Are We Spending Enough on Teachers in the U.S.?

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September 1, 2022

Abstract

In US public schools, expenditure on teachers' salaries exceeds $200B. We isolate plausibly exogenous changes in spending on teachers' salaries and property taxes to measure the capitalization of these changes into house prices. Using a nationally representative panel of 6,200 school districts covering 25 years we find that an exogenous tax-funded increase in salary spending would raise house prices. Our spatial equilibrium model predicts that spending on teachers' salaries is efficient if a tax-funded increase in salary spending leaves house prices unchanged. Consequently, our empirical and theoretical findings provide evidence that teacher spending in the United States is under-provided.

JEL Classification: I22, I24, and H41

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[§]This paper previously circulated under the title "A National Study of School Spending and House Prices". We received helpful comments from: Rucker Johnson, Thomas Kane, Claudia Persico, Edward Glaeser, Joseph Gyourko, Kirabo Jackson, Eric Hanushek, Michael Lovenheim, Amy Finkelstein, Hunt Allcott, Isaiah Andrews, Sarah Turner, Jesse Rothstein, Desmond Ang, David Deming, John Friedman, Nathaniel Hendren, William Doerner, Neale Mahoney, Jonathan Meer, Susan Dynarski, Raj Chetty, Jeffrey Smith, Chao Fu, Richard Murnane, Juan Carlos Suarez Serrato, Aimee Chin, Chinhui Juhn, 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, Wisconsin, UCLA, E-con of Education, Cleveland Fed, Clemson, Nebraska, Tulane, Houston, Southern Economic Association, Western Economic Association and The BE-Lab.

Annual public expenditures on K-12 schools in the United States totaled \$640B – or 3.6% of US GDP – in 2015. More than half of this amount was spent on staff salaries, with nearly 70% of the staff salary expenditures going to pay teachers (US Census Bureau 2017). Public expenditure on teachers salaries' as a share of GDP ($\sim$ 1.3%) is comparable to Medicaid spending ($\sim$ 2%) and total spending on income assistance programs ($\sim$ 1.2%).[1] Despite its importance as an expenditure line item, and a topic of current policy debate,[2] there is scant causal evidence on the efficiency of public spending on K-12 teacher salaries. In contrast, there is a well-identified study on whether school infrastructure is funded at an efficient level (Cellini et al., 2010).[3] Our paper fills this gap in the literature by providing a theoretically and empirically rigorous answer to the question: Are we spending efficiently on teachers in the United States?

We approach this longstanding question by first modelling an environment where changes in aggregate house prices reflect marginal changes in local public goods and the taxes required to fund them. The model is motivated by a classic literature in public economics on the efficient provision of public goods (Musgrave (1939), Samuelson 1954, Tiebout 1956) and builds upon the key theoretical insights in Oates (1969) and especially (Brueckner, 1979, 1982). The output of our model yields a simple efficiency test which we implement empirically by applying modern methods for causal inference to a panel of detailed data on local house prices, school expenditures and local tax revenues. Salary spending levels are efficient if a marginal tax-funded increase in salary spending has no impact on house prices. If instead house prices rise (fall) in response to a tax-funded increase in salary spending salary spending was inefficiently low (high) to begin with.[4]

[1] Income assistance programs include unemployment benefits, earned income tax credits, childcare tax credits, supplemental nutrition assistance program (SNAP) benefits, and other supplementary security income.

[2] As a part of the American Families Plan, U.S. President Joe Biden is proposing \$20B increase in Title I funding, some of which will be earmarked to increasing teacher pay (Will, 2021).

[3] There are also empirical exercises testing the efficiency of school spending overall, which do not disaggregate type of spending Barrow and Rouse (2004)

[4] We distinguish salaries from all other current and capital expenditures to get a closer proxy for school district labor inputs. The efficiency test can be flexibly estimated to compare aggregate current spending to capital spending, and we report the results in the Appendix.

Empirically, we document a large, statistically significant house price response to plausibly exogenous increases in per-pupil salary spending. By contrast we find an economically small and statistically insignificant impact of increases in non-salary expenditures on house prices. Moreover, holding school spending constant, we find that house prices fall for plausibly exogenous increases in local property tax revenue. Combining these estimates, we find that expenditure on salaries is inefficiently low. On the margin, increasing local taxes to spend more on teacher salaries would result in higher house prices. This market-based test for the efficiency of salary spending is consistent with the evidence from the teacher value-add literature and the school finance literature. These two literatures produce credible, consistent evidence of the positive impact of both high value-add teachers and increased school spending on educational attainment, earnings, intergenerational mobility and other long-run labor market outcomes (Card and Krueger 1992; Hoxby 1996; Loeb and Page 2000; Chetty et al. 2014a,b; Jackson et al. 2016; Biasi 2017; Jackson 2018a; Lafortune et al. 2018; Baron 2021; Hanushek et al. 2019; Brunner et al. 2020; Jackson and Mackevicius 2021).

There are two key empirical challenges to simultaneously estimating the house price capitalization of salary spending and property taxes on a national scale. First, credible research designs commonly used in the capitalization literature, e.g., boundary discontinuity designs and close bond referenda, require extensive amounts of micro data on house prices and local jurisdictions – making it challenging to construct a panel that spans both a long time period and a broad geography. Consequently, most estimates that leverage quasi-experimental research designs focus on a single state or metropolitan area (Black 1999; Bayer et al. 2007; Kane et al. 2006; Cellini et al. 2010). Second, there is a series of stubborn endogeneity problems that plague naive OLS regressions of school spending on house prices, which require either an instrumental variables strategy (Barrow and Rouse (2004)) or quasi-random policy variation (Dee (2000)).

To produce a credible empirical study with broad external validity, we construct a 25-

year national panel of quality-adjusted local house price indices (HPI), paired with annual data describing school district spending and property tax revenues. Our research design builds on the approach in Jackson et al. (2016) who leverage the timing of school finance reforms (SFRs) for exogenous variation in total school spending, which they use to estimate the impact of increased school spending on long term education and labor market outcomes of students.[5] In addition to their effects on spending, these same reforms contained tax incentives based on their redistributive nature, which caused previously lower (higher) spending districts to raise more (less) property tax revenue following the reforms (Hoxby, 2001; Hoxby and Kuziemko, 2004).[6]

We illustrate the impact of SFRs on school finances using a sequence of event study plots that show our research design yields exogenous, independent variation in property tax revenue, salary, and non-salary spending. From the shocks we can reliably estimate the elasticity of house prices with respect to school spending and property tax revenue. When we disaggregate school spending into spending on salaries and non-salary spending, we find that salary spending is the main driver for the capitalization of school spending into house prices. 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. Nagler et al. (2020) argue that increasing the financial payoff of becoming a teacher may improve educational outcomes, as the profession becomes more appealing to candidates with high quality outside options.

Indeed, how money is spent in schools matters crucially for productive efficiency (Hanushek 1986; Jackson 2018b). Holding per-pupil spending constant, we find that increasing property tax revenue per-pupil reduces house prices. Combining the estimated elasticity of house prices with respect to salary spending with the elasticity of house prices

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

[6] 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.

with respect to property tax revenue, we estimate that a 1 percent tax-financed increase in salary spending would increase house prices, on net, by 1.03 percent – which suggests that spending on salary is inefficiently low. Converting this into dollars, we find that a tax-funded $1 increase in per pupil expenditure on salary would increase monthly rents by $0.14 to $0.16, on average. These estimates imply that households would be willing to pay $158 more in monthly rent for a tax-funded increase in salaries comparable to the difference in salary spending between districts in the lowest versus highest spending quartiles in 2015.

All of our key results related to house price capitalization 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. Our results are also quite similar when we isolate variation coming from the bottom, middle, and top of the initial school district spending distribution. One limitation of our data is that for the full length of the sample we can only reliably disaggregate the data into salary and non-salary buckets, where the salary bucket consists primarily of teacher salaries but also includes salaries paid to support staff and administrators.[7]

The remainder of the paper is organized as follows: Section 1 sketches a theoretical model; Section 2 describes the data; Section 3 outlines the research design; Section 4 discusses our results; Section 5 discusses interpreting the results and presents robustness tests; and Section 6 concludes the paper.

1 Testing for Efficiency of School Spending: Theory

The empirical test for the efficiency of school spending that we propose and implement in this paper is rooted in the theoretical public finance literature developed since Samuel-

[7]Our composite non-salary measure is a sum of capital spending, spending on debt payments, non-pecuniary employment benefits, and other current expenditures outside of salary and wages. Capital spending on land acquisition, construction, and maintenance of facilities is the non-salary spending component traditionally highlighted by the literature.

son (1954). In this section, we begin with a short discussion of the historical development of the related theory, lay out the theoretical framework and key assumptions that provide the basis for our efficiency test, and close by discussing two subtle issues that are important for empirical implementation.

1.1 Background

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

The central insight of Tiebout (1956) was that the sorting of households across communities gives local governments both the information and incentives needed to provide local public goods - e.g., school spending - efficiently.[8] A major branch of the literature following Tiebout (1956) focused on theoretically grounding an empirical test for the efficient provision of local public goods. While some of the intuition for such a test appeared informally in the literature as early as Oates (1969), Brueckner (1979) provided the first formal statement of an ingenious test based on property values.[9] In particular, Brueckner (1979) showed an equivalence between the Samuelson condition for efficient public

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

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

goods provision and the first order condition that results from communities choosing the level of the local public good, financed on the margin through local property taxation, to maximize aggregate property values.

Brueckner's key insight was that the core tenet of spatial equilibrium - that households with identical income and preferences must receive the same indirect utility no matter where they live - was essentially all one needed to derive this equivalence. As a result, his proposed test is not only deep but very general. His framework accommodates heterogeneous housing consumption within communities and tenure choice (rent or own). Households can be heterogeneous in terms of income and, as we show below, preferences. Jurisdictions can collect property tax revenues from both businesses and residents, provide multiple public goods, and receive revenue transfers from the state or federal government.[10]

In what follows, we present a simplified and slightly extended version of Brueckner's model with two goals in mind: (i) to provide the key economic intuition behind the test he proposes and (ii) to show that Brueckner's framework can be generalized to allow for heterogeneous preferences. We then show that the version of the efficiency test that we implement in this paper relies only on the spatial equilibrium condition, requiring no assumptions about how local governments make decisions.

1.2 Theoretical Framework

We begin by dividing households into discrete heterogeneous types on the basis of income y and preferences β. β defines the preferences of each type over the bundle of housing services and neighborhood amenities that vary between communities. More specifically, household utility is defined over numeraire consumption c, housing services h, and the

[10]It is important to emphasize that Brueckner's theoretical framework does not make any claims about whether we should expect public goods to be provided efficiently. Instead, it provides the theoretical basis for an empirical test of whether local public goods are in fact efficiently provided in a very general framework no matter the system of local public finance.

public good g: $u(c,h,g,\beta)$. Households choose from a set J neighborhoods/school districts each of which provides N_j heterogeneous housing units with housing service levels $(h_{j1}, ...h_{jN_j})$.

The key implication of spatial equilibrium is that households of the same type (y,β) must receive the same indirect utility level u=$m(y,\beta)$. This uniform utility condition is equivalent to c=$c(h,g,y,\beta)$ such that for households with identical taste and income, the choice of (h,g) determines the consumption level needed to reach indirect utility level u. It follows that the household's budget constraint is given by c=y-R, where R is rent. As a result, spatial equilibrium implies the following bid-rent function for household type (y,β):

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

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

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

Note that equation (2) applies both within and across communities and holds whether households own or rent.[11]

Equation (2) uses the uniform utility condition derived from spatial equilibrium to create a tight link between house values across locations. In particular, on the margin, each housing unit's value must reflect the change in the willingness of the household type (y,β) who inhabits it in equilibrium to pay for any marginal change in the attributes of the housing unit or community (h,g,τ). Thus, as long as each household type (y, β) chooses housing units in multiple communities in equilibrium, the marginal change in the value of any house for a change in (g,τ) will reflect the marginal willingness to pay of the household who inhabits it for the associated change - e.g., MB_i. And, by summing over all housing units within a community, we recover $\sum MB_i$ in response to a change in

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

(g,τ), exactly what is needed to assess the Samuelson equation! Formally, the efficiency test that we use in this paper can be stated as:

Proposition 1 - Test for Efficiency of Public Goods Provision: Consider a marginal increase in the level of local public goods from g to g' funded completely from a corresponding increase in local tax revenues from τ to τ':

- Public goods are efficiently provided if $\sum V_i(g', \tau') - \sum V_i(g, \tau) = 0$.
- Public goods are under-provided if $\sum V_i(g', \tau') - \sum V_i(g, \tau) > 0$.
- Public goods are over-provided if $\sum V_i(g', \tau') - \sum V_i(g, \tau) < 0$.

Importantly, this form of the efficiency test, first developed in Brueckner (1982), follows directly from spatial equilibrium and holds under any system for the provision of school spending, including pure local financing and various hybrid systems that include transfers from the state and federal government.

1.3 Empirical Implementation

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

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

2 Data

The data used in our analysis are drawn from several sources. Average house prices within school district boundaries are measured by the FHFA house price index (HPI), derived from mortgage transactions on single-family properties securitized by Fannie Mae or Freddie Mac. We observe HPI annually from 1990-2015 for over 6,200 school districts, and pair the measure with district-reported finance data from the *F-33 Annual Survey of School System Finances*. The annual survey of school district finances provides aggregate expenditure data along with detailed breakdowns by expense type (salary vs. non-salary expenditures) and revenue source (federal, state, and property tax revenues).[12] The final piece of the data is the the initial passage year of state finance reforms, coded following Jackson et al. (2016) and described further in section 3.2. Following the literature, school districts are categorized into spending quartiles based on the pre-period distribution of per-pupil spending in each state. Summary statistics are presented in Table 1.

2.1 House Price Index

Following the methodology developed in Case and Shiller (1989), the FHFA HPI is a "constant quality" index, which estimates appreciation using a sample of houses that have been sold or refinanced multiple times.[13] The key advantage of the FHFA HPIs is that they are available at the census tract level for most of the United States over a long sample period, whereas the widely-used Case-Shiller indices are only available at the metropolitan level. Relative to the Case-Shiller indices, the FHFA HPIs differ in that they are based on data for a sample of houses with conforming mortgages, i.e. mortgages

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

[13] 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.

Table 1: Summary Statistics

	Full Sample	Quartile 1	Quartile 4	Universe
House Price Index - 1990	89.5	72.8	98.5	
House Price Index - 2015	117.2	116.4	113.4	
Real HPI Growth	*31%*	*60%*	*15%*	
District Finance Variables *2015 Dollars Per-Pupil*				
Salary Spending - 1990	5,545	4,795	6,333	5,459
Salary Spending - 2015	6,718	6,210	7,342	6,678
Salary Growth	*21%*	*30%*	*16%*	*22%*
Prop. Tax Revenue - 1990	3,884	2,688	5,224	3,711
Prop. Tax Revenue - 2015	5,162	3,776	6,558	5,020
Tax Rev. Growth	*33%*	*40%*	*26%*	*35%*

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

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.[14] In practice, the FHFA and Case-Shiller indices are very highly correlated and these differences in the sample selection create only small differences between the two indices (Leventis, 2008).

Over our study period, the house price indices are available for an increasingly large sample of census tracts. In each year, we aggregate the HPI for the available census tracts within a school district, creating a broad measure of real price growth with a district relative to the base year 2003.[15] As shown in Table 1, real house price growth was approximately 31% during the study period, with the largest gains (60%) in initially low school-spending districts.

2.2 District Finances & Salary Data

School finance data are publicly available through the F-33 finance survey maintained originally by the Census of Local Governments for 45 of the lower 48 states.[16] The Census of Local Governments is a massive historical database of public spending on schools and other services like municipal water and waste, public safety, fire departments and housing authorities. The line-item detail of the F-33 survey allows us to fully explore which school spending types matter and the effect of funding schools through local property taxes.

Salaries and wages are classified as current expenditures, a broad spending category

[14]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.

[15]See the appendix for aggregation steps and construction of the house price index. We compute a house price coverage measure - the fraction of residents within a school district living in a census tract with available HPI data - for each district by year observation. The average house price coverage is well over 80% throughout the sample period and above 90% for many years. We include this house price coverage measure as a control in the analysis for robustness.

[16]North Carolina, Maryland and Nevada inconsistently report district finances and are excluded from the sample. Washington DC is served by one public school district and is also excluded. Figure A.1 maps the coverage of states in the sample and the geographical distribution of reform status.

that makes up 92% of total district spending. Other current expenditures include teacher benefits and operational costs (support services and supplies). The remaining 8% of annual district expenditures are dedicated to capital spending on property, construction, and building rehabilitation. We diverge from the tradition of categorizing total expenditures into current and capital spending, instead dividing total expenditures into salary and non-salary components. Further, our salary measure does not include employee benefits and retirement earnings, a large fraction of total teacher compensation that Fitzpatrick (2015) argues is inefficiently funded. The subtle point is that all salary spending is current spending, but all non-salary spending is not capital spending. We provide a deep description of school expenditure layers in appendix Table A.1, and Figures A.2 and A.3.

Local property taxes have long been a contentious source of revenues for school districts. Despite the sheer volume of legislative reforms targeting budgetary reliance on property taxes, the average US school district raised 38% of total revenues via property taxes in 2015. Perhaps indicative of the uneven adoption of finance reforms, the NCES reported in 2017 that the share of revenues from property taxes varied from 17% to 53% by state. For the average district, the remaining funds come from state (48%) and federal (14%) sources, with 23 states receiving more than half of total funding from state governments.

2.3 Final Dataset

Following Jackson et al. (2016), we include two additional sets of control variables: (i) county level descriptive variables from 1960 such as the poverty rate, minority share, and rural population percentage, interacted with time trends and (ii) the amount of time elapsed since a state adopted or first funded various programs including Head Start, kindergarten, school desegregation, hospital desegregation, and Medicare certification. In all cases, the goal of adding these controls is to ensure that our empirical estimates are robust to possible heterogeneous trends across districts. The final data set consists of nearly 140,000 school district-by-year observations from 45 states and roughly 6,300 US school districts. Given that school districts in the final sample are limited to those with available house price data, we compare the finance data for our sample to the entire school district database in Table 1 and find no statistically significant differences in means.[17]

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. (2016) 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 describe the empirical specification for testing the efficiency of local public goods provision. We conclude this section with a discussion of two important practical issues related to our research design: (i) why we focus on salary spending in implementing the efficiency test and (ii) the general conditions under which this kind of stacked event study design can be used to estimate the effect of multiple endogenous variables.

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

[17]In the appendix we describe our procedure for computing the weighted average house price index for each school district. In Figure A.5, we show visually the average fraction of state total enrollment attending school districts with sufficient data to compute the weighted house price index. Figure A.7 shows the fraction of school district population living in a census tract where house prices are available.

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.[18] In fact, as described in Section 1, our efficiency test is premised on the notion that the effect of a marginal change in school spending financed through local taxes should be exactly zero if spending is efficient.

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

With these challenges in mind, the main empirical goal of our paper is to estimate the capitalization of school spending and local taxes into property values in a manner

[18]To give a sense of these endogeneity concerns in the context of our analysis: OLS estimates of the specifications shown in Table 6 below result in a coefficient on local property taxes that is positive and a coefficient on school spending that is close to zero.

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. (2016) to our context. This approach exploits the timing of court-mandated school finance reforms across US states to isolate plausibly 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."[19] In total, the existing school finance regime has been successfully challenged in 25 states since 1971 as shown in Table 2, which documents the date of the first court ruling in each state,

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

following the coding in Jackson et al. (2016).[20]

Table 2: First Year of Finance Reform as Mandated by State Supreme Courts

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

While successful challenges to existing school finance regimes often shared similar legal bases and the general goal of reducing inequality in school spending across students, the implementation of court-mandated school finance reforms varied widely across states, often requiring a lengthy back and forth between the state legislature and the courts until the final implementing legislation was deemed to have met the requirements of the state's constitution.[21] 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.[22] Each of these approaches embeds some form of redis-

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

[21]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.

[22]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).

tribution 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. (2016) research design.

3.3 First Stage SFR Event Study

The main idea underlying the school finance reform event study design developed in Jackson et al. (2016) 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. (2016) sort school districts by the quartile of per pupil school spending within the state in 1972 and form instruments for per pupil spending levels by interacting these initial spend quartiles with the time since the court first mandated a school finance reform.[23]

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

$$log(s_{d,t}) = \sum_{T=0}^{T=16} \sum_{Q_{72}=4}^{Q_{72}=1} \mathbb{1}(\mathbb{Q}) \times \mathbb{1}(\mathbb{T}) + f_d + \beta X_{d,t} + v_{d,t},$$

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

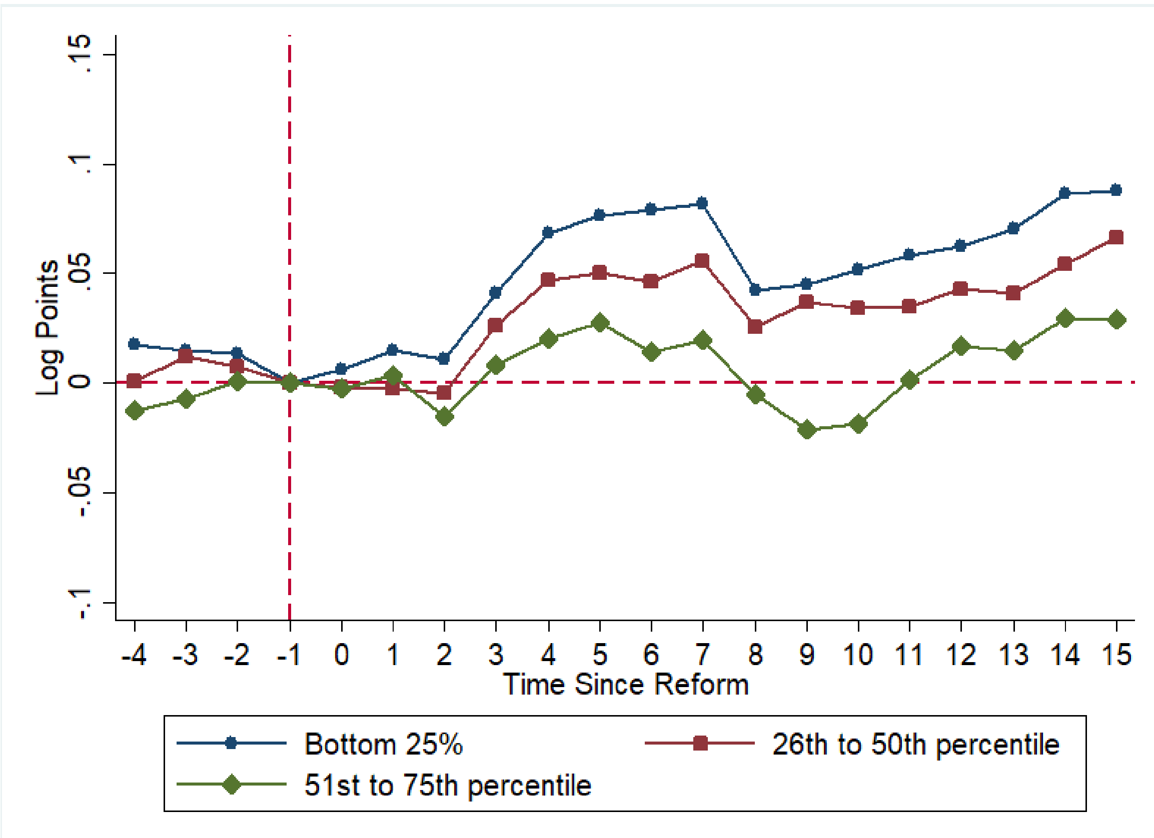
where:

- $s_{d,t}$ 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, and
- $v_{d,t}$: exogenous error term.

We plot the event study coefficients on the interacted instruments to trace out time paths of reform induced shocks to the components of school spending. This traditional approach is valuable in our framework as it allows us to lift the hood on the first-stage for total spending, and also disaggregate effects on salary spending, non-salary spending, and property taxes. When combined with a similar visualization of the reduced form effect of SFRs on house prices, we build our initial case for teacher salaries as the spending margin predominantly capitalized into house prices. Figure 1 highlights the broader variation in overall school spending isolated by the Jackson et al. (2016) instruments. In particular, the figure shows the predicted gap in spending between school districts in the bottom three quartiles of pre-reform spending quartile relative to the quartile that initially had the highest level of spending, conditional on district fixed effects.

Across all of the states that instituted such reforms, in the fifteen years following a court-ordered reform, spending increased in districts in the lower versus higher quartiles of the initial spending distribution. Notably, there is a small lag in the full realization of the reforms, reflecting the time it takes for the state legislatures to craft the implementing legislation.[24] There is also essentially no difference in trends in school expenditures across the four spending quartiles prior to a school finance reform, supporting 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.

Figure 1: Event-Study Estimates of The Reform Effect on School Spending

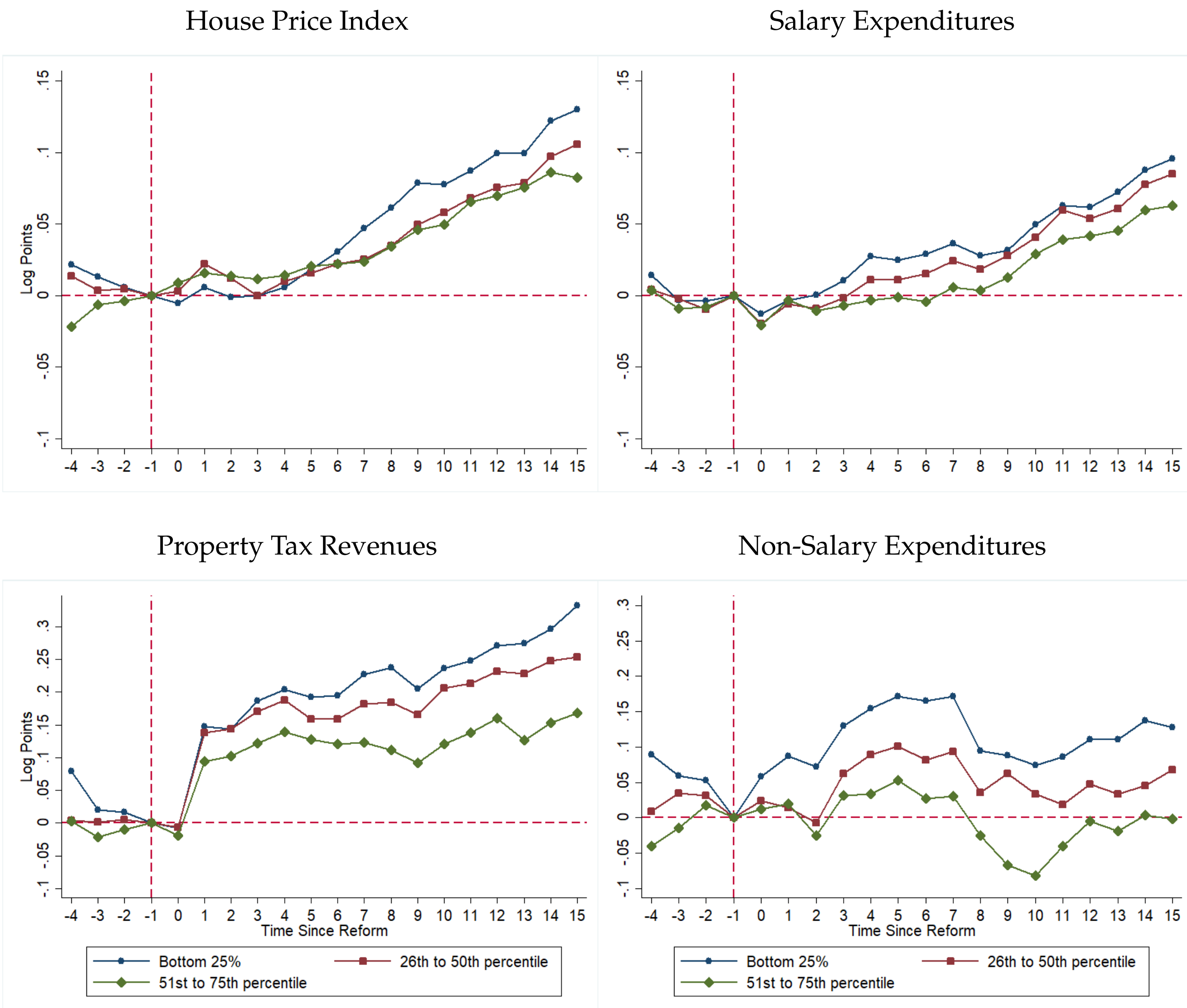


Notes: Event-study graph demonstrating the district per-pupil spending shock generated by state finance reforms. Of interest are a set of indicator variables that are equal to one for districts in a reform state *T years* relative to the reform year, interacted with indicators for the district spending quartile prior to reforms. The outcome is ln(total spending/pupil), thus the coefficients map percentage change in per-pupil spending due to the reforms. The reference group are school districts in the top quartile of historical school spending along with districts in non-reform states. Additional controls include policy controls for the concurrent rollout of healthcare and social service programs, 1960 county characteristics interacted with linear time trends, along with district and year fixed effects. For confidence intervals see appendix Figure C.1.

[24]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.

Notice that the Jackson et al. (2016) instruments effectively 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.

Figure 2: Event-Study Estimates of Reform Effects



Notes: Each plot includes event-study estimates for the effect of school finance reforms on house prices, property-tax revenues, salary and non-salary expenditures. Of interest are a set of indicator variables that equal to one for districts in a reform state *T years* relative to the reform year, interacted with indicators for the district spending quartile prior to reforms. All outcomes are in natural logs, thus the coefficients map percentage changes in each outcome due to the reforms. The reference group are districts in the top quartile of historical spending along with districts in non-reform states. Additional controls include policy controls for the concurrent rollout of healthcare and social service programs, 1960 county characteristics interacted with linear time trends, along with district and year fixed effects. For confidence intervals see appendix figures C.2-C.5.

Figure 2a plots the reduced form estimates for house prices, analogous to the spending event study figure shown above. Figure 2a shows that, starting a few years after the event date, house prices rose steadily in the initially lower spending quartiles (Q1-Q3) relative to the highest spending quartile (Q4). Like the corresponding changes in school spending, the relative increase in house prices was greatest for districts initially in the lowest spending quartile, with the difference in changes between Q1 and Q4 reaching a magnitude of 12-13% by the end of our 15-year post reform window.

Event study plots for salary and non-salary spending are shown in Figure 2b and 2d, revealing distinct variation in the time paths across quartiles following SFRs. In particular, salary spending increases smoothly and steadily in the initially lowest spending districts relative to those in top quartile, much like the pattern observed for house prices. In this case, the relative increase of the difference in changes between Q1 and Q4 reaches a magnitude of 7-8% by the end of our 15-year post reform window. The impact of SFRs on non-salary spending, on the other hand, is much lumpier with the largest differences occurring in years 4-7 post reform.[25] Unlike the steadily increasing pattern for salary spending, none of the large swings in relative non-salary spending are immediately evident in the corresponding house price figure event study plot. The correlation (or lack thereof) between the time paths of housing prices and salary and non-salary spending easily observed in Figure 2 strongly foreshadows the IV results to come in the next section of the paper.

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,

[25]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.

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 these reforms often led to increased 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). Following Jackson et al. (2016), 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.

Figure 2c shows the dynamics of local property tax revenues following a school finance reform, again separating districts into quartiles based on initial school spending in 1972. 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 compared to the previous figures. In particular, local tax revenue increased sharply in Q1-Q3 districts relative to Q4 in the first year immediately following the reform and then gradually thereafter. As we discuss below, this variation in the timing and extent of the changes across quartiles relative to the variation in school spending shown in Figures 2b and 2d provides the basis for separately identifying the capitalization of school spending and local property tax revenues.

3.5 IV Estimation and a Test of Efficiency

It is important to emphasize that the goal of our analysis is not to understand the effects of (particular) school finance reforms but to use them instrumentally, 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 in a single study. Of interest are the coefficients θ and γ from the baseline specification

$$
\begin{aligned}
log(p_{d,t}) =& \theta log(s_{d,t}) + \gamma log(\tau_{d,t}) + f_d \\
& + \beta X_{d,t} + \sum_{T=-4}^{T=-1} \sum_{Q_{72}=4}^{Q_{72}=1} \left[\lambda_{Q,T} \mathbb{1}(\mathbb{Q}) \times \mathbb{1}(\mathbb{T}) \right] + \epsilon_{d,t}
\end{aligned} \tag{3}
$$

where $p_{d,t}$ are average district house prices, $s_{d,t}$ is school spending per pupil and $\tau_{d,t}$ indicates local property tax revenue per pupil. To estimate equation 3, we instrument for both per pupil school spending and property tax revenue with the 1972 spend quartile by time since reform instruments laid plain in the event-study plots. Our model includes the pre-period interactions as exogenous covariates, along with district fixed effects and time-varying district controls.

Equation 3 estimates the capitalization of school spending and local taxes, the key parameters needed to implement the test of the efficiency of local public goods provision developed in Section 1. To properly implement the test, it is important to recognize a major difference in the way that school districts typically fund current expenditures such as salaries versus capital expenditures like major infrastructure improvements. In particular, a large fraction of non-salary spending on infrastructure is funded through school bonds, with the increase in spending then accompanied by future debt obligations (unobserved in our study) and not fully paid for out of current tax revenue. As a result, we would expect the capitalization of expenditures financed through bonds to capture the combined effect of both current spending (positive) and future debt obligations (negative) and, importantly, an appropriate test for the efficiency of capital spending would examine whether this combination of spending and future debt obligations has any effect on housing prices. Such a test for the efficiency of capital spending is discussed and implemented in Cellini et al. (2010) and Martorell et al. (2016) using a close-elections research design in school bond referenda.

With this important distinction between spending financed from current tax revenue versus future bond obligations in mind, the focus of our paper is on developing and implementing the efficiency test for the efficiency of a category of spending that we can be confident is funded completely from current revenues - salary spending. A test for the efficiency of salary spending is new to the literature. We are especially interested in testing for the capitalization and efficiency of salary versus non-salary spending given the recent evidence summarized in Hanushek (2003) and Jackson (2018b) suggesting that there is heterogeneity in the impact of different types of spending on student outcomes. Recent studies by Loeb and Page (2000), Hyman (2017), and Baron (2021), in particular, find clear evidence that increased salary spending improves student outcomes.

Formally, we implement the efficiency test by estimating the following equation:

$$\begin{aligned} log(p_{d,t}) =& \theta_1 log(sal_{d,t}) + \theta_2 log(nonsal_{d,t}) + \gamma log(\tau_{d,t}) + f_d \\ & + \beta X_{d,t} + \sum_{T=-4}^{T=-1} \sum_{Q_{72}=4}^{Q_{72}=1} \left[\lambda_{Q,T} \mathbb{1}(\mathbb{Q}) \times \mathbb{1}(\mathbb{T}) \right] + \epsilon_{d,t} \end{aligned} \tag{4}$$

where: $sal_{d,t}$ and $nonsal_{d,t}$ indicate salary and non-salary spending per pupil, which we instrument for using the time since reform interacted with the pre-reform spending quartile. Using the coefficients θ_1 and γ on salary spending and local property tax revenue, the Brueckner-Oates efficiency test for salary spending is given by:

$$H_0 : \sigma\theta_1 + \gamma = 0 \tag{5}$$

where σ is the ratio of average property taxes to average salary spending. This adjustment is necessary because spending, taxes, and prices enter equation (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 receive more value from an additional dollar raised and spent on local public goods. Importantly, broader notions of social efficiency would need to include the benefits of any positive externalities that better funded schools provide indirectly to others.[26] Any future education spillovers in the labor market or taxes collected by the government due to the higher wages of children attending the better funded schools would not be included here. Hendren and Sprung-Keyser (2020) estimate that many forms of social spending, especially programs that benefit young children, more than pay for themselves in discounted future tax receipts.

3.6 Instrumenting for Multiple Variables in an Event Study Design

A natural concern that arises when instrumenting for multiple variables in an event study design is whether the model is identified in a meaningful sense. From a purely counting perspective, of course, 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. But this counting exercise does nothing to ensure that there is enough meaningful independent variation to separately estimate the impact of two (or more) endogenous variables.

In practice, whether a particular application of this kind of research design generates enough statistical powerful to precisely estimate multiple causal channels is primarily a function of whether the underlying events create a distinct pattern of independent ex-

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

ogenous variation in each of the variables of interest. In our setting, this means that a necessary condition to separately identify θ_1, θ_2, and γ in equation (4) is that the extent and timing of local property tax, salary and non-salary changes across quartiles following SFRs vary substantially from one another.

Fortunately, as the various panels of Figure 2 highlight, these three key endogenous variables exhibit distinct time paths of variation across quartiles following school finance reforms. As a result, in the IV regressions below, we are able to estimate the key parameters with enough precision to reach meaningful economic conclusions. That the exogenous variation that respectively identifies these three variables - local taxes, salary, and non-salary spending - is fairly orthogonal will also be evident in the IV regression results presented below, as the estimated effect of each of these three endogenous variables is not particularly sensitive to whether the other variables are included in the analysis. It is important to recognize that such identifying variation is not generically guaranteed when using this kind of event study design, but is instead a matter of whether the events that form the basis for a particular study generate the needed richness in variation.[27]

4 Results

4.1 The Capitalization of School Spending

Table 3 reports the results of IV regressions of housing prices on school spending. The four columns 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.

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

(2016) and are intended to absorb any potential heterogeneous trends in house prices across different types of school districts.

The third column of Table 3 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. (2016) 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.

Table 3: Baseline IV Results for the Effect of School Spending on House Prices

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				✓

Notes: Standard errors reported and are clustered at the district level. In all models we instrument for endogenous per-pupil spending variable with the event-time shocks from school finance reforms. Census controls include historical census of 1960 county characteristics interacted with linear time trends. Policy controls included the timing of state adoption of Head Start, kindergarten, school desegregation, hospital desegregation, and Medicare certification. Data coverage is a calculated as a the share of total district population living in a census tract with house price data available. Data coverage is described further in the appendix.

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 1, is on the order of 5-10%.

The magnitude of the point estimates in Table 3 imply that households are willing to pay substantially more for access to better funded schools. The size of these estimates is consistent with the substantial effects of increased school spending on children's life outcomes documented in Jackson et al. (2016) and Lafortune et al. (2018). Jackson et al. (2016), 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 3 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 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. Focusing on expenditures for labor inputs is a step towards measuring how households value the contribution of teachers, aides, administrators, and staff in the school production function. Further, our empirical efficiency test is guidance for how close district staffing levels are to what households consider optimal. We document robust evidence for a house price response to salary spending that is larger in magnitude than the capitalization of aggregate current spending, and capital spending. [28]

Tables 4 and 5 present results for a series of specifications analogous to those included in Table 3 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. 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.

Table 4: Baseline IV Results for the Effect of Salary Spending on House Prices

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				✓

Notes: Standard errors reported and are clustered at the district level. Salary spending is district level spending on salaries for instruction, administration, and operations. The share of total salaries spent on instruction is roughly 74% and has remained consistent over time. In all models we instrument for endogenous salary spending per-pupil with the event-time shocks from school finance reforms. See Table 3 for a complete description of the various additional controls. More details of salary expenditures can be found in the appendix.

That spending on salaries is so highly valued by households suggests that house-

[28] Conducting our main analysis along the division of current and capital spending is in alignment with the literature on the heterogeneity in school spending channels. Results for the first-stage response of current and capital spending are shown in appendix Figure B.1. The results of our IV estimation are shown in appendix Table B.5.

Table 5: Baseline IV Results for the Effect of Non-Salary Spending on House Prices

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				✓

Notes: Standard errors reported and are clustered at the district level. Non-salary spending is computed as total district expenditures minus salaries for instruction, administration, and operations. In all models we instrument for endogenous non-salary spending per-pupil with the event-time shocks from school finance reforms. See Table 3 for a complete description of the various additional controls.

holds 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).[29] Interestingly, Jackson et al. (2016) 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. 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 4, 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.[30] Because salary spending is financed from concurrent

[29] 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.

[30] 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 3, 4 and 5, are 0.938 (0.143), 1.463 (0.237), and -0.010 (0.072), respectively.

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 Property Tax Revenues and School Spending Efficiency

Table 6 presents the results of IV regression that add the log of local property tax revenues to the specifications reported in Table 3. In this case, we instrument for both log school spending and log local property tax revenue using the school finance reform event study design. As expected, local property taxes enter 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 3. 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.

Table 6: IV Results for the Effect of Total Spending and Property Taxes on House Prices

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				✓

Notes: Standard errors reported and are clustered at the district level. Property tax revenue is district-reported revenue from local property taxes. In all models we instrument for endogenous spending and property tax revenue per-pupil with the event-time shocks from school finance reforms. See Table 3 for a complete description of the various additional controls.

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 7 report results for a series of log house price regressions analogous to those reported in the final columns of Table 3 and Table 6, respectively, but with spending broken down into salary and non-salary components. As in Table 4, 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 2c, 2b, and 2d 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 7. 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 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.[31]

A comparison of the size of the coefficients on log property 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

[31] We have also estimated a version of the specification shown in Column 2 of Table 7 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.

Table 7: IV Estimates of Salary Spending Efficiency

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: 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	✓	✓	✓	✓

Notes: Standard errors reported and are clustered at the district level. All models include the complete set of census, policy and data coverage controls described in Table 3. In all models we instrument for endogenous variables shown with the event-time shocks from school finance reforms. The hypothesis test for the efficiency elasticity of salary spending is an empirical Oates test as described in section 3.5.

Table 7 reports the results of the Brueckner-Oates efficiency test calculated for an increase of 1% 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.

Converting our efficiency estimates into dollar terms, we find that a tax-funded increase of $1 per pupil would increase monthly rents by $0.14 to $0.16 (see Appendix Table B.2 for details). The p-value for our efficiency calculation in levels is also below 0.0001. We therefore find that spending on salaries is inefficiently low when we report our results in levels as well as logs. To contextualize this result, we consider the salary spending gap of $1,132 between schools in the highest and lowest spending quartiles shown in Table 1. Our estimates suggest that households are willing to pay $158 more in monthly rent for a tax-funded increase in spending on salaries that closes the salary spending gap between the highest and lowest spending districts. Equally, we estimate that households are willing to pay an additional $252 in rent for a one standard deviation increase in salary spending ($1,800 per pupil) that is tax-funded.

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 3 - 7 but with the fraction of children in poverty as the dependent variable. The pattern of results shown in Table 8 is remarkably consistent with the house price regressions. The first column reports the results of a specification analogous to the fourth column of Table 3, 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 8 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.

Table 8: Estimating the Effect of School Spending and Taxes on Income Sorting

Outcome: % of Students in Poverty	(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	✓	✓	✓

Notes: Standard errors reported and are clustered at the district level. All models include the complete set of census, policy and data coverage controls described in Table 3. In all models we instrument for endogenous variables shown with the event-time shocks from school finance reforms. Students in poverty are 5-17 year olds living within district boundaries in households with total income below the poverty line. Poverty estimates for school districts are computed annually beginning in 1993 by the census small area income and poverty estimates (SAIPE) program.

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 9 repeats the earlier house price specifications reported in tables 3-7 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 9 re-estimates our baseline specification from column (4) of Table 6 for a sample that begins in 1993. The coefficient on school spending is significantly greater in this sub-sample perhaps because the early 1990s included an economic recession. The third column of Table 9 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.

Table 9: IV Estimates of Salary Spending Efficiency Accounting for 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)
Percent of Students in Poverty			-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)		✓	✓	✓	✓

Notes: Standard errors reported and are clustered at the district level. All models include the complete set of census, policy and data coverage controls described in Table 3. In all models we instrument for endogenous variables shown with the event-time shocks from school finance reforms. Students in poverty are 5-17 year olds living within district boundaries in households with total income below the poverty line. Poverty estimates for school districts are computed annually beginning in 1993 by the census small area income and poverty estimates (SAIPE) program. The hypothesis test for the efficiency elasticity of salary spending is an empirical Oates test as described in section 3.5.

The results reported in Table 9 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 32 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 fraction 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% for the efficient elasticity of salary spending and the corresponding 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.[32]

Table 10: Testing for Treatment Effect Heterogeneity Across Groupings of Quartiles

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: Salary Spending				
% 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	✓	✓	✓	✓

Notes: Standard errors reported and are clustered at the district level. In all models we instrument for endogenous variables shown with the event-time shocks from school finance reforms. See Table 3 for a full description of the complete set of controls.

To examine whether the implicit homogeneity assumption is reasonable, Table 10 re-

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

ports 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 10. 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 11 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 Brueckner-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.

Table 11: IV Estimates of Salary Efficiency with Forward Looking Prices

	(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: 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	✓	✓	✓	✓

Notes: Standard errors reported and are clustered at the district level. In all models we instrument for endogenous variables shown with the event-time shocks from school finance reforms. See Table 3 for a full description of the complete set of controls.

6 Conclusion

The efficient provision of public goods is a question that has animated the economics profession for nearly a century (Musgrave 1939; Samuelson 1954; Tiebout 1956; Oates 1969;

Brueckner 1979; Yinger 1982; Bagnoli and Lipman 1992; Barrow and Rouse 2004; Cellini et al. 2010; Nguyen-Hoang and Yinger 2011). National expenditure on public goods both in the US and abroad constitute a large share of national GDP. Moreover, determining the efficient level of provision is difficult precisely because public goods require the elicitation of individuals' willingness to pay in a context where free-riding is endemic.

Harkening back to a classic literature, we show that even in a model with households with heterogeneous preferences that the theoretical insights of Brueckner (1979) and Brueckner (1982), foreshadowed presciently by Oates (1969), yields an equivalency between the Samuelson (first order) condition for efficient public good provision and utility equalization in spatial equilibrium. This equivalence permits us to use the housing market to test for the efficiency of school spending on teachers, without relying on the strong assumptions inherent in the Tiebout (1956) model. The theory also provides clear guidance for how to use aggregate data on quality-adjusted house prices and the natural occurring variation in school spending on salaries and tax revenues used to fund school, which we show exist in the school finance reform instruments introduced by Jackson et al. (2016).

To identify the causal impact of salary 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). We pair panel data on local house prices over the 25-year period with finance data for over 6,000 districts experiencing state finance reforms at different points in time, along with those undergoing no reform at all. Our event study results show that the variation in spending on teacher salaries is particularly clean and well-suited to the efficiency test at the heart of our paper, because salaries are funded out of current spending and, unlike non-salary spending, not funded by bonds. In addition to providing independent variation in salary spending and local taxation, a key advantage of our empirical design is that the resulting estimates are based on a national sample of school districts rather than a single state or metropolitan area, which is common in the literature given how challenging it is to credibly test for efficiency.

We find that house prices are sharply increasing in spending on salaries and actually slightly decreasing in other forms of non-salary spending. 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 (Cellini et al., 2010). 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, which we can do because we are not only formally over-identified, but our event studies show distinctly different time paths for the salary spending and tax variation.

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. Real average wages for teachers in the US have not increased since 1990 – if anything they have slightly decreased from \$59,116 in 1990 to \$58,136 in 2017 (National Center for Education Statistics 2019a). During this time period, real median income increased from \$52,008 to \$57,423 and real expenditures per pupil increased from \$9,741 to \$13,634 (US Census Bureau 2021; National Center for Education Statistics 2019b). Moreover, compared to similarly credentialed workers in the 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 (Johnson and Jackson 2019; Loeb and Page 2000; Johnson and Nazaryan 2019).

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Online Appendix

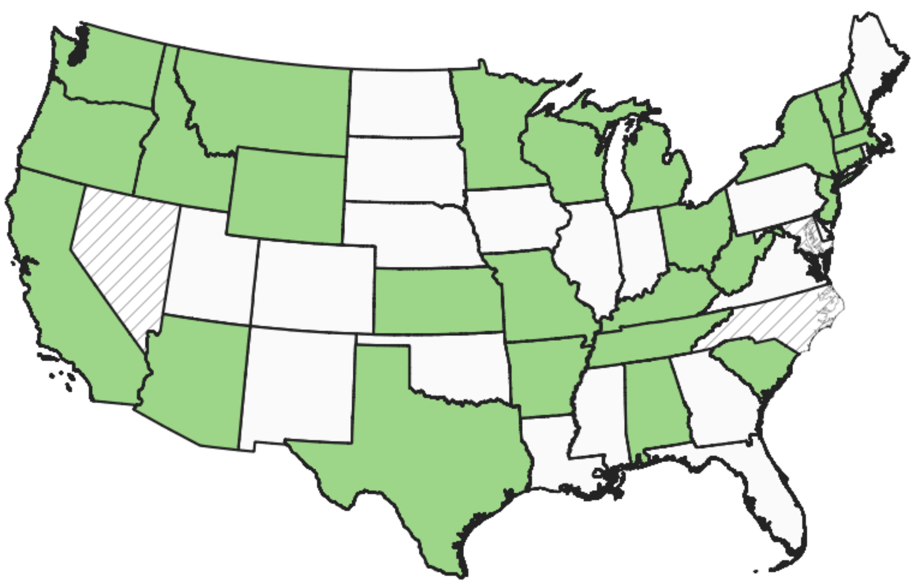
Are We Spending Enough on Teachers in the US?

Patrick Bayer, Peter Blair, and Kenneth Whaley

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

The data in our sample cover 45 of the lower 48 states from 1990-2015. North Carolina, Maryland, and Nevada do not comprehensively or consistently report district finances during the sample period and are excluded. Washington DC is served by one public school district and is also excluded. In Figure A.1, the geographic distribution of reform and non-reform states in our sample is shown. Reform states (including those with pre-1990 reforms) are coded to match Jackson et al. (2016) (exluding Michigan, as discussed in the property tax section of the appendix).

Figure A.1: Sample States by Reform Status



Notes: The geography of state finance reforms. Shaded states have at least one reform, non shaded states are coded as no-reform, striped states are not included in the sample.

The treatment window for the IV design begins in the reform year and ends 16 years post-reform. California, Kansas, and New Jersey are coded into the control group as each state experienced the initial reform prior to 1974 thus will not have sample years that fall within the treatment window. For the second component of the IV, school district finances from 1972 are used to estimate the pre-period state spending distribution and categorize districts into spending quartiles. The Census of Governments finance data is the sole provider of public data describing district finances as early as 1972, and for consistency is the primary data source for this study.[33]

A1. Categorizing District Finances

Table A.1: School Finance Variables

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

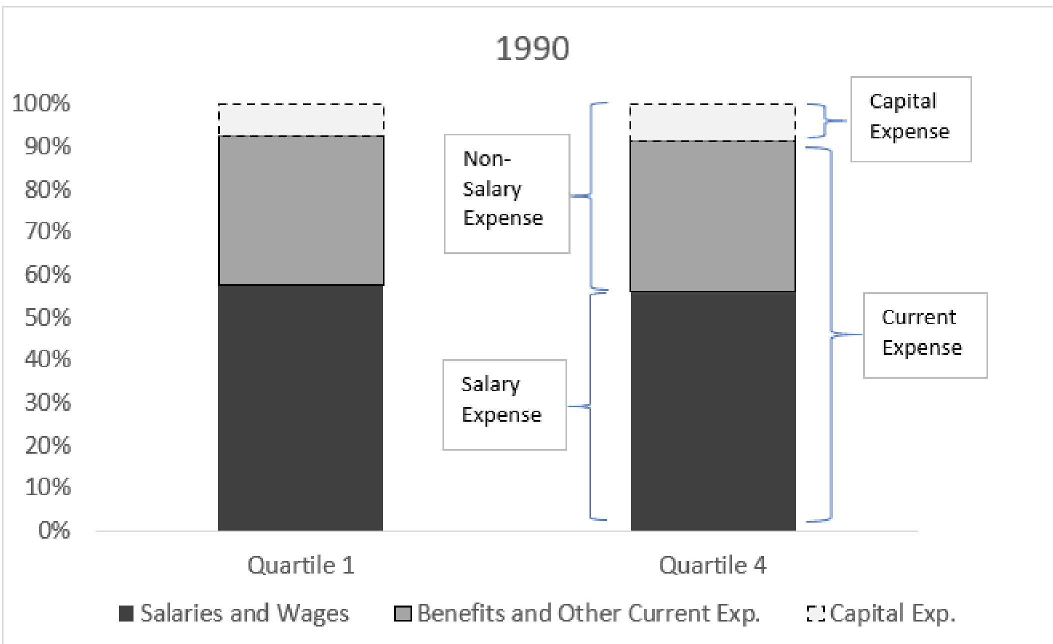
Notes: Table reference for the definitions of relevant school finance variables. Salaries and wages measured in this paper do not include benefit payments.

Our key results focus on salaries and distinguish between the elasticities of salary and non-salary spending. Table 3 details district finances and documents precisely what

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

the salary and non-salary variables measure. Salaries and wages broadly fall within the broader category of current expenditures, which make up about 92% of total. Other current expenditures include employee benefit payments, spending for educational and student support services, and supplies. The remaining 8% is capital spending on property, construction, and building rehabilitation. Figure A.2 shows the percentage split between current and capital expenditures is remarkably consistent between low and high expenditure districts.

Figure A.2: Spending Composition, 1990

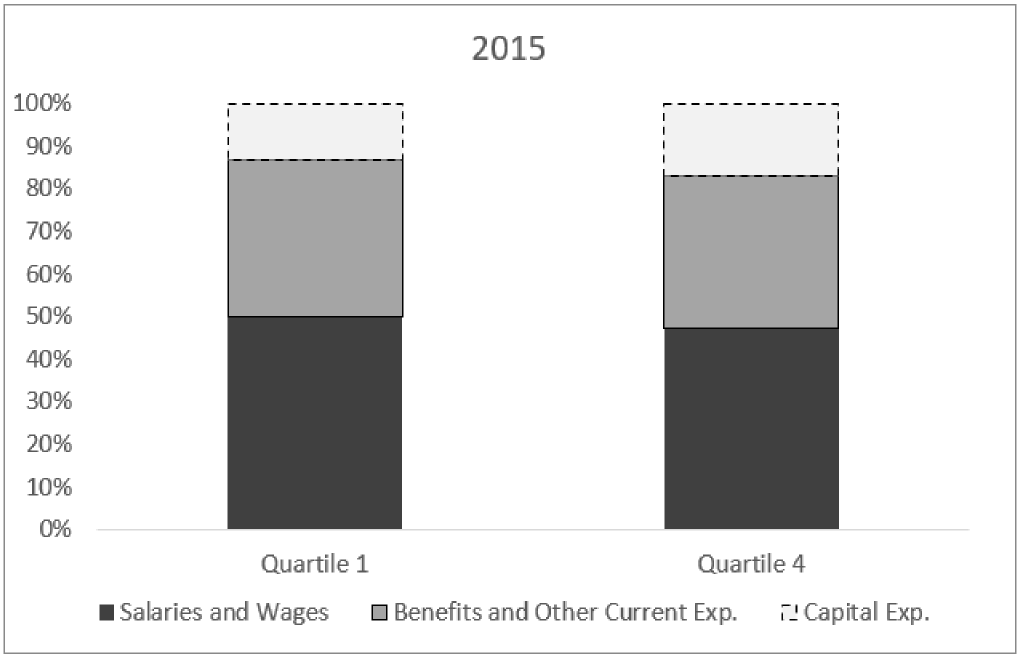


Notes: This diagram shows the allocation of school district expenditures to salary and non-salary expense categories in 1990. Non-salary expenses include both capital and current expenses not including wages and salaries. The composition of spending is remarkably consistent between low-spending (quartile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

A2. Spending on Salary and Non-Salary Inputs

We separate salaries and wages from other components of current spending. Employer benefit payments for employee retirement accounts and healthcare (medical, dental, and vision) are not included in our salaries and wages measure. Thus the non-salary spending

Figure A.3: Spending Composition, 2015



Notes: This diagram shows the allocation of school district expenditures to salary and non-salary expense categories n 2015. Non-salary expenses include both capital and current expenses not including wages and salaries. Similar to 1990, the composition of spending is consistent between low-spending (quartile 1) and high-spending (quartile 4) school districts.

measure is a combination of capital spending and current spending outside of salaries and wages, as shown in Figure A.2. Comparing Figure A.2 and figure A.3 shows the share of spending dedicated to salaries and wages has decreased, with increases in the share of both non-salary categories during the sample period.

There is available district data for salary and non-salary spending from 1990-2015. However, NCES did not report detailed salary breakouts until 2000, reporting expenditures for instruction (teachers and assistant teachers), administration (including pupil support services such as counselors), and operations (transportation, food service, maintenance). As a check, we compute the fraction of total salary spending dedicated to the three broad categories for the year 2000 (and 2015): instruction, 74% (73%); administration, 14% (15%); and operations 12% (12%). There are little changes in the salary breakdown over time, with instructional salaries (teachers and teaching assistants) representing the largest share. [34]

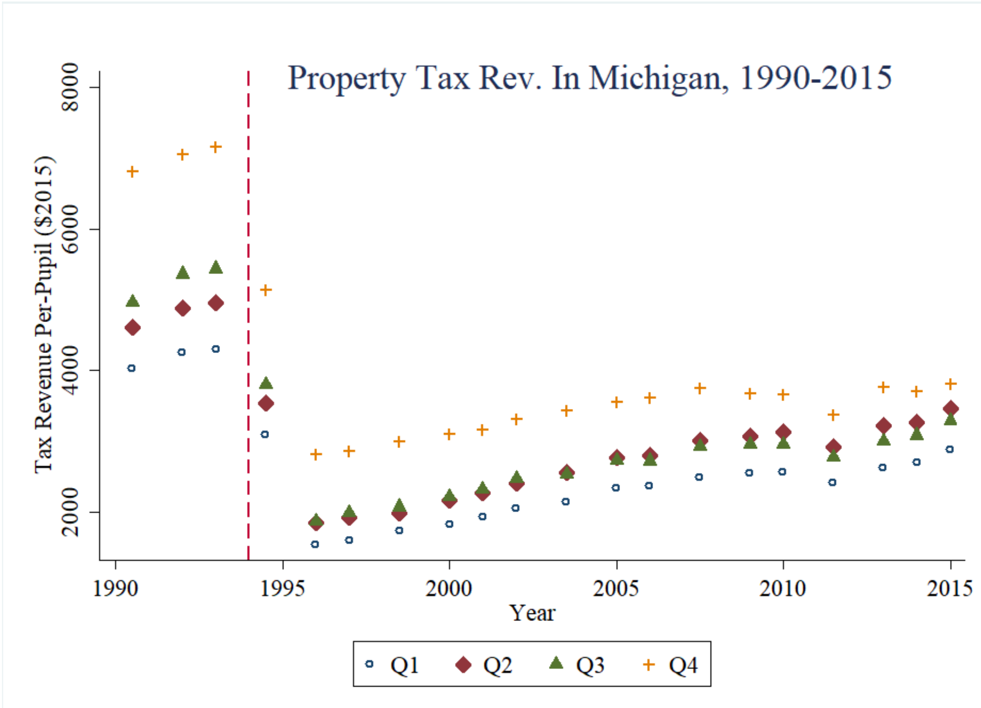
A3. Property Tax Revenues

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

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

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

Figure A.4: Coding Michigan Reform Based on Proposal A (1994)

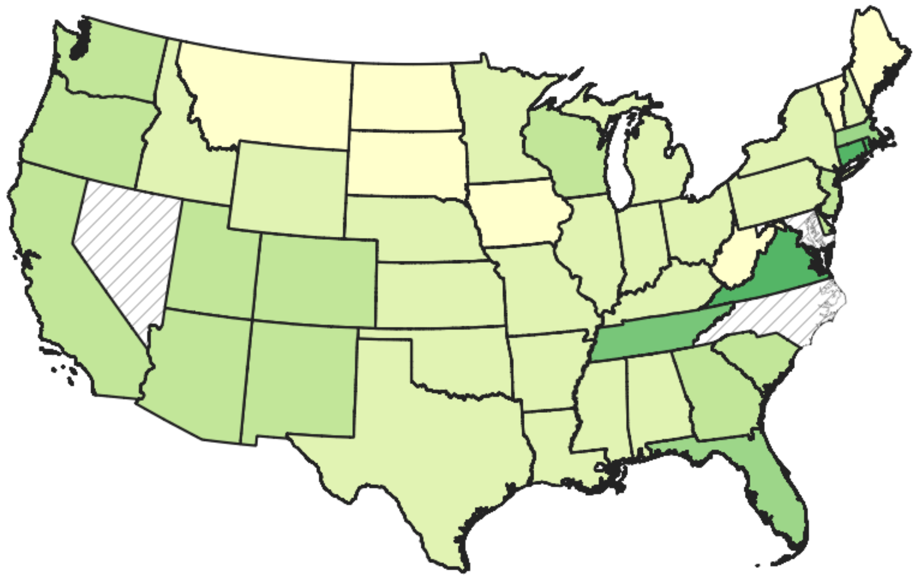


Notes: The 1994 passage of Proposal A in Michigan immediately restructured the funding of schools away from property taxes. Our coding of the reform date in Michigan differs from the literature but follows the implementation of Proposal A.

A4. House Price Data

The second major piece of data is the FHFA house price index used to compute district level house prices. It follows that our sample is constrained to the geographies where price estimates are available from 1990-2015. We refer to the share of a district (or state) population that lives in a census tract where house price data is available as *house price coverage*. Spatial aggregation of the house price data from census tracts to school districts has the potential to create two sources of measurement error, as there are census tracts within district boundaries where house prices are unobserved. Foremost, non-classical measurement error is a concern if house price coverage is systematically correlated with the likelihood of a state undergoing a school finance reform. We show visually in Figure A.5 average house price coverage by state, defined as aggregate enrollment for districts with house price data divided by total state enrollment for all districts in the sample.

Figure A.5: The Share of Aggregate State Enrollment Included in the Sample



Notes: School districts missing house price data are excluded from the final sample. Here, states with darker shading have a higher share of total enrollment within districts that have house price data.

Visual inspection of figures A.1 and A.5 shows that state house price coverage is largely independent of reform status. Further, the house price data becomes more robust in coverage over time, as more tract level data for prices becomes available from FHFA. The following section details the district house price aggregation step.

A5. District 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.[35] 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

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

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 the 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 $\mathbf{J}$ 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

$$\omega_j = \frac{n_{j,90}}{\sum_{j=1}^{J} n_{j,90}}; \tag{6}$$

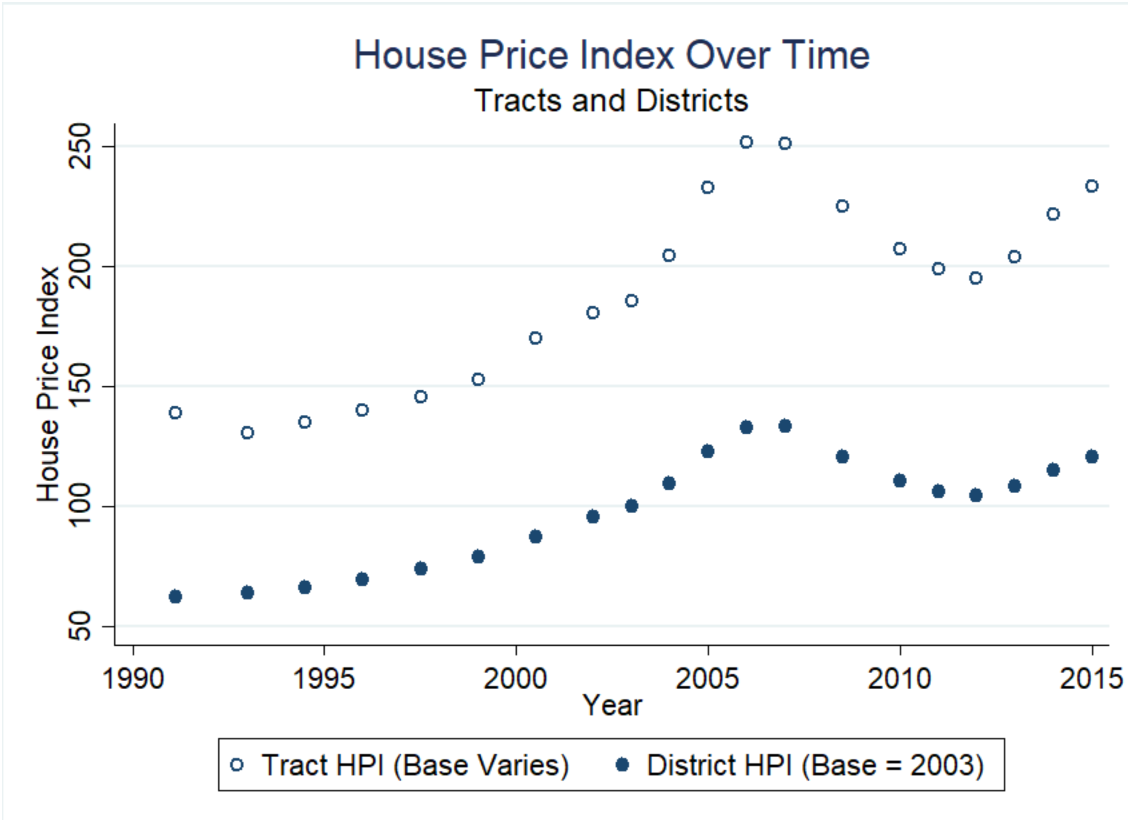
where $\sum_{j=1}^{J} \omega_j = 1$. Thus our district-level price outcome is the population weighted tract average in each year

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

Figure A.6 is a binned scatter plot of the mean district price $\mathbf{P_{d,t}}$ and tract raw price $\tilde{p}_{j,t}$. 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.

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

Figure A.6: House Price Index (HPI) Over Time



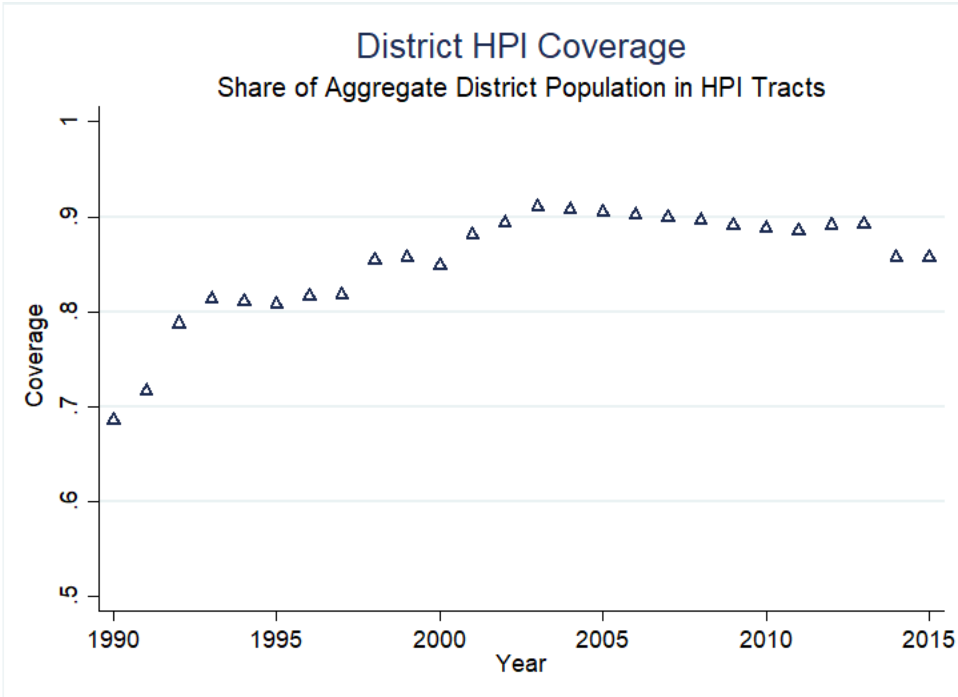
Notes: Binscatter comparing the mean HPI at the district and census tract level over time. District HPI is the population-weighted average of census tract HPI within the attendance boundary. The level difference between the two measures is attributable to differences in base years.

coverage in a district is defined as

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

where $\mathbf{1}(\mathbf{P_{j,t}}) = 1$ if tract HPI is observed in year t. Figure A.7 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 A.7: Coverage: Share of aggregate district population in a tract reporting HPI.



Notes: Binscatter shows the coverage rate rises during the sample period. Tract population is fixed to 1990, thus increases in data coverage is caused by increased availability of house price data at the census tract level.

A6. 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.

A7. House Price Capitalization Literature

We employ a standard expression of house values reflecting the change in households willingness to pay for attributes of the housing unit or local community attributes h, g, and t. There is a broad literature around the house price capitalization of marginal changes to public goods, as well as the internalization of individual and firm behavior within the community. In Table A.2 we describe a small sample of papers studying the house price capitalization of various school quality attributes. An extensive review of this literature is conducted in the Handbook of The Economics of Education, Chapter 10 (Black and Machin 2011). A second strand of literature studies the capitalization of environmental amenities from crime to pollution. Table C.1 summarizes a small sample of the local amenity capitalization literature.

Table A.2: The House Price Capitalization of School Quality

Article	Setting	Data	Capitalization
Cook (2018)	U.S. - Ohio	Ohio property tax records 1998-2009	Increased access to charter schools statewide lowered property values and depressed the tax base for traditional public schools.
Imberman and Lovenheim (2016)	U.S. - Los Angeles County	60,000 housing transactions in Los Angeles County from 2009-2011	Public release of school and teacher value-add ratings have no statistical effect on house prices. There is evidence that test scores are positively capitalized into house prices.
Gibbons et al. (2013)	United Kingdom	Price and basic characteristics of all U.K. house sales, linked with schools from 2000-2006. The boundary discontinuity design sample restricts observations to sales within 700 meters of a school zone boundary.	Both school value-add measures and prior student achievement have positive effects on house prices.
Black and Machin (2011)	U.S.	-	Handbook chapter reviewing various methodological approaches in the house price capitalization literature prior to 2011.
Cellini et al. (2010)	U.S. - California	Sale prices and physical characteristics of California home purchases between 1988-2005.	Willingness to pay for school infrastructure improvements reflected by positive house price capitalization of district capital expenditures.
Figlio and Lucas (2004)	U.S. - Florida	Over 70,000 residential properties near elementary schools in 47 Florida counties.	The release of report cards assigning letter grades to schools significantly increased house prices for schools with an ″A″ rating, relative to ″B″ and ″C″ rated schools. The timing of the report card release explains the sorting of households with high achieving students to ″A″ rated schools, above and beyond observed performance of the school on standardized tests.

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

Table A.3: The House Price Capitalization of Local Amenities

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

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

Appendix B. Additional Analysis and Estimation

B1. IV/2SLS Estimation in Dollar Cost of Rent

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

Since the salary spending and taxes are already in levels, estimating a model in levels only requires us to convert the house price index into a dollar denominated price. We implement the conversion in two steps. We first compute the weighted average house value in each school district in 2000, denoted $V_{d,00}$, using data from the 2000 Decennial Census on reported house prices in each census tract and the census tract population weights ($\sum_{j=1}$) introduced in Appendix section A5:

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

We next convert this stock measure of average housing value in a school district into a flow measure of monthly rental cost in the school district. To convert from house value to rental cost, we calculate the average value to rent ratio in the 2000 Decennial Census, and use this constant to convert the weighted house prices that we previously calculated for each school district ($V_{d,00}$) into an equivalent monthly rental price. From Table B.1, we estimate that the average home value in a school district in 2000 was $210,459 and the national value to rent ratio was 206.36. Because we have a price level in 2000 for each school district and an time-series of the house price index, we can construct a time-series of house price levels using the HPI in a given year as a scaling factor of the rental prices in 2000. We then show results on the capitalization of school spending and taxes into house price levels using rental prices computed using the value to rent ratio at the national level, the MSA level and the school district level. We calculate the rental value in these three distinct ways to explore how sensitive our results are alternative ways of measuring the home to value ratio. In practice we will find that using price to rent conversion factor from the more local levels of geography, i.e. school district and MSA, produce the similar if not slightly more conservative estimates of the capitalization of school spending and property taxes into house prices.

Table B.1: Summary Statistics

	1990	2000	2010	2015
Weighted Avg. Census Home Value		$210,459 (135,558)		
National V-R Ratio		206.36		
Implied Monthly Rental Costs	$958.06 (582.53)	$1019.57 (657.05)	$1019.87 (755.71)	$1037.34 (885.65)

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

We divide $V_{d,00}$ by the estimated value to rent ratio, converting converting the asset value to a measure of monthly rental costs. We calculate the value to rent ratio at the national level, the MSA level, and school district level. The rental cost measure is extended over the entire panel period using the annual variation in our high frequency house price index. Row 3 of table B.1 shows the mean implied monthly rental costs for several years of the sample, deflated to 2015 dollar values.

After computing average monthly rent costs for each district $\times$ year observation, we turn to estimating the IV/2SLS specification. Dollar-valued rent costs are modelled as an outcome of salary and non-salary spending per-pupil in levels instead of logs. For completeness, we present corroborating results for the main table 7, and robustness tables 10 and 11. First, table B.2 shows analogous estimates of column 2 of table 7. Across columns 1, 2 and 3 of table B.2 we vary the calculation of rental costs with the value to rent ratio calculated at different levels of geography. The results of the efficiency analysis show that increasing salary spending by $1 through property tax funding will increase monthly rental costs by 14-17 cents. It follows that a one standard deviation increase in salary spending per-pupil ($\approx \$1,800$) will increase monthly rental costs by $252.

Table B.2: Rental Cost Capitalization of Salary and Nonsalary Spending Levels

Implied Monthly Rent ($2015)	(1)	(2)	(3)
Salary Spending	0.269***	0.244***	0.242***
	(0.0425)	(0.0388)	(0.0377)
Non-Salary Spending	-0.0339	-0.0218	-0.0163
	(0.0254)	(0.0249)	(0.0255)
Property Tax Revenue	-0.101***	-0.0966***	-0.0995***
	(0.0155)	(0.0143)	(0.0141)
Efficiency: Salary Spending			
% ΔPrice	0.168	0.147	0.143
Confidence Interval	[0.095, 0.242]	[0.081, 0.214]	[0.077, 0.208]
Observations	134,200	134,186	121,537
V-R Ratio	National	MSA	Local
Complete Set of Controls	✓	✓	✓

Notes: Standard errors reported and are clustered at the district level. In all models we instrument for endogenous variables shown with the event-time shocks from school finance reforms. See Table 3 for a full description of the complete set of controls.

We conduct robustness tests to understand the source of variation along the initial spending distribution (Table 10), and the potential for price appreciation to reflect the anticipatory behavior of households (table 11). For completeness we show that the rental cost results are robust to unobserved confounders concerning our primary model. In table B.3 we vary the quartile groupings of the instruments, and the efficiency test results remain positive and statistically significant. In our test to rule out forward looking behavior in table B.4, we compare column 1 to column 2, and 3 with column 4. Given that the estimates are largely indistinguishable, we find no statistical evidence for anticipatory behavior affecting the results.

Table B.3: Rental Cost Capitalization Using Different Sources of Variation

Outcome: Implied Monthly Rent ($2015)	(1) Q1 vs. Q2-Q4	(2) Q1 vs. Q2-Q4	(3) Q1-Q3 vs. Q4	(4) Q1-Q3 vs. Q4
Salary Spending	0.529***	0.464***	0.407***	0.355***
	(0.197)	(0.176)	(0.107)	(0.0949)
Non-Salary Spending	-0.0228	-0.0193	-0.141	-0.135
	(0.141)	(0.133)	(0.148)	(0.135)
Property Tax Revenue	-0.0451	-0.0468	-0.0240	-0.0246
	(0.0350)	(0.0309)	(0.0230)	(0.0201)
Efficiency: Salary Spending				
% ΔPrice	0.484	0.416	0.383	0.331
Confidence Interval	[0.122, 0.846]	[0.092, 0.742]	[0.184, 0.582]	[0.153, 0.509]
Observations	134,200	134,186	134,200	134,186
V-R Ratio	National	MSA	National	MSA
Complete Set of Controls	✓	✓	✓	✓

Notes: Standard errors reported and are clustered at the district level. In all models we instrument for endogenous variables shown with the event-time shocks from school finance reforms. See Table 3 for a full description of the complete set of controls.

Table B.4: Rent Cost Estimates of Salary Efficiency with Forward Looking Prices

Outcome: Implied Monthly Rent ($2015)	(1)	(2)	(3)	(4)
Total Spending	0.0847***	0.0914***		
	(0.0187)	(0.0205)		
Salary Spending			0.335***	0.261***
			(0.0632)	(0.0437)
Nonsalary Spending			-0.0556**	-0.0173
			(0.0271)	(0.0260)
Property Tax Revenue			-0.0798***	-0.0896***
			(0.0215)	(0.0134)
Efficiency: Salary Spending				
% ΔPrice			0.255	0.171
Confidence Interval			[0.126, 0.384]	[0.093, 0.249]
Observations	128,363	128,363	128,363	128,363
V-R Ratio	National	National	National	National
Dependent Variable is a Lead (t+1)	X	✓	X	✓
Complete Set of Controls	✓	✓	✓	✓

Notes: Standard errors reported and are clustered at the district level. In all models we instrument for endogenous variables shown with the event-time shocks from school finance reforms. See Table 3 for a full description of the complete set of controls.

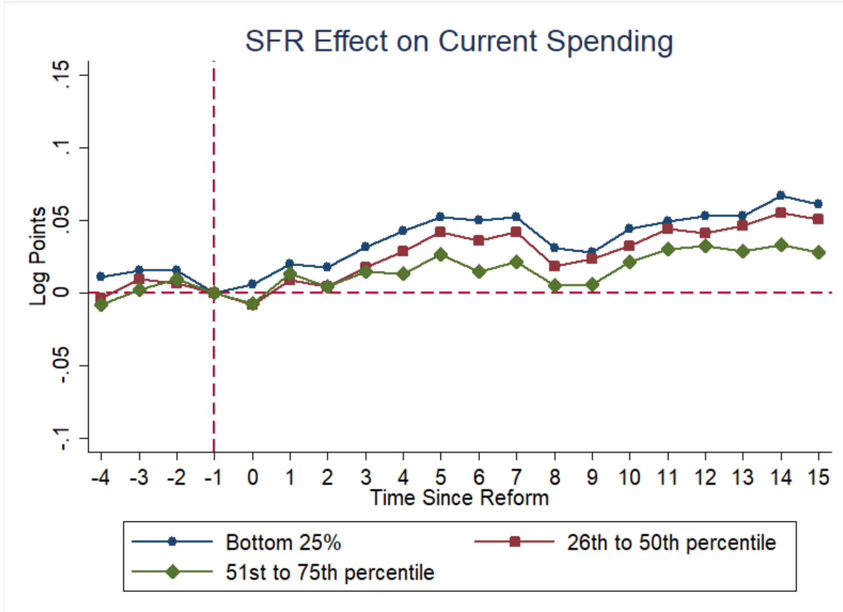
B2. Current and Capital Spending

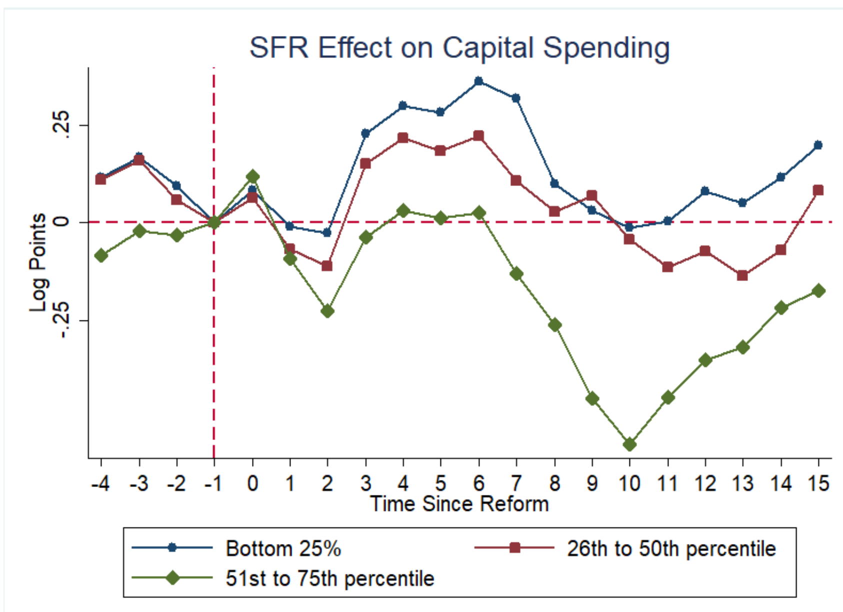
In appendix A1 we describe the many layers of school expenditures and our decision to categorize spending into salary and non-salary components. One traditional approach is to instead divide total spending into current and capital expenditures. In this section we carry out our analysis across current and capital expenditure categories. First we produce visual evidence of the first-stage effect of reforms on current and capital spending, by plotting the event-study coefficients. Second, we show results from the primary IV estimating equation - with current and capital spending per-pupil taken as the endogenous variables of interest instead of the salary/non-salary division of the main paper.

The event-study plots show the response of total current spending to the reforms is smaller than the visual effect we show for salary spending in Figure 2b. Since salary spending is a subset of current spending, the implication is that salary spending showed the sharpest increase of all current spending types following the reforms. The second plot shows the up and down response of capital spending to the reforms, underlying the lumpy variation in the non-salary spending diagram Figure 2d.

Table B.5 displays the results of our main estimating model with expenditures divided between current spending and capital spending. The point estimates for current spending are positive and similar in magnitude to estimates for total spending per-pupil. We find small, positive increases in house prices following increases in capital spending. The direction of our results are in line with Cellini et al. (2010).

Figure B.1: First-stage event-study of current and capital spending





Notes: Event-study graph demonstrating the district per-pupil spending shock generated by state finance reforms. Of interest are a set of indicator variables that equal to one for districts in a reform state *T years* relative to the reform year, interacted with indicators for the district spending quartile prior to reforms. The outcome is ln(total spending/pupil), thus the coefficients map percentage change in per-pupil spending due to the reforms. The reference group are school districts in the top quartile of historical school spending along with districts in non-reform states. Additional controls include policy controls for the concurrent rollout of healthcare and social service programs, 1960 county characteristics interacted with linear time trends, along with district and year fixed effects.

Table B.5: Capitalization of Current and Capital Expenditures (IV/2SLS)

Outcome: Log(HPI)	(1)	(2)	(3)	(4)
Log(Current Spending)	0.992***	1.014***	1.400***	
	(0.245)	(0.235)	(0.307)	
Log(Capital Spending)	0.225***	0.210***		0.269***
	(0.0545)	(0.0547)		(0.0495)
Log(Property Tax)		-0.158***	-0.208***	-0.149***
		(0.0525)	(0.0733)	(0.0555)
Efficiency: Current Spending				
% ΔPrice		0.306	0.432	
Confidence Interval		[0.078, 0.535]	[0.217, 0.647]	
Observations	130,778	130,778	130,778	130,778
Complete Set of Controls	✓	✓	✓	✓

Notes: Standard errors reported and are clustered at the district level. All models include the complete set of census, policy and data coverage controls described in Table 3. In all models we instrument for endogenous variables shown with the event-time shocks from school finance reforms.

Appendix C. Visual Supplement to Event-Study Analysis

The event-study diagrams in section 3.3 show the rich distributional effects of state finance reforms across initial school spending quartiles. As illustrated by Figure 2, places where salary-spending increased the most after reforms (spending quartile 1) also have the strongest reduced-form relationship between reforms and house prices. In this section we show the 90% confidence intervals around each event-study plot of our analysis, as is traditional in the literature. Plots are show separately for each of the first three quartiles relative to the fourth (highest) quartile of initial spending. Table C.1 links the confidence interval plots with corresponding figures in the main paper.

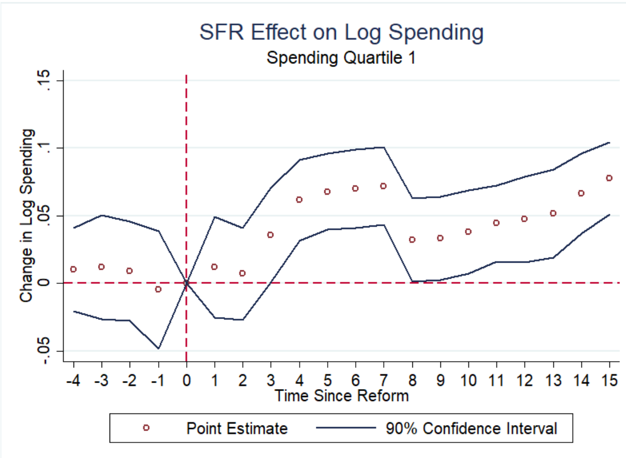
Table C.1: Summary of Appendix C Figures

Primary Figure	**Appendix**
Total Spending - Figure 1	C.1
House Prices - Figure 2a	C.2
Salary Spending - Figure 2b	C.3
Property Tax Rev. - Figure 2c	C.4
Nonsalary Spending - Figure 2d	C.5
Current Spending - Figure B.1a (Top Pane)	C.6
Capital Spending - Figure B.1b (Lower Pane)	C.7

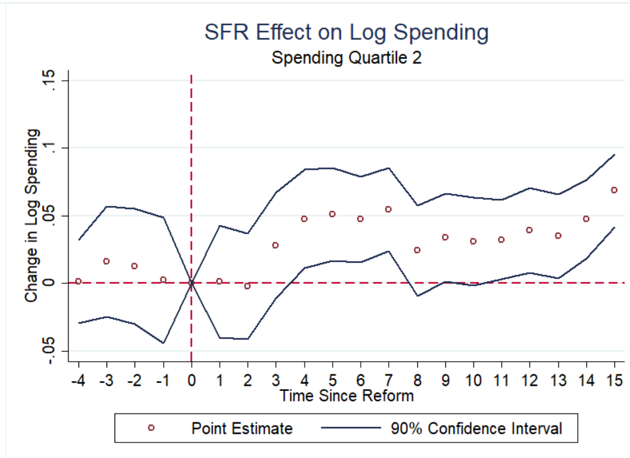
Notes: Brief descriptions of the event-study figures in appendix section C. Each primary figure plots the heterogeneous effects of reforms on each outcome by initial spending quartile groups, not including confidence intervals. The appendix figures replicate the primary figures, but plot each quartile group separately and includes the confidence intervals as traditional in the literature.

C1. Total-Spending

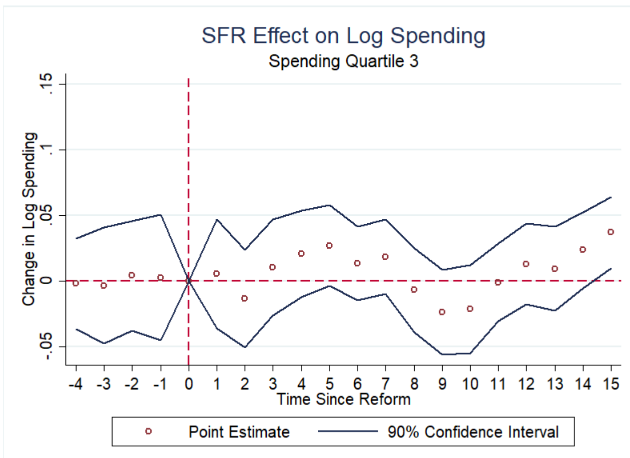
Figure C.1: First-stage event-study of school spending with 90% confidence interval



(a) Spending quartile 1



(b) Spending quartile 2

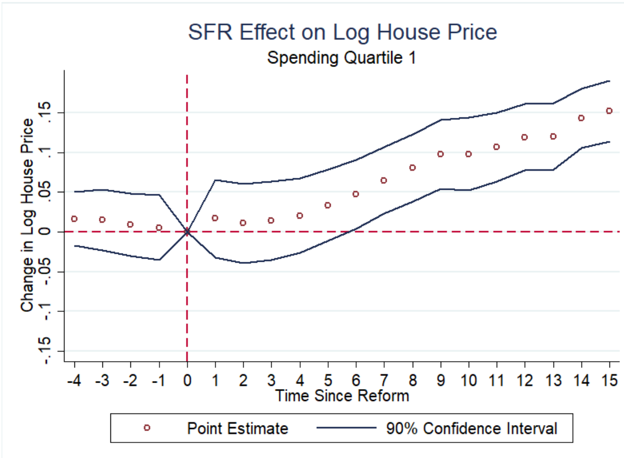


(c) Spending quartile 3

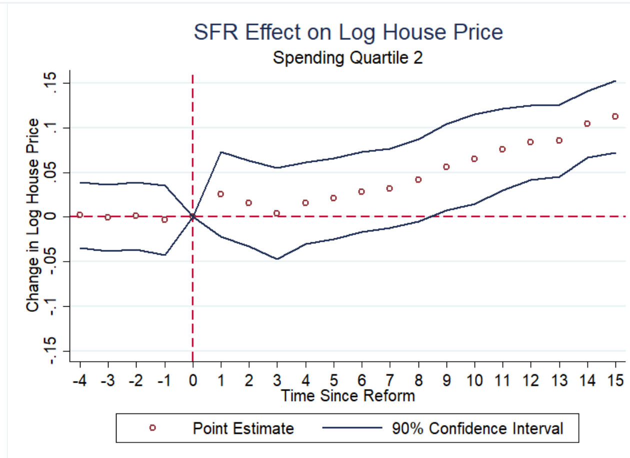
Robustness check for Figure 1. A plot of event-study coefficients mapping the effect of state finance reforms on total spending, with 90% confidence intervals. Additional controls include policy controls for the concurrent rollout of healthcare and social service programs, 1960 county characteristics interacted with linear time trends, along with district and year fixed effects.

C2. House Prices

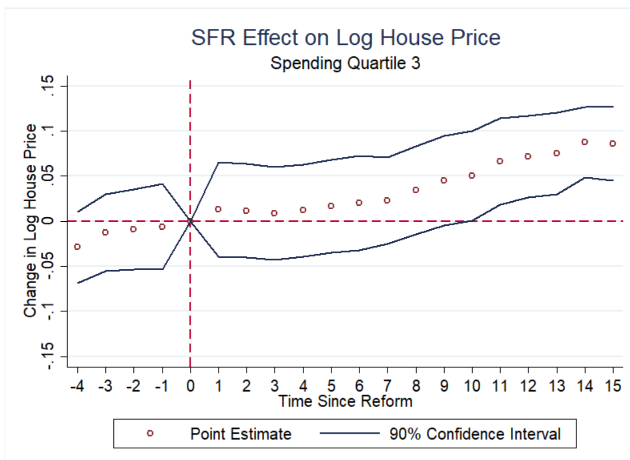
Figure C.2: Reduced-form event-study of house prices with 90% confidence interval



(a) Spending quartile 1



(b) Spending quartile 2

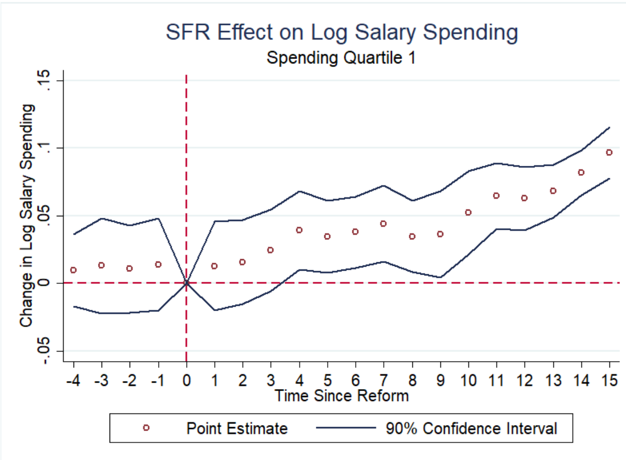


(c) Spending quartile 3

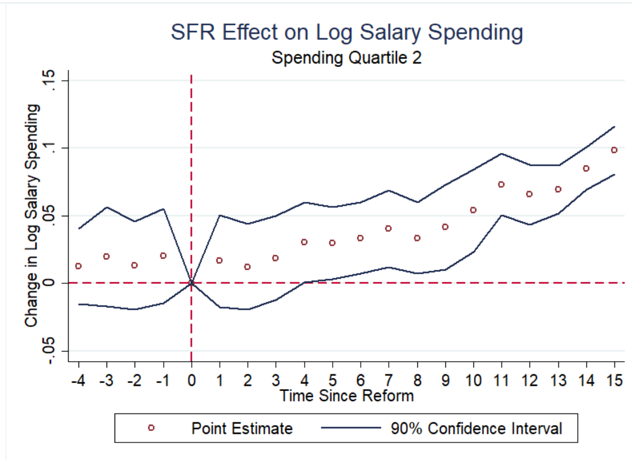
Robustness check for Figure 2a. A plot of event-study coefficients mapping the effect of state finance reforms on house prices, with 90% confidence intervals. Additional controls include policy controls for the concurrent rollout of healthcare and social service programs, 1960 county characteristics interacted with linear time trends, along with district and year fixed effects.

C3. Salary Spending

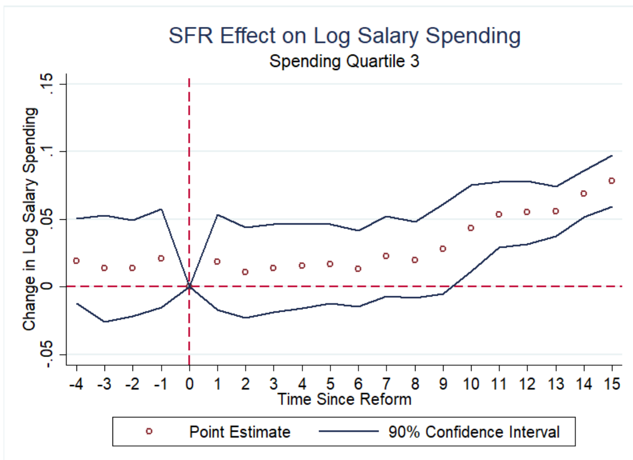
Figure C.3: First-stage event-study of salary spending with 90% confidence interval



(a) Spending quartile 1



(b) Spending quartile 2

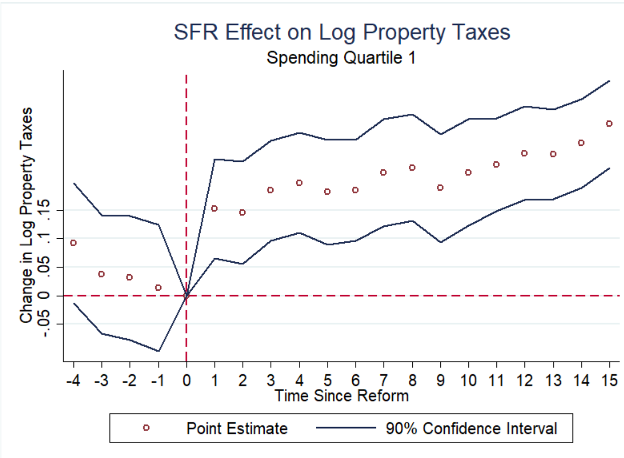


(c) Spending quartile 3

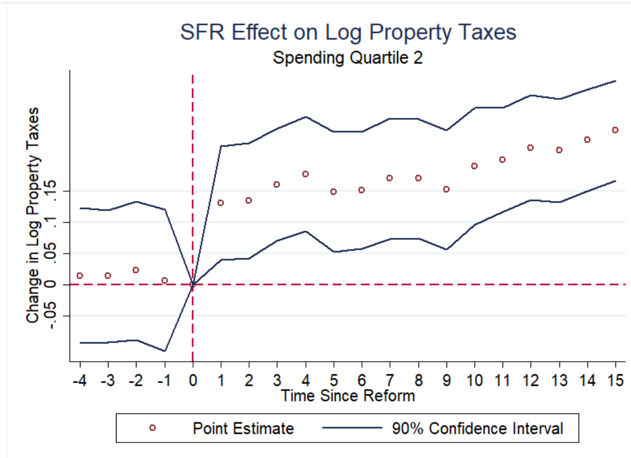
Robustness check for Figure 2b. A plot of event-study coefficients mapping the effect of state finance reforms on salary spending, with 90% confidence intervals. Additional controls include policy controls for the concurrent rollout of healthcare and social service programs, 1960 county characteristics interacted with linear time trends, along with district and year fixed effects.

C4. Property Taxes

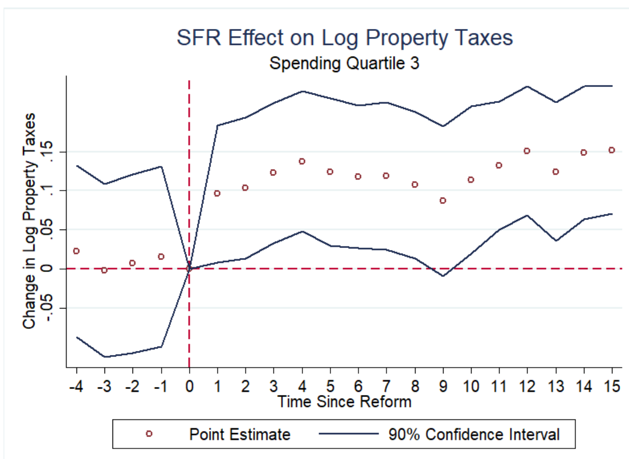
Figure C.4: First-stage event-study of property taxes with 90% confidence interval



(a) Spending quartile 1



(b) Spending quartile 2

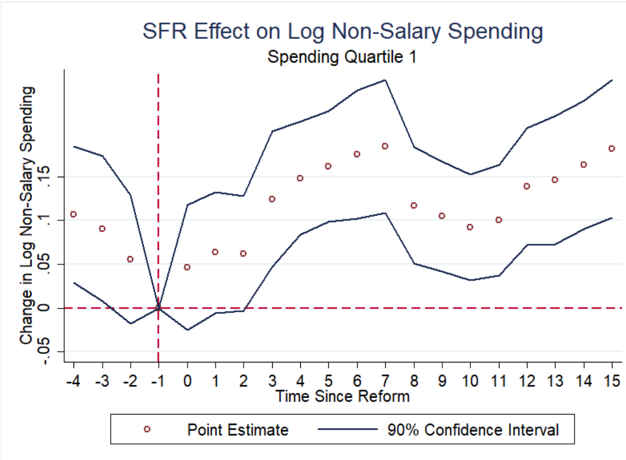


(c) Spending quartile 3

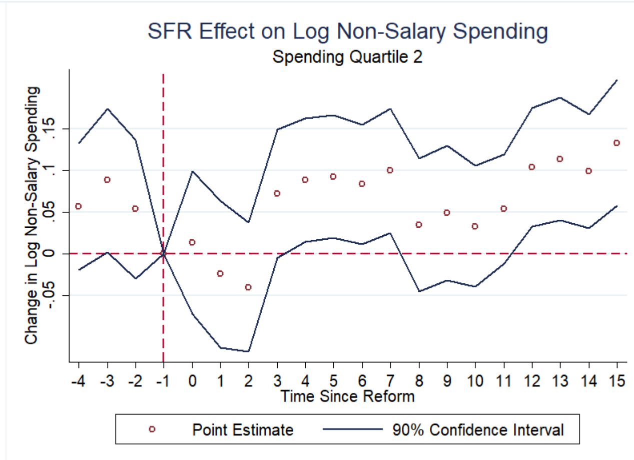
Robustness check for Figure 2c. A plot of event-study coefficients mapping the effect of state finance reforms on property taxes, with 90% confidence intervals. Additional controls include policy controls for the concurrent rollout of healthcare and social service programs, 1960 county characteristics interacted with linear time trends, along with district and year fixed effects.

C5. Non-Salary Spending

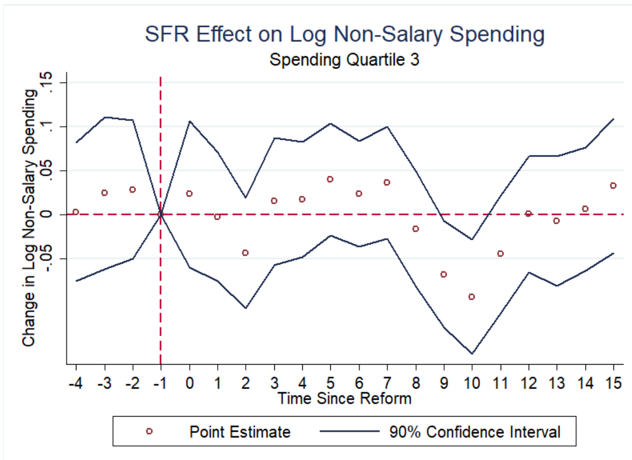
Figure C.5: First-stage event-study of non-salary spending with 90% confidence interval



(a) Spending quartile 1



(b) Spending quartile 2

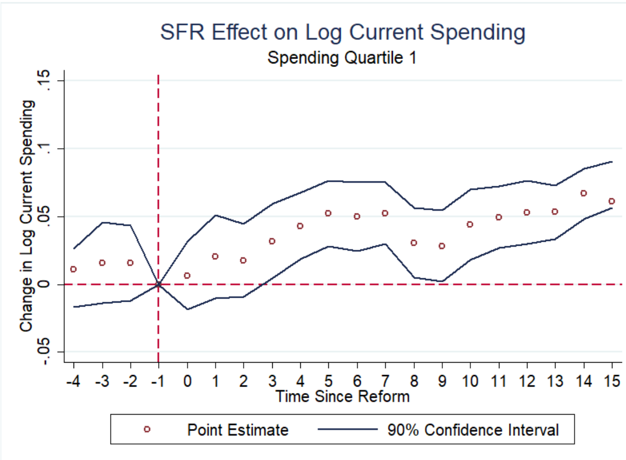


(c) Spending quartile 3

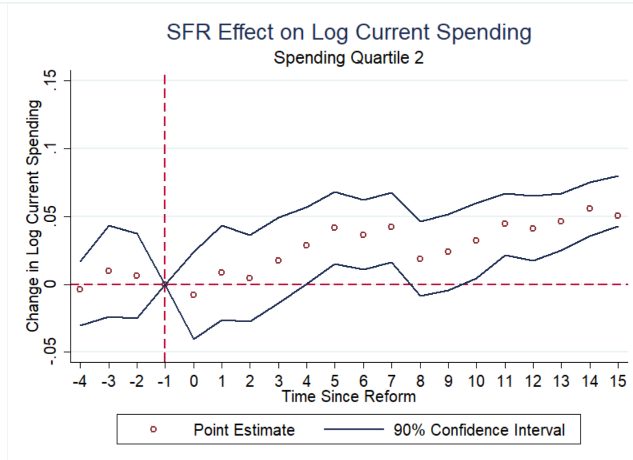
Robustness check for Figure 2d. A plot of event-study coefficients mapping the effect of state finance reforms on nonsalary spending, with 90% confidence intervals. Additional controls include policy controls for the concurrent rollout of healthcare and social service programs, 1960 county characteristics interacted with linear time trends, along with district and year fixed effects.

C6. Current Spending

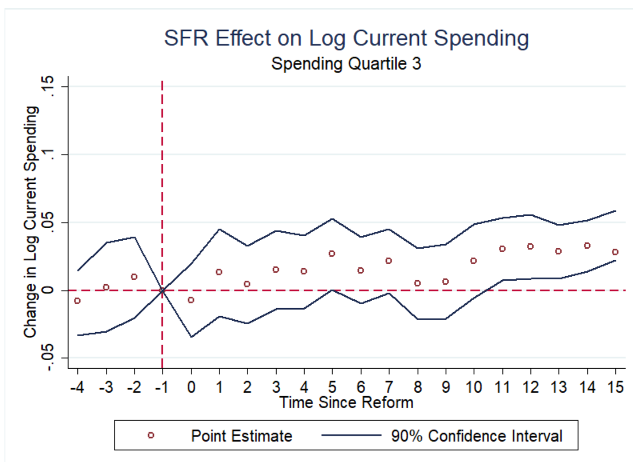
Figure C.6: First-stage event-study of current spending with 90% confidence interval



(a) Spending quartile 1



(b) Spending quartile 2

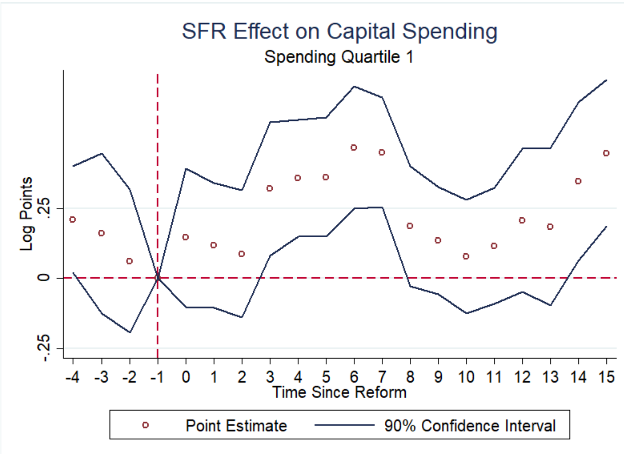


(c) Spending quartile 3

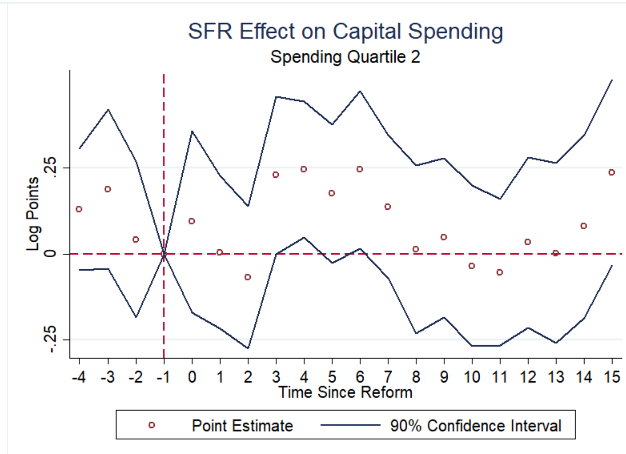
Robustness check for Figure B.1a. A plot of event-study coefficients mapping the effect of state finance reforms on current spending, with 90% confidence intervals. Additional controls include policy controls for the concurrent rollout of healthcare and social service programs, 1960 county characteristics interacted with linear time trends, along with district and year fixed effects.

C7. Capital Spending

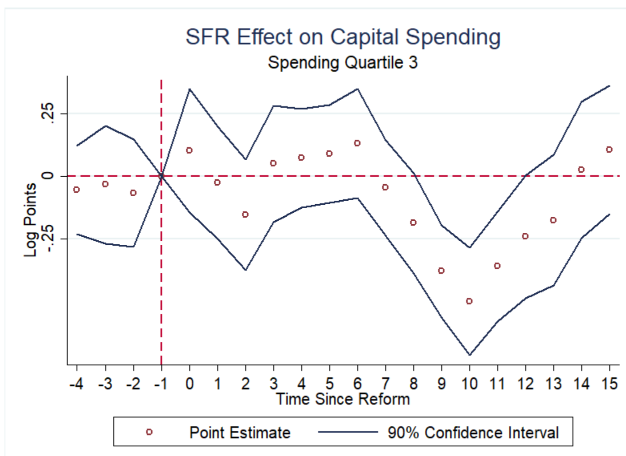
Figure C.7: First-stage event-study of capital spending with 90% confidence interval



(a) Spending quartile 1



(b) Spending quartile 2



(c) Spending quartile 3

Robustness check for Figure B.1b. A plot of event-study coefficients mapping the effect of state finance reforms on capital spending, with 90% confidence intervals. Additional controls include policy controls for the concurrent rollout of healthcare and social service programs, 1960 county characteristics interacted with linear time trends, along with district and year fixed effects.