Uneven Wage Growth and Public Goods

The Case of US Public Education

Ebba Mark

Abstract

This study estimates the elasticity of public education expenditure to changes in local economic conditions and livelihoods, offering insights into public service provision within a political economy marked by an uneven industrial landscape and rising income inequality. Over recent decades, the United States has experienced increasingly divergent patterns of economic and wage growth. Wage growth is a key driver of local wealth accumulation, enabling greater household and community investment in public goods. It is frequently posited that 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. Using a shift–share instrumental variable design, we examine how uneven wage growth affects public education spending across the US. Accounting for both observable and unobservable heterogeneity in state-level tax regimes, income, and economic growth, we find that this heterogeneity explains majority of the identified elasticity of education expenditure to wages. These results provide insight into region- and state-specific adjustments that can be made to ensure that uneven economic development and structural transformation does not exacerbate existing inequalities in public service delivery in the United States. Furthermore, these results underscore the importance of moving beyond average treatment efffects in panel regression studies to more careful specification choices. We further contribute to the shift–share literature by emphasizing the potential for model misspecification and the importance of interpreting instrument choice jointly with causal treatment effects.

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1 Working Notes

The following are notes to keep in mind while the project is still underway.

Items to be adjusted:

- Include additional control variables (migration, poverty, race, rurality, home ownership rates, private school enrollment)
- Local CPI. Do I need to correct for local price levels?
- Spatial autocorrelation term
- Run gets on central model with CFESIS (preliminary testing indicates coefficient breaks in 2006 and 2013 which neatly bookends the financial crisis/housing market collapse - interesting?)
- Separate shift-share on more coarse industrial categories (ie. high and low-wage areas correcting for local CPI) - in other words, to better answer the research question, divide industries by those whose wages line up with productivity growth and those that do not?

- Coefficient interpretation in the IV regressions are odd (elasticities > 1). I think this either results from not normalising employment shares when calculating the shift-share instrument or not properly dealing with the commuting zones where there is a high degree of missing data in local employment share data (See first histogram Figure 2).
- Label Figures and Tables. Figure out referencing in Quarto Markdown.

Note: Any warnings about "missing observations" or "NA being removed" relates to the lags incorporated.

2 Introduction

I follow the template for writing introductions to economics papers [here](https://www.cgdev.org/blog/how-write-introduction-your-development-economics-paper) - the headings guide the structure of the intro but will be removed later.

I am compiling this in Quarto Markdown and have yet to figure out how to reference my tables. Please excuse lack of referencing to regression tables and figures at the moment.

2.0.1 Motivate with a puzzle or a problem (1–2 paragraphs)

Since the 1970s, a persistent divergence between productivity growth and wage growth has emerged in the United States. While labour productivity has continued to rise, the earnings of typical workers have increased far more slowly, leading to a substantial decoupling between the two trends. Summers and 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. 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 labour's share of national income. 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.

This body of work underscores that the relationship between pay and productivity is contingent, and that productivity growth, while necessary, is not sufficient to secure broad-based improvements in living standards. **The direct consequences of this decoupling are clear.** Productivity growth is not sufficient to secure broad-based improvement in living standards and where inequality is not spatially segregated, and high- and low-income households share the same local markets, the divergence between wages and productivity is likely to generate upward pressure on prices that disproportionately burdens lower- and middle-income earners.

A link that has been far less explored in this context is the spillover effect of local wage levels to local wealth-building and its effect on public goods. Diverging economic and wage growth poses potentially severe consequences for local wealth-building which still relies on wages for lower- and middle-income familes and communities. Wage growth is an important contributor to local wealth-building, allowing households and communities to invest more in local public goods. 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. Put plainly, given the structure of US public services, wherein they are funded largely through property taxes and thus tied to asset values, inequality in wealth-building can have significant effects for the quality of local public services.

This study therefore aims to determine whether an elasticity of public expenditure to local economic growth exists. If productivity gains translate unevenly into wages across industries and regions, then the fiscal capacity of local governments may be shaped as much by institutional and structural conditions as by aggregate economic growth.

Community well-being and public expenditure in the US is already characterised by a high degree of spatial heterogeneity. Not only does the US consistently rank among the top five most unequal

OECD countries [1], evidence of how income and wealth inequality perpetuate other forms of inequality (opportunity, health, infrastructure quality, and broader well-being) is steadily increasing (Chetty et al. (2016), Logan, Minca, and Adar (2012), Semuels (2016), Avanceña et al. (2021), Flavin et al. (2009)). Boustan et al. (2013) 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.

Economic history and industrial activity have heterogeneously impacted the development trajectories of US regions...[]

One public service that has particularly important ties to ensuring generational resilience to economic decline is education. Public schools around the US are responsible for educating over 80% of 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 (National Center for Education Statistics (2023)) . However, the quality of services delivered varies widely across the country. In 2016, for example, the Connecticut State Department of Education reported that the town of Greenwich, one of the highest-income towns in the country, spent $8,000 more per pupil than Bridgeport ($21.9k versus $13.7k per pupil), despite both towns being part of the same county, located less than 40 kilometers apart (Semuels (2016)), and competing in academic and extra-curricular activities.

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 (Alfonso and DuPaul (2020)). Therefore, ensuring that local or regional economic decline does not disrupt or worsen the

[1] The US consistently ranks among the top 5 most unequal countries in the OECD alongside Turkey, Mexico, Chile, and Costa Rica across all relevant indicators reported by the OECD: Gini coefficient, three interdecile income ratios (P50/P10; P90/P10; P90/P50), Palma ratio, S80/S20 quintile share.

quality of education delivered is of paramount importance to ensure greater equality in the long-run. [2] [3] [4]

2.0.2 Clearly state your research question (1 paragraph)

Altogether, this evidence points to the value of identifying the extent to which expenditure on public education is reliant on local economic health, measured in wages and real value added, across the country. This work aims to answer the following research questions:

RQ1: Over the last twenty years, has wage inequality impacted local public education expenditure?

RQ2: Do intergovernmental transfers alleviate wealth-driven inequalities in public education expenditure?

RQ3: Can accounting for non-constant relationships between explanatory variables improve our understanding of the relationship between public goods and uneven economic growth?

2.0.3 Empirical approach (1 paragraph)

This work interrogates the elasticity of public education expenditure to uneven economic and wage growth across US commuting zones.

[2] 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 Wiens, Poast, and Clark (2014). The literature is divided into two strands focusing on either political (the relationship between resource wealth and governance) Deacon (2011) 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 Sincovich et al. (2018). 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 Dialga and Ouoba (2022). Although awareness of this strand of literature is of relevance to this work, the unresolved nature of the 'debate' surrounding its existence requires caution if eventually utilised as a theoretical framework for answering the research question.

[3] Ahlerup, Baskaran, and Bigsten (2020) find that for 30 countries in Africa, the presence of gold mines during adolescence have a significant effect on educational attainment. Badeeb, Lean, and Clark (2017) investigates whether resource dependence slows economic growth with no explicit mention of education. Blanco and Grier (2012) find that in Latin America, petroleum export has a significant long-run negative relationships with human capital. Borge, Parmer, and Torvik (2015) find support for the paradox of plenty hypothesis in Norway - that higher local public revenue negatively affects the efficiency of local public good provision. Brunnschweiler and Bulte (2008) 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.' Cockx and Francken (2014) use a panel on 140 countries from 1995-2009 and find an inverse relationship between resource dependence and and public health spending over time. Cockx and Francken (2016) investigate a panel of 140 countries from 1995-2009 to find an adverse effect of resource depdence on public education expenditures relative to GDP. Dialga and Ouoba (2022) find disparate results for health and education controlling for institutional quality. Douglas and Walker (2017) "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.' Haber (n.d.) focus on authoriarian regimes. Menaldo (2016) argues again that this is an institutions curse and not a resource curse issure. Sincovich et al. (2018) provide a literature review of resource curse investigations in the Australian context.

[4] One investigation assessed the dependence of local public revenues on fossil fuel production finding that such production generated about $138 billion annually for US localities, states, tribes, and the federal government Raimi et al. (2022) . This amount is forecast to decline by 2050 even in a business-as-usual scenario (assuming no changes in climate policy stringency). Wyoming, North Dakota, Alaska, and New Mexico are the states most dependent on fossil fuel revenues with at least 14% of state and local revenues generated from the fossil fuel industry (Wyoming's dependence is above 50%). The work makes a demonstrative statement about the link between this revenue stream and essential services like schools, public health, and infrastructure, but stops short of an empirical analysis into the impact of fossil fuel decline on revenues and associated expenditure, even at the state level.

We construct a shift-share (Bartik) instrument that combines fixed local industry employment shares with national industry-level changes in wages and real value added. Following the established literature (Bartik 1991; Goldsmith-Pinkham, Sorkin, and Swift 2020; Ferri 2022), we fix local employment shares to a baseline period and interact them with national growth rates in industry wages and real value added. Using data from the U.S. Bureau of Labor Statistics and Bureau of Economic Analysis, we construct commuting-zone-level Bartik instruments based on both outcomes. This provides a credible and transparent identification strategy that links macroeconomic shocks to local education funding.

This strategy 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, depends heavily on industries that are unevenly distributed across regions but likely subject to similar industry-specific wage shocks. Finally, we use this instrument to identify the effect of shocks to this instrument on local public education expenditure as reported in a panel dataset from the Annual Survey of State and Local Government Finances.

The outlined instrumental variable strategy tackles the central endogeneity challenge present in any study of the linkage between local economic growth and measures of well-being. In the context of this study, wages and public education expenditure are undoubtedly exogenous, since higher-income families may self-select into districts with greater education spending, confounding causal inference. Therefore, we instrument local wages with the constructed shift-share instruments to circumvent this challenge allowing for plausibly causal inference.

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 single, well-defined national average treatment effect is inherently limited. We provide an initial benchmark using a pooled estimation to establish baseline relationships between wages, GDP, and asset values that appear to generalize reasonably across the national economy, before investigating the htereogeneity that these pooled estimates mask in our main analysis. Centrally, we employ an instrumental variable design using wage and GDP-based shift share instruments to identify the dependence of local public education expenditure on local economic prosperity. We further advance this analysis via state-by-state and industry-by-industry estimations which allow for industry and region-specific results to emerge. Furthermore, we group commuting zones by their historic growth trajectories to improve comparability of treatment and control groups in our instrumental variable design as well as emphasize context-specific outcomes.

2.0.4 Detailed results (3–4 paragraphs)

1. Public education expenditure is not agnostic to local economic conditions. Across all estimations, we find a strong positive relationship, descriptive causal, between wages, property values, GDP and local public expenditure. This result contributes to the wealth of evidence demonstrating the inequality of public education services across the US.
2. We establish a causal link between public education expenditure and local wage and industry value added growth using a shift-share exposure treatment estimation in specific industries and states, though not broadly.
3. State-level estimation result.
4. Industry-level estimation result.
5. Incorporate analysis about diverging growth rates to link to intro. Would be nice to contribute to this debate.

2.0.5 Value-added relative to related literature (1–3 paragraphs)

2.0.6 Optional paragraphs: robustness checks, policy relevance

2.0.7 Roadmap (1 paragraph)

In the sections that follow, we outline in Section 3 the data to be used in the analysis; Section 4 the methodological approach with accompanying results; Section 6 and Section 7 provide a discussion and concluding remarks.

3 Data

We compile a panel dataset of 636 commuting zones across 40 US states between 2001-2021 including the following metrics:

Expenditure and Revenue: This work employs Willamette University's Annual Government Finance Database at the commuting zone (CZ) level. This resource is a harmonised repository of the data collected annually as part of the US Census Bureau's Annual Survey of State & Local Government Finances, the 'only comprehensive source of information on the finances of local governments in the United States' (Pierson, Hand, and Thompson (n.d.)). The data includes commuting-zone level revenue 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. The data for property taxes collected used in regressions below also come from this dataset. Expenditure on vocational training and from Educational Service Agencies (ESAs) are also sourced from this dataset. We aggregate school district measures up to the commuting zone-level to ensure the availability of adequate control and treatment variables. [5]

Thus, this dataset provides estimates in $USD on total public school revenue disaggregated by source (federal, state, local intergovernmental versus own local sources) and expenditure disaggregated by item (level of schooling, teacher salaries, debt, etc.).

Population controls: US Census Bureau.

GDP Controls: We gather GDP control variables from the Bureau of Economic Analysis (BEA). This BEA data is only available after 2001, therefore the panel reported and used below is restricted to 2001-2021. The controls used in the below are commuting zone-level private industry GDP. We decide to use private industry GDP as a control variable given the remaining portion of GDP is government expenditure which includes education expenditure.

Property Prices: The US Federal Housing Financy Agency provides a geographically linked data on single-family house prices called the Housing Price Index. HPI is a broad measure of the movement of single-family house prices. The FHFA HPI is a weighted, repeat-sales index, meaning that it measures 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 since January 1975 Source. It is reported at the county level at an annual frequency. We aggregate to the commuting zone level via a mean. Should do a population weighted average.

This data aggregation results in a complete and balanced panel of 636 US commuting zones across 40 states

[5]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 (n.d.)). We choose to conduct the analysis on the commuting zone level because (1) it is a more accurate picture of a local labor market area (Carpenter, Lotspeich-Yadao, and Tolbert (2022)) and (2) a lack of availability of control variables at a school district level.

between 2001-2021. [6] [7] All data used is reported annually at the commuting zone level. [8] Therefore, no time-invariant variables are included (apart from an indicator of the state a CZ is in).

3.1 Summary statistics

Table 1 reports summary statistics across relevant variables. All (dollar) values are reported in (real 2017-chained) thousands.

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
Elem. 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
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

4 Analysis

Given the high degree of both structural (state-specific tax, regulatory, and legislative regimes) and evolved heterogeneity (industrial activity, income, inequality, economic diversity) the following analysis only briefly explores the potential to arrive at national-level average treatment effects using various pooled estimation strategies. These serve to establish foundational relationships between local economic conditions that seem to reasonably generalise across the country.

However, drawing meaningful conclusions necessitates a regional or state–by-state estimation strategy to truly account for the heterogeneity across the units of observation. Therefore, the body of the analysis is dedicated to state-by-state and industry-by-industry estimation of the relevant econometric specifications. We dedicate majority of this manuscript to discussion of this latter exploration of heterogeneity, aiming, wherever possible, for a causal interpretation. We include information about the uncertainty surrounding our results as candidly as possible. Jennie: Any places where you think such uncertainty can be better highlighted, please note.

4.1 Descriptive Regressions

First, we employ a two-way fixed effects ordinary least-squares panel model with standard errors clustered by commuting zone. We outline the model specification immediately below:

$$Y_{it} = \beta_0 + \beta_x X_{it} + \delta_1 Enrollment_{it} + \delta_2 IGR_{it} + \alpha_i + \eta_i + \gamma_t + \varepsilon_{it} \tag{1}$$

[6] The reason 13% of CZs are missing from the dataset is because of (1) the exclusion criteria already outlined; (2) Hawaii and Alaska have been excluded due to the methodological challenge of incorporating their school districts into spatial econometric work; and (3) Connecticut, Maryland, North Carolina, and Virginia have been excluded due to unconventional or incomplete public school district reporting. We aim to resolve this, especially in the case of Virginia given its relatively high rates of employment in the coal sector.

[7] Given the work's intent to rely on data on property taxes collected, any CZ that reports more than five 0 values for property taxes collected is excluded.

[8] 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 labour market areas/economies ({https://www.ddorn.net/data.htm\#Local\%20Labor\%20Market\%20Geography}(David Dorn's Resource Page), {https://www.nature.com/articles/s41597-024-03829-5}(Fowler et al. 2024)).

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 human capital investments. Second, in terms of public impact, elementary education is of foundational importance in the lives of children (Stefi provided some sources on this fac)t. Slips in public education provision at such a young age could have scarring effects for children in such school systems. α_i, η_i, γ_t represent CZ, state, and 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 2, Equation 3, Equation 4 where h represents h-year time lags. We estimate all equations in levels and growth rates.

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

$$X_{it}^{Wage} = \sum_{h=0}^{2} \beta_h^{\text{Wage}} \log(\text{Wage}_{i,t-h}) \tag{3}$$

$$X_{it}^{HPI} = \sum_{h=0}^{4} \beta_h^{\text{HPI}} \log(\text{HPI}_{i,t-h}) \tag{4}$$

Table 2 reports the results from regressions of log elementary education expenditure per pupil on contemporaneous and lagged measures of local economic activity. Table 3 presents the analogous specifications using annualized growth rates to capture short-run dynamics.The estimates in Table 2 show that per-pupil education spending is systematically higher in commuting zones with 'stronger' local economies measured in local wages, industrial GDP, and asset prices.

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 one-percent increase in local GDP per capita two years prior is associated with roughly a 0.15% increase in current education expenditure per pupil, suggesting that fiscal capacity effects unfold gradually over time. Intergovernmental revenue per pupil emerges as the strongest and most consistent predictor of education expenditure. A 1% increase in intergovernmental transfers is associated with approximately a 0.2–0.35% increase in per-pupil education spending, controlling for unit/state and year fixed effects. (I think this interpretation is correct. Though it might just reflect a "share" of expenditure from those sources?) This finding highlights the importance of state and federal aid in sustaining local education budgets. Lagged economic indicators, particularly private industry GDP and average weekly wages, are also positively and significantly associated with education spending. 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 1% increase in lagged (t–2) real private GDP per capita is associated with a 0.15% increase in per-pupil spending.

The house price index also enters positively and significantly in contemporaneous and time-dynamic specifications (up to t–3) underscoring the fundamental relationship between community asset wealth and public education expenditure.

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, with coefficients >0.3%. 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 which hints 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, cumulative 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.

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.

We also estimate these regressions using state rather than commuting-zone level fixed effects to account for the relevance of state-level tax regimes and policies that govern education. Though the relationships remain somewhat stable across both level and growth rate specifications, the state-fixed effects affect the stability of our coefficient estimates. Jennie: I don't yet know what this means. But given the heterogeneity across states but homogeneity within states in how school funding is set, I am tempted to use state-level fixed effects over CZ level fixed effects which would change the analysis above.

Table 2: Descriptive Results in Levels

Dependent Variable:	(log) Elem.Ed.Exp.pp								
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Variables									
(log) Real GDP Priv. Industry pc	0.0130			-0.0043			0.0130		
	(0.0187)			(0.0199)			(0.0187)		
(log,l1) Real GDP Priv. Industry pc	0.0691***			0.0642***			0.0691***		
	(0.0135)			(0.0132)			(0.0135)		
(log,l2) Real GDP Priv. Industry pc	0.1457***			0.1335***			0.1457***		
	(0.0231)			(0.0226)			(0.0231)		
(log) IG Revenue pp	0.3512***	0.3220***	0.3287***	0.2827***	0.2259***	0.2072***	0.3512***	0.3220***	0.3287***
	(0.0295)	(0.0328)	(0.0318)	(0.0274)	(0.0243)	(0.0276)	(0.0295)	(0.0328)	(0.0319)
(log) Enrollment	-0.2936***	-0.3022***	-0.3297***	-0.0329***	-0.0644***	-0.0341***	-0.2936***	-0.3022***	-0.3297***
	(0.0241)	(0.0247)	(0.0270)	(0.0043)	(0.0064)	(0.0050)	(0.0241)	(0.0248)	(0.0270)
(log) Annual Avg. Wkly. Wage		0.1706***			0.2275***			0.1706***	
		(0.0600)			(0.0784)			(0.0601)	
(log, l1) Annual Avg. Wkly. Wage		0.1767***			0.2106***			0.1767***	
		(0.0459)			(0.0487)			(0.0459)	
(log, l2) Annual Avg. Wkly. Wage		0.3169***			0.0748			0.3169***	
		(0.0796)			(0.0687)			(0.0798)	
(log) House Price Index			0.1450***			0.0385			0.1450***
			(0.0256)			(0.0325)			(0.0257)
(log, l1) House Price Index			0.0557**			0.1013***			0.0557**
			(0.0263)			(0.0301)			(0.0263)
(log, l2) House Price Index			0.0481**			0.0780***			0.0481**
			(0.0208)			(0.0245)			(0.0209)
(log, l3) House Price Index			0.0447**			0.0570***			0.0447**
			(0.0210)			(0.0220)			(0.0211)
(log, l4) House Price Index			0.0024			-0.1416***			0.0024
			(0.0215)			(0.0255)			(0.0215)
Fixed-effects									
unit	Yes	Yes	Yes				Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state				Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics									
Observations	12,084	13,356	12,588	12,084	13,356	12,588	12,084	13,356	12,588
R^2	0.86608	0.86135	0.86500	0.68861	0.67088	0.66621	0.86608	0.86135	0.86500
Within R^2	0.31070	0.30255	0.29461	0.32932	0.28070	0.16006	0.31070	0.30255	0.29461

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

Table 3: Descriptive Results in Growth Rates

Dependent Variable:	(GR) Elem.Ed.Exp.pp								
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Variables									
(GR) Real GDP Priv. Industry pc	0.0048			0.0087			0.0048		
	(0.0138)			(0.0134)			(0.0138)		
(GR,l1) Real GDP Priv. Industry pc	0.0509***			0.0544***			0.0509***		
	(0.0148)			(0.0149)			(0.0148)		
(GR,l2) Real GDP Priv. Industry pc	0.0191***			0.0198***			0.0191***		
	(0.0070)			(0.0070)			(0.0071)		
(GR) IG Revenue pp	0.3061***	0.3266***	0.3286***	0.3088***	0.3259***	0.3271***	0.3061***	0.3266***	0.3286***
	(0.0321)	(0.0224)	(0.0228)	(0.0317)	(0.0223)	(0.0228)	(0.0321)	(0.0224)	(0.0229)
(GR) Enrollment	-0.5990***	-0.0144**	-0.0060	-0.5741***	-0.0144**	-0.0063	-0.5990***	-0.0144**	-0.0060
	(0.0420)	(0.0064)	(0.0069)	(0.0397)	(0.0063)	(0.0068)	(0.0421)	(0.0064)	(0.0069)
(GR) Annual Avg. Wkly. Wage		-0.0269			-0.0263			-0.0269	
		(0.0547)			(0.0544)			(0.0548)	
(GR, l1) Annual Avg. Wkly. Wage		0.2065***			0.2079***			0.2065***	
		(0.0500)			(0.0494)			(0.0501)	
(GR, l2) Annual Avg. Wkly. Wage		0.3108***			0.3101***			0.3108***	
		(0.0600)			(0.0591)			(0.0601)	
(GR) House Price Index			0.0631***			0.0614**			0.0631***
			(0.0240)			(0.0239)			(0.0240)
(GR, l1) House Price Index			0.1074***			0.1069***			0.1074***
			(0.0289)			(0.0290)			(0.0290)
(GR, l2) House Price Index			0.0586***			0.0592***			0.0586***
			(0.0205)			(0.0205)			(0.0205)
(GR, l3) House Price Index			0.0207			0.0204			0.0207
			(0.0256)			(0.0257)			(0.0257)
(GR, l4) House Price Index			0.0325			0.0328			0.0325
			(0.0211)			(0.0211)			(0.0212)
Fixed-effects									
unit	Yes	Yes	Yes				Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state				Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics									
Observations	12,083	13,355	12,534	12,083	13,355	12,535	12,083	13,355	12,534
R^2	0.26799	0.35113	0.36047	0.26154	0.34055	0.34934	0.26799	0.35113	0.36047
Within R^2	0.22090	0.15363	0.14768	0.21898	0.15384	0.14687	0.22090	0.15363	0.14768

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

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.

Jennie: I need help interpreting Table 4 below...? I could not figure out how to shrink the table adequately so columns 1-3 = columns 4-6. Ignore 4-6. :)

Table 4: Descriptive Results with Funding Source Interaction Effects

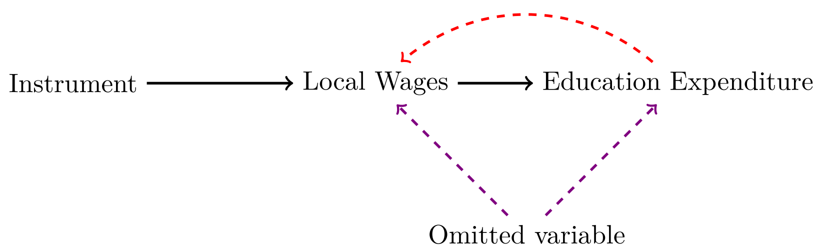
Dependent Variable:	(log) Elem.Ed.Exp.pp					
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
(log) Real GDP Priv. Industry pc	-0.2313***			-0.2313***		
	(0.0743)			(0.0743)		
(log,l1) Real GDP Priv. Industry pc	0.1026*			0.1026*		
	(0.0590)			(0.0590)		
(log,l2) Real GDP Priv. Industry pc	0.3435***			0.3435***		
	(0.0794)			(0.0794)		
Funding Share_state × (log) Real GDP Priv. Industry pc	0.4094***			0.4094***		
	(0.1201)			(0.1201)		
Funding Share_state × (log,l1) Real GDP Priv. Industry pc	-0.1083			-0.1083		
	(0.0948)			(0.0948)		
Funding Share_state × (log,l2) Real GDP Priv. Industry pc	-0.4455***			-0.4455***		
	(0.1203)			(0.1203)		
(log) Annual Avg. Wkly. Wage		-0.0203			-0.0203	
		(0.2249)			(0.2249)	
(log, l1) Annual Avg. Wkly. Wage		0.2182			0.2182	
		(0.1495)			(0.1495)	
(log, l2) Annual Avg. Wkly. Wage		0.5056**			0.5056**	
		(0.2281)			(0.2281)	
Funding Share_state × (log) Annual Avg. Wkly. Wage		0.4976			0.4976	
		(0.3530)			(0.3530)	
Funding Share_state × (log, l1) Annual Avg. Wkly. Wage		-0.0604			-0.0604	
		(0.2369)			(0.2369)	
Funding Share_state × (log, l2) Annual Avg. Wkly. Wage		-0.6441*			-0.6441*	
		(0.3567)			(0.3567)	
(log) House Price Index			-0.0812			-0.0812
			(0.1099)			(0.1099)
(log, l1) House Price Index			0.1573			0.1573
			(0.1348)			(0.1348)
(log, l2) House Price Index			0.3775***			0.3775***
			(0.1026)			(0.1026)
(log, l3) House Price Index			0.0077			0.0077
			(0.1204)			(0.1204)
(log, l4) House Price Index			-0.1349			-0.1349
			(0.0940)			(0.0940)
Funding Share_state × (log) House Price Index			0.4726***			0.4726***
			(0.1732)			(0.1732)
Funding Share_state × (log, l1) House Price Index			-0.1596			-0.1596
			(0.2050)			(0.2050)
Funding Share_state × (log, l2) House Price Index			-0.5034***			-0.5034***
			(0.1557)			(0.1557)
Funding Share_state × (log, l3) House Price Index			0.0251			0.0251
			(0.1870)			(0.1870)
Funding Share_state × (log, l4) House Price Index			0.1231			0.1231
			(0.1478)			(0.1478)
Funding Share_state	0.8906	0.7015	-0.4431	0.8906	0.7015	-0.4431
	(0.5640)	(0.4808)	(0.3669)	(0.5640)	(0.4808)	(0.3669)
(log) Fed IG Rev. pp	-0.0030	-0.0014	-0.0016	-0.0030	-0.0014	-0.0016
	(0.0024)	(0.0020)	(0.0020)	(0.0024)	(0.0020)	(0.0020)
(log) Enrollment	-0.3461***	-0.3498***	-0.3723***	-0.3461***	-0.3498***	-0.3723***
	(0.0263)	(0.0257)	(0.0264)	(0.0263)	(0.0257)	(0.0264)
Fixed-effects						
unit	Yes	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	12,084	13,356	12,588	12,084	13,356	12,588
R^2	0.86083	0.85889	0.86831	0.86083	0.85889	0.86831
Within R^2	0.28372	0.29018	0.31190	0.28372	0.29018	0.31190

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

4.2 Approaching Causal Identification

We are centrally interested in the effect of changes in local wages on public education expenditure. Though the descriptive relationship between various economic variables and public education expenditure provides convincing evidence of the reliance of local education expenditure on economic conditions in both levels and growth rates, this relationship has no causal interpretation. Indeed, there is a significant endogeneity concern in using local wages as a treatment variable for two reasons: (1) the likely attracting factor of high levels of education expenditure for higher-income families and (2) absent migration, education systems feed individuals with diverse human capital into local labor markets.. Therefore, we instrument local wages using a shift-share based instrument of industry-specific wages and real value added across 19 industrial categories.

Figure 1: Instrumental Variable Path Diagram



Shift-share or *Bartik* instruments have gained popularity in empirical work as a method of handling endogeneity issues in panel data (Ferri (2022), Goldsmith-Pinkham, Sorkin, and Swift (2020), Bartik (1991)). Such instruments combine time-variant, unit-invariant changes in aggregate economic variables (ie., national changes in industry wage levels) with time-invariant, unit-variant shares in exposure to these macro-level changes (ie., local shares of employment in particular industries). This decomposition of local-level changes via a delocalisation over space and time allows for a defensible 'de-endogenising' of the treatment. Notably, the method can also be considered to serve a further purpose as, by construction, it allows for the examination of a macro phenomenon's effect on more local units.

[9] [10]

Therefore, we adopt an identification strategy via a shift-share or Bartik instrument. A shift-share instrument interacts local industry shares with national industry-level growth rates to attain a plausibly exogenous local shock. In the context of this work, we construct the instrument by interacting a constant industrial employment share variable with a national industry-level wage and real value added data.

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 would look as follows:

$$Z_{it} = \sum_{j=1}^{k} S_{ij\tau} G_{njt} \tag{5}$$

[9]Autor et al. use a shift-share instrument to assess the effect of Chinese import competition on manufacturing employment in US commuting zones (@autor2013). As an extension, @feler2017 use a similar shift-share instrument to assess the effect of the same shock on the size of local government. @baccini2021 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.

[10]An additional popular indicator for modelling industrial shocks is *oil price* as values are often assumed to be exogenous to local and even national conditions (@scheer2022). Third, various indicators for measuring *deindustrialisation* have been proposed including the manufacturing share of employment, value added, and GDP [@tregenna2009, @tregenna2020]. Finally, in rare instances, exogeneity can be secured due to *geographical, climatological, or geological factors*. For example, @borge2015 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 [@esposito2021, @chen2022].

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 could construct the instrument as follows 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.

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

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 choose to employ the first of these options, assuming that industry shares are only exogenous at a given base period and that national level growth rates are exogenous and therefore allowed to vary with time.

Using data from the US Bureau of Labor Statistics' Quarterly Census of Employment and Wages (QCEW) and Bureau of Economic Analysis, we construct two types of shift-share Bartik instruments at the commuting zone level using local employment shares by industry and national changes in industry-specific wages and real value added represented in Equation Equation 10. G_{njt} represents national-level changes in wages or 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'.

[11]

$$\tilde{Z}_{it} = \sum_{j=1}^{k} G_{njt} * \frac{N_{ij\tau}}{N_{i\tau}} \tag{6}$$

[11] 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. I have done this unsystematically so far (testing 2001, 2004, and 2005) but arrived at the decision to compute an average to deal with missing data. Will include a more systematic testing of this in the appendix.

This yields a 2SLS model defined by the first- and second-stage regressions represented in Equation 7 and Equation 8 and instrument $\tilde{Z}_{ijt}$ is represented in Equation 10.

$$\text{First stage:} X_{it} = \pi_0 + \pi_1 \tilde{Z}_{it} + \pi_2 \text{Enrollment}_{it} + \pi_3 \text{IGR}_{it} + \alpha_i + \eta_i + \gamma_t + u_{it} \tag{7}$$

$$\text{Second stage:} \quad Y_{it} = \beta_0 + \beta_x \hat{X}_{it} + \delta_1 \text{Enrollment}_{it} + \delta_2 \text{IGR}_{it} + \alpha_i + \eta_i + \gamma_t + \varepsilon_{it} \tag{8}$$

We compute the two relevant shift-share instruments across 19 two-digit NAICS industrial categories listed in the table below. Given industry-level disaggregation of local employment and wage 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 categorisation we proceed with the 2-digit NAICS codes in the rest of the work. I need to double check how I handle Public Adminsitration and Educational Services wage and employment data as I think these would have an additional endogeneity issue...?

% latex table generated in R 4.5.0 by xtable 1.8-4 package % Fri Oct 10 10:20:09 2025

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

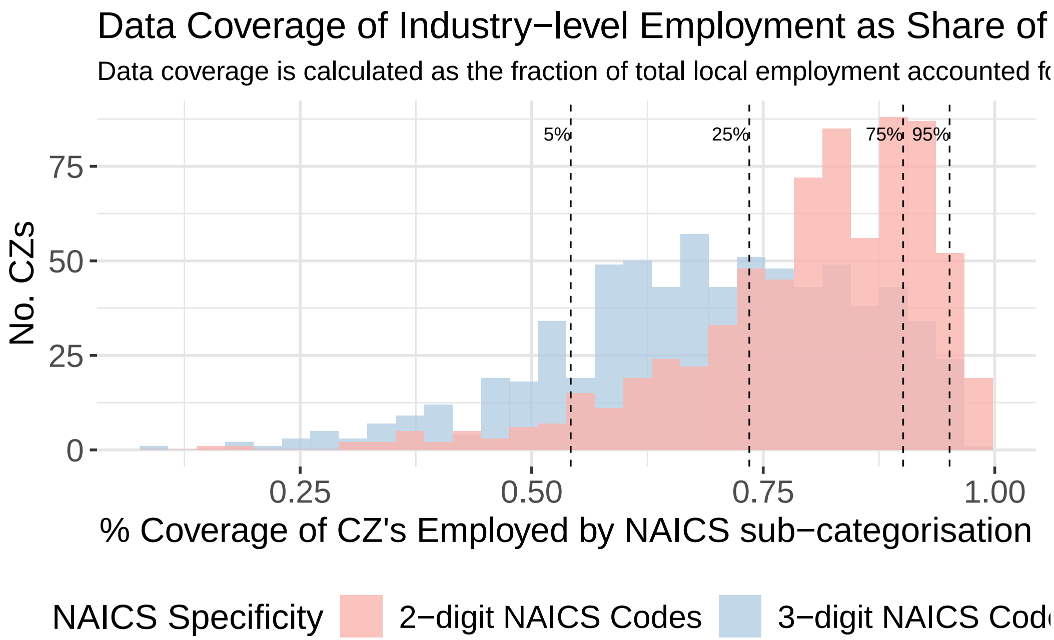


Figure 2: Data Coverage of Industry-level Employment as Share of Total Reported Employed

4.2.1 Industry-level Wages

First, we display the results for a 2SLS estimation using our wage-based shift-share instrument in Tables 5 and 6.

In Table 5, the instrumental variable estimates provide 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 Checking the interpretation of this. Varying the time-lag and inclusion of state or commuting zone fixed effects, we see that a 1% increase in the shift-share measure (which can be interpreted as a natural logarithm) is associated with a 0.02-0.06% increase in average weekly wages ($p < 0.01$), with an F-statistic between 11-103 (near or 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 (Table 6), the instruments lose predictive power (first-stage F-statistics ~1–2), resulting in weak identification. The second-stage coefficients become unstable and often insignificant, while Hausman tests fail to reject exogeneity. 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.

Jennie: Two things on this section: (1) I'm looking into coefficient interpretation of the wage-based shift share instrument. It is possible that the elasticity is greater than 1 but likely this has to do with the calculation of the wage-based SS instrument. (2) I am unsure how to communicate the differences between the level and growth rate specifications. See below for my attempt.

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. At the same time, 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. While the large first-stage F-statistics and Hausman tests in the level specification support its validity, the weak performance of the growth-rate version cautions that the results could be sensitive to issues of persistence and trending in the data. Taken together, these results suggest that while the level specification provides strong identification and compelling evidence of a positive causal effect of local wages on education spending, the weak performance of the growth-rate specification highlights the need for caution, as the strength of the findings may partly reflect long-run trending relationships rather than purely exogenous 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 5: IV Estimation Using Wage-based Shift-Share Instrument (l0, l1) 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	(log) Annual Avg. Wkly. Wage	(log) Elem.Ed.Exp.pp	(log) Annual Avg. Wkly. Wage	(log) Elem.Ed.Exp.pp
Stage	1st	2nd	1st	2nd	1st	2nd	1st	2nd
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
Wage SS (lvl)	0.0575***		0.0162***					
	(0.0169)		(0.0016)					
(log) IG Revenue pp	0.0492***	0.1147	0.0318***	0.2253***	0.0487***	-1.965	0.0314***	0.2274***
	(0.0029)	(0.0747)	(0.0036)	(0.0104)	(0.0029)	(7.706)	(0.0037)	(0.0102)
(log) Real GDP Priv. Industry pc	0.1679***	-0.6278**	0.1779***	-0.1978***	0.1643***	-7.617	0.1787***	-0.1879***
	(0.0023)	(0.2464)	(0.0023)	(0.0428)	(0.0023)	(25.95)	(0.0023)	(0.0413)
(log) Enrollment	0.0550***	-0.5341***	0.0664***	-0.1785***	0.0638***	-3.315	0.0658***	-0.1739***
	(0.0043)	(0.0884)	(0.0007)	(0.0165)	(0.0044)	(10.13)	(0.0007)	(0.0158)
(log) Annual Avg. Wkly. Wage		4.818***		2.100***		47.49		2.040***
		(1.474)		(0.2346)		(158.1)		(0.2251)
Wage SS (lvl,l1)					0.0053		0.0170***	
					(0.0178)		(0.0017)	
Fixed-effects								
unit	Yes	Yes			Yes	Yes		
year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state			Yes	Yes			Yes	Yes
Fit statistics								
Observations	13,356	13,356	13,356	13,356	12,720	12,720	12,720	12,720
R2 (1st stage)	0.97708		0.87046		0.97678		0.86275	
Adj. R2 (1st stage)	0.97589		0.86985		0.97552		0.86207	
F-test (IV only)	11.505	49.881	98.779	209.93	0.08905	35.832	103.66	205.41
F-test (IV only), p-value	0.00070	1.72×10^{-12}	3.4×10^{-23}	3.26×10^{-47}	0.76539	2.21×10^{-9}	2.98×10^{-24}	3.16×10^{-46}
Wu-Hausman		43.589		166.83		36.183		161.95
Wu-Hausman, p-value		4.21×10^{-11}		6.16×10^{-38}		1.85×10^{-9}		7.13×10^{-37}
Wald (IV only)	11.505	10.681	98.779	80.103	0.08905	0.09026	103.66	82.169
Wald (IV only), p-value	0.00070	0.00109	3.4×10^{-23}	4.02×10^{-19}	0.76539	0.76385	2.98×10^{-24}	1.43×10^{-19}

IID standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 6: IV Estimation Using Wage-based Shift-Share Instrument (l0, l1) in Growth Rates 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	(log) Annual Avg. Wkly. Wage	(log) Elem.Ed.Exp.pp	(log) Annual Avg. Wkly. Wage	(log) Elem.Ed.Exp.pp
IV stages	First	Second	First	Second	First	Second	First	Second
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
Wage SS (GR)	0.0226		0.1687**					
	(0.0363)		(0.0819)					
(log) IG Revenue pp	0.0497***	0.4024*	0.0266***	0.2189***	0.0487***	0.4518***	0.0258***	0.1640*
	(0.0029)	(0.2119)	(0.0036)	(0.0334)	(0.0029)	(0.1035)	(0.0037)	(0.0839)
(log) Real GDP Priv. Industry pc	0.1670***	0.3384	0.1807***	-0.2414	0.1642***	0.5226	0.1817***	-0.6391
	(0.0022)	(0.7114)	(0.0023)	(0.2214)	(0.0023)	(0.3473)	(0.0023)	(0.5852)
(log) Enrollment	0.0582***	-0.1972	0.0701***	-0.1954**	0.0641***	-0.1400	0.0697***	-0.3471
	(0.0042)	(0.2483)	(0.0006)	(0.0860)	(0.0043)	(0.1362)	(0.0006)	(0.2247)
(log) Annual Avg. Wkly. Wage		-0.9680		2.341*		-2.082		4.522
		(4.260)		(1.223)		(2.115)		(3.218)
Wage SS (GR,l1)					-0.0571		0.1149	
					(0.0355)		(0.0823)	
Fixed-effects								
unit	Yes	Yes			Yes	Yes		
year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state			Yes	Yes			Yes	Yes
Fit statistics								
Observations	13,356	13,356	13,356	13,356	12,720	12,720	12,720	12,720
R2 (1st stage)	0.97706		0.86954		0.97679		0.86164	
Adj. R2 (1st stage)	0.97587		0.86892		0.97552		0.86096	
F-test (IV only)	0.38741	0.06759	4.2373	11.104	2.5798	1.9888	1.9501	18.855
F-test (IV only), p-value	0.53368	0.79489	0.03957	0.00086	0.10826	0.15850	0.16260	1.42×10^{-5}
Wu-Hausman		0.13333		9.0492		2.8510		17.291
Wu-Hausman, p-value		0.71501		0.00263		0.09134		3.23×10^{-5}
Wald (IV only)	0.38741	0.05162	4.2373	3.6605	2.5798	0.96935	1.9501	1.9746
Wald (IV only), p-value	0.53368	0.82027	0.03957	0.05574	0.10826	0.32486	0.16260	0.15999

IID standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

4.2.2 Industry-level GDP

We perform a similar IV estimation using the GDP-based shift-share instrument.(These are technically value added shocks, not GDP shocks. I will make the language consistent throughout.) In Tables 7 and 8, we find quite different relationships and evidence of a weaker identification strategy. The sign on the coefficient of the first-stage relationship depends on the fixed effect specification (CZ vs. state).

Though the first-stage F-statistics, Wu-Hausman, and Wald tests indicate the plausible relevance of our instrumental variable, endogeneity of our wage variable, and the significance of our instrumented wage variable, respectively, the temperamental nature of our first- and second-stage relationships of interest complicates the interpretation of our result. Furthermore, the growth rate specifications provide discordant results about the direction of the effect of the causal relationships.

Together with the results from our wage-based shift-hsare instrument, we find evidence that shifts in wage growth, rather than simply changes in economic growth is more important for local public education expenditure. In other words, national level wage growth is a more important predictor local wage effects than the mere presence of industry-level GDP growth. This makes intuitive sense in that the link from industrial success (labour) to personal and community wealth creation is mediated via wage and not necessarily the total industrial output which might not be reflected in wages (especially given recent evidence of decoupling of wages from productivity) Source here - OECD and FRED Data.

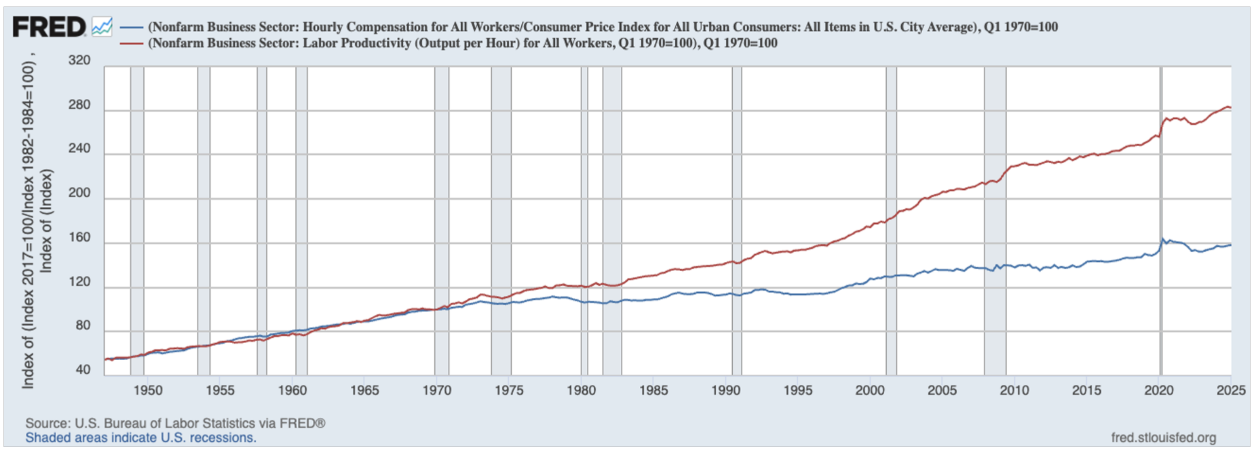


Figure 3: Wages and Productivity

Table 7: IV Estimation Using GDP-based Shift-share instrument (l0,l1) 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	(log) Annual Avg. Wkly. Wage	(log) Elem.Ed.Exp.pp	(log) Annual Avg. Wkly. Wage	(log) Elem.Ed.Exp.pp
IV stages	First	Second	First	Second	First	Second	First	Second
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
GDP SS (Lvl)	-0.0517**		0.0079***					
	(0.0231)		(0.0018)					
(log) IG Revenue pp	0.0500***	0.6076***	0.0301***	0.1935***	0.0496***	0.5180***	0.0298***	0.1990***
	(0.0029)	(0.1328)	(0.0037)	(0.0238)	(0.0029)	(0.0511)	(0.0038)	(0.0215)
(log) Real GDP Priv. Industry pc	0.1666***	1.028**	0.1795***	-0.4165***	0.1635***	0.7456***	0.1803***	-0.3903***
	(0.0022)	(0.4426)	(0.0023)	(0.1415)	(0.0023)	(0.1668)	(0.0023)	(0.1279)
(log) Enrollment	0.0606***	0.0432	0.0680***	-0.2635***	0.0684***	-0.0530	0.0674***	-0.2516***
	(0.0043)	(0.1563)	(0.0008)	(0.0549)	(0.0044)	(0.0678)	(0.0008)	(0.0491)
(log) Annual Avg. Wkly. Wage		-5.096*		3.309***		-3.440***		3.154***
		(2.650)		(0.7809)		(1.014)		(0.7025)
GDP SS (lvl,l1)					-0.1068***		0.0087***	
					(0.0242)		(0.0019)	
Fixed-effects								
unit	Yes	Yes			Yes	Yes		
year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state			Yes	Yes			Yes	Yes
Fit statistics								
Observations	13,356	13,356	13,356	13,356	12,720	12,720	12,720	12,720
R2 (1st stage)	0.97707		0.86968		0.97682		0.86185	
Adj. R2 (1st stage)	0.97588		0.86906		0.97556		0.86117	
F-test (IV only)	5.0090	24.260	18.558	97.724	19.533	41.181	21.126	99.839
F-test (IV only), p-value	0.02523	8.52×10^{-7}	1.66×10^{-5}	5.77×10^{-23}	9.98×10^{-6}	1.44×10^{-10}	4.34×10^{-6}	2.02×10^{-23}
Wu-Hausman		28.715		85.679		52.198		86.779
Wu-Hausman, p-value		8.53×10^{-8}		2.44×10^{-20}		5.32×10^{-13}		1.41×10^{-20}
Wald (IV only)	5.0090	3.6993	18.558	17.955	19.533	11.503	21.126	20.156
Wald (IV only), p-value	0.02523	0.05446	1.66×10^{-5}	2.28×10^{-5}	9.98×10^{-6}	0.00070	4.34×10^{-6}	7.2×10^{-6}

IID standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 8: IV Estimation Using GDP-based Shift-share instrument (l0,l1) in Growth Rates 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	(log) Annual Avg. Wkly. Wage	(log) Elem.Ed.Exp.pp	(log) Annual Avg. Wkly. Wage	(log) Elem.Ed.Exp.pp
IV stages	First	Second	First	Second	First	Second	First	Second
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
GDP SS (GR)	0.1370**		0.6755***					
	(0.0605)		(0.1366)					
(log) IG Revenue pp	0.0498***	0.6026***	0.0270***	0.3081***	0.0486***	0.2807***	0.0258***	0.3076***
	(0.0029)	(0.1294)	(0.0036)	(0.0124)	(0.0029)	(0.0375)	(0.0037)	(0.0151)
(log) Real GDP Priv. Industry pc	0.1670***	1.011**	0.1804***	0.3730***	0.1641***	-0.0535	0.1813***	0.3828***
	(0.0022)	(0.4311)	(0.0023)	(0.0711)	(0.0023)	(0.1238)	(0.0023)	(0.0945)
(log) Enrollment	0.0578***	0.0373	0.0695***	0.0432	0.0645***	-0.3647***	0.0692***	0.0453
	(0.0042)	(0.1522)	(0.0006)	(0.0276)	(0.0043)	(0.0495)	(0.0007)	(0.0363)
(log) Annual Avg. Wkly. Wage		-4.995*		-1.056***		1.427*		-1.098**
		(2.580)		(0.3925)		(0.7532)		(0.5191)
GDP SS (GR,l1)					-0.2266***		0.5805***	
					(0.0665)		(0.1525)	
Fixed-effects								
unit	Yes	Yes			Yes	Yes		
year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state			Yes	Yes			Yes	Yes
Fit statistics								
Observations	13,356	13,356	13,356	13,356	12,720	12,720	12,720	12,720
R2 (1st stage)	0.97707		0.86974		0.97681		0.86178	
Adj. R2 (1st stage)	0.97588		0.86912		0.97554		0.86110	
F-test (IV only)	5.1173	23.812	24.460	13.020	11.607	4.2023	14.493	8.2484
F-test (IV only), p-value	0.02371	1.07×10^{-6}	7.68×10^{-7}	0.00031	0.00066	0.04039	0.00014	0.00409
Wu-Hausman		28.263		21.056		2.3296		13.110
Wu-Hausman, p-value		1.08×10^{-7}		4.5×10^{-6}		0.12696		0.00029
Wald (IV only)	5.1173	3.7478	24.460	7.2339	11.607	3.5902	14.493	4.4756
Wald (IV only), p-value	0.02371	0.05290	7.68×10^{-7}	0.00716	0.00066	0.05814	0.00014	0.03440

IID standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

4.3 Accounting for Heterogeneity

In order to make meaningful policy-related insights, we need to unmask the substantial heterogeneity obscured by the national-level average treatment effects described above. Barring design and data issues with our shift-share instrument, these national-level estimates are unlikely to apply uniformly across states and commuting zones. Therefore, this next section is dedicated to unpacking this heterogeneity. Below, we explore various metrics of local economic growth and decline to (1) partition our sample in a data-driven manner, employ (2) industry-by-industry and (2) state-by-state estimations in our baseline descriptive models and IV specifications using our wage- and GDP-based shift-share instruments.

4.3.1 Declining vs. Growing Regions

First, we identify declining and growing regions by estimating commuting-zone 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...

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

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

...where each GDP term represents the private industry GDP per capita at the CZ, state, or national level, denoted by superscript. We estimate the local growth rate 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. 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. I think this can be supported by interesting literature from the "left behind" and "geographies of discontent" literature in which 'relative' economic performance is what matters most for individuals' happiness. Might be a conceptual leap but an interesting connection? Economically, 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.

We then classify commuting zones by the value of α_g which represents their deviation from state- and national-level GDP growth rates. We estimate this trend deviation in per capita values of private industry GDP. [12] Figure 5 below demonstrates the considerable variability in GDP-level growth rates across commuting zones in the US between 2001-2021. Visualising the per capita growth rate deviations by state and region demonstrates heterogeneity in this variability across states and regions. For example, Texas, Montana, and Colorado have outstanding positive outliers in the distribution whereas Kentucky, Louisiana, South Dakota have outstanding negative outliers. This makes intuitive sense but I should make this more clear with text labels in the plot. I have marked the negatively trending outliers and they are all from Louisiana, Oklahoma, and Wyoming which makes sense. I will make this outlier marking clearer.)

[12] We provide similar analysis of gross GDP in Appendix X.

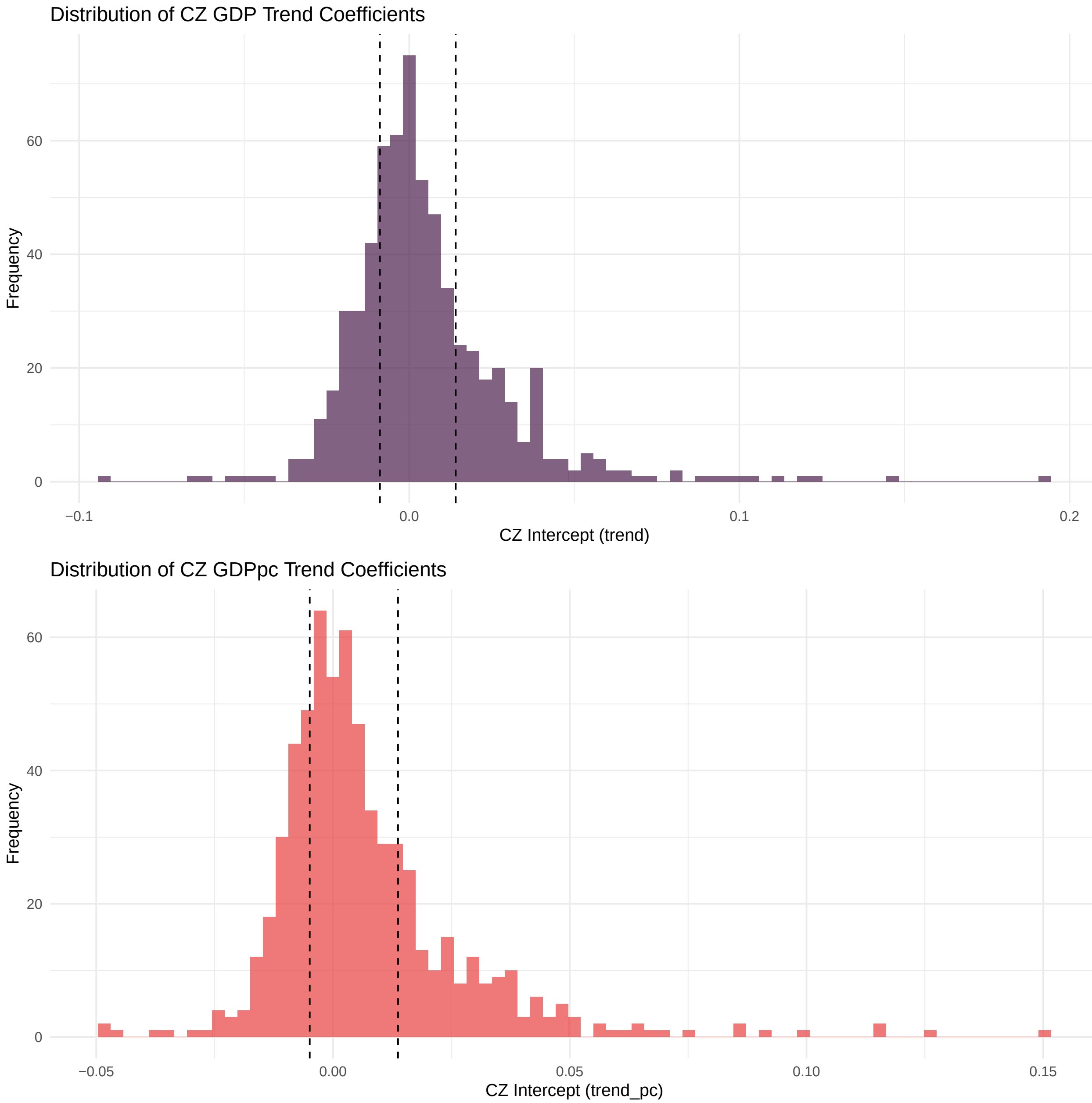


Figure 4: Distribution of GDP Trend Coefficients

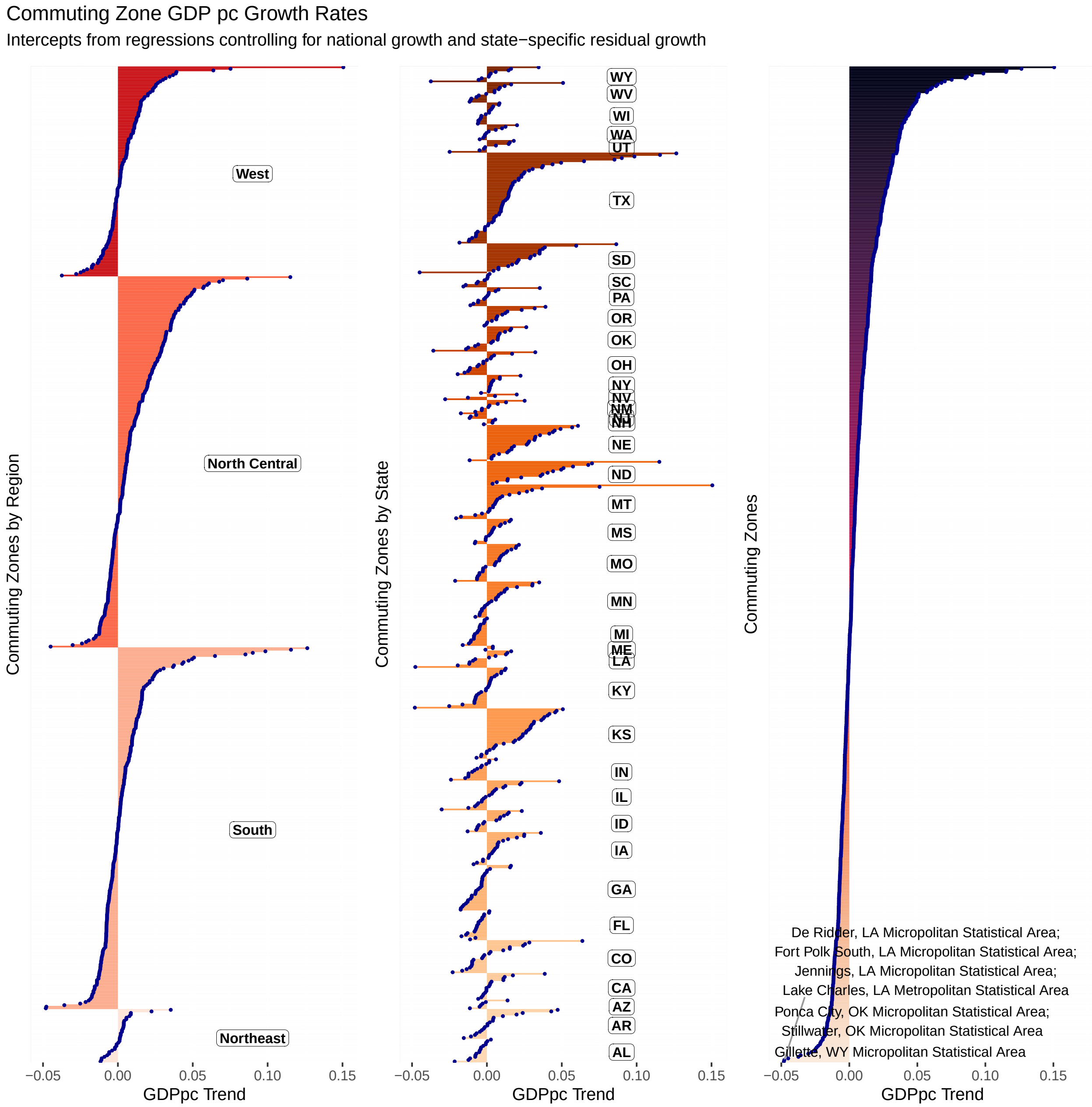


Figure 5: Lollipop Plot of GDPpc Growth Rates

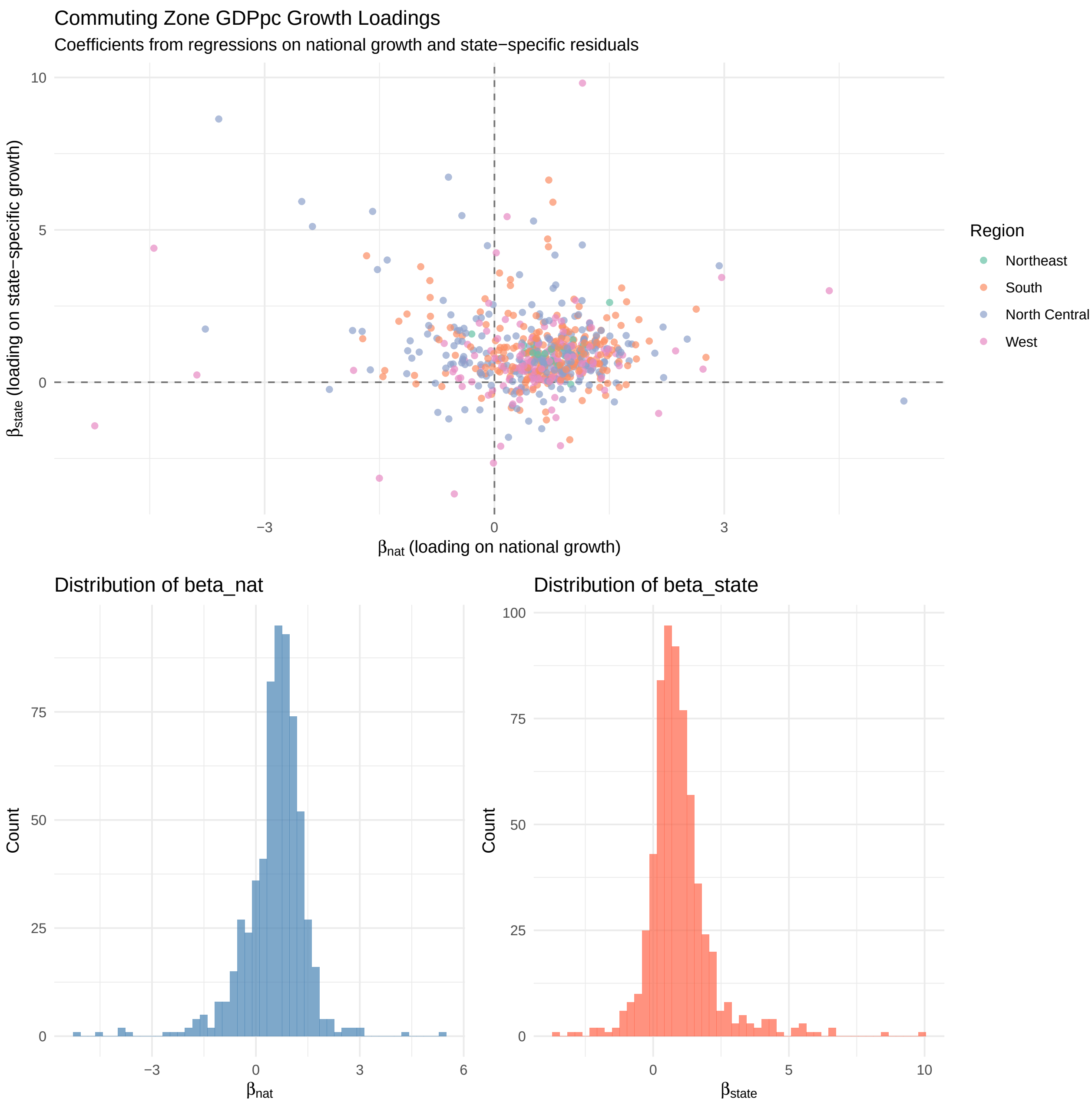


Figure 6: Beta Loadings on GDP pc Growth Rates by CZ

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. We only have national level wage growth data. I have not yet implemented state-level wage growth data. So for now we only use the CZ trend, netting out the national trend. This also explains the missing sections in Figure 13 of the relevant betas.

$$\Delta \log Wage_t^{CZ} = \alpha_w + \beta_n \Delta \log Wage_t^{nat} + \varepsilon_t$$

In Figure 8, we see that there is similar variability though the patterns do not consistently indicate the same high- and low-performing outliers across states indicating that GDP and wage growth are not consistently correlated across regions. We demonstrate this fact in Figure 9 where, although there is a positive correlation between commuting zone GDPpc and wage trend deviations, the wage trend deviation represents a nearly inelastic relationship to GDP growth. Figure 13 presents a correlation coefficient by commuting zone between the two rates, providing greater detail on this relationship. Georgia is an incredibly intereting case in which nearly all cmomuting zones have relatively declinign GDPpc growth rates but relatively growing wage growth rates.

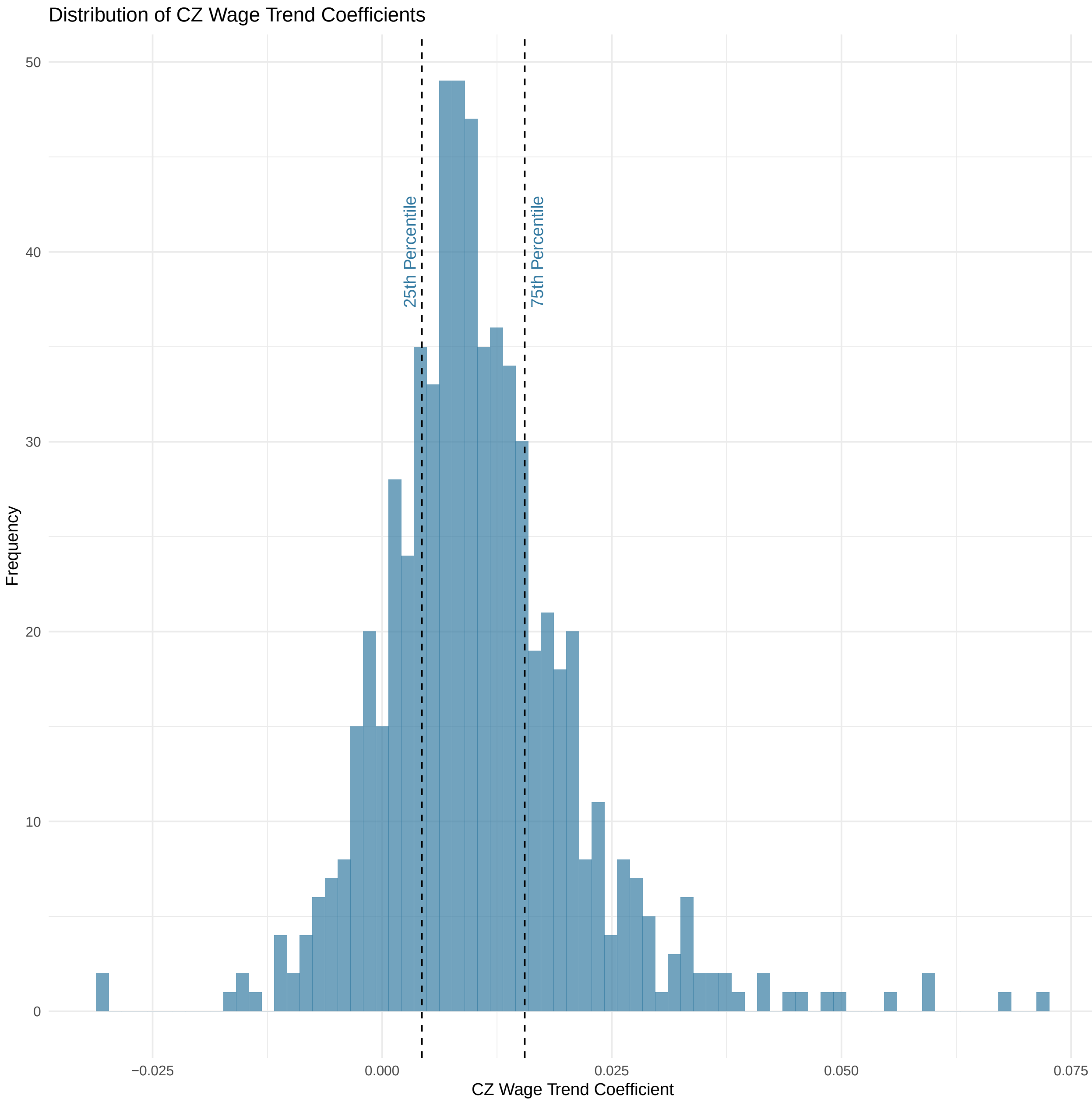


Figure 7: Distribution of Wage Trend Coefficients

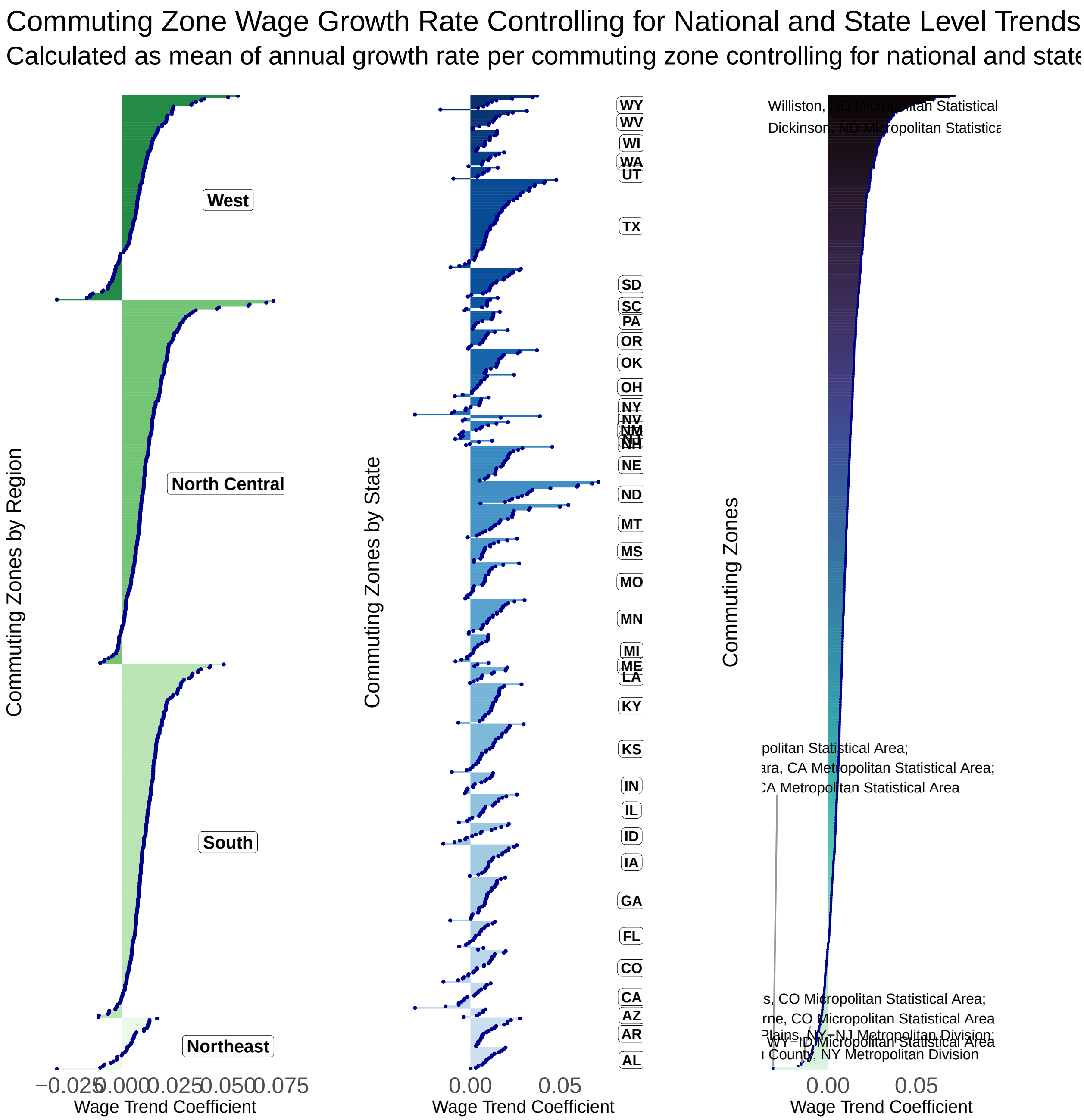


Figure 8: Lollipop Plot of Wage Growth Rates

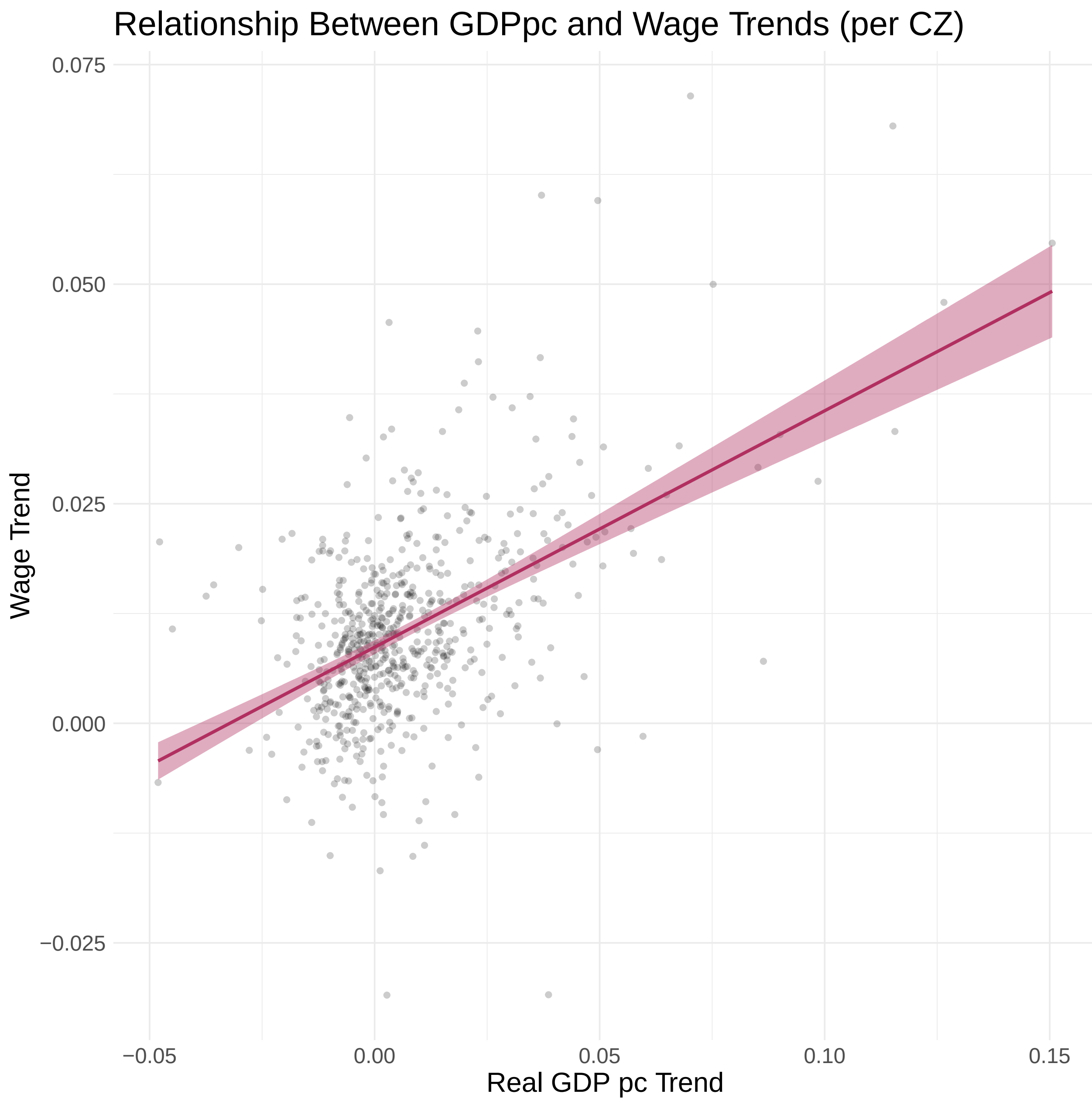


Figure 9: Correlation between Wage and GDPpc Trends

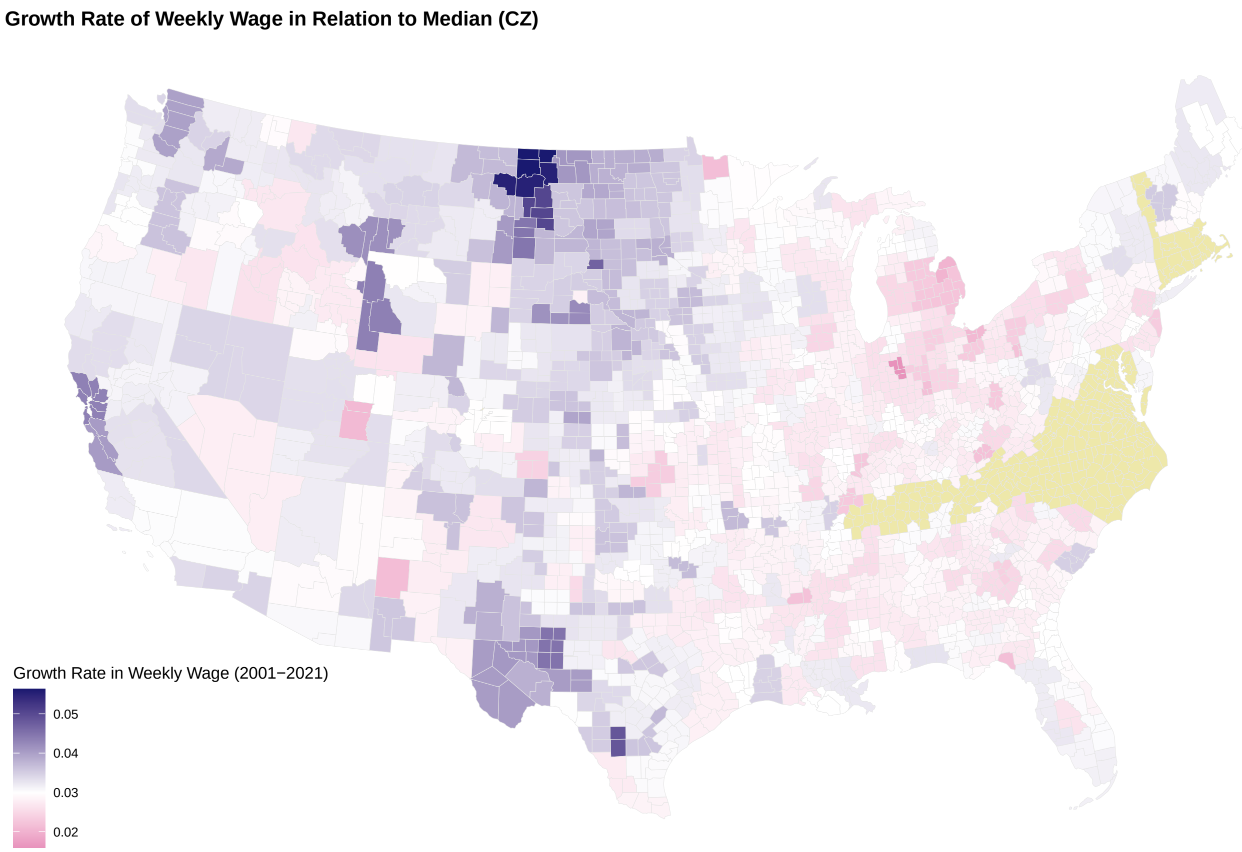


Figure 10: Map of Wage GR - Median

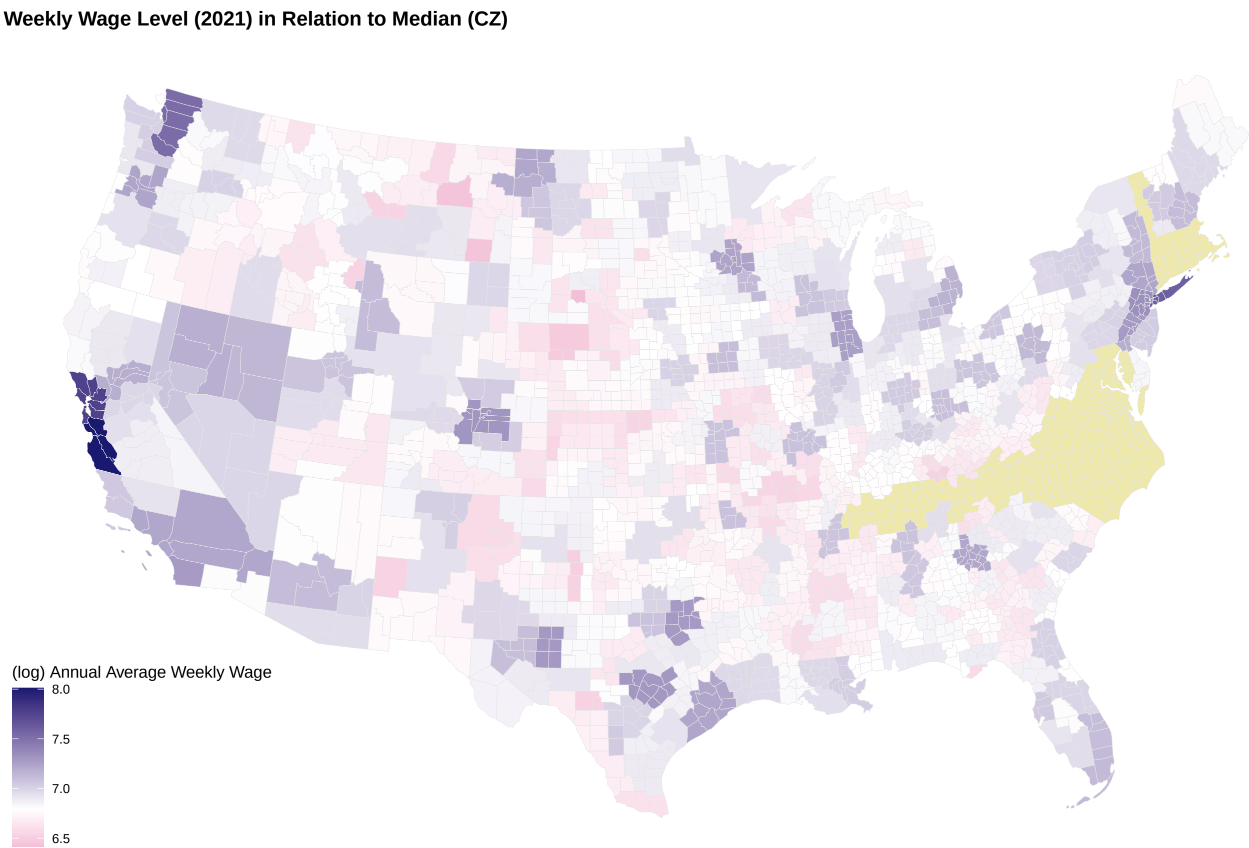


Figure 11: Map of Wage Level - Median

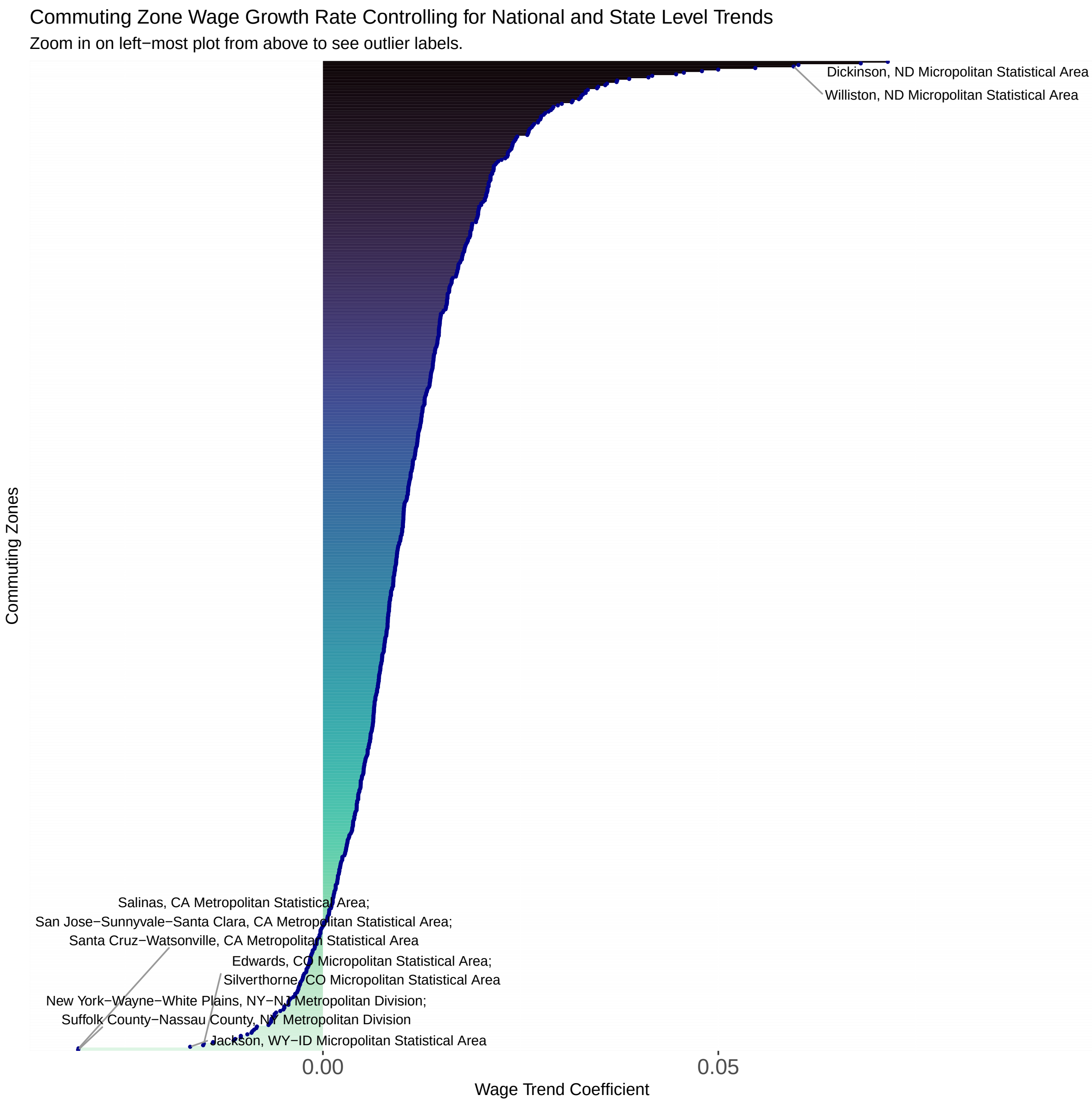
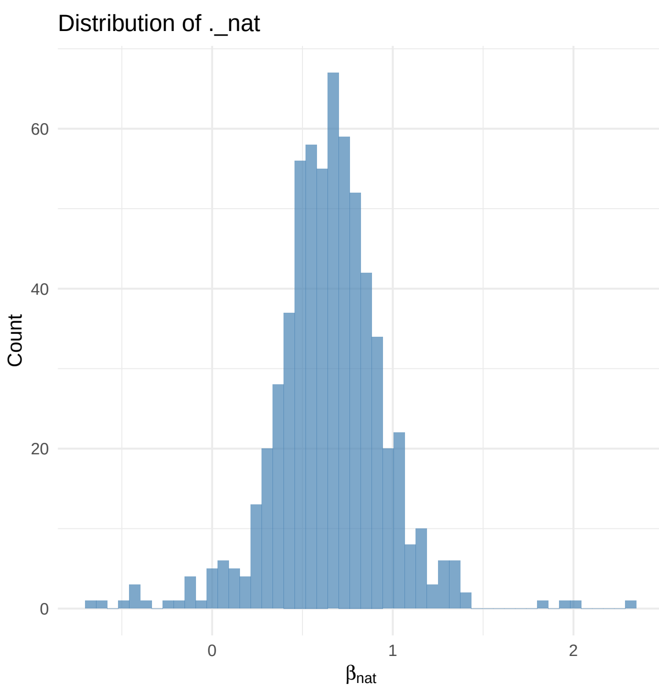


Figure 12: Lollipop Plot of Wage Growth Rates - right panel of Figure 8

Expected error:
Missing state wage growth rates so histogram fails.



Expected error:
Missing state wage growth rates so histogram fails.

Figure 13: Beta Loadings on Wage Growth Rates by CZ

Below, we display, by state, the Pearson correlation coefficient between CZ level GDP growth rates and wage growth rates. Interestingly, many states see nearly exclusively positive correlation coefficients, whereas others see a mix of commuting zones keeping up and lagging local GDP growth.

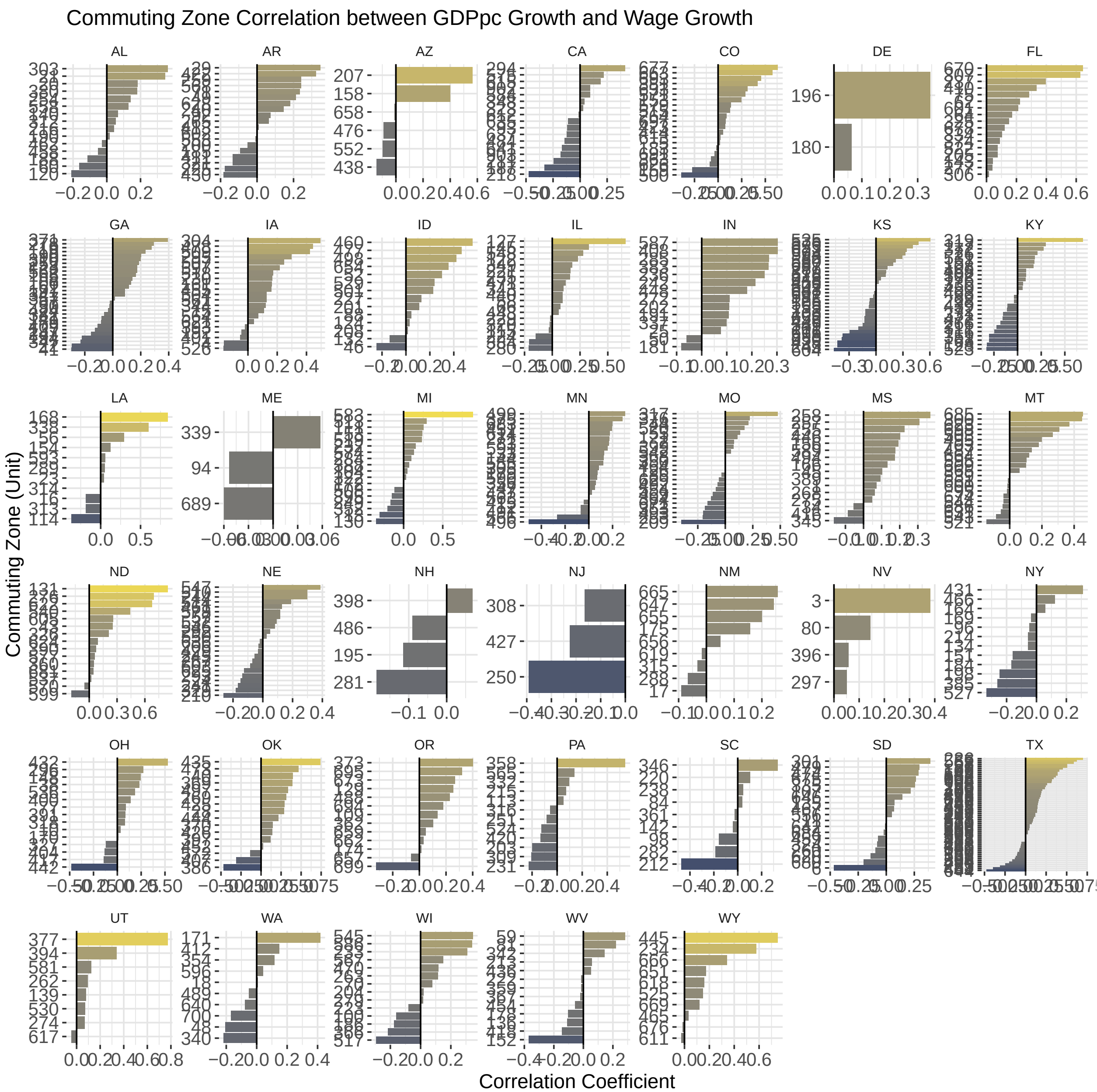


Figure 14: Correlation Between GDP Growth Rates and Wage Growth Rates by State

4.3.2 Baseline Models

Tables 10 and 11 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.

Category	Definition Wage	Definition GDP
Declining	$\alpha_w < 0$	$\alpha_g < 0$
Hyper-Declining	$\alpha_w < P_{25}(\alpha_w)$	$\alpha_g < P_{25}(\alpha_g)$
Growing	$\alpha_w > 0$	$\alpha_g > 0$
Hyper-Growing	$\alpha_w > P_{75}(\alpha_w)$	$\alpha_g > P_{75}(\alpha_g)$

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 10 partitions CZs by long-run GDP per capita trends. Enrollment is consistently negative, confirming that scaling effects apply across all commuting zones, regardless of long-run trends. However, the responsiveness of education spending to local GDP differs slightly across regimes. In declining and hyper-declining areas, GDP coefficients are small and statistically insignificant (except l3), suggesting that output growth in these regions does not translate into higher fiscal capacity or local public investment. By contrast, in growing and hyper-growing CZs, GDP effects become positive and significant-typically 0.07-0.14, indicating that structurally expanding economies reinvest local income gains into education. Lagged wage effects are also stronger in these dynamic regions, implying that education budgets adjust upward with rising labor costs or income-driven demand for schooling. Interpret house price elasticities and l3 of real GDP and Wage growth in declining and hyper-declining regions. The positive effects in growing regions could also indicate more expensive education provision (ie. teacher salaries rise, etc.) which is worth noting as an alternative interpretation that does not serve our narrative.

Table 11 partitions CZs by α_w. The broad patterns are similar, but an important distinction emerges wherein the elasticity of education spending with respect to wages is remarkably consistent across all regional regimes. In both declining and growing CZs, a 1% increase in local wages is associated with roughly a 0.18–0.25 percent increase in per-pupil expenditure, and these coefficients are uniformly significant. This suggests that local wage changes influence education budgets through a consistent channel across local economies, not just in dynamic regions. Hypothesise why this might be...?

Potential interpretation: This asymmetry implies that while the wage–spending elasticity is pervasive, the sources of fiscal responsiveness differ: in expanding regions, wage growth coincdies with broader income and asset growth, amplifying local revenues; in declining regions, wage changes affect spending primarily through higher labor costs rather than expanded fiscal capacity. Overall, the comparison between the GDP- and wage-based partitions reveals two complementary insights. First, local fiscal systems everywhere are sensitive to wage movements. Second, the degree to which broader economic variables (GDP, property values, and transfers) translate into education spending depends on structural growth performance: prosperous regions exhibit stronger links between economic activity and public investment, whereas struggling regions rely more heavily on intergovernmental support. These results highlight that the uniform wage effect coexists with substantial heterogeneity in other transmision channels.

Table 10: 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	0.0263			0.0112			0.0088			-0.0013		
	(0.0330)			(0.0377)			(0.0219)			(0.0278)		
(log,l1) Real GDP Priv. Industry pc	0.0439			0.0415			0.0680***			0.0644***		
	(0.0293)			(0.0332)			(0.0155)			(0.0192)		
(log,l2) Real GDP Priv. Industry pc	0.1157***			0.1106***			0.1399***			0.1739***		
	(0.0293)			(0.0317)			(0.0279)			(0.0332)		
(log) IG Revenue pp	0.3905***	0.3955***	0.3768***	0.3356***	0.3469***	0.3255***	0.3340***	0.2902***	0.3043***	0.3088***	0.2444***	0.2456***
	(0.0555)	(0.0514)	(0.0551)	(0.0670)	(0.0625)	(0.0666)	(0.0361)	(0.0424)	(0.0384)	(0.0520)	(0.0609)	(0.0600)
(log) Enrollment	-0.2997***	-0.2890***	-0.3245***	-0.3433***	-0.3225***	-0.3685***	-0.2930***	-0.3075***	-0.3144***	-0.3250***	-0.3479***	-0.3384***
	(0.0374)	(0.0345)	(0.0378)	(0.0512)	(0.0469)	(0.0519)	(0.0317)	(0.0349)	(0.0382)	(0.0446)	(0.0543)	(0.0702)
(log) Annual Avg. Wkly. Wage		0.0614			0.0115			0.1939**			0.1439	
		(0.0785)			(0.0941)			(0.0777)			(0.1204)	
(log, l1) Annual Avg. Wkly. Wage		0.1626*			0.1894			0.1628***			0.2245**	
		(0.0938)			(0.1330)			(0.0535)			(0.0871)	
(log, l2) Annual Avg. Wkly. Wage		0.4677***			0.4807***			0.2365**			0.3271**	
		(0.1012)			(0.1340)			(0.1033)			(0.1516)	
(log) House Price Index			0.0785**			0.0352			0.1620***			0.2044***
			(0.0330)			(0.0472)			(0.0340)			(0.0575)
(log, l1) House Price Index			0.0907***			0.1589***			0.0365			0.0642
			(0.0301)			(0.0353)			(0.0343)			(0.0514)
(log, l2) House Price Index			0.0227			0.0459			0.0432*			0.0708
			(0.0302)			(0.0411)			(0.0261)			(0.0463)
(log, l3) House Price Index			0.0804**			0.0969*			0.0198			-0.0087
			(0.0384)			(0.0524)			(0.0248)			(0.0352)
(log, l4) House Price Index			-0.0140			-0.0493			-0.0043			-0.0543
			(0.0381)			(0.0487)			(0.0269)			(0.0473)
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	5,544	5,496	3,021	3,339	3,291	7,068	7,812	7,092	3,021	3,339	2,796
R^2	0.85305	0.85739	0.85658	0.84306	0.84996	0.85081	0.86948	0.86249	0.87101	0.81211	0.79761	0.79196
Within R^2	0.29337	0.33568	0.32925	0.25448	0.31138	0.31023	0.29444	0.26447	0.24872	0.31424	0.25178	0.20329

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

Table 11: 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	0.0304			0.0217			0.0099			-0.0134		
	(0.0535)			(0.0408)			(0.0199)			(0.0286)		
(log,l1) Real GDP Priv. Industry pc	0.1269***			0.1402***			0.0632***			0.0490**		
	(0.0358)			(0.0289)			(0.0143)			(0.0195)		
(log,l2) Real GDP Priv. Industry pc	0.0802*			0.0462			0.1525***			0.1792***		
	(0.0444)			(0.0434)			(0.0248)			(0.0339)		
(log) IG Revenue pp	0.3186***	0.3030***	0.3143***	0.4039***	0.3996***	0.3781***	0.3543***	0.3228***	0.3320***	0.2938***	0.2230***	0.2381***
	(0.0670)	(0.0566)	(0.0676)	(0.0549)	(0.0482)	(0.0507)	(0.0324)	(0.0365)	(0.0354)	(0.0516)	(0.0663)	(0.0552)
(log) Enrollment	-0.3328***	-0.3399***	-0.3900***	-0.3460***	-0.3467***	-0.3995***	-0.2881***	-0.2956***	-0.3172***	-0.2625***	-0.2972***	-0.2026***
	(0.0615)	(0.0580)	(0.0667)	(0.0444)	(0.0417)	(0.0431)	(0.0262)	(0.0272)	(0.0292)	(0.0435)	(0.0576)	(0.0553)
(log) Annual Avg. Wkly. Wage		0.0460			0.0711			0.1763***			0.1328	
		(0.1169)			(0.0821)			(0.0674)			(0.1121)	
(log, l1) Annual Avg. Wkly. Wage		0.2621**			0.1982***			0.1756***			0.1738**	
		(0.1229)			(0.0736)			(0.0491)			(0.0756)	
(log, l2) Annual Avg. Wkly. Wage		0.4818***			0.4251***			0.2982***			0.3363**	
		(0.1326)			(0.1070)			(0.0898)			(0.1508)	
(log) House Price Index			0.1085**			0.0849*			0.1501***			0.1631***
			(0.0526)			(0.0442)			(0.0296)			(0.0594)
(log, l1) House Price Index			0.1056*			0.1252**			0.0507*			0.0065
			(0.0573)			(0.0502)			(0.0284)			(0.0531)
(log, l2) House Price Index			0.0296			0.0137			0.0493**			0.0402
			(0.0530)			(0.0376)			(0.0233)			(0.0470)
(log, l3) House Price Index			-0.0100			0.0084			0.0457*			-0.0035
			(0.0558)			(0.0350)			(0.0234)			(0.0372)
(log, l4) House Price Index			0.0864*			0.0463			-0.0013			0.0397
			(0.0460)			(0.0351)			(0.0238)			(0.0474)
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	1,680	1,593	3,021	3,339	3,203	10,564	11,676	10,995	3,021	3,339	2,867
R^2	0.90202	0.90302	0.89766	0.87794	0.87767	0.87846	0.85732	0.85133	0.85669	0.86401	0.84668	0.85164
Within R^2	0.30517	0.34695	0.33101	0.33808	0.35539	0.36751	0.31081	0.29539	0.28817	0.31419	0.24337	0.16605

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

4.3.3 IV Models

When applying our instrumental variable design we find that majority of our relevant signal is picked up in regions across both trend metrics. Though the effects are more consistent across wage-trending sub-samples Not yet sure how to interpret.

Table 12: Wage-based Shift-Share Instrument (l1) Applied to Declining GDP vs. Growing GDP Regions

Dependent Variable:	(log) Elem.Ed.Exp.pp				
	All	Declining (GDP)	Hyper-Declining (GDP)	Growing (GDP)	Hyper-Growing (GDP)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
(log) Annual Avg. Wkly. Wage	2.040^{***}	2.836^{***}	6.026^{***}	1.483^{*}	0.7320^{**}
	(0.2251)	(0.2850)	(1.578)	(0.7693)	(0.3127)
(log) IG Revenue pp	0.2274^{***}	0.1581^{***}	0.0759	0.2749^{***}	0.3102^{***}
	(0.0102)	(0.0180)	(0.0516)	(0.0188)	(0.0152)
(log) Real GDP Priv. Industry pc	-0.1879^{***}	-0.4687^{***}	-1.264^{***}	-0.0615	0.0797^{*}
	(0.0413)	(0.0702)	(0.3871)	(0.1203)	(0.0449)
(log) Enrollment	-0.1739^{***}	-0.2010^{***}	-0.4166^{***}	-0.1473^{**}	-0.1197^{***}
	(0.0158)	(0.0175)	(0.1023)	(0.0574)	(0.0252)
Fixed-effects					
year	Yes	Yes	Yes	Yes	Yes
state	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	12,720	5,280	3,180	7,440	3,180
F-test (IV only)	205.41	443.58	221.26	5.8864	5.8989
F-test (IV only), p-value	3.16×10^{-46}	1.41×10^{-94}	2.1×10^{-48}	0.01528	0.01521
Wu-Hausman	161.95	411.00	214.68	3.8050	2.8964
Wu-Hausman, p-value	7.13×10^{-37}	5.07×10^{-88}	4.61×10^{-47}	0.05114	0.08888
Wald (IV only)	82.169	99.040	14.576	3.7166	5.4800
Wald (IV only), p-value	1.43×10^{-19}	3.97×10^{-23}	0.00014	0.05391	0.01930

IID standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 13: Wage-based Shift-Share Instrument (l1) Applied to Declining Wage vs. Growing Wage Regions

Dependent Variable:	(log) Elem.Ed.Exp.pp				
	All	Declining (Wage)	Hyper-Declining (Wage)	Growing (Wage)	Hyper-Growing (Wage)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
(log) Annual Avg. Wkly. Wage	2.040***	1.839***	3.567***	1.513***	0.2600
	(0.2251)	(0.4215)	(0.5852)	(0.1512)	(0.1773)
(log) IG Revenue pp	0.2274***	0.2669***	0.2611***	0.2487***	0.2807***
	(0.0102)	(0.0236)	(0.0270)	(0.0087)	(0.0113)
(log) Real GDP Priv. Industry pc	-0.1879***	-0.3624***	-0.6539***	-0.0813***	0.1590***
	(0.0413)	(0.1331)	(0.1484)	(0.0262)	(0.0283)
(log) Enrollment	-0.1739***	-0.1425***	-0.2610***	-0.1408***	-0.0814***
	(0.0158)	(0.0262)	(0.0376)	(0.0107)	(0.0144)
Fixed-effects					
year	Yes	Yes	Yes	Yes	Yes
state	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	12,720	1,600	3,180	11,120	3,180
F-test (IV only)	205.41	39.088	227.94	174.88	2.1719
F-test (IV only), p-value	3.16×10^{-46}	5.23×10^{-10}	9.19×10^{-50}	1.27×10^{-39}	0.14066
Wu-Hausman	161.95	27.150	197.87	133.40	0.90598
Wu-Hausman, p-value	7.13×10^{-37}	2.14×10^{-7}	1.28×10^{-43}	1.11×10^{-30}	0.34126
Wald (IV only)	82.169	19.028	37.152	100.09	2.1494
Wald (IV only), p-value	1.43×10^{-19}	1.37×10^{-5}	1.23×10^{-9}	1.83×10^{-23}	0.14272

IID standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 14: GDP-based Shift-Share Instrument (l1) Applied to Declining Wage vs. Growing Wage Regions

Dependent Variable:	(log) Elem.Ed.Exp.pp				
	All	Declining (Wage)	Hyper-Declining (Wage)	Growing (Wage)	Hyper-Growing (Wage)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
(log) Annual Avg. Wkly. Wage	3.154***	-1.071*	-5.546***	1.454***	-0.8508**
	(0.7025)	(0.5532)	(2.066)	(0.2103)	(0.4169)
(log) IG Revenue pp	0.1990***	0.1713***	0.1281**	0.2503***	0.3094***
	(0.0215)	(0.0261)	(0.0531)	(0.0094)	(0.0165)
(log) Real GDP Priv. Industry pc	-0.3903***	0.5453***	1.631***	-0.0713**	0.3330***
	(0.1279)	(0.1738)	(0.5195)	(0.0362)	(0.0657)
(log) Enrollment	-0.2516***	0.0363	0.3208**	-0.1367***	0.0075
	(0.0491)	(0.0342)	(0.1321)	(0.0148)	(0.0335)
Fixed-effects					
year	Yes	Yes	Yes	Yes	Yes
state	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	12,720	1,600	3,180	11,120	3,180
F-test (IV only)	99.839	7.3040	125.12	79.647	6.0949
F-test (IV only), p-value	2.02×10^{-23}	0.00696	1.66×10^{-28}	5.18×10^{-19}	0.01361
Wu-Hausman	86.779	14.113	149.50	59.598	7.5920
Wu-Hausman, p-value	1.41×10^{-20}	0.00018	1.29×10^{-33}	1.26×10^{-14}	0.00590
Wald (IV only)	20.156	3.7494	7.2046	47.788	4.1644
Wald (IV only), p-value	7.2×10^{-6}	0.05301	0.00731	5.01×10^{-12}	0.04137

IID standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 15: GDP-based Shift-Share Instrument (l1) Applied to Declining GDP vs. Growing GDP Regions

Dependent Variable:	(log) Elem.Ed.Exp.pp				
	All	Declining (GDP)	Hyper-Declining (GDP)	Growing (GDP)	Hyper-Growing (GDP)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
(log) Annual Avg. Wkly. Wage	3.154^{***}	19.24	-49.53	-1.110	1.560^{***}
	(0.7025)	(13.33)	(114.1)	(1.243)	(0.5382)
(log) IG Revenue pp	0.1990^{***}	-0.2303	1.281	0.3300^{***}	0.2834^{***}
	(0.0215)	(0.3321)	(2.495)	(0.0283)	(0.0223)
(log) Real GDP Priv. Industry pc	-0.3903^{***}	-4.438	12.31	0.3436^{*}	-0.0380
	(0.1279)	(3.227)	(27.88)	(0.1943)	(0.0769)
(log) Enrollment	-0.2516^{***}	-1.193	3.172	0.0462	-0.1860^{***}
	(0.0491)	(0.8065)	(7.371)	(0.0928)	(0.0432)
Fixed-effects					
year	Yes	Yes	Yes	Yes	Yes
state	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	12,720	5,280	3,180	7,440	3,180
F-test (IV only)	99.839	352.27	185.59	1.5183	13.745
F-test (IV only), p-value	2.02×10^{-23}	4.16×10^{-76}	4.26×10^{-41}	0.21792	0.00021
Wu-Hausman	86.779	350.06	187.37	2.6451	10.278
Wu-Hausman, p-value	1.41×10^{-20}	1.17×10^{-75}	1.84×10^{-41}	0.10391	0.00136
Wald (IV only)	20.156	2.0826	0.18848	0.79729	8.4064
Wald (IV only), p-value	7.2×10^{-6}	0.14905	0.66422	0.37193	0.00377

IID standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

4.3.4 State-by-state estimation

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

First, states vary in the number of commuting zones they contain. Figure 15 demonstrates that states contain anywhere from 2 (Delaware) - 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 (of course, these should be interpreted with caution in those states that contain very few commuting zones).

Second, given our shift-share instruments are the composite effect of shifts in industry-level wages and real value added, we can decompose this instrument into industry-specific shocks wage and 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, our instrument is...

$$\tilde{Z}_{ijt} = G_{njt} * \frac{N_{ij\tau}}{N_{i\tau}} \tag{9}$$

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

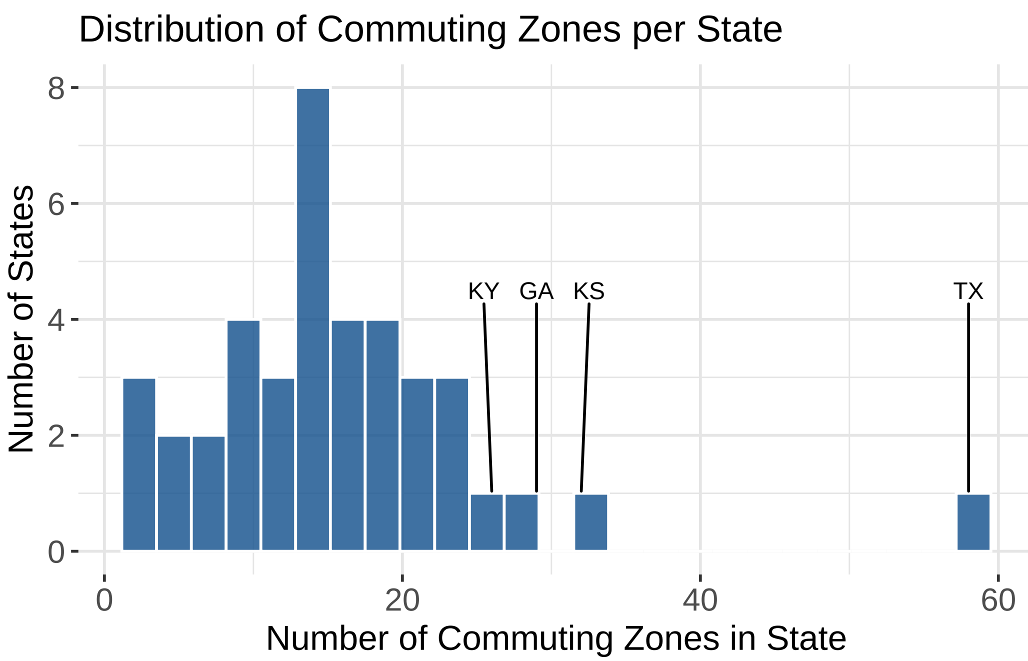


Figure 15: Histogram: Commuting Zones by State

4.3.5 Industry by Industry

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

Using our value added-based shift share instrument, Figure 15 demonstrates the overall treatment effect of local wage changes instrumented via an industry-specific GDP shock.

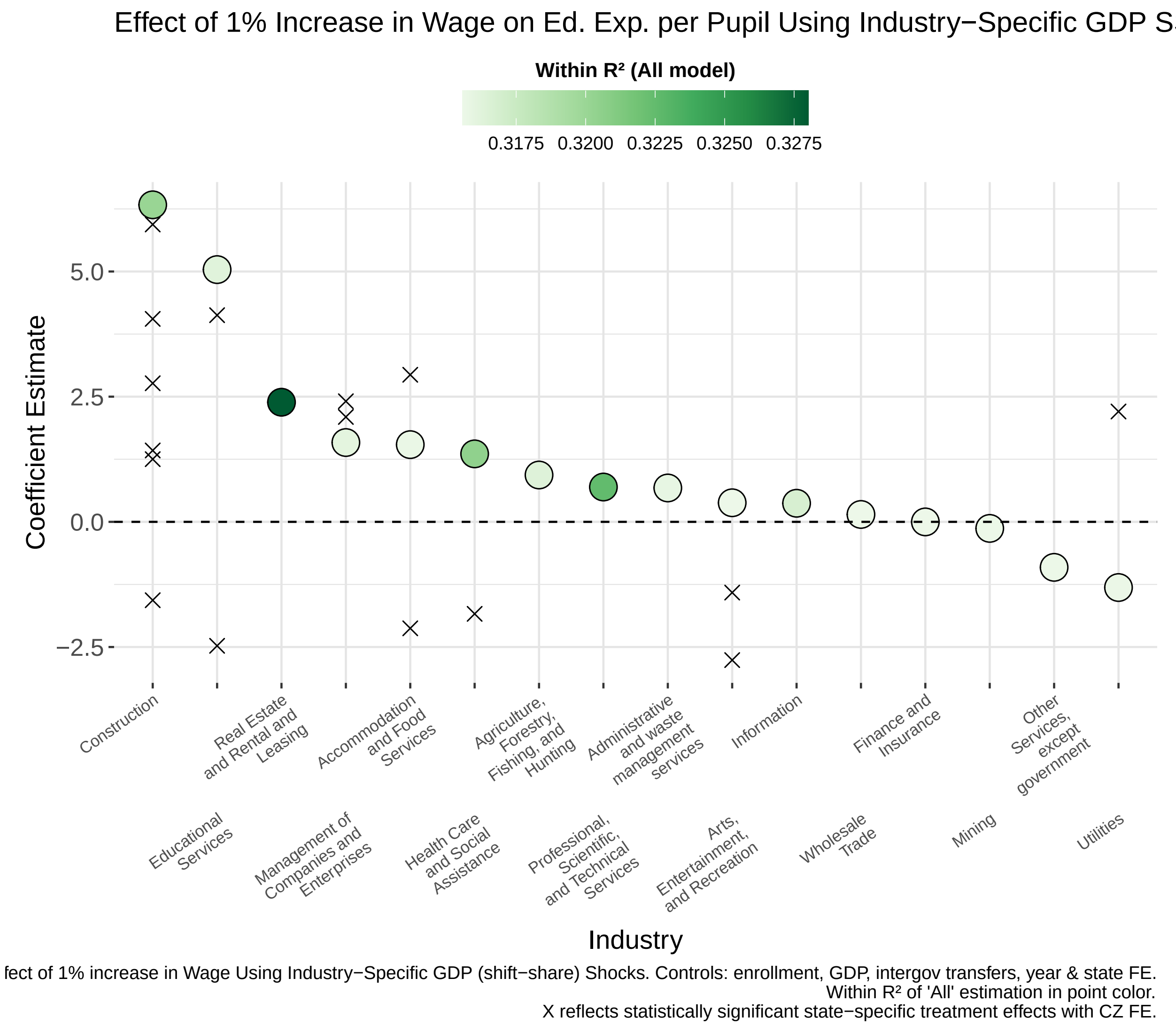


Figure 16: Wage Effect via Industry VA SS Shock

4.3.5.1 Wage

Using our wage-based shift share instrument, Figure 17 demonstrates the overall treatment effect of local wage changes instrumented via our wage-based shift-share shock.

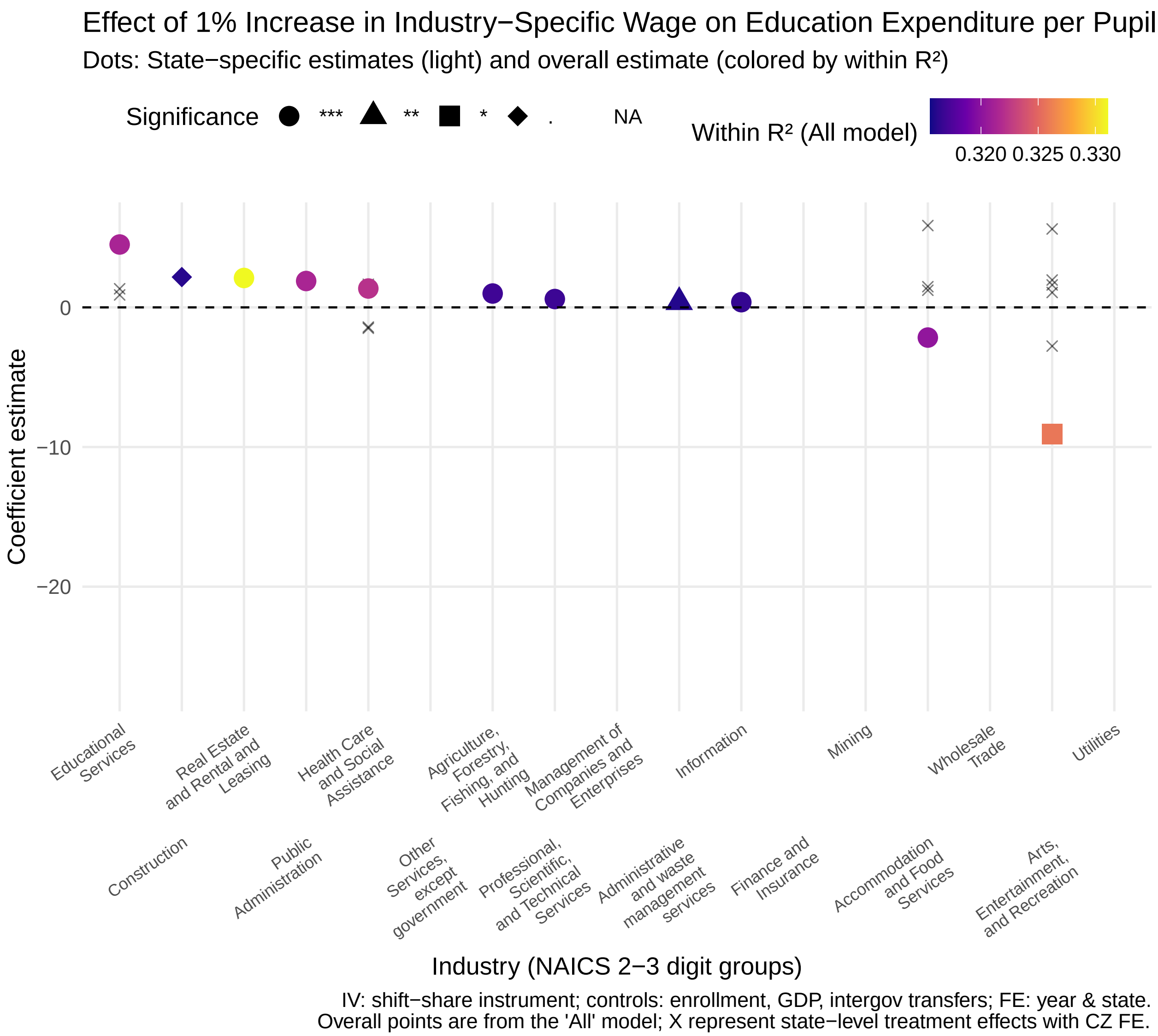


Figure 17: Wage Effect via Industry Wage SS Shock

Across both, we find consistently positive effects when using shift-share shocks from the construction; real estate, rental and leasing; agriculture, forestry, and fishing; professional, scientific and technical services industries.

However, for other industries we find opposite effects depending on the instrument shock chosen.

Jennie: Again, interpretation of the relationship between the instrument and the outcome is shady here...might it be worth applying this outside of an IV framework, arguing that the shift-share instrument is in itself exogenous.

Next, we turn to state-by-state estimation, the regression coefficients of which are reported in Figure 18. First, we re-employ our baseline regression in which we regress our outcome variable on wage levels at time $t-h$ where $h = [t-0, t-2]$. 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 betas on each time lag (ie. the linearly combined coefficients that form the total dynamic effect). Majority of states see positive elasticities in relation to wage changes in this descriptive specification, but some (Kansas, Wyoming, Missouri, Washington) see negative elasticities. I think we should have a commuting zone N sample size exclusion restriciton here. The results for Maine, New Jersey, and Delaware, for example are based on only 2-3 units.

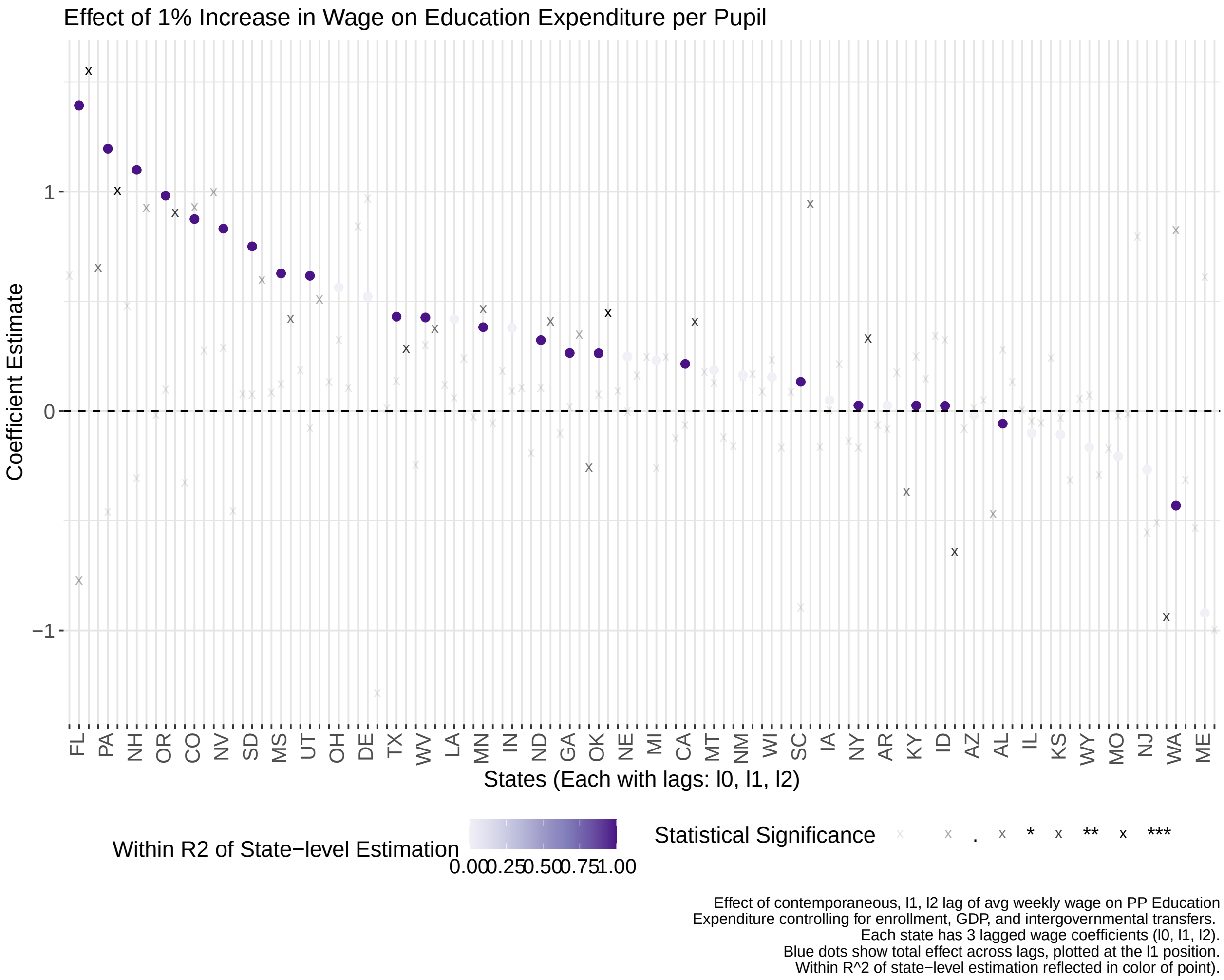


Figure 18: State-by-State Wage Effect - Descriptive

We then proceed by instrumenting local wage levels with our wage- and real-value added based instruments by state. Again, the choice of instrument matters crucially. We see greatest consistency between the descriptive plot (Figure 18) and the wage-based instrument plot (Figure 20), whereas the GDP-based shift-share instrument (Figure 19) provides different wage elasticities, notably even in sign.

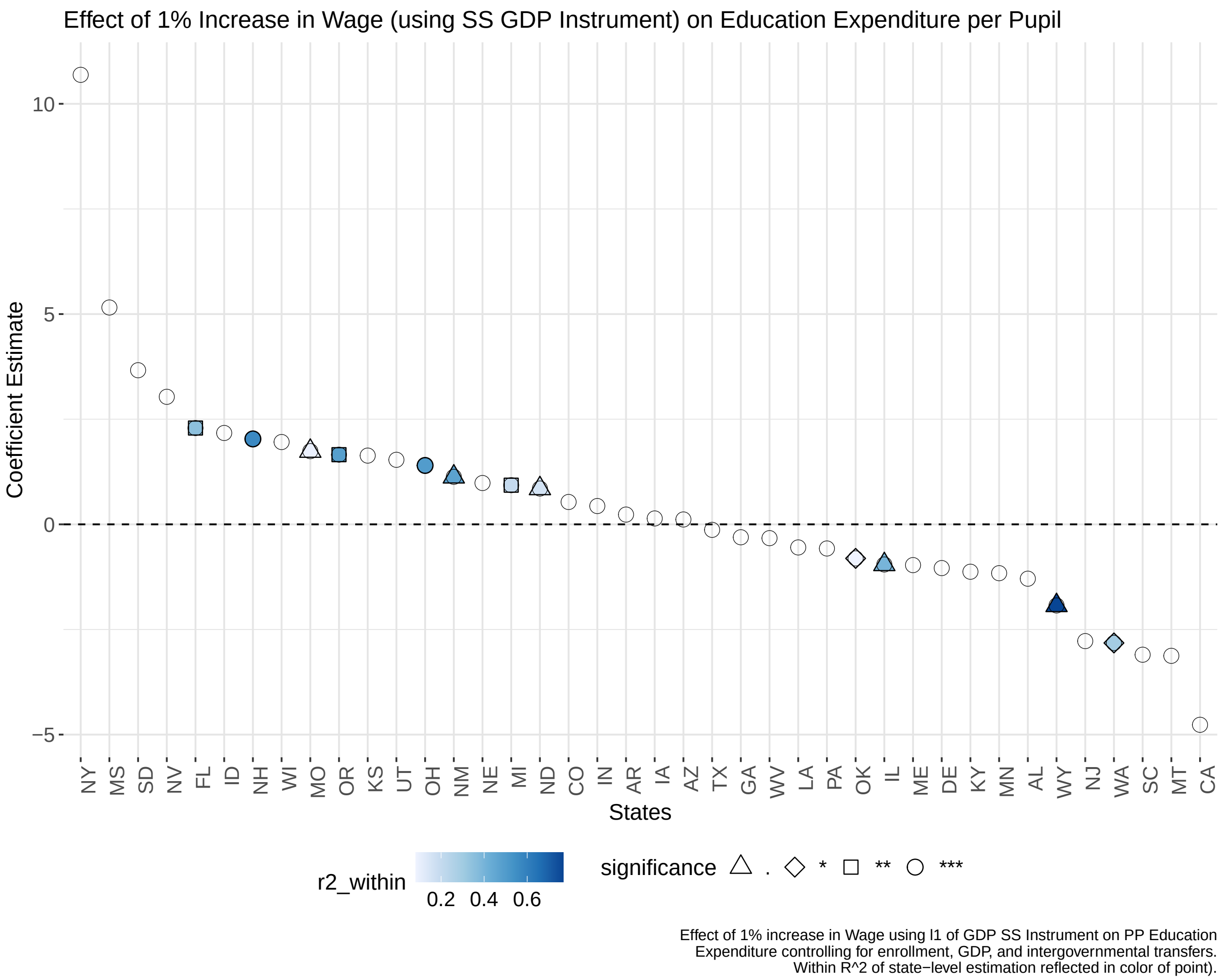


Figure 19: State-by-State Wage Effect Using SS GDP Shock

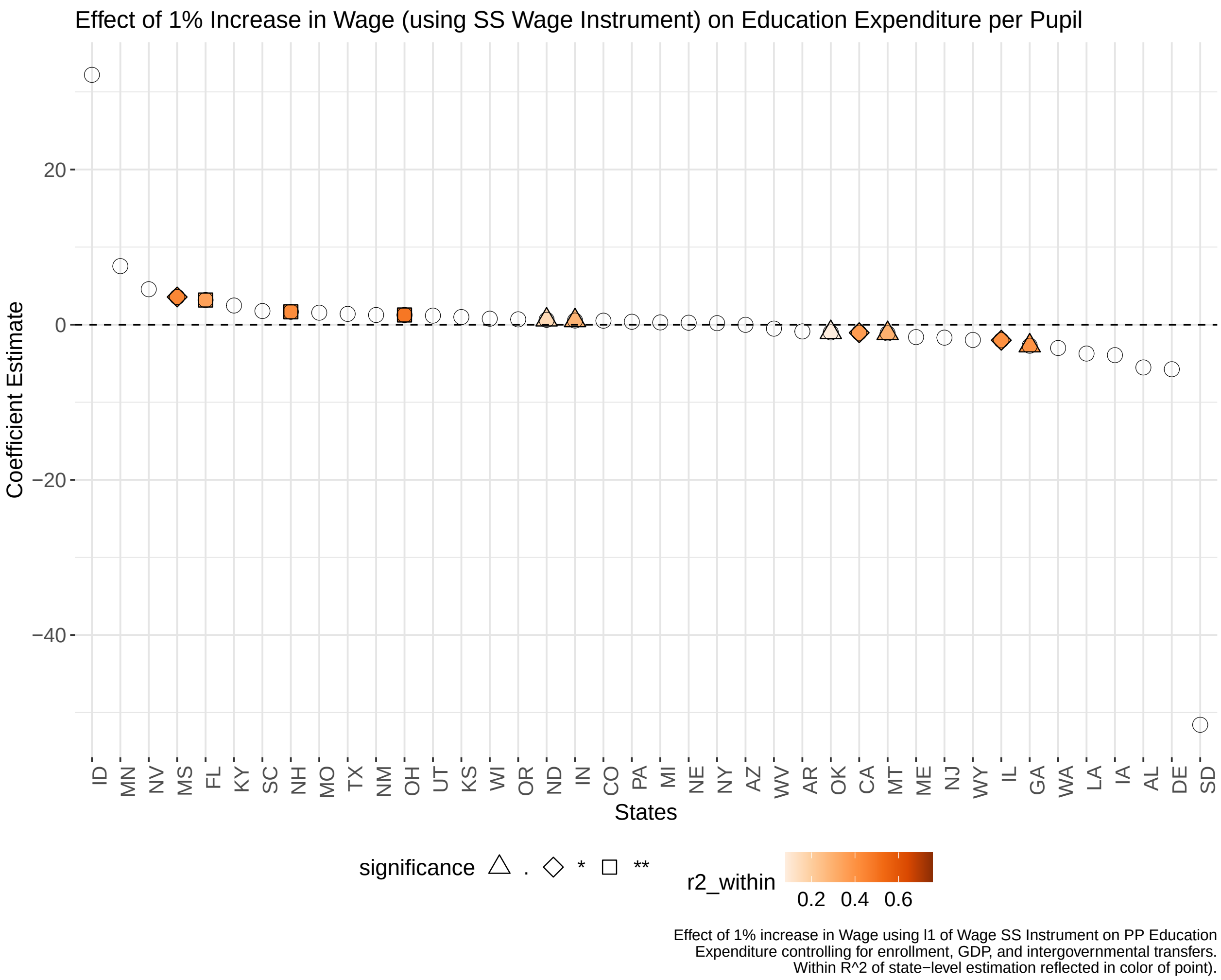


Figure 20: State-by-State Wage Effect Using SS Wage Shock

4.4 Additional Analysis

4.4.1 Quantile Regression

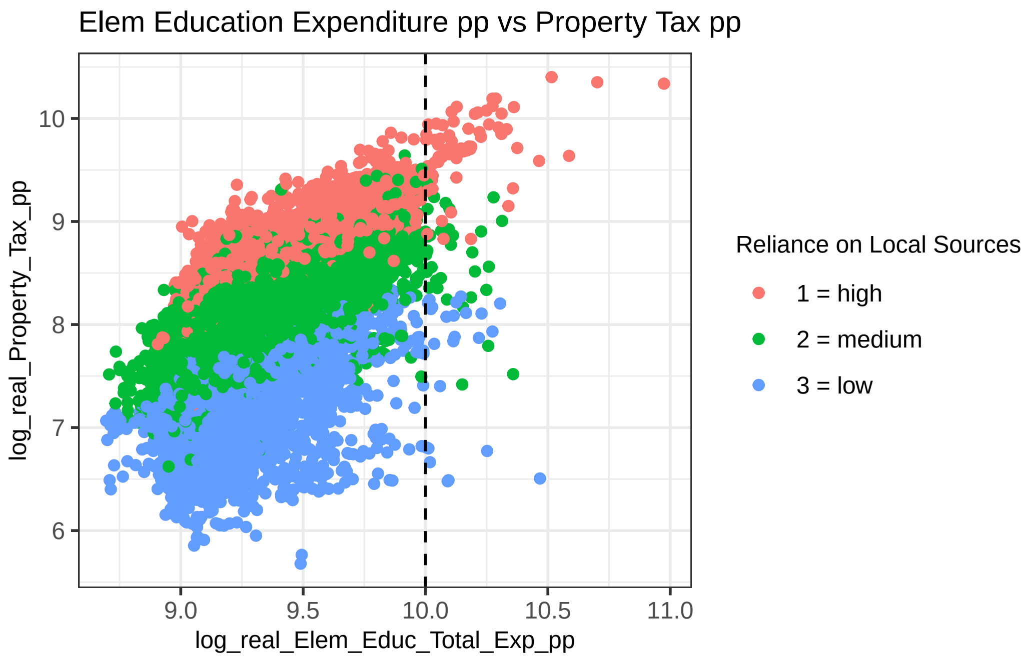
Figure 21: Quantile Regression

4.4.2 High-income Outliers

This will likely be moved to the Appendix.

There is a somewhat non-linear relationship between property taxes and elementary expenditure as property taxes collected rise as represented in Figure 22 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). This could benefit from more robust outlier detection.

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 GDP-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	GDP-based SS
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
(log) Real GDP Priv. Industry pc	0.0130			-0.1879***	-0.3903***
	(0.0187)			(0.0413)	(0.1279)
(log,l1) Real GDP Priv. Industry pc	0.0691***				
	(0.0135)				
(log,l2) Real GDP Priv. Industry pc	0.1457***				
	(0.0231)				
(log) IG Revenue pp	0.3512***	0.3220***	0.3287***	0.2274***	0.1990***
	(0.0295)	(0.0328)	(0.0318)	(0.0102)	(0.0215)
(log) Enrollment	-0.2936***	-0.3022***	-0.3297***	-0.1739***	-0.2516***
	(0.0241)	(0.0247)	(0.0270)	(0.0158)	(0.0491)
(log) Annual Avg. Wkly. Wage		0.1706***		2.040***	3.154***
		(0.0600)		(0.2251)	(0.7025)
(log, l1) Annual Avg. Wkly. Wage		0.1767***			
		(0.0459)			
(log, l2) Annual Avg. Wkly. Wage		0.3169***			
		(0.0796)			
(log) House Price Index			0.1450***		
			(0.0256)		
(log, l1) House Price Index			0.0557**		
			(0.0263)		
(log, l2) House Price Index			0.0481**		
			(0.0208)		
(log, l3) House Price Index			0.0447**		
			(0.0210)		
(log, l4) House Price Index			0.0024		
			(0.0215)		
Fixed-effects					
unit	Yes	Yes	Yes		
year	Yes	Yes	Yes	Yes	Yes
state				Yes	Yes
Fit statistics					
Observations	12,084	13,356	12,588	12,720	12,720
R2 (1st stage)	0.86608	0.86135	0.86500		
Adj. R2 (1st stage)	0.85836	0.85414	0.85784		
F-test (IV only)				205.41	99.839
F-test (IV only), p-value				3.16×10^{-46}	2.02×10^{-23}
Wu-Hausman				161.95	86.779
Wu-Hausman, p-value				7.13×10^{-37}	1.41×10^{-20}
Wald (IV only)				82.169	20.156
Wald (IV only), p-value				1.43×10^{-19}	7.2×10^{-6}

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

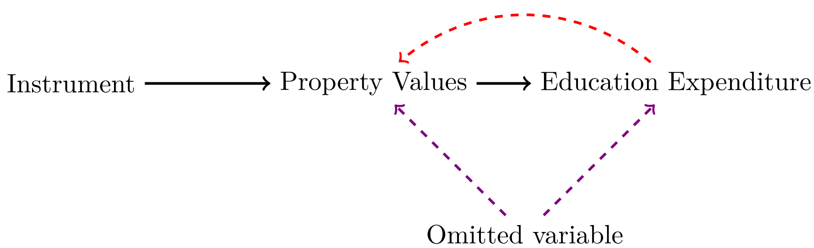
4.4.3 Property Prices as Endogenous Regressor

Until a few weeks ago, I was using the following identification strategy which produced perhaps more interesting results (in terms of instrument relevance and statistically significant results. However, it did not answer our fundamental research question of the relationship between local economic performance and education expenditure. They provide evidence of a causal relationship between property values and public education expenditure using the same shift-share instruments as above. I include the results of this work at the end of the document, for reference. In case there is something useful there for the narrative.

The structure of public financing (described in further detail in Section A of the Supplementary Materials) provides an avenue for a causal identification strategy. In brief, although revenue for public education comes from a combination of intergovernmental and local sources; revenue generated from local sources comes almost entirely from property taxes. Given this, we can isolate the channel through which our treatment (industry-specific wage growth) will affect our outcome variable using an instrumental variable approach. We outline the underlying path diagram of this econometric specification in Figure 22.

As seen in Figure 22, we hypothesize that property values have an effect on education expenditure. However, there is significant concern of a reverse causal effect as higher income families likely gravitate towards school districts with higher levels of expenditure per pupil, driving up property values (Could likely add sources here).

Figure 22: Instrumental Variable Path Diagram



Therefore, we adopt an identification strategy via a shift-share or Bartik instrument. A shift-share instrument interacts local industry shares with national industry-level growth rates to attain a plausibly exogenous local shock. In the context of this work, we construct the instrument by interacting a constant industrial employment share variable with a national industry-level wage.

We choose to employ the first of these options, assuming that industry shares are only exogenous at a given base period and that national level growth rates are exogenous and therefore allowed to vary with time.

Using data from the US Bureau of Labor Statistics' Quarterly Census of Employment and Wages (QCEW) and Bureau of Economic Analysis, we construct two types of shift-share Bartik instruments at the commuting zone level using local employment shares by industry and national changes in industry-specific wages and real value added. Equation Equation 10 demonstrates the Bartik instrument as outlined in Ferri (2022) and Goldsmith-Pinkham, Sorkin, and Swift (2020) and defined in Bartik (1991) . G_{njt} represents national-level changes in wages or 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'.

[13]

$$\tilde{Z}_{ijt} = G_{njt} * \frac{N_{ij\tau}}{N_{i\tau}} \tag{10}$$

We compute the two relevant shift-share instruments across 19 two-digit NAICS industrial categories listed in Table X below. Given industry-level disaggregation of local employment and wage data requires data

[13] 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. I have done this unsystematically so far (testing 2001, 2004, and 2005) but arrived at the decision to compute an average to deal with missing data. Will include a more systematic testing of this in the appendix.

suppression for anonymity reasons, the plot immediately following displays the data coverage of our commuting zone level shift-share instruments. Given the high degree of missingness in the 3-digit categorisation we proceed with the 2-digit NAICS codes in the rest of the work.

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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

Before displaying the regression results using our shift-share instrument we discuss the plausibility of our identification strategy by exploring the relationships between wage and GDP on housing prices. We provide evidence of this relationship below.

4.4.3.1 Industry-level Wages

The two regression models show that wage levels and wage growth both play important roles in influencing house prices, but in different ways. The log-level model indicates that increases in average wages have a strong and persistent effect on the level of house prices, with significant positive effects extending up to four years. In contrast, the growth rate model suggests that house price growth responds primarily to contemporaneous wage growth, with little evidence of lagged effects. Together, these findings imply that while higher wages steadily raise housing values over time, short-term changes in wage growth do not affect the growth rate of house prices over time. Together, they suggest that housing markets are more responsive to trends than to transitory shocks in wages. I am unsure about this interpretation - given GR regressed on GR is simply meant to handle non-stationarity issues...I worry that the GR results might just indicate that the level regression is meaningless?

Dependent Variables:	(log) House Price Index		(GR) House Price Index	
Model:	(1)	(2)	(3)	(4)
Variables				
(log) Annual Avg. Wkly. Wage	0.9365***	0.7937***		
	(0.0208)	(0.0232)		
(log, l1) Annual Avg. Wkly. Wage	0.0544***	-0.0415***		
	(0.0168)	(0.0119)		
(log, l2) Annual Avg. Wkly. Wage	0.0861***	-0.0936***		
	(0.0162)	(0.0118)		
(log, l3) Annual Avg. Wkly. Wage	0.0303*	0.0374***		
	(0.0161)	(0.0117)		
(log, l4) Annual Avg. Wkly. Wage	0.0863***	-0.0269**		
	(0.0161)	(0.0118)		
(log, l5) Annual Avg. Wkly. Wage	0.0261	0.0410***		
	(0.0166)	(0.0118)		
(log, l6) Annual Avg. Wkly. Wage	0.0388**	0.0060		
	(0.0161)	(0.0120)		
(log, l7) Annual Avg. Wkly. Wage	0.0222	0.0369***		
	(0.0163)	(0.0118)		
(log) Real GDP Priv. Industry pc	0.0710***	-0.0188**		
	(0.0066)	(0.0076)		
(log) Population	0.4165***	0.1001***		
	(0.0149)	(0.0023)		
(GR) Annual Avg. Wkly. Wage			0.3164***	0.3236***
			(0.0208)	(0.0203)
(GR, l1) Annual Avg. Wkly. Wage			0.0040	0.0056
			(0.0193)	(0.0187)
(GR, l2) Annual Avg. Wkly. Wage			0.0242	0.0252
			(0.0191)	(0.0185)
(GR, l3) Annual Avg. Wkly. Wage			0.0035	0.0073
			(0.0190)	(0.0184)
(GR, l4) Annual Avg. Wkly. Wage			0.0263	0.0292
			(0.0191)	(0.0185)
(GR, l5) Annual Avg. Wkly. Wage			6.34×10^{-5}	0.0002
			(0.0192)	(0.0186)
(GR, l6) Annual Avg. Wkly. Wage			-0.0118	-0.0073
			(0.0192)	(0.0186)
(GR, l7) Annual Avg. Wkly. Wage			-0.0093	-0.0091
			(0.0193)	(0.0186)
(GR) Real GDP Priv. Industry pc			0.0078**	0.0080**
			(0.0033)	(0.0032)
(GR) Population			0.0063***	0.0066***
			(0.0011)	(0.0010)
Fixed-effects				
unit	Yes		Yes	
year	Yes	Yes	Yes	Yes
state		Yes		Yes
Fit statistics				
Observations	12,570	12,570	12,542	12,543
R^2	0.96815	0.76680	0.39690	0.38516
Within R^2	0.32132	0.49909	0.02349	0.02496

IID standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

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

The instrumental variable estimates provide evidence of a robust causal relationship between national wage and GDP fluctuations 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 both in levels and growth rates. Though the growth rate specification is only relevant with at least a 1-year lag and using state- instead of commuting zone-level fixed effects.

In each case in the first table, 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 1% increase in the shift-share measure (which can be interpreted as a natural logarithm) is associated with a 0.05-0.5% increase in the local House Price Index ($p < 0.01$), with an F-statistic between 47-140 (all well above conventional weak instrument thresholds) confirming instrument relevance. The second-stage results are significant in all cases except the growth rate shocks with commuting zone level fixed effects.

In the level shift-share regressions, the instrumental variables estimates suggest that increases in house prices have a strong and statistically significant effect on education expenditure per pupil. Across specifications (columns 2, 4, 6, and 8), the estimated elasticity is close to one, implying that a 1% increase in house prices translates into nearly a 1% increase in elementary education spending. The instruments (intergovernmental revenue per capita and state wages) are highly relevant, as indicated by the very large first-stage F-statistics (well above the conventional threshold of 10), alleviating concerns about weak instruments. 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. Taken together, these results provide robust evidence that higher property values causally increase local education spending, consistent with a mechanism in which rising property wealth expands the fiscal capacity of local governments. Furthermore, given the dependent variable measures per pupil expenditure, this result implies direct effects in experience per student.

Using wage shocks in levels yields strong instruments, high first-stage F-statistics, and stable second-stage estimates: higher house prices robustly increase education spending. In contrast, when shocks are measured in growth rates, the instruments lose predictive power (first-stage F-statistics ~1–2), resulting in weak identification. The second-stage coefficients become unstable and often insignificant, while Hausman tests fail to reject exogeneity. 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. The instruments are strong, as indicated by very large first-stage F-statistics well above conventional thresholds, and the second-stage results are both large and robust across specifications. The Wu–Hausman tests reject exogeneity, reinforcing the necessity of IV estimation over OLS. By contrast, when wage shocks are expressed in growth rates, the identifying variation is substantially reduced, resulting in weak first stages and unstable second-stage estimates. 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.

At the same time, 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. In this sense, 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. While the large first-stage F-statistics and Hausman tests in the level specification support its validity, the weak performance of the growth-rate version cautions that the results could be sensitive to issues of persistence and trending in the data.

Taken together, these results suggest that while the level specification provides strong identification and compelling evidence of a positive causal effect of house prices on education spending, the weak performance of the growth-rate specification highlights the need for caution, as the strength of the findings may partly reflect long-run trending relationships rather than purely exogenous shocks.

However, examining the structure of the growth rate shock, the instability of the variable in growth rate is likely causing the poor identification in the growth rate regressions.

Dependent Variables:	(log) House Price Index	(log) Elem.Ed.Exp.pp	(log) House Price Index	(log) Elem.Ed.Exp.pp	(log) House Price Index	(log) Elem.Ed.Exp.pp	(log) House Price Index	(log) Elem.Ed.Exp.pp
Stage	1st	2nd	1st	2nd	1st	2nd	1st	2nd
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
Wage SS (lvl)	0.5390***		0.0525***					
	(0.0417)		(0.0044)					
(log) IG Revenue pp	0.1625***	0.2852***	-0.1687***	0.4166***	0.1687***	0.2285***	-0.1711***	0.4177***
	(0.0071)	(0.0148)	(0.0099)	(0.0186)	(0.0072)	(0.0303)	(0.0101)	(0.0191)
(log) Real GDP Priv. Industry pc	0.2563***	0.0594***	0.1007***	0.0631***	0.2554***	-0.0118	0.1014***	0.0633***
	(0.0059)	(0.0199)	(0.0064)	(0.0116)	(0.0059)	(0.0431)	(0.0065)	(0.0119)
(log) Enrollment	0.2498***	-0.3893***	0.1329***	-0.1635***	0.2726***	-0.4983***	0.1327***	-0.1642***
	(0.0105)	(0.0239)	(0.0019)	(0.0125)	(0.0108)	(0.0504)	(0.0020)	(0.0126)
(log) House Price Index		0.4890***		0.9686***		0.7861***		0.9707***
		(0.0767)		(0.0856)		(0.1695)		(0.0866)
Wage SS (lvl,l1)					0.2925***		0.0534***	
					(0.0436)		(0.0045)	
Fixed-effects								
unit	Yes	Yes			Yes	Yes		
year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state			Yes	Yes			Yes	Yes
Fit statistics								
Observations	12,717	12,717	12,717	12,717	12,122	12,122	12,122	12,122
R2 (1st stage)	0.96262		0.74285		0.96437		0.74242	
Adj. R2 (1st stage)	0.96066		0.74157		0.96242		0.74109	
F-test (IV only)	167.22	43.490	142.56	472.97	44.997	28.861	140.07	461.61
F-test (IV only), p-value	5.35×10^{-38}	4.44×10^{-11}	1.1×10^{-32}	5.49×10^{-103}	2.07×10^{-11}	7.93×10^{-8}	3.87×10^{-32}	1.62×10^{-100}
Wu-Hausman		17.914		395.73		17.515		385.52
Wu-Hausman, p-value		2.33×10^{-5}		9.85×10^{-87}		2.87×10^{-5}		1.62×10^{-84}
Wald (IV only)	167.22	40.638	142.56	127.94	44.997	21.501	140.07	125.66
Wald (IV only), p-value	5.35×10^{-38}	1.9×10^{-10}	1.1×10^{-32}	1.6×10^{-29}	2.07×10^{-11}	3.58×10^{-6}	3.87×10^{-32}	5.08×10^{-29}

IID standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Dependent Variables:	(log) House Price Index	(log) Elem.Ed.Exp.pp	(log) House Price Index	(log) Elem.Ed.Exp.pp	(log) House Price Index	(log) Elem.Ed.Exp.pp	(log) House Price Index	(log) Elem.Ed.Exp.pp
IV stages	First	Second	First	Second	First	Second	First	Second
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
Wage SS (GR)	0.0907		0.8346***					
	(0.0899)		(0.2230)					
(log) IG Revenue pp	0.1694***	0.5244**	-0.1815***	0.3618***	0.1725***	0.3459***	-0.1841***	0.3897***
	(0.0072)	(0.2437)	(0.0099)	(0.0388)	(0.0072)	(0.1252)	(0.0101)	(0.0500)
(log) Real GDP Priv. Industry pc	0.2480***	0.4096	0.1120***	0.0969***	0.2509***	0.1589	0.1129***	0.0804***
	(0.0059)	(0.3566)	(0.0063)	(0.0240)	(0.0059)	(0.1820)	(0.0065)	(0.0307)
(log) Enrollment	0.2819***	0.0088	0.1432***	-0.1203***	0.2888***	-0.3017	0.1432***	-0.1425***
	(0.0103)	(0.4055)	(0.0017)	(0.0300)	(0.0105)	(0.2097)	(0.0018)	(0.0382)
(log) House Price Index		-0.9224		0.6692***		0.1056		0.8201***
		(1.437)		(0.2077)		(0.7251)		(0.2648)
Wage SS (GR,l1)					0.1165		0.7554***	
					(0.0874)		(0.2236)	
Fixed-effects								
unit	Yes	Yes			Yes	Yes		
year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state			Yes	Yes			Yes	Yes
Fit statistics								
Observations	12,717	12,717	12,717	12,717	12,122	12,122	12,122	12,122
R2 (1st stage)	0.96210		0.74024		0.96424		0.73967	
Adj. R2 (1st stage)	0.96012		0.73895		0.96228		0.73833	
F-test (IV only)	1.0164	0.95001	14.002	21.631	1.7770	0.02059	11.410	26.184
F-test (IV only), p-value	0.31339	0.32974	0.00018	3.34×10^{-6}	0.18255	0.88591	0.00073	3.15×10^{-7}
Wu-Hausman		1.4158		16.199		0.01227		20.835
Wu-Hausman, p-value		0.23412		5.74×10^{-5}		0.91179		5.05×10^{-6}
Wald (IV only)	1.0164	0.41212	14.002	10.379	1.7770	0.02121	11.410	9.5932
Wald (IV only), p-value	0.31339	0.52091	0.00018	0.00128	0.18255	0.88422	0.00073	0.00196

IID standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

4.4.3.2 Industry-level GDP

We perform a similar combined baseline and causal IV estimation using the GDP-based shift-share instrument.

The estimates indicate that house prices are strongly correlated with local economic conditions. In the levels specification, both GDP per capita and population are positively associated with higher house prices, although the inclusion of state fixed effects alters the sign of lagged GDP coefficients, suggesting long-run mean reversion or heterogeneity across states. In the growth specification, contemporaneous GDP growth and short-run lags exert a positive effect on house price growth, while population growth also contributes significantly. Overall, these results imply that both the size and short-run dynamics of the local economy play a central role in shaping housing markets.

Overall, compared to wages, the relatively more sensitive stability of the relationship between GDP per capita and house prices indicates a weaker, and potentially more spurious link between real GDP and house prices. This conclusion logically aligns with the fact that the channel through which local economic activity links to house prices is through wages. Wages offer a more direct way of measuring the effect of local economic health on house prices than does real GDP. Nonetheless, the relationship still indicates a positive association between local economic health an dhousing prices, enough to warrant an investigation of our real value added shift-share instrument.

Dependent Variables:	(log) House Price Index		(GR) House Price Index	
Model:	(1)	(2)	(3)	(4)
Variables				
(log) Real GDP Priv. Industry pc	0.2411***	0.1438***		
	(0.0059)	(0.0062)		
(l1,log) Real GDP Priv. Industry pc	0.0276***	-0.0136***		
	(0.0055)	(0.0050)		
(l2,log) Real GDP Priv. Industry pc	0.0204***	-0.0256***		
	(0.0055)	(0.0049)		
(l3,log) Real GDP Priv. Industry pc	0.0199***	0.0373***		
	(0.0053)	(0.0049)		
(l4,log) Real GDP Priv. Industry pc	0.0045	-0.0078		
	(0.0052)	(0.0050)		
(l5,log) Real GDP Priv. Industry pc	0.0371***	-0.0061		
	(0.0053)	(0.0049)		
(l6,log) Real GDP Priv. Industry pc	0.0112**	0.0212***		
	(0.0053)	(0.0050)		
(l7,log) Real GDP Priv. Industry pc	0.0060	0.0146***		
	(0.0054)	(0.0050)		
(log) Population	0.5459***	0.1524***		
	(0.0158)	(0.0017)		
(GR) Real GDP Priv. Industry pc			0.0161***	0.0169***
			(0.0034)	(0.0033)
(GR,l1) Real GDP Priv. Industry pc			0.0093***	0.0101***
			(0.0032)	(0.0032)
(GR,l2) Real GDP Priv. Industry pc			0.0062*	0.0063**
			(0.0032)	(0.0031)
(GR,l3) Real GDP Priv. Industry pc			-0.0009	-0.0010
			(0.0032)	(0.0031)
(GR,l4) Real GDP Priv. Industry pc			-0.0025	-0.0027
			(0.0032)	(0.0031)
(GR,l5) Real GDP Priv. Industry pc			0.0070**	0.0062**
			(0.0032)	(0.0031)
(GR,l6) Real GDP Priv. Industry pc			8.67×10^{-5}	0.0010
			(0.0033)	(0.0032)
(GR,l7) Real GDP Priv. Industry pc			-0.0002	-3.7×10^{-5}
			(0.0031)	(0.0030)
(GR) Population			0.0060***	0.0064***
			(0.0011)	(0.0011)
Fixed-effects				
unit	Yes		Yes	
year	Yes	Yes	Yes	Yes
state		Yes		Yes
Fit statistics				
Observations	12,570	12,570	12,541	12,542
R^2	0.96261	0.74492	0.38587	0.37331
Within R^2	0.20336	0.45210	0.00564	0.00618

IID standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Moving on to investigate the GDP-based shift-share instrument, we find a similar effect as with wages when estimating level shift-share shocks. The first-stage relationship between GDP shocks and house prices ranges from 0.1-0.7% increase in response to a 1% increase in the shift-share instrument (interpreted as wage levels) with an F-statistic between 100-400. These results hold after controlling for local revenues, GDP per capita, and enrollment, and the Wu-Hausman tests reject OLS in favor of IV, suggesting that naive estimates are biased. Overall, the evidence supports a causal channel from rising local housing wealth to increased investment in public education. However, we see an even weaker relationship when imposing the shift-share instrument as a growth rate shock where the first-stage relationship are spurious and statistically insignificant.

Together, these findings suggest that levels (rather than short-run changes) in GDP are more systematically and substantially associated with house price dynamics. The stronger fit and significant lag effects in the level regression underscore the longer-term influence of economic fundamentals on housing markets. It also indicates that wage growth is more important for house prices than the more general presence of industry-level GDP growth. This makes intuitive sense in that the link from industrial success (labour) to personal and community wealth creation is mediated via wage and not necessarily the total industrial output which might not be reflected in wages (especially given recent evidence of decoupling of wages from productivity).

Dependent Variables:	(log) House Price Index	(log) Elem.Ed.Exp.pp	(log) House Price Index	(log) Elem.Ed.Exp.pp	(log) House Price Index	(log) Elem.Ed.Exp.pp	(log) House Price Index	(log) Elem.Ed.Exp.pp
IV stages	First	Second	First	Second	First	Second	First	Second
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
GDP SS (Lvl)	0.6954***		0.0997***					
	(0.0575)		(0.0049)					
(log) IG Revenue pp	0.1629***	0.2987***	-0.1390***	0.3209***	0.1659***	0.2618***	-0.1412***	0.3217***
	(0.0071)	(0.0152)	(0.0100)	(0.0085)	(0.0072)	(0.0193)	(0.0102)	(0.0089)
(log) Real GDP Priv. Industry pc	0.2520***	0.0792***	0.0906***	0.1222***	0.2542***	0.0366	0.0915***	0.1221***
	(0.0058)	(0.0207)	(0.0063)	(0.0054)	(0.0059)	(0.0265)	(0.0065)	(0.0055)
(log) Enrollment	0.2482***	-0.3668***	0.1185***	-0.0881***	0.2620***	-0.4426***	0.1183***	-0.0897***
	(0.0106)	(0.0247)	(0.0021)	(0.0047)	(0.0108)	(0.0317)	(0.0022)	(0.0048)
(log) House Price Index		0.4094***		0.4456***		0.5933***		0.4537***
		(0.0802)		(0.0318)		(0.1028)		(0.0325)
GDP SS (lvl,l1)					0.6109***		0.1007***	
					(0.0598)		(0.0051)	
Fixed-effects								
unit	Yes	Yes			Yes	Yes		
year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state			Yes	Yes			Yes	Yes
Fit statistics								
Observations	12,717	12,717	12,717	12,717	12,122	12,122	12,122	12,122
R2 (1st stage)	0.96255		0.74809		0.96456		0.74773	
Adj. R2 (1st stage)	0.96059		0.74683		0.96261		0.74643	
F-test (IV only)	146.08	26.630	408.46	276.75	104.36	37.957	397.06	275.80
F-test (IV only), p-value	1.95×10^{-33}	2.5×10^{-7}	2.03×10^{-89}	1.73×10^{-61}	2.15×10^{-24}	7.48×10^{-10}	5.99×10^{-87}	2.95×10^{-61}
Wu-Hausman		8.5597		176.20		18.721		176.29
Wu-Hausman, p-value		0.00344		6.06×10^{-40}		1.53×10^{-5}		5.98×10^{-40}
Wald (IV only)	146.08	26.057	408.46	196.83	104.36	33.277	397.06	194.41
Wald (IV only), p-value	1.95×10^{-33}	3.36×10^{-7}	2.03×10^{-89}	2.21×10^{-44}	2.15×10^{-24}	8.2×10^{-9}	5.99×10^{-87}	7.58×10^{-44}

IID standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Dependent Variables:	(log) House Price Index	(log) Elem.Ed.Exp.pp	(log) House Price Index	(log) Elem.Ed.Exp.pp	(log) House Price Index	(log) Elem.Ed.Exp.pp	(log) House Price Index	(log) Elem.Ed.Exp.pp
IV stages	First	Second	First	Second	First	Second	First	Second
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
GDP SS (GR)	-0.0032		1.239***					
	(0.1459)		(0.3619)					
(log) IG Revenue pp	0.1695***	-35.39	-0.1819***	0.1682***	0.1724***	-1.014	-0.1856***	-0.0077
	(0.0072)	(1,637.9)	(0.0099)	(0.0392)	(0.0072)	(4.966)	(0.0101)	(0.3120)
(log) Real GDP Priv. Industry pc	0.2481***	-52.16	0.1118***	0.2167***	0.2509***	-1.820	0.1134***	0.3241*
	(0.0059)	(2,398.0)	(0.0063)	(0.0243)	(0.0059)	(7.227)	(0.0065)	(0.1913)
(log) Enrollment	0.2821***	-59.76	0.1427***	0.0321	0.2889***	-2.580	0.1436***	0.1657
	(0.0103)	(2,726.0)	(0.0018)	(0.0304)	(0.0105)	(8.319)	(0.0018)	(0.2416)
(log) House Price Index		211.0		-0.3891*		7.993		-1.320
		(9,665.1)		(0.2107)		(28.80)		(1.678)
GDP SS (GR,l1)					-0.0435		0.3658	
					(0.1592)		(0.4043)	
Fixed-effects								
unit	Yes	Yes			Yes	Yes		
year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state			Yes	Yes			Yes	Yes
Fit statistics								
Observations	12,717	12,717	12,717	12,717	12,122	12,122	12,122	12,122
R2 (1st stage)	0.96210		0.74020		0.96423		0.73944	
Adj. R2 (1st stage)	0.96012		0.73890		0.96227		0.73810	
F-test (IV only)	0.00048	23.305	11.711	6.1087	0.07461	4.9568	0.81846	4.8641
F-test (IV only), p-value	0.98260	1.4×10^{-6}	0.00062	0.01346	0.78475	0.02601	0.36565	0.02744
Wu-Hausman		24.151		10.029		4.9045		5.8489
Wu-Hausman, p-value		9.03×10^{-7}		0.00154		0.02681		0.01560
Wald (IV only)	0.00048	0.00048	11.711	3.4087	0.07461	0.07702	0.81846	0.61888
Wald (IV only), p-value	0.98260	0.98258	0.00062	0.06488	0.78475	0.78138	0.36565	0.43148

IID standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

4.4.3.3 Accounting for Heterogeneity

In order to make meaningful policy-related insights, we need to unmask the substantial heterogeneity obscured by the national-level average treatment effects described above. Barring design and data issues with our shift-share instrument, these national-level estimates are unlikley to apply uniformly across states and commuting zones. Therefore, this next section is dedicated to unpacking this heterogeneity. Below, we explore various metrics of local economic growth and decline to (1) partition our sample according to metrics of economic health, employ (2) industry-by-industry and (2) state-by-state uestimations sing our baseline descriptive models, wage- and GDP-based shift-share instruments.

4.4.3.3.1 Declining vs. Growing Regions

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

In Figure X, we see that there is similar variability though the patterns do not consistently indicate the same high- and low-performing outliers across states indicating that GDP and wage growth are not consistently correlated across regions. We demonstrate this fact in Figure X (scatterplot with pink regression linear fit) where, although there is a positive correlation between commuting zone GDP and wage trend deviations, the wage trend deviation represents a nearly inelastic relationship to GDP growth.

4.4.3.3.2 Baseline Models

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	0.0277			0.0214			0.0120			0.0022		
	(0.0359)			(0.0410)			(0.0222)			(0.0284)		
(log,l1) Real GDP Priv. Industry pc	0.0479			0.0524			0.0651***			0.0616***		
	(0.0313)			(0.0360)			(0.0160)			(0.0202)		
(log,l2) Real GDP Priv. Industry pc	0.1205***			0.1191***			0.1302***			0.1647***		
	(0.0303)			(0.0324)			(0.0291)			(0.0349)		
(log) IG Revenue pp	0.4154***	0.4193***	0.4114***	0.3556***	0.3651***	0.3518***	0.3671***	0.3292***	0.3462***	0.3478***	0.2888***	0.2903***
	(0.0605)	(0.0561)	(0.0603)	(0.0746)	(0.0700)	(0.0753)	(0.0352)	(0.0415)	(0.0386)	(0.0500)	(0.0588)	(0.0605)
(log) Annual Avg. Wkly. Wage		0.0394			0.0299			0.1855**			0.1696	
		(0.0865)			(0.1070)			(0.0758)			(0.1191)	
(log, l1) Annual Avg. Wkly. Wage		0.1796*			0.2173			0.1605***			0.2229**	
		(0.0977)			(0.1394)			(0.0544)			(0.0899)	
(log, l2) Annual Avg. Wkly. Wage		0.4072***			0.4024***			0.1573			0.1891	
		(0.1059)			(0.1402)			(0.1004)			(0.1518)	
(log) House Price Index			-0.0213			-0.0521			0.1053***			0.1689***
			(0.0354)			(0.0510)			(0.0324)			(0.0523)
(log, l1) House Price Index			0.1280***			0.1933***			0.0321			0.0468
			(0.0330)			(0.0377)			(0.0355)			(0.0547)
(log, l2) House Price Index			0.0232			0.0411			0.0447*			0.0605
			(0.0308)			(0.0415)			(0.0262)			(0.0450)
(log, l3) House Price Index			0.0811**			0.1080**			0.0185			-0.0212
			(0.0384)			(0.0516)			(0.0254)			(0.0365)
(log, l4) House Price Index			-0.0407			-0.0958*			-0.0286			-0.0912**
			(0.0405)			(0.0524)			(0.0269)			(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	5,544	5,496	3,021	3,339	3,291	7,068	7,812	7,092	3,021	3,339	2,796
R^2	0.84183	0.84524	0.84205	0.83022	0.83653	0.83399	0.86135	0.85283	0.86232	0.79838	0.78135	0.77960
Within R^2	0.23941	0.27905	0.26129	0.19348	0.24973	0.23246	0.25046	0.21280	0.19813	0.26413	0.19167	0.15594

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

4.4.3.3.3 Wage SS Instrument

Result: The baseline estimate of an l-1 effect on public education expenditure is entirely dominated by declining regions. Interestingly, sample division by GDP growth rates isolates the wage-based SS instrument effect almost entirely indicating that wage changes in declining regions matter most. Potentially an indicator of property price spirals.

Result: Whereas, in the case of the wage-based instrument when applied to growing and declining wage regions, the effect is more widespread across regions.

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	0.0124			0.0155			0.0165			-0.0074		
	(0.0577)			(0.0435)			(0.0201)			(0.0291)		
(log,l1) Real GDP Priv. Industry pc	0.1342***			0.1419***			0.0600***			0.0478**		
	(0.0414)			(0.0297)			(0.0148)			(0.0204)		
(log,l2) Real GDP Priv. Industry pc	0.0867*			0.0436			0.1448***			0.1680***		
	(0.0453)			(0.0415)			(0.0258)			(0.0348)		
(log) IG Revenue pp	0.3638***	0.3516***	0.3719***	0.4551***	0.4497***	0.4437***	0.3845***	0.3566***	0.3706***	0.3208***	0.2613***	0.2605***
	(0.0704)	(0.0643)	(0.0747)	(0.0570)	(0.0511)	(0.0540)	(0.0328)	(0.0368)	(0.0369)	(0.0490)	(0.0630)	(0.0537)
(log) Annual Avg. Wkly. Wage		-0.0397			0.0202			0.1911***			0.1556	
		(0.1478)			(0.0928)			(0.0660)			(0.1067)	
(log, l1) Annual Avg. Wkly. Wage		0.3092**			0.2210***			0.1709***			0.1649**	
		(0.1382)			(0.0813)			(0.0498)			(0.0761)	
(log, l2) Annual Avg. Wkly. Wage		0.4446***			0.3423***			0.2241**			0.2232	
		(0.1351)			(0.1090)			(0.0889)			(0.1418)	
(log) House Price Index			-0.0295			-0.0387			0.0947***			0.1485***
			(0.0608)			(0.0465)			(0.0299)			(0.0561)
(log, l1) House Price Index			0.1615**			0.1686***			0.0521*			0.0014
			(0.0668)			(0.0515)			(0.0294)			(0.0560)
(log, l2) House Price Index			0.0389			0.0030			0.0499**			0.0360
			(0.0564)			(0.0367)			(0.0235)			(0.0459)
(log, l3) House Price Index			-0.0167			0.0330			0.0471**			-0.0077
			(0.0618)			(0.0386)			(0.0237)			(0.0371)
(log, l4) House Price Index			0.0537			0.0014			-0.0260			0.0163
			(0.0506)			(0.0369)			(0.0242)			(0.0471)
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	1,680	1,593	3,021	3,339	3,203	10,564	11,676	10,995	3,021	3,339	2,867
R^2	0.89240	0.89101	0.88268	0.86591	0.86376	0.86041	0.84837	0.84083	0.84564	0.85678	0.83702	0.84799
Within R^2	0.23695	0.26609	0.23308	0.27288	0.28206	0.27358	0.26758	0.24564	0.23328	0.27774	0.19569	0.14548

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

Table 18: Wage-based Shift-Share Instrument (l1) Applied to Declining GDP vs. Growing GDP Regions

Dependent Variable:	(log) Elem.Ed.Exp.pp				
	All	Declining (GDP)	Hyper-Declining (GDP)	Growing (GDP)	Hyper-Growing (GDP)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
(log) House Price Index	0.9707**	1.110***	0.9796***	0.7658	0.3677
	(0.3789)	(0.3718)	(0.3648)	(0.6049)	(0.8017)
(log) IG Revenue pp	0.4177***	0.4912***	0.4201***	0.4028***	0.2624***
	(0.0906)	(0.1084)	(0.1032)	(0.1272)	(0.0622)
(log) Real GDP Priv. Industry pc	0.0633	0.1163	0.1446*	0.0766	0.1156
	(0.0529)	(0.0744)	(0.0865)	(0.0685)	(0.1147)
(log) Enrollment	-0.1642***	-0.1711***	-0.1594***	-0.1450	-0.1249
	(0.0547)	(0.0488)	(0.0522)	(0.0950)	(0.1511)
Fixed-effects					
year	Yes	Yes	Yes	Yes	Yes
state	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	12,122	5,240	3,140	6,882	2,761
F-test (IV only)	461.61	537.75	298.28	80.036	3.6949
F-test (IV only), p-value	1.62×10^{-100}	2.87×10^{-113}	7.18×10^{-64}	4.67×10^{-19}	0.05468
Wu-Hausman	385.52	433.95	227.64	65.722	2.9286
Wu-Hausman, p-value	1.62×10^{-84}	1.29×10^{-92}	1.11×10^{-49}	6.11×10^{-16}	0.08714
Wald (IV only)	6.5624	8.9225	7.2100	1.6027	0.21038
Wald (IV only), p-value	0.01043	0.00283	0.00729	0.20556	0.64650

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

Table 19: Wage-based Shift-Share Instrument (l1) Applied to Declining Wage vs. Growing Wage Regions

Dependent Variable:	(log) Elem.Ed.Exp.pp				
	All	Declining (Wage)	Hyper-Declining (Wage)	Growing (Wage)	Hyper-Growing (Wage)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
(log) House Price Index	0.9707**	0.7404	1.169*	0.8769**	0.4162
	(0.3789)	(0.4451)	(0.6305)	(0.3641)	(0.4671)
(log) IG Revenue pp	0.4177***	0.3630***	0.4551***	0.4119***	0.2829***
	(0.0906)	(0.1343)	(0.1654)	(0.0884)	(0.0621)
(log) Real GDP Priv. Industry pc	0.0633	-0.0124	-0.0335	0.0861**	0.1094**
	(0.0529)	(0.1669)	(0.1910)	(0.0436)	(0.0544)
(log) Enrollment	-0.1642***	-0.1246**	-0.1747**	-0.1502***	-0.1184
	(0.0547)	(0.0603)	(0.0778)	(0.0509)	(0.0747)
Fixed-effects					
year	Yes	Yes	Yes	Yes	Yes
state	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	12,122	1,520	3,060	10,602	2,822
F-test (IV only)	461.61	70.621	256.33	383.32	21.828
F-test (IV only), p-value	1.62×10^{-100}	1.01×10^{-16}	2×10^{-55}	7.19×10^{-84}	3.12×10^{-6}
Wu-Hausman	385.52	47.014	210.21	324.43	23.718
Wu-Hausman, p-value	1.62×10^{-84}	1.04×10^{-11}	4.31×10^{-46}	1.84×10^{-71}	1.18×10^{-6}
Wald (IV only)	6.5624	2.7674	3.4365	5.8010	0.79401
Wald (IV only), p-value	0.01043	0.09642	0.06387	0.01603	0.37297

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

4.4.3.3.4 GDP SS Instrument

Result: In the case of the GDP-based instrument, the sub-sampling procedure indicates that GDP growth has an effect on public education expenditure in all but hyper-growing areas (as defined in both GDP and wage subsampling) indicating that GDP growth translates to changes in public expenditure more directly in all but non-superstar regions. This could indicate a spillover into private education...possible to investigate?

Table 20: GDP-based Shift-Share Instrument (l1) Applied to Declining Wage vs. Growing Wage Regions

Dependent Variable:	(log) Elem.Ed.Exp.pp				
	All	Declining (Wage)	Hyper-Declining (Wage)	Growing (Wage)	Hyper-Growing (Wage)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
(log) House Price Index	0.4537***	0.4421**	0.6652***	0.3869***	-0.0710
	(0.1225)	(0.1987)	(0.2376)	(0.1208)	(0.1747)
(log) IG Revenue pp	0.3217***	0.2843***	0.3311***	0.3261***	0.2659***
	(0.0392)	(0.0832)	(0.0794)	(0.0402)	(0.0401)
(log) Real GDP Priv. Industry pc	0.1221***	0.0806	0.0835	0.1297***	0.1622***
	(0.0258)	(0.0891)	(0.0973)	(0.0239)	(0.0331)
(log) Enrollment	-0.0897***	-0.0838***	-0.1109***	-0.0822***	-0.0427
	(0.0180)	(0.0298)	(0.0286)	(0.0171)	(0.0317)
Fixed-effects					
year	Yes	Yes	Yes	Yes	Yes
state	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	12,122	1,520	3,060	10,602	2,822
F-test (IV only)	275.80	51.996	206.30	194.26	1.3917
F-test (IV only), p-value	2.95×10^{-61}	8.87×10^{-13}	2.7×10^{-45}	9.11×10^{-44}	0.23822
Wu-Hausman	176.29	23.467	135.86	126.44	0.91974
Wu-Hausman, p-value	5.98×10^{-40}	1.41×10^{-6}	9.74×10^{-31}	3.61×10^{-29}	0.33763
Wald (IV only)	13.718	4.9519	7.8373	10.253	0.16524
Wald (IV only), p-value	0.00021	0.02621	0.00515	0.00137	0.68441

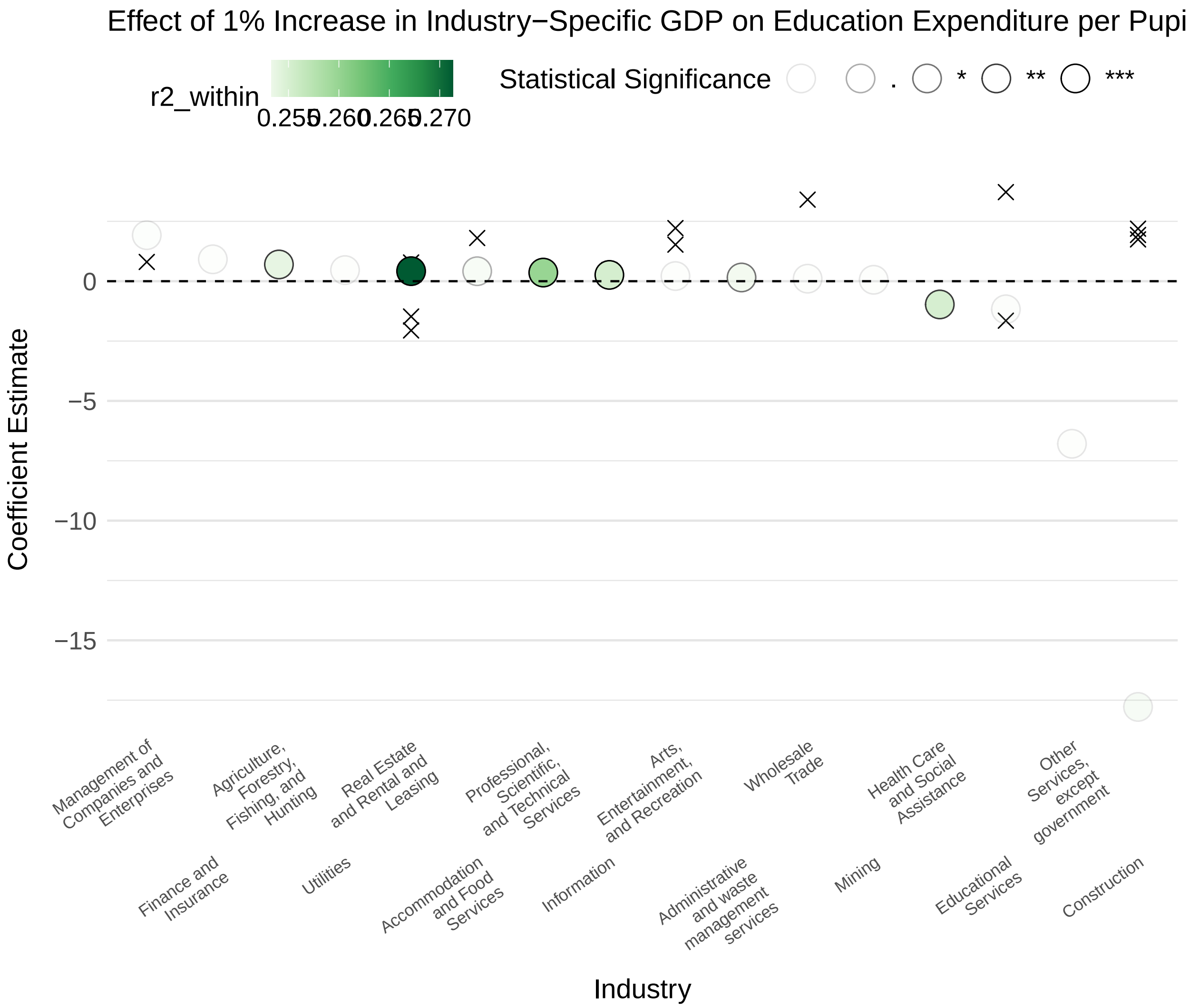
Clustered (unit) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 21: GDP-based Shift-Share Instrument (l1) Applied to Declining GDP vs. Growing GDP Regions

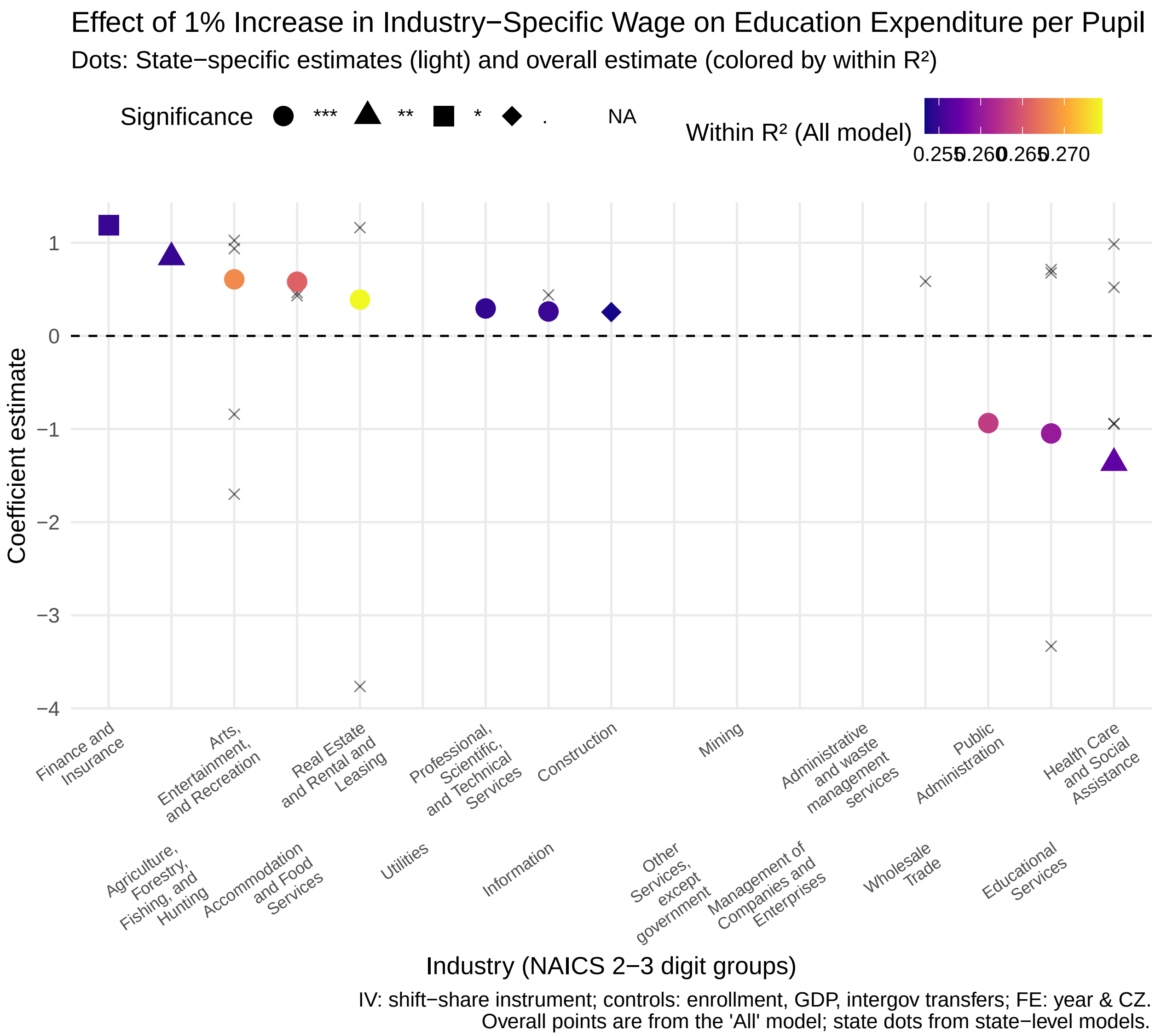
Dependent Variable:	(log) Elem.Ed.Exp.pp				
	All	Declining (GDP)	Hyper-Declining (GDP)	Growing (GDP)	Hyper-Growing (GDP)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
(log) House Price Index	0.4537***	0.6799***	0.7473***	0.1898	-0.1220
	(0.1225)	(0.1706)	(0.2250)	(0.1388)	(0.2311)
(log) IG Revenue pp	0.3217***	0.3877***	0.3695***	0.2906***	0.2348***
	(0.0392)	(0.0589)	(0.0751)	(0.0463)	(0.0482)
(log) Real GDP Priv. Industry pc	0.1221***	0.1559***	0.1606**	0.1360***	0.1814***
	(0.0258)	(0.0507)	(0.0717)	(0.0267)	(0.0443)
(log) Enrollment	-0.0897***	-0.1158***	-0.1277***	-0.0555**	-0.0329
	(0.0180)	(0.0218)	(0.0318)	(0.0217)	(0.0442)
Fixed-effects					
year	Yes	Yes	Yes	Yes	Yes
state	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	12,122	5,240	3,140	6,882	2,761
F-test (IV only)	275.80	417.78	238.81	21.107	1.8623
F-test (IV only), p-value	2.95×10^{-61}	2.3×10^{-89}	6.04×10^{-52}	4.42×10^{-6}	0.17247
Wu-Hausman	176.29	279.33	163.43	7.2715	3.4328
Wu-Hausman, p-value	5.98×10^{-40}	4.09×10^{-61}	1.68×10^{-36}	0.00702	0.06402
Wald (IV only)	13.718	15.885	11.027	1.8703	0.27885
Wald (IV only), p-value	0.00021	6.82×10^{-5}	0.00091	0.17149	0.59750

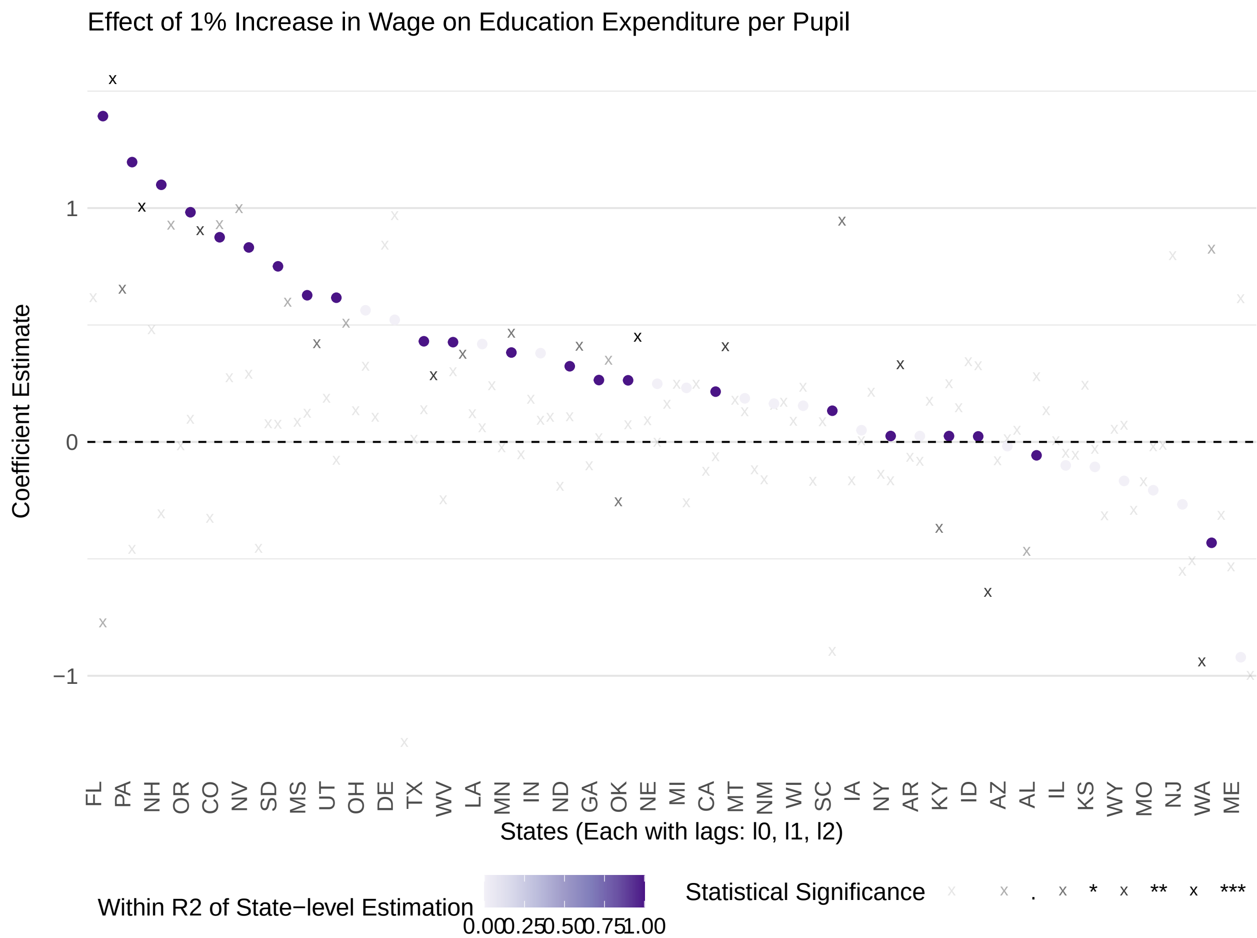
Clustered (unit) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

4.4.3.3.5 State-by-state and industry-by-industry estimation



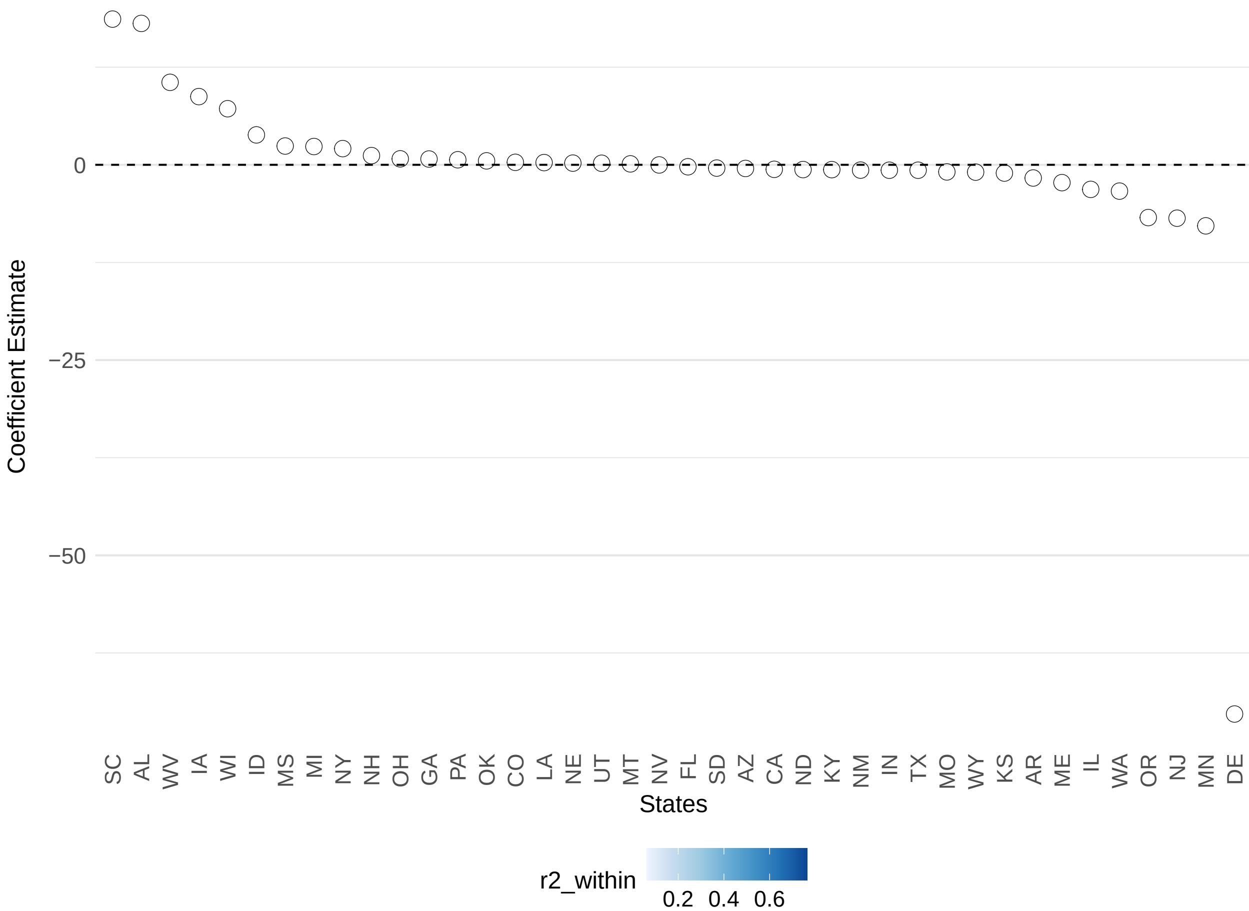
Effect of 1% increase in Industry−Specific GDP (shift−share). Controls: enrollment, GDP, intergov transfers, AR(1), year & state FE.
Within R² of 'All' estimation in point color.


Effect of 1% Increase in Industry−Specific Wage on Education Expenditure per Pupil
Dots: State−specific estimates (light) and overall estimate (colored by within R²)
Significance *** ** * . NA
Within R² (All model)
0.255 0.260 0.265 0.270
Coefficient estimate
1
0
−1
−2
−3
−4
Finance and Insurance
Agriculture, Forestry, Fishing, and Hunting
Arts, Entertainment, and Recreation
Accommodation and Food Services
Real Estate and Rental and Leasing
Utilities
Professional, Scientific, and Technical Services
Information
Construction
Other Services, except government
Mining
Management of Companies and Enterprises
Administrative and waste management services
Wholesale Trade
Public Administration
Educational Services
Health Care and Social Assistance
Industry (NAICS 2−3 digit groups)
IV: shift−share instrument; controls: enrollment, GDP, intergov transfers; FE: year & CZ.
Overall points are from the 'All' model; state dots from state−level models.

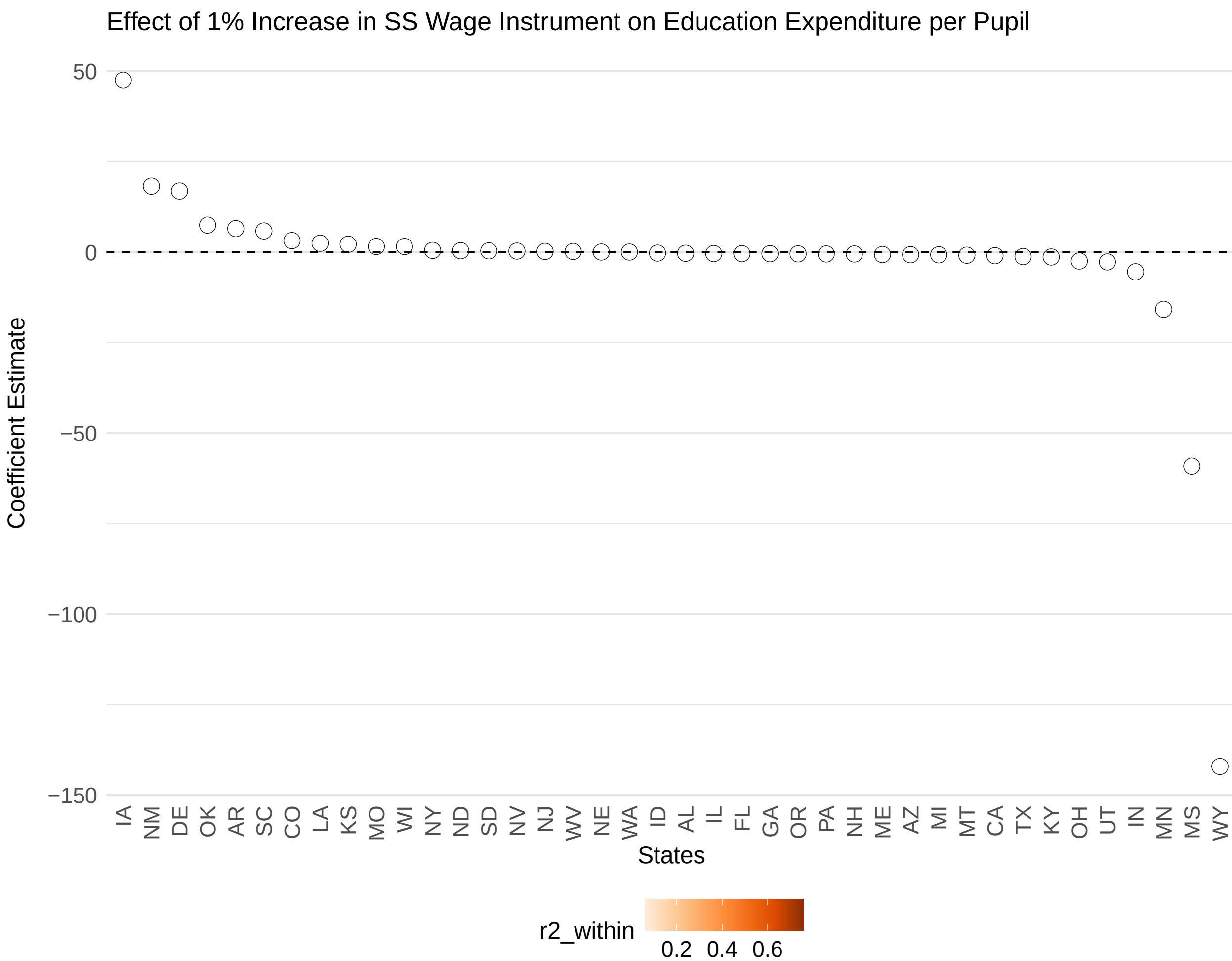


Effect of contemporaneous, l1, l2 lag of avg weekly wage on PP Education Expenditure controlling for enrollment, GDP, and intergovernmental transfers. Each state has 3 lagged wage coefficients (l0, l1, l2). Blue dots show total effect across lags, plotted at the l1 position. Within R^2 of state−level estimation reflected in color of point).

Effect of 1% Increase in SS GDP Instrument on Education Expenditure per Pupil



Effect of l1 of GDP SS Instrument on PP Education
Expenditure controlling for enrollment, GDP, and intergovernmental transfers and AR(1) term.
Within R^2 of state−level estimation reflected in color of point).



Effect of l1 of Wage SS Instrument on PP Education Expenditure controlling for enrollment, GDP, and intergovernmental transfers and AR(1) term. Within R^2 of state−level estimation reflected in color of point).

5 Results

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6 Discussion

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7 Conclusion

While results are in flux, I am collecting statements I want to make in the conclusion.

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. Though this work does not directly inform this debate, further work could explore the extent to which wage growth interacts with such structural policies.

8 Data and Code Availability

Code and data to reproduce the analysis will be made available on Github or Zenodo.

9 Use of AI

- Used ChatGPT to help improve readability of plots (formatting, margins, labeling).
- Used ChatGPT to debug errors in R during data cleaning and plotting.
- Used ChatGPT to provide suggestions for reducing run time of repetitive tasks (ex. downloading and processing multiple data files).

10 Acknowledgements

Appendices

A Potential Methodological Challenges

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

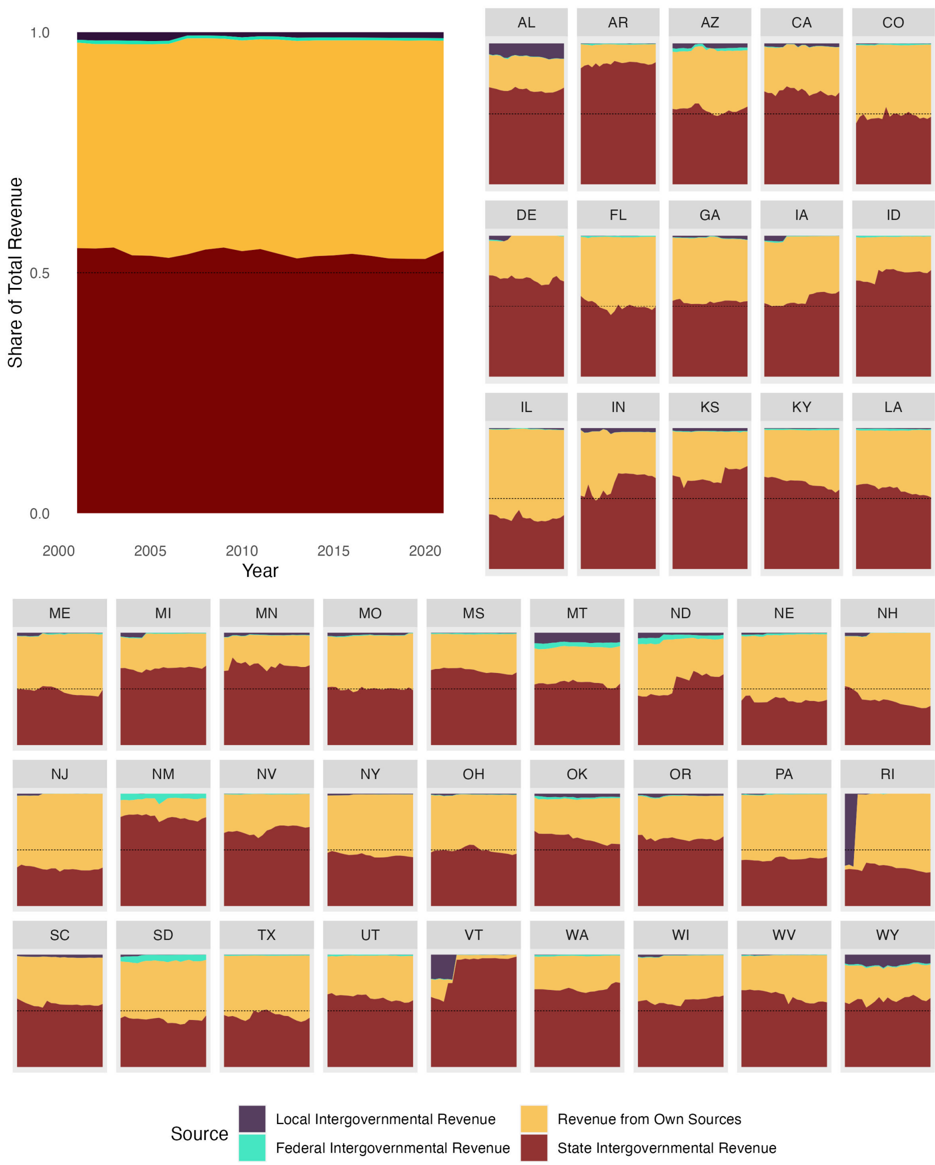
A.1 Structure of Financing for Local Public Education

In order to appropriately make use of the outlined data as well as robustly define the econometric methods to be utilised 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 **?**, 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 **?**. Using the data outlined, Figure 23 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 **?**.

Figure 23: Share of Revenue from Federal, State, Local Sources



A.3 Historical efforts to "equalise" 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 "equalise" public education by aiming for "per pupil" expenditure targets ?. 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 22. 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 ?. 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 22: 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 Descriptive Regression Results

In the following set of results, I report descriptive regressions to establish relationships between property taxes, education expenditure, GDP (total, private industry, O&G&mining), etc. All regression models that follow include TWFE (CZ- and year- fixed effects) and standard errors clustered by commuting zone. All functional forms in the feols() functions below are of the form Y ~ X In the cases in which multiple estimations are included via sw(Xa, Xb, Xc + Xd), the function will return results for Y~Xa, Y~Xb, Y ~ Xc + Xd.

B.1 Property Tax ~ GDP

GDP has a highly relevant relationship to property taxes. A 1% increase in GDP (per capita) leads to a 0.38% (0.32%) increase in property taxes collected (per capita).

Dependent Variables:	log_real_Property_Tax		(log) Prop Taxpp	
Model:	(1)	(2)	(3)	(4)
Variables				
(log) Real GDP	0.3854***	0.1226***		
	(0.0480)	(0.0325)		
l(log_real_gdp_total,1)		0.1193***		
		(0.0274)		
l(log_real_gdp_total,2)		0.0697**		
		(0.0285)		
l(log_real_gdp_total,3)		0.0790***		
		(0.0183)		
l(log_real_gdp_total,4)		0.1198***		
		(0.0384)		
(log) Real GDP pc			0.3151***	0.1212***
			(0.0616)	(0.0366)
l(log_real_gdp_total_pc,1)				0.0929***
				(0.0271)
l(log_real_gdp_total_pc,2)				0.0677**
				(0.0328)
l(log_real_gdp_total_pc,3)				0.0731***
				(0.0229)
l(log_real_gdp_total_pc,4)				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. These regressions simply corroborate what is displayed in the section on Key Relationships in LINK (ie. that 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_real_Elem_Educ_Total_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 Taxpp		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_real_Property_Tax					0.2565***	0.3014***	0.3070***	
					(0.0195)	(0.0194)	(0.0192)	
log_real_Total_IG_Revenue						0.5020***		0.4853***
						(0.0252)		(0.0234)
log_real_Total_Fed_IG_Revenue							0.0005	
							(0.0007)	
log_real_Total_State_IG_Revenue							0.4823***	
							(0.0269)	
log_real_Total_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*

B.3 Education Expenditure ~ GDP

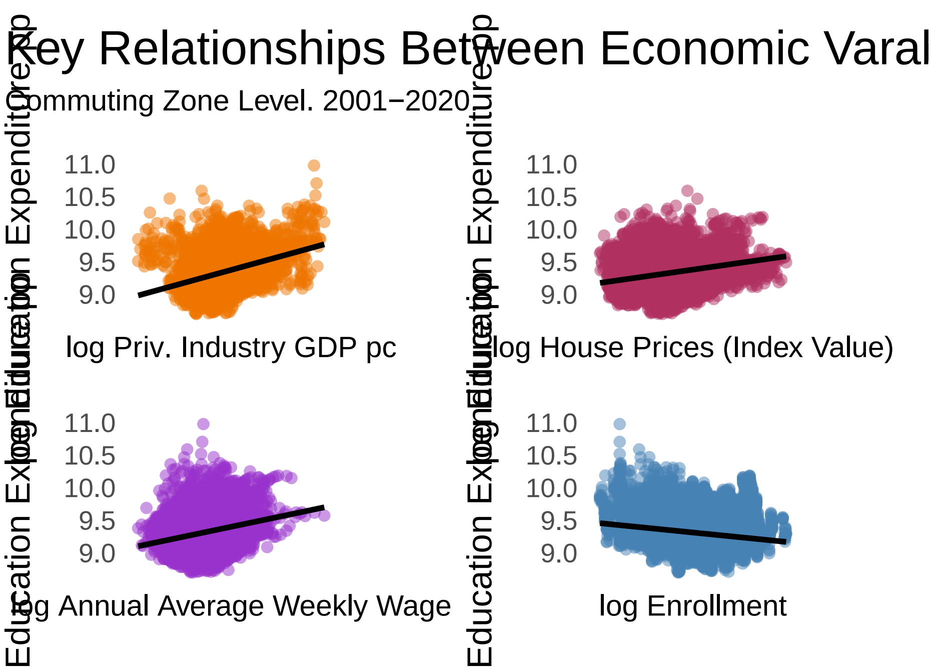
A 1% increase in GDP pc is associated with a 0.19% increase in education expenditure per pupil, dominated by the effect of GDP from private industry (0.16%). I include here also the GDP generated from the oil, gas, mining, and quarrying sector. The effect is small and statistically insignificant.

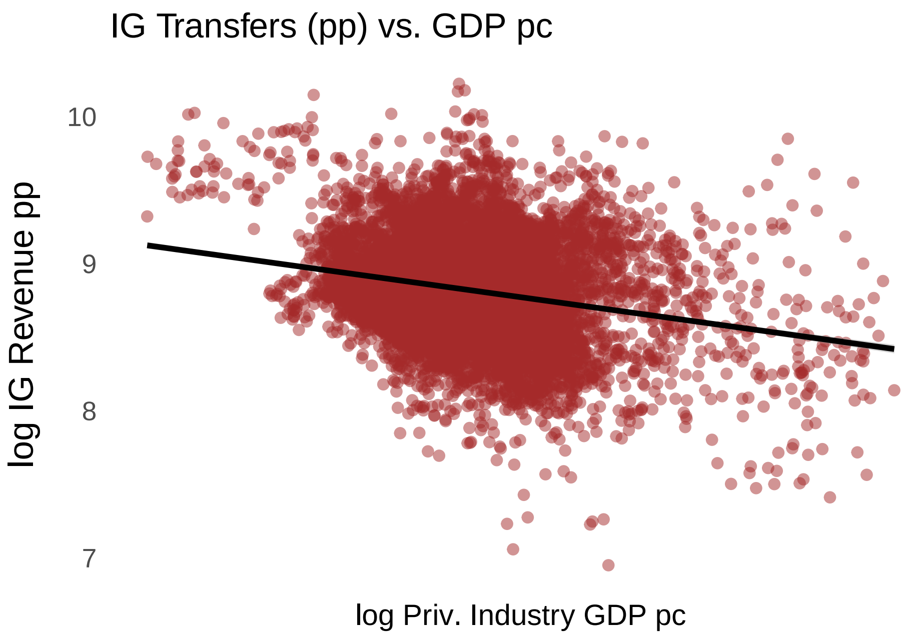
Dependent Variable:	(log) Elem.Ed.Exp.pp		
Model:	(1)	(2)	(3)
Variables			
(log) Real GDP pc	0.1926***		
	(0.0210)		
(log) Real GDP Priv. Industry pc		0.1674***	
		(0.0182)	
log_real_gdp_o_g_mining_quarr_21_pc			0.0155***
			(0.0032)
Fixed-effects			
unit	Yes	Yes	Yes
year	Yes	Yes	Yes
Fit statistics			
Observations	13,356	13,356	13,356
R^2	0.81378	0.81283	0.80330
Within R^2	0.06328	0.05847	0.01055

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

C Key Relationships between Economic Variables

Below we display key relationships between several of the economic variables in our study.





C.1 Baseline Regressions with 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.

Dependent Variable:			(log) Elem.Ed.Exp.pp			
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
(log) Real GDP Priv. Industry pc	-0.0043	0.0076				
	(0.0199)	(0.0147)				
(log,l1) Real GDP Priv. Industry pc	0.0642***	0.0397**				
	(0.0132)	(0.0187)				
(log,l2) Real GDP Priv. Industry pc	0.1335***	0.0053				
	(0.0226)	(0.0134)				
(log) IG Revenue pp	0.2827***	0.0940***	0.2259***	0.0732***	0.2072***	0.0712***
	(0.0274)	(0.0089)	(0.0243)	(0.0079)	(0.0276)	(0.0081)
(log) Enrollment	-0.0329***	-0.0079***	-0.0644***	-0.0141***	-0.0341***	-0.0078***
	(0.0043)	(0.0012)	(0.0064)	(0.0013)	(0.0050)	(0.0012)
(l1, log) Elem.Ed.Exp.pp		0.7696***		0.7922***		0.8011***
		(0.0118)		(0.0144)		(0.0118)
(log) Annual Avg. Wkly. Wage			0.2275***	0.0459		
			(0.0784)	(0.0488)		
(log, l1) Annual Avg. Wkly. Wage			0.2106***	0.2030***		
			(0.0487)	(0.0621)		
(log, l2) Annual Avg. Wkly. Wage			0.0748	-0.1302***		
			(0.0687)	(0.0404)		
(log) House Price Index					0.0385	0.0931***
					(0.0325)	(0.0219)
(log, l1) House Price Index					0.1013***	0.0053
					(0.0301)	(0.0395)
(log, l2) House Price Index					0.0780***	-0.0033
					(0.0245)	(0.0332)
(log, l3) House Price Index					0.0570***	-0.0299
					(0.0220)	(0.0273)
(log, l4) House Price Index					-0.1416***	-0.0305
					(0.0255)	(0.0198)
Fixed-effects						
state	Yes	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	12,084	12,084	13,356	12,720	12,588	12,029
R^2	0.68861	0.88123	0.67088	0.88105	0.66621	0.88481
Within R^2	0.32932	0.74418	0.28070	0.74264	0.16006	0.71338

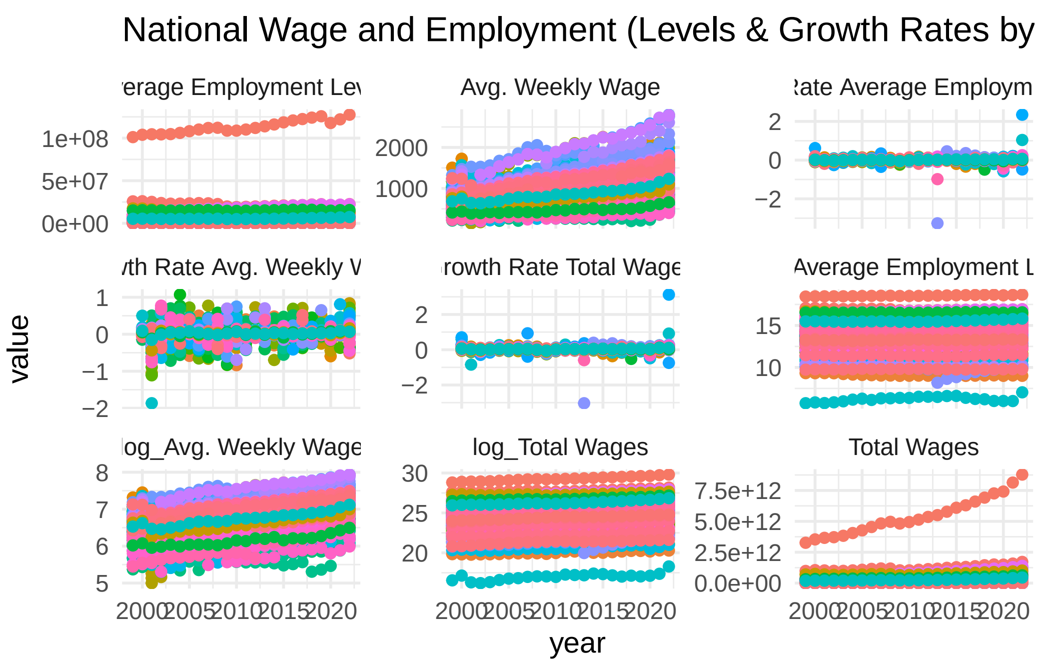
Clustered (unit) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Dependent Variable:	(GR) Elem.Ed.Exp.pp					
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
(GR) Real GDP Priv. Industry pc	0.0087	0.0074				
	(0.0134)	(0.0130)				
(GR,l1) Real GDP Priv. Industry pc	0.0544***	0.0548***				
	(0.0149)	(0.0149)				
(GR,l2) Real GDP Priv. Industry pc	0.0198***	0.0206***				
	(0.0070)	(0.0070)				
(GR) IG Revenue pp	0.3088***	0.3072***	0.3259***	0.3066***	0.3271***	0.3085***
	(0.0317)	(0.0320)	(0.0223)	(0.0313)	(0.0228)	(0.0305)
(GR) Enrollment	-0.5741***	-0.5662***	-0.0144**	-0.5649***	-0.0063	-0.5768***
	(0.0397)	(0.0407)	(0.0063)	(0.0408)	(0.0068)	(0.0440)
(GR, l1) Elem.Ed.Exp.pp		-0.0580***		-0.0381***		-0.0443***
		(0.0149)		(0.0097)		(0.0093)
(GR) Annual Avg. Wkly. Wage			-0.0263	0.0240		
			(0.0544)	(0.0445)		
(GR, l1) Annual Avg. Wkly. Wage			0.2079***	0.1823***		
			(0.0494)	(0.0439)		
(GR, l2) Annual Avg. Wkly. Wage			0.3101***	0.3061***		
			(0.0591)	(0.0563)		
(GR) House Price Index					0.0614**	0.1045***
					(0.0239)	(0.0195)
(GR, l1) House Price Index					0.1069***	0.0769***
					(0.0290)	(0.0246)
(GR, l2) House Price Index					0.0592***	0.0601***
					(0.0205)	(0.0186)
(GR, l3) House Price Index					0.0204	0.0288
					(0.0257)	(0.0198)
(GR, l4) House Price Index					0.0328	0.0216
					(0.0211)	(0.0171)
Fixed-effects						
state	Yes	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	12,083	12,083	13,355	12,719	12,535	11,978
R^2	0.26154	0.26458	0.34055	0.26778	0.34934	0.28087
Within R^2	0.21898	0.22219	0.15384	0.22431	0.14687	0.23107

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

C.2 SS Construction

Plots of the data inputs to the shift-share instrument.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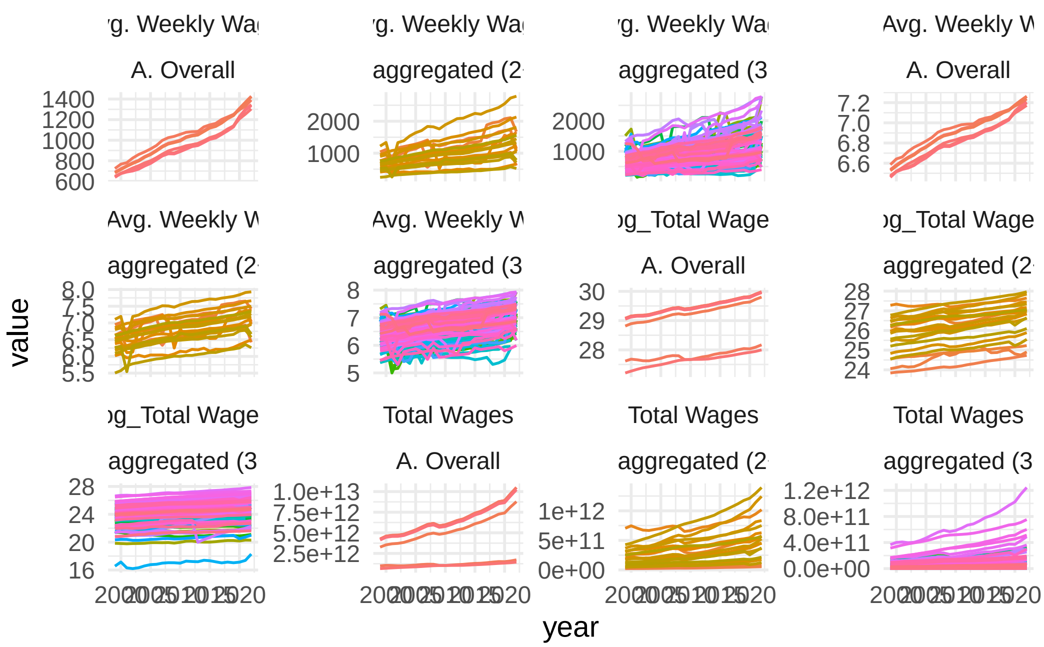
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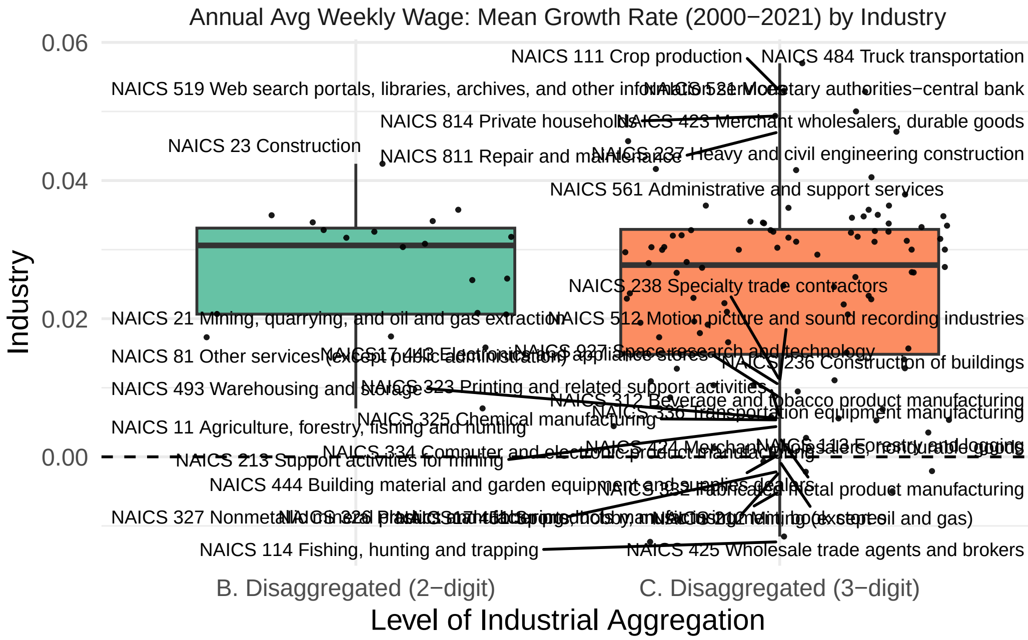


C.3 Panel VAR Specification

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

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.

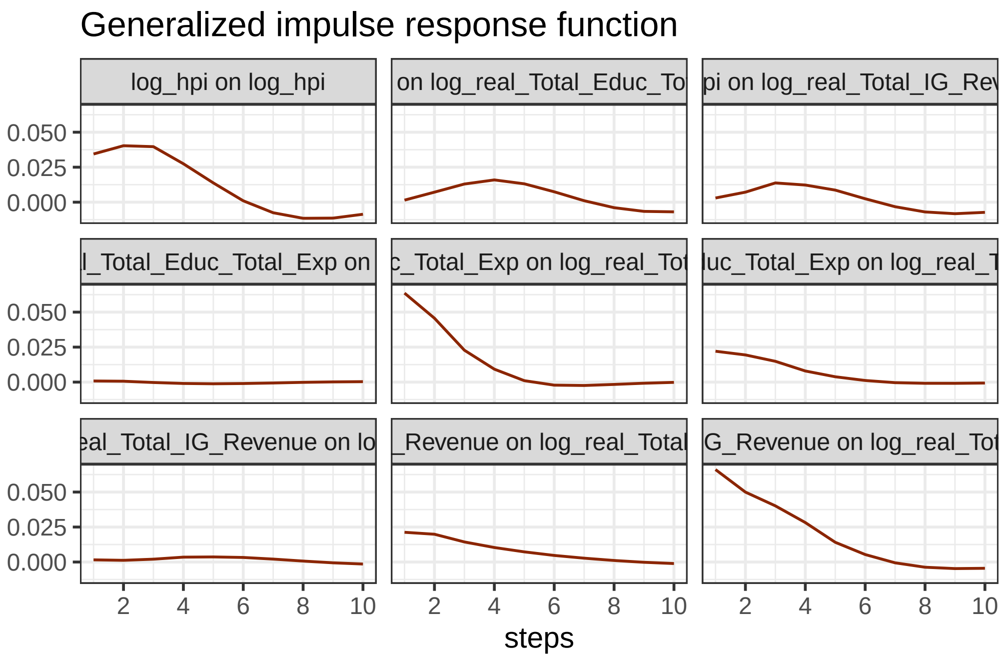
$$
Y_{it} = \begin{bmatrix} \log(\text{real Total Educ. Exp.})_{it} \\ \log(\text{real Total IG Revenue})_{it} \\ \log(\text{HPI})_{it} \end{bmatrix}, \quad X_{it} = \begin{bmatrix} \log(\text{real GDP})_{it} \\ \log(\text{wage})_{it} \end{bmatrix}
$$

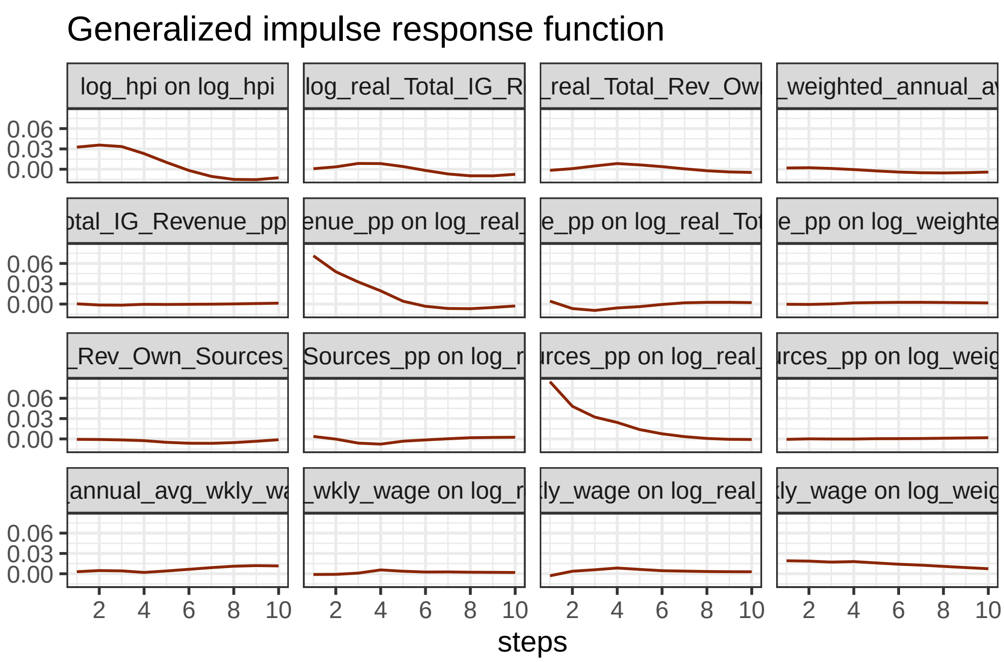
- A_1, A_2, A_3, A_4 are 3×3 coefficient matrices
- β is a 3×2 matrix of coefficients on the exogenous variables
- α_i is a vector of unit fixed effects
- ε_{it} is the error term

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





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