the log of the spending and the log of taxes. The virtue of the log-log specification is that our estimated parameters are reduced-form elasticities with a natural interpretation. In this section, we complement the approach of estimating log-log specifications by re-estimating our model in levels for house prices, school spending and taxes. The virtue of recasting our results in levels is that we can directly report households' willingness to pay for tax-funded increases in salary spending in dollar amounts.

Since current spending and property taxes are already in levels, estimating a model in levels only requires us to convert the house price index into dollar denominations. We implement the conversion in two steps. First, compute the weighted average house value in each school district in 2000, denoted $V_{d,00}$, using data from the 2000 Decennial Census on reported house prices in each census tract and the census tract population weights ($\sum_{j=1}$) described in Section 2:

$$\mathbf{V_{d,00}} = \sum_{j=1}^{J} \omega_j \mathbf{V_{j,00}}.$$

We next convert this stock measure of average housing value in a school district into a flow measure of monthly rental cost in the school district. To convert from house value to rental cost, we calculate the average value to rent ratio in the 2000 Decennial Census, and use this constant to convert the weighted house prices that we previously calculated for each school district ($V_{d,00}$) into an equivalent monthly rental price. From Table B.5, we estimate that the average home value in a school district in 2000 was $205,684 and the national value to rent ratio was 206.36. Because we have a price level in 2000 for each school district and a time-series of the house price index, we can construct a time-series of house price levels using the HPI in a given year as a scaling factor of the rental prices in 2000. We then show results on the capitalization of school spending and taxes into house price levels using rental prices computed using the value to rent ratio at the national level, the MSA level and the school district level. We calculate the rental value in these three distinct ways to explore how sensitive our results are alternative ways of measuring the home to value ratio. In practice we will find that using price to rent conversion factor from the more local levels of geography, i.e. school district and MSA, produce the similar if not slightly more conservative estimates of the capitalization of school spending and property taxes into house prices.

Table B.5: Summary Statistics

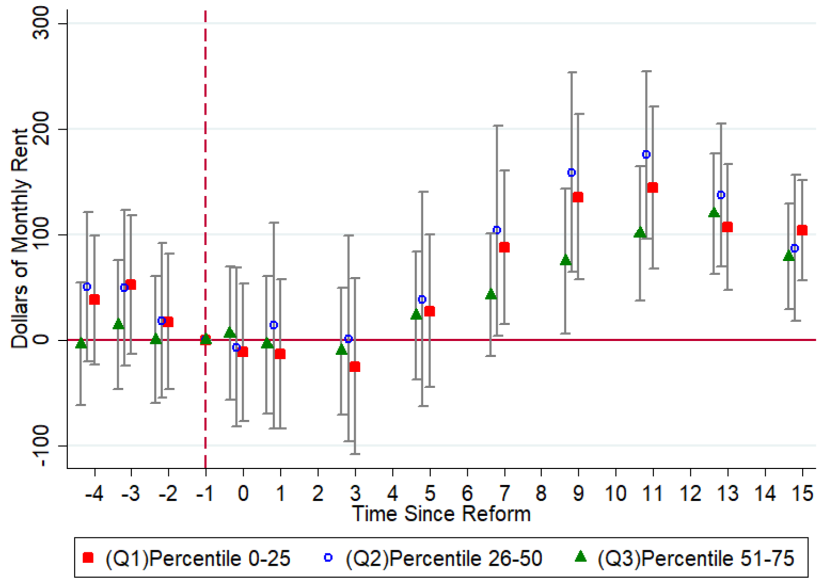
| | 1990 | 2000 | 2010 | 2015 |
|---|---|---|---|---|
| Weighted Avg. Census Home Value | | \$205,684 | | |
| | | (148,457) | | |
| National V-R Ratio | | 206.36 | | |
| Implied Monthly Rental Costs | \$942.46 | \$996.72 | \$984.73 | \$1003.52 |
| | (553.74) | (618.96) | (719.52) | (838.15) |

Notes: All dollar values are reported in 2015 levels. The monthly rental cost for a school district in 2000 is the weighted average census home value divided by the value to rent (V-R) ratio. The district's 2000 rental cost is multiplied by the relative change in the house price index over time to produce the implied rental costs in other years.

We divide $V_{d,00}$ by the estimated value to rent ratio, converting converting the asset value to a measure of monthly rental costs. We calculate the value to rent ratio at the national level, the MSA level, and school district level. The rental cost measure is extended over the entire panel period using the annual variation in our high frequency house price index. Row 3 of table B.5 shows the mean implied monthly rental costs for several years of the sample, deflated to 2015 dollar values.

After computing average monthly rent costs for each district $\times$ year observation, we turn to estimating the IV/2SLS specification. In Figure B.1 we show the reduced form variation in monthly rental costs relative to the event year T=-1. Next, dollar-valued rent costs are modelled as an outcome in a level-log regression analogous to the log-log specification of Equation 11, conducting the efficiency test in the same way. Using

Figure B.1: Reduced Form Effect of Reforms : Dollar Cost of Rent




Notes: The figure event-study graph illustrates the effect of finance reform timing on our measure of monthly rental costs. Of interest are a set of indicator variables that are equal to one for districts in a reform state $T$ years relative to the reform year, interacted with indicators for the the 1972 per-pupil spending quartile. The reference group are school districts in the top quartile of historical total spending along with districts in non-reform states. Additional controls include policy controls for the concurrent rollout of healthcare and social service programs, 1960 county characteristics interacted with linear time trends, along with district and year fixed effects. 95% confidence intervals are depicted.

standard interpretation of a level-log regression, the results of column three show that a 1% increase in current spending increases monthly rent by $\frac{1127.8}{100} = \$11.28$ holding all else constant. A 1% increase in property-tax revenues decreases monthly rent by $\frac{279.0}{100} = \$2.79$. Conducting our efficiency test, which simulates a 1% increase in property tax revenues used entirely on current expenditures, we find that monthly rental costs would increase by $\frac{217.24}{100} = \$2.17$.

Table B.6: Rental Cost Capitalization of Current Spending

| Outcome: Monthly Rental Costs ($2015) | (1) | (2) | (3) |
|---|---|---|---|
| Log(Current Spending) | 1066.1*** | 1099.8*** | 1127.8*** |
| | (195.4) | (193.7) | (193.3) |
| Log(Property Tax Revenue) | -264.8*** | -269.7*** | -279.0*** |
| | (44.04) | (42.15) | (43.37) |
| **Efficiency: Current Spending** | | | |
| Δ Monthly Rental Costs ($) | 204.25** | 214.25** | 217.24** |
| 95% Confidence Interval | [12.35, 396.16] | [21.91, 406.59] | [23.82, 410.65] |
| N | 114890 | 114888 | 103819 |
| Fstat1 | 66.13 | 66.13 | 74.82 |
| Fstat2 | 51.97 | 51.96 | 89.56 |
| V-R Ratio | National | MSA | Local |
| Complete Set of Controls | ✓ | ✓ | ✓ |

Notes: Standard errors reported and are clustered by district. For interpretation of a level-log regression, divide each coefficient by 100 to obtain the dollar-valued response of a 1% increase in the variable of interest. Current spending is spending on salaries and all other costs excluding capital investments and construction costs. In all models we instrument for endogenous current spending per-pupil with the event-time shocks from school finance reforms. Property tax revenues are measured at the district level and do not include any other local tax revenues.

Table B.7: Twenty Largest and Smallest On Diagonal School Districts (by Enrollment)

| District | Enrollment | MSA | State | Quartile | Reform |
|---|---|---|---|---|---|
| **20 Largest School Districts** | | | | | |
| Chicago Public Schools | 392,558 | Chicago, IL | IL | 4 | 0 |
| Palm Beach County | 186,605 | West Palm Beach-Boca Raton, FL | FL | 4 | 0 |
| Cypress-Fairbanks ISD | 113,023 | Houston, TX | TX | 4 | 1 |
| Pinellas County | 103,774 | Tampa-St. Petersburg-Clearwater, FL | FL | 3 | 0 |
| Dekalb County | 101,103 | Atlanta, GA | GA | 4 | 0 |
| Jefferson County | 100,602 | Louisville, KY-IN | KY | 4 | 1 |
| Fulton County | 95,460 | Atlanta, GA | GA | 4 | 0 |
| Denver County | 88,839 | Denver, CO | CO | 4 | 0 |
| Fort Worth ISD | 85,975 | Fort Worth-Arlington, TX | TX | 3 | 1 |
| Austin ISD | 84,564 | Austin-San Marcos, TX | TX | 3 | 1 |
| Long Beach Unified | 79,709 | Los Angeles-Long Beach, CA | CA | 3 | 1 |
| Milwaukee School District | 77,316 | Milwaukee-Waukesha, WI | WI | 4 | 1 |
| Alpine District | 75,161 | Provo-Orem, UT | UT | 1 | 0 |
| Davis District | 70,857 | Salt Lake City-Ogden, UT | UT | 1 | 0 |
| Katy ISD | 70,330 | Houston, Tx | TX | 4 | 1 |
| North East ISD | 67,971 | San Antonio, TX | TX | 2 | 1 |
| Elk Grove Unified | 62,888 | Sacramento, CA | CA | 2 | 1 |
| El Paso ISD | 60,852 | El Paso, TX | TX | 2 | 1 |
| San Francisco Unified | 58,414 | San Francisco, CA | CA | 4 | 1 |
| Santa Ana Unified | 56,815 | Orange County, CA | CA | 3 | 1 |
| **20 Smallest School Districts** | | | | | |
| North Gem District | 191 | Rural Idaho | ID | 4 | 1 |
| Big Pine Unified | 189 | Rural California | CA | 4 | 1 |
| Renick R-V | 187 | Rural Missouri | MO | 4 | 0 |
| Limestone Walters CCSD 316 | 184 | Peoria-Pekin, IL | IL | 2 | 0 |
| Dorchester Public School | 182 | Rural Nebraska | NE | 4 | 1 |
| South Nodaway Co. R-IV | 179 | Rural Missouri | MO | 4 | 0 |
| Walnut Comm School District | 175 | Rural Iowa | IA | 3 | 1 |
| Lake Quinault School District | 170 | Rural Washington | WA | 2 | 1 |
| Bradleyville R-I | 166 | Rural Missouri | MO | 1 | 0 |
| Hampton Public Schools | 158 | Rural Nebraska | NE | 4 | 1 |
| Julian Union High | 157 | San Diego, CA | CA | 4 | 1 |
| Gilmanton School District | 157 | Rural Wisconsin | WI | 2 | 0 |
| Bartelso SD 57 | 156 | St. Louis, Mo-Il | IL | 1 | 0 |
| Braceville SD 75 | 150 | Chicago, Il | IL | 1 | 1 |
| St. John School District | 150 | Spokane, Wa | WA | 2 | 0 |
| St Rose SD 14-15 | 149 | St. Louis, M0-IL | IL | 1 | 0 |
| Lake Benton Public School District | 131 | Rural Minnesota | MN | 2 | 1 |
| Wyoming Central School District | 123 | Rural New York | NY | 1 | 1 |
| Lavallette Borough Board Of Education | 122 | Monmouth-Ocean, NJ | NJ | 4 | 0 |
| Gilmore City-Bradgate Comm School District | 120 | Rural Iowa | IA | 2 | 1 |