A National Study of Public School Spending and House Prices*

Patrick J. Bayer† Duke & NBER

Peter Q. Blair‡ Harvard & NBER

Kenneth Whaley§ University of Houston

December 2020

Abstract

We conduct a national study of the causal impact of school spending and local taxes on housing prices by pairing variation induced by school finance reforms with 25 years of national data on housing prices. Our analysis speaks to two classic questions in economics: whether school spending matters and whether it is provided at efficient levels. The results indicate that households highly value school spending and, in particular, spending on the salaries of teachers and staff. Moreover, we find that salary spending is provided at inefficiently low levels throughout much of the United States, as increases in salary spending within a school district funded entirely by local taxes would generally raise house prices. Our analysis points to both the hiring of more teachers and increasing teacher pay as mechanisms for improving the efficiency of the provision of public schooling in the United States.

JEL Classification: I22, I24, and H41

*We received helpful comments from: Rucker Johnson, Thomas Kane, Claudia Persico, 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, Neale Mahoney, Jonathan Meer, Susan Dynarski, Raj Chetty, Jeffrey Smith, Chao Fu, Richard Murnane, Juan Carlos Suarez Serrato, Mark Shepard, Martin West, Morgan Adderley, Annemarie 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, UW Madison, UCLA, E-con of Education, Cleveland Fed, Clemson, Nebraska, Tulane, Houston, Southern Economic Association, Western Economic Association and The BE-Lab.

†Patrick Bayer is a Professor at Duke University in the Economics Department.

‡Peter Blair is an Assistant Professor at Harvard University in the Graduate School of Education.

§Kenneth Whaley is an Economist at Caterpillar and Adjunct Professor at University of Houston.

1 Introduction

This paper addresses two central questions in the economics of education and public finance that have interested scholars for over fifty years: whether public school spending matters and whether it is efficiently provided.

The first question has been heavily debated in the United States since the publication of the Coleman report in 1966, which found no correlation between school spending and student test scores once family and peer characteristics were taken into account (Coleman et al. 1966; Hanushek 1986, 2003). A recent burst of papers using quasi-experimental methods has brought renewed attention to this long-standing question along with a series of credible new findings: notably, showing that increasing school spending improves test scores, graduation rates, wages, economic mobility, and other life outcomes for students (Jackson et al. (2015), Lafortune et al. (2018), Jackson (2018), Biasi (2017), Hyman (2017), Brunner et al. (2019), and Baron (2019)).

The second question of whether public goods can be efficiently provided has an even longer history in economics, going back to at least Musgrave (1939). Samuelson (1954) argued that the centralized provision of public goods naturally leads to under-provision because of the free-rider problem. Tiebout (1956) posited that local provision and financing of public goods can overcome the free-rider problem. This hypothesis has catalyzed a vibrant literature, exceeding 20,000 academic papers. Tiebout's key insight was that local provision and financing creates choice and hence a market for the public good, solving the free-rider problem and helping to drive efficient provision.

Inspired by Tiebout's hypothesis, Oates (1969) developed a simple but powerful empirical test for whether local public goods are efficiently provided. The Oates test for efficiency is valid even when the strong assumptions of the Tiebout model do not hold. In particular, Oates conjectured that if households are voting with their feet and the public good is efficiently provided, a marginal increase in local taxes used to fund a marginal increase in the local public good should have no effect on house prices. If, instead, a marginal increase in locally financed public goods provision increased house prices, this would indicate that such an increase would raise the value of living in the district and therefore the local public good had been under-provided.[1]

Economists have long viewed the housing market as a natural setting to study whether public school spending matters to households and is efficiently provided. Empirical implementation of these intuitive ideas on a national scale has been constrained by the intensive data requirements of credible research designs, on the one hand, and a series of stubborn endogeneity problems that plague naive OLS regressions, on the other hand. Most of the recent literature on school capitalization has exploited school attendance zone boundaries to estimate what households are willing to pay for other differences in schools such as test scores and peer composition rather than spending per se (Black 1999; Bayer et al. 2007; Kane et al. 2006).[2] For the most part, existing studies have focused on a single local setting - metropolitan area or state - due to requirements of the data or research design.

We thread the needle of producing a credible study with broad external validity by leveraging the timing of court-mandated school finance reforms (SFRs) to isolate plausibly exogenous variation in school spending and local taxes.[3] Our approach for constructing valid instruments is based on the research design of Jackson et al. (2015). Intuitively, the same SFRs that give rise to increased spending in previously low spending districts contain tax incentives based on the redistributive nature of SFRs which cause previously higher (lower) spending districts to raise less (more) tax revenue following the reforms (Hoxby, 2001; Hoxby and Kuziemko, 2004).[4] We pair the exogenous variation in school

[1] Note that this is a private notion of efficiency, which considers only households' willingness/ability to pay for the public good. It does not consider frictions due to credit constraints or any externalities or spillovers.

[2] In some states, school districts are allowed to raise funds through bond referenda. Cellini et al. (2010) exploit close elections in California to estimate the impact of increases in funding on local property values.

[3] Murray et al. (1998) and Card and Payne (2002) showed that these reforms significantly reduced inequality in spending across school districts.

[4] There are also components of SFRs that guarantee a foundation level of spending per student which operate separately from the tax incentives due to redistribution. This feature of SFRs allows us to separately identify the effects of both school spending and local taxes from the same set of policy reforms.

district spending and taxes with a 25-year national panel of local house price indices (HPI) from the Federal Housing Finance Agency.

We find that a 1 percent increase in school spending increases house prices by 0.95 percent.[5] The elasticity of the house prices with respect to per-pupil school spending gives us a national measure of how much households value spending on schools – answering our first question. For comparison, Jackson et al. (2015) find that a 1 percent increase in school spending increases the adult wages of a student by 0.70 to 0.80 percent. Taken together, these findings are consistent with the notion that households value the causal impacts of greater school funding on the lifetime outcomes of their children and are willing to pay higher prices for homes that provide access to better-funded schools.

When we dis-aggregate school spending into spending on salaries and non-salary spending, we find that that salary spending is positively capitalized into house prices whereas non-salary spending is not.[6] These findings are consistent with the results in Baron (2019) for Wisconsin which show that increases in operational expenses (salary inputs) improved student test scores and reduced drop out rates whereas increases in capital expenses (non-salary) had no impact.[7] This implies that how money is spent matters crucially, as argued in Hanushek (1986) and the literature reviewed in Jackson (2018).

To test for the efficiency of school spending, we add local tax revenues to the model and jointly estimate the elasticity of house prices with respect to both school spending and taxes. Holding school spending constant, we find that a 1 percent increase in local tax revenues per-pupil reduces house prices by 0.20 percent. Combining the estimated elasticities of house prices with respect to school spending and local tax revenue, we calculate the efficiency elasticity, which we define as the percent increase in house prices due

[5]We caution against extrapolating results to increases in school spending that are larger than 10 percent, given that this is the range of the variation of school spending in our data.

[6]Additional analysis indicates that households respond positively to additional salary spending regardless of whether it is used to increase the teacher-student ratio or the average salary expenditure per teacher.

[7]Similarly, using a national sample Martorell et al. (2016) find that increased capital expenditure did not increase student's test scores.

to a 1 percent tax-financed increase in school spending.[8] A positive efficiency elasticity indicates that school spending is inefficiently low and a negative efficiency elasticity that school spending is inefficiently high. We estimate that the efficiency elasticity of salary spending is 1.03, with a 95% confidence interval that ranges from 0.58 to 1.48. Therefore, we strongly reject the hypothesis that salary spending is provided at efficient levels in the United States.

All of our key results related to the capitalization of school spending and local taxes, the importance of salary vs. non-salary spending, and the efficiency tests are robust to the inclusion of numerous controls for county time trends in demographics, potentially concurrent policy changes, and the subsequent sorting of households across school districts. Interestingly, these results are also quite similar when we isolate variation coming from the bottom, middle, and top of the initial school district spending distribution, suggesting a remarkable homogeneity in these important elasticities throughout the distribution.

Taken as a whole, our findings strongly support two clear conclusions. First, households highly value increases in school spending, particularly spending on salaries, at a level that is consistent with estimates from the recent literature on the impact of school spending on future wages and other economic outcomes. Second, spending on salaries of teachers and staff is currently provided at inefficiently low levels throughout much of the United States. Our analysis points to both the hiring of more teachers and increasing teacher pay as mechanisms for improving the efficiency of the provision of public schooling in the United States.

2 Data

Our analysis combines data from several sources to form a panel of school district expenditures, revenues, demographics, and house prices covering the years 1990-2015. The

[8] We refer to this derived quantity as our efficiency elasticity in the spirit of the Oates (1969) test for the private efficiency of providing a local public good.

length of the study period is driven by the availability of house price indices at the school district level.

2.1 House Price Index

Our measure of house prices in each school district is based on the local house price indices (HPI) constructed by the Federal Housing Finance Agency (FHFA). Over our study period, these indices are constructed for an increasingly large sample of Census tracts covering most of the United States. 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.[9]

The key advantage of the FHFA HPIs is that they are available at a fine level of geography for most of the United States for a long sample period. 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.[10] In practice, the FHFA and Case-Shiller indices are very highly correlated and these differences in the basis for the underlying sample of house prices creates only small differences in the indices

2.2 District Finance & Demographic Data

School finance data are publicly available and come from the National Center for Education Statistics (NCES). Data include total expenditures in various categories (salaries,

[9]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.

[10]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.

capital, and construction expenses) and revenues from various sources (federal, state, and local) for school districts each year. We construct per-pupil revenue and expenditure measures for our analysis.[11] Beginning in 1993, the NCES also provides some basic student demographic data including the fraction of students in poverty. Finally, we use district level finance data from 1972 provided by the US Census Historical Database on Individual Government Finances to form the pre-reform school spending quartiles within each state, following the approach in Jackson et al. (2015).

2.3 Court Mandated School Finance Reforms

Our coding of the timing of the court mandated school finance reforms follows Jackson et al. (2015) as shown in Table 1. It is important to note that this approach dates the reform to the date of the court ruling rather than the implementing legislation in each state. As a result, changes to school expenditures and tax revenues often take several years to fully kick in. 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 and the local tax burden, 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.

2.4 Final Dataset

We aggregate Census tract house price data to the school district level using a population weighted average of the tracts within each school district.[12] Following Jackson et al. (2015), we include two additional sets of control variables: (i) county level descriptive

[11] To deal with some obvious coding errors related to pupil counts, we drop a small number of districts for which pupil counts are 50 percent higher in one of any two consecutive years.

[12] The assignment of Census tracts to school districts is based on the IPUMS National Historical GIS shapefiles.

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 140,000 school district-by-year observations from 35 states and roughly 6,300 US school districts.

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 school spending on housing prices. We then lay out the school finance reform event study design, inspired by the recent studies of Jackson et al. (2015) and Lafortune et al. (2018), and discuss why it is well suited for identifying the capitalization of not only school spending but also local property taxes. With the ability to identify the capitalization of both spending and taxes in a single study, we conclude this section of the paper by describing an intuitive test for the private efficiency of local public goods provision first introduced in Oates (1969).

3.1 The Empirical Challenge

Because households sort across school districts and local taxation has historically played a major role in the funding of K-12 schools in the United States, 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 directly. 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.[13] In fact, as we have discussed, the Oates 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 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 slightly adapt) the research design developed by Jackson et al. (2015) to study the impact of school spending on the future life outcomes of children. This approach exploits the timing of court-mandated school finance reforms across US states to isolate plausibly

[13] To give a sense of these endogeneity concerns in the context of our analysis: OLS estimates of the specifications shown in Tables 2 and 5 below result in a coefficient on local property taxes that is positive and a coefficient on school spending that is close to zero.

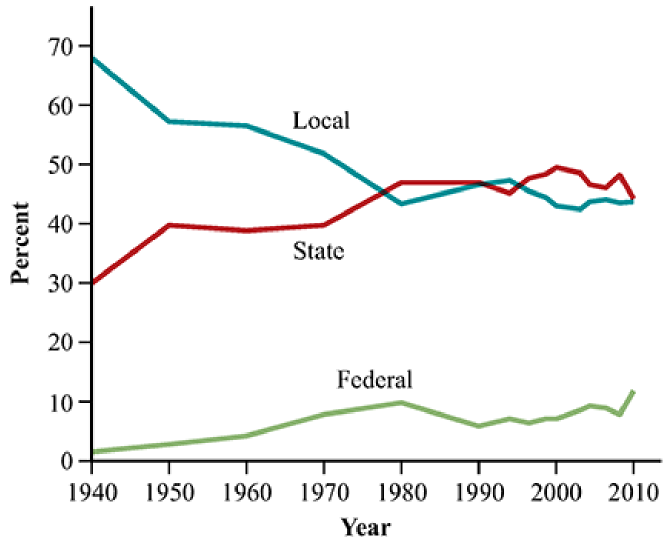

Figure 8.2
Lovenheim/Turner, *Economics of Education*, 1e
Data from: Digest of Education Statistics:
http://nces.ed.gov/programs/digest/d12/tables/dt12_202.asp

Figure 1: Fraction of school revenues by source: local, state and federal, reproduced with permission from Lovenheim and Turner (2018)

.

exogenous changes in school spending. 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 United States 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 United States has historically relied heavily on local taxation, primarily 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 US 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".[14] In total, the existing school finance regime has been successfully challenged in 25 states since 1971 as shown in Table 1, which documents the date of the first court ruling in each state, following the coding in Jackson et al. (2015).[15]

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

Table 1: List of SFRs by state and year that were mandated by state supreme courts.

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 require-

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

[15]We make one change relative to Jackson et al. (2015) 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.

ments of the state's constitution.[16] 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 equalize per-pupil spending across districts.[17] 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 the Jackson et al. (2015) research design.

3.3 SFR Event Study Design

The main idea underlying the school finance reform event study design developed in Jackson et al. (2015) is that these reforms generated systematic changes in school spending that reduced inequality in spending across districts - i.e., raised spending in previously low spending districts relative to previously high spending districts. To isolate these kinds of SFR-induced shocks to spending across districts, Jackson et al. (2015) 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.[18]

Figure 2 highlights the variation in school spending isolated by the Jackson et al. (2015) instruments. In particular, the figure shows the predicted gap in spending between school

[16] 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.

[17] 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).

[18] Beginning in 1972, per-pupil expenditure at the school district level is continuously available nationwide on an annual basis from the NCDB.

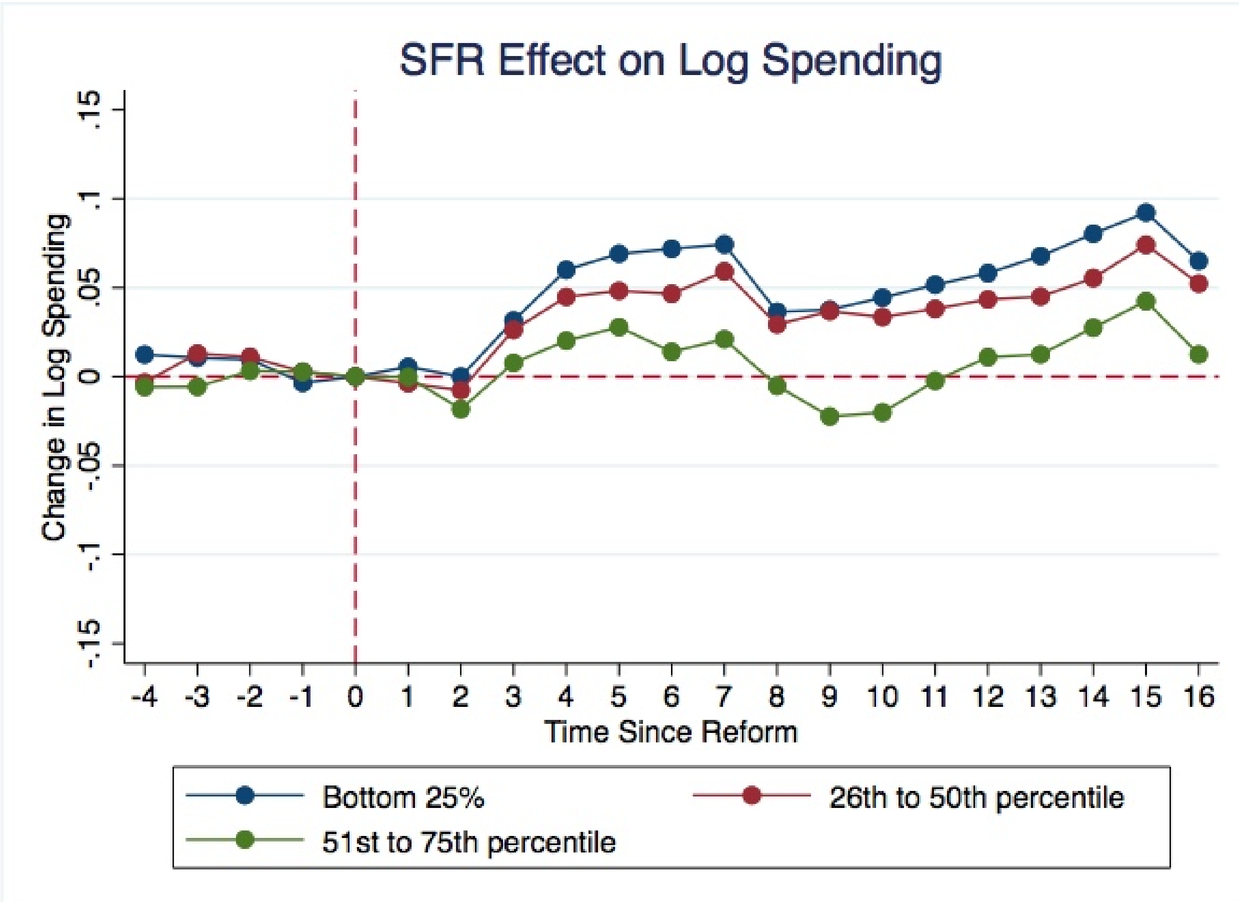

Figure 2: Event-study graph demonstrating the change in the difference in per-pupil school expenditures between school districts in the top 25 percentile of expenditures in 1972 and school districts in the bottom 75 percentile of the expenditures in 1972 before and after court-mandated school finance reforms.

districts in the bottom three quartiles of pre-reform spending quartile relative to the quartile that initially had the highest level of spending, conditional on district fixed effects.

Three features of the figure are important to highlight. First, across all of the states that instituted such reforms, spending increased steadily in districts in the lower versus higher quartiles of the initial spending distribution in the two decades following a court-ordered reform. Second, there is a lag in the full realization of the reforms, reflecting the time it takes for the state legislatures to craft the implementing legislation. And, third, there is essentially no difference in trends in school expenditures across the four spending quartiles prior to a school finance reform. This third feature of the data supports the assumption that the subsequent changes in school spending across the four quartiles in initial spending are effectively shocks to school spending levels, uncorrelated with any prior trends in relative spending levels.

Notice that the Jackson et al. (2015) instruments aggregate the predicted change in spending post-reform across both districts within an initial spend quartile and 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.

Our exact implementation of the SFR event study design 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:

$$\sum_{T=0}^{T=16} \sum_{Q_{72}=4}^{Q_{72}=1} \mathbb{1}(\mathbb{Q}) \times \mathbb{1}(\mathbb{T}).$$

Our main estimating equation also includes district fixed effects, a vector of other time-varying controls, and controls for 1972 spending quartile interacted with pre-reform event time dummies from $T = -4$ to $T = -1$.[19]

$$log(p_{d,t}) = \theta log(s_{d,t}) + f_d + \beta X_{d,t} + \sum_{T=-4}^{T=-1} \sum_{Q_{72}=4}^{Q_{72}=1} \left[\lambda_{Q,T} \mathbb{1}(\mathbb{Q}) \times \mathbb{1}(\mathbb{T})\right] + \epsilon_{d,t}$$

[19] Our choice of the number of periods for which to include pre-reform controls is governed by the fact that our main study period spans only the years 1990-2015. Given the relatively small number of states that implemented reforms after 1997, extending the pre-reform period further back would require estimating pre-trends for those periods on the basis of just a handful of states.

where:

- $p_{d,t}$ indicates housing price index of school district d in time period t,
- $s_{d,t}$ indicates per-pupil school spending of school district d in time period t,
- f_d indicates district fixed effects,
- $X_{d,t}$ indicates time varying district controls,
- $\mathbb{1}(\mathbb{Q})$: indicator for pre-reform 1972 spending quartile in state, and
- $\mathbb{1}(\mathbb{T})$: indicator for time relative to SFR reform.

3.4 Adding Taxes to the Analysis

One key advantage of using the school finance reform event study design is that it is possible to estimate school spending capitalization in a broad national data set. A second, more subtle advantage of this approach is that it allows us to break the link between school spending and local taxation. As mentioned above, a longstanding challenge in the empirical literature on the capitalization of school spending is the natural coupling of changes in spending and taxation.

An attractive feature of using the SFR event study design is that SFRs often increase revenue to previously low-spending districts from multiple levels of government. In addition to some direct redistribution at the state level, certain kinds of SFRs, such as district power equalization formulas and matching grants, create incentives for districts with relatively poor local tax bases to increase local tax revenue and, often, for high spending districts to decrease local tax revenue (Hoxby 2001; Hoxby and Kuziemko 2004).[20]

Figure 3 shows the dynamics of local tax revenues following a school finance reform, separating districts again into quartiles (Q1-Q4) based on initial school spending in 1972.

[20] Following Jackson et al. (2015), we do not attempt to exploit variation across states in the exact type of school finance reform that was implemented because we do not want to introduce endogenous factors at the state level that may have led different states to pass different types of reforms.

As the figure makes clear, local property tax revenue increased in districts with relatively low vs. high initial levels of spending, but the timing and extent of the changes vary substantially across spending quartiles. In particular, local tax revenue increased sharply in Q1 and Q2 districts relative to the other quartiles and, primarily, in one level shift upwards in the first year following the reform and then gradually thereafter. This variation in the timing and extent of the changes across quartiles relative to the variation in school spending shown in Figure 2 provides the basis for separately identifying the capitalization of school spending and local tax revenues.

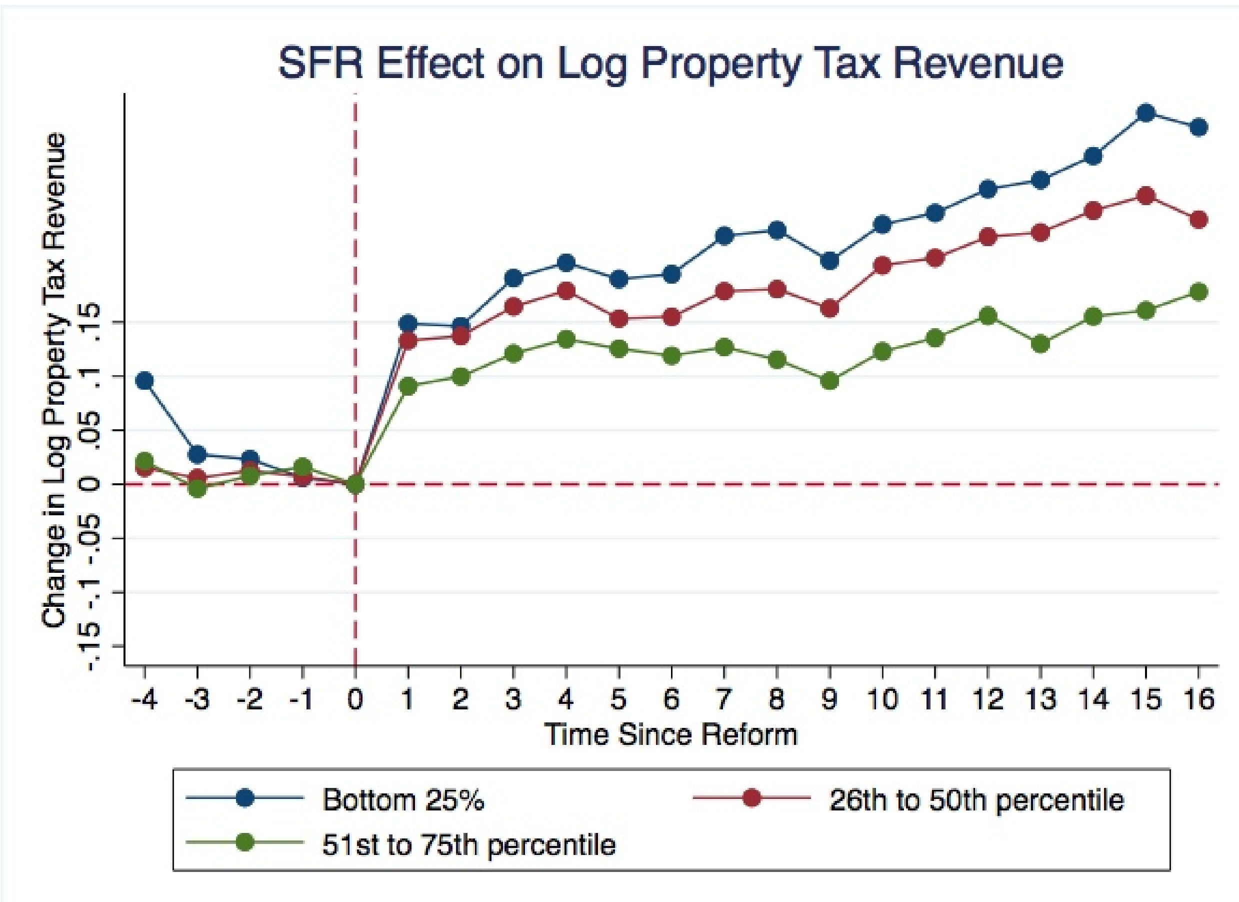

Figure 3: Event-study graph demonstrating the change in the difference in log(tax) between school districts in the top 25 percentile of expenditures in 1972 and school districts in the bottom 75 percentile of the expenditures in 1972 before and after court-mandated school finance reforms.

To incorporate the impact of SFRs on local tax levels, we include both per pupil school spending and per pupil local tax revenues, instrumenting for both with the 1972 spend quartile by time since reform instruments. Using the same notation as above, this consists of adding a second endogenous variable to the right hand side:

$$log(p_{d,t}) = \theta log(s_{d,t}) + \gamma log(\tau_{d,t}) + f_d + \beta X_{d,t} + \sum_{T=-4}^{T=-1} \sum_{Q_{72}=4}^{Q_{72}=1} \left[\lambda_{Q,T} \mathbb{1}(\mathbb{Q}) \times \mathbb{1}(\mathbb{T})\right] + \epsilon_{d,t}$$

where: $\tau_{d,t}$ indicates local tax revenue per pupil. Because the extent and timing of local tax changes following SFRs varies substantially from the school spending changes, we are able to separately identify the effect of spending and taxes in this extended model and estimate the key parameters θ and γ with enough precision to reach meaningful economic conclusions. From a purely counting perspective, the model is formally over-identified, as each of the quartile-by-event-time dummies is an instrumental variable, giving us $N \times Q$ instrumental variables, where N represents the number of time periods post reform and Q the number of quartiles.[21]

3.5 Testing the Efficiency of School Spending

It is important to emphasize that the goal of our analysis is not to understand the effects of (particular) school finance reforms per se but to use them instrumentally, as major shocks to the ways that schools are financed and funded. These shocks generate plausibly exogenous district variation in spending and local taxes revenues dedicated to schools, allowing us to credibly identify the separate effects of school spending and local tax revenues, θ and γ, in a single study.

With estimates of the capitalization of school spending and local taxes in hand, we can carry out the intuitive test of the private efficiency of local public goods provision proposed by Oates (1969).[22] The thought experiment underlying Oates's test is straight-

[21] We thank Isaiah Andrews for a fruitful discussion on this point.

[22] Brueckner (1982) provides a formal basis for the Oates test in a model of local public goods provision in a system of local governments.

forward: consider the effect on housing prices of raising a marginal dollar from local sources and spending it on the local public good. If this leads to an increase in house prices, it suggests this would raise the value of living in the district and, as such, that the local public good had been under-provided. In contrast, if raising and spending an extra dollar leads to a decrease in house prices, the same logic implies that the public good had been over-provided. Thus, in the context, of equation (3.4), the Oates efficiency test is given by:

$$\sigma_{local}\theta + \gamma = 0$$

where σ_{local} is the fraction of school spending from local sources. This adjustment is necessary because spending, taxes, and prices enter equation (3.4) in logs.

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 get 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. [23]

4 Results

4.1 The Capitalization of School Spending

We begin our analysis of the effect of school spending on house prices by creating an event study figure for house prices analogous to the one for school spending shown in Figure 2. Specifically, Figure 4 shows the dynamics of log house prices by initial school spending quartile (Q1-Q4) for four years before and sixteen years after a school finance reform. As the figure makes clear, house prices rose sharply in Q1-Q3 districts relative to highest quartile (Q4) districts, following the same general pattern as the impact of SFRs on school spending.

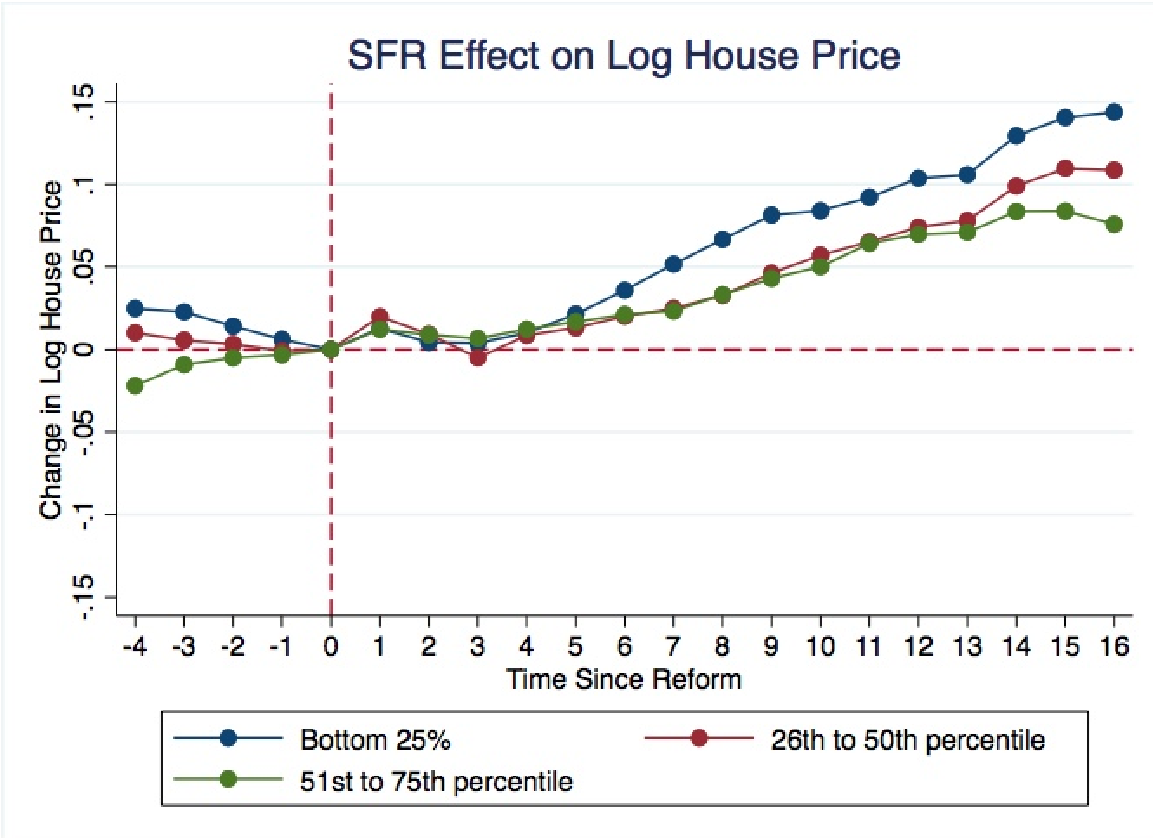

Figure 4: Event-study graph demonstrating the change in the difference in log(HPI) between school districts in the top 25 percentile of expenditures in 1972 and school districts in the bottom 75 percentile of the expenditures in 1972 before and after court-mandated school finance reforms.

Table 2 reports the results of IV regressions of housing prices on school spending. The four columns of successively include more control variables. The first column includes controls for school district fixed effects and calendar year dummies. The second column adds controls for 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. These same controls were used in Jackson et al. (2015) and are intended to absorb any potential heterogeneous trends in house prices across different types of school districts.

The third column of Table 2 adds 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. These controls are again taken directly from

Jackson et al. (2015) and are intended to absorb any changes in house prices that may be due to these other policy changes rather than school finance reforms. 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.

Outcome: Log(HPI)	(1)	(2)	(3)	(4)
Log(Total Spending/Pupil)	0.802***	1.033***	0.973***	0.949***
	(0.139)	(0.158)	(0.158)	(0.158)
Observations	140,194	130,772	130,772	130,772
District FE	✓	✓	✓	✓
Census Controls		✓	✓	✓
Policy Controls			✓	✓
Data Coverage				✓

Table 2: Results from baseline specification of log(HPI) on log(total spending/pupil) with successively more detailed control variables.

The results are qualitatively similar across the four columns, implying a substantial elasticity of school spending with respect to house prices ranging from 0.8 to 1.0. The result in the final column implies that a 1 percent increase in school spending leads to a 0.95 percent increase in property values. Importantly, this is the estimated impact on the margin within the range of the variation in school spending data generated by SFRs, which, as shown in Figure 2, is on the order of 5-10%.

The magnitude of the point estimates in Table 2 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. (2015) and Lafortune et al. (2018). Jackson et al. (2015), for example, estimate an elasticity of future wages with respect to school spending on the order of 0.7-0.8. 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 Which Kinds of Spending Matter?

The results presented in Table 2 make clear that households highly value the change in school spending resulting from school finance reform shocks. A natural follow-up question is: does it matter how the money is spent? To address this question, we utilize the spending categories available in the Common Core Data to separate spending into a component that captures the total salaries of all personnel in the district and a component that captures all other non-salary spending.

Figures 5 and 6 shows how the log of these two spending components changes following a school finance reform. There is substantial variation in both the timing and extent of the changes across the four spending quartiles that varies by the type of spending, with salary spending rising steadily in Q1-Q3 districts relative to Q4 districts and non-salary spending varying in lumpier ways in the period following a school finance reform.

The comparison in spending patterns between Q3 and Q4 districts is particularly interesting, with Q3 districts simultaneously increasing salary spending and reducing non-salary spending relative to Q4 districts, perhaps reflecting differences in priorities.

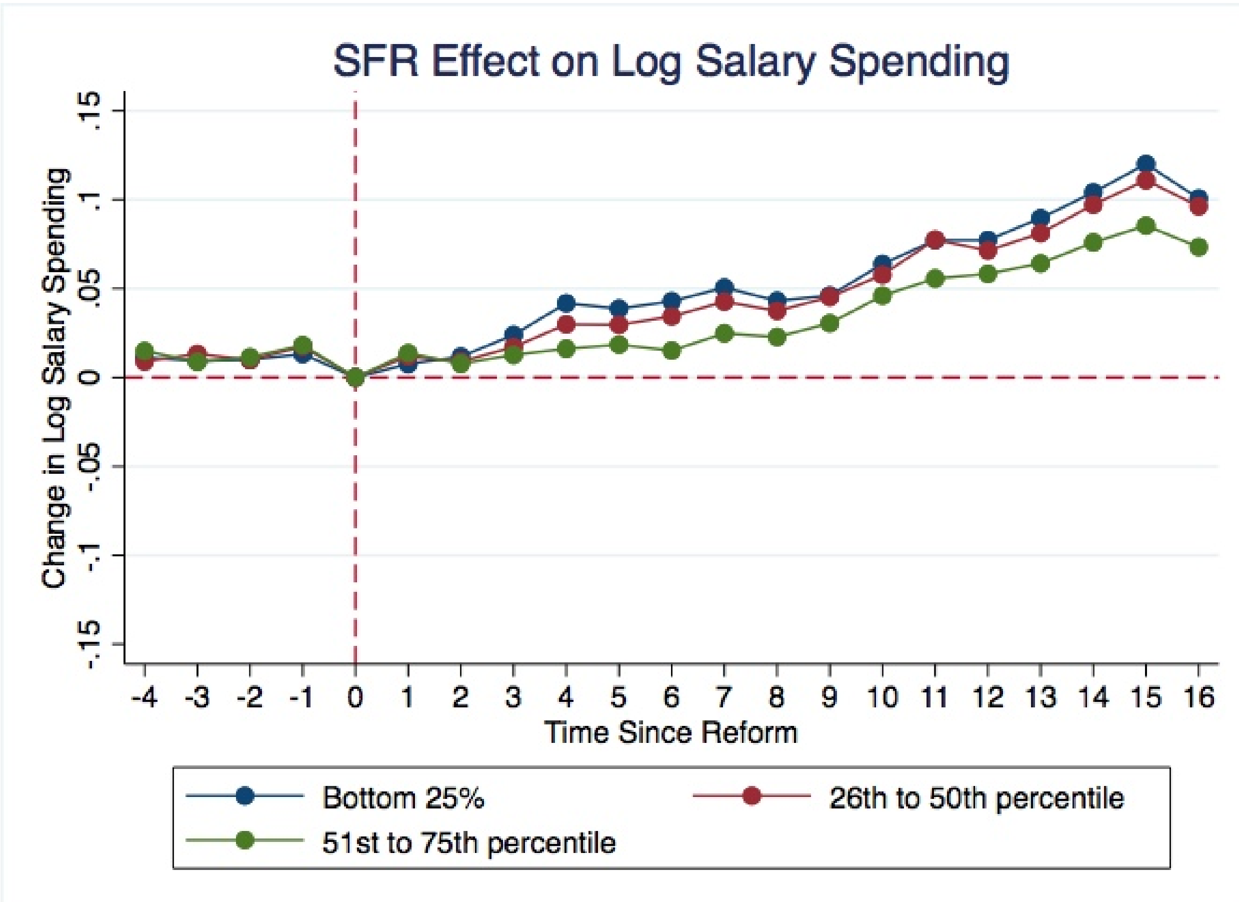

Figure 5: Event-study graph demonstrating the change in the difference in log(Salary) between school districts in the top 25 percentile of expenditures in 1972 and school districts in the bottom 75 percentile of the expenditures in 1972 before and after court-mandated school finance reforms.

There are several important reasons to consider the impact of salary and non-salary expenditure separately. First, the evidence summarized in Hanushek (2003) and Jackson (2018) suggests that there is heterogeneity in the impact of different types of spending on student outcomes. Recent studies by Hyman (2017) and Baron (2019), in particular, find clear evidence that increased salary spending improves student outcomes. Second, non-salary spending on infrastructure is often funded through school bonds, with the increase

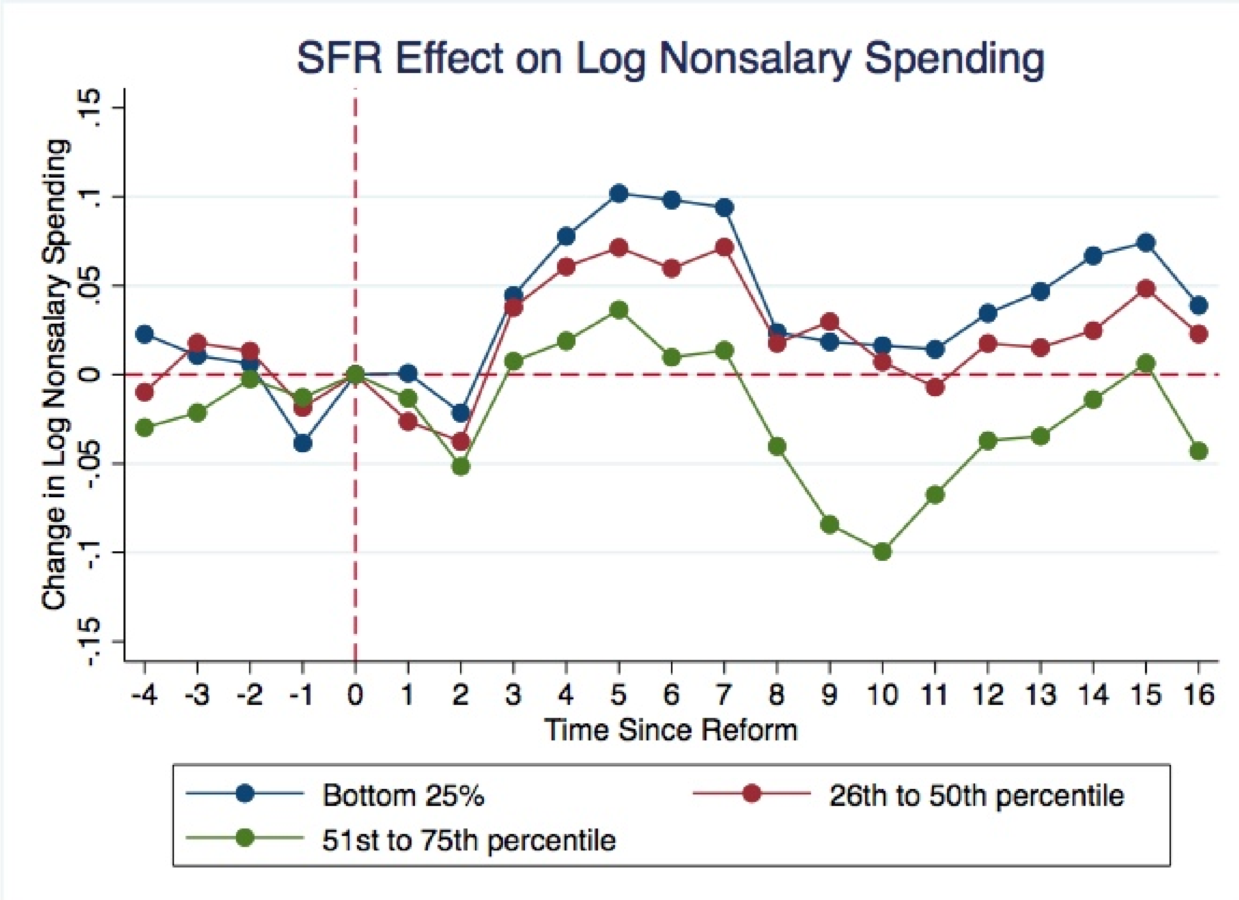

Figure 6: Event-study graph demonstrating the change in the difference in log(Non-Salary) between school districts in the top 25 percentile of expenditures in 1972 and school districts in the bottom 75 percentile of the expenditures in 1972 before and after court-mandated school finance reforms.

in funding then accompanied by future debt obligations (unobserved in our study). In this case, we would expect the capitalization of non-salary expenditures to capture the combined effect of both the spending and debt obligations. In this way, the estimate of the coefficient on non-salary expenditure provides a direct test of the efficiency of non-salary expenditures as discussed and implemented in Cellini et al. (2010) and Martorell et al. (2016) using a close-elections research design in school bond referenda. A third (related) reason to disaggregate spending into salary and non-salary, therefore, is to be able to implement an efficiency test for salary expenditures, something which has not been done in the literature to date.

Tables 3 and 4 present results for a series of specifications analogous to those included in Table 2 for the log of per-pupil salary and non-salary expenditure, respectively. Strikingly, the capitalization of overall spending on house prices loads strongly and completely on salary spending with non-salary spending estimated to have essentially no effect on house prices. As discussed above, this null effect for non-salary capitalization is consistent with the notion that these expenditures are efficiently provided on the margin to the extent that the coefficient here captures the combined effect of spending and future debt obligations.[24]

Outcome: Log(HPI)	(1)	(2)	(3)	(4)
Log(Salary Spending/Pupil)	1.882***	2.035***	2.064***	2.066***
	(0.371)	(0.385)	(0.367)	(0.372)
Observations	140,194	130,772	130,772	130,772
District FE	✓	✓	✓	✓
Census Controls		✓	✓	✓
Policy Controls			✓	✓
Data Coverage				✓

Table 3: Results from baseline specification of log(HPI) on log(salary spending/pupil) with successively more detailed control variables.

That spending on salaries is so highly valued by households suggests that households observe and appreciate the increase in either the number of positions funded, which might reduce class sizes, or the average salary per position, which might improve teacher quality (Hanushek et al., 2019).[25] Interestingly, Jackson et al. (2015) show that school finance reforms induced a response on both of these margins, increasing the teacher-student ratio and average teacher salaries in the lowest versus highest quartile districts.

[24] Restricting attention to school finance reforms that occurred during the sample period - i.e., after 1990 - has little effect on the qualitative pattern or statistical significance of the results presented throughout the paper. The estimated coefficients and standard errors for the capitalization of overall spending, spending on salaries and spending on non-salary expenditure, i.e., column 4 of Tables 2, 3 and 4, are 0.938 (0.143), 1.463 (0.237), and -0.010 (0.072), respectively.

[25] That households highly value marginal increases in spending on school personnel belies the notion that such spending would largely lead to infra-marginal windfalls for existing teachers and staff with no resulting benefits to children.

Outcome: Log(HPI)	(1)	(2)	(3)	(4)
Log(Non-Salary Spending/Pupil)	-0.114	0.030	-0.110	-0.126*
	(0.070)	(0.080)	(0.073)	(0.076)
Observations	140,194	130,772	130,772	130,772
District FE	✓	✓	✓	✓
Census Controls		✓	✓	✓
Policy Controls			✓	✓
Data Coverage				✓

Table 4: Results from baseline specification of log(HPI) on log(non-salary spending/pupil) with successively more detailed control variables.

And, as it turns out, when we decompose the log of per pupil salary spending into two components: (i) log of teacher-student ratio and (ii) log of salary spending per teacher and include these in a specification analogous to column 4 of Table 3, the estimated coefficient and standard error on the log of the teacher-student ratio is 0.808 (0.251) and on the log of salary spending per teacher is 1.865 (0.350). These coefficients are both statistically significant at the 0.001 level, suggesting that households place significant value on both dimensions of salary spending. Because salary spending is financed from concurrent taxes and transfers, however, to test for the efficiency of salary spending, we need to add taxes to the analysis, which is where we turn next.

4.3 Taxes and Spending

Table 5 presents the results of IV regression that add the log of local tax revenues to the specifications reported in Table 2. In this case, we instrument for both log school spending and log local tax revenue using the school finance reform event study design. As expected, local tax revenue enters negatively in all of the specifications. Interestingly, the inclusion of local property tax revenue has only a modest impact on the coefficients on school spending in all four specifications, when compared to the analogous result presented in Table 2. That the coefficients on school spending change so little suggests that there is only a modest amount of high frequency correlation between variation in school spending and local taxes within the event study framework.

Outcome: log(HPI)	(1)	(2)	(3)	(4)
Log(Total Spending/Pupil)	0.836***	1.047***	0.965***	0.943***
	(0.142)	(0.159)	(0.157)	(0.158)
Log(Property Tax/Pupil)	-0.145***	-0.151***	-0.195***	-0.197***
	(0.0457)	(0.0464)	(0.0453)	(0.0449)
Observations	138,144	128,832	128,832	128,832
District FE	✓	✓	✓	✓
Census Controls		✓	✓	✓
Policy Controls			✓	✓
Data Coverage				✓

Table 5: The Effect of Overall School Spending and Taxes

Given the potential for non-salary expenditure to be financed through bonds, we focus on salary expenditure when implementing the Oates efficiency test. To that end, the first two columns of Table 6 report results for a series of log house price regressions analogous to those reported in the final columns of Table 2 and Table 5, respectively, but with spending broken down into salary and non-salary components. As in Table 3, the coefficients on salary spending are large and statistically significant, implying that households highly value spending on salaries.

The differences in the timing and extent of variation in property taxes, salary, and non-salary expenditures across quartiles shown in Figures 3, 5, and 6 suggest that there is not a tremendous amount of correlation in the variation used to separately identify the coefficients shown in Columns (1) and (2) of Table 3. The specifications shown in the final two columns of the table provide another way to see this. In particular, the point estimates on salary spending and property taxes change very little with the inclusion of any combination of the other measures, implying little correlation in the variation used to identify

Outcome: log(HPI)	(1)	(2)	(3)	(4)
Log(Salary Spending/Pupil)	2.259***	2.291***	2.099***	
	(0.405)	(0.390)	(0.358)	
Log(Non-Salary Spending/Pupil)	-0.485***	-0.510***		-0.156**
	(0.130)	(0.130)		(0.0767)
Log(Property Tax/Pupil)		-0.252***	-0.228***	-0.229***
		(0.0632)	(0.0548)	(0.0439)
Efficiency Elasticity: Salary Spending				
% ΔHPI		1.029	0.946	
Confidence Interval		[0.58, 1.48]	[0.53, 1.36]	
Observations	130,753	128,820	128,822	128,830
Complete Set of Controls	✓	✓	✓	✓

Table 6: Capitalization and efficiency of salary spending

these coefficients. The magnitude and statistical significance of the point estimate on non-salary spending does decrease somewhat when salary spending is excluded in column (4), suggesting that any implication that increases in non-salary expenditures (with any accompanying future debt obligations) actually reduces property values is somewhat more sensitive to the exact specification.[26]

A comparison of the size of the coefficients on log local tax revenue and log salary spending provides an assessment of the efficiency of salary spending. In particular, we want to estimate the impact on house prices of a marginal dollar raised through local taxes and spent on salaries. For our sample as a whole, local tax revenue represents about 55 percent of salary spending. So, in dollar terms, a 1.0 percent increase in local tax revenues is equivalent to only about a 0.55 percent increase in salary spending. The lower panel of Table 3 reports the results of the Oates efficiency test calculated for an increase of 1%

[26]We have also estimated a version of the specification shown in Column 2 of Table 6 that breaks log non-salary spending into two components: (i) log of capital expenditures and (ii) log of all other non-salary spending (including debt payments). In this specification, the estimated coefficient and standard error on log of capital expenditures is: 0.055 (0.037), while the coefficient on the log of all other non-salary spending is negative and significant: -0.603 (0.134). Because capital expenditures are typically accompanied by future debt obligations, the precise zero estimate is consistent with the notion that these are efficiently provided.

in local taxes used to increase salary or spending. We find that a 1% increase in taxes that is spent on salaries would increase house prices by 0.95%-1.03%. Not surprisingly given the point estimates and standard errors for the coefficients, the results imply that spending on salaries is inefficiently far too low. Both of the tests reported have p-values below 0.0001. That salary spending may be inefficiently low following a school finance reform is not completely surprising. As Hoxby and Kuziemko (2004) pointed out, 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 Interpreting the Results

5.1 Household Sorting

The sharp 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 school spending levels on sorting across districts, focusing on the fraction of children in poverty in a school district as a summary measure of sorting.

We begin by looking directly at the effects of school spending and local taxes on sorting by estimating analogous specifications to a number of those reported in Tables 2 - 6 but with the fraction of children in poverty as the dependent variable. The pattern of results shown in Table 7 is remarkably consistent with the house price regressions. The first column reports the results of a specification analogous to the fourth column of Table 2, revealing that a 1% increase in overall school spending is associated with a 0.21% decrease in the school poverty rate. This effect remains largely unchanged when we control for local property taxes in column (2). In the third column of Table 7 we again disaggregate spending into salary and non-salary components and, strikingly, the entire impact of increased spending on school district composition is driven by salary spending; changes in non-salary expenditures have a negligible effect on sorting.

	Poverty		
Outcome: % Poor in District	(1)	(2)	(3)
Log(Total Spending)	-0.205***	-0.204***	
	(0.0404)	(0.0409)	
Log(Property Tax)		0.0186	
		(0.0121)	
Log(Salary Spending)			-0.255***
			(0.0650)
Log(Non-Salary Spending)			0.0120
			(0.0284)
Observations	121,483	119,705	121,466
Complete Set of Controls	✓	✓	✓

Table 7: The effect of school spending and taxes on school-level poverty.

5.2 The Direct vs. Indirect Capitalization of School Spending

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 8 repeats the earlier house price specifications reported in Tables 2-6 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 8 re-estimates our baseline specification from column (4) of Table 5 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 8 includes the fraction of children in poverty as an additional control. Columns (4) and (5) repeat this comparison with and without poverty for a specification that separates spending into salary and non-salary components.

House Prices w/ Sorting

Outcome: log(HPI)	(1)	(2)	(3)	(4)	(5)
Log(Total Spending/Pupil)	0.943***	1.787***	1.224***		
	(0.158)	(0.254)	(0.204)		
Log(Property Tax/Pupil)	-0.197***	-0.222***	-0.301***	-0.254***	-0.267***
	(0.0449)	(0.0606)	(0.0427)	(0.0592)	(0.0514)
Pct. Poverty, 5-17 yr olds			-1.493***		-1.247***
			(0.143)		(0.206)
Log(Salary Spending/Pupil)				2.483***	2.114***
				(0.454)	(0.414)
Log(Non-Salary Spending/Pupil)				-0.347**	-0.369***
				(0.155)	(0.129)
Efficiency: Salary Spending					
% ΔHPI				1.134	0.915
Confidence Interval				[0.59, 1.68]	[0.43, 1.40]
Observations	128,832	118,703	118,694	118,693	118,684
Consistent Sample (Year $\geq$ 1993)		✓	✓	✓	✓

Table 8: House price capitalization and efficiency of school spending accounting for sorting.

The results reported in Table 8 reveal a remarkably consistent pattern, with the inclusion of controls for demographic and socioeconomic composition reducing the estimated direct effect of school spending on house prices by about a third percent for overall spending (column (3) vs. column (2)) and about 15 percent for salary spending (column (5) vs. column (4)). In this way, the vast majority of the capitalization of school spending, and especially salary spending, into house prices is a direct effect of the spending, while a smaller fractions appears to be due to the sorting that occurs following the spending change. The efficiency tests for salary spending continue to imply that such spending is inefficiently far too low, with point estimates of 0.92-1.13 and p-values remaining below 0.001 levels.

5.3 Identification from Different Local Sources of Variation in the Data

In using variation across both time and the four quartiles of 1972 school spending level, the point estimates and efficiency tests reported above implicitly assume that the capitalization of school spending and local property taxes is homogeneous - i.e., the same across all districts regardless of initial spending level. One potential concern with this assumption, particularly with the efficiency tests, is that this homogeneity assumption may be masking variation in efficiency in different types of districts if, for example, 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.

House Prices: Q1-Q3 vs Q4

Outcome log(HPI)	(1) All (Q1-Q4)	(2) Q1 vs. Q2-Q4	(3) Q1-Q2 vs. Q3-Q4	(4) Q1-Q3 vs. Q4
Log(Salary Spending/Pupil)	2.291***	3.068***	2.959***	2.435***
	(0.390)	(0.475)	(0.487)	(0.449)
Log(Property Tax/Pupil)	-0.252***	-0.123***	-0.171***	-0.286***
	(0.063)	(0.067)	(0.072)	(0.104)
Efficiency Calculation				
% ΔHPI	1.029	1.592	1.484	1.075
Confidence Interval	[0.58, 1.48]	[1.07, 2.11]	[0.94, 2.03]	[0.58, 1.57]
Observations	128,832	128,832	128,832	128,832
Complete Set of Controls	✓	✓	✓	✓

Table 9: Test for heterogeneity in treatment effect by permuting the groupings of quartiles used for the variation.

To examine whether the implicit homogeneity assumption is reasonable, Table 9 reports the results of three additional specifications that 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. In particular, the first column of the table repeats our baseline results for a specification that includes salary spending and taxes using the full variation across quartiles, while the final three columns report results that only use variation among districts above and below the 25th, 50th, and 75th percentiles, respectively. Put another way, these specifications group different combinations of the original quartiles together to examine how isolating variation on different margins affects the parameter estimates.

Remarkably, there is little change in the coefficient estimates across the three specifications reported in Columns (2)-(4). The point estimates on both salary 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 9. The efficiency tests are also similar across specifications, strongly rejecting the efficiency of salary expenditures in all specifications.

5.4 Can Households Anticipate Future Spending Changes?

Another issue that naturally arises in estimating house price regressions is whether households may be able to anticipate future changes, as a result, 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, an easy 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 and fourth columns of Table 10 replace all of the right hand side variables (including controls) with their one year ahead leads. Because estimating this specification means that we are unable to include the final year of the sample in the specification, columns (1) and (3) re-estimate our baseline specifications dropping observations from the year 2015. The results of including leads actually increases the point estimates for overall spending in the baseline specification. Most importantly, however, the results of including leads have almost no impact on the specifications that dis-aggregate spending into salary and non-salary components and the resulting Oates test is almost identical in the leads specification. Thus, it does not appear that ignoring forward-looking behavior is a first-order concern for our main analysis.

House Prices: Forward Looking

	(1)	(2)	(3)	(4)
Log(Total Spending/Pupil)	0.949***	1.414***		
	(0.158)	(0.189)		
Log(Salary Spending/Pupil)			2.291***	2.270***
			(0.390)	(0.406)
Log(Property Tax/Pupil)			-0.252***	-0.233***
			(0.063)	(0.057)
Efficiency Calculation for Salary Spending				
% ΔHPI			1.029	1.036
Confidence Interval			[0.58, 1.48]	[0.56, 1.51]
Observations	128,832	128,832	128,832	128,832
Dependent Variable is a Lead (t+1)	X	✓	X	✓
Complete Set of Controls	✓	✓	✓	✓

Table 10: Test for heterogeneity in treatment effect by permuting the groupings of quartiles used for the variation.

6 Conclusion

The main goal of this paper is to provide new evidence on two longstanding questions in economics: whether school spending matters and whether it is provided at an efficient level. To answer both questions, we turn to the housing market, examining how school spending and taxes are capitalized into house prices. To estimate the causal impact of spending and taxes on house prices, we exploit plausibly exogenous variation in school spending and local taxation resulting from court-ordered school finance reforms (SFRs) paired with panel data on local house prices over a 25 year period. In addition to providing independent variation in school spending and local taxation, a key advantage of this SFR event study design is that the resulting estimates are based on a national sample of school districts rather than a single state or metropolitan area.

We begin our study by taking up the question of whether exogenous increases in school spending are capitalized into house values. While the answer to this question may seem obvious, a strand of the literature since Hanushek (1986) has argued that the value of school spending on the margin is close to zero. We instead find strong evidence that an exogenous increase in school spending is sharply capitalized into housing prices, implying that households place a high value on marginal school spending. This result is in line with the substantial benefits of school spending on the lifetime outcomes of children estimated in Jackson et al. (2015). Strikingly, house prices are sharply increasing in spending on salaries and actually slightly decreasing in other forms of spending. We also find that the combination of higher house prices and increased school spending affects who sorts into a school district. The vast majority of school spending capitalization, however, is due to the direct effect of school spending rather than any indirect effect related to sorting.

We next take up the question of whether school spending is efficient. Since Oates (1969), economists have argued that if local public goods are provided efficiently, a marginal dollar raised through local taxes and spent on local public goods should have no effect on house values. While this implication is theoretically straightforward, testing it empirically has proven difficult, as local tax and spending levels are often highly correlated with the local socioeconomic composition of the district and/or local economic conditions. To address this question, we take advantage of the fact that court ordered SFRs resulted in changes in school spending through multiple channels, both increasing redistribution at the state level and changing incentives to raise revenue at the local level.

Because some components of non-salary spending, e.g. capital expenditure, are typically funded by bond referenda and include a future tax liability, the house price capitalization of non-salary spending provides a direct test of the efficiency of that form of spending. By contrast, to test for the efficiency of salary spending, we estimate the independent causal effects of salary spending and local taxation on house prices. Our results indicate that a dollar raised through local taxes and spent on salaries has a positive and statistically significant impact on local house prices, which implies that school spending on salaries in the US is inefficiently low. We further find that both increases in salary expenditure per teacher and teachers per student are capitalized into higher house prices, suggesting that parents value both the increased quality and quantity of school personnel made possible by higher spending on salaries.

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 salary spending measured in our paper is potentially a lower bound on what is possible with greater spending on salary. Both a national and international comparison of teacher pay in the US is suggestive of why the efficiency gains from greater teacher pay are potentially so large. Compared to similarly credentialed workers in the US, teachers experience a 22% pay gap, the second largest among 23 peer countries (Hanushek et al., 2019).

Finally, it is important to point out that the analysis of the provision of public school spending in this paper examines the efficiency of current spending levels taking into account only the private returns to households and their children. Any broader social and civic returns to education as well as concerns about the equitable provision of educational opportunities would further raise the value of increased spending on school personnel, especially in relatively poor and low-spending districts.

References

BARON, E. J. (2019): "School Spending and Student Outcomes: Evidence from Revenue Limit Elections in Wisconsin," .

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.

BIASI, B. (2017): "School finance equalization and intergenerational mobility: A simulated instruments approach," NBER Working Papers: No. 20983.

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

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

BRUNNER, E., J. HYMAN, AND A. JU (2019): "School Finance Reforms, Teachers' Unions, and the Allocation of School Resources," Forthcoming: Review of Economics and Statistics.

CALHOUN, C. A. (1996): "OFHEO House Price Indexes: HPI Technical Description," .

CARD, D. AND A. A. PAYNE (2002): "School finance reform, the distribution of school spending, and the distribution of student test scores," *The Quarterly Journal of 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.

COLEMAN, J. S., UNITED STATES, OFFICE OF EDUCATION, AND NATIONAL CENTER FOR EDUCATION STATISTICS (1966): *Equality of educational opportunity*, U.S. Dept. of Health, Education, and Welfare, Office of Education : [For sale by the Superintendent of Documents, U.S. Govt. Print. Off., OCLC: 2806374.

HANUSHEK, E. A. (1986): "The economics of schooling: Production and efficiency in public schools," *Journal of Economic Literature*, 24, 1141–1177.

——— (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.

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.

HYMAN, J. (2017): "Does money matter in the long run? Effects of school spending on educational attainment," *American Economic Journal: Economic Policy*, 9, 256–280.

JACKSON, C. K. (2018): "Does School Spending Matter? The New Literature on an Old Question," NBER Working Papers.

JACKSON, C. K., R. C. JOHNSON, AND C. PERSICO (2015): "The effects of school spending on educational and economic outcomes: Evidence from school finance reforms," *Quarterly Journal of Economics*, 131, 157–218.

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.

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.

LOVENHEIM, M. AND S. TURNER (2018): *Economics of Education*, Worth Publishers.

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

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.

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.

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

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

Online Appendix: A National Study of Public School Spending and House Prices

District House Price Aggregation and Coverage

This section describes the construction of our measure for district-level house prices from 1990-2015. The underlying data are a census tract×year panel of weighted indices $\tilde{p}_{j,t}$, measuring average price changes in repeat sales or refinancings on the same properties relative to a tract-specific base year.[27] There are two hurdles to obtain district×year outcome $\mathbf{P_{d,t}}$ in our main estimation. Since the base year varies for each tract j, we must first choose a new base year that is consistent across all tracts in a district. This will parse out within-district differences in HPI harming aggregation purely due to differences in tract base years. We must also population weight out census tract measures of house prices to obtain district level house prices.

We first convert all tract prices to base year 2003, the sample year with maximum data coverage:

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

Within each district there are **J** census tracts. We weight the tract indices by the 1990 tract decennial population, $n_{j,90}$ as a share of the 1990 district aggegate population

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

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

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

Figure 7 is a binned scatter plot of the mean district price $\mathbf{P_{d,t}}$ and tract raw price $\tilde{p}_{j,t}$.

[27] https://www.fhfa.gov/DataTools/Downloads/Pages/House-Price-Index.aspx

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 purely due to differences in base years.

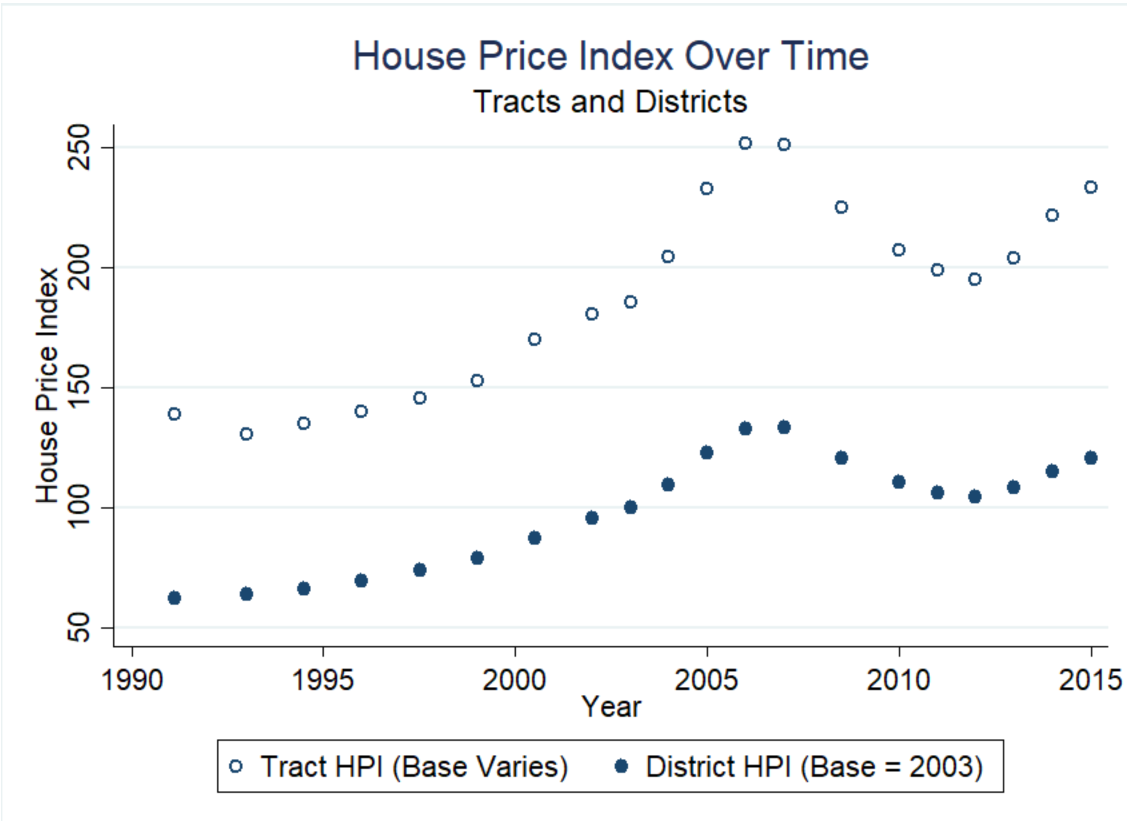

Figure 7: Weighted average district house prices as compared to underlying tract prices.

We do not require the tract panel be fully balanced throughout the sample period. This could bias the aggregation step if missing tract-level observations create inter-temporal differences in $\mathbf{P_{d,t}}$ unrelated to real price changes. To proxy for this, we measure district *coverage* as the share of district residents in a tract with reported house prices. House price coverage in a district is defined as

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

where $\mathbf{1}(\mathbf{P_{j,t}}) = 1$ if tract HPI is observed in year t. Figure 8 is a plot of the mean coverage for a district during the sample period. As the tract-level price data improves in later years, district coverage improves to 90% on average.

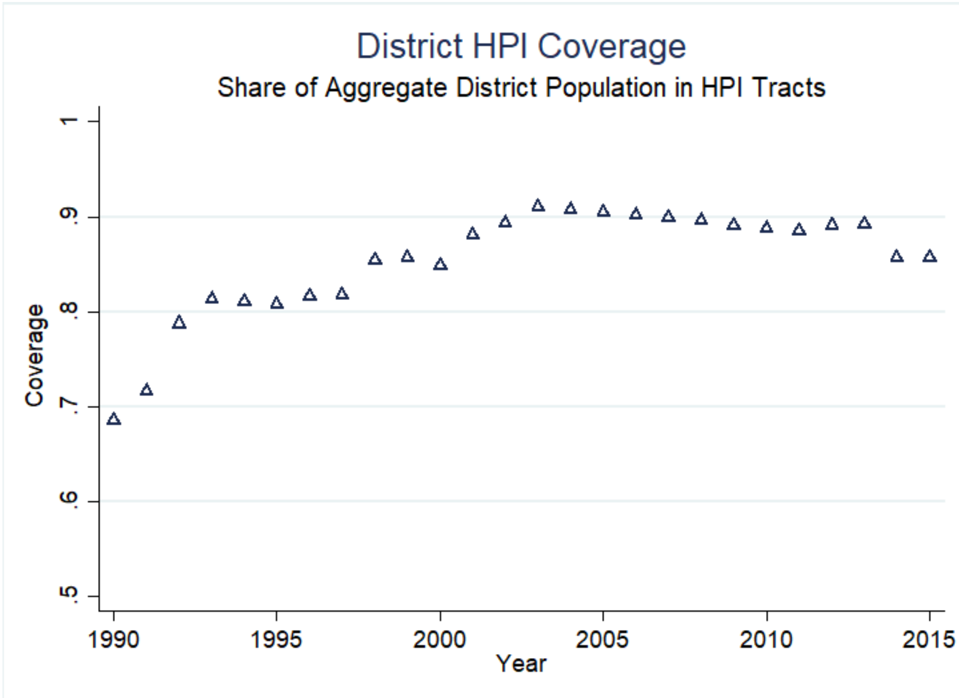

Figure 8: District Coverage: Fraction of aggregate 1990 district population in a tract reporting HPI.

High Cost Housing Areas

Fannie Mae and Freddie Mac are restricted from purchasing mortgages above a conforming loan limit (CLL). As the house price index tracks house sales from Fannie Mae and Freddie Mac backed loans, the index could be biased by the exclusion of particularly high priced house sales. Further, a 2008 program change allowed for the loan limits to be 50% higher in certain high-cost areas of the contiguous US. High-cost areas can be found within California, Colorado, Connecticut, District of Columbia, Florida, Georgia, Idaho, Maryland, Massachusetts, New Hampshire, New Jersey, New York, North Carolina, Pennsylvania, Tennessee, Utah, Virginia, Washington, West Virginia, and Wyoming.

Loan limits will bias our main estimation if high-cost areas face binding loan limits prior to the change in 2008, as the house price index would mechanically increase after 2008 as higher priced house sales are included. In our robustness checks we proxy for the likelihood of house sales facing binding loan limits with 1990 census counts of owner-occupied housing within various price bins for census tracts and counties. We target the fraction of owner-occupied housing valued over $250,000 within a 1990 census area as a crude measure of the potential for exposure to high cost loan limits.

SFR Timing and Michigan Reforms of 1994

The reform timing of our paper follows that of Jackson et al. (2015), save the timing of reforms in Michigan. In the Jackson et al. (2015) paper, 1997 is treated as the initial year of court-ordered reform. We treat 1994 as the initial reform year, as two major legislative reforms were passed that directly affected property taxes and equalization. This also differs from Lafortune et al. (2018), who consider Michigan a non-reform state. In figures 9 and 10 we plot property tax and state revenues per-pupil, respectively, around the year 1994.

The Michigan reforms were complex. 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 9 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 is an example of the variation we desire to isolate in our understanding of house price responses to exogenous changes in funding.

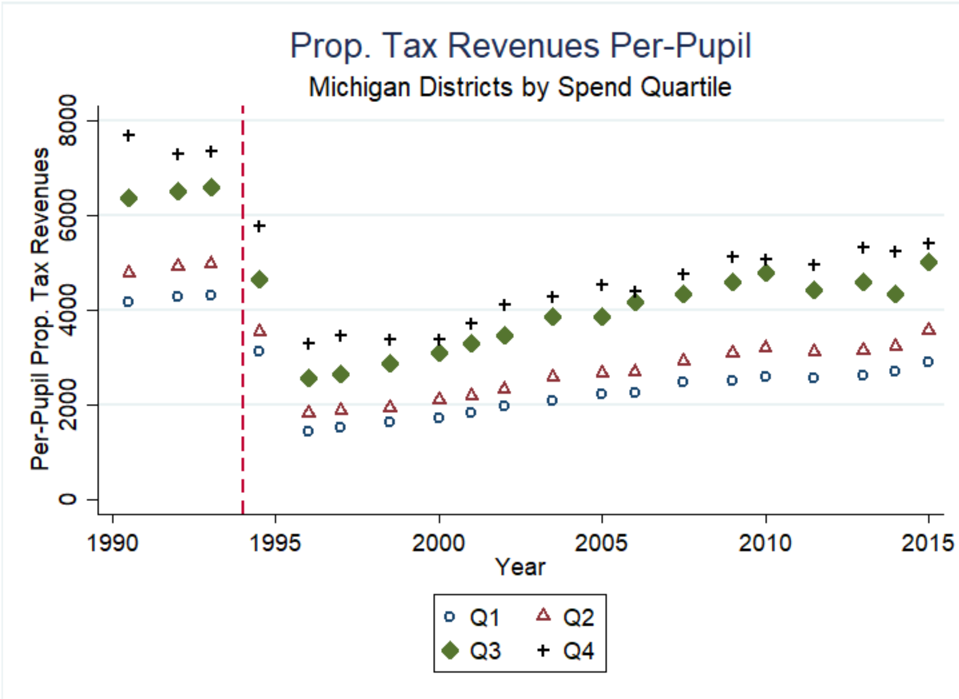

Figure 9: Property Tax Revenue Per-Pupil in Michigan

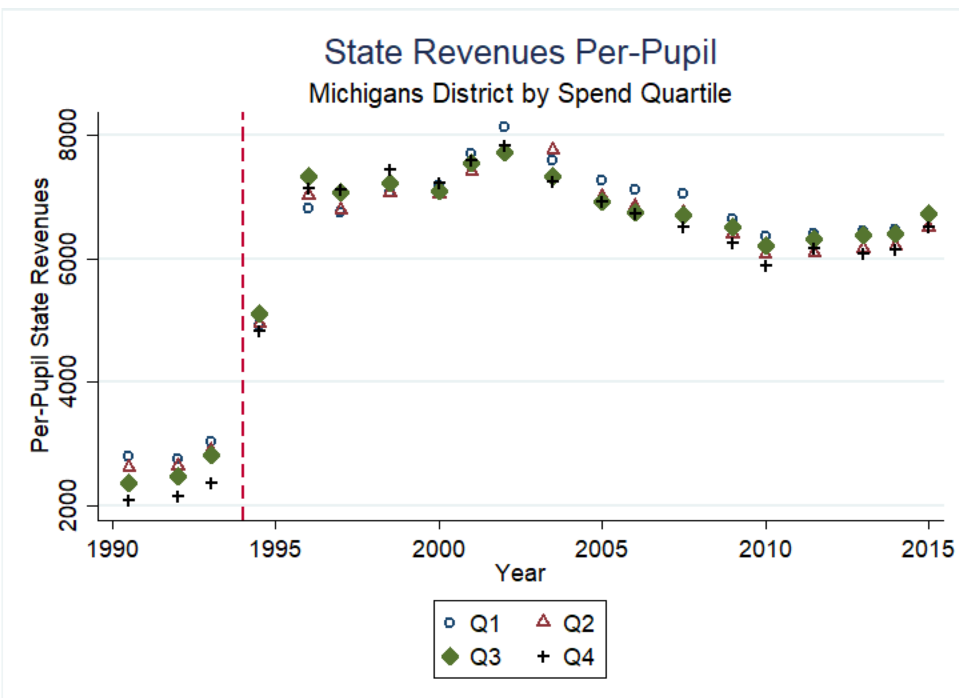

Figure 10: State Revenue Per-Pupil in Michigan.