Uneven Wage Growth and Public Goods

The Case of US Public Education

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Abstract

Wage growth is a key driver of local wealth accumulation, enabling greater household and community investment in public goods. Unfortunately, wage growth has diverged markedly from productivity growth in the United States in recent decades. As such, regions whose wages track productivity gains tend to benefit from broader economic growth, while lagging regions risk weaker savings capacity and declining support for public services. This work estimates the effect of local wage shocks on education spending using a shift-share instrumental variable design, exploiting variation in commuting zone industrial composition interacted with national industry growth shocks. We estimate that a 10% increase in local wages generates a 2.3% short-run increase in per-pupil education expenditure, with a long-run effect of approximately 4.6%. This result is only driven by a handful of states, whereas others exhibit little wage responsiveness. These results expedite the need to enable education equalization programmes to account for future potential wage disparities in key states.

1 Introduction

Since the 1970s, the United States has experienced a persistent divergence between productivity growth and wage growth [1, 2, 3, 4]. While labor productivity has continued to rise, the earnings of typical workers have increased far more slowly, leading to a substantial decoupling between the two trends. Explanations range from heterogeneous demand for human capital [5, 6, 7, 8, 9] to competition [2, 10, 11, 12, 13] to weakened labor market institutions and policy forces [14, 15, 16, 17, 18, 19, 20, 21].[1] Though its causes are hotly debated, the fact itself is well-documented, especially for lower- and middle-income workers [23].[2]

Regardless of its causes, the decline in the wage–productivity link has important consequences. Wages remain the most direct channel through which national productivity growth translates into household well-being. When wage growth lags productivity, improvements in aggregate efficiency do not translate into broad-based improvements in living standards [16]. Furthermore, in a context in which the benefits of economic growth have already accrued unevenly across communities in the United States, this pattern may reinforce existing spatial inequalities in economic activities, especially in regions where industrial composition and historical shocks have already left communities economically vulnerable [26, 27, 28, 29, 30, 31, 32].

[1] Summers & Stansbury 2018 argue that productivity growth still exerts a positive influence on wages overall, but that institutional and structural changes have weakened the link for large segments of the workforce [22, 2]. They point to declining union density, erosion of the minimum wage, globalization, and increased market concentration as key factors that have shifted bargaining power away from workers and reduced labor's share of national income [5]. Furthermore, additional evidence finds that this decoupling is far from a universal phenomenon. Rather, decoupling applies almost strictly to lower- and medium-wage earners, while already higher wages manage to keep up (relatively) with productivity growth rates.

[2] Though authors find that wage inequality growth has stagnated in the last decade, this is not a result of a "catch-up" effect of lower- or middle-income earners with top income earners, but rather wage growth at the bottom of the wage distribution [24]. Additionally, further evidence indicates that metric choice can influence the ambiguity of earnings and wage inequality conclusions [25].

A link that has been far less explored in this context is the spillover effect of local wages to local wealth-building and its effect on public goods. Wage growth is an important contributor to local wealth-building, allowing households and communities to invest more in local public goods [33]. Communities whose wages rise in line with productivity growth will likely reap the benefits of economic growth whereas those who do not, risk falling behind. This link is particularly important in the US given the structure of local public financing. Majority of local public services are funded via property taxes. This funding structure entrenches a mechanism for generating inequality of opportunity between diversely affluent regions of the country [34, 35, 36, 37].[3] Existing work shows that income inequality and uneven local income growth shape inequalities in local public goods provision, residential mobility, and long-run opportunity [41]. Wage stagnation can translate directly into weakened fiscal capacity. Put plainly, given the structure of US public services, wherein they are tied to asset values, inequality in wealth-building can have significant effects for the quality of local public services.

Community well-being and public expenditure in the US is already characterized by a high degree of spatial heterogeneity [34, 42, 43, 44]. Evidence of how income inequality perpetuates other forms of inequality (opportunity, health, infrastructure quality, and broader well-being) is steadily increasing [45, 46, 47, 48, 49, 50] find that greater income inequality leads to higher public expenditure across all public goods indicating that a presence of higher-earners in a local area contributes to higher levels of expenditure. Though this does not support an unambiguous denunciation of inequality in itself, it provides additional evidence for the fact that local incomes affect public expenditure raising the potential for "superstar" and "left behind" regions to emerge absent even income growth.

Local public education is particularly relevant in this context. Public schools are responsible for education over 80% of American school-age children. In 2019, governments around the US (including the federal government) spent a total of $870 billion on public education, roughly $17,013 per pupil [51, 52]. However, the quality of services delivered and fiscal resources spent vary widely across the country [53]. A sometimes lacking, or at best under-performing, federal equalization system perpetuates this structural force [54, 55, 56, 57, 58, 59]. Take Connecticut for example. In 2016, according to the Connecticut State Department of Education, the town of Greenwich town, one of the highest-income towns in the country, spent $22,000 per pupil while Bridgeport, although only located 40 kilometers away, spent $14,000 [47].

The quality of public education, especially at an early age, can have long-lasting consequences for personal and economic well-being over an individual's lifetime as well as generations following them [60, 61]. Although only a small piece of the puzzle that determines quality public education, expenditure levels ensure adequate funding for facilities, teachers, administrators, and other services [62, 63, 64]. Furthermore, evidence suggests that progressive spending delivers efficiency gains in school performance [65]. Therefore, ensuring that local or regional economic decline does not disrupt or worsen the quality of education delivered is of paramount importance to ensure greater equality in the long-run. Altogether, this evidence points to the value of identifying the extent to which expenditure on public education is reliant on local wage growth across the country.

Despite the large literature on the existence of wage-productivity decoupling, inequality in local public goods provision, and US school financing system, one central link remains under-explored: Are local public education budgets sensitive to local wages, and if so, for whom and under what conditions?

To be more precise, this study investigates the following questions:

RQ1: Do local wage gains affect public education expenditure in levels?

RQ2: If so, is this relationship constant across commuting zones? What sources of heterogeneity mediate this relationship?

RQ3: In light of recent reforms to intergovernmental education funding aimed at reducing disparities in educational expenditure, do intergovernmental transfers mitigate wealth-driven inequalities in public education spending?

[3] A burgeoning literature points to the role of racial segregation and its enduring legacy in perpetuating inequality in local economic health, wealth, and public service expenditure [38, 39, 40].

1.1 Theoretical Motivation and Empirical Approach

Although as mentioned above, existing work provides descriptive evidence that higher income communities invest more in local services, the causal effect of wage growth on education spending is unclear due to substantial endogeneity. Any study of the linkage between two or more local socio-economic outcomes presents considerable endogeneity challenges. Higher-wage households select into districts with better schools. Education quality affects local human capital and, thus, wages. Local fiscal decisions respond to political, demographic, and institutional environments that co-evolve with economic conditions. Moreover, regional heterogeneity is large. States differ in their reliance on local versus state funding; commuting zones differ in industrial composition and exposure to national shocks; and localities vary in their ability to translate tax bases into expenditures. On a cultural level, the quality of local education can potentially impact preferences for education in turn. Thus, a national "average' ' elasticity may be misleading. What is missing is a causally identified, regionally sensitive, and mechanism-informed estimate of the elasticity of public education expenditure to local wages.

To overcome these challenges, we construct a shift–share instrumental variable for local wages. More precisely, to interrogate the elasticity of public education expenditure to wages across US commuting zones, we construct a shift-share instrument that combines fixed local industry employment shares with national industry-level changes in real value added using data from the US Bureau of Labor Statistics and Bureau of Economic Analysis. This instrument generates plausibly exogenous local variation by exploiting how different regions are differentially exposed to common national trends, while abstracting from endogenous local dynamics. It is particularly well suited in this setting, since the local tax base, and thus education spending, likely depends on industries that are unevenly distributed across regions but subject to similar industry-specific wage shocks. We use this instrument to identify the effect of wage shocks on local public education expenditure as reported by the US Census Bureau's Annual Survey of State and Local Government Finances.

Given the substantial heterogeneity across U.S. states arising both from structural sources (such as differences in tax systems, regulatory environments, and legislative institutions) and from evolved characteristics (including industrial composition, income levels, inequality, and broader measures of economic diversity) the scope for identifying a policy-relevant single national average treatment effect is inherently limited. Therefore, we proceed in two steps. First, we provide an initial benchmark using a pooled estimation to establish a baseline relationship between wages and education expenditure that generalizes reasonably across the national economy. Second, we investigate the regional and industrial heterogeneity that these pooled estimates mask via a state-by-state estimation, industry-by-industry estimation, and grouping commuting zones by their historic wage and GDP growth trajectories to improve comparability of treatment and control groups in our instrumental variable design. In the latter heterogeneity analysis, we construct commuting zone growth rates as idiosyncratic variables absent state and national-level growth rates, understanding that *relative* growth rates are of particular importance in a landscape where local economies determine the actual *value* of wage levels. This allows us to identify where the wage–expenditure link is strongest or weakest.

1.2 Main Results and Contribution

1. First, we find that public education expenditure responds strongly and positively to local wage growth. A 10% increase in local wages is associated with a 2.2% increase in public elementary education expenditure, with a long-run increase of 4.6%.

2. Second, estimating the instrumental variable model separately for each state reveals substantial heterogeneity in the relationship between wages and education expenditure. Only a third of the 40 states analysed in this study show persistent causal relationships between wages and education expenditure, indicating that these carry the weight of national-level identification. Many of these states rely on an above-average share of education funding coming from local sources rather than intergovernmental transfers. Across these states the wage elasticity varies between 2-10% increases in response to a 10% increase in local wages, with Colorado, Florida, and South Dakota exhibiting highest wage elasticities.

3. Third, elasticities are largest, both in terms of size and statistical significance, in commuting zones with historically low wage growth compared to other regions, and smallest in regions with historically

strong wage trajectories. This indicates that regions already experiencing economic stagnation may face compounded disadvantages through weaker fiscal capacity for education spending.

As such, these results make three important contributions to the existing literature. First, the paper provides causal estimates of the elasticity of local public education expenditure to local wage growth across US commuting zones, documenting both the average national relationship and substantial regional heterogeneity.

Second, the results show that local fiscal capacity is closely tied to historically uneven wage growth, implying that wage-productivity decoupling has important implications not only for households but also for public sector institutions that shape long-run opportunity. Lastly, the paper demonstrates how the structure of the US school financing system amplifies or dampens the effects of local economic change, with weaker equalizing opportunities exacerbating the consequences of wage stagnation. Together, these findings show that the wage-productivity nexus has implications that go far beyond household earnings: it shapes the fiscal foundations of local public education and, through that channel, the intergenerational transmission of economic opportunity.

In the sections that follow, we outline the various data sources used in Section 2; provide a detailed overview of our shift-share construction and methodological approach Section 3 with accompanying results; and conclude with a discussion of policy implications in Section 4.

2 Data

We compile a panel dataset of the following indicators across 636 commuting zones (CZ) in 40 US states annually between 2001-2021.

Expenditure and Revenue: This work relies on a harmonized repository from Willamette University of the data collected annually as part of the US Census Bureau's Annual Survey of State & Local Government Finances (SLGF). The SLGF is the 'only comprehensive source of information on the finances of local governments in the United States' [66]. The data includes county-level revenue, property taxes, and expenditure on public education including disaggregated values by revenue source (federal, state, or other intergovernmental revenue) and expenditure item (lunches, wages, debt). All values are reported in real US dollars. We aggregate school district measures up to the commuting zone-level to ensure the availability of adequate control and treatment variables. We choose to conduct the analysis on the commuting zone level because it is a more accurate picture of a local labor market area [67].[4] Our main outcome variable is per pupil spending on elementary education.

Population controls: We source commuting-zone level population statistics by aggregating data from county-level populations statistics from the US Census Bureau

Local GDP: We gather local GDP control variables by aggregating county-level GDP data from the Bureau of Economic Analysis (BEA). This BEA data is only available after 2001, defining the lower limit of our panel's time dimension. We primarily rely on private industry GDP as a control variable given a large remaining portion of GDP is government expenditure which includes public education expenditure.

Property Prices: The US Federal Housing Finance Agency maintains an annual county-level Housing Price Index (HPI) metric, a geographically linked measure of the movement of single-family house prices. The HPI is a weighted, repeat-sales index, measuring average price changes in repeat sales or refinancings on the same properties. This information is obtained by reviewing repeat mortgage transactions on single-family properties whose mortgages have been purchased or securitized by Fannie Mae or Freddie Mac (two US government-sponsored enterprises that guarantee most US mortgages) [68]. We aggregate to the commuting zone level via a population-weighted mean.

[4]The database is provided for six different levels of government: state, county, municipal, township, special district, and school district. Reporting is only mandated in Census years (every five years), and even then missing data remains a challenge. This means that data provided at any other level of government suffers from significant levels of missing data, with a high level of selection bias correlated with administrative capacity. However, strengthened by a partnership with the National Center for Education Statistics, observations for US school districts exhibit near-complete coverage between 1997-2021 (Pierson, Hand, and Thompson [66]).

Race Controls: The National Institute of Health National Cancer Institute's Surveillance, Epidemiology, and End Results (SEER) Program provides annual estimates of total White, Black, American Indian/Alaska Native, Asian/Pacific Islander populations at the county level, optionally by Hispanic or non-Hispanic origin [69]. Though the US Census Bureau provides county-level estimates of racial make-up of local areas in the American Community Survey, this data is unavailable prior to 2009 and is considered less accurate than those provided by the National Cancer Institute's SEER Database.

Adequate Education Spending: The University of Wisconsin's School Finance Indicators Database provides provides detailed data on School Funding Adequacy, measured as the dollar estimate needed per pupil to achieve U.S. average test scores [70].

This data aggregation results in a complete and balanced panel of 636 US commuting zones across 40 states between 2001-2021. [5] All data used is reported annually at the commuting zone level. [6] Therefore, apart from an indicator of a commuting zone's state, all variables are time-variant.

Table 1 reports summary statistics across relevant variables. All (dollar) values are reported in (real 2017-chained) thousands except for the House Price Index. Table 2 represents the order of integration of relevant variables calculated using a unit root test designed for heterogeneous panels [73]. All variables are integrated I(0) indicating minimal concern for non-stationarity except in the case of enrollment and population numbers.

Table 1

Statistic	N	Mean	St. Dev.	Min	Max
Enrollment	13,356	62.39	169.90	0.13	3,169.73
Population	13,356	405.18	1,077.99	0.88	18,732.54
Elementary Expenditure per pupil	13,356	11.39	2.99	5.97	58.35
Property Tax per pupil	13,356	3.60	2.43	0.29	32.91
Intergovernmental (IG) Revenue per pupil	13,356	7.12	2.28	1.04	27.50
State IG Revenue per pupil	13,356	6.73	2.03	0.79	26.23
GDP per capita	13,356	44.52	25.27	15.32	388.73
GDP pc - Private Industry	13,356	38.42	25.18	5.85	383.06
House Price Index	12,717	255.18	155.71	85.53	1,947.97

Figure 1 demonstrates the spread of elementary education expenditure per pupil by commuting zone, grouped by state. Each state's population-weighted mean expenditure per pupil is represented in black. There is considerable within-state variation in per-pupil expenditure levels. Notably, Texas, Montana, and Idaho have commuting zones that spend nearly twice as much as other zones in the same state. Furthermore, the mean level of expenditure is nearly four times as high in the highest-spending state (New York) than in the lowest-spending state, Idaho. We additionally display these values in relation to the estimated "adequate" level of expenditure required for students to achieve U.S. average test scores, represented by the green and red arrows. Majority of states fall short of the deemed adequate expenditure value. Mississippi boasts the greatest shortfall in spending while Wyoming boasts the greatest overshoot in spending. Figure 1 demonstrates the considerable within state variability in spending rates as well as between state variation in levels and student needs.

[5] 20% of US states are missing from the dataset because (1) we impose an exclusion restriction wherein any commuting zone reporting more than five $0 values for property taxes collected is excluded due to likely measurement error and (2) Connecticut, Maryland, North Carolina, and Virginia have been excluded due to unconventional or incomplete public school district reporting.

[6] In line with similar work on US economic geography, commuting zones were chosen as the unit of analysis as they are a far less arbitrary and more accurate representation of local labor market areas/economies [71, 72].

Table 2: Order of Integration

Variable	Order of Integration	I(0) test p-value	I(1) test p-value
Enrollment	I(1)	0.608	<0.0001
Population	I(1)	0.286	<0.0001
Elementary Expenditure per pupil	I(0)	<0.0001	
Property Tax per pupil	I(0)	<0.0001	
Intergovernmental (IG) Revenue per pupil	I(0)	<0.0001	
State IG Revenue per pupil	I(0)	<0.0001	
GDP per capita	I(0)	<0.0001	
GDP pc - Private Industry	I(0)	<0.0001	

Note:
Order of integration determined using the Im-Pesaran-Shin (IPS) panel unit root test with intercept. Lag length selected via AIC with maximum of 4 lags. The null hypothesis is non-stationarity; rejection at the 5% level indicates stationarity. I(0) denotes stationarity in levels and I(1) denotes stationarity in first differences. All variables are log-transformed prior to testing to account for heteroskedasticity.

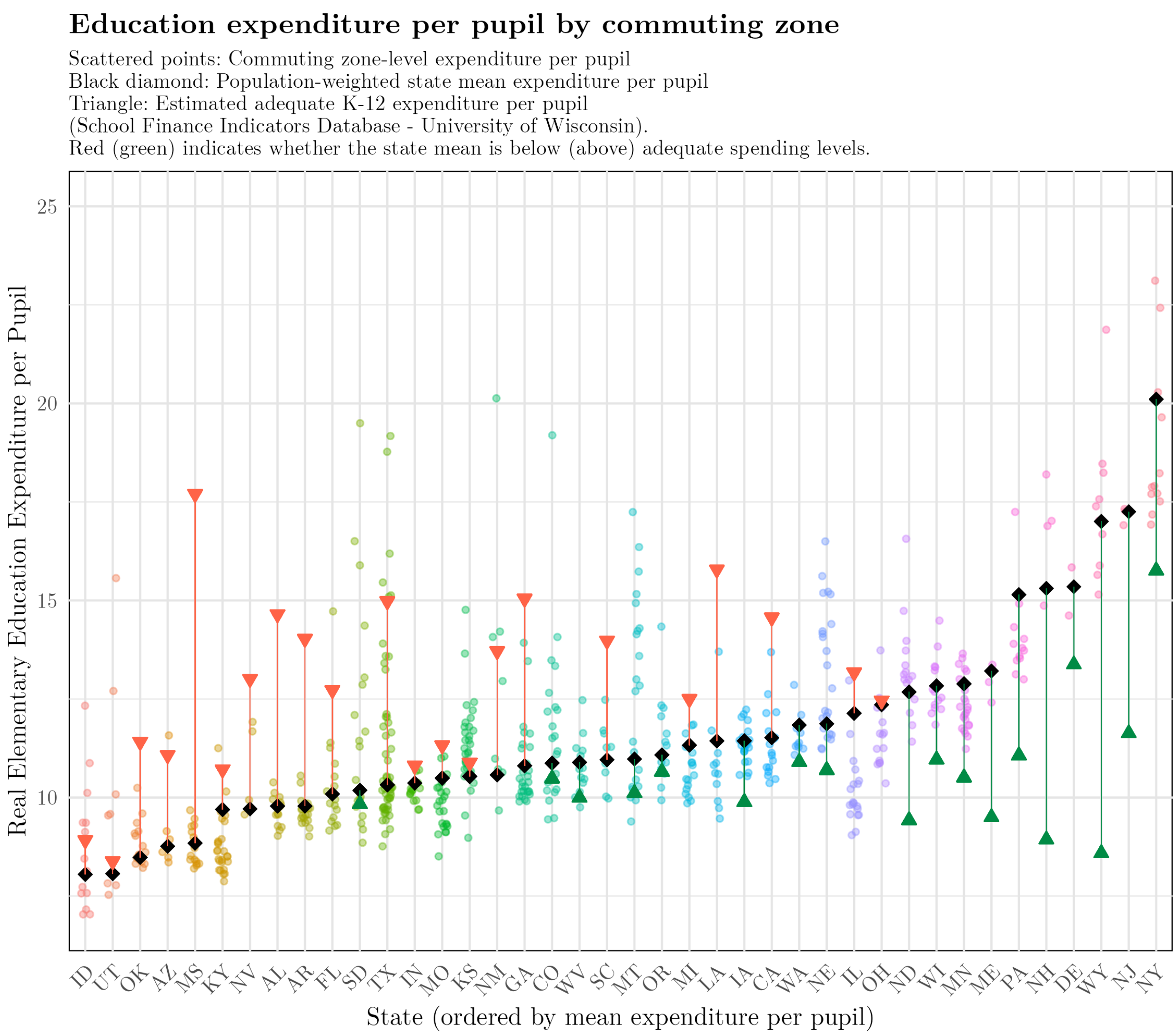


Figure 1: Per Pupil Education Expenditure

Figure 2 demonstrates the trajectories of local wages from an initial level in 2001, demonstrating, the diverging wage growth trajectories of commuting zones in our sample. Blue commuting zones represents those exhibiting relatively high wage growth trajectories as deemed by the calculation in Section 3.2.1. The black dashed line represents the annual national wage growth rate.

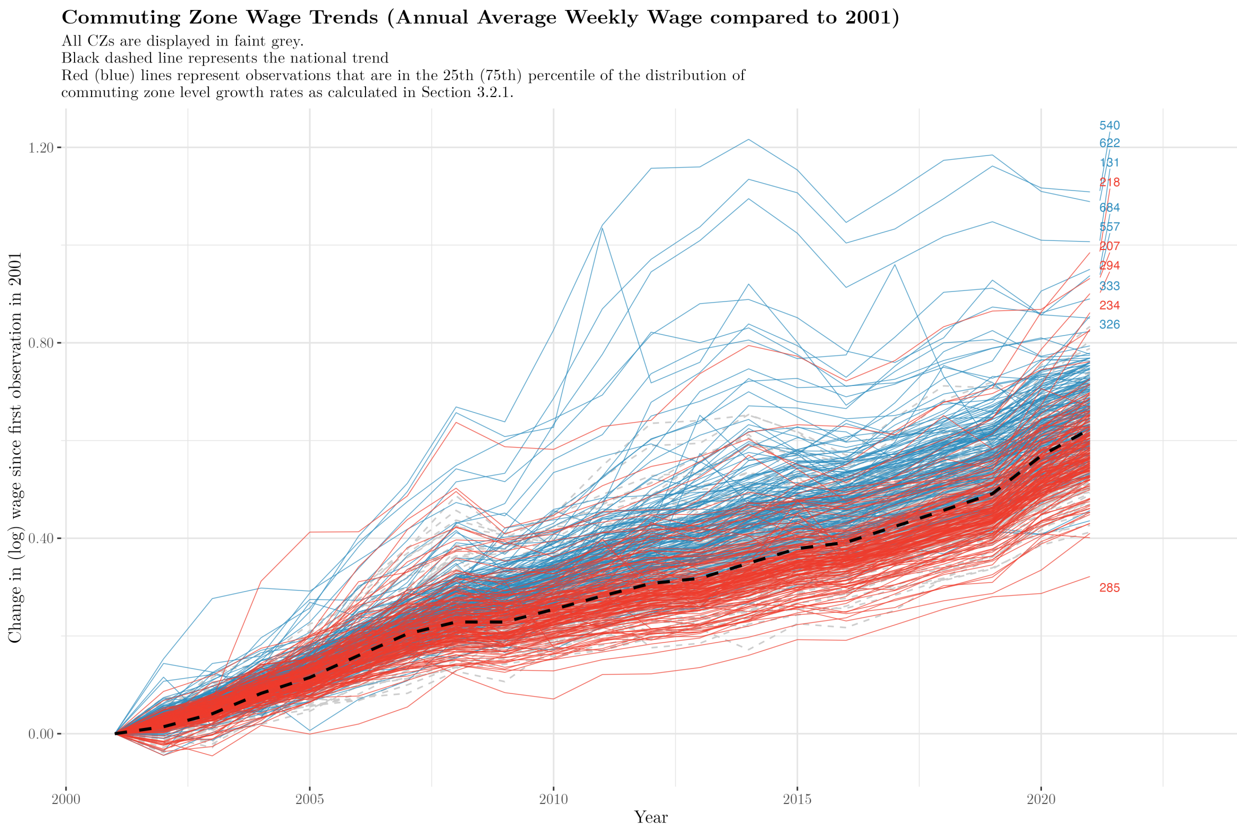


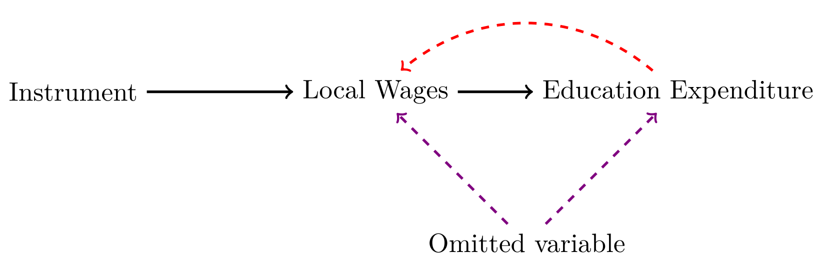
Figure 2: Wages and Productivity

3 Methods

3.1 Identification Strategy

We are centrally interested in the effect of changes in local wages on public education expenditure where there is an evident endogeneity concern between public education expenditure and wage. First, there is a likely attracting factor of high levels of education expenditure for higher-income families. Second, absent migration, education systems provision local labor markets with individuals with diverse human capital.

Figure 3: Instrumental Variable Path Diagram



3.1.1 Shift-share Instrument

Therefore, we adopt a causal identification strategy via a shift-share instrument. Shift-share or *Bartik* instruments have gained popularity in empirical work as a method of handling endogeneity issues in panel data [74, 75, 76].[7] Such instruments combine time-variant, unit-invariant changes in aggregate economic variables (i.e., national changes in industry value added levels) with time-invariant, unit-variant shares in exposure to these macro-level changes (i.e., local shares of employment in particular industries). This decomposition of local-level changes delocalizes variation over space and time. In doing so, it provides a defensible strategy for 'de-endogenizing' the treatment, as local exposure is predetermined with respect to contemporaneous shocks. Moreover, by construction, the approach enables the examination of how macro-level phenomena propagate to and affect local units, as it generates local shocks that are driven by national industry trends weighted but the community exposure to this industries.[8]

In the context of this work, we construct the instrument by interacting commuting-zone level industrial employment shares held constant at a base period with national real value added growth by industry. The literature on Bartik instruments allows for an argument of plausible exogeneity via various channels. First, authors argue that local industry shares are exogenous by imposing that shares be fixed to a particular base year and are therefore unable to adapt to changes in national-level growth rates. Such a shift-share instrument is designed as in Equation 1.

$$Z_{it} = \sum_{j=1}^{k} S_{ij\tau} G_{njt} \tag{1}$$

where $S_{ij\tau}$ is the local share of unit i's economy (measured using metrics like employment, wages, revenue) in industry j at a fixed base year τ and G_{njt} is the growth rate of industry j at a national level n at time t.

Alternatively, authors may argue that the claim of exogeneity in the national-level growth rates is unlikely to be violated even when allowing the local shares to vary over time. This approach is likely to come at significant expense to instrument exogeneity. It is constructed as follows:

$$Z_{it} = \sum_{j=1}^{k} S_{ijt} G_{njt}$$

Finally, authors might be concerned about the implausible exogeneity of both shares and national-level growth rates in which case they construct the instrument as in Equation 2 where the local shares are fixed at a common base year and industry-specific growth rates G are derived from data on other similar regions o rather than national-level changes that are inherently comprised of local-level shifts. This approach likely comes at significant expense to instrument relevance.

$$Z_{it} = \sum_{j=1}^{k} S_{0jt} G_{ojt} \tag{2}$$

[7] Autor et al. use a shift-share instrument to assess the effect of Chinese import competition on manufacturing employment in US commuting zones [77]. As an extension, [43] use a similar shift-share instrument to assess the effect of the same shock on the size of local government. [78] employ a shift-share instrument for manufacturing layoffs to tease out the effect of a decline in manufacturing on both economically motivated and racial identity voting patterns in the US.

[8] An additional popular indicator for modelling industrial shocks exogenously is *oil price* as these values are often assumed to be exogenous to local and even national conditions [79]. Furthermore, various indicators for measuring *deindustrialization* have been proposed including the manufacturing share of employment, value added, and GDP [80, 81]. Finally, in rare instances, exogeneity can be secured due to *geographical, climatological, or geological factors*. For example, [82] obtain an exogenous measure of local revenue by "instrumenting the variation in hydropower revenue, and thus total revenue, by topology, average precipitation and meters of river in steep terrain." Certain authors have argued that the fact that the location of hydrocarbon deposits is dictated by geomorphological processes provides a plausible argument for exogeneity [83, 84].

Finally, the authors can make an additional design choice about whether the effect of these instruments should be assumed common to an aggregate local-level wage growth indicator or allowed to vary by industry. In other words, whether to construct the first-stage relationship of the 2SLS as...:

$$X_{it} = \alpha_i + \beta \sum_{j=1}^{k} S_{ijt} G_{njt} + \epsilon_{it}$$

...or...:

$$X_{it} = \alpha_i + \sum_{j=1}^{k} \beta_j S_j G_{jt} + \epsilon_{it}$$

We employ the formulation in Equation 1, assuming that base-period local industry shares and time-varying national rates are exogenous to local outcomes and construct the former of the first-stage relationship assuming a common β to the sum of these shares.

Using data from the Bureau of Economic Analysis, we construct a shift-share Bartik instrument at the commuting zone level using local employment shares by industry and national changes in industry-specific real value added represented in Equation 3. G_{njt} represents national-level changes in value added in industry j in time t and $\frac{N_{ij\tau}}{N_{i\tau}}$ represents the 'sensitivity' of a CZ to these national shocks proxied by an initial share of local employment in industry j in a baseline time period τ. The product of these two values defines the shift-share indicator $\tilde{Z}_{i,t,s}$. In order to construct the share portion, we compute the total local share of employment in a particular industry j. Due to challenges with missing data, we compute an average share across 2001-2005 as our 'base year'.

We compute the relevant shift-share instrument across 19 two-digit NAICS industrial categories listed in Table 3. Given industry-level disaggregation of local employment data requires data suppression for anonymity reasons, Figure 2 displays the data coverage of our commuting zone level shift-share instruments. Given the high degree of missingness in the 3-digit categorization we proceed with the 2-digit NAICS codes.

In the Appendices, we provide an additional estimation using a wage-based shift-share instrument constructed using data from the US Bureau of Labor Statistics' Quarterly Census of Employment and Wages (QCEW). This shift-share instrument is constructed as described above using industry-level changes in real wages. Concerns about endogeneity between the instrument and outcome variable are greater using this shift-share instrument and is therefore excluded from the main text. [9]

$$\tilde{Z}_{it} = \sum_{j=1}^{k} G_{njt} * \frac{N_{ij\tau}}{N_{i\tau}} \tag{3}$$

3.1.2 Empirical Estimation

This yields a 2SLS AR(1) model defined by the first- and second-stage regressions represented in Equation 4 and Equation 5. Due to the likely presence of time-dynamic effects, we include contemporaneous, 1-year, 2-year time lags as instruments.

$$\textbf{(First stage)} \qquad X_{it} = \rho X_{i,t-1} + \phi Y_{i,t-1} + \sum_{\ell=0}^{2} \pi_\ell \tilde{Z}_{i,t-\ell} + \theta \mathbf{W}'_{it} + \alpha_i + \lambda_t + u_{it}, \tag{4}$$

$$\textbf{(Second stage)} \qquad Y_{it} = \phi^* Y_{i,t-1} + \beta \widehat{X}_{it} + \delta \mathbf{W}'_{it} + \alpha_i + \lambda_t + \varepsilon_{it} \tag{5}$$

[9]We explore the sensitivity of results to the choice of base period τ by constructing the instrument for various base periods as well as a rolling window.

where W_{it} is a vector of control variables. We control for enrollment levels to account for scaling factors in education expenditure, intergovernmental transfers to account for the significant role of such transfers in funding education expenditure, percentage of the population that is Black, percentage of the population that is Hispanic, and private industry GDP per capita levels to account for local price levels.

Y_{it} is the natural logarithm of elementary (serving ages 6-12) education expenditure per pupil for CZ i in year t. We focus on elementary education for two reasons. First, this restriction partly shields against a justifiable concern about the endogeneity between wages and quality of local public education. Whereas funding for high school could likely affect local wages given such students are of working age, funding for elementary education is unlikely to impact wage rates via a human capital or skills channel. Second, in terms of public impact, elementary education is of foundational importance in the lives of children. Slips in public education provision at a young age could have scarring effects.

α_i represents a CZ fixed effect and λ_t represents year-fixed effects, with stage-relevant superscripts. ε_{it} and u_{it} represents the error term of the second and first stage, respectively.

We additionally adopt a dynamic specification by including lagged dependent variables in both stages of the IV estimation to avoid spurious correlation identification arising from persistence in expenditure and wage levels, as well as better accounting for heterogeneity across units. Education spending likely exhibits inertia due to slowly evolving budgetary and relevant policy cycles (i.e., property tax rate setting). Similarly, local wage levels are well-predicted by a previous year's wage levels. Failing to account for these dynamics, our estimates would conflate the causal effect of wage innovations with mechanical persistence in levels. This yields a more conservative and interpretable elasticity, though we demonstrate that the exclusion of the AR(1) term in the second-stage yields a contemporaneous effect estimate nearly identical to the long-run wage effect as derived from the dynamic specification in Table 4.

3.1.3 Interpretation

The elasticity of public education expenditure to local wages has an ambiguous interpretation. A positive elasticity would suggest that higher wages increase household savings rates and willingness to invest in local public goods, consistent with standard wealth effects. However, this relationship raises concerns about possible divergence wherein wage growth in high-earning regions could amplify educational investment, potentially widening spatial inequality in public education quality aligning with patterns of income inequality.

Conversely, a negative elasticity could emerge through several channels. Any response in needs-based intergovernmental revenue mechanisms may partially offset local fiscal capacity, creating an inverse relationship between wages and education spending. Alternatively, in more affluent communities, rising wages may enable households to substitute towards private education, crowding out or reducing demand for public expenditure. Furthermore, such a relationship could provide additional empirical support for a "resource curse" dynamic[10] wherein local communities reprioritize fiscal windfalls toward government expenditure other than

[10] Perhaps the most prominent and often-cited relationship between education and extractive industries is through the lens of the 'resource curse.' The validity and empirical existence of a 'resource curse' has been tested since its conception with disparate results [85, 86, 87, 82, 88, 89, 90, 91, 92, 93, 94, 95, 96, 97, 98, 99]. The literature is divided into two strands focusing on either political (the relationship between resource wealth and governance) [97, 97, 99, 91] or economic (the relationship between resource wealth and economic growth or human capital) resource curses. Empirical investigation of the economic resource curse has explored the effect of resource dependence on economic growth, public health and education expenditure and outcomes, mainly at a national level [82, 90, 93, 98]. In the case of education, the distinct outcome measured is level of educational attainment, in other words, whether the presence of a booming resource extraction economy provides disincentives to education for young people. It is worth noting that this literature has been repeatedly questioned on theoretical and conceptual grounds as institutional context often dictates whether a resource curse exists and empirical analyses seem to be very sensitive to methodological choices [96, 97, 92]. [82] find support for the paradox of plenty hypothesis in Norway - that higher local public revenue negatively affects the efficiency of local public good provision. [88] critically evaluate 'the empirical basis for the so-called resource curse and find that, despite the topic's popularity in economics and political science research, this apparent paradox may be a red herring. The most commonly used measure of "resource abundance" can be more usefully interpreted as a proxy for "resource dependence"-endogenous to underlying structural factors. In multiple estimations that combine resource abundance and dependence, institutional, and constitutional variables, we find that (i) resource abundance, constitutions, and institutions determine resource dependence, (ii) resource dependence does not affect growth, and (iii) resource abundance positively affects growth and institutional quality.' [89] use a panel on 140 countries from 1995-2009 and find an inverse relationship between resource dependence and and public health spending over time. [90] investigate a panel of 140 countries from 1995-2009 to find an adverse effect of resource dependence on public education expenditures relative to GDP. [92] find disparate results for

public education.

In either case, the consequence of a non-zero elasticity, whether positive or negative, has potential adverse consequences for spatial inequality of public education delivery by either boosting public education in affluent areas or dampening investment in less affluent areas.

Finally, a near-zero elasticity either has a modelling or policy-relevant implication. On the modelling side, a near-zero elasticity could indicate that the wage-public goods relationship operates on a longer time scale than that examined in this work. This would indicate the need for an alternative identification strategy. Alternatively, a near-zero elasticity could indicate that local public education systems are effectively insulated from local wage changes partly because intergovernmental transfers successfully equalize funding across regions.

3.1.4 Results

NAICS.Code	Industry
11	Agriculture, Forestry, Fishing, and Hunting
21	Mining
23	Construction
31-33	Manufacturing
42	Wholesale Trade
44-45	Retail Trade
48-49	Transportation and Warehousing
22	Utilities
51	Information
52	Finance and Insurance
53	Real Estate and Rental and Leasing
54	Professional, Scientific, and Technical Services
55	Management of Companies and Enterprises
56	Administrative and waste management services
61	Educational Services
62	Health Care and Social Assistance
71	Arts, Entertainment, and Recreation
72	Accommodation and Food Services
81	Other Services, except government
92	Public Administration

Table 3: Industry Categories

health and education controlling for institutional quality. [93] "measure the effect of resource-sector dependence on long-run income growth using the natural experiment of coal mining in 409 Appalachian counties selected for homogeneity. Using a panel data set (1970–2010), we find a one standard deviation increase in resource dependence is associated with 0.5–1 percentage point long-run and a 0.2 percentage point short-run decline in the annual growth rate of per capita personal income. We also measure the extent to which the resource curse operates through disincentives to education, and find significant effects, but this "education channel" explains less than 15 percent of the apparent curse.'

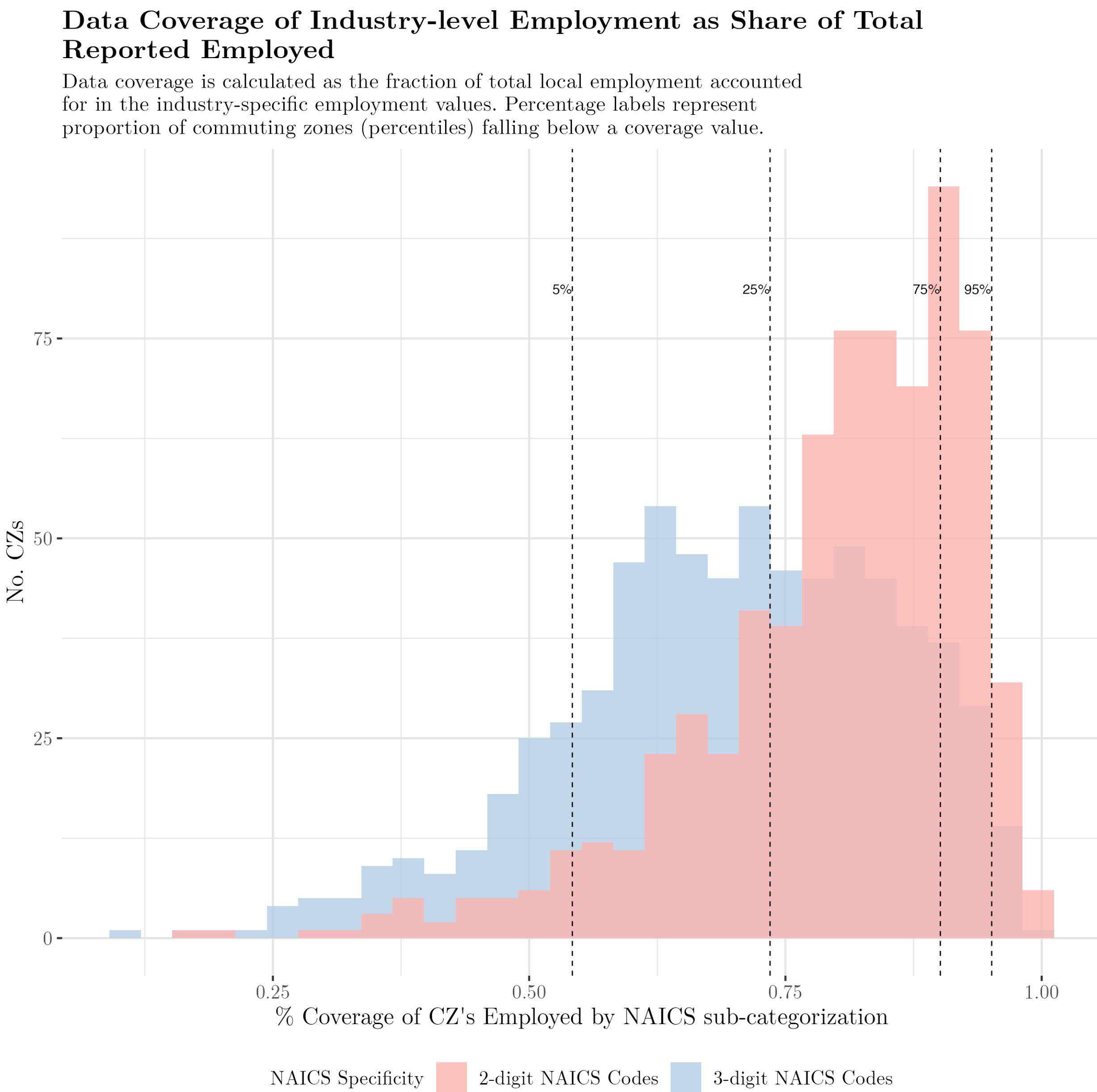


Figure 4: Data Coverage of Industry-level Employment as Share of Total Reported Employed

In Table 4, we demonstrate a strong and highly significant first-stage relationship wherein our shift-share instrument indicates a strong positive contemporaneous relationship with local wages. Our main specification in Columns 1-2 indicates that a 10% increase in local wages leads to a 2.23% increase in per-pupil education expenditure in the short run. The first-stage F-statistic substantially exceeds conventional weak instrument thresholds. The Wu-Hausman test definitively rejects the exogeneity of wages, validating our instrumental variable approach. However, the Wald over-identification test suggests potential instrument invalidity, though this is likely an artifact of the inclusion of AR(1) terms in the first-stage and not an indictment of the exclusion restriction itself. The implied long-run elasticity is near 0.46, indicating that the cumulative effect of a 10% wage increase is a 4.6% increase in education spending.

In Columns 3-4, we corroborate this long-run effect by removing the AR(1) term in the second-stage regression. The statistically significant causal effect of the treatment approaches this long-run effect of 4.6% (5.6%), capturing the total association between wages and expenditures, including both the immediate and long-run effects. This near-equivalence in the estimated effect reflects the fact that the underlying data-generating process is defined by dynamics, validating our use of a fully dynamic system.

To contextualize these estimates, we can translate this fiscal response into resource implications. Given a national average per-pupil expenditure of $18,000 [51] in 2021, a 10% wage increase generating a 2-5% spending increase translates to $350-$850 per student annually. For a district serving 1,000 students, this $350,000-$850,000 increase could support hiring 5-13 additional teachers (assuming an average teacher salary of $65,000 [100]).

Understanding that wage shocks are likely transmitted to education expenditure through property taxes, we test this potential mechanism in columns 5-6 (adjusting the set of control variables to better suit the first-stage relationship in theory). We find a highly significant house price elasticity, wherein a 10% increase in wages generate an 8.4% increase in local house prices. Note that the sample size decreases because of missing data in the housing price index for several of our commuting zones.

Our findings indicate that, in a decentralized education system, local labor market strength affects public education expenditure. Regions experiencing wage growth see spillovers into public education expenditure, whereas communities facing wage stagnation or decline might see their educational spending erode as a result.

Table 4: IV Estimation Using VA-based Shift-share instrument (l0, l1, l2) in Levels with CZ and year fixed effects and lags.

Dependent Variables:	(log) Annual Avg. Wkly. Wage	(log) Elem.Ed.Exp.pp	(log) Annual Avg. Wkly. Wage	(log) Elem.Ed.Exp.pp	(log) Annual Avg. Wkly. Wage	(log) House Price Index
IV stages	First	Second	First	Second	First	Second
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
(log, l1) Annual Avg. Wkly. Wage	0.7828***		0.7848***		0.7787***	
	(0.0120)		(0.0113)		(0.0120)	
VA SS (Lvl)	-0.0179		-0.0203		0.0257	
	(0.0484)		(0.0485)		(0.0454)	
VA SS (Lvl, l1)	-0.1808***		-0.1831***		-0.1759***	
	(0.0592)		(0.0600)		(0.0509)	
VA SS (Lvl, l2)	0.1614***		0.1668***		0.1318***	
	(0.0497)		(0.0516)		(0.0444)	
(l1, log) Elem.Ed.Exp.pp	0.0039	0.5086***				
	(0.0041)	(0.0156)				
(log) IG Revenue pp	0.0135***	0.2230***	0.0143***	0.3213***		
	(0.0030)	(0.0211)	(0.0028)	(0.0316)		
(log) Real GDP Priv. Industry pc	0.0390***	0.0662***	0.0394***	0.0946***	0.0403***	0.0381***
	(0.0044)	(0.0144)	(0.0044)	(0.0243)	(0.0060)	(0.0083)
(log) Enrollment	0.0109**	-0.1957***	0.0099**	-0.3306***		
	(0.0044)	(0.0157)	(0.0042)	(0.0282)		
% Black	-0.1685**	0.3333*	-0.1668**	0.6541*	-0.0916	-0.4356***
	(0.0724)	(0.1924)	(0.0720)	(0.3622)	(0.0745)	(0.1586)
% Hispanic	-0.0526	0.0483	-0.0529	0.0381	-0.0433	-0.0217
	(0.0327)	(0.1509)	(0.0326)	(0.2516)	(0.0345)	(0.0594)
(log) Annual Avg. Wkly. Wage		0.2269***		0.5618***		0.0873**
		(0.0477)		(0.0765)		(0.0404)
(log, l1) House Price Index					0.0142***	0.8378***
					(0.0049)	(0.0098)
Fixed-effects						
unit	Yes	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	12,084	12,084	12,084	12,084	11,509	11,509
R^2	0.99247	0.90636	0.99247	0.86539	0.99337	0.99074
Within R^2	0.77623	0.51804	0.77617	0.30715	0.78832	0.78569
Wu-Hausman		44.905		138.75		115.32
Wu-Hausman, p-value		2.17×10^{-11}		7.64×10^{-32}		9.12×10^{-27}
Wald (IV only)	1,345.2	22.645	1,517.9	53.948	1,103.7	4.6738
Wald (IV only), p-value	0×10^{-16}	1.97×10^{-6}	0×10^{-16}	2.19×10^{-13}	0×10^{-16}	0.03065
F-test (1st stage)	5,972.4		6,261.7		5,008.7	
F-test (1st stage), (log) Annual Avg. Wkly. Wage		5,972.4		6,261.7		5,008.7
F-test (1st stage), p-value	0×10^{-16}		0×10^{-16}		0×10^{-16}	
F-test (1st stage), p-value, (log) Annual Avg. Wkly. Wage		0×10^{-16}		0×10^{-16}		0×10^{-16}

Clustered (unit) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

3.2 Accounting for Heterogeneity

In order to make meaningful policy-related insights, we need to unmask the substantial heterogeneity obscured by national-level average treatment effects. These national-level estimates are unlikely to apply uniformly across states and commuting zones, especially given heterogeneity in local tax regimes.

Therefore, we (1) use data on local wages and GDP to create indicators for whether regions are exhibiting relative decline or growth compared to other commuting zones to partition our sample in a data-driven manner, employ (2) industry-by-industry and (2) state-by-state estimations in our IV specifications using our VA-based shift-share instrument.

For completeness, we provide results of average treatment effects for all implemented estimations in the Appendices.

3.2.1 Declining vs. Growing Regions

First, we identify declining and growing regions by estimating commuting-zone wage and private industry GDP growth rates conditional on state and national level growth rates and partition our sample across this distribution.

In order to identify declining and growing commuting zones, we estimate separate time series models by commuting zone as follows. These models allow for the identification of commuting-zone level growth rates while controlling for state and national trends in a two-step framework. First, we orthogonalize the state-level growth rate with respect to the national trend, isolating state-specific fluctuations unrelated to the national business cycle:

$$\Delta \log \widetilde{GDPpc}_t^{state} = \Delta \log GDPpc_t^{state} - \hat{\gamma}\, \Delta \log GDPpc_t^{nat}$$

Second, we regress commuting zone growth on both the national growth rate and the orthogonalized state residuals, thereby decomposing local growth into national, state, and idiosyncratic components. This approach identifies commuting zones whose trajectories systematically diverge from higher-level aggregate patterns, providing a clean measure of relative local economic performance.

$$\Delta \log GDPpc_t^{CZ} = \alpha_g + \beta_n\, \Delta \log GDPpc_t^{nat} + \beta_s\, \Delta \log \widetilde{GDPpc}_t^{state} + \varepsilon_t$$

In these equations, each GDP term represents the private industry GDP per capita at the CZ, state, or national level, denoted by superscript.

Intuitively, this specification measures how much of each CZ's growth can be explained by broader aggregate trends versus localized factors. By controlling for orthogonalized state and national variation, the estimated intercept (α_g) and residual terms capture persistent, region-specific trends that are not driven by common macroeconomic forces. This allows us to identify which commuting zones are systematically growing or declining relative to their state and national baselines, thereby providing a purer measure of local economic dynamics that is robust to shared higher-level shocks.[11]

We perform the same trend deviation calculation for wages where each wage variable represents the commuting zone, state, and national level growth rate in the weekly average wage as reported in QCEW.

$$\Delta \log \widetilde{Wage}_t^{state} = \Delta \log Wage_t^{state} - \hat{\gamma}\, \Delta \log Wage_t^{nat}$$

$$\Delta \log Wage_t^{CZ} = \alpha_w + \beta_n\, \Delta \log Wage_t^{nat} + \beta_s\, \Delta \log \widetilde{Wage}_t^{state} + \varepsilon_t$$

Figure 5 plots the distribution of values of α_g, α_w, and the distribution of commuting-zone level loadings on national and state-level growth rates. The figure demonstrates that commuting zones load more variably

[11] We provide similar analysis of gross GDP in the Appendix.

onto state-level growth rates and more consistently onto national-level growth rates. These distributions are expected by design as the state-level growth rates provide variation that the national level rates do not.

Next, Figure 6 demonstrates the considerable variability in GDP-level growth rates across commuting zones in the US between 2001-2021. Visualizing the per capita growth rate deviations by state and region demonstrates heterogeneity in this variability across states and regions. For example, Texas, Montana, North Dakota, and Colorado have outstanding positive outliers in the distribution whereas Kentucky, Louisiana, South Dakota have outstanding negative outliers. The grey lines represent the commuting zones value of α_w, indicating that in a large share of cases, wage growth rates are defined by a different sign than the GDP growth rates, indicating even potential local divergence in growth rates. Though the calculations above could lead to insignificant such relationships because the growth rates are calculated in reference to different data (indeed the distributions represented in the top left panel of Figure 5 have different median values), the volume of states exhibiting diverging wage and GDP per capita growth rates indicate that such a divergence is likely a fact of life in many commuting zones.

To account for this inherent incomparability of the growth rates α_w and α_g, we display a standard Pearson correlation coefficient between the commuting zone time series of GDP per capita and wages, indicating that several states house commuting zones whose wages do not track GDP growth. Many states see nearly exclusively positive correlation coefficients, whereas others see a mix of commuting zones where the relationship is positive or negative.

Commuting Zone Growth Rates and Jurisdictional Loadings

Each point or component unit of a distribution represents a single commuting zone.

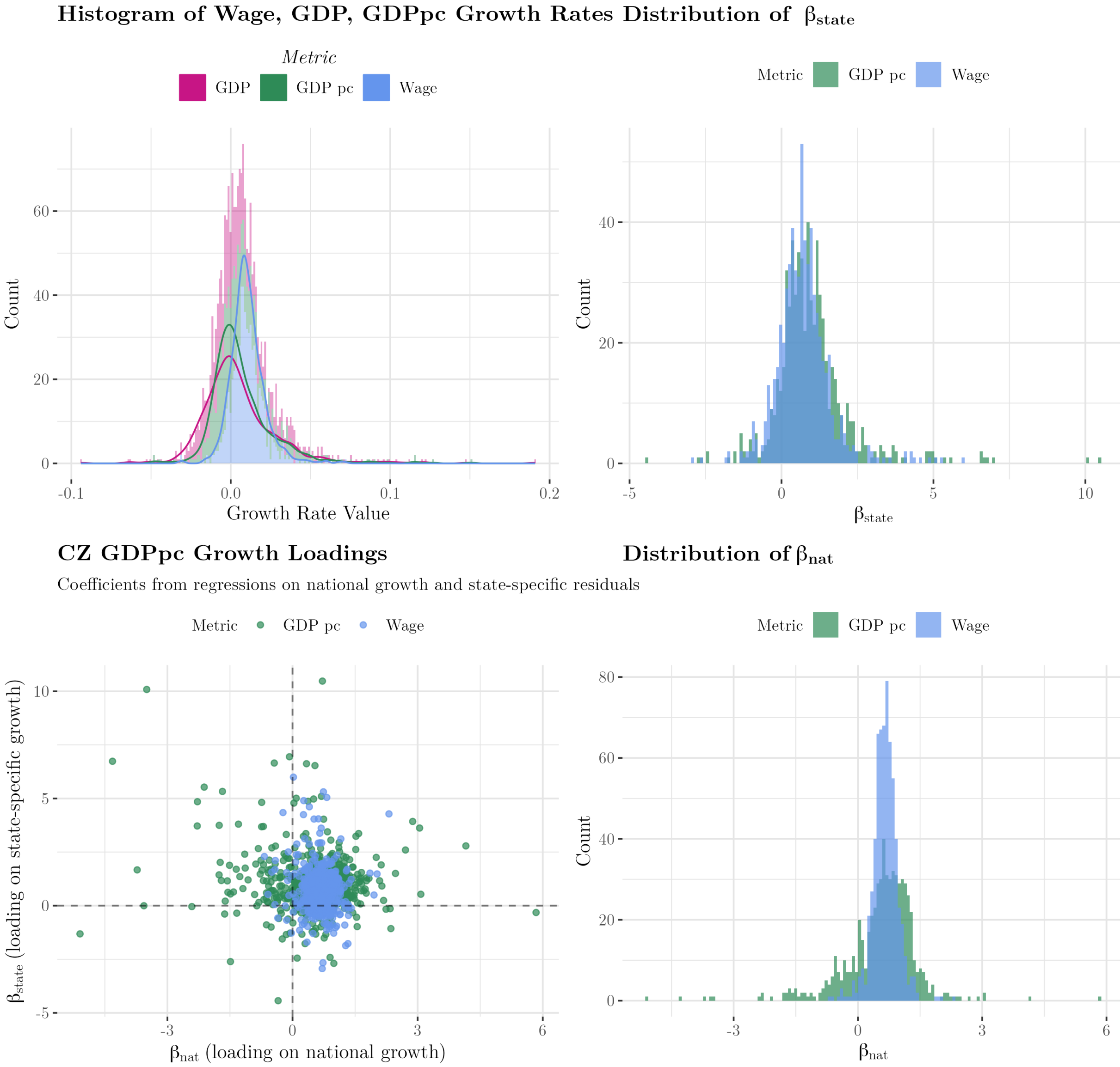


Figure 5: GDPpc and Wage Growth Rates and Loadings

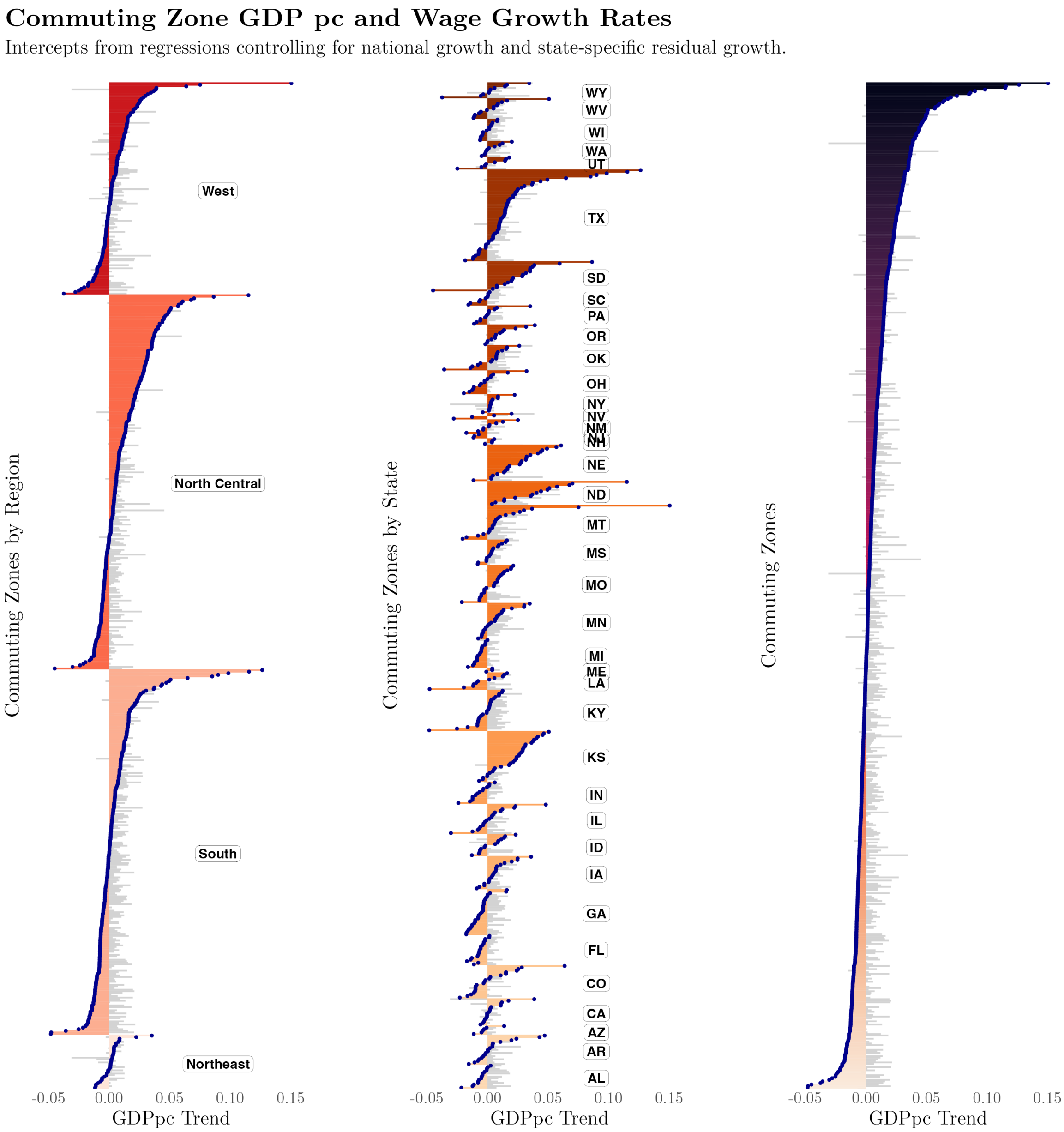


Figure 6: Lollipop Plot of Wage and GDPpc Growth Rates

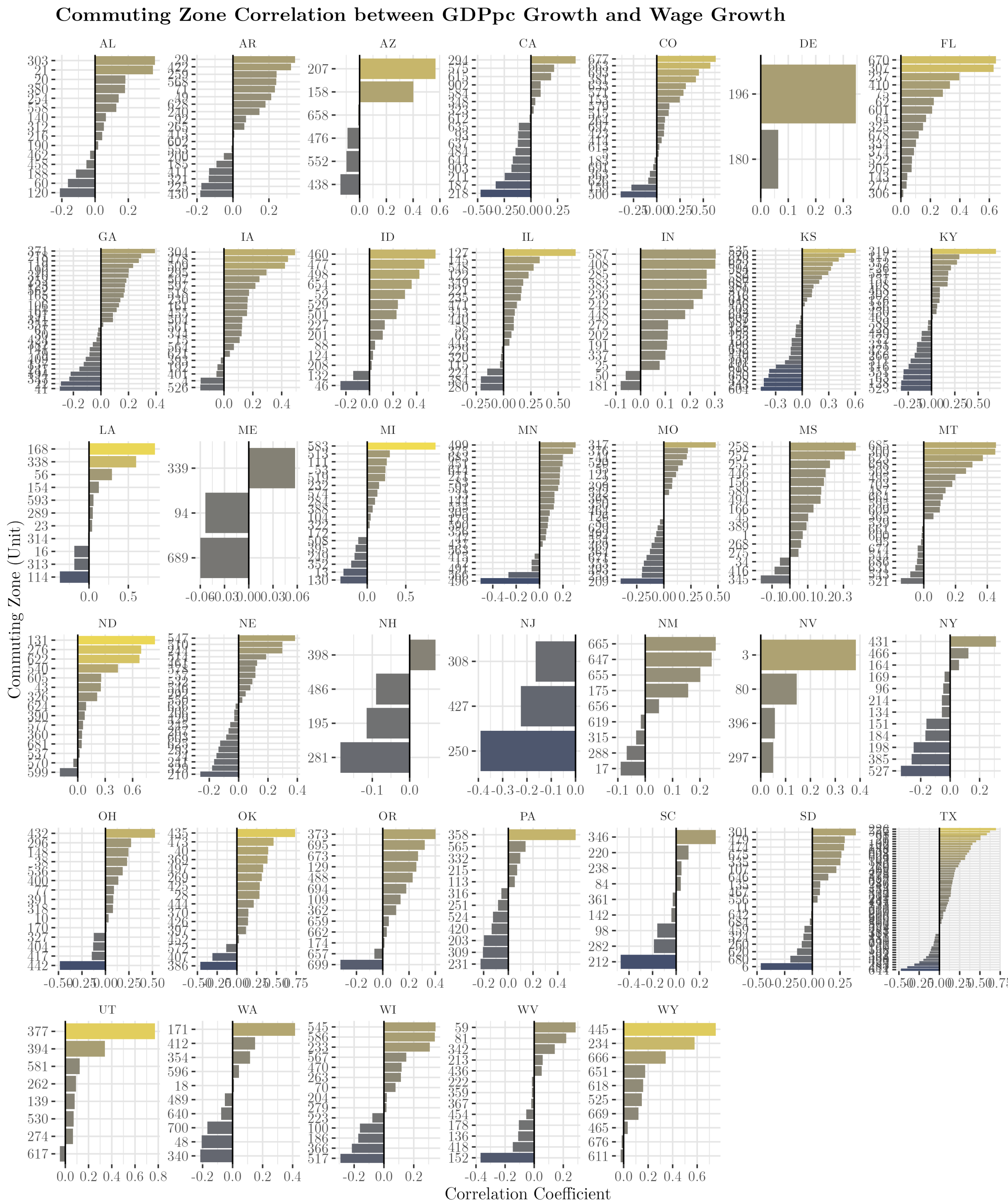


Figure 7: Correlation Between GDP Growth Rates and Wage Growth Rates by State

3.2.1.1 Sample Partitioning by Growth Rates

Using these growth rates, we partition the sample according to the percentiles described above. Table 6 and Table 7 examine how the relationship between local economic conditions and elementary education expenditure per pupil varies across structurally growing and declining regions as defined in the previous section. We partition our sample into four sub-samples by their values of α_w and α_g as shown in Table 5.

Table 5: Category Definitions

Category	Definition (α_w, α_g)
Declining	$\alpha < 0$
Hyper-Declining	$\alpha < P_{25}$
Growing	$\alpha > 0$
Hyper-Growing	$\alpha > P_{75}$

Zones with negative (positive) values of α_w or α_g are designated as declining (growing), while those in the bottom (P25) and top (P75) quartiles are labelled hyper-declining and hyper-growing, respectively. This stratification enables comparison of fiscal responsiveness across local economies with different long-run growth trajectories.

Table 6 partitions CZs by α_w. Interestingly, we observe positive statistically significant relationships between wages and public education expenditure though the magnitude and statistical significance of this relationship declines almost uniformly as α_w discretely increases. We see a similar declining size and significance in the explanatory variable accounting for inter-governmental revenue, indicating that these revenues play a smaller role in regions exhibiting above-average wage growth.

In Table 7 partitions CZs by long-run GDP per capita trends. The statistical significance and magnitude variations in the wage response tell a less monotonic story here, likely mirroring the lack of systematic correlation between α_w and α_g. When partitioning the sample by GDP growth rates, the interpretation is more straight-forward.

Regardless, all models remain well-identified, with high first stage F statistics, convincing performance on the Wu-Hausman endogeneity tests and Wald tests. Combined, these results indicate that partitioning by background wage growth rates provides greater insight into the sample's heterogeneity than diversity in GDP per capita growth rates. More precisely, given communities feel wage growth more directly than GDP per capita improvements, it is likely that the relationship between public education and wage changes is better captured by sub-sampling by wage growth rates.

Table 6: Second-Stage: VA-based Shift-Share Instrument (l1) Applied to Declining Wage vs. Growing Wage Regions

Dependent Variable:	(log) Elem.Ed.Exp.pp				
	All	Hyper-Declining (Wage)	Declining (Wage)	Growing (Wage)	Hyper-Growing (Wage)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
(log) Annual Avg. Wkly. Wage	0.2269***	0.3277***	0.3394***	0.2128***	0.1551*
	(0.0477)	(0.0683)	(0.0861)	(0.0527)	(0.0830)
(l1, log) Elem.Ed.Exp.pp	0.5086***	0.5148***	0.5543***	0.5019***	0.5311***
	(0.0156)	(0.0267)	(0.0332)	(0.0173)	(0.0290)
(log) IG Revenue pp	0.2230***	0.2800***	0.2101***	0.2245***	0.1553***
	(0.0211)	(0.0341)	(0.0317)	(0.0239)	(0.0391)
(log) Real GDP Priv. Industry pc	0.0662***	0.0156	0.0220	0.0703***	0.0685***
	(0.0144)	(0.0238)	(0.0313)	(0.0150)	(0.0160)
(log) Enrollment	-0.1957***	-0.2269***	-0.1995***	-0.1941***	-0.2001***
	(0.0157)	(0.0254)	(0.0351)	(0.0175)	(0.0332)
% Black	0.3333*	0.2172	0.2137	0.3892*	1.193**
	(0.1924)	(0.2664)	(0.3680)	(0.2294)	(0.5557)
% Hispanic	0.0483	0.2597	0.2509	0.0110	0.1335
	(0.1509)	(0.2566)	(0.1897)	(0.1781)	(0.3037)
Fixed-effects					
unit	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	12,084	3,021	1,520	10,564	3,021
R^2	0.90636	0.92102	0.94210	0.89805	0.90098
Within R^2	0.51804	0.57170	0.58942	0.50752	0.50064
Wu-Hausman	44.905	12.495	8.9646	36.833	18.989
Wu-Hausman, p-value	2.17×10^{-11}	0.00041	0.00280	1.33×10^{-9}	1.36×10^{-5}
Wald (IV only)	22.645	23.023	15.549	16.276	3.4973
Wald (IV only), p-value	1.97×10^{-6}	1.68×10^{-6}	8.41×10^{-5}	5.52×10^{-5}	0.06157
F-test (1st stage), (log) Annual Avg. Wkly. Wage	5,972.4	1,354.5	813.43	5,156.1	1,214.4
F-test (1st stage), p-value, (log) Annual Avg. Wkly. Wage	0×10^{-16}	0×10^{-16}	0×10^{-16}	0×10^{-16}	0×10^{-16}

Clustered (unit) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 7: Second-Stage: VA-based Shift-Share Instrument (l1) Applied to Declining GDP vs. Growing GDP Regions

Dependent Variable:	(log) Elem.Ed.Exp.pp				
	All	Hyper-Declining (GDP)	Declining (GDP)	Growing (GDP)	Hyper-Growing (GDP)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
(log) Annual Avg. Wkly. Wage	0.2269***	0.2785***	0.2912***	0.1874***	0.2532***
	(0.0477)	(0.0737)	(0.0648)	(0.0595)	(0.0822)
(l1, log) Elem.Ed.Exp.pp	0.5086***	0.5430***	0.5286***	0.4937***	0.4893***
	(0.0156)	(0.0212)	(0.0193)	(0.0218)	(0.0282)
(log) IG Revenue pp	0.2230***	0.2290***	0.2575***	0.2086***	0.1911***
	(0.0211)	(0.0405)	(0.0345)	(0.0275)	(0.0397)
(log) Real GDP Priv. Industry pc	0.0662***	0.0378	0.0398	0.0714***	0.0759***
	(0.0144)	(0.0358)	(0.0320)	(0.0160)	(0.0172)
(log) Enrollment	-0.1957***	-0.2011***	-0.1911***	-0.2017***	-0.2353***
	(0.0157)	(0.0332)	(0.0238)	(0.0209)	(0.0295)
% Black	0.3333*	0.1517	0.1511	0.6266*	0.7367
	(0.1924)	(0.2226)	(0.1964)	(0.3421)	(0.7621)
% Hispanic	0.0483	-0.1417	-0.0061	0.0687	-0.0070
	(0.1509)	(0.2286)	(0.1702)	(0.1938)	(0.2165)
Fixed-effects					
unit	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	12,084	3,021	5,016	7,068	3,021
R^2	0.90636	0.90369	0.90765	0.90377	0.85786
Within R^2	0.51804	0.54248	0.55592	0.47980	0.48120
Wu-Hausman	44.905	10.070	18.286	20.577	22.401
Wu-Hausman, p-value	2.17×10^{-11}	0.00152	1.94×10^{-5}	5.83×10^{-6}	2.32×10^{-6}
Wald (IV only)	22.645	14.299	20.216	9.9366	9.5022
Wald (IV only), p-value	1.97×10^{-6}	0.00016	7.07×10^{-6}	0.00163	0.00207
F-test (1st stage), (log) Annual Avg. Wkly. Wage	5,972.4	1,846.6	2,651.0	3,287.8	1,501.4
F-test (1st stage), p-value, (log) Annual Avg. Wkly. Wage	0×10^{-16}	0×10^{-16}	0×10^{-16}	0×10^{-16}	0×10^{-16}

Clustered (unit) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

3.2.2 State-by-state estimation

Next, given the substantial heterogeneity in state-level economic makeup and public finance regimes, we investigate state-specific relationships between our variables of interest.

First, states vary in the number of commuting zones they contain. Figure 12 demonstrates that states have anywhere between 2 (Delaware) and 58 (Texas) commuting zones. This allows us to estimate panel-style regressions within each state to net out between-state variation that might be confounding our current treatment estimates. We exclude any states that contain less than 5 commuting zones due to sample size concerns.

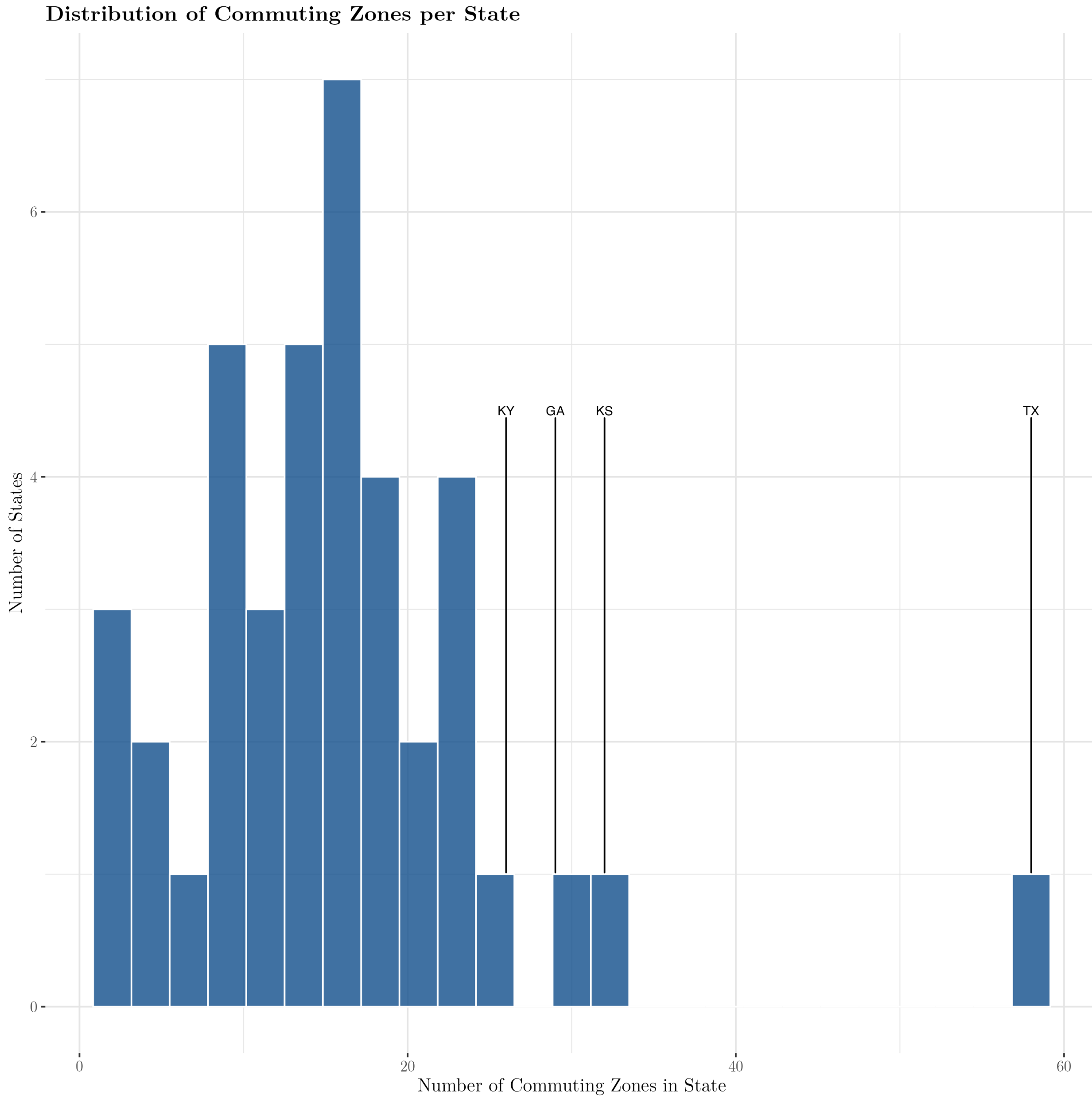


Figure 8: Histogram: Commuting Zones by State

Using our instrumental variable approach with a value-added based shift-share instrument, we corroborate the directionality and magnitude of the effect for 11 states: Colorado, Florida, South Dakota, Kentucky, Louisiana, Pennsylvania, North Dakota, Oregon, Oklahoma, Arizona, and Indiana. In estimating the state-by-state regressions, we exclude any states where our F-statistic is below conventional weak instrument thresholds (F statistics <= 12 and p-value < 0.05) and the p-value of the second stage coefficient of interest is < 0.1.

Examining various characteristics of these states, we find that they vary widely in demographic composition, enrollment levels, and wage levels indicating that the detected effects are not attributable to any extremes in these values. Notably, they even vary in reliance on local sources educational expenditure.

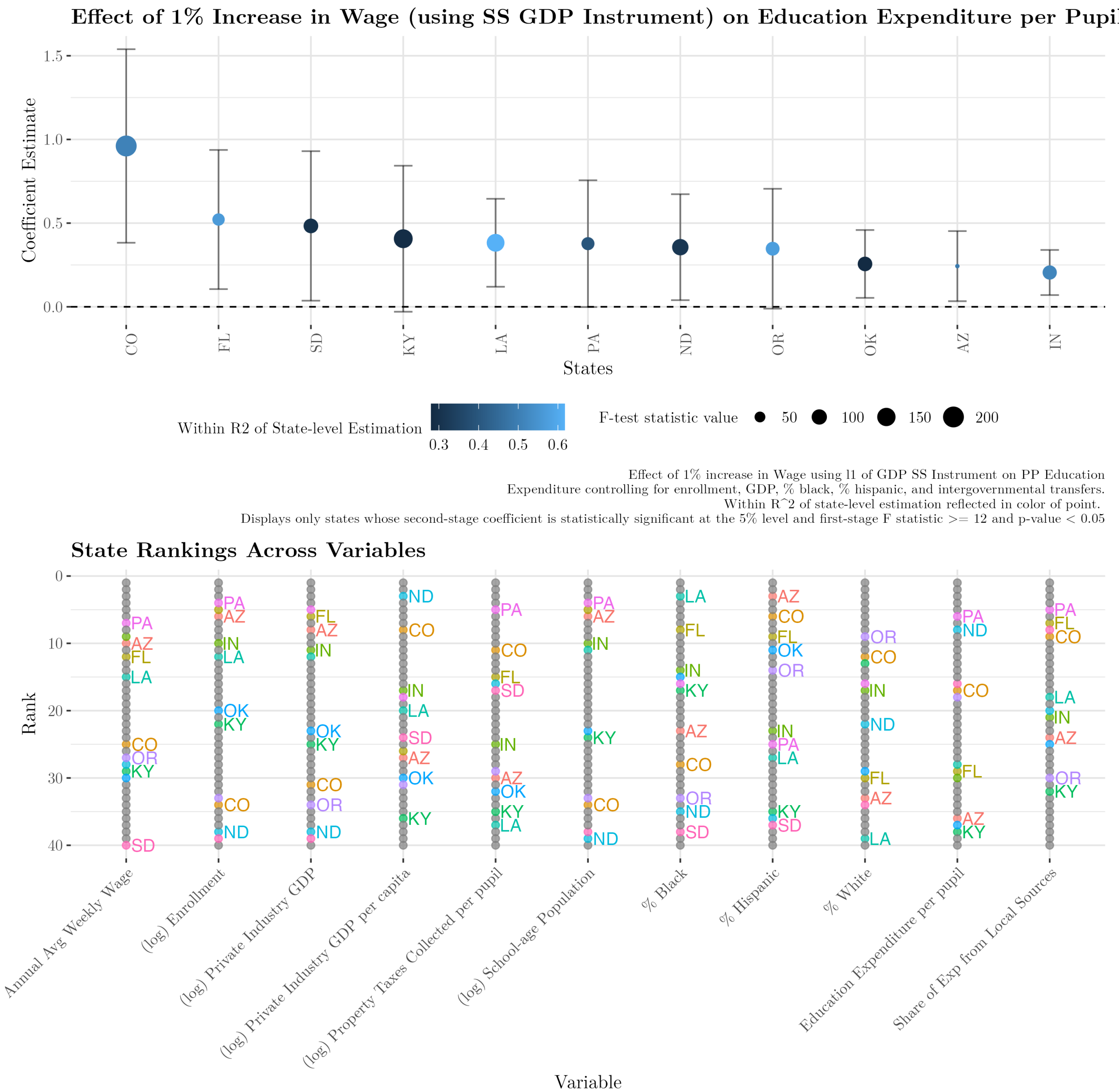


Figure 9: State-by-State Wage Effect Using SS GDP Shock

3.2.3 Industry by Industry

Finally, given our shift-share instruments are the composite effect of shifts in industry-level real value added (VA), we can decompose this instrument into industry-specific real value added shocks. This decomposition allows us to examine the effect of industry-specific changes across states in a more explicit manner. In other words, we re-design our instrument as…

$$\tilde{Z}_{ijt} = G_{njt} * \frac{N_{ij\tau}}{N_{i\tau}} \tag{6}$$

…rather than the sum of all industry-level shocks.

We estimate separate panel regressions using the full commuting zone sample and then grouping commuting zones by state instrumenting local level wages by these decomposed shift-share shocks by industry.

Using our value added-based shift share instrument, Figure 10 demonstrates the overall treatment effect of local wage changes instrumented via an industry-specific GDP shock. We find that regardless of the instrument design, the coefficient estimate is consistent with the baseline results, where the estimated effect of a 10% change to local wages on public education expenditure is a 2.2% increase, with the effect's magnitude varying meaningfully for several states. We plot the relevant state-level effects for those states whose estimations pass the same restriction criteria as above (F statistics <= 12 and p-value < 0.05 and the p-value of the second stage coefficient of interest is < 0.1).

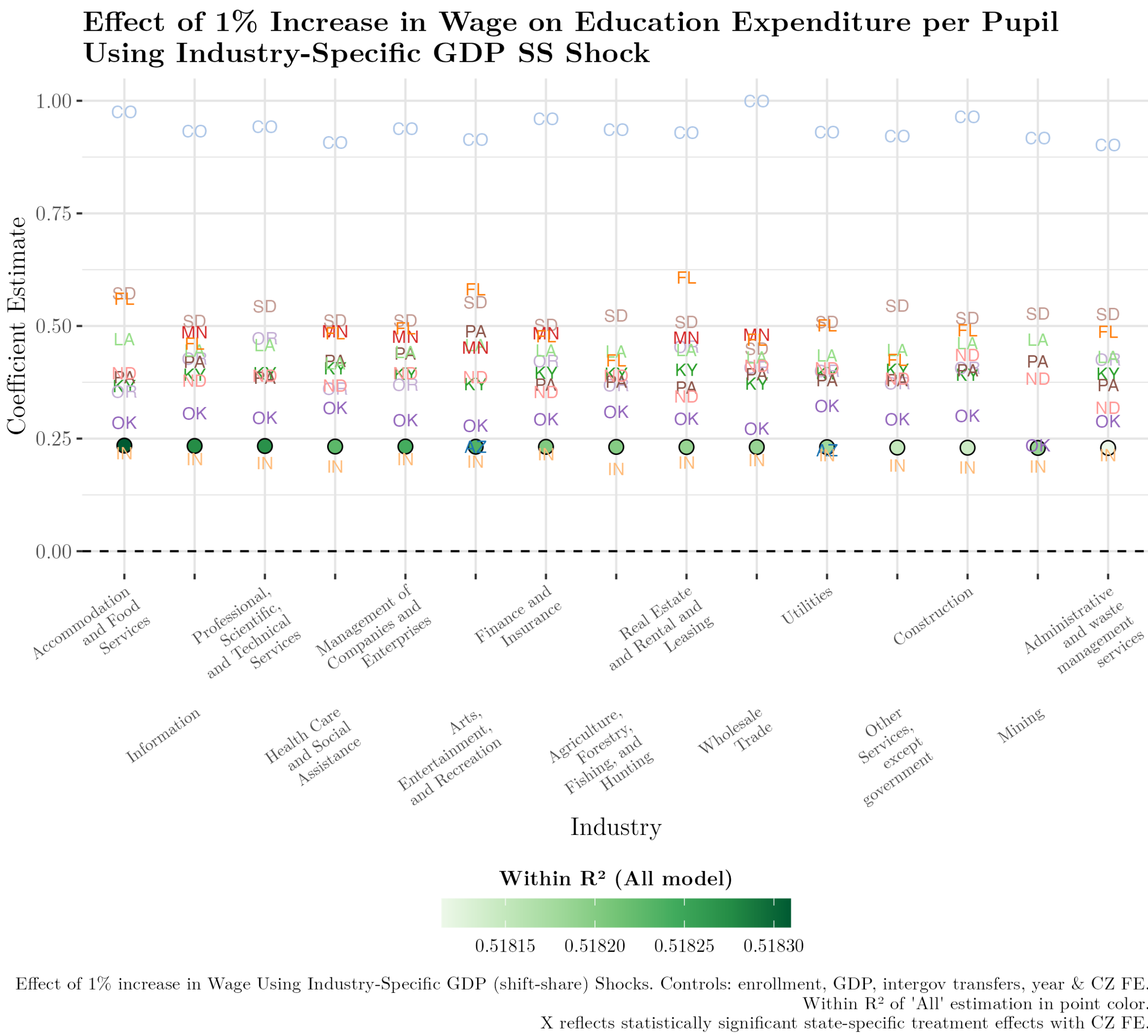


Figure 10: Wage Effect via Industry VA SS Shock

4 Conclusion

4.1 Discussion

This paper set out to examine whether local wage growth translates into higher local public education expenditure across US commuting zones, and whether this relationship varies across institutional environments, labor-market conditions, and long-run regional trajectories. Motivated by the persistent divergence between productivity growth and wage gains for typical workers, we asked whether the decoupling of wages from productivity has implications beyond household well-being and specifically, whether uneven wage growth shapes the fiscal capacity of communities to invest in public education.

We establish a causal link between local wage growth and public education expenditure. We show that public education expenditure is not insulated from local labor-market conditions. In the baseline specification, a 10 percent increase in local wages generates a 2.2 percent increase in education spending in the short run and a 4.6 percent increase in the long run. The dynamic components of the econometric specification indicate significant persistence in local public education budgets. This persistence likely reflects institutional inertia but also the infrequency with which local tax rates are revised, allowing local wage growth to boost education spending. It also suggests asymmetric adjustment: communities experiencing wage growth are likely to see gradual increases in education spending as higher incomes strengthen their revenue base, while declining communities could experience the opposite effect.

Importantly, the positive elasticity we document is not uniform across the country. It is concentrated in less than a third of the states in the sample (Colorado, Florida, South Dakota, Kentucky, Louisiana, Pennsylvania, North Dakota, Oregon, Oklahoma, Arizona, and Indiana). Many other states exhibit minimal or insignificant responsiveness, suggesting that these risks are not uniformly distributed. These findings underscore that equalization programmes need to consider different types of risks facing communities when designing their work.

Though outcomes measured in this study are not direct inequality metrics, the results have clear implications for spatial disparities in local public well-being. Our findings reveal that the decentralized school financing system in the US has the potential to exacerbate inequalities in local public well-being by failing to equalize across regions experiencing diverging growth trajectories. As a result, in the states in which local spending is responsive to changes in local wages, the quality of early childhood education might be compromised by macroeconomic trends and industry-specific shocks beyond local control. Thus, equalization formulas at the state and federal level that fail to account for wage trends and fiscal multipliers may contribute to disparities in public goods delivery. Theories of effective equalization, and indeed preferences for redistribution, differ especially across regions of the the United States. However, equalization efforts should at least aim to insulate communities from potential downward pressures on public education expenditure.

The determinants of inequality in public education delivery in the US are multiple and complex. Significant evidence exists of the role of historically discriminatory policies related to congressional districting, under-investment in low-income areas of color [39, 40, 101]. Though this work does not directly inform this debate, further work could explore the extent to which wage growth interacts with such structural policies.

Addressing structural inequality requires rethinking education finance, taking the decentralized American context as given. Ensuring educational equity for all children requires not just strengthening existing mechanisms for redistribution, but also, in light of our results, insulating communities from macroeconomic trends that impact communities heterogeneously.

4.2 Limitations

- Though we have attempted to bypass deep endogeneity issues in the relationship between wages and public education expenditure, our excludability assumptions about local industry composition are strong.
- Data limits true consideration of non-stationarity issues. Estimating this model in first differences or growth rates would be preferable to address non-stationarity concerns as well as allow for asymmetric treatment estimation to distinguish between negative and positive wage pressures. However, this would require improvements in data collection of the required variables.
- Commuting zones mask substantial within-commuting zone heterogeneity. Further work could apply the same estimation strategy to county-level observations to address this challenge.

5 Inventory of Remaining Work

Below, we outline the remaining work planned prior to pursuing journal submission. In addition to cosmetic improvements to the manuscript, we intend to:

- Revise the industry-by-industry estimation. The industry-by-industry estimation in the main text could benefit from revision. Rather than instrumenting local wage levels, we intend to instrument *industry-specific* wages using the described industry-specific shocks. We believe this could link well to the intro in which only those regressions in which there is a strong first stage (i.e., GDP changes predict local wages well) would qualify for analysis.
- Explore the states in which we detect a statistically significant effect in greater detail. We plan to evaluate their industrial composition, economic history, and public goods and taxation regimes to better understand the source of their non-zero elasticities to local wage changes. This investigation would hopefully allow for greater detail in the resulting policy recommendations of the work.
- Provide an improved calculation about the economic meaning of the results. Though we provide a preliminary back-of-the-envelope calcuation about the impact of a 2% change in expenditure per pupil in terms of staff costs, this calculation is likely simplified. We plan to make a comparison of the scale of potential changes to public education budgets in terms of staff, food, or other resource costs using data from the SLGF. Additionally, we might be able to speak to the potential for such a change in expenditure per pupil to reduce class sizes or fund salary increase for teachers, for example.
- Report the marginal F-statistic of the instrumental variable specifications to demonstrate the relative value of the instruments independent of the effect of the auto-regressive term in the first-stage. The autoregressive terms are likely inflating the F-statistic which is an important limitation to highlight in the main text.
- Report over-identification tests in regression tables.
- Include additional appendices reporting the results when instrumenting local property prices rather than local wage levels.
- Incorporate data on private school enrollment to test whether these rates respond to changes in local wages.
- Provide a robustness check using a cross-sectional empirical strategy to determine the relative importance of cross-sectional versus time variation in the sample.

6 Data and Code Availability

Code and data to reproduce the analysis will be made available on Zenodo. A working version of the database and code is currently public on [Github](https://github.com/ebbam/pub_fin_schools).

7 Use of AI

Below, we provide a summary of the tasks where AI has been used. We limited its use to debugging and efficiency improvements during data cleaning and analysis, formatting of tables and figures, and helping with compatiblity between various typesetting tools:

- Used ChatGPT or Claude to help improve readability of plots (formatting, margins, labeling, font size, distinguishing between model fit and testing values).
- Used ChatGPT or Claude to debug errors in R during data cleaning and plotting.
- Used ChatGPT to improve compatibility between Quarto Markdown and Latex integrations.
- Used ChatGPT to provide suggestions for reducing run time of repetitive tasks (ex. downloading and processing multiple data files).

Appendices

A Modelling Challenges

Below, I provide a brief discussion of anticipated methodological challenges and constraints.

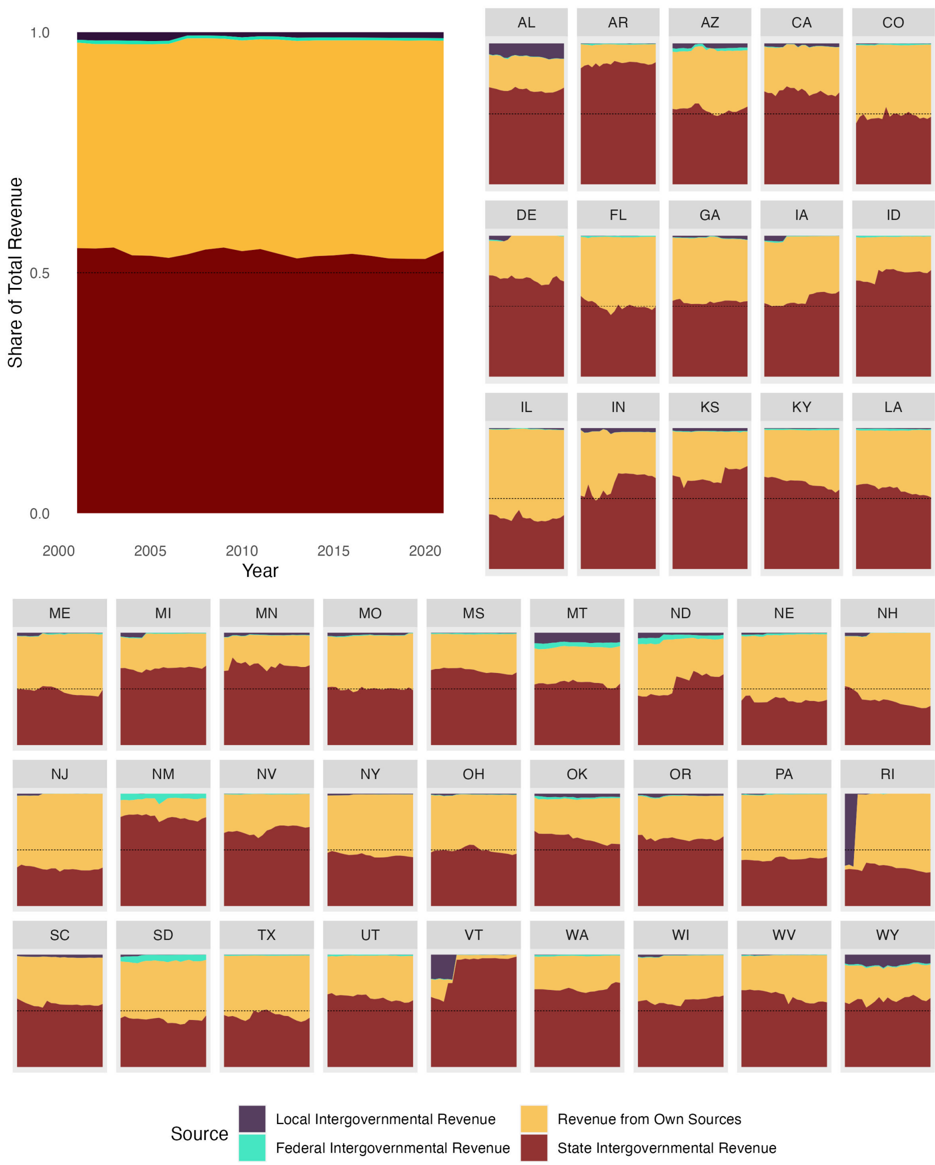
A.1 Structure of Financing for Local Public Education

In order to appropriately make use of the outlined data as well as robustly define the econometric methods to be utilized in this work, an understanding of the funding structure of public school districts in the US is critical. Public school districts in the United States are funded by a combination of federal (8.3% in 2019), state (47% in 2019), and local (44.8% in 2019) revenues [52], with shares varying by county. This variation in public funding structure will need to be incorporated into the modelling efforts, likely through a weighted regression approach based on shares of intergovernmental versus own-source revenues [52]. Using the data outlined, Figure 11 displays the share of public education revenue coming from three sources of intergovernmental revenue (federal, state, and local) as well as revenue from own county-level sources by state. The figure demonstrates the clear near-even split between state intergovernmental and own source revenue and the overall small share of revenue coming from federal or other local governments. The larger panel on the top left provides the summarising share at the national level. All plots share the axes as labeled in the top left panel.

A.2 Trends over time

According to the most recent data available from the US Congressional Research Service, the revenue share has shifted from local to state sources whereas federal funding has remained the same albeit with fluctuations over time [52].

Figure 11: Share of Revenue from Federal, State, Local Sources



A.3 Historical efforts to "equalize" US public education

Another factor that greatly impacts the data generating process in this study is that increasing recognition of the level of inequality of public education provision in the US has led to the implementation of several efforts to "equalize" public education by aiming for "per pupil" expenditure targets [52]. The most significant change in this respect has been the creation of Educational Service Agencies (ESAs). These ESAs are apportioned state funding to serve multiple school districts in sub-regions of each state. Most of these ESAs were established around 2007 and persist to this day. ESAs are listed by state in Table 8. Currently, there are 553 agencies nationwide in 45 states. According to the Association of Educational Service Agencies (AESA), ESAs reach over 80% of the public school districts and well over 80% of public and private school students. Annual budgets for ESAs total approximately $15 billion [59]. Because ESA revenue and expenditure is inconsistently reported across years in our dataset, as well as attributed to individual counties despite often serving multiple, there is a significant risk that ESA expenditure is misattributed to counties in our dataset. Therefore, I exclude ESA revenue and expenditure totals from the measures of county-level expenditure and revenue at all levels of aggregation, and retain these values as possible control variables.

Preliminary investigation, both descriptive and using regression models, indicate that public expenditure from ESAs have not acted as a substitute for other revenue sources. In other words, they have not displaced intergovernmental or local school revenue. Although this fact ensures that changes in public spending on education detected in our models are not overestimated due to substitution effects from unmodelled ESA expenditure, it does risk underestimating values of actual expenditure per pupil. This remains to be resolved.

A.4 Availability of varying local-level outcomes

Approaching a more "local" analysis of such challenges is often inhibited by data availability. First, data limitations including infrequent periodicity and missingness due to strained local reporting capacity or low stringency impose a limit on the statistical power in a panel analysis. Furthermore, infrequent periodicity poses the additional challenge to interpretation when assessing the impact of industrial changes that are often subject to within-year cyclicality.

A.5 Structural and policy heterogeneity

County-level analysis of the US poses an inherent trade-off between greater local insight and requisite model complexity. First, county-level variables are subject to unit- and time-dependent variation, which can be partly, although likely not adequately, dealt with through the incorporation of appropriate control variables and two-way fixed effects. This work will aim to incorporate consideration of spatial auto-correlation between counties to further deal with these estimation challenges. Second, and perhaps most challenging, counties are subject to state-wide regulatory, economic, and social conditions that can vary greatly across states. I aim to control for state-level variation using either an additional state-fixed effect in our regression models or state-level time trends. However, I remain wary of the residual effect of state-level heterogeneity in policy regimes and culture on our estimation results. I remain open to the idea of restricting our analysis to a smaller set of states or even a state-by-state analysis.

A.6 Cross-Sectional Dependence

This latter point on state-level heterogeneity points to an additional challenge when modelling more local- or county-level variation: cross-sectional dependence. Neighboring counties, particularly counties in the same state, will inevitably exhibit high levels of spatial dependence and auto-correlation. Adding further complication, state boundaries implicate any assumption of linearity in spatial dependence at the county level (ie. neighboring counties on either side of a state border will likely be less similar than neighboring counties within the same border).

Table 8: Educational Service Agencies by State

State	**ESA Name**	**#**
Alabama		
Alaska	Educational Resource Center (SERRC)	1
Arizona	Office County of School Superintendent	15
Arkansas	Education Service Cooperative	15
California	County Office of Education	58
Colorado	Board of Cooperative Educational Services	21
Connecticut	Regional Education Service Center	6
Delaware		
Florida	Regional Consortium Service Organization	3
Georgia	Regional Education Service Agency	16
Hawaii		
Idaho		
Illinois	Regional Office of Education; Intermediate Service Center	35; 3
Indiana	Educational Service Center	9
Iowa	Area Education Agency	9
Kansas	Interlocal Cooperative - Service Center	7
Kentucky	Education Cooperative	8
Louisiana	Special School District	0
Maine		
Maryland		
Massachusetts	Educational Collaborative	25
Michigan	Intermediate School District	56
Minnesota	Regional Service Cooperative; Intermediate School District	9; 4
Mississippi	Regional Educational Service Agency	6
Missouri	Educational Service Agency	4
Montana	Educational Cooperative	2
Nebraska	Educational Service Unit	17
Nevada		
New Hampshire	Educational Service Center	4
New Jersey	Educational Services Commission	11
New Mexico	Regional Education Cooperative	10
New York	Board of Cooperative Educational Services	37
North Carolina	Regional Educational Service Agency	8
North Dakota	Regional Education Association	7
Ohio	Educational Service Center	51
Oklahoma		
Oregon	Educational Service District	19
Pennsylvania	Intermediate Unit	29
Rhode Island	Educational Collaborative	3
South Carolina	Regional Consortium	6
South Dakota	Educational Service Unit	14
Tennessee	Educational Cooperative	Unknown
Texas	Regional Education Service Center	20
Utah	Regional Education Service Agency	4
Vermont		
Virginia		
Washington	Educational Service District	9
West Virginia	Educational Service Cooperative	3
Wisconsin	Cooperative Educational Service Agency	12
Wyoming	Board of Cooperative Educational Services	3

[a] Source: Association of Educational Service Agencies, State by State ESA Report 2021

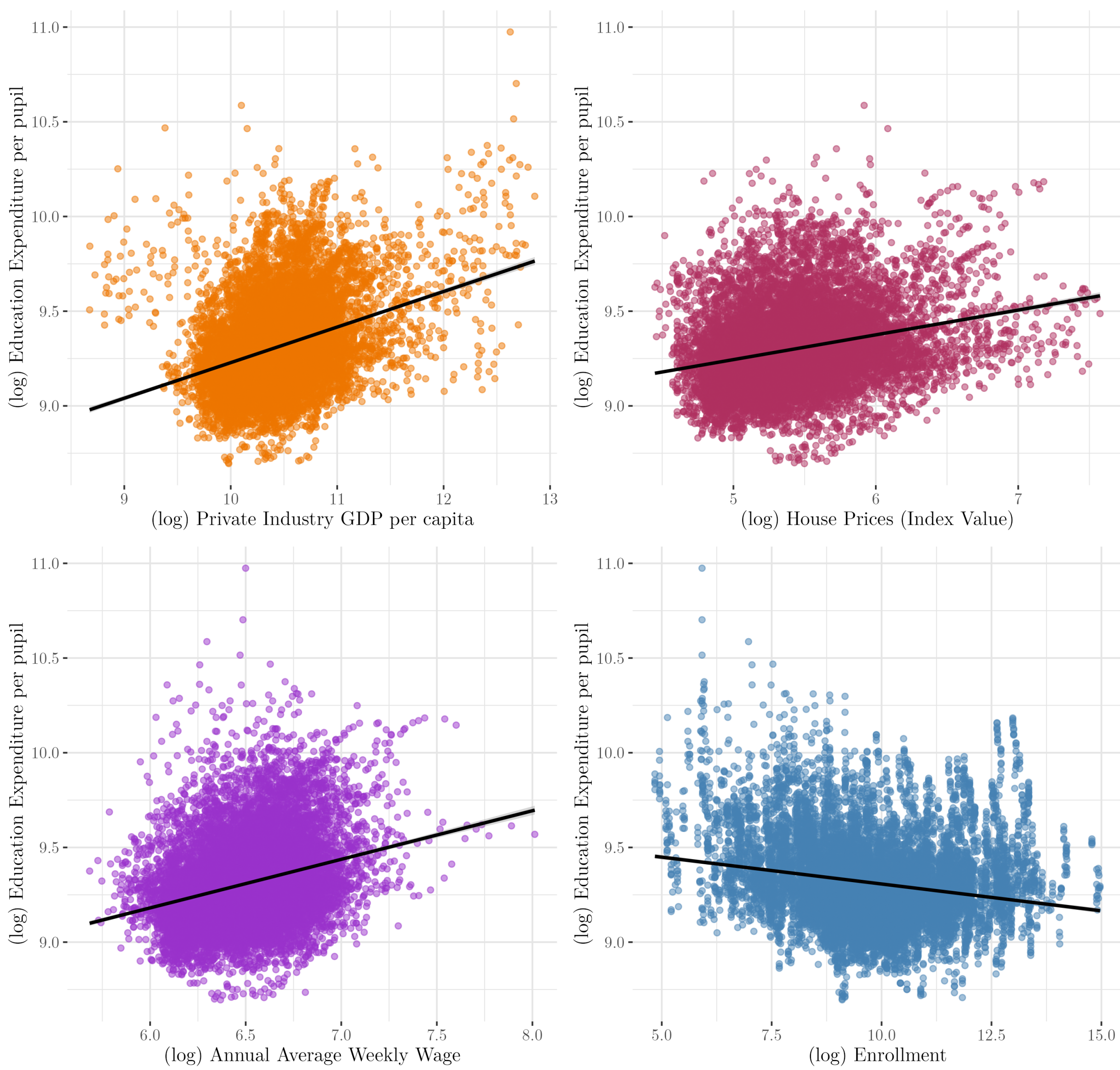
B Key Relationships between Economic Variables

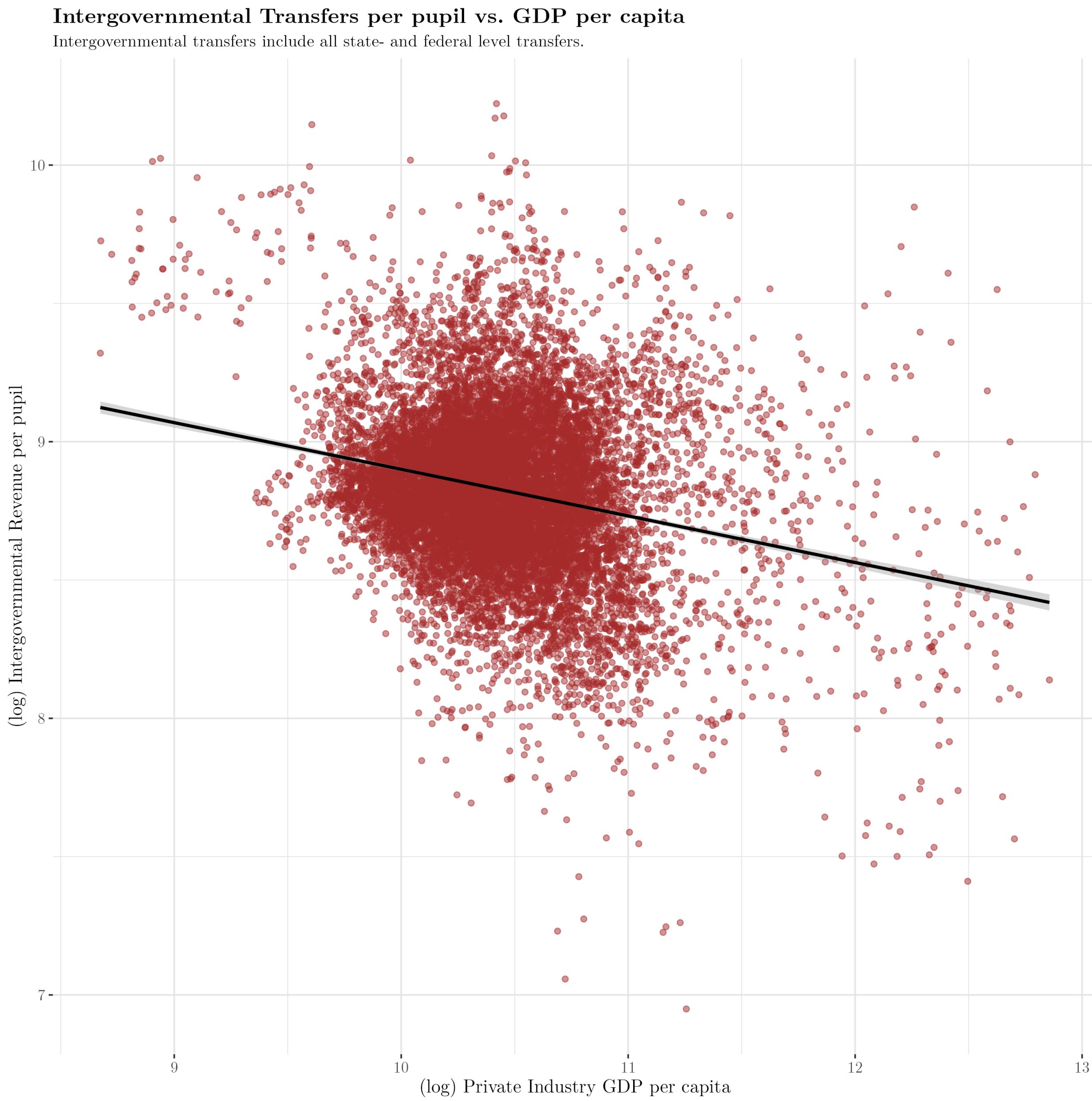
Below we display key relationships between several of the economic variables in our study. All economic indicators (house prices, private industry GDP, and wages are positively associated with education expenditure, whereas enrollment is negatively associated with education expenditure per pupil.

We report descriptive regressions in this section to establish relationships between property taxes, education expenditure, private industry GDP, and house prices. All regression models are estimated using two-way fixed effects and standard errors clustered by commuting zone.

Key Relationships Between Economic Variables and Levels of per pupil Education Expe

Commuting Zone Level. 2001-2020.





B.1 Property Tax ~ GDP

GDP has a highly relevant relationship to property taxes. A 1% increase in GDP (per capita) is assocaited with a 0.38% (0.32%) increase in property taxes collected (per capita) (Table 9).

Table 9: Relationship between Property Tax and Local GDP

Dependent Variables:	(log) Prop Tax		(log) Prop Tax pp	
Model:	(1)	(2)	(3)	(4)
Variables				
(log) Real GDP	0.3854***	0.1226***		
	(0.0480)	(0.0325)		
(log,l1) Real GDP		0.1193***		
		(0.0274)		
(log,l2) Real GDP		0.0697**		
		(0.0285)		
(log,l3) Real GDP		0.0790***		
		(0.0183)		
(log,l4) Real GDP		0.1198***		
		(0.0384)		
(log) Real GDP pc			0.3151***	0.1212***
			(0.0616)	(0.0366)
(log,l1) Real GDP pc				0.0929***
				(0.0271)
(log,l2) Real GDP pc				0.0677**
				(0.0328)
(log,l3) Real GDP pc				0.0731***
				(0.0229)
(log,l4) Real GDP pc				0.0624*
				(0.0351)
Fixed-effects				
unit	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	13,356	10,812	13,356	10,812
R^2	0.99175	0.99329	0.93467	0.94256
Within R^2	0.10787	0.15702	0.06308	0.08956

Clustered (unit) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

B.2 Education Expenditure ~ Revenue Sources

The below regressions are included to establish the relationship between education expenditure and its component parts (ie. the largest form of IG revenue is state funding and Own Source revenue is largely sourced from Property Taxes).

Dependent Variables:	(log) Elem.Ed.Exp.pp				(log) Elem.Ed.Exp.			
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
(log) Rev. Own Sources pp	0.3604***							
	(0.0190)							
(log) IG Revenue pp	0.4469***		0.4532***					
	(0.0244)		(0.0265)					
(log) Prop Tax pp		0.2266***	0.2871***	0.2897***				
		(0.0180)	(0.0185)	(0.0181)				
(log) Fed IG Rev pp				0.0019				
				(0.0019)				
(log) State IG Rev pp				0.4307***				
				(0.0283)				
(log) Prop Tax					0.2565***	0.3014***	0.3070***	
					(0.0195)	(0.0194)	(0.0192)	
(log) IG Revenue						0.5020***		0.4853***
						(0.0252)		(0.0234)
(log) Fed IG Rev							0.0005	
							(0.0007)	
(log) State IG Rev							0.4823***	
							(0.0269)	
(log) Rev. Own Sources								0.3760***
								(0.0191)
Fixed-effects								
unit	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics								
Observations	13,356	13,356	13,356	13,356	13,356	13,356	13,356	13,356
R^2	0.89075	0.82859	0.88016	0.87791	0.99566	0.99738	0.99732	0.99763
Within R^2	0.45044	0.13778	0.39717	0.38586	0.14427	0.48315	0.47095	0.53223

Clustered (unit) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

C Baseline Descriptive Estimates

We descriptively establish foundational relationships between local economic conditions that seem to reasonably generalise across the country using an AR(1) two-way fixed effects ordinary least-squares panel model with standard errors clustered by commuting zone. We outline the model specification immediately below:

$$Y_{it} = \beta_0 + \beta_x X_{it} + \theta Y_{it-1} + \delta_1 Enrollment_{it} + \delta_2 IGR_{it} + \delta_3 Black_{it} + \delta_3 Hispanic_{it} + \alpha_i + \gamma_t + \varepsilon_{it} \quad (7)$$

Y_{it} is the natural logarithm of elementary (serving ages 6-12) education expenditure per pupil for CZ i in year t. α_i represents a CZ fixed effect and γ_t represents year-fixed effects, respectively. ε_{it} represents the error term. We control for enrollment to account for scaling factors in education expenditure and intergovernmental transfers to account for the significant role of such transfers in funding education expenditure. X_{it} takes three forms represented by Equation 8, Equation 9, Equation 10 where h represents h-year time lags. We estimate all equations in levels and growth rates.

$$X_{it}^{GDP} = \sum_{h=0}^{2} \beta_h^{\text{GDP}} \log(\text{GDP}_{i,t-h}) \tag{8}$$

$$X_{it}^{Wage} = \sum_{h=0}^{2} \beta_h^{\text{Wage}} \log(\text{Wage}_{i,t-h}) \tag{9}$$

$$X_{it}^{HPI} = \sum_{h=0}^{2} \beta_h^{\text{HPI}} \log(\text{HPI}_{i,t-h}) \tag{10}$$

Table 12 reports the results from regressions of log elementary education expenditure per pupil on contemporaneous and lagged measures of local economic activity. Table 13 presents the analogous specifications using annualized growth rates to capture short-run dynamics. The estimates in Table 12 show that per-pupil education spending is systematically higher in commuting zones with 'stronger' local economies measured in local wages, industrial GDP, and house prices.

Lagged economic indicators, particularly private industry GDP and average weekly wages, are positively and significantly associated with education spending. In the baseline specification (Column 1), the elasticity of education spending with respect to local GDP per capita (private industry) is positive and statistically significant once lagged values are included.

A ten-percent increase in local GDP per capita two years prior is associated with roughly a 1% increase in current education expenditure per pupil, suggesting that fiscal capacity effects unfold gradually over time. In the case of industry GDP, the magnitude of the coefficients increases with the number of lags, suggesting a gradual adjustment process by which local economic growth translates into higher public investment in education over time. For example, a 10% increase in lagged (t–2) real private GDP per capita is associated with a 0.6% increase in per-pupil spending. The house price index also enters positively and significantly but only contemporaneously reflecting the immediate link between house prices and the property taxes that fund public education. This points to the fundamental relationship between community asset wealth and public education expenditure.

Intergovernmental revenue per pupil emerges as the strongest and most consistent predictor of education expenditure after the auto-regressive coefficient. A 10% increase in intergovernmental transfers is associated with approximately a 2% increase in per-pupil education spending, controlling for CZ and year fixed effects. This finding highlights the importance of state and federal aid in sustaining local education budgets.

The growth rate regressions, while explaining less variance overall, largely confirm the patterns observed in the level specifications. Intergovernmental revenue growth remains a strong and highly significant determinant of education expenditure growth. Lagged wage and GDP growth also emerge as important predictors, particularly at longer lags. Notably, wage growth two years prior is associated with a 0.31% increase in education spending growth, suggesting that labor market improvements take at least a year to materialize in local education budgets hinting at the relevance of our primary identifying relationship.

Taken together, these results offer three key insights. First, public education investment is strongly mediated by external fiscal flows, reaffirming the role of intergovernmental transfers in equalizing local education finance. Second, local labor market conditions, captured through wages and GDP, exert lagged effects on education spending consistent with lagged effects of local economic conditions to industrial change. Third, local housing markets play a significant role shaping education budgets, reflecting the link between property values and tax revenues which respond contemporaneously as a result of the direct mechanical link between property values and local public revenue generation.

Additionally, in both levels and growth rates, the consistently negative coefficient on enrollment indicates a scaling relationship in which expenditure per pupil declines as enrollment sizes grow.

Table 10: Descriptive Results in Levels

Dependent Variable:	(log) Elem.Ed.Exp.pp			
Model:	(1)	(2)	(3)	(4)
Variables				
(log) Real GDP Priv. Industry pc	0.0135			0.0092
	(0.0152)			(0.0164)
(log,l1) Real GDP Priv. Industry pc	0.0519***			0.0345**
	(0.0155)			(0.0173)
(log,l2) Real GDP Priv. Industry pc	0.0578***			0.0492***
	(0.0138)			(0.0128)
(l1, log) Elem.Ed.Exp.pp	0.5069***	0.5281***	0.5315***	0.4920***
	(0.0142)	(0.0176)	(0.0178)	(0.0145)
(log) IG Revenue pp	0.2352***	0.2088***	0.2088***	0.2332***
	(0.0204)	(0.0229)	(0.0219)	(0.0204)
(log) Enrollment	-0.1839***	-0.1999***	-0.2174***	-0.2153***
	(0.0149)	(0.0145)	(0.0162)	(0.0176)
% Black	0.2051	0.2227	0.2817*	0.1950
	(0.1834)	(0.1724)	(0.1689)	(0.1810)
% Hispanic	0.0548	0.1131	0.1957	0.0044
	(0.1436)	(0.1470)	(0.1280)	(0.1355)
(log) Annual Avg. Wkly. Wage		0.0639		-0.0309
		(0.0491)		(0.0614)
(log, l1) Annual Avg. Wkly. Wage		0.1926***		0.1442**
		(0.0547)		(0.0612)
(log, l2) Annual Avg. Wkly. Wage		0.0512		-0.0420
		(0.0484)		(0.0453)
(log) House Price Index			0.1098***	0.0708***
			(0.0208)	(0.0202)
(log, l1) House Price Index			0.0294	0.0132
			(0.0325)	(0.0348)
(log, l2) House Price Index			-0.0020	-0.0054
			(0.0212)	(0.0225)
Fixed-effects				
unit	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	12,084	12,720	12,078	11,493
R^2	0.90689	0.90577	0.90936	0.91151
Within R^2	0.52075	0.52326	0.52459	0.52726

Clustered (unit) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 11: Descriptive Results in Growth Rates

Dependent Variable:	(GR) Elem.Ed.Exp.pp			
Model:	(1)	(2)	(3)	(4)
Variables				
(GR) Real GDP Priv. Industry pc	0.0047			-0.0127
	(0.0137)			(0.0152)
(GR,l1) Real GDP Priv. Industry pc	0.0508***			0.0249
	(0.0148)			(0.0154)
(GR,l2) Real GDP Priv. Industry pc	0.0184***			0.0104*
	(0.0071)			(0.0054)
(GR) IG Revenue pp	0.3064***	0.3275***	0.3321***	0.3013***
	(0.0321)	(0.0223)	(0.0225)	(0.0317)
(GR) Enrollment	-0.5994***	-0.0146**	-0.0079	-0.6194***
	(0.0421)	(0.0064)	(0.0067)	(0.0465)
% Black	0.0950	0.0236	0.0261	0.1351
	(0.1327)	(0.2001)	(0.1954)	(0.1213)
% Hispanic	-0.1428*	-0.2636**	-0.2558**	-0.1754***
	(0.0796)	(0.1084)	(0.1118)	(0.0600)
(GR) Annual Avg. Wkly. Wage		-0.0317		0.0071
		(0.0547)		(0.0528)
(GR, l1) Annual Avg. Wkly. Wage		0.2021***		0.1229**
		(0.0500)		(0.0499)
(GR, l2) Annual Avg. Wkly. Wage		0.3087***		0.2357***
		(0.0600)		(0.0524)
(GR) House Price Index			0.0515**	0.0774***
			(0.0240)	(0.0215)
(GR, l1) House Price Index			0.1125***	0.0572**
			(0.0280)	(0.0260)
(GR, l2) House Price Index			0.0669***	0.0517***
			(0.0198)	(0.0197)
Fixed-effects				
unit	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	12,083	13,355	12,637	11,467
R^2	0.26823	0.35172	0.36223	0.28775
Within R^2	0.22116	0.15439	0.15065	0.23614

Clustered (unit) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

C.1 Including State Fixed Effects

Regressions establishing baseline relationships between local economic variables and elementary education expenditure using state-fixed effects rather than commuting-zone level effects.

Table 12: Descriptive Results in Levels

Dependent Variable:	(log) Elem.Ed.Exp.pp			
Model:	(1)	(2)	(3)	(4)
Variables				
(log) Real GDP Priv. Industry pc	0.0096			-0.0011
	(0.0147)			(0.0158)
(log,l1) Real GDP Priv. Industry pc	0.0401**			0.0333*
	(0.0186)			(0.0202)
(log,l2) Real GDP Priv. Industry pc	0.0049			0.0122
	(0.0134)			(0.0145)
(l1, log) Elem.Ed.Exp.pp	0.7681***	0.7920***	0.8039***	0.7568***
	(0.0118)	(0.0144)	(0.0121)	(0.0118)
(log) IG Revenue pp	0.0965***	0.0733***	0.0714***	0.0963***
	(0.0091)	(0.0080)	(0.0087)	(0.0092)
(log) Enrollment	-0.0074***	-0.0142***	-0.0083***	-0.0117***
	(0.0011)	(0.0014)	(0.0013)	(0.0015)
% Black	-0.0048	-0.0100	0.0125	0.0015
	(0.0124)	(0.0120)	(0.0106)	(0.0120)
% Hispanic	-0.0348**	0.0085	-0.0105	-0.0379***
	(0.0139)	(0.0153)	(0.0148)	(0.0127)
(log) Annual Avg. Wkly. Wage		0.0453		-0.0163
		(0.0489)		(0.0543)
(log, l1) Annual Avg. Wkly. Wage		0.2028***		0.1518**
		(0.0621)		(0.0708)
(log, l2) Annual Avg. Wkly. Wage		-0.1284***		-0.1167***
		(0.0405)		(0.0438)
(log) House Price Index			0.1031***	0.0853***
			(0.0210)	(0.0217)
(log, l1) House Price Index			0.0125	0.0071
			(0.0380)	(0.0403)
(log, l2) House Price Index			-0.0780***	-0.0607***
			(0.0221)	(0.0234)
Fixed-effects				
state	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	12,084	12,720	12,078	11,493
R^2	0.88140	0.88108	0.88464	0.88697
Within R^2	0.74455	0.74269	0.71388	0.72072

Clustered (unit) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 13: Descriptive Results in Growth Rates

Dependent Variable:	(GR) Elem.Ed.Exp.pp			
Model:	(1)	(2)	(3)	(4)
Variables				
(GR) Real GDP Priv. Industry pc	0.0084			-0.0098
	(0.0134)			(0.0149)
(GR,l1) Real GDP Priv. Industry pc	0.0540***			0.0274*
	(0.0148)			(0.0152)
(GR,l2) Real GDP Priv. Industry pc	0.0197***			0.0116**
	(0.0070)			(0.0052)
(GR) IG Revenue pp	0.3086***	0.3259***	0.3299***	0.3042***
	(0.0317)	(0.0223)	(0.0226)	(0.0312)
(GR) Enrollment	-0.5759***	-0.0144**	-0.0080	-0.5952***
	(0.0398)	(0.0063)	(0.0067)	(0.0443)
% Black	-0.0104***	0.0138**	0.0138**	-0.0065*
	(0.0038)	(0.0066)	(0.0065)	(0.0038)
% Hispanic	0.0085*	-0.0014	-0.0063	0.0033
	(0.0048)	(0.0053)	(0.0050)	(0.0037)
(GR) Annual Avg. Wkly. Wage		-0.0253		0.0091
		(0.0544)		(0.0515)
(GR, l1) Annual Avg. Wkly. Wage		0.2088***		0.1225**
		(0.0494)		(0.0497)
(GR, l2) Annual Avg. Wkly. Wage		0.3112***		0.2320***
		(0.0592)		(0.0516)
(GR) House Price Index			0.0525**	0.0781***
			(0.0241)	(0.0213)
(GR, l1) House Price Index			0.1132***	0.0569**
			(0.0281)	(0.0260)
(GR, l2) House Price Index			0.0669***	0.0520***
			(0.0197)	(0.0196)
Fixed-effects				
state	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	12,083	13,355	12,637	11,467
R^2	0.26170	0.34062	0.35073	0.28153
Within R^2	0.21915	0.15393	0.14949	0.23417

Clustered (unit) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

C.2 Including State Funding Share Interaction Term

Furthermore, given the heterogeneity in reliance on intergovernmental transfers (largely coming from the state), we interact all economic predictors above with a variable that represents the share of total elementary education expenditure (as a continuous variable) coming from state-level funding.

Table 14: Descriptive Results with Funding Source Interaction Effects

Dependent Variable:	(log) Elem.Ed.Exp.pp				
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
(log) Real GDP Priv. Industry pc			-0.2219***		
			(0.0735)		
(log,l1) Real GDP Priv. Industry pc			0.1022*		
			(0.0585)		
(log,l2) Real GDP Priv. Industry pc			0.3272***		
			(0.0771)		
Funding Share_state × (log) Real GDP Priv. Industry pc			0.3946***		
			(0.1198)		
Funding Share_state × (log,l1) Real GDP Priv. Industry pc			-0.1113		
			(0.0941)		
Funding Share_state × (log,l2) Real GDP Priv. Industry pc			-0.4261***		
			(0.1176)		
(log) Annual Avg. Wkly. Wage	0.0031			0.0031	
	(0.2218)			(0.2218)	
(log, l1) Annual Avg. Wkly. Wage	0.2176			0.2176	
	(0.1487)			(0.1487)	
(log, l2) Annual Avg. Wkly. Wage	0.4606**			0.4606**	
	(0.2232)			(0.2232)	
Funding Share_state × (log) Annual Avg. Wkly. Wage	0.4692			0.4692	
	(0.3484)			(0.3484)	
Funding Share_state × (log, l1) Annual Avg. Wkly. Wage	-0.0685			-0.0685	
	(0.2359)			(0.2359)	
Funding Share_state × (log, l2) Annual Avg. Wkly. Wage	-0.5923*			-0.5923*	
	(0.3508)			(0.3508)	
(log) House Price Index		-0.0741			-0.0741
		(0.1037)			(0.1037)
(log, l1) House Price Index		0.1510			0.1510
		(0.1316)			(0.1316)
(log, l2) House Price Index		0.3736***			0.3736***
		(0.1009)			(0.1009)
(log, l3) House Price Index		0.0015			0.0015
		(0.1208)			(0.1208)
(log, l4) House Price Index		-0.1466			-0.1466
		(0.0944)			(0.0944)
Funding Share_state × (log) House Price Index		0.4725***			0.4725***
		(0.1646)			(0.1646)
Funding Share_state × (log, l1) House Price Index		-0.1587			-0.1587
		(0.2008)			(0.2008)
Funding Share_state × (log, l2) House Price Index		-0.4995***			-0.4995***
		(0.1536)			(0.1536)
Funding Share_state × (log, l3) House Price Index		0.0354			0.0354
		(0.1879)			(0.1879)
Funding Share_state × (log, l4) House Price Index		0.1343			0.1343
		(0.1483)			(0.1483)
Funding Share_state	0.5937	-0.5972	0.8604	0.5937	-0.5972
	(0.4862)	(0.3661)	(0.5614)	(0.4862)	(0.3661)
(log) Fed IG Rev pp	-0.0014	-0.0018	-0.0030	-0.0014	-0.0018
	(0.0021)	(0.0020)	(0.0024)	(0.0021)	(0.0020)
(log) Enrollment	-0.3831***	-0.4186***	-0.3917***	-0.3831***	-0.4186***
	(0.0271)	(0.0280)	(0.0271)	(0.0271)	(0.0280)
% Black	1.366***	1.679***	1.323***	1.366***	1.679***
	(0.4114)	(0.4564)	(0.4891)	(0.4114)	(0.4564)
% Hispanic	0.4890**	0.6243***	0.8102***	0.4890**	0.6243***
	(0.2303)	(0.2072)	(0.2408)	(0.2303)	(0.2072)
Fixed-effects					
unit	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	13,356	12,588	12,084	13,356	12,588
R^2	0.86011	0.87031	0.86240	0.86011	0.87031
Within R^2	0.29630	0.32234	0.29180	0.29630	0.32234

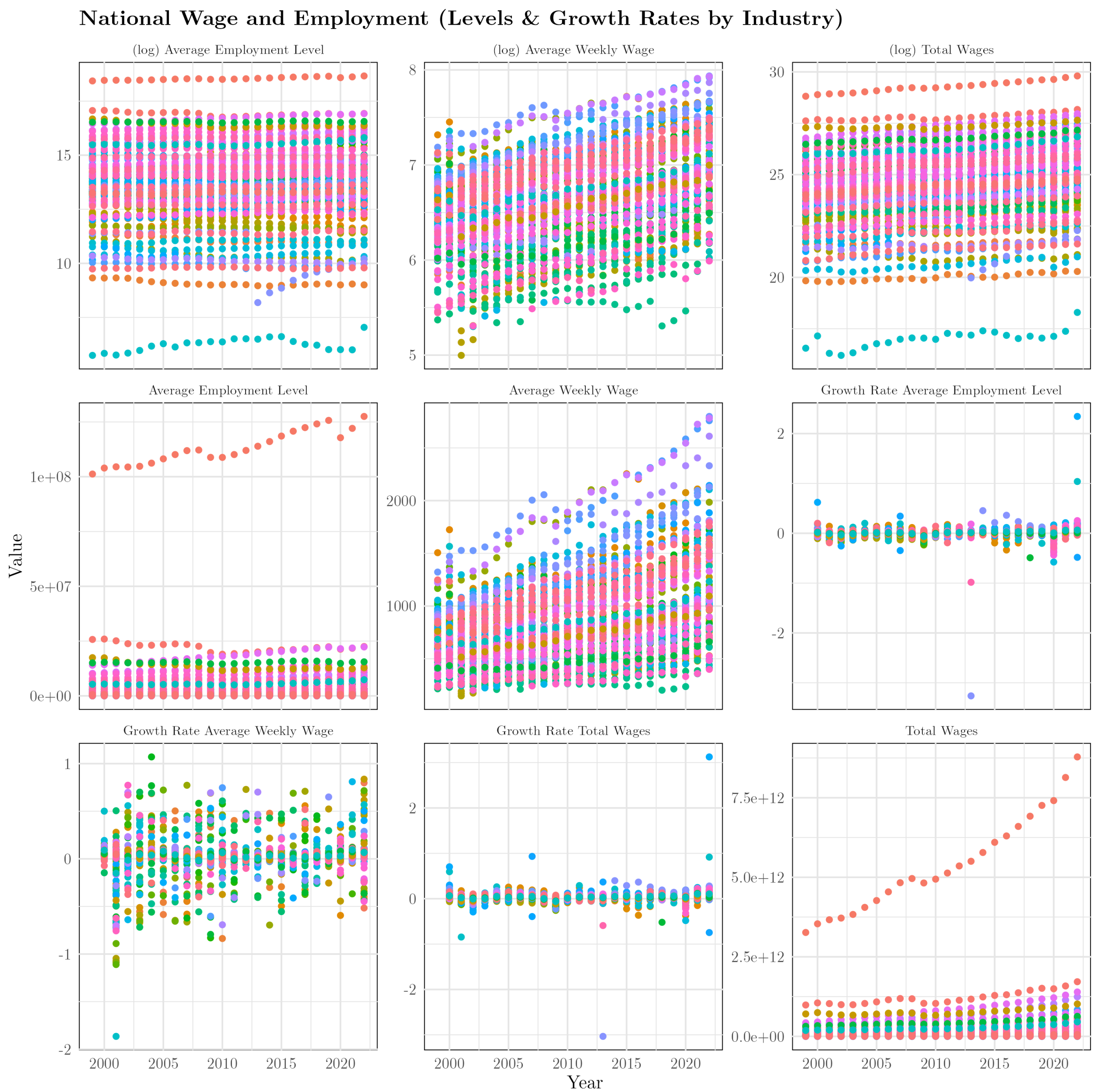
Clustered (unit) standard-errors in parentheses

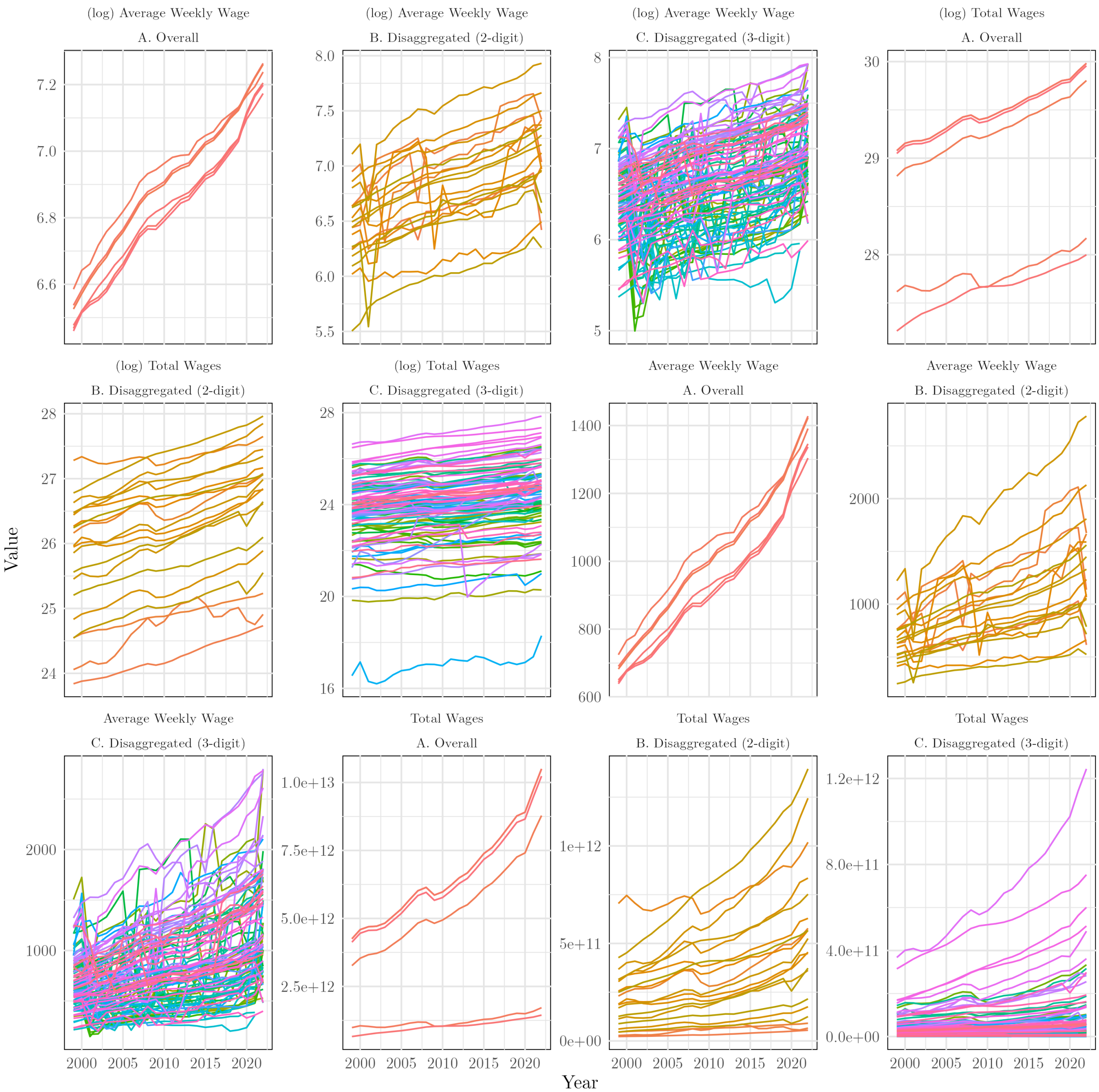
D Shift-Share Instrument

The following section provides further detail on the shift-share instrument as well as supplementary regressions estimated using a wage-based shift share instrument rather than the value-added based shift-share instrument in the main text. We use the abbreviation `SS` to refer to the shift-share instrument.

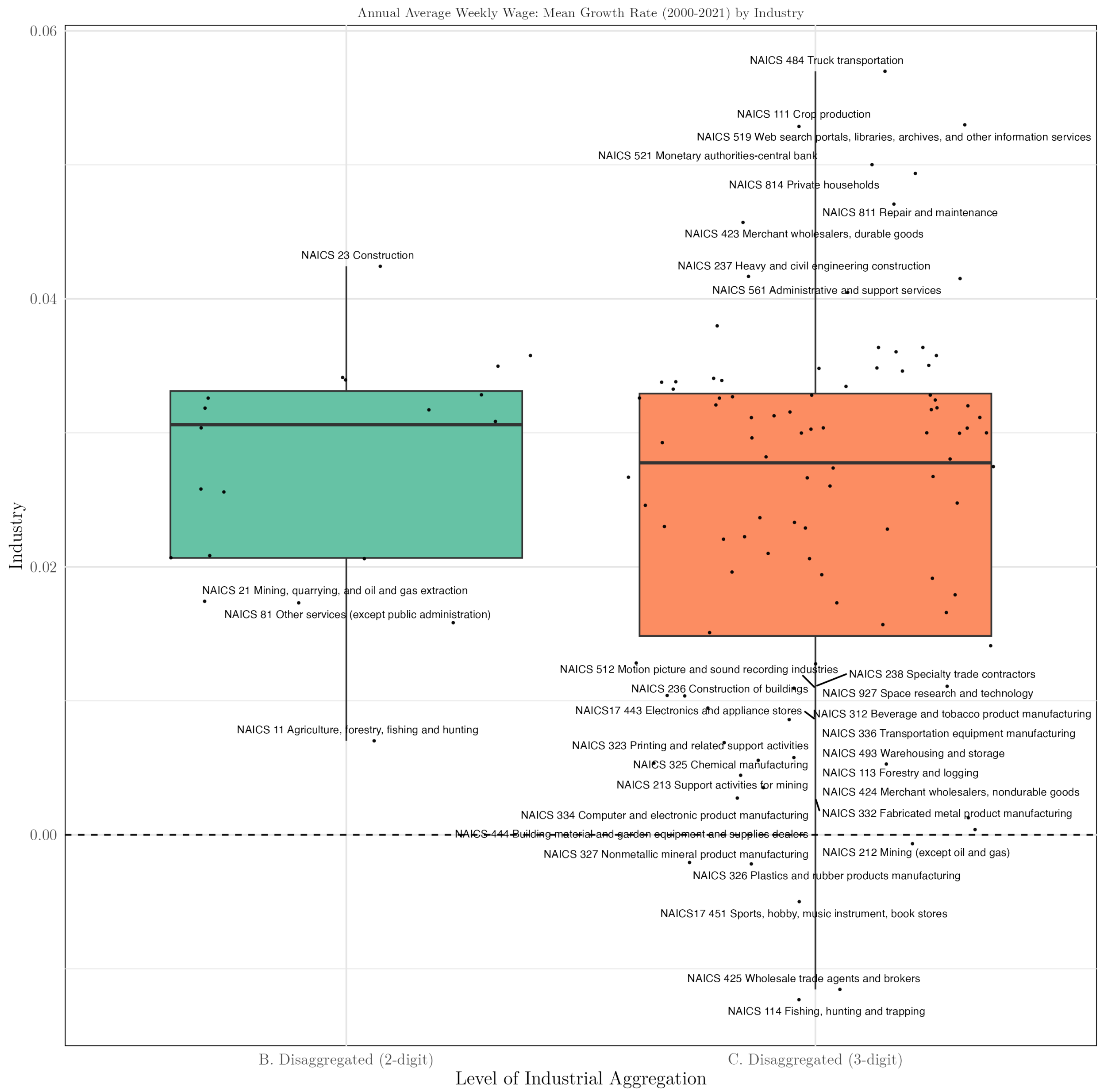
D.1 Data Inputs to the SS Instrument

Plots of the data inputs to the shift-share instrument. This inputs include national and county-level employment and average weekly wage levels by 2-digit NAICS industry code.





(log) Average Weekly Wage
A. Overall
(log) Average Weekly Wage
B. Disaggregated (2-digit)
(log) Average Weekly Wage
C. Disaggregated (3-digit)
(log) Total Wages
A. Overall
(log) Total Wages
B. Disaggregated (2-digit)
(log) Total Wages
C. Disaggregated (3-digit)
Average Weekly Wage
A. Overall
Average Weekly Wage
B. Disaggregated (2-digit)
Average Weekly Wage
C. Disaggregated (3-digit)
Total Wages
A. Overall
Total Wages
B. Disaggregated (2-digit)
Total Wages
C. Disaggregated (3-digit)
Value
Year



D.2 Results Using the Wage-based Shift-Share Instrument

First, we display the results for a 2SLS estimation using our wage-based shift-share instrument.

The instrumental variable estimates provide similar evidence of a robust causal relationship between local wages and public education expenditure. Utilising our wage-based shift-share instrument we see highly significant and relevant first-stage relationships when the shift-share instrument is imposed in levels (except in column 5). In each case except columns 5-6 (l1 SS, CZ FE), the first-stage regression yields a statistically significant and economically large coefficient. Varying the time-lag and inclusion of state or commuting zone fixed effects, we see that a 10% increase in the shift-share measure (which can be interpreted as a natural logarithm) is associated with a 2-6% increase in average weekly wages ($p < 0.01$), with an F-statistic well above conventional weak instrument thresholds confirming instrument relevance. The Wu-Hausman tests reject the null of exogeneity, confirming that OLS estimates are biased and IV estimation is appropriate. Wald tests of joint significance further support the strength of the instruments.

Using wage shocks in levels yields strong instruments, high first-stage F-statistics, and stable second-stage estimates: higher local wages robustly increase education spending. Furthermore, given the dependent variable measures per pupil expenditure, this result implies direct effects in experience per student. In contrast, when shocks are measured in growth rates, though the F-statistic remains strong, the second-stage coefficients become statistically insignificant. This suggests that the growth-rate specification is poorly identified and cannot provide reliable causal inference, whereas the level specification produces credible and consistent results.

The specification that uses wage shocks in levels provides the most credible identification strategy. Since levels capture the cross-sectional fiscal variation that drives differences in property values and school spending, the level specification is more consistent with the economic mechanisms of interest and delivers more reliable causal estimates. Simultaneously, the weakness of the growth-rate specification does raise concerns about the robustness of the results. If the relationship between wages, house prices, and education expenditure is driven by common non-stationary trends, then regressions in levels risk spurious correlation. The fact that the IV design loses power when variables are differenced into growth rates may suggest that part of the strong level results reflect long-run trends rather than short-run causal shocks. However, examining the structure of the growth rate shock, the instability of the variable is likely causing the poor identification in the growth rate regressions.

Table 15: IV Estimation Using Wage-based Shift-share instrument (l0, l1, l2) in Levels varying state and CZ fixed effects and lags.

Dependent Variables:	(log) Annual Avg. Wkly. Wage	(log) Elem.Ed.Exp.pp	(log) Annual Avg. Wkly. Wage	(log) Elem.Ed.Exp.pp	(GR) Annual Avg. Wkly. Wage	(GR) Elem.Ed.Exp.pp	(log) Annual Avg. Wkly. Wage	(log) House Price Index
IV stages	First	Second	First	Second	First	Second	First	Second
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
(log, l1) Annual Avg. Wkly. Wage	0.7838***		0.7860***				0.7802***	
	(0.0120)		(0.0112)				(0.0119)	
Wage SS (lvl)	0.0519*		0.0538**				0.0841***	
	(0.0274)		(0.0274)				(0.0274)	
Wage SS (lvl,l1)	-0.1690***		-0.1693***				-0.1824***	
	(0.0413)		(0.0414)				(0.0427)	
Wage SS (lvl,l2)	0.1063***		0.1056***				0.1079**	
	(0.0371)		(0.0371)				(0.0422)	
(l1, log) Elem.Ed.Exp.pp	0.0043	0.5085***						
	(0.0042)	(0.0156)						
(log) IG Revenue pp	0.0131***	0.2230***	0.0140***	0.3213***				
	(0.0030)	(0.0211)	(0.0028)	(0.0316)				
(log) Real GDP Priv. Industry pc	0.0388***	0.0660***	0.0392***	0.0948***			0.0406***	0.0379***
	(0.0044)	(0.0144)	(0.0044)	(0.0244)			(0.0060)	(0.0082)
(log) Enrollment	0.0096**	-0.1959***	0.0085**	-0.3306***				
	(0.0045)	(0.0157)	(0.0042)	(0.0282)				
% Black	-0.1761**	0.3337*	-0.1738**	0.6539*			-0.0993	-0.4357***
	(0.0713)	(0.1925)	(0.0707)	(0.3622)			(0.0726)	(0.1587)
% Hispanic	-0.0566*	0.0480	-0.0570*	0.0383			-0.0528	-0.0219
	(0.0335)	(0.1510)	(0.0334)	(0.2516)			(0.0361)	(0.0594)
(log) Annual Avg. Wkly. Wage		0.2288***		0.5610***				0.0885**
		(0.0480)		(0.0767)				(0.0404)
Wage SS (GR)					0.0481			
					(0.0312)			
Wage SS (GR,l1)					-0.1187**			
					(0.0466)			
Wage SS (GR,l2)					0.0145			
					(0.0347)			
(GR) IG Revenue pp					0.0029	0.3077***		
					(0.0028)	(0.0319)		
(GR) Real GDP Priv. Industry pc					0.0579***	0.0359		
					(0.0077)	(0.0404)		
(GR) Enrollment					0.0123*	-0.5901***		
					(0.0064)	(0.0420)		
fd_pct_black					-0.7066***	-0.8133		
					(0.2635)	(1.304)		
fd_pct_hispanic					0.3057	1.621*		
					(0.2976)	(0.8331)		
(GR) Annual Avg. Wkly. Wage						-0.6453		
						(0.6719)		
(log, l1) House Price Index							0.0134***	0.8376***
							(0.0049)	(0.0098)
Fixed-effects								
unit	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics								
Observations	12,084	12,084	12,084	12,084	12,084	12,084	11,509	11,509
R^2	0.99248	0.90637	0.99248	0.86538	0.29883	0.26592	0.99339	0.99074
Within R^2	0.77657	0.51811	0.77649	0.30709	0.04956	0.21870	0.78900	0.78570
Wu-Hausman		46.934		137.81		1.5265		113.27
Wu-Hausman, p-value		7.72×10^{-12}		1.22×10^{-31}		0.21666		2.54×10^{-26}
Wald (IV only)	1,262.2	22.696	1,454.0	53.437	5.0709	0.92252	1,235.9	4.8071
Wald (IV only), p-value	0×10^{-16}	1.92×10^{-6}	0×10^{-16}	2.84×10^{-13}	0.00165	0.33683	0×10^{-16}	0.02836
F-test (1st stage)	5,986.0		6,275.1		15.760		5,034.1	
F-test (1st stage), (log) Annual Avg. Wkly. Wage		5,986.0		6,275.1				5,034.1
F-test (1st stage), (GR) Annual Avg. Wkly. Wage						15.760		
F-test (1st stage), p-value	0×10^{-16}		0×10^{-16}		3.17×10^{-10}		0×10^{-16}	
F-test (1st stage), p-value, (log) Annual Avg. Wkly. Wage		0×10^{-16}		0×10^{-16}				0×10^{-16}
F-test (1st stage), p-value, (GR) Annual Avg. Wkly. Wage						3.17×10^{-10}		

Clustered (unit) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

E Growth Rate Sample Partitioning

In the following section, we provide supplementary information and econometric estimation details and results not included in the main text pertaining to the sample partitioning exercise by commuting zone level growth rates.

E.1 Baseline Models for Growth Rate Sample Partitioning

Table 16: Baseline Regression Applied to Declining GDP vs. Growing GDP Regions

Dependent Variable:	(log) Elem.Ed.Exp.pp											
	Declining			Hyper-Declining			Growing			Hyper-Growing		
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
(log) Real GDP Priv. Industry pc	0.0687**			0.0700**			0.0009			-0.0088		
	(0.0278)			(0.0312)			(0.0169)			(0.0209)		
(log,l1) Real GDP Priv. Industry pc	-0.0095			-0.0165			0.0626***			0.0650***		
	(0.0337)			(0.0376)			(0.0163)			(0.0201)		
(log,l2) Real GDP Priv. Industry pc	0.0389			0.0354			0.0599***			0.0853***		
	(0.0245)			(0.0275)			(0.0157)			(0.0183)		
(l1, log) Elem.Ed.Exp.pp	0.5433***	0.5352***	0.5443***	0.5593***	0.5484***	0.5552***	0.4856***	0.5182***	0.5147***	0.4714***	0.5174***	0.5283***
	(0.0194)	(0.0203)	(0.0206)	(0.0207)	(0.0226)	(0.0227)	(0.0192)	(0.0249)	(0.0261)	(0.0244)	(0.0320)	(0.0389)
(log) IG Revenue pp	0.2628***	0.2579***	0.2374***	0.2339***	0.2302***	0.2097***	0.2233***	0.1872***	0.1953***	0.2126***	0.1638***	0.1595***
	(0.0337)	(0.0339)	(0.0342)	(0.0394)	(0.0396)	(0.0397)	(0.0263)	(0.0303)	(0.0285)	(0.0374)	(0.0420)	(0.0422)
(log) Enrollment	-0.1758***	-0.1860***	-0.2082***	-0.1880***	-0.2013***	-0.2241***	-0.1925***	-0.2083***	-0.2167***	-0.2249***	-0.2451***	-0.2574***
	(0.0219)	(0.0216)	(0.0240)	(0.0308)	(0.0294)	(0.0342)	(0.0200)	(0.0199)	(0.0231)	(0.0291)	(0.0293)	(0.0433)
% Black	0.0411	0.1851	0.3205*	0.0614	0.2138	0.3777	0.4372	0.3804	0.3402	0.3804	0.1681	0.9634
	(0.1798)	(0.1818)	(0.1857)	(0.1868)	(0.2196)	(0.2297)	(0.3310)	(0.3130)	(0.2928)	(0.7297)	(0.7498)	(0.8856)
% Hispanic	0.0388	0.0430	0.0669	-0.0980	-0.0225	-0.0291	0.0587	0.1315	0.2251	-0.0259	0.0529	0.2028
	(0.1585)	(0.1519)	(0.1536)	(0.2051)	(0.1967)	(0.2004)	(0.1862)	(0.1913)	(0.1684)	(0.2217)	(0.2169)	(0.2181)
(log) Annual Avg. Wkly. Wage		0.0678			0.0704			0.0572			0.0114	
		(0.0698)			(0.0830)			(0.0614)			(0.1024)	
(log, l1) Annual Avg. Wkly. Wage		0.1269			0.1422			0.1973***			0.2848***	
		(0.0997)			(0.1357)			(0.0651)			(0.1050)	
(log, l2) Annual Avg. Wkly. Wage		0.1080			0.0728			0.0267			0.0583	
		(0.0835)			(0.1129)			(0.0608)			(0.0923)	
(log) House Price Index			0.0835***			0.0595**			0.1129***			0.1402***
			(0.0219)			(0.0285)			(0.0280)			(0.0498)
(log, l1) House Price Index			0.0629*			0.1333***			0.0116			0.0210
			(0.0326)			(0.0418)			(0.0419)			(0.0691)
(log, l2) House Price Index			-0.0298			-0.0642**			-0.0004			-0.0208
			(0.0240)			(0.0300)			(0.0287)			(0.0450)
Fixed-effects												
unit	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	5,016	5,280	5,239	3,021	3,180	3,139	7,068	7,440	6,839	3,021	3,180	2,727
R^2	0.90665	0.90781	0.90943	0.90254	0.90417	0.90691	0.90511	0.90325	0.90886	0.86162	0.85654	0.85323
Within R^2	0.55110	0.56615	0.57079	0.53705	0.55560	0.56306	0.48702	0.48202	0.47095	0.49494	0.47612	0.44737

Clustered (unit) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 17: Baseline Regression Applied to Declining Wage vs. Growing Wage Regions

Dependent Variable:	(log) Elem.Ed.Exp.pp											
		Declining			Hyper-Declining			Growing			Hyper-Growing	
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
(log) Real GDP Priv. Industry pc	0.0187			0.0133			0.0115			-0.0175		
	(0.0489)			(0.0339)			(0.0159)			(0.0202)		
(log,l1) Real GDP Priv. Industry pc	0.1233***			0.1210***			0.0454***			0.0499**		
	(0.0458)			(0.0301)			(0.0161)			(0.0215)		
(log,l2) Real GDP Priv. Industry pc	-0.0471			-0.0521			0.0684***			0.0851***		
	(0.0418)			(0.0329)			(0.0145)			(0.0167)		
(l1, log) Elem.Ed.Exp.pp	0.5756***	0.5669***	0.5945***	0.5294***	0.5271***	0.5290***	0.4968***	0.5226***	0.5213***	0.5044***	0.5666***	0.5604***
	(0.0319)	(0.0329)	(0.0340)	(0.0259)	(0.0256)	(0.0267)	(0.0155)	(0.0197)	(0.0200)	(0.0246)	(0.0341)	(0.0406)
(log) IG Revenue pp	0.2190***	0.2047***	0.1876***	0.2848***	0.2718***	0.2511***	0.2373***	0.2082***	0.2116***	0.1770***	0.1274***	0.1293***
	(0.0319)	(0.0288)	(0.0286)	(0.0335)	(0.0313)	(0.0325)	(0.0230)	(0.0260)	(0.0253)	(0.0354)	(0.0426)	(0.0341)
(log) Enrollment	-0.1708***	-0.1951***	-0.2233***	-0.1994***	-0.2192***	-0.2418***	-0.1854***	-0.1999***	-0.2158***	-0.2016***	-0.2099***	-0.2083***
	(0.0325)	(0.0307)	(0.0348)	(0.0242)	(0.0227)	(0.0249)	(0.0168)	(0.0163)	(0.0182)	(0.0313)	(0.0327)	(0.0432)
% Black	-0.0444	0.0683	-0.0895	0.0416	0.1238	0.0038	0.2672	0.2714	0.3569*	0.9080*	0.6110	1.245**
	(0.3023)	(0.2677)	(0.2635)	(0.2515)	(0.2241)	(0.1938)	(0.2191)	(0.2107)	(0.2037)	(0.5066)	(0.5661)	(0.6130)
% Hispanic	0.2651	0.2707	0.2915*	0.2226	0.2715	0.0073	0.0197	0.0899	0.1916	0.1892	0.1584	0.5310*
	(0.2040)	(0.1664)	(0.1728)	(0.2298)	(0.2274)	(0.1727)	(0.1696)	(0.1756)	(0.1534)	(0.2892)	(0.3254)	(0.3137)
(log) Annual Avg. Wkly. Wage		0.0631			0.0670			0.0621			-0.0256	
		(0.1036)			(0.0823)			(0.0547)			(0.0831)	
(log, l1) Annual Avg. Wkly. Wage		0.2533			0.1561			0.1863***			0.2397**	
		(0.1712)			(0.0977)			(0.0577)			(0.0924)	
(log, l2) Annual Avg. Wkly. Wage		0.0179			0.0925			0.0556			0.0526	
		(0.1351)			(0.0879)			(0.0533)			(0.0826)	
(log) House Price Index			0.1038**			0.0644			0.1108***			0.1171**
			(0.0436)			(0.0406)			(0.0235)			(0.0502)
(log, l1) House Price Index			0.0555			0.1089			0.0235			-0.0513
			(0.0743)			(0.0704)			(0.0355)			(0.0716)
(log, l2) House Price Index			-0.0304			-0.0495			0.0047			0.0356
			(0.0485)			(0.0431)			(0.0235)			(0.0450)
Fixed-effects												
unit	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	1,520	1,600	1,520	3,021	3,180	3,058	10,564	11,120	10,558	3,021	3,180	2,788
R^2	0.94127	0.94215	0.94235	0.92038	0.92081	0.92209	0.89898	0.89729	0.90151	0.90426	0.89961	0.90309
Within R^2	0.58355	0.60273	0.61588	0.56824	0.57899	0.59033	0.51202	0.51130	0.50968	0.51719	0.50212	0.45257

Clustered (unit) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

E.2 Wage-based SS Instrument for Growth Rate Sample Partitioning

When applying our instrumental variable design we find that the signal is consistent across various GDP growth trajectories, but the relationship weakens as communities are defined by higher rates of wage growth.

Dependent Variable:	(log) Elem.Ed.Exp.pp				
	All	Hyper-Declining (GDP)	Declining (GDP)	Growing (GDP)	Hyper-Growing (GDP)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
(log) Annual Avg. Wkly. Wage	0.2269***	0.2785***	0.2912***	0.1874***	0.2532***
	(0.0477)	(0.0737)	(0.0648)	(0.0595)	(0.0822)
(l1, log) Elem.Ed.Exp.pp	0.5086***	0.5430***	0.5286***	0.4937***	0.4893***
	(0.0156)	(0.0212)	(0.0193)	(0.0218)	(0.0282)
(log) IG Revenue pp	0.2230***	0.2290***	0.2575***	0.2086***	0.1911***
	(0.0211)	(0.0405)	(0.0345)	(0.0275)	(0.0397)
(log) Real GDP Priv. Industry pc	0.0662***	0.0378	0.0398	0.0714***	0.0759***
	(0.0144)	(0.0358)	(0.0320)	(0.0160)	(0.0172)
(log) Enrollment	-0.1957***	-0.2011***	-0.1911***	-0.2017***	-0.2353***
	(0.0157)	(0.0332)	(0.0238)	(0.0209)	(0.0295)
% Black	0.3333*	0.1517	0.1511	0.6266*	0.7367
	(0.1924)	(0.2226)	(0.1964)	(0.3421)	(0.7621)
% Hispanic	0.0483	-0.1417	-0.0061	0.0687	-0.0070
	(0.1509)	(0.2286)	(0.1702)	(0.1938)	(0.2165)
Fixed-effects					
unit	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	12,084	3,021	5,016	7,068	3,021
R^2	0.90636	0.90369	0.90765	0.90377	0.85786
Within R^2	0.51804	0.54248	0.55592	0.47980	0.48120
Wu-Hausman	44.905	10.070	18.286	20.577	22.401
Wu-Hausman, p-value	2.17×10^{-11}	0.00152	1.94×10^{-5}	5.83×10^{-6}	2.32×10^{-6}
Wald (IV only)	22.645	14.299	20.216	9.9366	9.5022
Wald (IV only), p-value	1.97×10^{-6}	0.00016	7.07×10^{-6}	0.00163	0.00207
F-test (1st stage), (log) Annual Avg. Wkly. Wage	5,972.4	1,846.6	2,651.0	3,287.8	1,501.4
F-test (1st stage), p-value, (log) Annual Avg. Wkly. Wage	0×10^{-16}	0×10^{-16}	0×10^{-16}	0×10^{-16}	0×10^{-16}

Clustered (unit) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Dependent Variable:	(log) Elem.Ed.Exp.pp				
	All	Hyper-Declining (Wage)	Declining (Wage)	Growing (Wage)	Hyper-Growing (Wage)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
(log) Annual Avg. Wkly. Wage	0.2269***	0.3277***	0.3394***	0.2128***	0.1551*
	(0.0477)	(0.0683)	(0.0861)	(0.0527)	(0.0830)
(l1, log) Elem.Ed.Exp.pp	0.5086***	0.5148***	0.5543***	0.5019***	0.5311***
	(0.0156)	(0.0267)	(0.0332)	(0.0173)	(0.0290)
(log) IG Revenue pp	0.2230***	0.2800***	0.2101***	0.2245***	0.1553***
	(0.0211)	(0.0341)	(0.0317)	(0.0239)	(0.0391)
(log) Real GDP Priv. Industry pc	0.0662***	0.0156	0.0220	0.0703***	0.0685***
	(0.0144)	(0.0238)	(0.0313)	(0.0150)	(0.0160)
(log) Enrollment	-0.1957***	-0.2269***	-0.1995***	-0.1941***	-0.2001***
	(0.0157)	(0.0254)	(0.0351)	(0.0175)	(0.0332)
% Black	0.3333*	0.2172	0.2137	0.3892*	1.193**
	(0.1924)	(0.2664)	(0.3680)	(0.2294)	(0.5557)
% Hispanic	0.0483	0.2597	0.2509	0.0110	0.1335
	(0.1509)	(0.2566)	(0.1897)	(0.1781)	(0.3037)
Fixed-effects					
unit	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	12,084	3,021	1,520	10,564	3,021
R^2	0.90636	0.92102	0.94210	0.89805	0.90098
Within R^2	0.51804	0.57170	0.58942	0.50752	0.50064
Wu-Hausman	44.905	12.495	8.9646	36.833	18.989
Wu-Hausman, p-value	2.17×10^{-11}	0.00041	0.00280	1.33×10^{-9}	1.36×10^{-5}
Wald (IV only)	22.645	23.023	15.549	16.276	3.4973
Wald (IV only), p-value	1.97×10^{-6}	1.68×10^{-6}	8.41×10^{-5}	5.52×10^{-5}	0.06157
F-test (1st stage), (log) Annual Avg. Wkly. Wage	5,972.4	1,354.5	813.43	5,156.1	1,214.4
F-test (1st stage), p-value, (log) Annual Avg. Wkly. Wage	0×10^{-16}	0×10^{-16}	0×10^{-16}	0×10^{-16}	0×10^{-16}

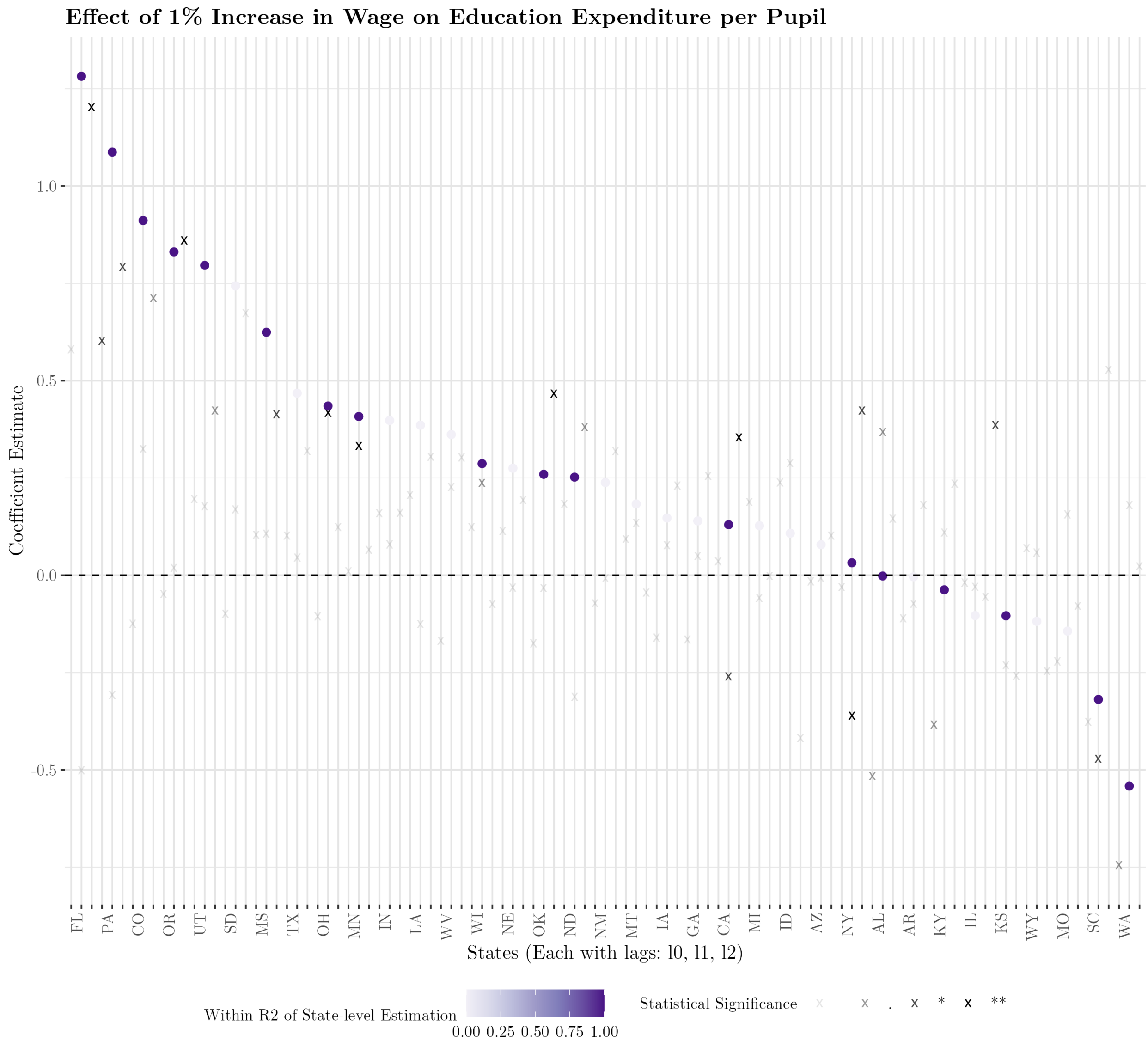
Clustered (unit) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

F State-by-State Estimation

In the following section, we provide additional detail on the estimated state-by-state models outlined in the main text. We also provide descriptive regression results as benchmarks for comparison with the instrumental variable approach.

F.1 Wage Effect Using Descriptive Estimation Strategy

We report the effect of a 1% increase in Wage on education expenditure per pupil as defined in Equation 7 where $X_{i,t}$ takes on the values in Equation 9. First, we re-employ our baseline regression in which we regress our outcome variable on wage levels at time $t-h$ where $h=[0,2]$. The plot below displays the cumulative effect of a 1% increase in wage levels on education expenditure per pupil in purple. The X marks represent the individual coefficients on each time lag of the treatment variable, the linear combination of which form the total dynamic effect represented by the purple dots. Majority of states see positive elasticities in relation to wage changes in this descriptive specification, but some (South Carolina, Missouri, Kentucky, Washington) see negative elasticities.



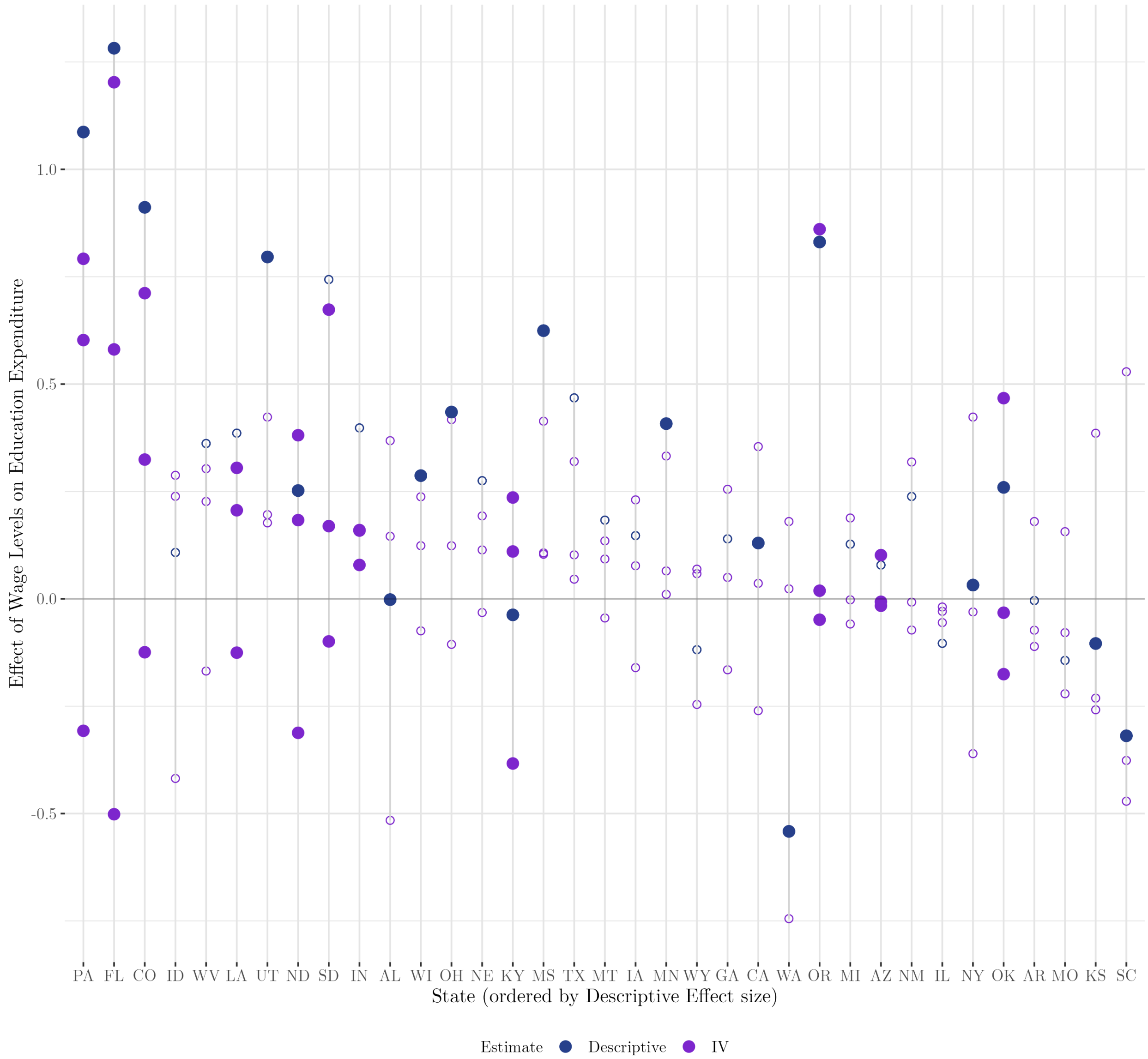
Effect of contemporaneous, l1, l2 lag of (log) average weekly wage on per pupil Education Expenditure controlling for enrollment, GDPpc, and intergovernmental transfers.
Each state has 3 lagged wage coefficients (l0, l1, l2).
X's represent coefficients on each time lag.
Purple dots show total effect across lags, plotted at the l1 position.
Within R^2 of state-level estimation reflected in color of point).

F.2 Instrumental Variable versus Descriptive Relationship

The below plots displays the descriptive regression estimate compared to the IV fitted regression coefficient, describing the effect of a 1% increase in local wages on education expenditure per pupil. Majority of states find at least a coefficient of equal sign. Interestingly though, the valid and statistically significant instrumental variable coefficients only ever identify positive elasticities.

Difference between Descriptive and IV Estimates by State

Filled blue circles represent the statistically significant estimate in IV regressions for DE, MO, OH, AZ, WA, NH.
We exclude MS as the statistically significant regression result is unreliable.

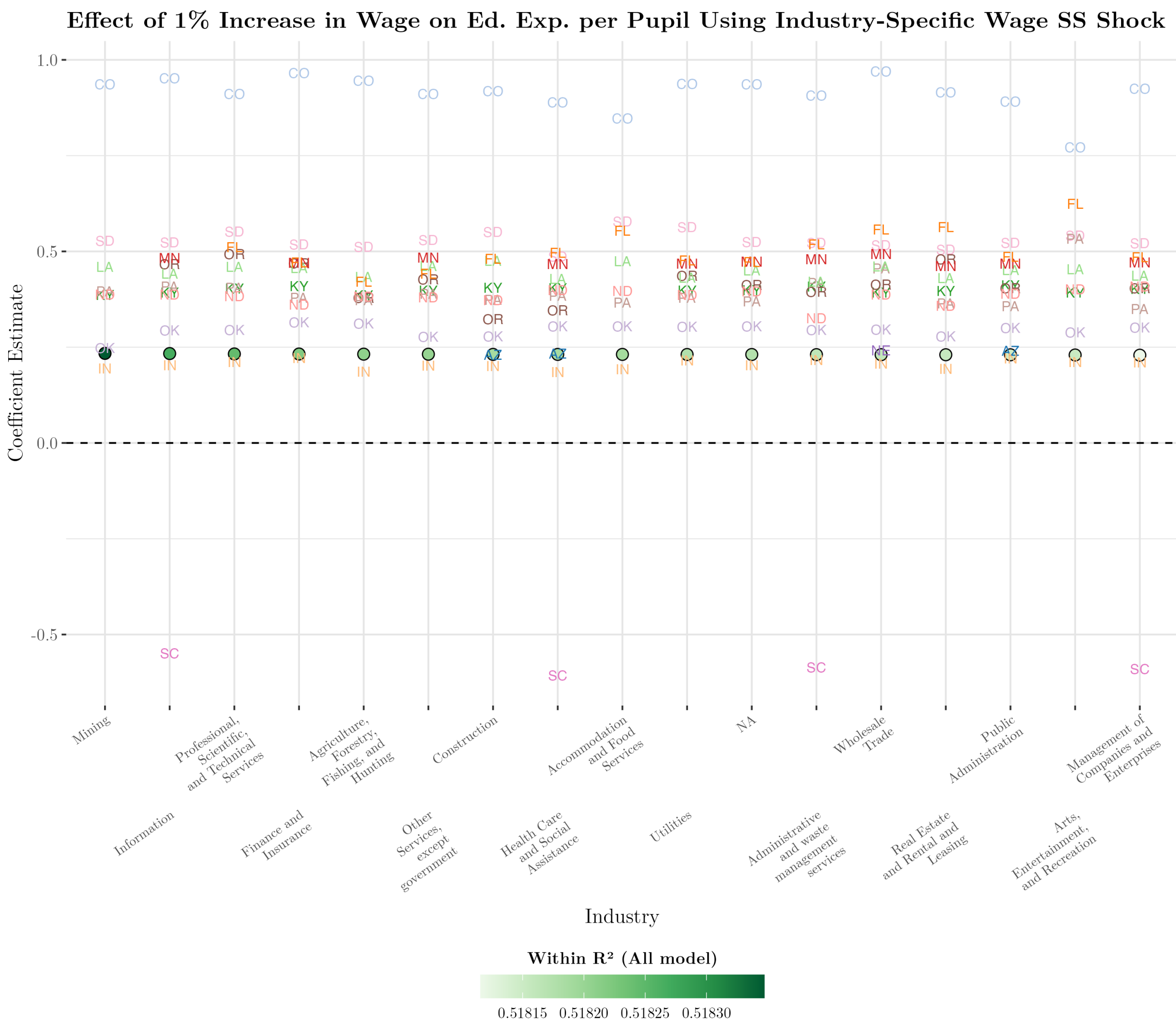


G Industry-by-Industry Estimation

In the following section, we provide additional detail about the estimated industry-by-industry models in the main text.

G.1 Wage-based SS in Industry-by-Industry Estimation

Using our wage-based shift share instrument, the below demonstrates the overall treatment effect of local wage changes instrumented via our wage-based shift-share shock.



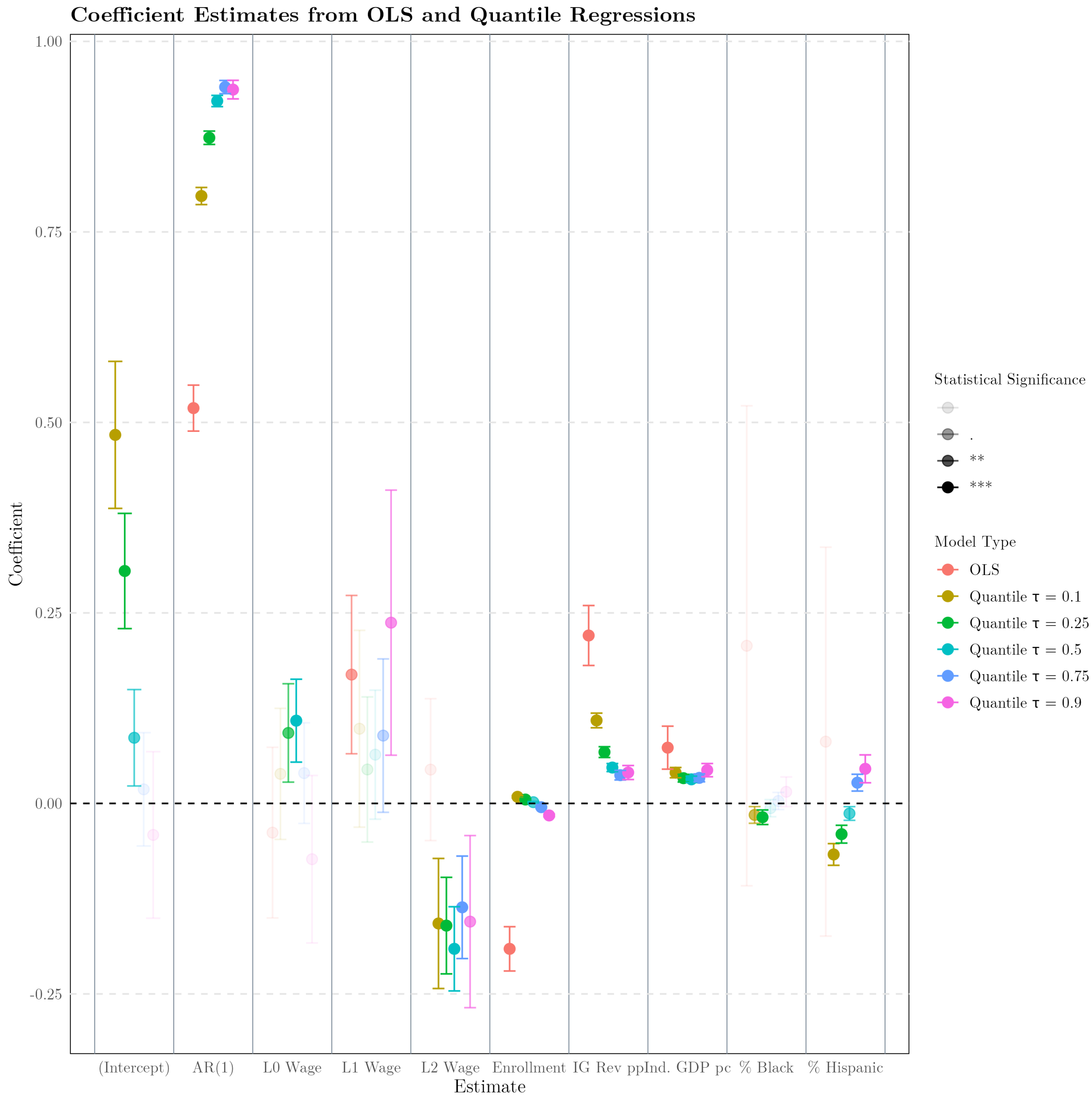
Effect of 1% increase in Wage Using Industry-Specific Wage (shift-share) Shocks. Controls: enrollment, GDP, intergovernmental transfers, year & CZ FE.
Within R^2 of 'All' estimation in point color.
X reflects statistically significant state-specific treatment effects with CZ FE.

H Robustness Checks and Additional Analyses

In the following section, we report various robustness checks and additional analyses conducted to illuminate patterns in the data and additional heterogeneity. These include additional panel econometric estimations via quantile regressions, vector auto regressions, and some outlier testing.

H.1 Quantile Regression

We perform a comparative quantile regression, where commuting zones are grouped by their levels of per pupil expenditure. We group them into the 10th, 25th, median, 75th, and 90th percentiles. We find that all coefficients are relatively stable apart from the relationship between per pupil expenditure and race, where the regression coefficient notably switches sign from negative to positive as the level of expenditure increases. Additionally, examining the central relationship of interest in this work, we find that the effect of wages on expenditure is significant contemporaneously in those commuting zones near the median level of expenditure, whereas those spending a relatively high amount of resources on local education, see a delayed effect.



H.2 High-Income Outliers

There is a somewhat non-linear relationship between property taxes and elementary expenditure as property taxes collected rise as represented in the figure below. This happens largely as a result of very high-income commuting zones. Therefore, we exclude any commuting zone that spends more than 22k per pupil to avoid any distorting effects. This removes 12 CZs (~2% of the sample). To assess whether the main results are driven by a small number of very high-income jurisdictions, I re-estimate the baseline and IV specifications excluding such outliers. The findings are fully consistent with the baseline analysis: house prices remain a strong predictor of local education spending, and the IV estimates continue to imply that a 10% increase in house prices raises per-pupil expenditure by roughly 4–6%. The wage-based shift-share instrument yields somewhat larger point estimates, though with wider standard errors, while the VA-based instrument produces effects in line with earlier results. Overall, this robustness exercise confirms that the causal relationship between housing wealth and education spending is not confined to affluent areas but reflects a broader, generalizable pattern. These findings confirm that the main result is not driven solely by affluent jurisdictions, but reflects a more general relationship between local housing wealth and education spending.

Dependent Variable:	(log) Elem.Ed.Exp.pp				
	Baseline 1	Baseline 2	Baseline 3	Wage-based SS	VA-based SS
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
(log) Real GDP Priv. Industry pc	0.0263**			0.0429***	0.0434***
	(0.0126)			(0.0108)	(0.0108)
(log,l1) Real GDP Priv. Industry pc	0.0320**				
	(0.0142)				
(log,l2) Real GDP Priv. Industry pc	0.0396***				
	(0.0129)				
(l1, log) Elem.Ed.Exp.pp	0.5163***	0.5200***	0.5283***	0.5046***	0.5049***
	(0.0139)	(0.0140)	(0.0149)	(0.0136)	(0.0136)
(log) IG Revenue pp	0.2251***	0.2130***	0.2025***	0.2195***	0.2196***
	(0.0164)	(0.0166)	(0.0181)	(0.0160)	(0.0160)
(log) Enrollment	-0.1595***	-0.1796***	-0.2012***	-0.1775***	-0.1773***
	(0.0143)	(0.0132)	(0.0138)	(0.0147)	(0.0147)
% Black	0.0457	0.0817	0.2051	0.1182	0.1173
	(0.1666)	(0.1517)	(0.1555)	(0.1703)	(0.1702)
% Hispanic	0.0522	0.0842	0.1515	0.0253	0.0261
	(0.1272)	(0.1207)	(0.1238)	(0.1289)	(0.1288)
(log) Annual Avg. Wkly. Wage		0.1097**		0.2702***	0.2665***
		(0.0463)		(0.0382)	(0.0380)
(log, l1) Annual Avg. Wkly. Wage		0.1439**			
		(0.0565)			
(log, l2) Annual Avg. Wkly. Wage		0.0537			
		(0.0427)			
(log) House Price Index			0.1091***		
			(0.0182)		
(log, l1) House Price Index			0.0381		
			(0.0291)		
(log, l2) House Price Index			-0.0134		
			(0.0208)		
Fixed-effects					
unit	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	11,248	11,840	11,496	11,248	11,248
R^2	0.90241	0.90325	0.90362	0.90304	0.90301
Within R^2	0.51047	0.52242	0.52121	0.51363	0.51346
Wu-Hausman				33.448	30.202
Wu-Hausman, p-value				7.52×10^{-9}	3.98×10^{-8}
Wald (IV only)				50.051	49.282
Wald (IV only), p-value				1.59×10^{-12}	2.34×10^{-12}
F-test (1st stage), (log) Annual Avg. Wkly. Wage				5,524.3	5,506.7
F-test (1st stage), p-value, (log) Annual Avg. Wkly. Wage				0×10^{-16}	0×10^{-16}

Clustered (unit) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

H.3 Panel VAR Specification

$$Y_{it} = \alpha_i + \sum_{k=1}^{4} \gamma_k A_{i,t-k} + \beta X_{it} + \varepsilon_{it}$$

Where we approach a level and per capita value expression of the relationship between total education expenditure, intergovernmental revenue, house prices conditioned on GDP and wage levels.

$$Y_{it} = \begin{bmatrix} \log(\text{real Total Educ. Exp.})_{it} \\ \log(\text{real Total IG Revenue})_{it} \\ \log(\text{HPI})_{it} \end{bmatrix}, \quad X_{it} = \begin{bmatrix} \log(\text{real GDP})_{it} \\ \log(\text{wage})_{it} \end{bmatrix}$$

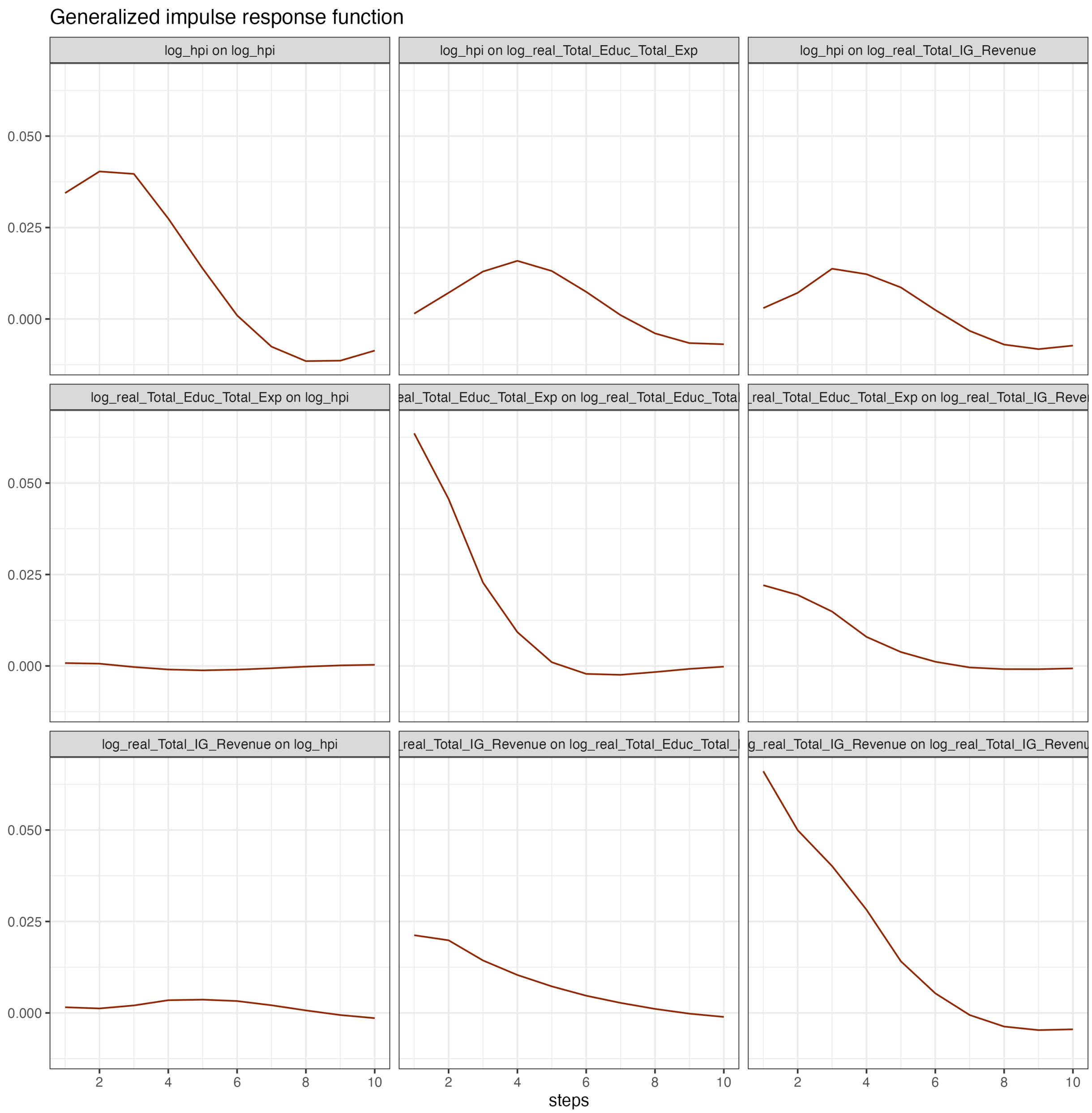
- A_1, A_2, A_3, A_4 are 3×3 coefficient matrices

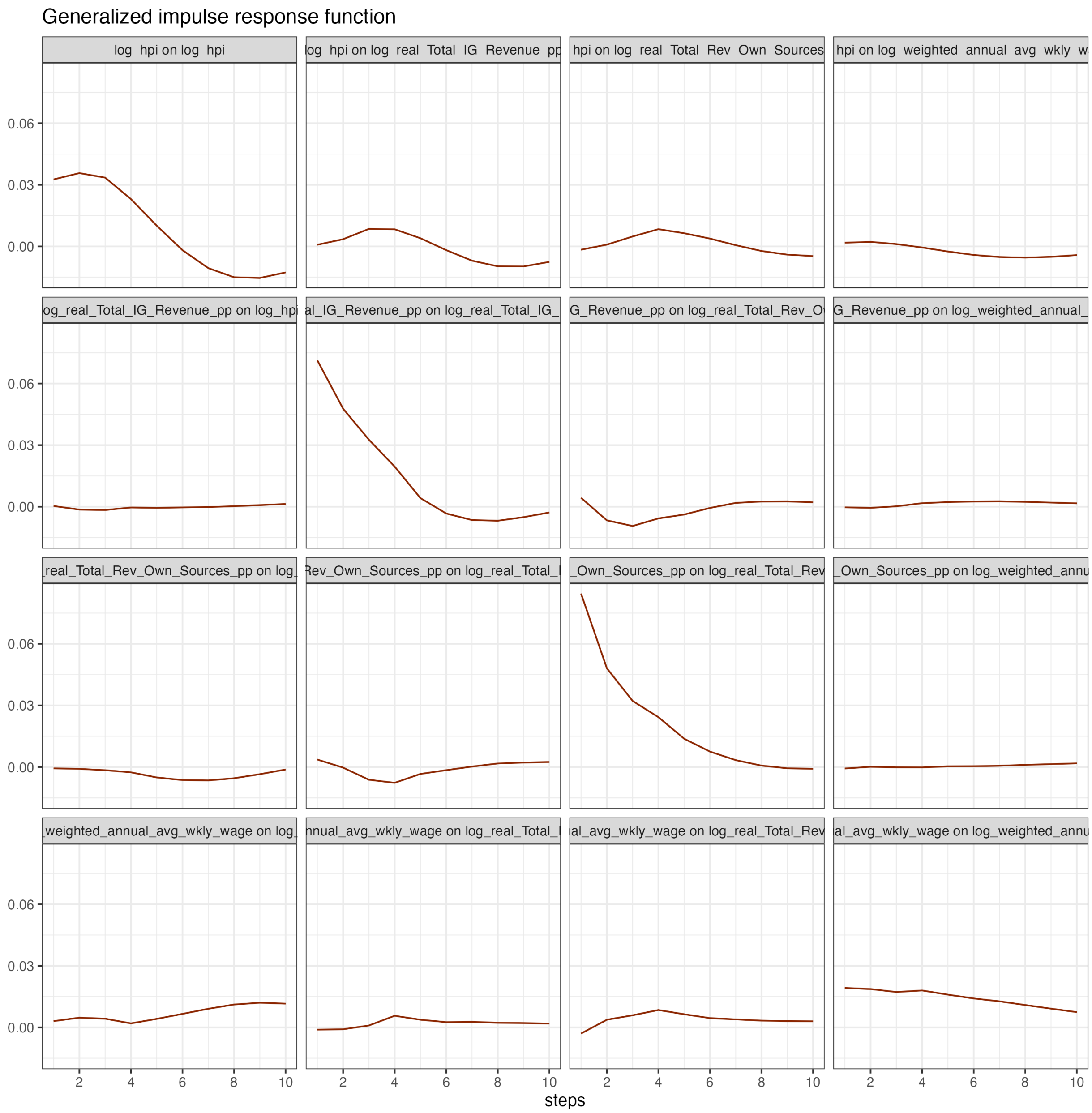
- β is a 3×2 matrix of coefficients on the exogenous variables
- α_i is a vector of unit fixed effects
- ε_{it} is the error term

Where

$$Y_{it} = \begin{bmatrix} \log(\text{real Own Source Rev. per person})_{it} \\ \log(\text{real IG Revenue per person})_{it} \\ \log(\text{wage})_{it} \\ \log(\text{HPI})_{it} \end{bmatrix}, \quad X_{it} = [\log(\text{real GDP per capita})_{it}]$$

- A_1, A_2, A_3, A_4 are 4×4 coefficient matrices
- B is a 4×1 coefficient matrix
- α_i unit fixed effects
- ε_{it} error term




Generalized impulse response function
log_hpi on log_hpi
og_hpi on log_real_Total_IG_Revenue_pp
_hpi on log_real_Total_Rev_Own_Sources
_hpi on log_weighted_annual_avg_wkly_w
og_real_Total_IG_Revenue_pp on log_hpi
al_IG_Revenue_pp on log_real_Total_IG_
G_Revenue_pp on log_real_Total_Rev_O
G_Revenue_pp on log_weighted_annual_
_real_Total_Rev_Own_Sources_pp on log_
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0.06
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2
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6
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10
steps

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