Is Spending on Schools Valuable and Efficient? A National Study of the Capitalization of School Spending and Local Taxes*

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Abstract

Despite the tremendous amount of money spent on education in the US, it remains an open question whether the level of education spending is adequate, too little or too much? To answer this question, we estimate how much parents value school expenditure and their willingness to finance it through higher taxes. We accomplish this by exploiting plausibly exogenous variation in school expenditures and local taxes arising from School Finance Reforms. We find that school expenditures are positively capitalized into house prices ($\epsilon = 0.86$), i.e. households value more spending on schools. Taxes, conversely, are negatively capitalized into house prices ($\epsilon = -0.17$). At the efficient level of education expenditure, a 1% increase in taxes to fund schools would yield no change in house prices. We find evidence that education is efficiently funded. According to our estimates, a 1% increase in taxes to fund education increases house prices by an economically small and statistically insignificant 0.06%. This provides empirical support for a core prediction of the Tiebout (1956) hypothesis that decentralized jurisdictions can efficiently provide local public goods like education. Decomposing this result by geography, we find no gains in rural areas (<0.008%), small gains in suburban areas (0.02%) and the largest gains in urban areas (0.19%). While these estimates are all statistically insignificant, the point estimate for urban districts is an order of magnitude larger than those for rural and suburban areas, suggesting that if education is under-funded anywhere, that it is under-funded in urban school districts.

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1 Introduction

Expenditures on education in the U.S. represent 7.3% of GDP. A key component of this expenditure is investments in K-12 education, which is partially funded by local property taxes. The use of local taxes as a funding mechanism makes the U.S. context a powerful laboratory for studying education as an investment, when compared to its OECD peer countries, where education is primarily funded through the central government. To the extent that expenditures on schools are valued by households, these expenditures should be capitalized into house prices. This is a direct implication of Tiebout (1956). This capitalization of school expenditures into house prices is therefore a revealed preference estimate of the value of investing in education. Likewise, the negative capitalization of local property taxes into house prices is a revealed-preference estimate of the cost-side of investing in education. The efficient production of education requires equalization between the benefits and cost of education on the margin (Oates, 1969).

The most compelling estimates of house price capitalization in the market for education employ a boundary discontinuity design (BDD) in which researchers attribute differences in house prices across school attendance boundaries to differences in the average test scores across the attendance boundaries (Black 1999; Bayer et al. 2007; Kane et al. 2006). To obtain a clean estimate of how much households value test scores the BDD holds constant: housing characteristics, neighborhood characteristics, school spending, and taxes.

While the BDD has contributed fundamentally to our understanding of the willingness to pay for higher test scores, it has three important limitations when it comes to fully understanding education as an investment good and testing whether expenditures on schooling is efficient. First, the boundary discontinuity estimates in the literature do not provide a direct measure of the capitalization of school expenditures into house prices. Because households vote over school expenditures, obtaining reduced form estimates of how much households value school expenditures is the direct object of policy interest, as opposed to test scores (Cellini et al., 2010). While increased school spending increases test scores, it also affects long-term outcomes like adult wages and employment, as showed by Jackson et al. (2015); hence, boundary discontinuity measurements of test score capitalization into house prices may understate the full return on investing in education.[1]

Second, because the BDD holds both school spending and taxes constant, it cannot be used to measure the reduced-form elasticities of school spending or local taxes on house prices, both of which are necessary ingredients for testing for efficiency of school expenditures.

Third, the external validity of most BDD studies in the literature is limited because these studies focus on a narrow geography at a single point in time (Black 1999; Bayer et al. 2007; Kane et al. 2006). For example, Black (1999) relies on house price data from 3 suburban counties in Massachusetts (Middlesex, Essex, and Norfolk) over a 3-year period (1993-1995). Likewise, Bayer et al. (2007) relies on restricted-used census data and house prices transaction data from 6 counties in the San Francisco Bay Area for a single year (1990). The narrow geographic and temporal focus of BDD studies is a natural consequence of the extensive data requirements of this empirical design. Moreover, this methodology is not well-suited to areas that are sparsely populated, which includes most rural counties in the US and some suburban areas.

In this paper, we overcome the core limitations of the BDD by pairing annual sub-school district data on house price indices from the Federal Housing Finance Agency (FHFA) with a source of plausibly exogenous variation in school expenditures and local taxes. Our instrument for school expenditures leverages variation in the timing of the court-mandated school finance reforms (SFRs), which rolled out across 25 states over a 39-year period (1971-2009). Jackson et al. (2015) persuasively demonstrated that these court mandated SFRs induced plausibly exogenous variation in school funding that shrunk

[1] There is also a large literature on test score fade-out which suggests that test score impacts are at best transitory and as such may not be of great interest in and of themselves in evaluating the effectiveness of educational interventions (Elango et al., 2016).

the gap in per pupil expenditure between school districts in the top and bottom quartiles of the pre-reform distribution of school expenditure. We provide crisp evidence that the court mandated SFRs also induced variation in tax revenue that is not collinear with the changes in per pupil expenditure. Hoxby (2001) makes this point theoretically, arguing that SFRs that increase redistribution created incentives for richer school districts to shrink their tax base. Hoxby and Kuziemko (2004) also provide empirical support for this insight using the 1994 SFR in Texas. We leverage the school spending and local tax variation induced by a national sample of SFRs to jointly estimate the elasticity of house prices with respect to school spending and taxes, and then use these two elasticities to test for efficiency in the level of expenditures on schooling.

We employ an event study design to show that house prices appreciated in school districts where per-pupil expenditure increased because of redistribution due to the court-mandated SFRs. In our first empirical specification, we regress the log of the FHFA house price index on the log of per pupil school expenditures in the census tract, instrumenting for the per pupil expenditure with SFR event-time dummies that are interacted with the school district's pre-reform quartile in the per pupil expenditure distribution. We find an elasticity of housing price appreciation with respect to school expenditures of 0.86. As predicted by economic theory, we find that the house price capitalization is greater in places where the housing supply is less elastic (Cellini et al., 2005; Gyourko et al., 2013). Accordingly, the elasticity of house prices with respect to school expenditure is largest in urban areas ($\epsilon = 1.1$) and smallest in rural areas ($\epsilon = 0.8$).

Consistent with the SFRs increasing house prices, we find that the fraction of poor students in a school district declines as a result of these exogenous increases in school expenditure. Some low-income households are priced out of these neighborhoods. While these reforms do induce sorting, we find that the house price capitalization effects in our paper are primarily driven by the direct effect of increased school expenditures rather than the secondary sorting effects (Lafortune et al., 2018). Controlling for the fraction students who are: poor, black, white and Hispanic in the district, we find that our house price capitalization elasticity falls by just 17%, *i.e.* from $\epsilon = 0.86$ to $\epsilon = 0.71$.

To test for whether expenditures on schools is efficient, we augment our specification to include the log of tax revenues as an explanatory variable. We also instrument the log of tax revenues using the event-time dummies interacted with the dummy variables for pre-reform expenditure quartiles. We find that a 1% increase in school expenditures increases house prices by 0.78%, whereas a 1% increase in tax revenue decreases house prices by 0.17%. Tax increases are most negatively capitalized in suburban areas and rural areas ($\epsilon = -0.21$). Our estimates suggest that, increasing tax revenues by 1% in order to increase expenditure on schools would increase house prices by 0.06% (p-value 0.66), on average. The current level of education spending is therefore efficient, overall. This can be seen as a triumph of the Tiebout model (Tiebout, 1956; Oates, 1969).

Disaggregating the efficiency estimate of education spending by geography, we find that a 1% increase in taxes to fund schools would increase house prices in urban areas by 0.19%, which while statistically insignificant (p-value: 0.26) is 3 times the average across all geographies and nearly an order of magnitude larger than the size of the capitalization effects in suburban and rural areas. This suggests education expenditures may be inefficiently low in urban areas. Performing the same efficiency calculation in suburban and rural areas yields both a statistical zero and an economically trivial impact of increased taxes to fund schools on house prices. The point estimate is 0.02% (p-value = 0.86) in suburban areas and 0.0008% (p-value = 0.99) in rural areas. Correspondingly, the level of education expenditures in suburban and rural areas is approximately efficient.

While education is funded efficiently on average, the allocation of school expenditures across educational inputs may be suboptimal. To test for optimality in the allocation of educational inputs, we separate expenditures into two broad categories – salary expenses and non-salary expenses and regress the log of the FHFA home price indices on the log of salary and non-salary spending. We find that expenses on salary are highly capitalized into house prices. A 1% increase in expenditures on salaries increases house prices by 2.2%. Moreover, this large price effect is nearly uniform across geography. Non-salary expenditures, by contrast, are negatively capitalized to prices with an elasticity of -0.8. This suggest two things. First, the optimal mix of salary to non-salary expenditure would favor more expenditures on salaries and less expenditures on other inputs relative to the status quo. Second, households are discriminating in what types of school spending that they value. Given the evidence that increased expenditures on teacher salaries from SRFs increased student performance more than non-salary expenditures it also appears that households are rational (Brunner et al., 2019).

Our paper speaks to a longstanding debate in economics on whether there exists a decentralized system for efficiently providing public goods. Musgrave (1939) and Samuelson (1954) argued that such a system does not exist and as a result public goods would be subject to under-provision because of the free-rider problem. Tiebout (1956) argued that while this may be true at the federal level, that at the local level, inter-jurisdictional competition over the level of public goods provided and the corresponding taxes levied to provide these public goods could lead to efficient matching between households and jurisdictions, given a sufficiently thick market of jurisdictions, low mobility cost, among other assumptions. A natural implication of the efficient allocation of public goods in a system of local governments, which motivated Oates (1969) and many subsequent empirical studies,[2] is that an additional dollar raised locally and spent on a local public good should have no effect on property values.[3] If, instead, such an investment increased demand to live in a community (and thus raised property values), the level of local goods provision was inefficiently low. Many early empirical studies of this efficiency condition suffered from a host of endogeneity problems; while more recent papers, for example Cellini et al. (2010) which use a credible identification strategy are done in the context of

[2]See Church (1974) and Rosen and Fullerton (1977).

[3]Important theoretical contributions to this literature include Edel and Sclar (1974), Sonstelie and Portney (1978), Wildasin (1979), Yinger (1982), Brueckner (1982) and Epple and Zelenitz (1984).

a specific state and focus on a particular subset of school expenditures – infrastructure spending. Our main contribution is to study this 50-year old question in an empirical setting that provides plausibly exogenous variation in both local tax revenues and school spending that covers a national geography.

Our paper also contributes to several lines of the literature on court-ordered school finance reforms. Murray et al. (1998) and Card and Payne (2002) showed that these reforms reduced inequality in school spending across districts and subsequent work including Jackson et al. (2015), Lafortune et al. (2018), Hyman (2017), Brunner et al. (2019), and Biasi (2017) provide evidence of an impact of the corresponding school spending changes on long term life outcomes, student test scores, and economic mobility. Our paper contributes most directly to this literature by showing that parents and other residents of local communities observe and highly value these changes in spending. In addition to the implications of the resulting capitalization and sorting for schools and cities, the size of these effects on property values provides a form of corroborating evidence for the large effects of spending on life outcomes estimated in Jackson et al. (2015). In the tradition of Hoxby and Kuziemko (2004), our paper also exploits the fact that many SFRs changed the incentives that local governments had to raise revenue through local taxes. We use this variation to simultaneously estimate the hedonic value of both school spending and local taxes, thereby providing a modern test for efficiency of school spending that follows in the tradition of (Oates, 1969) and (Cellini et al., 2010).

Finally, our paper contributes to the economics of education literature on what types of school spending matter. Hanushek (1986) provides an early summary of this literature, which is built on by more recent papers by Cellini et al. (2010), Lavy (2015), Brunner et al. (2019), and Martorell et al. (2016) who measure the returns to educational expenditure on both teachers and school infrastructure.

2 Data

We combine data from several sources to form a balanced panel of school expenditure and house price data covering the years 1990-2015.

2.1 Local House Price Indicies

Our data on home values come from a house price index (HPI) that is constructed by the Federal Housing Finance Agency (FHFA) using a national data set on conventional mortgages that covers the 50 states and the District of Columbia. The mortgage data underlying the HPI is collected by FHFA from Fannie Mae and Freddie Mac, the two government sponsored enterprises with a congressional mandate to increase home ownership in the US through providing a secondary market for conventional mortgages. A key feature of this HPI is that it is a "constant quality" index. To control for quality, the index tracts changes over time in the price of houses that have been sold or refinanced multiple times. Moreover, the index employs a weighting procedure that allows for greater sampling variability in the price appreciation for houses that experience a longer time between transactions.[4] Both the repeated-sales nature and the weighting scheme that is proportional to the transaction time following the weighted repeat-sales (WRS) index methodology developed in Case and Shiller (1989).

For our purposes the FHFA HPI has two distinct advantages relative to the Case Schiller Index. First, FHFA HPI varies at the census tract level, whereas the Case-Schiller HPI varies at the metro-level. A census tract is a geographic area that is with a population of on average 4,000 people. Because we are using with-in state variation in school expenditures to estimate the house price capitalization of school expenditures, the local

[4] As noted in Calhoun (1996), given two identical properties, differential rates of appreciation, change in the neighborhood socio-demographics and other idiosyncratic deviations from market-level mean appreciation are more liable to arise the longer the time between transactions. This motivates using a generalized least squares weighting procedure in which the variance in house price appreciation is quadratic in the time between consecutive transactions for a given property.

variation in the FHFA HPI allows us to more precisely estimate the house price capitalization effects. Second, the Case-Shiller Indices does not have house price data from 13 states, whereas the FHFA has data on all states. The Case-Shiller indices do not include data from: Texas, Idaho, Indiana, Kansas, Louisiana, Maine, Mississippi, Missouri, Montana, New Mexico, North Dakota, Utah and Wyoming because these states do not require the disclosure of house sale prices on deeds.

The FHFA HPI has two distinct weaknesses relative to the Case-Shiller Index. First, the Case-Shiller Index uses data from the county assessors and recorders offices, which includes all transactions, whereas the FHFA HPI only includes data on conforming mortgages, i.e. mortgages below a certain cut-off value and above a certain loan to income value (LTV). As of 2019, the conforming limit in expensive markets coastal housing markets is a loan value of $726,525 and the minimum LTV is 3%.[5] Relatedly, the Case-Shiller Index value-weights the house prices, whereas the FHFA HPI weights housing transactions observations equally. This suggests that the FHFA HPI will be an under-estimate of the HPI and especially in more expensive census tracts (Cellini et al., 2005). The second disadvantage of the FHFA HPI relative to the Case-Shiller Index is that includes observations based on refinancing, which are based on appraisals rather than transaction prices. The Case-Shiller index only includes purchase prices.

By using the FHFA HPI rather than the Case-Shiller Index we are trading off the use of better house price variation in the FHFA (due to the superior coverage across states and finer level of geographic reporting of the HPI within state) against having estimates that are measured with more noise (due to the limits deriving from the loan cut-offs imposed by Fannie Mae and Freddie Mac and the choice to include house price appraisal values from the refinancing). We are prepared to make this trade-off because we believe that the gains in precision from having better geographic variation in house prices that pairs with the geographic variation in the school expenditure outweighs the greater potential

[5] The conforming limit is $484,350 in the least expensive housing markets.

for measurement error. Moreover, to the extent that the measurement error varies across space and time in systematic ways, the school district fixed effects and time fixed effects in our regressions will absorb some of the systematic variation in the measurement error, hopefully leaving us with just classical measurement error, which would bias against us finding house price capitalization of school expenditures.

In addition to these price data we use measures of housing supply elasticity and indicator variables for whether a census tract is located in an urban, suburban or a rural area to test for heterogeneity in the capitalization of schools spending and local taxes into house prices. The supply elasticities are from Saiz (2010) and are constructed to capture the metropolitan area (MSA) supply elasticity as a function of the regulatory environment, terrain and the availability of land for development.[6] In Table 1, we provide summary statistics for the house price data, the census tract geography data and the other sources of data that we use in the project which we will describe in subsequent sections.

2.2 Court Mandated School Finance Reforms

Our coding of the court mandated SFRs follows the list Jackson et al. (2015). As showing in Table 2, the first court decision was the Serrano v. Priest decision in California, where the Justice Sullivan, writing for the majority, ruled that the funding mechanism of public education through local property taxes violated the equal protection clause of the state's constitution:

> ”We are called upon to determine whether the California public school financing system, with its substantial dependence on local property taxes and resultant wide disparities in school revenue, violates the equal protection clause of the Fourteenth Amendment. We have determined that this funding scheme invidiously discriminates against the poor because it makes the quality of a

[6] Zoning and regulatory constraints are measured by the Wharton Regulatory Index and the elasticity is representative of the year 2010. Saiz (2010) uses GIS technology to recovery estimates for available land supply and the fraction of the land available for development.

Table 1: Summary Statistics

	mean	sd
Housing Variables		
HPI (1990)	162.55	53.55
Supply Elasticity	1.61	0.96
Urban	0.32	0.47
Suburban	0.62	0.49
Rural	0.06	0.23
District Finance Data ($ 2015)		
Total Expenditures	11,728	4022
Teacher Salary Expenditures	6,305	1928
Capital Expenditures	1,127	1464
Property Tax Revenue	4,294	3426
State Revenue	5,871	2577
In a Reform State	0.60	0.49
District Socio-Demographics		
Frac. In Poverty	0.13	0.09
Frac. Black	0.15	0.19
Frac. Hispanic	0.17	0.21
Frac. White	0.61	0.29
1960 Census Controls, County Level		
Frac. In Poverty	0.09	0.05
Frac. Minority	0.04	0.05
Frac. Rural Population	0.10	0.09
District$\times$Year Observations	184,825	
Tract$\times$Year Observations	405,550	

child's education a function of the wealth of his parents and neighbors. Recognizing as we must that the right to an education in our public schools is a fundamental interest which cannot be conditioned on wealth, we can discern no compelling state purpose necessitating the present method of financing. We have concluded, therefore, that such a system cannot withstand constitutional challenge and must fall before the equal protection clause." (Serrano v. Priest, 1971)

The first wave of challenges to school financing through local property taxes followed the script of Serrano, ushering a wave of "equity reforms" which focused on equalizing funding between school districts. As reported in Lafortune et al. (2018), the second wave of school finance reforms, initiated by the Kentucky State Supreme Court decision in Rose vs. Council for Better for Education (1989), was predicated on a constitutional right to the provision of an adequate level of education for children in all parts of the state.

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	VT	1997
WY	1980	OH	1997
AR	1983	MI	1997
MT	1989	ID	1998
TX	1989	NY	2003
KY	1989	SC	2005
		OR	2009

Table 2: List of SFRs by state and year that were mandated by state supreme courts.

2.3 District Finance & Demographic Data

The school finance data are publicly available and come from the National Center for Education Statistics (NCES) and includes total expenditures and revenues for individual school districts each year. Along with the total expenditures and revenues, we can also observe finer level data describing the source of revenue (federal, state, and local) along with the channel of expenditures (teacher salaries, capital and construction expenses). In addition to the annual data, we also use district level finance data from 1972 provided by the US Census Historical Database on Individual Government Finances to form the pre-reform spend quartiles we use to identify heterogeneity in the effect of state reforms on per pupil spending.

We use GIS to match US census tracts to school district boundaries with shapefiles publicly available through IPUMS National Historical GIS shapefiles. At the tract level we observe the house price index annually as a constant quality measure of average price appreciation. We balance the final panel by tracts where the price index is observed each year from 1990 to 2015. The final panel consists of nearly 390,000 census-tract-by-year observations from 35 states and roughly 6,300 US school districts. Additional time varying data describing student body race and poverty levels are included in the main specifications, along with fixed county level descriptive variables from 1960 to control for pre-existing conditions related to current levels of school spending as well as house prices.

Districts in our sample on average are majority white, with 13% of students living in households at or below the poverty line (Table 1). Along with these time varying descriptors we account for the fact that historical factors can influence the relationship between spending and prices observed in recent periods. In our robustness tests we control for county level measures of the poverty rate, minority share, and rural population share in 1960 under the implication these measures would otherwise influence estimates of the main effect.

3 Empirical Strategy

3.1 Background and Endogeneity Concerns

Because households sort across local jurisdictions and local taxation has historically played a major role in the funding of K-12 schools in the United States, estimating the capitalization of spending into property values has proven to be an especially challenging endeavor. 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, which might impact local housing prices directly. Even more directly, the level of local school spending is highly correlated with composition of the community itself, which also might affect property values in any number of direct and indirect ways. Another generic complication that arises in a world of primarily local school funding 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 any bundled spending and tax increases. This can severely negatively bias estimates of the value that households place on the school spending itself.[7]

When financing moves to higher levels of government a host of additional endogeneity issues arise. In general, because transfers from the state and federal government are often explicitly tied to a district's property tax base and other local economic conditions, state and federal funding may be highly correlated with many factors that directly influence a district's property values.

[7] We discuss the issue of local taxation and the related literature in local public finance in greater detail later in this section.

3.2 School Finance Reform Event Study Design

Our empirical strategy relies on an event study design based on the timing of court-mandated school finance reforms. As in Jackson et al. (2015), the key identifying assumption is that conditional on a state ever passing a school finance reform, the timing of the court order to do so is as good as random.[8] Our primary specification consists of an instrumental variables regression of house prices on per pupil expenditures, using the school finance reform events to construct instruments for per pupil expenditures. These instruments are based on a school district's initial quartile of the school expenditure distribution within its state crossed with the time since the court ordered SFR. As shown in Figure 1, 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.

The primary goal of most SFRs was to reduce spending inequality between school districts with low per pupil expenditure and school districts with high per pupil expenditure.[9] Following Jackson et al. (2015), we sort school districts in a state into per pupil expenditures by quartile of per pupil expenditure in 1972.[10] We also create event time dummies that run from $T = -20$ to $T = 20$, where event time $T = 0$ corresponds to the time of the first court-mandated SFR.[11] As in Jackson et al. (2015) our instrument for per pupil expenditure is the event time dummies interacted with the 1972 spending quartiles. We also add in district fixed effects and linear trends that vary with pre-reform characteristics.[12] In this way, our first stage regression specification is given by:

[8] More precisely, we require that the timing of a court-ordered SFR is as good as random after controlling for pre-trends in school expenditure and other trend controls.

[9] States use a wide variety of approaches including block grants, matching grants, and district power equalizations, to accomplish this goal. See Murray et al. (1998), Hoxby (2001), and Card and Payne (2002) for more a greater discussion of the impact of SFRs on expenditures and other outcomes.

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

[11] One could use subsequent reforms to serve as additional events as done in Lafortune, Rothstein and Shanzenbach, but we following Jackson et al. (2015)'s design in this paper.

[12] $X_{d,60}$ is a vector of 1960 county level measures of population, poverty rate, percent black, and percent rural.

$$\boxed{PPE_{d,t} = \left[\sum_{T=-20}^{T=20}\sum_{Q_{72}=4}^{Q_{72}=1} \lambda_{Q,T} 1(Q) \times 1(T)\right] + f_d + \beta X_{d,60} \times t + \epsilon_{d,t}}$$

where:

- f_d: district fixed effects

- $X_{d,60} \times t$: time trends in 1960 pre-reform characteristics

Figure 1 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 had the highest level of spending, conditional on district fixed effects and trends in pre-reform characteristics, following the strategy in Jackson (2018).

From the perspective of our empirical event design strategy, an important feature of Figure 1 is that there is essentially no difference in trends in school expenditures across the four spending quartiles prior to a school finance reform. This 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.

After an SFR event, spending increases steadily in the lower pre-reform spending quartiles relative to the top quartile, with school districts in the lowest quartile experiencing the largest gains in expenditure. In fact, the increase in expenditure is monotonically increasing in how low the pre-reform spending quartile is. In practice the lag in the full realization of the reforms reflects the time it takes for the state legislatures to craft policy governing the new school financing system following a court order, delays in full implementation of the reforms, and the speed of other adjustments at the state and local levels to the new regime.

To estimate the impact of school spending on prices and other outcomes, like the fraction of school children in poverty, we regress the outcome of interest on the fitted value

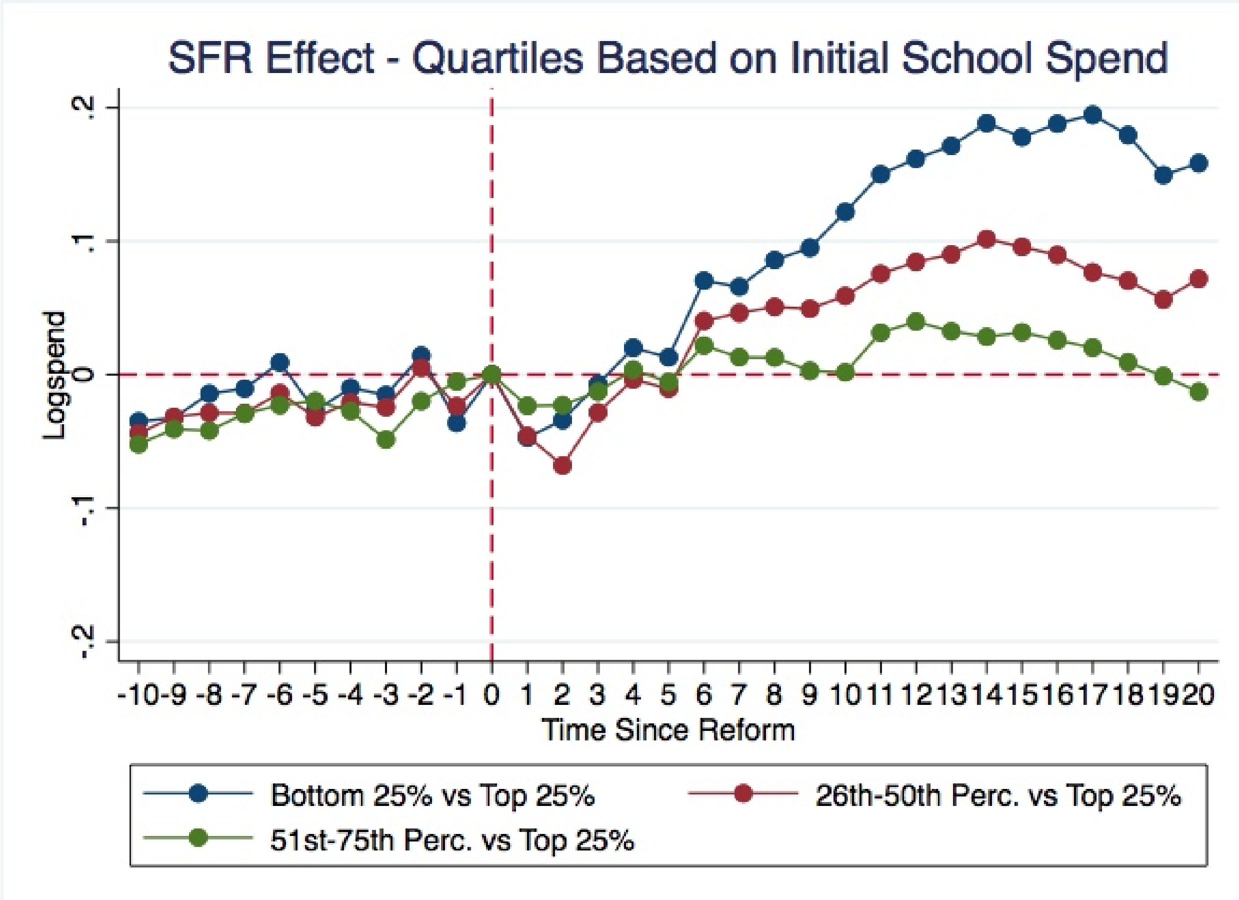

Figure 1: Event-study graph demonstrating the change in the difference in per-pupil school expenditures between school districts in the top 25 percentile of expenditures in 1972 and school districts in the bottom 75 percentile of the expenditures in 1972 before and after court-mandated school finance reforms.

of per pupil school expenditure from our first stage instrument:[13]

$$\boxed{Y_{d,t} = \theta P\hat{P}E_{d,t} + \beta X_{d,60} \times t + f_d + \epsilon_{d,t}}$$

3.3 SFR Event Study Design - Strengths and Challenges

Examining the impact of school finance reforms in this kind of broad event study design and several potential complications that we examine carefully in the analysis that follows. A key advantage of this approach is that it is possible to estimate school spending cap-

[13]All of the preliminary results presented below are based on estimates of the IV regression in two steps. We intend to use a one-step IV estimator in future drafts of the paper.

italization in a broad national data set and to consider heterogeneity along a number of dimensions.

A second, more subtle advantage of this approach is that it allows us to break the link between school spending and local taxation. Ideally, the school spending shocks shown in Figure 1 would be completely orthogonal to changes in local taxes, allowing us to isolate how households value the spending itself. As we explore in more detail in Section 5, it turns out that the school spending shocks shown in Figure 1 are in fact accompanied by some changes in the level of local taxation. This is likely due to the fact that SFRs often changed the incentives for local jurisdictions to raise money from local sources, often subsidizing such efforts in low spending districts and implicitly taxing such efforts in high spending districts.

To deal with any the impact of SFRs on local tax levels, we estimate a broader version of our main specification in Section 5. In particular, we estimate a specification that includes both per pupil school spending and per pupil local tax revenues, instrumenting for both with the SFR shocks. Using the same notation as above, this consists of estimating a second first stage for local tax revenues (PPLTR) and including those fitted values along with the ones for per pupil school spending in the second stage.

$$\boxed{PPLTR_{d,t} = \left[\sum_{T=-20}^{T=20} \sum_{Q_{72}=4}^{Q_{72}=1} \lambda_{Q,T} 1(Q) \times 1(T)\right] + f_d + \beta X_{d,60} \times t + \epsilon_{d,t}}$$

$$\boxed{Y_{d,t} = \theta P\hat{P}E_{d,t} + \theta PP\hat{L}TR_{d,t} + \beta X_{d,60} \times t + f_d + \epsilon_{d,t}}$$

As we discuss in Section 5, because 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.

4 Main Results

4.1 House Prices

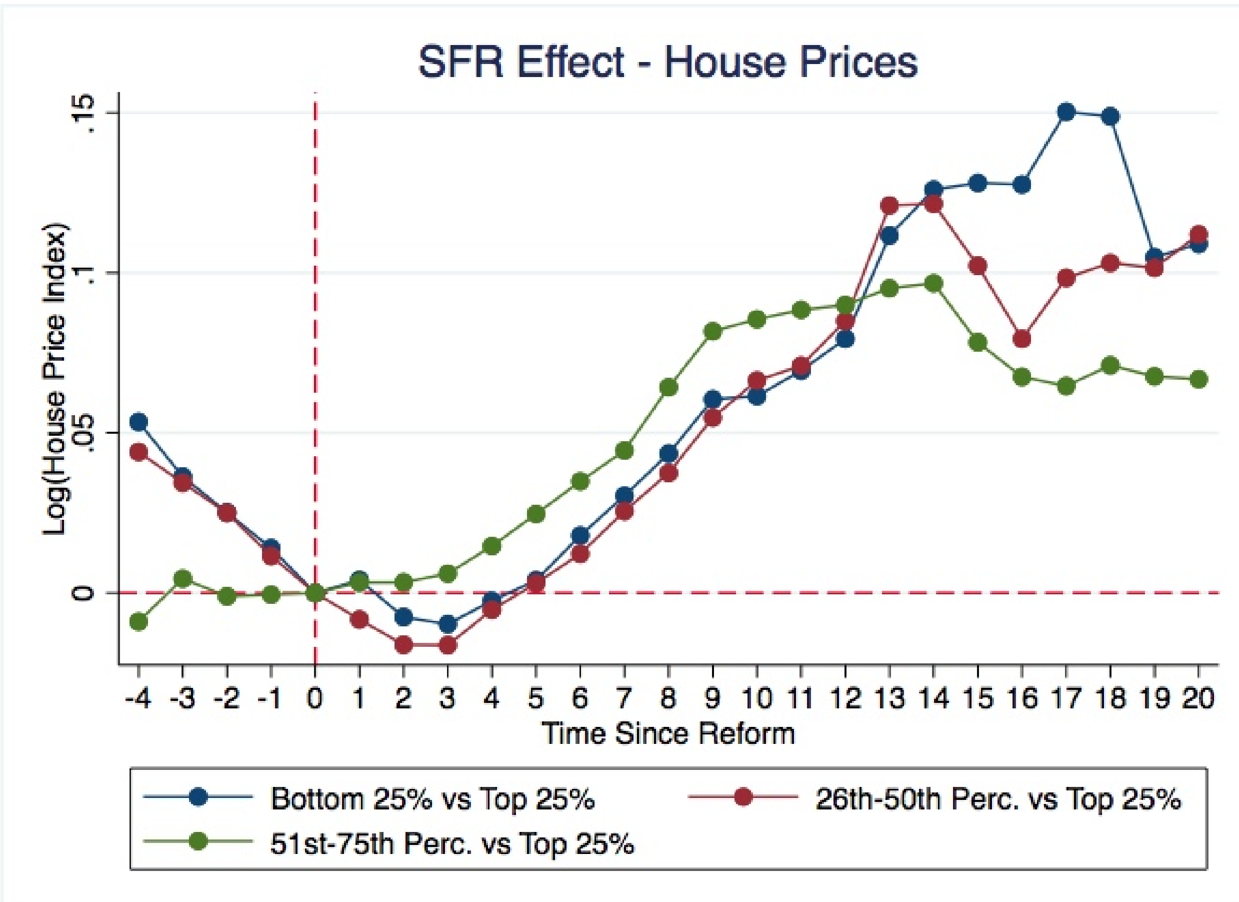

Figure 2: Event-study graph demonstrating the change in FHFA House Price Index before and after court-mandated school finance reforms. The three series represent differences in the log of the house price index between the top 25 percentile of school spending districts and the bottom 75 percent of school spending districts before and after the reforms.

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 1. Specifically, Figure 2 shows the dynamics of log house prices by initial school spending quartile (Q1-Q4) for three years before and twenty 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.

House Price Capitalization of Per Pupil School Expenditures

	(1)	(2)	(3)	(4)	(5)	(6)
Log(PPE)	0.772**	0.861**	1.539***	1.409***		
	(0.330)	(0.369)	(0.524)	(0.460)		
Log(PPE) $\times$ Supply Elasticity			-0.339***	-0.330***		
			(0.0994)	(0.0698)		
Log(PPE) $\times$ Urban					1.115***	1.090***
					(0.358)	(0.375)
Log(PPE) $\times$ Suburban					0.869**	0.899**
					(0.338)	(0.368)
Log(PPE) $\times$ Rural					0.600*	0.756*
					(0.349)	(0.385)
Observations	425,456	390,142	359,900	329,744	425,456	390,142
R-squared	0.793	0.801	0.804	0.809	0.797	0.803
Number of tracts	16,394	15,015	13,872	12,692	16,394	15,015
District FE	YES	YES	YES	YES	YES	YES
Calendar Year FE	YES	YES	YES	YES	YES	YES
Census Controls	NO	YES	NO	YES	NO	YES

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3: Table: House prices on spending instrument, w. and w.out 1960 Census controls; Urban-Suburban-Rural; Housing Elasticity interaction

Table 3 shows the impact of school spending on housing prices overall and across various geographies. The first column reports the results of a regression of the log house price index on predicted log school spending, where predicted log school spending is based on the variation across school districts for twenty years following a school finance reform, as shown in Figure 1.[14] All specifications also include census tract fixed effects and calendar year dummies.

The results imply a substantial impact of school spending on house prices, a 1 percent increase in school spending leads to a 0.8 percent increase in property values. This sharp increase in the willingness to pay for access to better funded schools implies that households observe and value increases in school spending as an investment in their children.

[14] The first stage regression also includes four years of pre-reform trends by school spending quartile in 1990.

The magnitude of the estimated impact of school spending on property values is also consistent with the substantial effects on children's life outcomes documented in Jackson et al. (2015) and Lafortune et al. (2018).

The second column of Table 3 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.[15] The coefficient on predicted school spending changes only slightly with the inclusion of these controls, suggesting that these results are not at all sensitive to differential time trends across school districts.

The specifications reported in the middle columns of Table 3 identify the impact of school spending on house values separately for urban, suburban, and rural geographies. The point estimates are largest in urban and suburban areas and slightly smaller in rural areas, although the estimates are large and statistically significant everywhere and not statistically different from one another.

The final two columns of Table 3 examine how the estimated impact of school spending on property values varies with housing supply elasticity. In general, we expect increases in local amenities to lead to smaller price changes in regions with more elastic housing supply, as increases in the subsequent housing stock dampen the price effect. This is, in fact, exactly, what the results reveal, implying a much larger degree of price capitalization in inelastic housing supply regions. For example, the implied coefficient in a market with an elasticity of 0.5 (San Francisco) is about 1.1, while the implied coefficient in a market with an elasticity of 2.5 (Dallas) is only 0.4. In this way, the exact degree of capitalization of school spending into housing prices is quite dependent on urban housing market conditions, exactly as economic theory would predict.

[15]These are the same controls used in Jackson et al. (2015).

Effect of School Expenditures on School District Poverty Rate						
	(1)	(2)	(3)	(4)	(5)	(6)
Log(PPE)	-0.150***	-0.144***	-0.250***	-0.212***		
	(0.0254)	(0.0174)	(0.0710)	(0.0417)		
Log(PPE) × Supply Elasticity			0.0502**	0.0430***		
			(0.0217)	(0.0125)		
Log(PPE) × Urban					-0.226***	-0.201***
					(0.0649)	(0.0415)
Log(PPE) × Suburban					-0.147***	-0.141***
					(0.0270)	(0.0220)
Log(PPE) × Rural					-0.158***	-0.150***
					(0.0258)	(0.0238)
Observations	376,255	345,083	318,271	291,659	376,255	345,083
R-squared	0.372	0.399	0.415	0.439	0.387	0.406
Number of tract	16,394	15,015	13,872	12,692	16,394	15,015
District FE	YES	YES	YES	YES	YES	YES
Calendar Year FE	YES	YES	YES	YES	YES	YES
Census Controls	NO	YES	NO	YES	NO	YES

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4: Table: Percent Poverty on spending instrument, w. and w.out 1960 Census controls; Urban-Suburban-Rural; Housing Elasticity interaction (6 columns)

4.2 Capitalization and Poverty

The sharp increase in housing prices following an increase in school spending naturally affects who can afford to live in a school district. Moreover, the changes in school spending might affect sorting on the basis of willingness to pay. Thus, as a natural extension of our main capitalization results, we now investigate the impact of school spending levels on the fraction of children in poverty in a school district.

Figure 3 shows how the fraction of children in poverty within a district changes following a school finance reform. As the figure makes clear, the fraction of children in poverty declines steadily, especially in Q1 relative to Q4 districts, again very much in line with the dynamics of school spending changes.

Table 4 follows the same general format as Table 3 above, showing the impact of school spending on the fraction of children in poverty, with and without 1960 Census controls

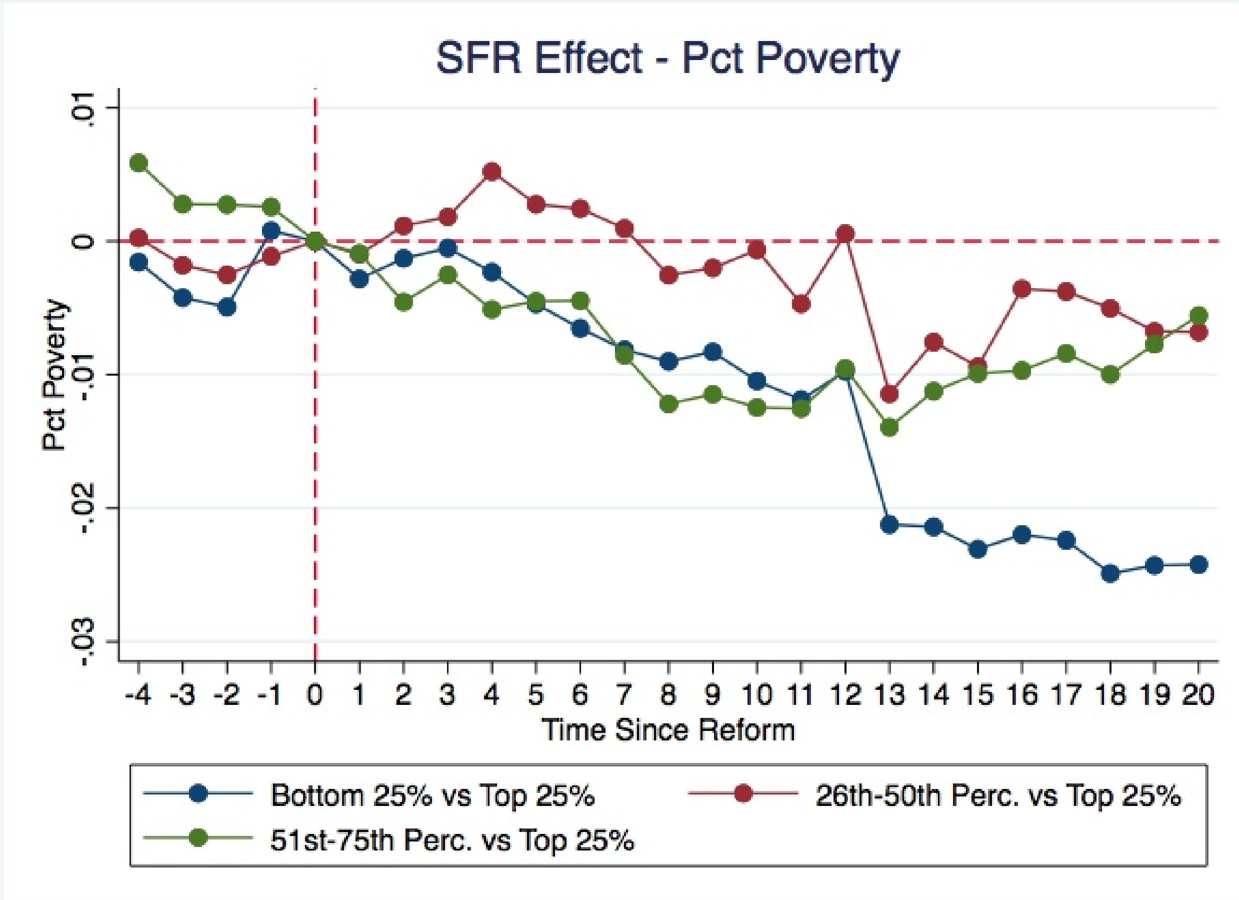

Figure 3: Event-study graph demonstrating the change in fraction of poor students in school districts before and after court-mandated school finance reforms. The three series represent differences in the fraction of poor students between the top 25 percentile of school spending districts and the bottom 75 percent of school spending districts before and after the reforms.

interacted with a linear time trend, and across various geographies. The results reported in the first two columns imply that the fraction of children in poverty declines by about 0.2 percentage points following a 1 percent increase in school spending. The middle two columns interact school spending with geography, again revealing statistically significant and similar effect sizes in urban, suburban, and rural districts, with slightly larger point estimates in urban areas. The final two columns of Table 4 include interactions with housing supply elasticity. Consistent with the house price effects in Table 4, the impact of changes in school spending on the fraction of children in poverty is largest in regions with more inelastic vs. elastic housing supply, in other words, exactly where the impact

Conditional House Price Capitalization of Per Purpil Expenditures

	(1)	(2)	(3)	(4)	(5)	(6)
Log(Per Pupil Expenditure)	0.861**	0.710**	1.409***	1.113***		
	(0.369)	(0.350)	(0.460)	(0.328)		
Log(PPE) × Supply Elast.			-0.330***	-0.273***		
			(0.0698)	(0.0660)		
Log(PPE) × Urban					1.090***	0.774**
					(0.375)	(0.334)
Log(PPE) × Suburban					0.899**	0.766**
					(0.368)	(0.345)
Log(PPE) × Rural					0.756*	0.538
					(0.385)	(0.355)
Observations	390,142	343,479	329,744	290,606	390,142	343,479
R-squared	0.801	0.817	0.809	0.818	0.803	0.818
Number of tract	15,015	15,015	12,692	12,692	15,015	15,015
District FE	YES	YES	YES	YES	YES	YES
Calendar Year FE	YES	YES	YES	YES	YES	YES
Census Controls	YES	YES	YES	YES	YES	YES
District Demographics	NO	YES	NO	YES	NO	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5: Table: House prices on spending instrument, w. 1960 Census trend controls, w. and w.out school demographic controls (race and poverty); Note: just report spending results (6 columns)

on the cost of housing is greatest.

4.3 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 may combine a direct effect of school spending and an indirect effect that results from the changing demographic and socioeconomic composition of the school district. To separate these components, Table 5 repeats the earlier house price specifications reported in Table 3 with additional controls for the fraction of children in poverty and the racial and ethnic composition of the school district.

The results reported in Table 5 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 20 percent in each specification. In this way, the vast majority – about 80 percent – of the overall capitalization of school spending into house prices is a direct effect of the spending, while 20 percent is due to the sorting that occurs following the spending change.

5 Taxes and Spending

A longstanding challenge in the empirical literature on the capitalization of school spending is the natural coupling of changes in spending and taxation. When all taxes and spending are local, for example, any increase in school spending must be accompanied 1-for-1 with an increase in local taxes. In such a setting, it would not be surprising for price regressions like those estimated above to reveal a very small willingness to pay for increases in school spending, as the estimates would instead be capturing the combined effect of the spending and tax changes. In fact, a strong prediction of the theoretical local public finance literature following Tiebout is that the effect of a marginal change in school spending (with the accompanied increase in local taxes) should be exactly zero in equilibrium.

Even in settings in which a portion of tax revenues comes from higher levels of government - so that school spending and local taxes are not perfectly collinear – funding from the state and federal level is typically explicitly tied to the local tax base and other economic conditions. The resulting endogeneity problems have traditionally made it very difficult to isolate the causal effect of school spending changes on house prices.

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, in particular, like 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).

Figures 4 and 5 show the dynamics of local and non-local tax revenues following a school finance reform, separating districts again into quartiles (Q1-Q4) based on initial school spending in 1972. As the figures makes clear, both sources of 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, non-local sources of revenue increased primarily in Q1 districts relative the other quartiles and, primarily, in one level shift upwards in years 4-7 following the reform. Local tax revenue, on the other hand, increased more gradually and in the line with initial school spending levels, increasing the most for Q1 and moderately for Q2 and Q3 relative to Q4 districts. This variation in the timing and extent of the changes across school spending quartiles provides the basis for separately identifying the capitalization school spending and local tax revenues.

To separately identify the role of spending and taxes on local housing prices, Table 6 presents the results from a series of specifications that add the log of local tax revenues to the specifications reported in Table 3 above. 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 and across all geographies. Importantly, the inclusion of local property tax revenue has only a modest impact on the coefficients on school spending in all six specifications, when compared to the analogous result presented in Table 3. That the coefficients on school spending change so little suggests that there is only a modest amount of high frequency correlation between local and non-local tax revenue sources within the event study framework.

A comparison of the size of the coefficients on log local tax revenue and log school spending provides an assessment of the efficiency of school spending. This is an es-

House Price Capitalization of Per Pupil School Expenditures & Taxes						
	1	2	3	4	5	6
Log(Per Pupil Expenditures)	0.700**	0.777**	1.600***	1.477***		
	(0.327)	(0.364)	(0.498)	(0.476)		
Log(PPE) × Supply Elasticity			-0.387***	-0.380***		
			(0.115)	(0.0982)		
Log(Per Pupil Tax Revenues)	-0.150**	-0.166*	-0.131	-0.190		
	(0.0707)	(0.0844)	(0.194)	(0.215)		
Log(PPTR) × Supply Elasticity			0.0578	0.0653		
			(0.0916)	(0.0795)		
Log(PPE) × Urban					0.952**	0.925**
					(0.357)	(0.377)
Log(PPE) × Suburban					0.828**	0.830**
					(0.335)	(0.367)
Log(PPE) × Rural					0.603	0.691*
					(0.370)	(0.383)
Log(PPTR) × Urban					-0.0450	-0.0739
					(0.147)	(0.161)
Log(PPTR) × Suburban					-0.210***	-0.214***
					(0.0626)	(0.0713)
Log(PPTR) × Rural					-0.248	-0.206
					(0.151)	(0.154)
Resulting % ↑ House Price from ↑ PPE funded by a 1% ↑ in PPTR						
All Areas	0.0596	0.0566				
	(0.117)	(0.129)				
Urban Areas (%)					0.239	0.186
					(0.145)	(0.163)
Suburban Areas (%)					0.0364	0.0216
					(0.105)	(0.118)
Rural Areas (%)					-0.0672	0.0008
					(0.160)	(0.175)
Observations	425,456	390,142	359,900	329,744	425,456	390,142
R-squared	0.793	0.802	0.805	0.810	0.798	0.804
Number of tract	16,394	15,015	13,872	12,692	16,394	15,015
District FE	YES	YES	YES	YES	YES	YES
Calendar Year FE	YES	YES	YES	YES	YES	YES
Census Controls	NO	YES	NO	YES	NO	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6: Table: House prices on spending and local tax instrument, w. and w.out 1960 Census controls; Urban-Suburban-Rural; Housing Elasticity interaction (6 columns)

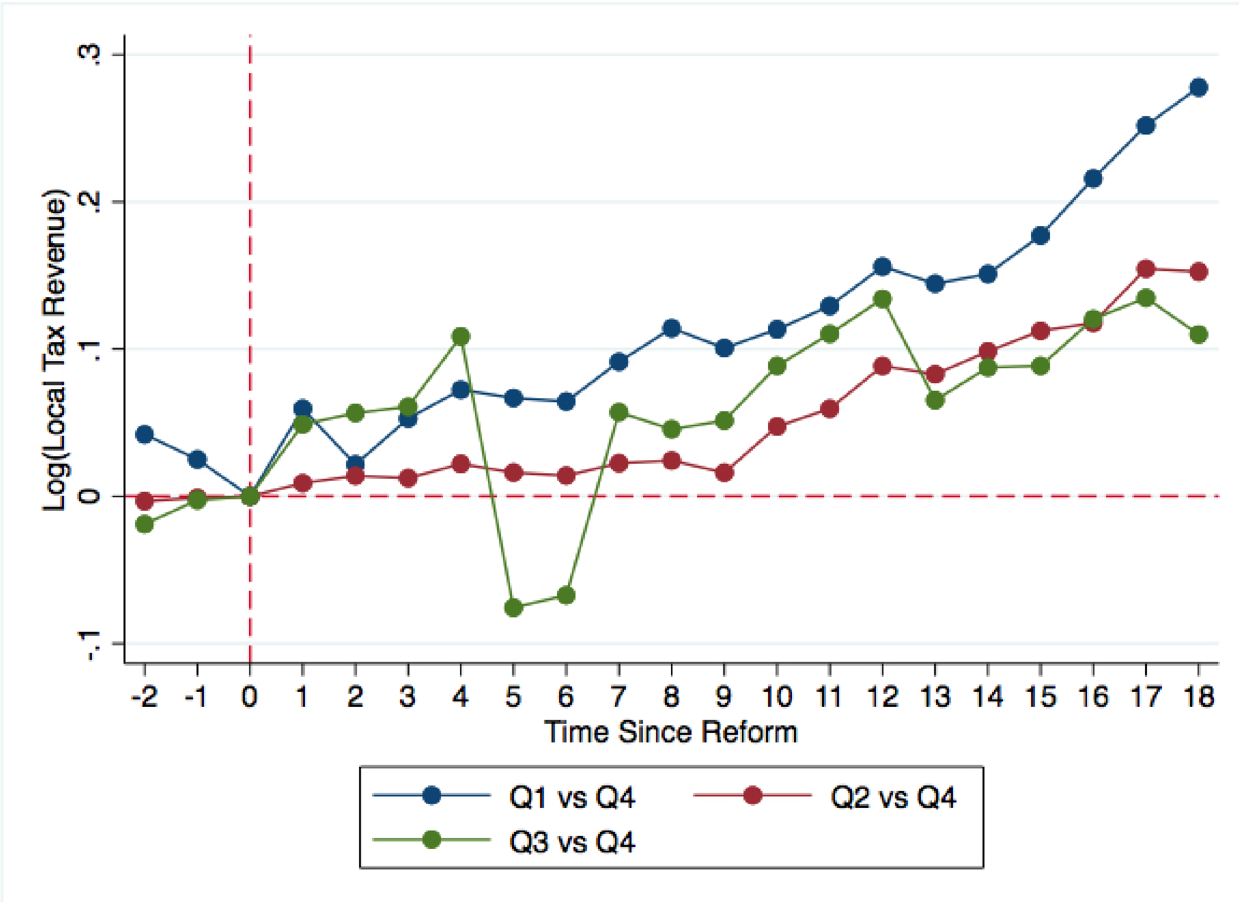

Figure 4: Event-study graph demonstrating the change in log local tax revenue in school districts before and after court-mandated school finance reforms. The three series represent differences in the fraction of poor students between the top 25 percentile of school spending districts and the bottom 75 percent of school spending districts before and after the reforms.

pecially attractive feature of the SFR event study design, which allows us to separately identify how both spending and taxation are capitalized into house prices within a single national study.

To assess the efficiency of school spending, we need to compare the value of a dollar in local taxes versus school spending. For our sample as a whole, local tax revenue represents about 35 percent of school spending. So, in dollar terms, a 1.0 percent increase in local tax revenues is equivalent to only about a 0.35 percent increase in school spending. In the lower panel of Table 6 we perform this calculation to assess the percent change

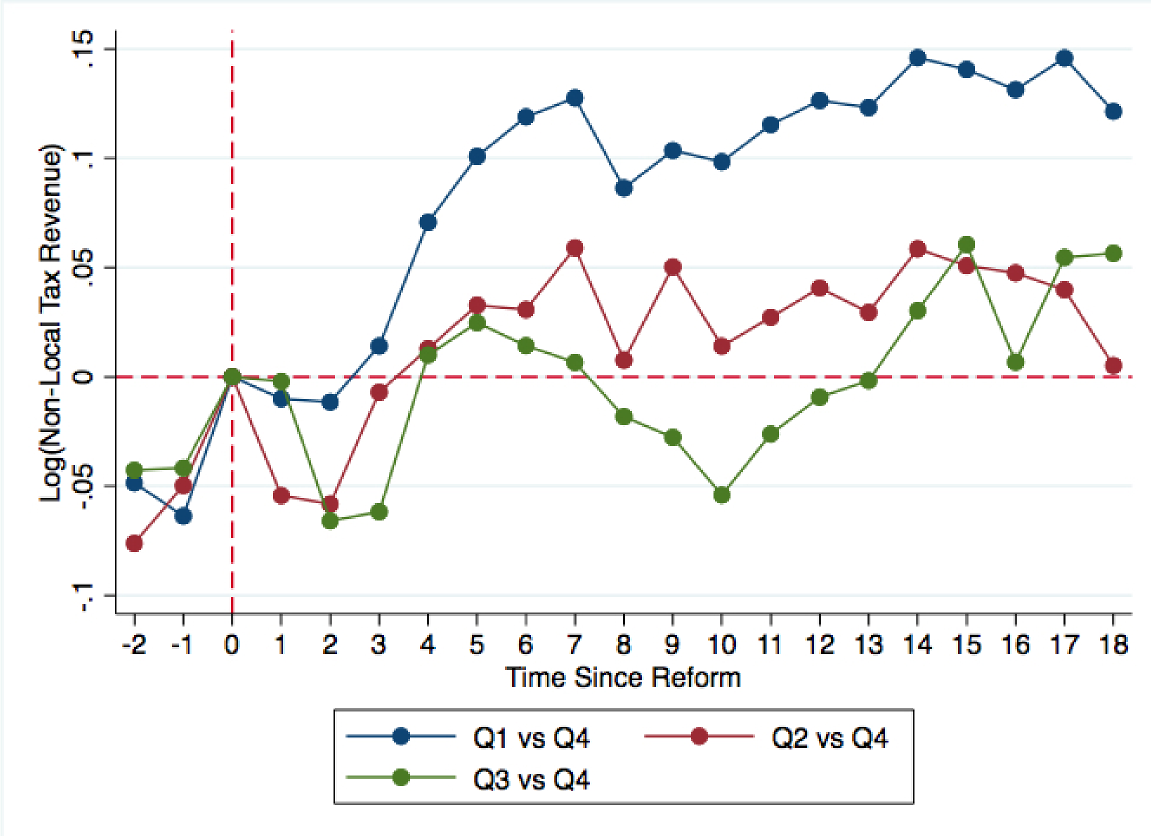

Figure 5: Event-study graph demonstrating the change in log non-local tax revenue in school districts before and after court-mandated school finance reforms. The three series represent differences in the fraction of poor students between the top 25 percentile of school spending districts and the bottom 75 percent of school spending districts before and after the reforms.

in house prices that would result from an increase in spending that is financed by a 1% increase in taxes. Overall, we find a positive but statistically insignificant effect of 0.06% both with and without the 1960 census controls. Since this effect is also economically small, the evidence suggests that education spending overall is approximately efficient, if not slightly under-funded. When we break out our results by geography, the effect sizes are even smaller for urban (0.02%) and rural areas (0.008%) and closer to zero, which suggests efficiency. While the change in house prices is statistically insignificant across all geographies, the effect size is an order of magnitude larger in urban areas (0.19%) when compared to either rural or suburban areas. This suggests that if education is underfunded anywhere, it is most likely to be underfunded in urban areas.

6 Which Kinds of Spending Matter?

The results of the previous two sections make clear that households highly value the changes in school spending resulting from school finance reform shocks however the money is spent in practice. A natural next question, then, 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 capture the total salaries of all personnel in the district and a component that captures all other non-salary spending. Figures 6 and 7 shows how the log of these two spending components changes following a school finance reform. There is, once again, substantial variation the timing and extent of the changes across the four spending quartiles.

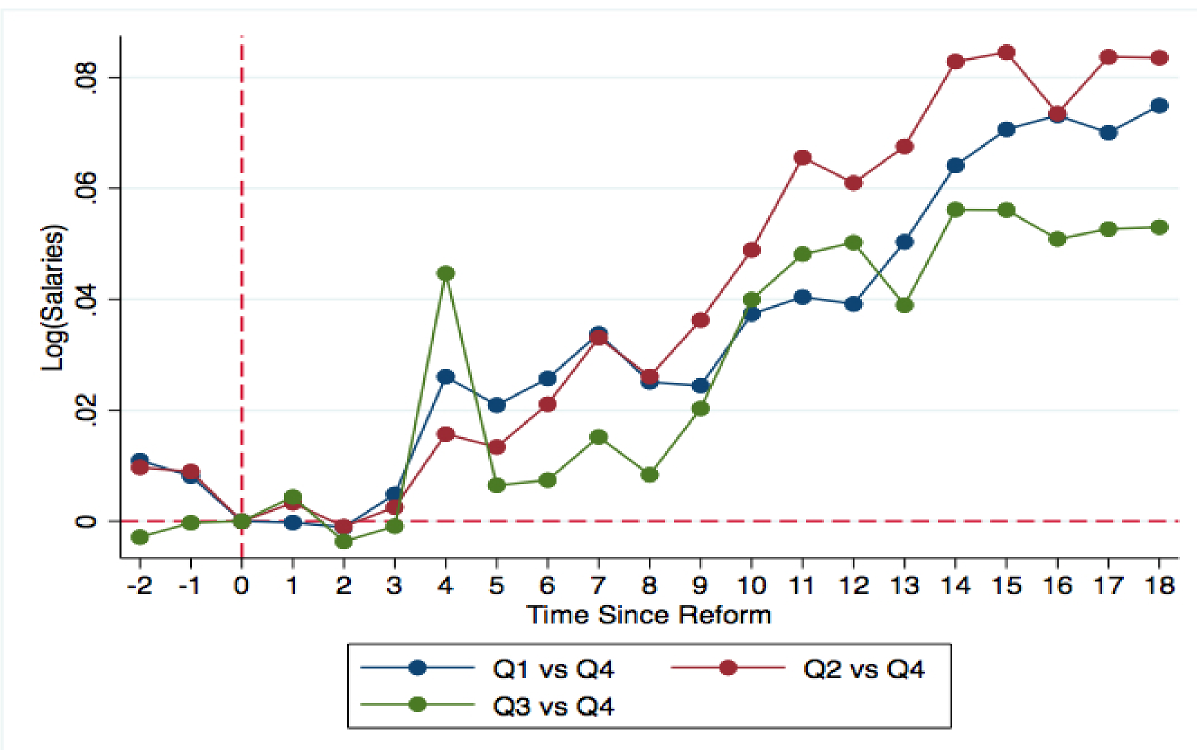

Figure 6: Event-study graph demonstrating the change in log salary expenditures in school districts before and after court-mandated school finance reforms. The three series represent differences in the fraction of poor students between the top 25 percentile of school spending districts and the bottom 75 percent of school spending districts before and after the reforms.

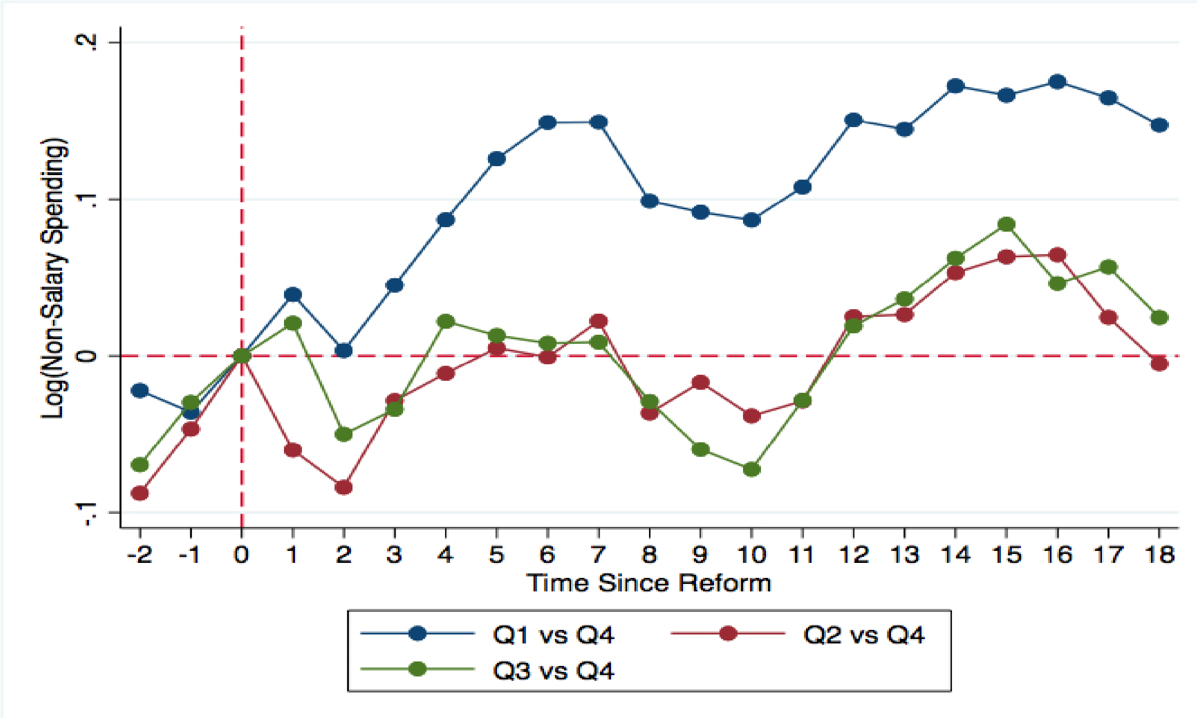

Figure 7: Event-study graph demonstrating the change in log non-salary expenditures in school districts before and after court-mandated school finance reforms. The three series represent differences in the fraction of poor students between the top 25 percentile of school spending districts and the bottom 75 percent of school spending districts before and after the reforms.

Using this variation, Table 7 reports results for a series of log house price regressions analogous to those reported in Table 3 but with spending broken down into per pupil salary and non-salary components. The results reveal that households highly value spending on salaries. The coefficients on salary spending are large and statistically significant in all geographies. The coefficients on non-salary spending, on the other hand, are negative and statistically indistinguishable from zero in every specification in Table 7.

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. It also belies the notion that higher spending on personnel would largely lead to infra-marginal windfalls for existing teachers and staff with no resulting benefits to children.

House Price Capitalization of Salary and Non-Salary Expenditures

	(1)	(2)	(3)	(4)	(5)	(6)
Log(Salaries)	1.892***	2.216***	2.109***	2.083***		
	(0.476)	(0.509)	(0.595)	(0.681)		
Log(Non-Salary Spending)	-0.820*	-0.756**	-0.430	-0.376		
	(0.429)	(0.356)	(0.359)	(0.299)		
Log(Sal.) × Supply Elast.			-0.175	-0.110		
			(0.123)	(0.138)		
Log(Non-Sal.) × Supply Elast.			-0.140	-0.153*		
			(0.0987)	(0.0868)		
Log(Sal.) × Urban					1.875***	2.085***
					(0.534)	(0.566)
Log(Sal.) × Suburban					1.942***	2.274***
					(0.471)	(0.509)
Log(Sal.) × Rural					1.885***	2.348***
					(0.503)	(0.551)
Log(Non-Sal.) × Urban					-0.591	-0.574
					(0.383)	(0.353)
Log(Non-Sal.) × Suburban					-0.762*	-0.751**
					(0.404)	(0.349)
Log(Non-Sal.) × Rural					-0.900**	-0.854**
					(0.414)	(0.346)
Observations	425,456	390,142	359,900	329,744	425,456	390,142
R-squared	0.803	0.813	0.814	0.819	0.807	0.815
Number of tract	16,394	15,015	13,872	12,692	16,394	15,015
District FE	YES	YES	YES	YES	YES	YES
Calendar Year FE	YES	YES	YES	YES	YES	YES
Census Controls	NO	YES	NO	YES	NO	YES

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 7: Table: House prices on salary and non-salary spending, with and without 1960 Census controls; Urban-Suburban-Rural; Housing Elasticity interaction (6 columns)

7 Conclusion

This paper uses variation in school spending and local taxation resulting from court-ordered school finance reforms to provide new empirical evidence on several of the longest standing questions in local public finance and the economics of education. In addition to providing independent variation in school spending and local taxation, a key advantage of the SFR event study design that we employ is that the resulting estimates are based on a national sample of school districts rather than a single 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, Johnson, and Persico (2015). We also find that the combination of higher house prices and increased school spending affects who sorts into a school district, decreasing the fraction of children in poverty. 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 co-linear and correlated with local the 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. Our results indicate that the total impact of a local dollar raised for school spending is close to zero, implying that school spending is efficient on average nationwide. While not quite statistically significant, the marginal value of a dollar of school spending raised from local taxation is positive in urban areas, suggesting that spending is inefficiently low in urban districts in contrast to suburban and rural ones.

We close the paper by investigating what forms of school spending households value. Strikingly, house prices are sharply increasing in spending on salaries and actually slightly decreasing in other forms of spending. This implies that parents value either the increased quality or quantity of school personnel made possible by higher spending on salaries, something we are exploring further in ongoing work.

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