Uneven Wage Growth and Public Goods

The Case of US Public Education

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Abstract

This study estimates the elasticity of public education expenditure to changes in local 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.

Table of contents

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
- Include discussion of CFESIS result (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?
- 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 - 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 systematically. Please excuse lack of referencing to regression tables and figures at the moment.

2.0.1 Motivate with a puzzle or a problem

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 families 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

[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.

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

[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.

2.0.2 Clearly state your research question

Altogether, this evidence points to the value of identifying the extent to which expenditure on public education is reliant on local wage growth across the country. This work aims to answer the following research questions:

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

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

RQ3: In light of reforms to intergovernmental education funding to alleviate uneven educational expenditure, do intergovernmental transfers alleviate wealth-driven inequalities in public education expenditure?

2.0.3 Empirical approach

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

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 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 heterogeneity that these pooled estimates mask in our main analysis. Centrally, we employ an instrumental variable design using a VA-based shift share instrument 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 descriptive estimations, we find a strong positive relationship 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 wages using a shift-share exposure treatment estimation generally, and subsequently for specific industries and states and growth cohorts.
3. State-level estimation result.
4. Industry-level estimation result.

2.0.5 Value-added relative to related literature

2.0.6 Optional paragraphs: robustness checks, policy relevance

2.0.7 Roadmap

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 5 and Section 6 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.

[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.

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 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).

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
Intergovernmental 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

4.1 Baseline Estimates

First, we descriptively establish foundational relationships between local economic conditions that seem to reasonably generalise across the country using an AR(1) two-way fixed effects panel model.

However, given the high degree of both structural (state-specific tax, regulatory, and legislative regimes) and evolved heterogeneity (industrial activity, income, inequality, economic diversity) the proposed analysis necessitates a disaggregated estimation strategy to truly account for the heterogeneity across the units of observation. Therefore, subsequent treatment estimation is dedicated to state-by-state, industry-by-industry, and a growth cohort sub-sampling procedure taking into account legacy income and wage growth rates.

The Appendices provide national-level average treatment effects across all estimation strategies, for reference.

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:

[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.

[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 (David Dorn's Resource Page) (Fowler et al. 2024).

$$,Y_{it} = \beta_0 + \beta_x X_{it} + \delta_1 Enrollment_{it} + \delta_2 IGR_{it} + \alpha_i + \gamma_t + \varepsilon_{it}", \quad (1)$$

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 fact.). Slips in public education provision at such a young age could have scarring effects. α_i represents either a CZ or state fixed effect and γ_t represents year-fixed effects, respectively. ε_{it} represents the error term. We control for enrollment to account for scaling factors in education expenditure and intergovernmental transfers to account for the significant role of such transfers in funding education expenditure. X_{it} takes three forms represented by Equation 2, Equation 3, Equation 4 where h represents h-year time lags. We estimate all equations in levels and growth rates.

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

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

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

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 house prices.

Lagged economic indicators, particularly private industry GDP and average weekly wages, are positively and significantly associated with education spending. In the baseline specification (Column 1), the elasticity of education spending with respect to local GDP per capita (private industry) is positive and statistically significant once lagged values are included. A one-percent increase in local GDP per capita two years prior is associated with roughly a 0.1% increase in current education expenditure per pupil, suggesting that fiscal capacity effects unfold gradually over time. In the case of industry GDP, the magnitude of the coefficients increases with the number of lags, suggesting a gradual adjustment process by which local economic growth translates into higher public investment in education over time. For example, a 1% increase in lagged (t–2) real private GDP per capita is associated with a 0.06% increase in per-pupil spending. The house price index also enters positively and significantly but only contemporaneously reflecting the immediate link between house prices and the property taxes that fund public education. This points to the fundamental relationship between community asset wealth and public education expenditure.

Intergovernmental revenue per pupil emerges as the strongest and most consistent predictor of education expenditure after the auto-regressive coefficient. A 1% increase in intergovernmental transfers is associated with approximately a 0.2% increase in per-pupil education spending, controlling for CZ and year fixed effects. This finding highlights the importance of state and federal aid in sustaining local education budgets.

The growth rate regressions, while explaining less variance overall, largely confirm the patterns observed in the level specifications. Intergovernmental revenue growth remains a strong and highly significant determinant of education expenditure growth, with coefficients greater than 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 hinting at the relevance of our primary identifying relationship.

Taken together, these results offer three key insights. First, public education investment is strongly mediated by external fiscal flows, reaffirming the role of intergovernmental transfers in equalizing local education finance. Second, local labor market conditions, captured through wages and GDP, exert lagged, 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 which respond contemporaneously as a result of the direct mechanical link between property values and local public revenue generation.

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

We provide additional robustness checks of these relationships in the Appendix including esitmation using state-level fixed effects. However, given our interest in commuting-zone specific outcomes, we proceed with commuting zone level fixed effects.

Finally, we approach the estimation using levels. The coefficient on our AR(1) term is far from 1 which indicates that unit roots are unlikely to be present in our dependent variable.

Table 2: Descriptive Results in Levels

Dependent Variable:	(log) Elem.Ed.Exp.pp			
Model:	(1)	(2)	(3)	(4)
Variables				
(log) Real GDP Priv. Industry pc	0.0132			0.0089
	(0.0152)			(0.0164)
(log,l1) Real GDP Priv. Industry pc	0.0520***			0.0346**
	(0.0155)			(0.0173)
(log,l2) Real GDP Priv. Industry pc	0.0583***			0.0496***
	(0.0138)			(0.0129)
(l1, log) Elem.Ed.Exp.pp	0.5071***	0.5282***	0.5322***	0.4922***
	(0.0142)	(0.0177)	(0.0179)	(0.0146)
(log) IG Revenue pp	0.2361***	0.2103***	0.2112***	0.2340***
	(0.0205)	(0.0230)	(0.0218)	(0.0204)
(log) Enrollment	-0.1796***	-0.1925***	-0.2041***	-0.2130***
	(0.0142)	(0.0142)	(0.0155)	(0.0162)
(log) Annual Avg. Wkly. Wage		0.0624		-0.0315
		(0.0493)		(0.0618)
(log, l1) Annual Avg. Wkly. Wage		0.1936***		0.1446**
		(0.0548)		(0.0616)
(log, l2) Annual Avg. Wkly. Wage		0.0544		-0.0415
		(0.0480)		(0.0452)
(log) House Price Index			0.1080***	0.0698***
			(0.0210)	(0.0202)
(log, l1) House Price Index			0.0292	0.0131
			(0.0326)	(0.0348)
(log, l2) House Price Index			0.0008	-0.0052
			(0.0213)	(0.0226)
Fixed-effects				
unit	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	12,084	12,720	12,078	11,493
R^2	0.90687	0.90573	0.90926	0.91149
Within R^2	0.52065	0.52306	0.52409	0.52719

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

Table 3: Descriptive Results in Growth Rates

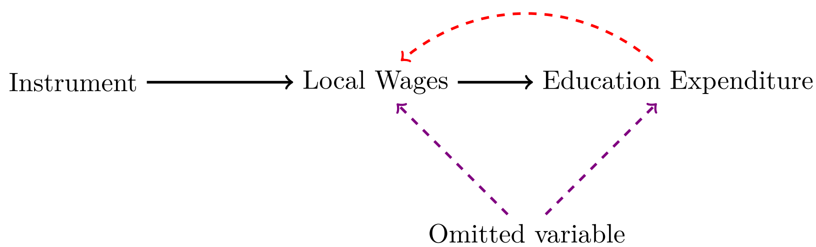
Dependent Variable:	(GR) Elem.Ed.Exp.pp			
Model:	(1)	(2)	(3)	(4)
Variables				
(GR, l1) Elem.Ed.Exp.pp	-0.0604***	-0.0394***	-0.0443***	-0.0729***
	(0.0149)	(0.0098)	(0.0093)	(0.0142)
(GR) Real GDP Priv. Industry pc	0.0030			-0.0129
	(0.0135)			(0.0152)
(GR,l1) Real GDP Priv. Industry pc	0.0510***			0.0241
	(0.0148)			(0.0154)
(GR,l2) Real GDP Priv. Industry pc	0.0199***			0.0120**
	(0.0070)			(0.0054)
(GR) IG Revenue pp	0.3044***	0.3033***	0.3033***	0.2976***
	(0.0325)	(0.0317)	(0.0307)	(0.0319)
(GR) Enrollment	-0.5897***	-0.5899***	-0.6056***	-0.6089***
	(0.0432)	(0.0430)	(0.0456)	(0.0478)
(GR) Annual Avg. Wkly. Wage		0.0233		0.0139
		(0.0459)		(0.0533)
(GR, l1) Annual Avg. Wkly. Wage		0.1815***		0.1210**
		(0.0441)		(0.0495)
(GR, l2) Annual Avg. Wkly. Wage		0.3066***		0.2451***
		(0.0557)		(0.0523)
(GR) House Price Index			0.0947***	0.0789***
			(0.0199)	(0.0212)
(GR, l1) House Price Index			0.0795***	0.0646**
			(0.0235)	(0.0253)
(GR, l2) House Price Index			0.0732***	0.0587***
			(0.0182)	(0.0194)
Fixed-effects				
unit	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	12,083	12,719	12,052	11,467
R^2	0.27126	0.27420	0.28649	0.29204
Within R^2	0.22438	0.22691	0.23366	0.24074

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 provision local labour markets with individuals with diverse human capital (though, as described in the previous section, we protect against this due to our choice of primary education as the dependent variable). Therefore, we instrument local wages using a shift-share based instrument of 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 instrument. A shift-share instrument interacts local industry shares with national industry-level growth rates to obtain a plausibly exogenous local shock. In the context of this work, we construct the instrument by interacting a commuting-zone level constant industrial employment share variable with national industry-level 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:

[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].

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

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 Bureau of Economic Analysis, we construct a shift-share Bartik instruments at the commuting zone level using local employment shares by industry and national changes in industry-specific real value added represented in Equation 6. G_{njt} represents national-level changes in value added in industry j in time t and $\frac{N_{ij\tau}}{N_{i\tau}}$ represents the 'sensitivity' of a CZ to these national shocks proxied by an initial share of local employment in industry j in a baseline time period τ. The product of these two values defines the shift-share indicator $\tilde{Z}_{i,t,s}$. In order to construct the share portion, we compute the total local share of employment in a particular industry j. Due to challenges with missing data, we compute an average share across 2001-2005 as our 'base year'.

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

11

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

This yields a 2SLS AR(1) model defined by the first- and second-stage regressions represented in Equation 7 and Equation 8. Due to the likely presence of time-dynamic effects, we include contemporaneous, 1-year, 2-year time lags as instruments.

$$\textbf{(First stage)} \qquad X_{it} \;=\; \pi_0 \;+\; \phi\, X_{i,t-1} \;+\; \sum_{\ell=0}^{2} \pi_{Z,\ell}\, \tilde{Z}_{i,t-\ell} \;+\; \theta_1\, \text{Enrollment}_{it} \;+\; \theta_2\, \text{IGR}_{it} \;+\; \alpha_i \;+\; \lambda_t \;+\; u_{it}, \tag{7}$$

$$\textbf{(Second stage)} \qquad Y_{it} \;=\; \beta_0 \;+\; \phi\text{'}\, X_{i,t-1} \;+\; \beta_X\, \widehat{X} it \;+\; \delta_1\, \text{Enrollment} it \;+\; \delta_2\, \text{IGR} it \;+\; \alpha_i \;+\; \lambda_t \;+\; \varepsilon it \tag{8}$$

We compute the relevant shift-share instrument across 19 two-digit NAICS industrial categories listed in the table below. Given industry-level disaggregation of local employment data requires data suppression for anonymity reasons, Figure 2 displays the data coverage of our commuting zone level shift-share instruments. Given the high degree of missingness in the 3-digit categorisation we proceed with the 2-digit NAICS codes in the rest of the work. We should arguably exclude industries 61 and 92 from the shift-share instrument as there represent the wages of public administration and educational services employees.

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

[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 only 2001, 2004, and 2005. Will include a more systematic testing of this in the appendix.

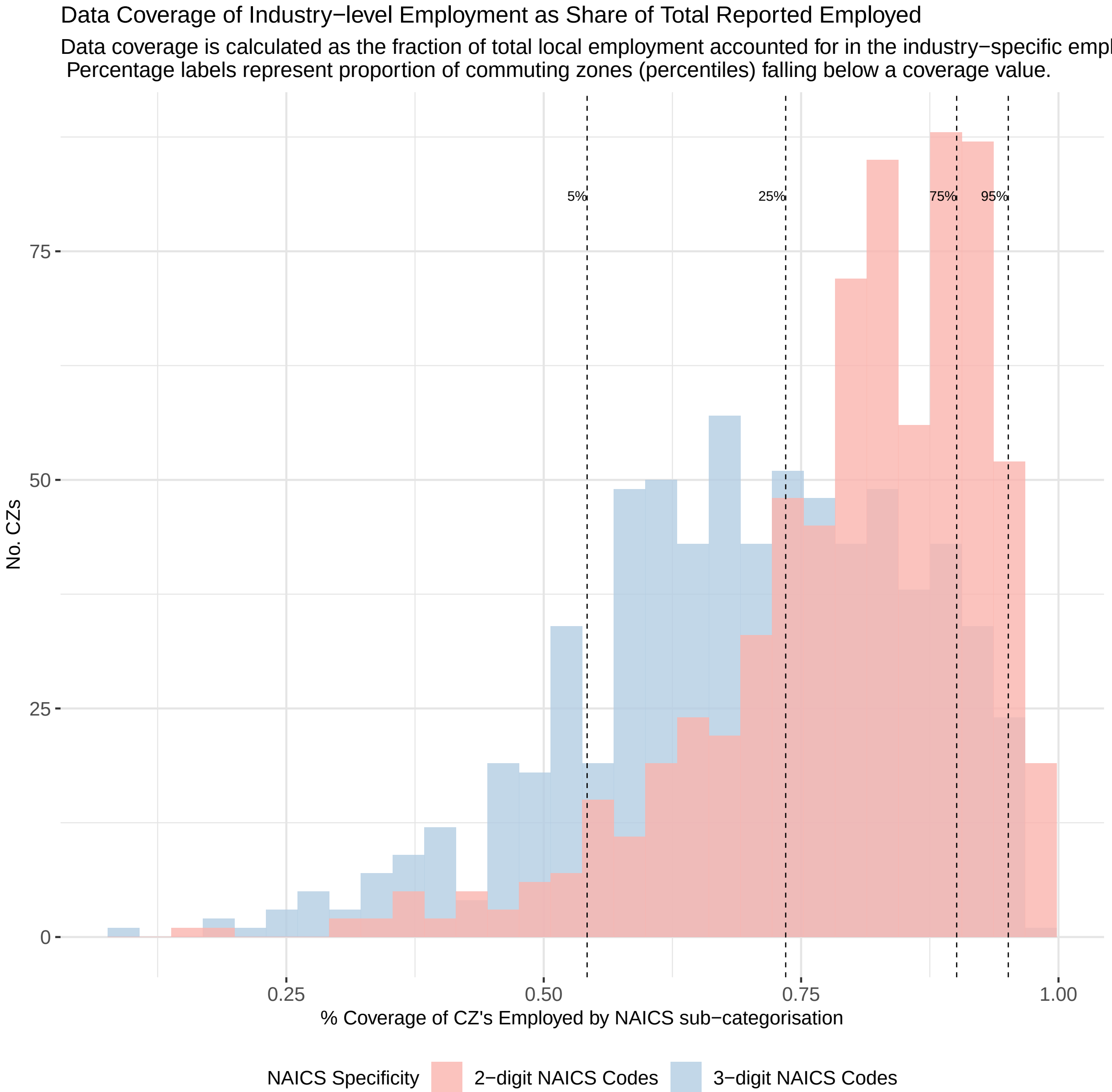


Figure 2: Data Coverage of Industry-level Employment as Share of Total Reported Employed

In Table 4, we demonstrate a strong and highly significant first-stage relationship wherein our VA-based shift-share instrument indicates a strong positive contemporaneous relationship with local wages. We find that a 1% increase in our shift-share instrument raises local wages by 0.25% in the same year though these wages rebound as evidenced by the statistically significant negative coefficient of a similar magnitude on the l1 regressor. Essentially, the time dynamics indicate that local wages respond quickly to national sectoral shocks, though not permanently. This can likely be interpreted as a temporary local demand boom following industry-level value added shocks. This provides additional evidence that the shift-share instrument captures short-term exogenous variation rather than slow-moving trends in local wages, boosting credibility in our instrumental variable relevance. The growth rate specification in columns 3-4 provide further evidence of the transitory nature of these demand shocks as only the contemporaneous growth rate effect is significant.

The first-stage F-statistic using the level SS instrument is well above conventional weak instrument thresholds confirming instrument relevance. Furthermore, 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 and the importance of incorporating time-lagged instruments.

Using wage shocks in levels yields strong instruments, defensible first-stage F-statistics, and stable second-stage estimates: positive (negative) local wage shocks robustly decrease (increase) education spending.

Taken together, these two specifications yield consistent and meaningful results wherein the shift-share instrument allows for the causal identification of negative relationship between local wage shocks and education expenditure. The similarity in magnitude of the two designs implies that the estimated effect is robust to the timing and persistence of the underlying economic shocks. Most importantly, this indicates that the mechanism identified is driven by temporary demand shocks rather than permanent income effects. Interpretation of this results requires a further exploration of local heterogeneity, outlined in the following sections.

For example, one could interpret this result as either a confirmation of resource course dynamics in communities dependent on industries employing lower-skill workers. If these effects are concentrated in less economically prosperous areas, local wage booms in particular sectors might be distracting public and administrative attention away from local public education investments. On the other hand, if these effects are driven by wage booms in communities reliant on higher-skill labour, one could see this as a potential feedback between decreases in intergovernmental revenues which are intended to be needs-based, even though the specification controls for this effect, or potentially a crowding out of public education by private education. The results below do not provide a meaningful ability to distinguish the relative accuracy of either of these hypotheses.

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

Dependent Variables:	(log) Annual Avg. Wkly. Wage	(log) Elem.Ed.Exp.pp	(log) Annual Avg. Wkly. Wage	(log) Elem.Ed.Exp.pp
IV stages	First	Second	First	Second
Model:	(1)	(2)	(3)	(4)
Variables				
VA SS (Lvl)	0.2450**			
	(0.1019)			
VA SS (Lvl, l1)	-0.2684***			
	(0.0620)			
VA SS (Lvl, l2)	-0.1037			
	(0.1060)			
(l1, log) Elem.Ed.Exp.pp	0.0676***	0.6561***	0.0672***	0.6452***
	(0.0086)	(0.0619)	(0.0086)	(0.0559)
(log) IG Revenue pp	0.0318***	0.2915***	0.0309***	0.2866***
	(0.0097)	(0.0355)	(0.0098)	(0.0345)
(log) Real GDP Priv. Industry pc	0.1473***	0.3926***	0.1480***	0.3685***
	(0.0153)	(0.1315)	(0.0152)	(0.1235)
(log) Enrollment	0.0868***	-0.0092	0.0820***	-0.0226
	(0.0146)	(0.0774)	(0.0148)	(0.0684)
(log) Annual Avg. Wkly. Wage		-1.974**		-1.812**
		(0.8249)		(0.7975)
VA SS (GR)			0.2945***	
			(0.1009)	
VA SS (GR,l1)			0.0693	
			(0.1046)	
VA SS (GR,l2)			-0.0886	
			(0.0905)	
Fixed-effects				
unit	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	12,084	12,084	12,084	12,084
R2 (1st stage)	0.97757		0.97752	
Adj. R2 (1st stage)	0.97628		0.97623	
F-test (IV only)	17.764	50.004	9.3872	22.248
F-test (IV only), p-value	1.68×10^{-11}	1.62×10^{-12}	3.41×10^{-6}	2.42×10^{-6}
Wu-Hausman		54.645		24.537
Wu-Hausman, p-value		1.54×10^{-13}		7.4×10^{-7}
Wald (IV only)	7.8955	5.7282	9.2600	5.1615
Wald (IV only), p-value	2.94×10^{-5}	0.01671	4.1×10^{-6}	0.02311

Clustered (unit) 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. 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 (1) explore various metrics of local economic growth and decline to partition our sample in a data-driven manner, employ (2) industry-by-industry and (2) state-by-state estimations in our IV specifications using our VA-based shift-share instrument.

4.3.1 Declining vs. Growing Regions

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

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

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

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

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

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

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 4 plots the distribution of values of α_g where the vertical dashed lines represent the 25th and 75th percentiles. I recently attended a talk that noted this is a common distribution for personal income growth rates. Might be interesting to note that here.

Next, 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

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

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.

Finally, Figure 6 represents the distribution of the loadings on the residualised state factors and the national growth rates at the commuting zone level. I wonder if there is a potential interpretation for the right skew of the betas. I will look into this.

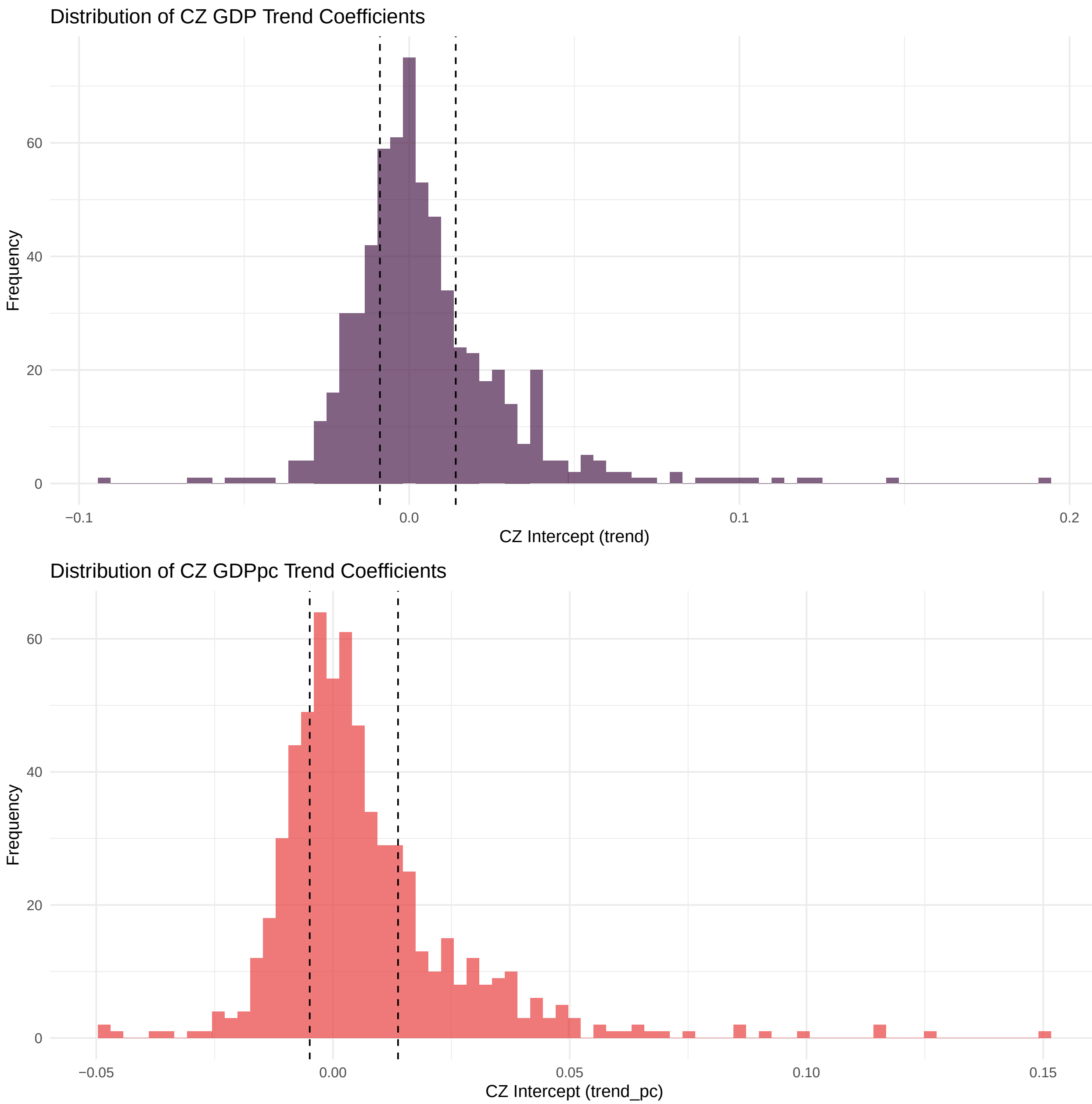


Figure 3: Distribution of GDP Trend Coefficients

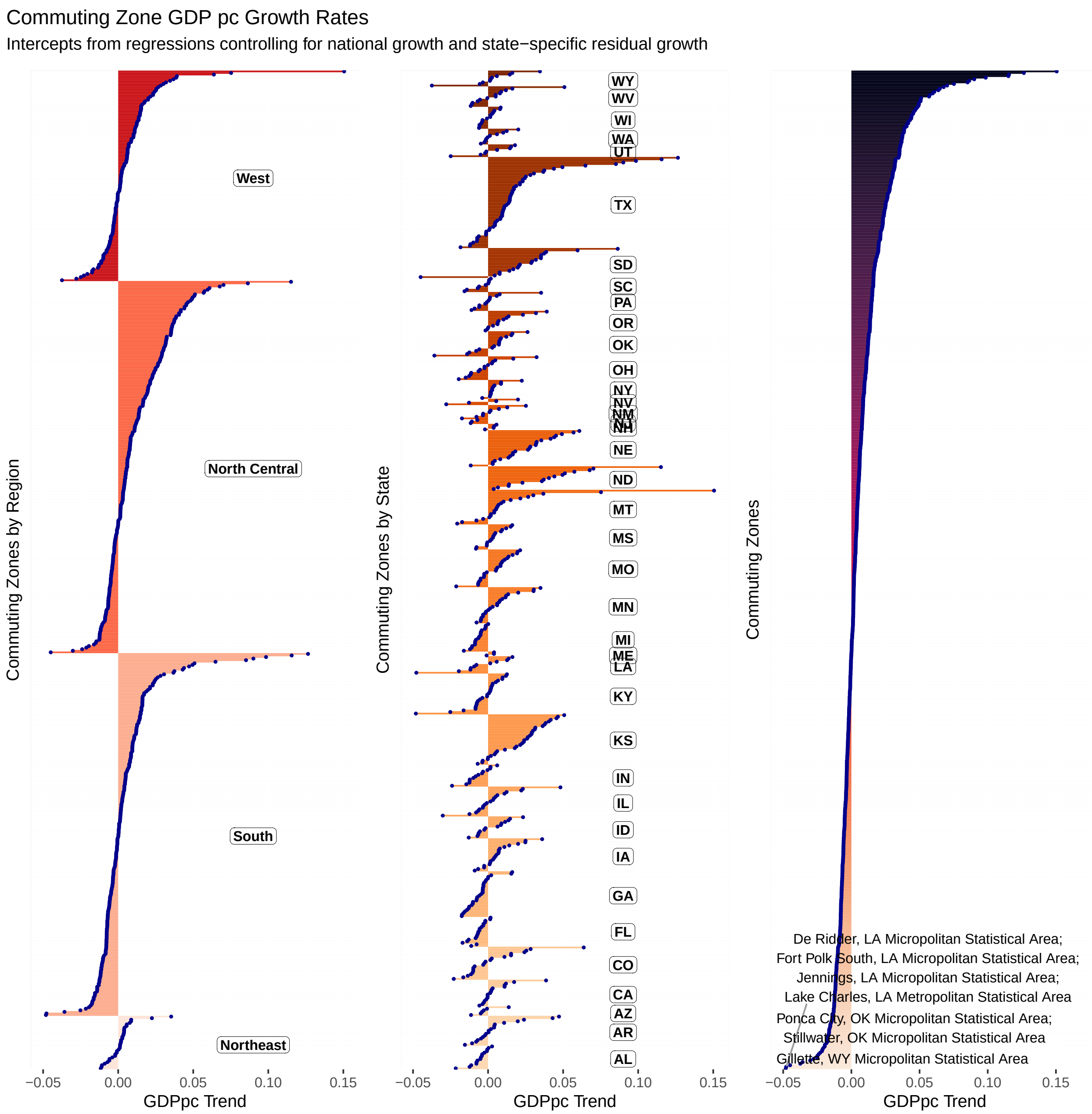


Figure 4: Lollipop Plot of GDPpc Growth Rates

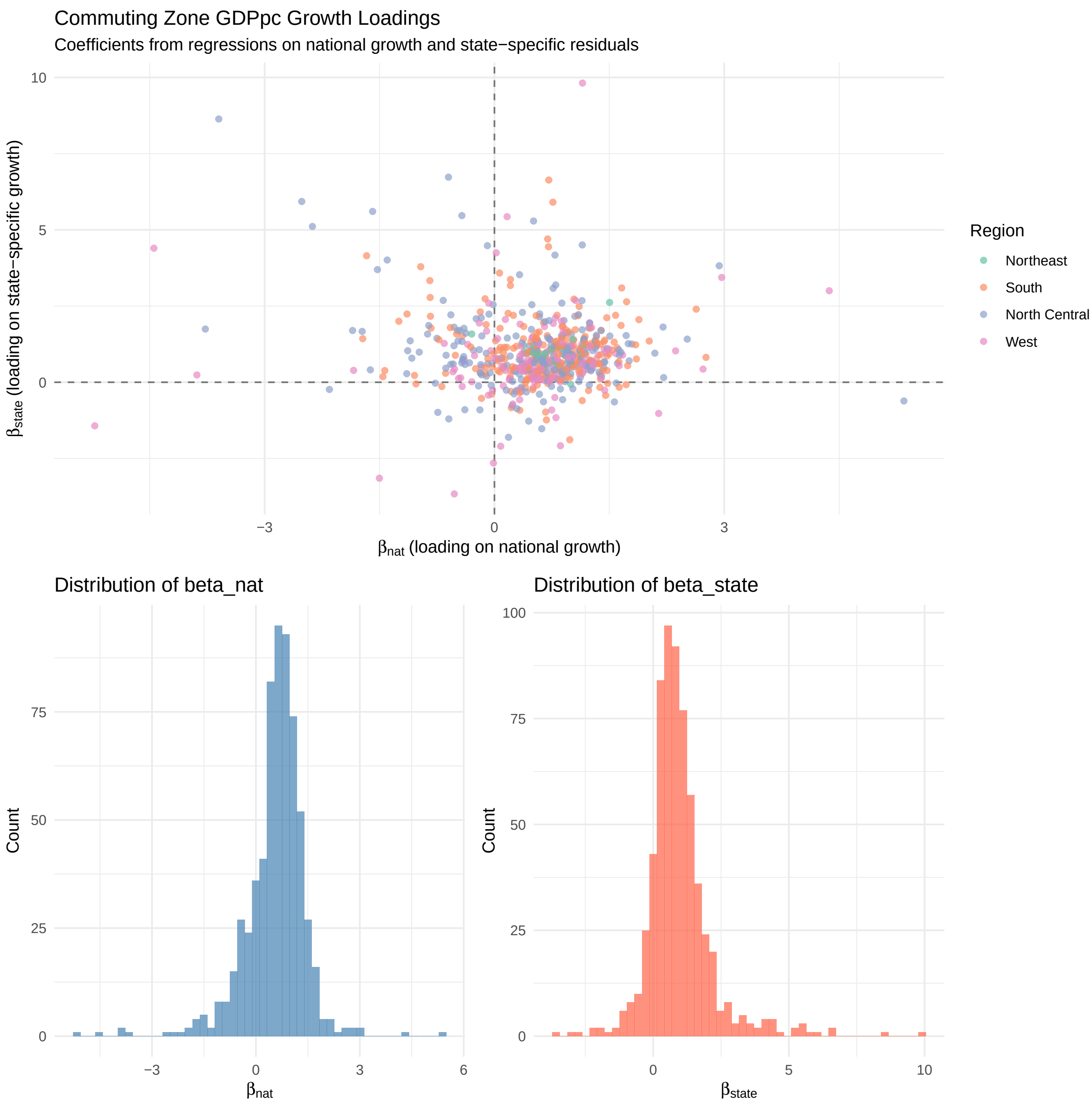


Figure 5: Beta Loadings on GDP pc Growth Rates by CZ

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 11 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 decile-decile plot demonstrates a noisy relationship largely driven by certain outliers.

Figure 14 presents a correlation coefficient by commuting zone between the two rates, providing greater detail on this relationship. Georgia is an incredibly interesting case in which nearly all cmomuting zones have relatively declining GDPpc growth rates but relatively growing wage growth rates.

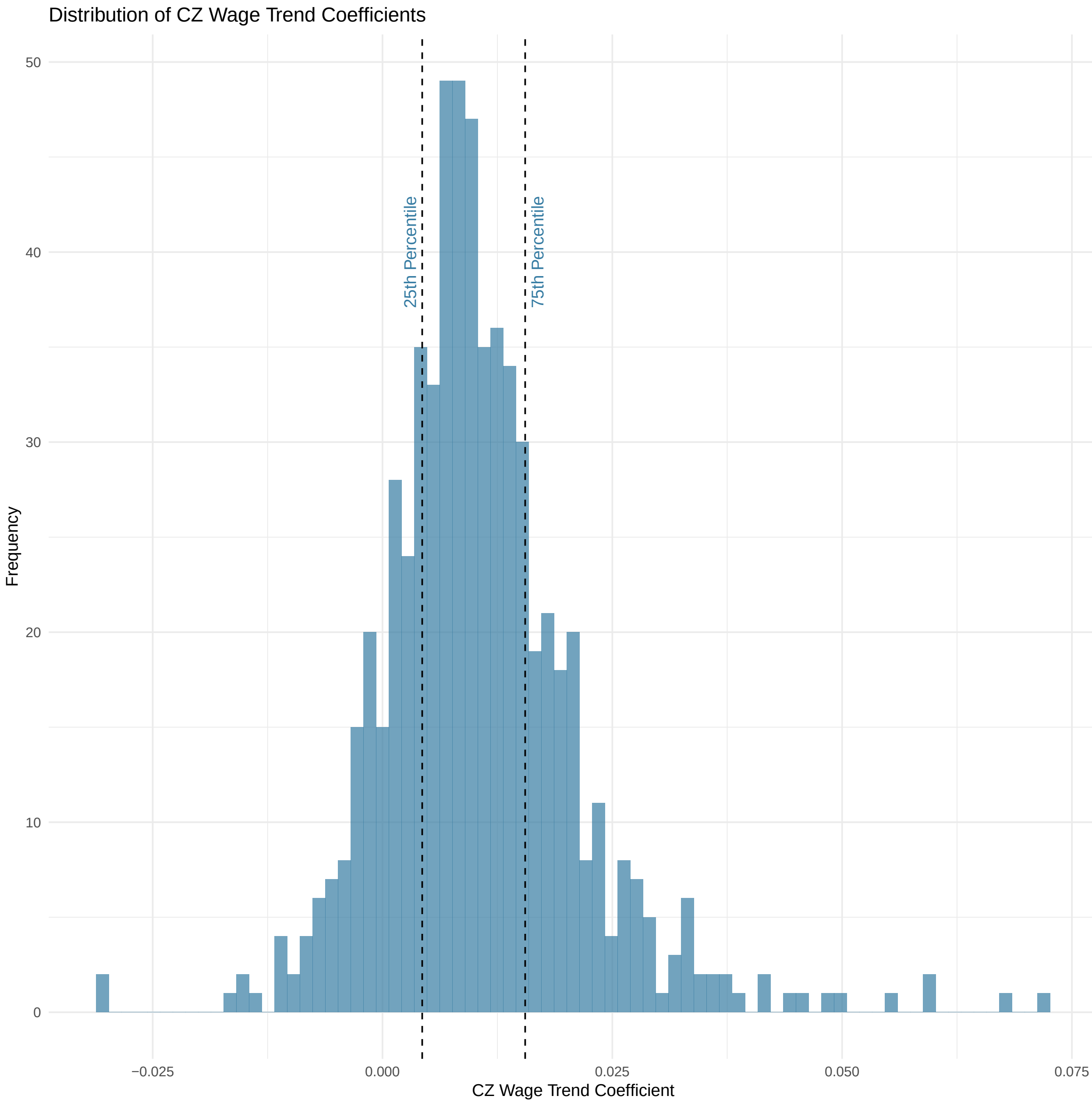


Figure 6: Distribution of Wage Trend Coefficients

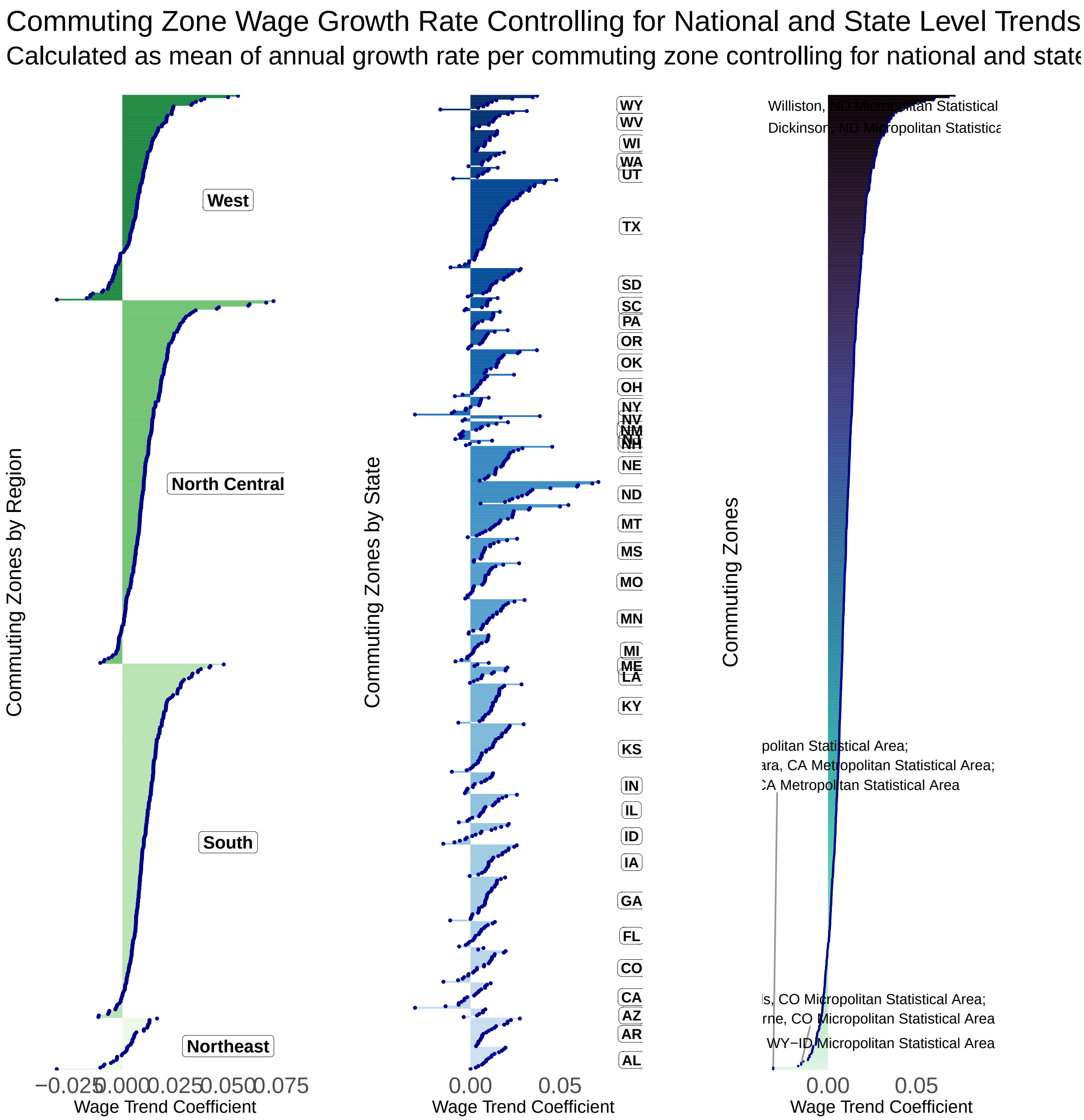


Figure 7: Lollipop Plot of Wage Growth Rates

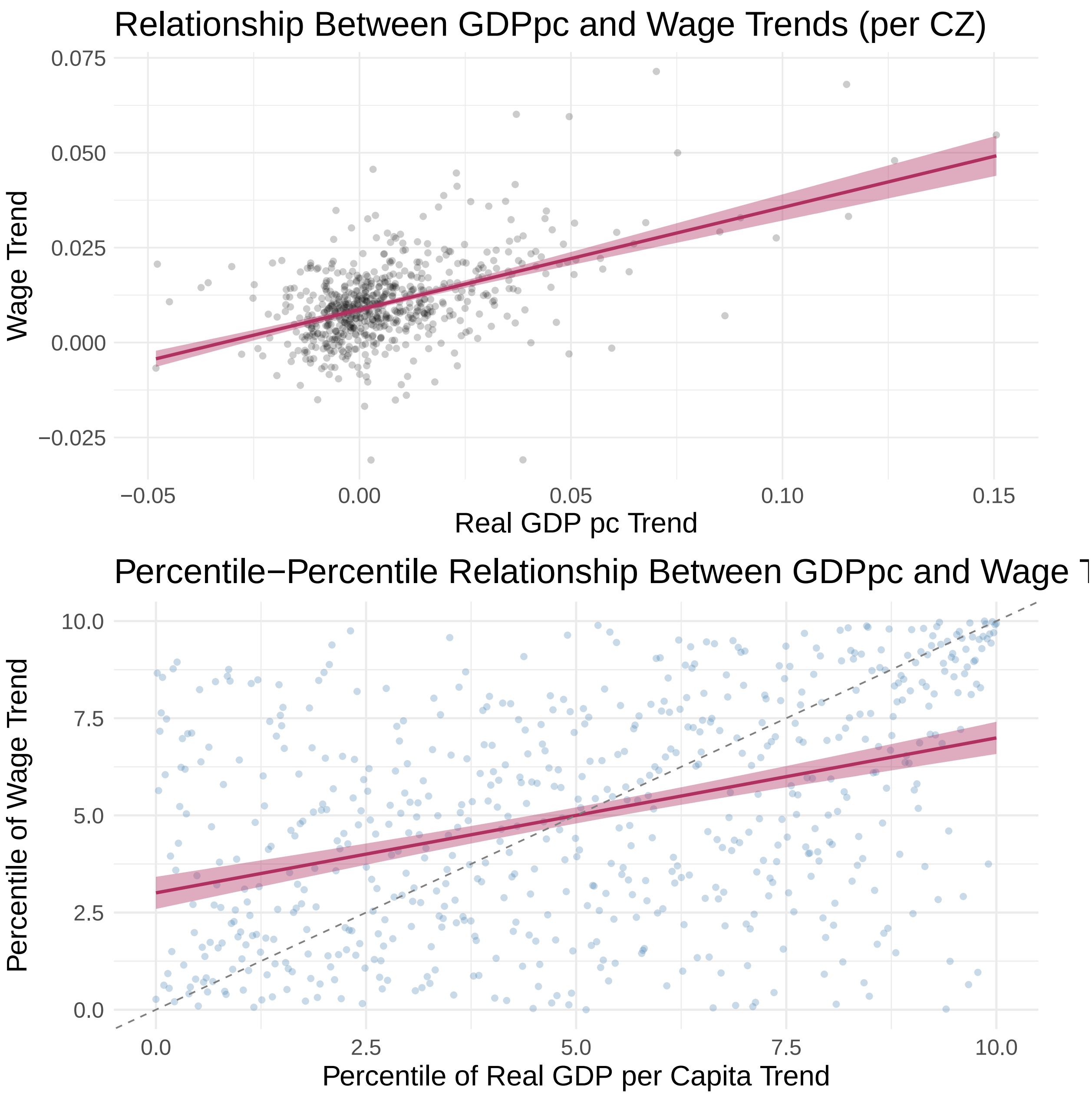


Figure 8: Correlation between Wage and GDPpc Trends

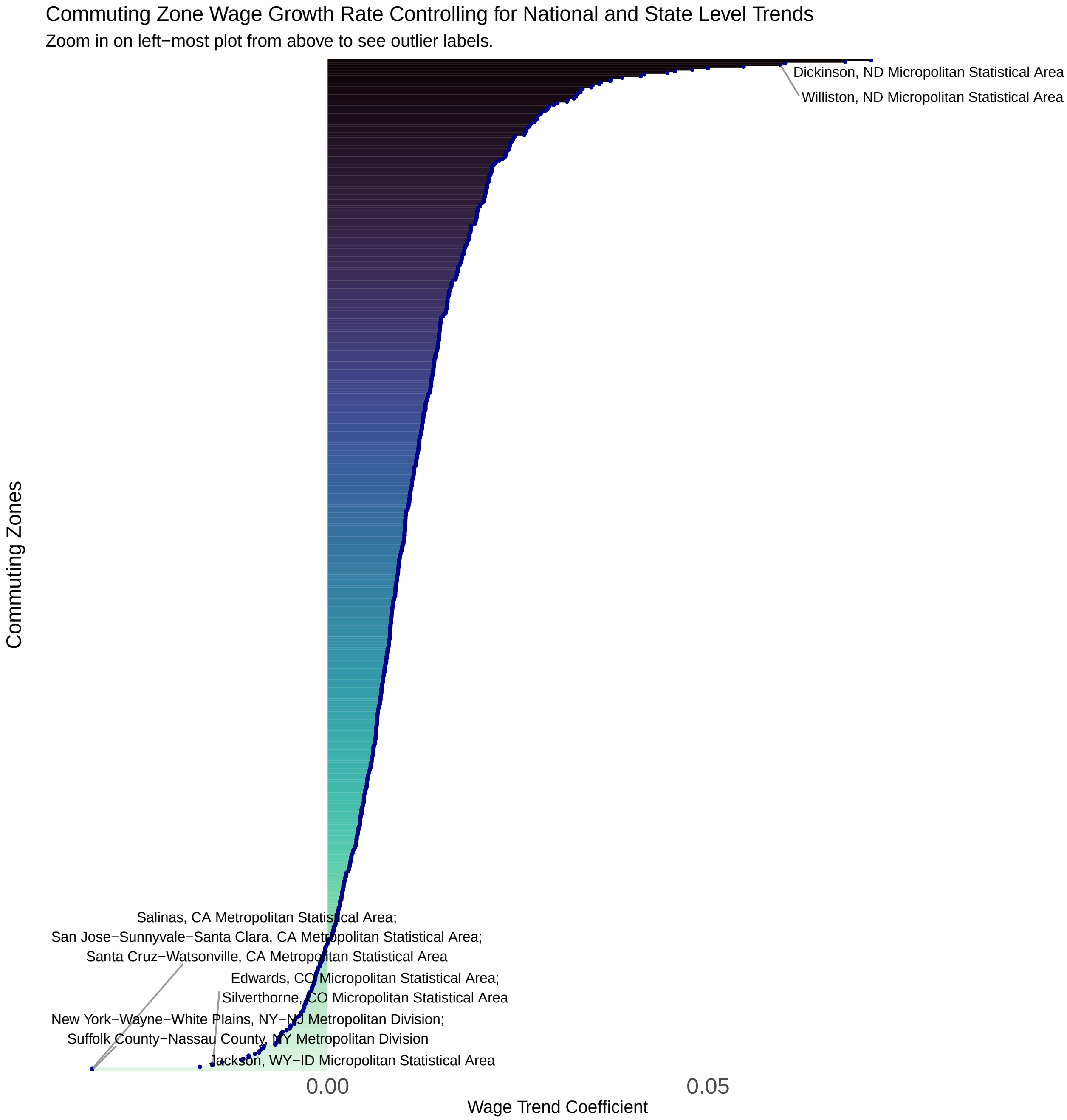


Figure 9: Lollipop Plot of Wage Growth Rates - right panel of Figure 8

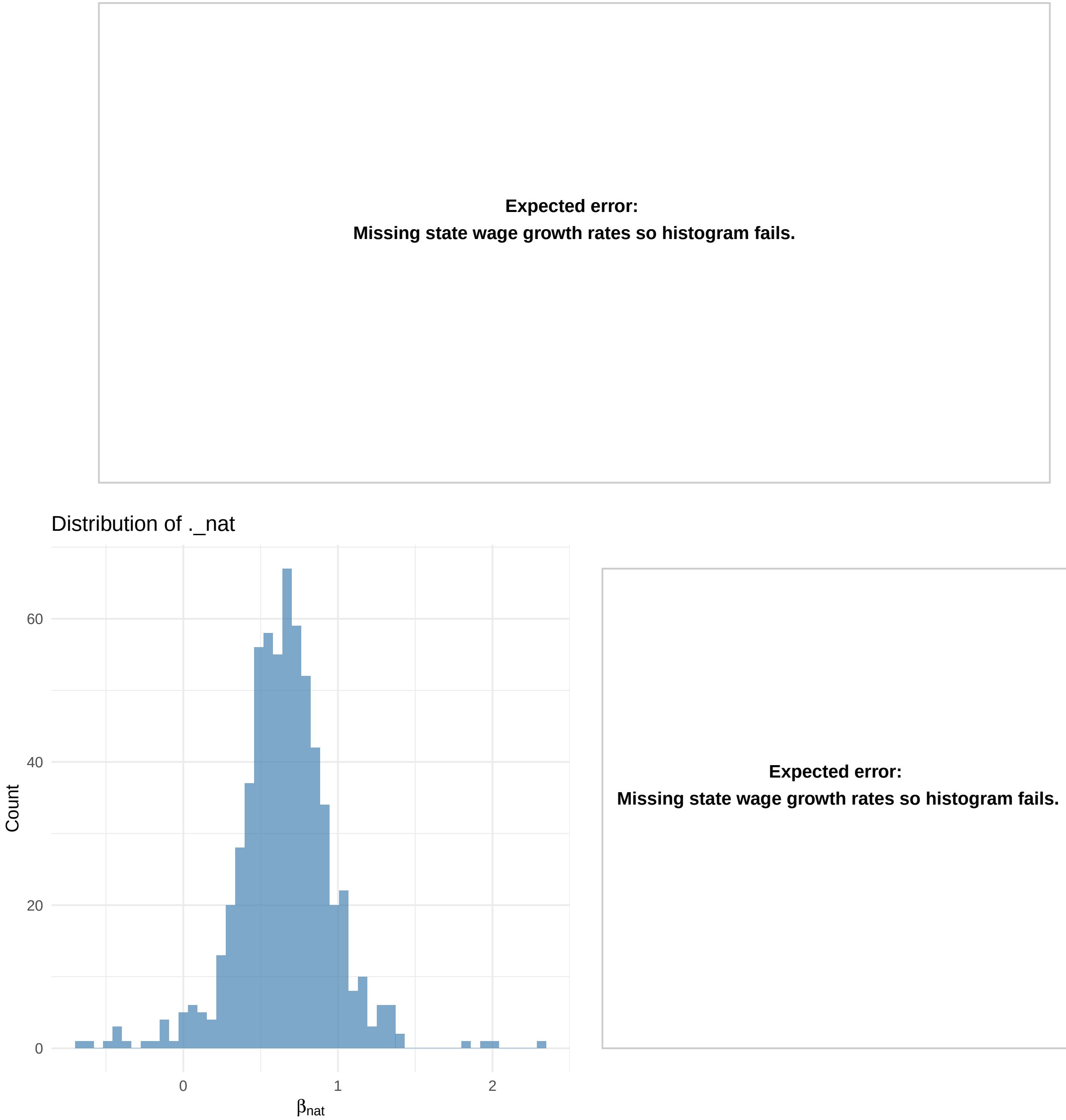


Figure 10: Beta Loadings on Wage Growth Rates by CZ

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 where the relationship is positive or negative.

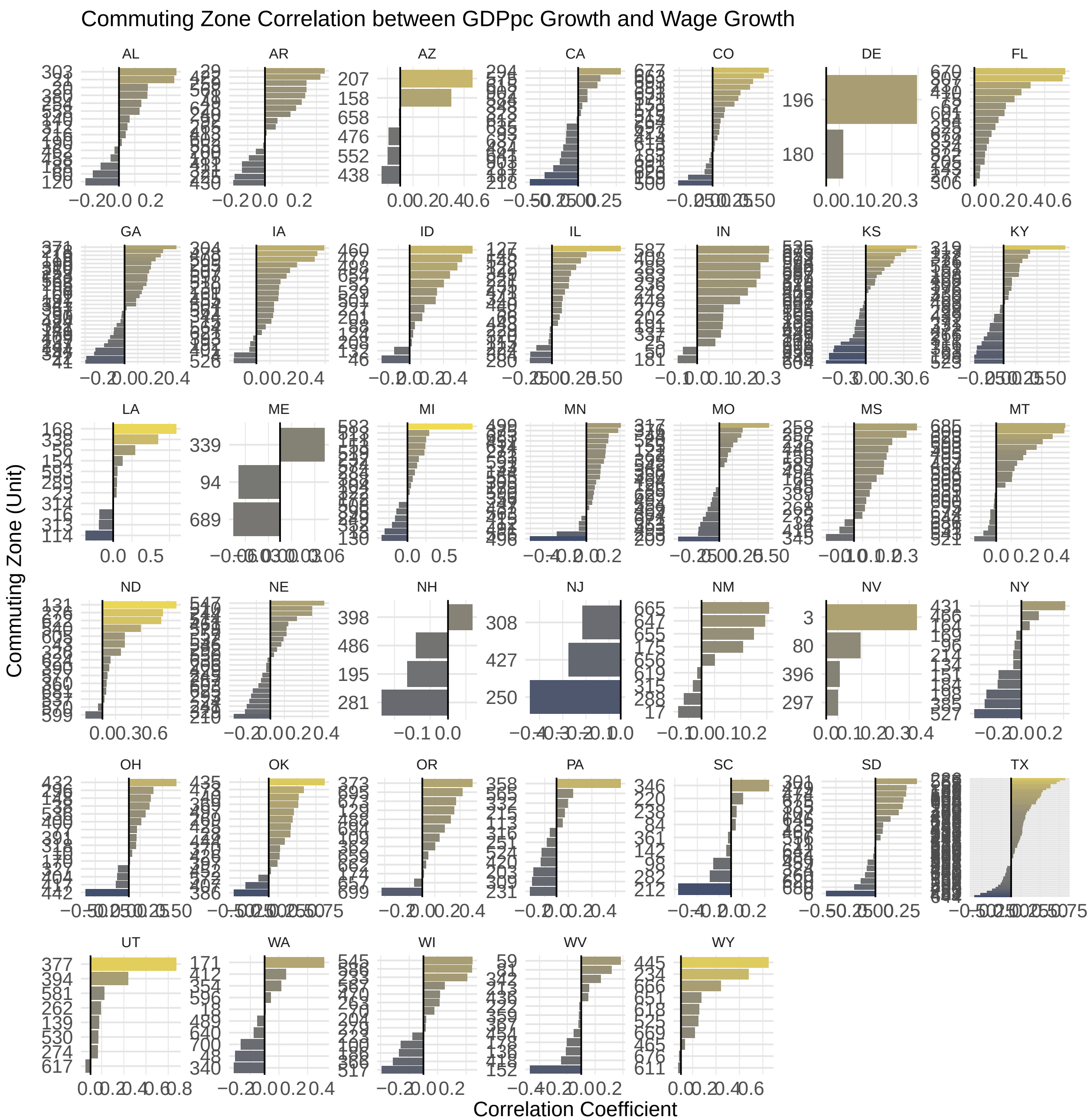


Figure 11: Correlation Between GDP Growth Rates and Wage Growth Rates by State

4.3.2 Sample Partitioning by Growth Rates

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

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

Category	Definition Wage	Definition GDP
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 6 partitions CZs by α_w. Interestingly, we see that the effect of intergovernmental revenue is nearly half the size in hyper-growing areas than the hyper-declining areas indicating the importance of intergovernmental transfers in areas where wages are declining. Additionally, the scaling effects of enrollment are only significant in regions where wages exhibit high growth or are moderately declining. Furthermore, the role of private industry GDP in local elementary education expenditure explains variation in education expenditure in those regions in which scaling laws do not seem to apply. The switch in significance between enrollment and private industry GDP contributions is really interesting. Wonder if there is an interesting interpretation here?. Most importantly though, the negative relationship between annual wages and elementary education expenditure is only present in areas that exhibit historically hyper-declining or moderately growing wage trends. Again, this might change once I incorporate the state-level trends into the calculation of α_w.. Additionally, the first-stage F statistics only support causal identification in these columns.

In regions where wages are already declining, positive wage shocks decrease spending on local public education providing potential support for a story in which regions with declining wages de-prioritise education when positive wage shocks "come to town." In the case of moderately growing regions, I am thinking of a few different interpretations here but not quite sure yet....

Table 7 partitions CZs by long-run GDP per capita trends. When partitioning the sample by GDP growth rates, the interpretation is more straight-forward. Areas in which GDP is exhibiting relative decline, the causal relationship between wages and education expenditure is consistent with high first stage F statistics, convincing performance on the Wu-Hausman endogeneity test, and defensible, albeit weak, performance on the Wald test. These results suggest an asymmetric fiscal response where wage shocks in areas whose GDP is declining, have a negative effect on local public education spending, in contrast to growing regions.

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

Dependent Variable:	(log) Elem.Ed.Exp.pp				
	All	Hyper-Declining (Wage)	Declining (Wage)	Growing (Wage)	Hyper-Growing (Wage)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
(log) Annual Avg. Wkly. Wage	-1.974**	-2.192*	0.1749	-1.197**	0.5982
	(0.8249)	(1.300)	(0.5914)	(0.5414)	(0.4487)
(l1, log) Elem.Ed.Exp.pp	0.6561***	0.6395***	0.5683***	0.5924***	0.5086***
	(0.0619)	(0.0791)	(0.0597)	(0.0414)	(0.0376)
(log) IG Revenue pp	0.2915***	0.3234***	0.2155***	0.2700***	0.1318***
	(0.0355)	(0.0476)	(0.0327)	(0.0295)	(0.0455)
(log) Real GDP Priv. Industry pc	0.3926***	0.4859**	0.0564	0.2712***	0.0192
	(0.1315)	(0.2240)	(0.1127)	(0.0837)	(0.0507)
(log) Enrollment	-0.0092	-0.0227	-0.1752***	-0.0754	-0.2309***
	(0.0774)	(0.1103)	(0.0615)	(0.0492)	(0.0572)
Fixed-effects					
unit	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	12,084	3,021	1,520	10,564	3,021
F-test (IV only)	50.004	12.043	0.13431	21.660	4.3224
F-test (IV only), p-value	1.62×10^{-12}	0.00053	0.71406	3.29×10^{-6}	0.03770
Wu-Hausman	54.645	13.959	0.01551	25.182	3.4490
Wu-Hausman, p-value	1.54×10^{-13}	0.00019	0.90091	5.31×10^{-7}	0.06340
Wald (IV only)	5.7282	2.8408	0.08749	4.8879	1.7771
Wald (IV only), p-value	0.01671	0.09200	0.76743	0.02707	0.18261

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

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

Dependent Variable:	(log) Elem.Ed.Exp.pp				
	All	Hyper-Declining (GDP)	Declining (GDP)	Growing (GDP)	Hyper-Growing (GDP)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
(log) Annual Avg. Wkly. Wage	-1.974**	-1.095**	-1.333**	0.1730	0.1270
	(0.8249)	(0.4834)	(0.5584)	(0.4163)	(0.3819)
(l1, log) Elem.Ed.Exp.pp	0.6561***	0.6346***	0.6258***	0.4953***	0.4975***
	(0.0619)	(0.0435)	(0.0436)	(0.0301)	(0.0321)
(log) IG Revenue pp	0.2915***	0.2581***	0.2934***	0.2113***	0.1970***
	(0.0355)	(0.0420)	(0.0370)	(0.0328)	(0.0419)
(log) Real GDP Priv. Industry pc	0.3926***	0.2405***	0.2898***	0.0736	0.0895**
	(0.1315)	(0.0893)	(0.1027)	(0.0554)	(0.0440)
(log) Enrollment	-0.0092	-0.1380***	-0.0994**	-0.1925***	-0.2177***
	(0.0774)	(0.0392)	(0.0399)	(0.0474)	(0.0448)
Fixed-effects					
unit	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	12,084	3,021	5,016	7,068	3,021
F-test (IV only)	50.004	29.968	38.265	0.31864	0.19999
F-test (IV only), p-value	1.62×10^{-12}	4.76×10^{-8}	6.67×10^{-10}	0.57245	0.65476
Wu-Hausman	54.645	41.588	49.254	0.04377	9.88×10^{-6}
Wu-Hausman, p-value	1.54×10^{-13}	1.32×10^{-10}	2.57×10^{-12}	0.83429	0.99749
Wald (IV only)	5.7282	5.1323	5.6978	0.17264	0.11061
Wald (IV only), p-value	0.01671	0.02355	0.01702	0.67779	0.73948

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

4.3.3 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 12 demonstrates that states contain anywhere from 2 (Delaware) to 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).

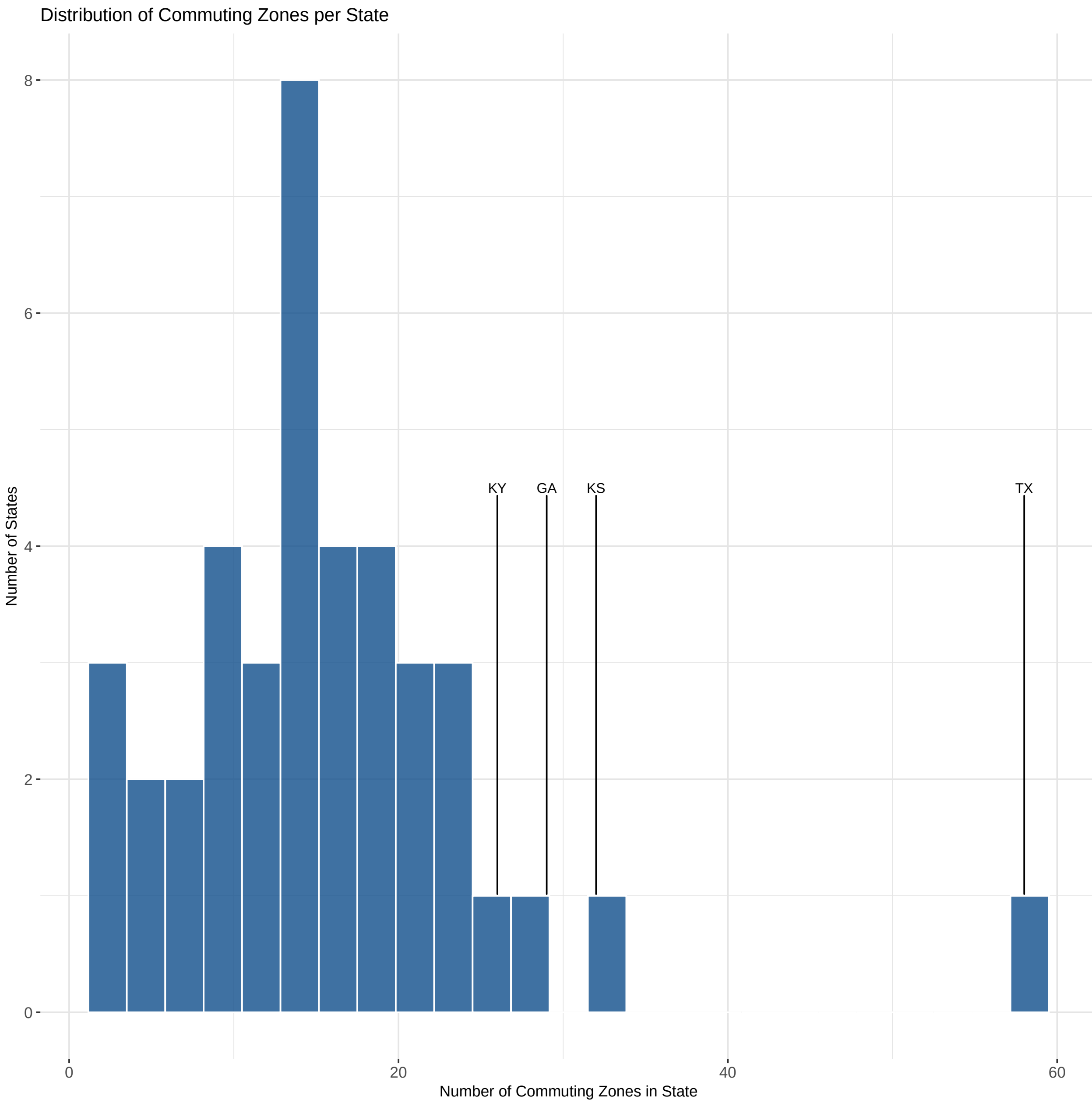


Figure 12: Histogram: Commuting Zones by State

We report the regression coefficients of these estimations in Figure 13. 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 of the treatment variable, the linear combination of which form the total dynamic effect represented by the purple dots. Majority of states see positive elasticities in relation to wage changes in this descriptive specification, but some (Kansas, Wyoming, Missouri, Washington) see negative elasticities. I think we should have a commuting zone N sample size exclusion restriction here. The results for Maine, New Jersey, and Delaware, for example are based on only 2-3 units.

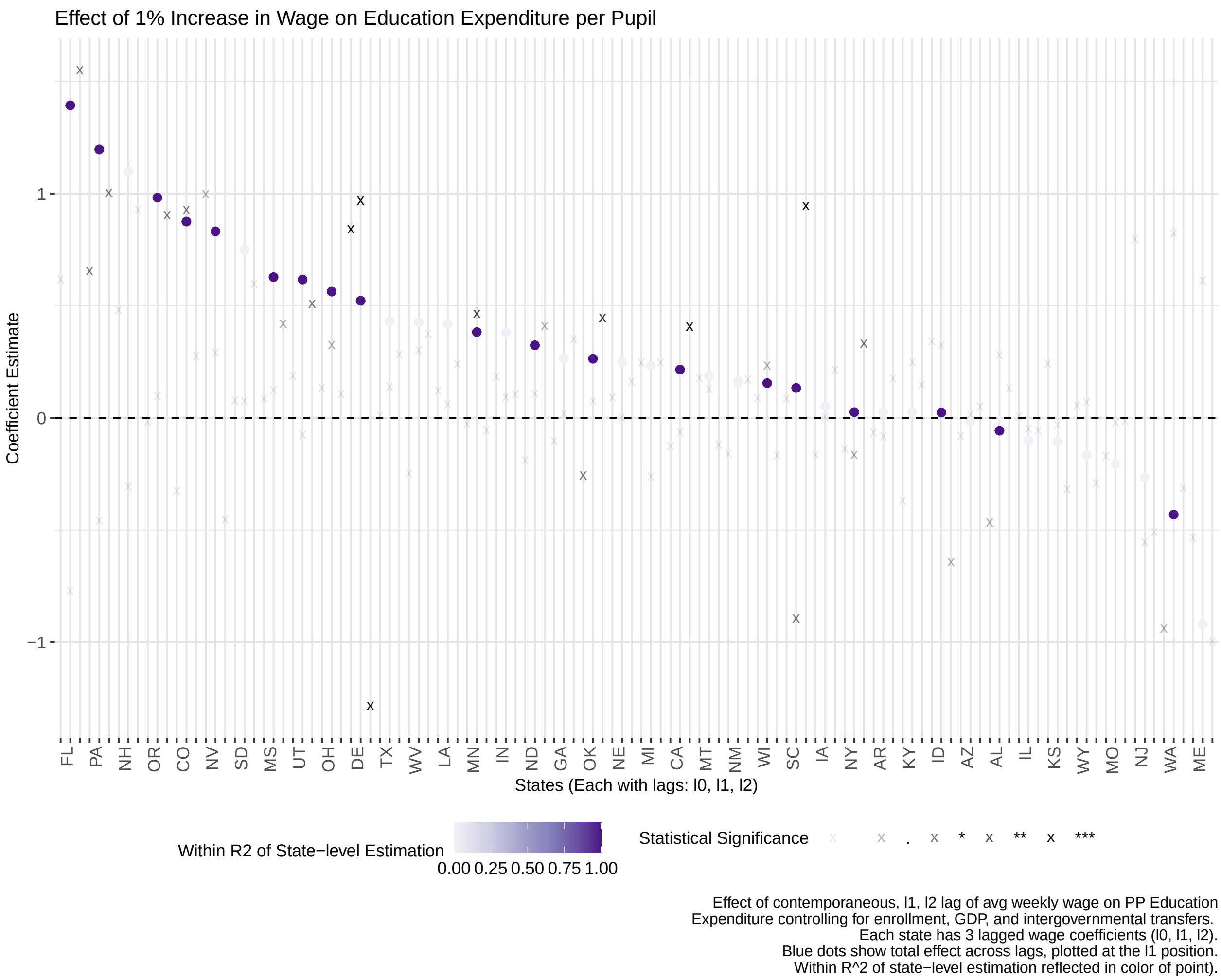


Figure 13: State-by-State Wage Effect - Descriptive

Using our instrumental variable approach with a value-added based shift-share instrument, we corroborate the directionality and magnitude of the effect for 5 states: Ohio, Missouri, Arizona, Washington, Delaware, and New Hampshire. Delaware's sample size is so small that it's likely uninformative. However, the rest of the states provide interesting points of analysis.

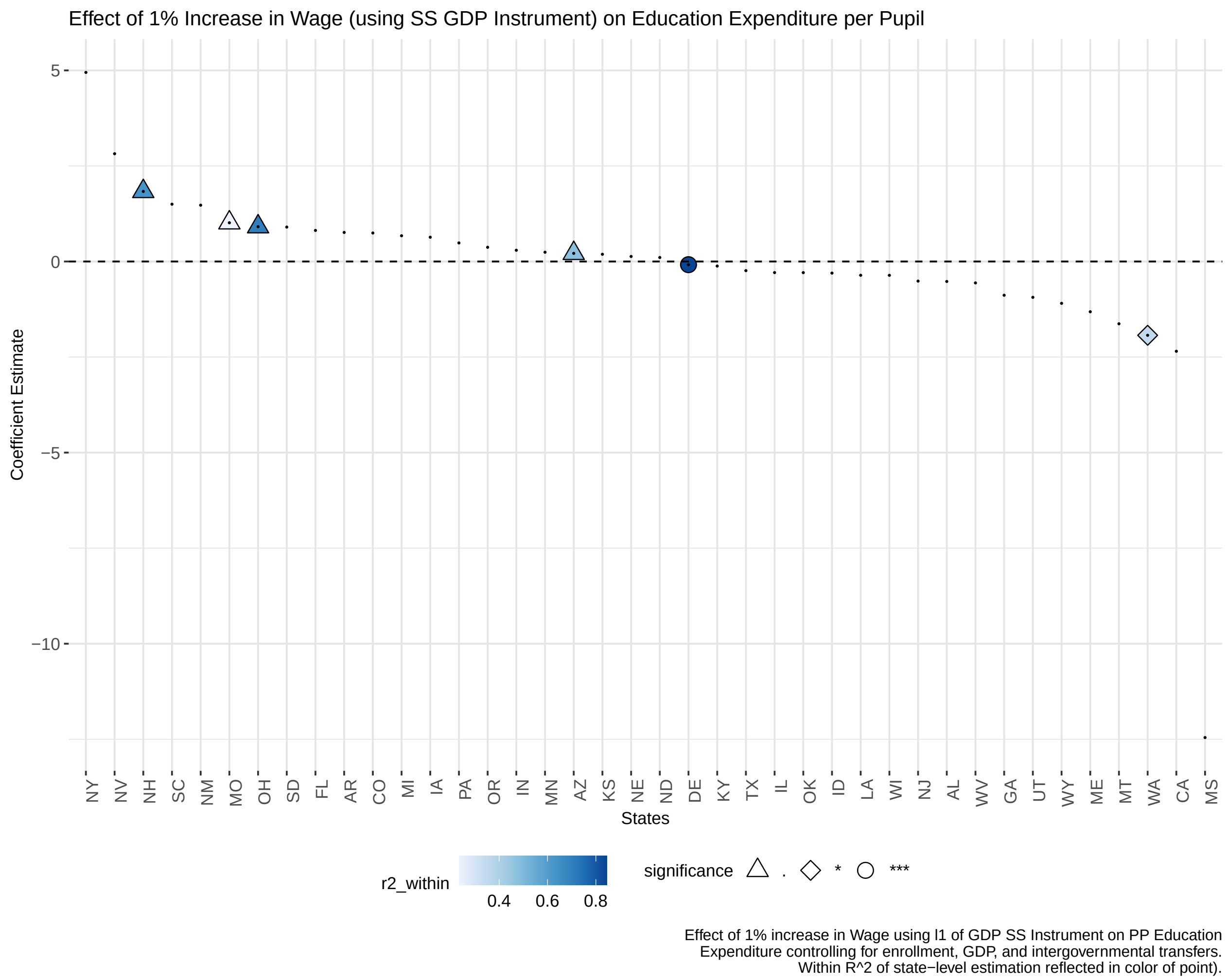
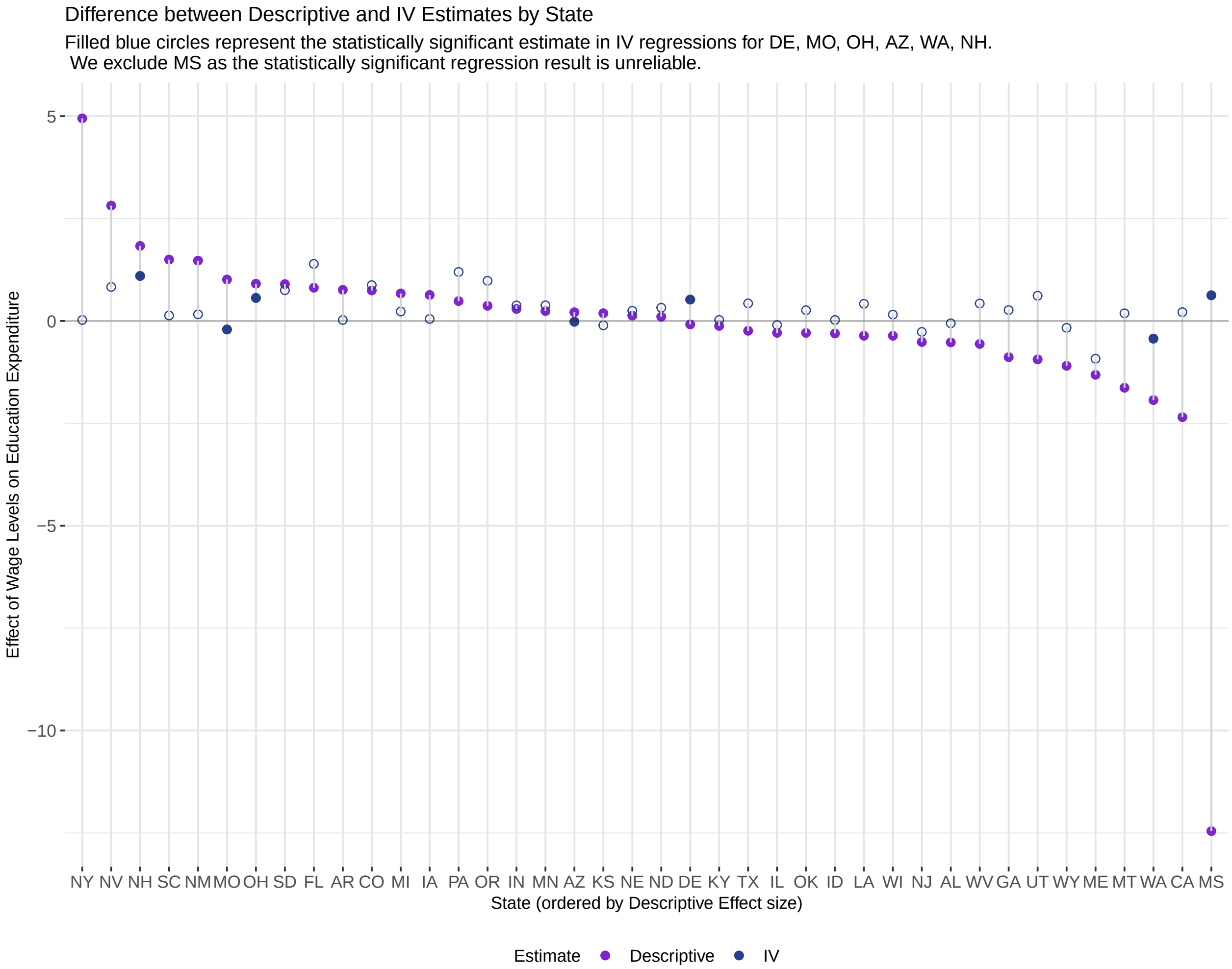


Figure 14: State-by-State Wage Effect Using SS GDP Shock

Placing the descriptive and causal estimates side-by-side (linear combination of l0, l1, l2 lagged wage coefficient values) and the second stage regression estimate (statistically significant second-stage estimates are represented by solid blue filled circles) on instrumented local wages we can see an emerging pattern. First, positive wage-education relationships (Ohio, Arizona, New Hampshire) are corroborated more often than a negative one (WA). Among the well-identified realtionships, only Missouri's relationship is revealed to be the opposite of what is suggested by the descriptive regression results. Note the very unusualy behaviour of Mississippi. I tried excluding Mississippi from the earlier regression estimations to ensure that it was not driving the negative relationship between wages and education expenditure. However, the strength and direction of the effect in the overall and partitioned sample regression results were consistenw even without Missouri.

Ohio...sign matched. Arizona...sign matched. Washington...sign matched. New Hampshire...sign matched. Missouri...sign **not** matched.



4.3.4 Industry by Industry

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 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.

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 16 demonstrates the overall treatment effect of local wage changes instrumented via an industry-specific GDP shock.

Note: I think that the below implementation/interpretation is wrong as we cannot formally interpret anything from the instrument itself. Two options: (1) This table should be filtered by F-test or instrumental variable relevance to find what is most useful. What we would ideally see is a consistent wage response regardless of quality instrument choice. (2) Instrument INDUSTRY-SPECIFIC wages not fully local wage levels using these industry-specific shocks. In the second case, this could link well to the intro in which only those regressions in which there is a strong first stage (ie. GDP changes do predict local wages well) would qualify for analysis. If this reveals any significant effect, we would have a potential answer to the broader question as currently outlined in the intro...?

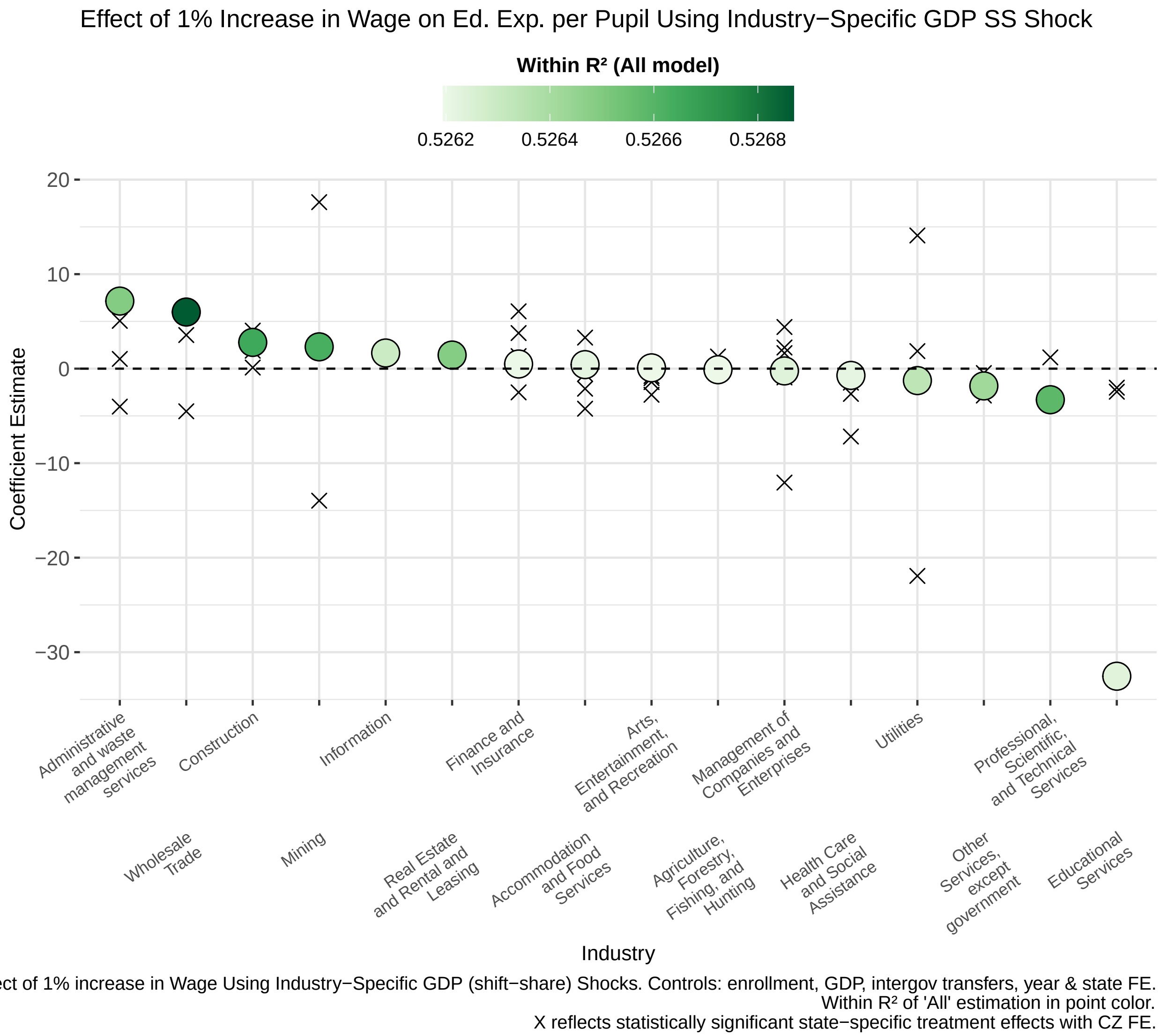


Figure 15: Wage Effect via Industry VA SS Shock

4.4 Additional Analysis Inventory

Provide brief inventory of additional analyses and robustness checks in supplementary materials here.

4.4.1 Quantile Regression

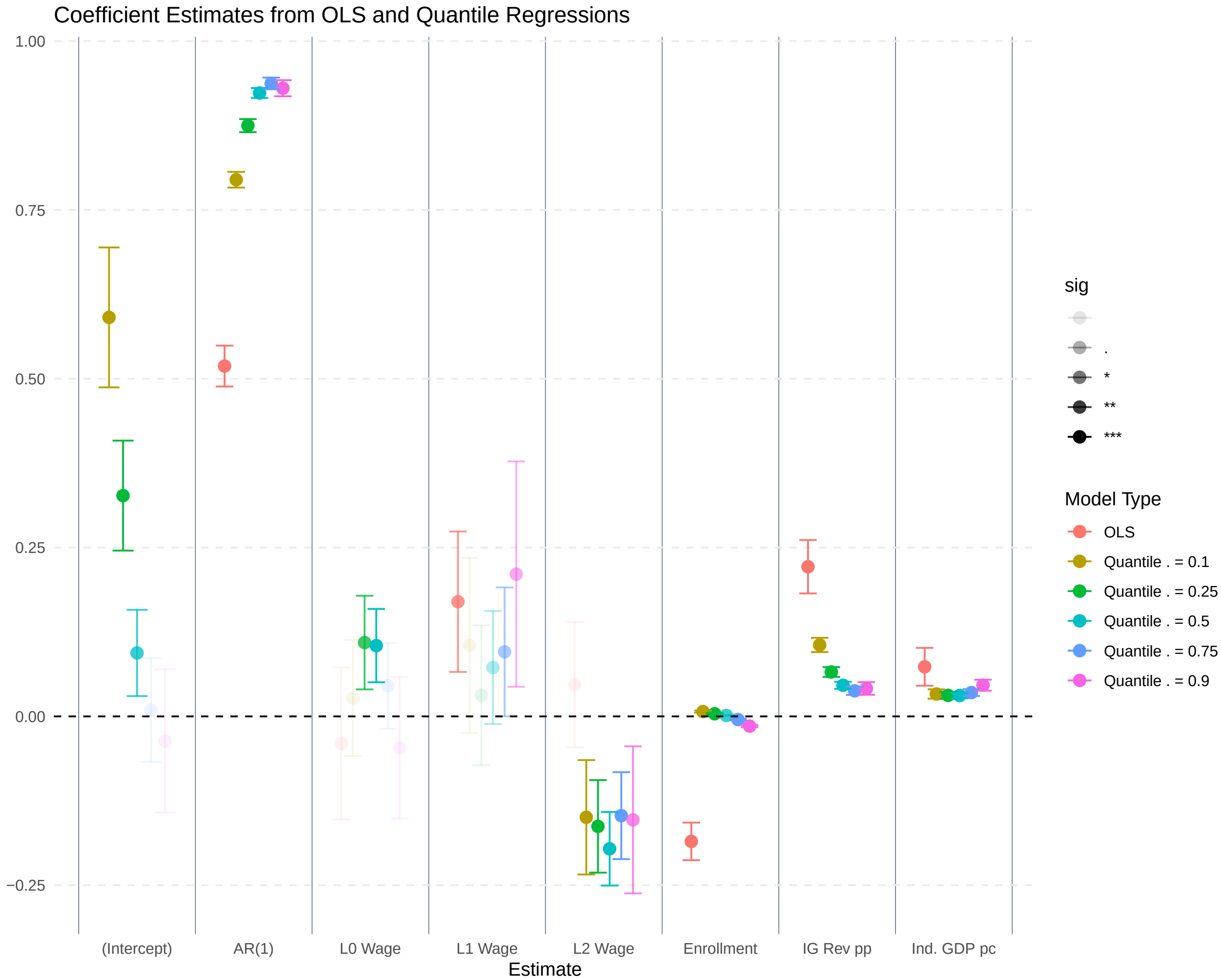


Figure 16: Quantile Regression

5 Discussion

...

6 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.

7 Data and Code Availability

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

8 Use of AI

- Used ChatGPT to help improve readability of plots (formatting, margins, labeling, font size).
- 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).

9 Acknowledgements

Appendices

A Modelling Challenges

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

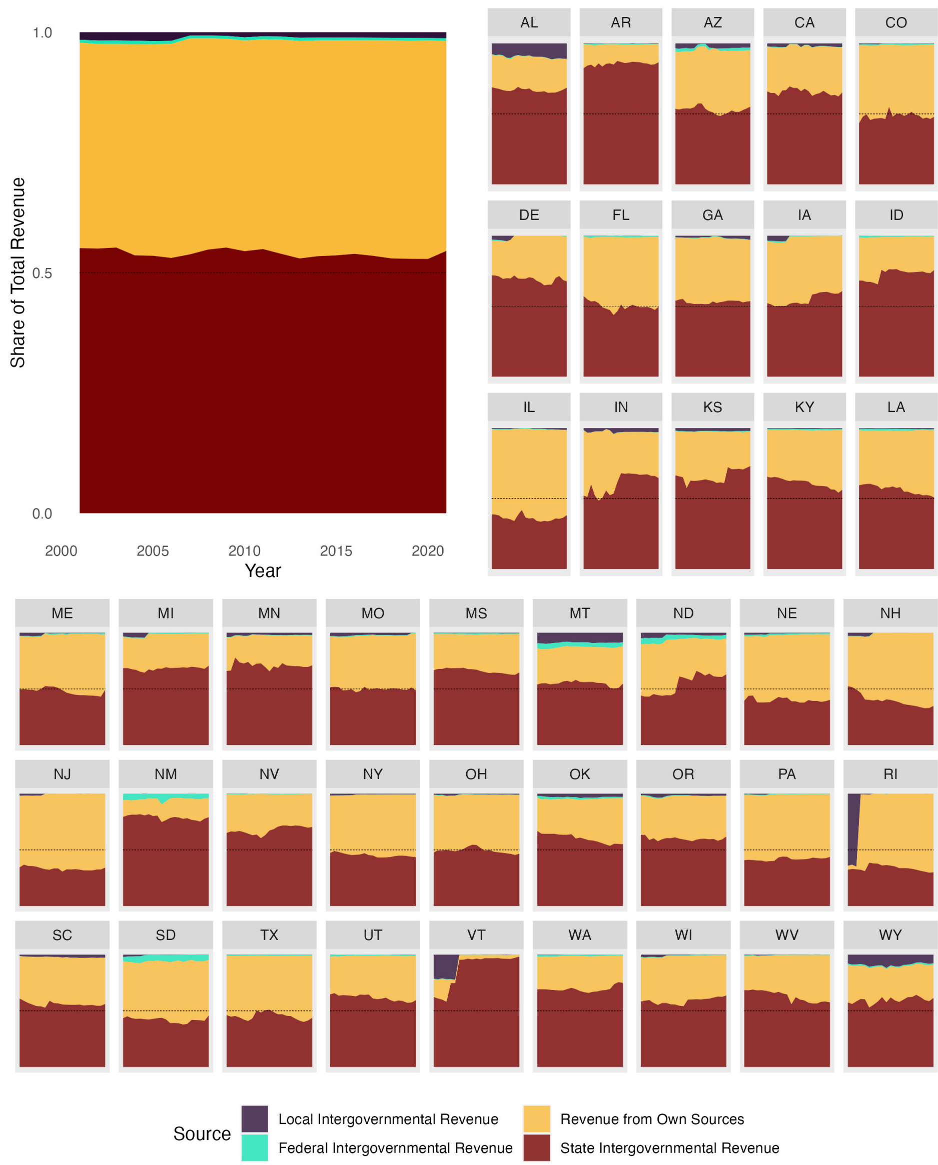
A.1 Structure of Financing for Local Public Education

In order to appropriately make use of the outlined data as well as robustly define the econometric methods to be 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 17 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 17: 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 8. Currently, there are 553 agencies nationwide in 45 states. According to the Association of Educational Service Agencies (AESA), ESAs reach over 80% of the public school districts and well over 80% of public and private school students. Annual budgets for ESAs total approximately $15 billion ?. Because ESA revenue and expenditure is inconsistently reported across years in our dataset, as well as attributed to individual counties despite often serving multiple, there is a significant risk that ESA expenditure is misattributed to counties in our dataset. Therefore, I exclude ESA revenue and expenditure totals from the measures of county-level expenditure and revenue at all levels of aggregation, and retain these values as possible control variables.

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

A.4 Availability of varying local-level outcomes

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

A.5 Structural and policy heterogeneity

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

A.6 Cross-Sectional Dependence

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

Table 8: Educational Service Agencies by State

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

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

B 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 (ie. the largest form of IG revenue is state funding and Own Source revenue is largely sourced from Property Taxes).

Dependent Variables:			(log) Elem.Ed.Exp.pp				log_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

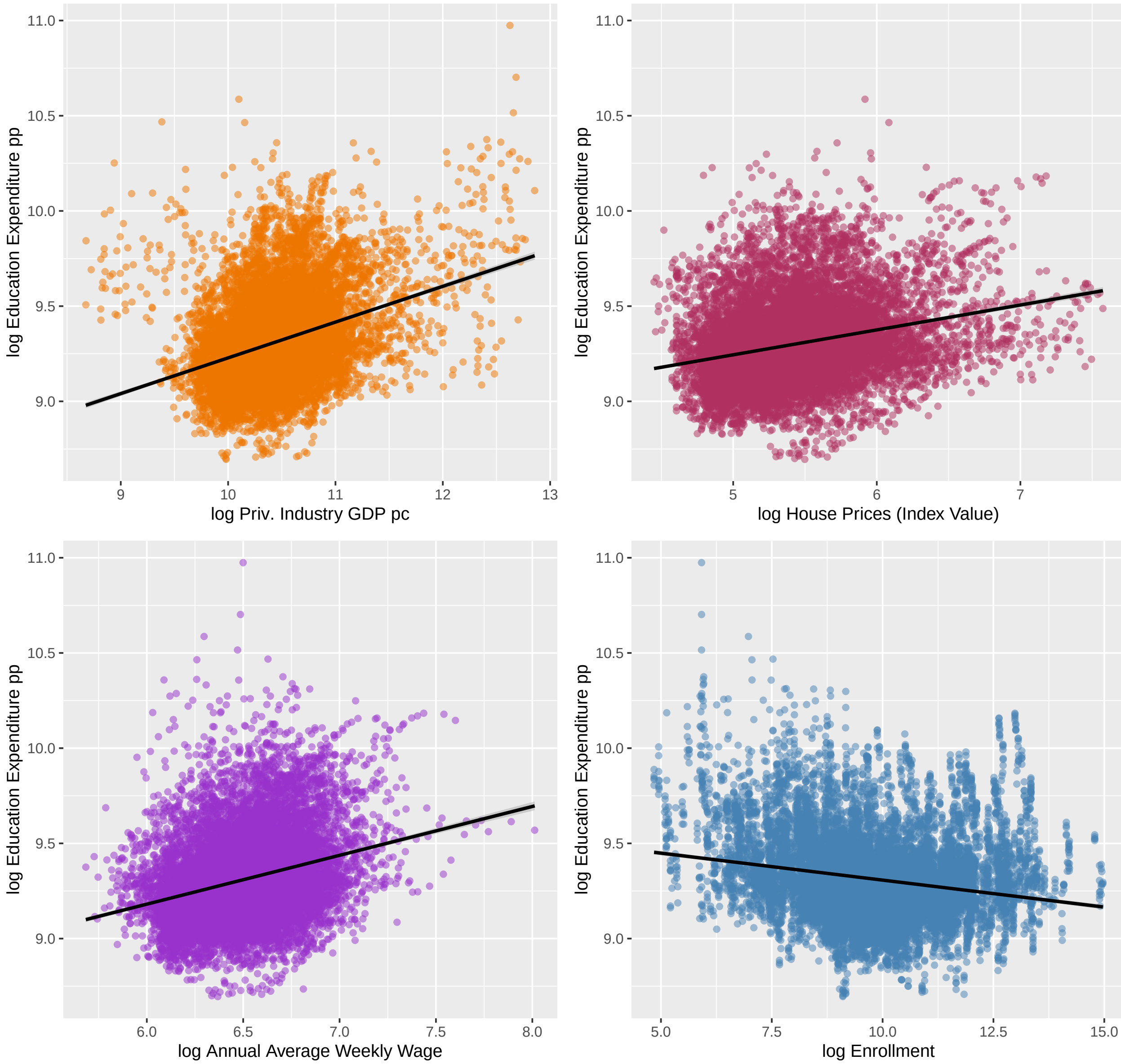
Clustered (unit) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

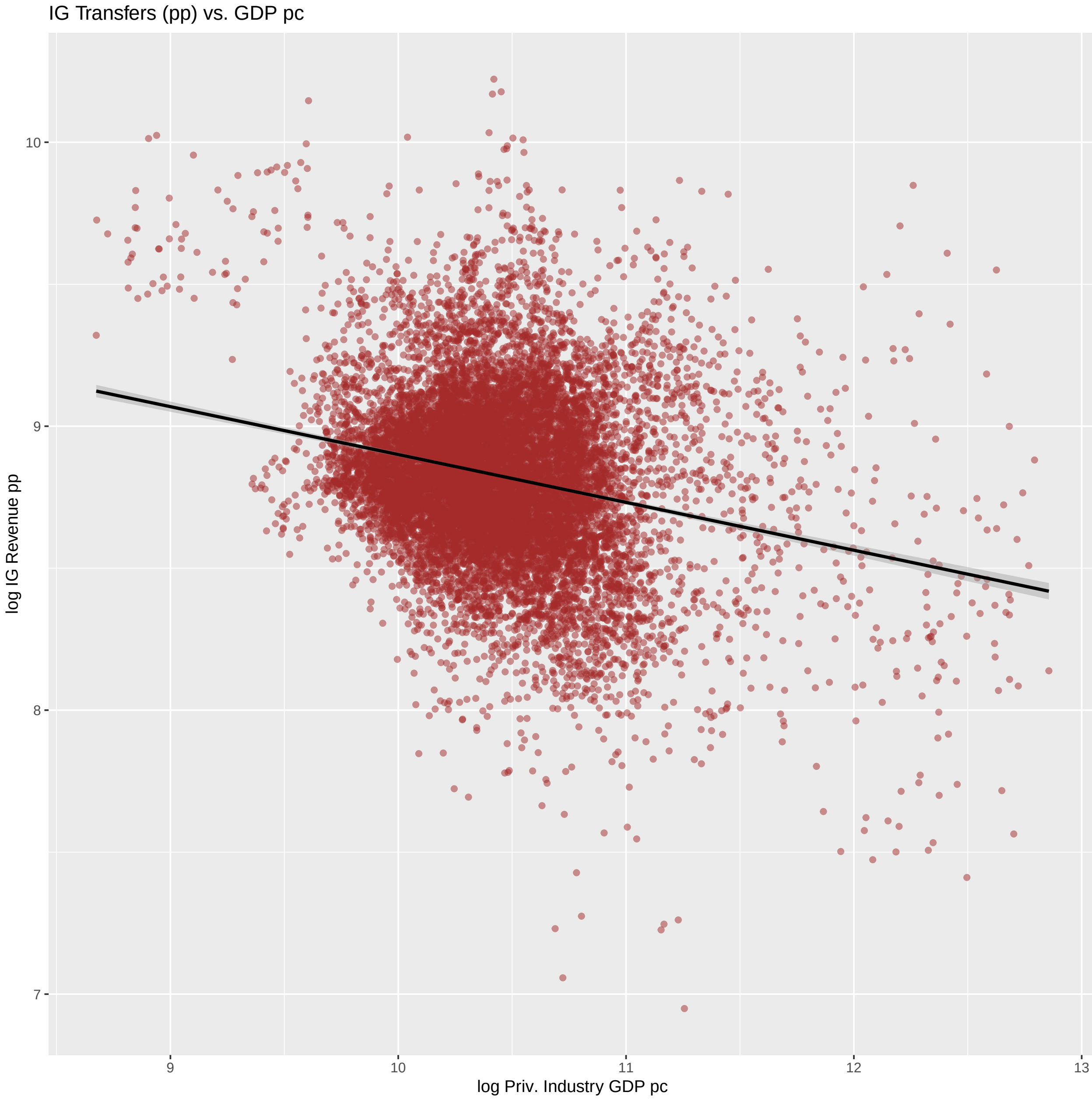
C Key Relationships between Economic Variables

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

Key Relationships Between Economic Varables and Ed.Exp.

Commuting Zone Level. 2001−2020.





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 Funding Share Interaction

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.

1. heatmap of effect vs. share when varying the share in a linear combinantion of interactive and stand-alone beta. 2. Make sure you can reconcile table 4 and table 2 together by showing that the linear combination of the coefficients using the mean share is the same as in table 2.

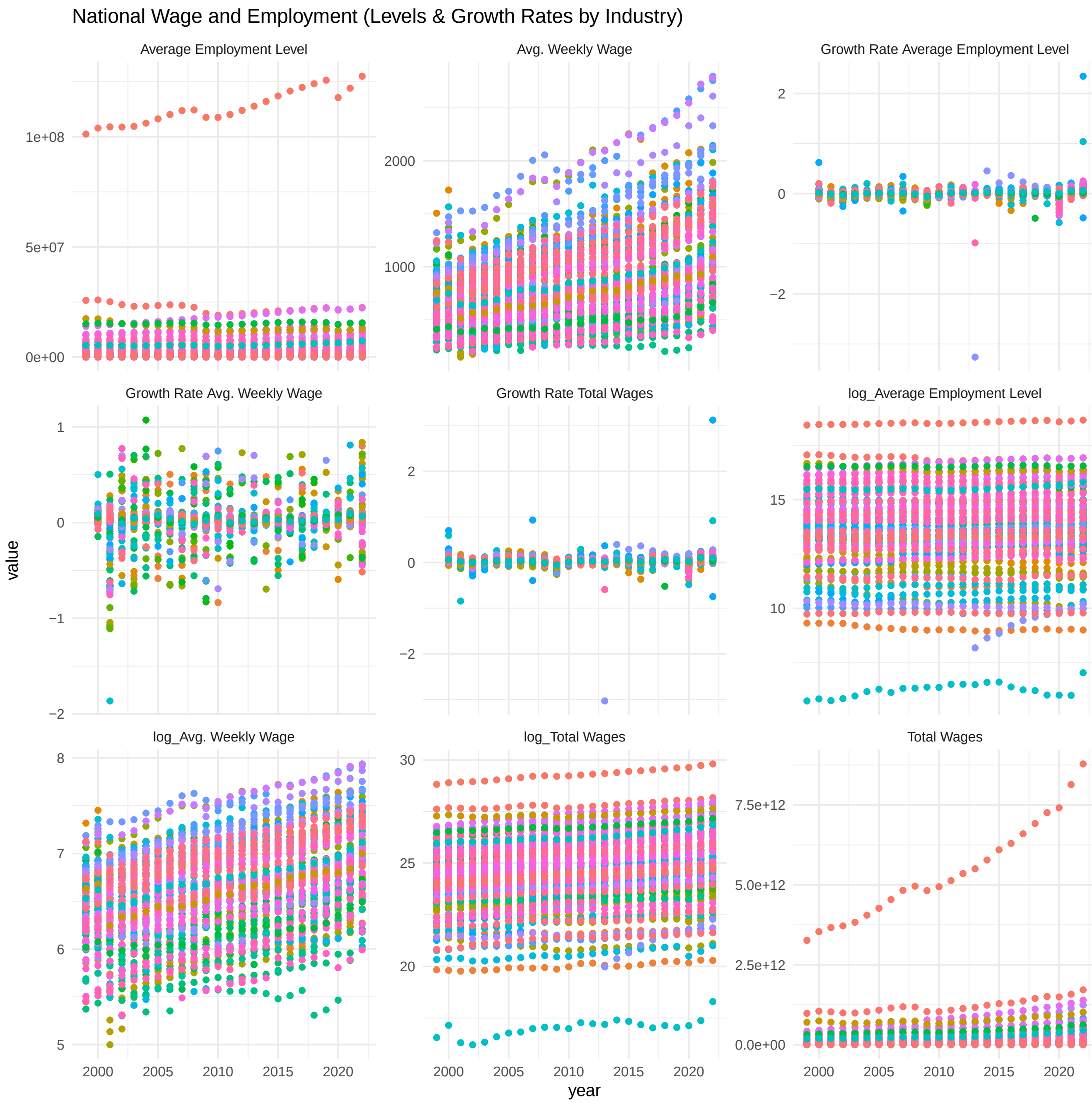
Table 9: Descriptive Results with Funding Source Interaction Effects

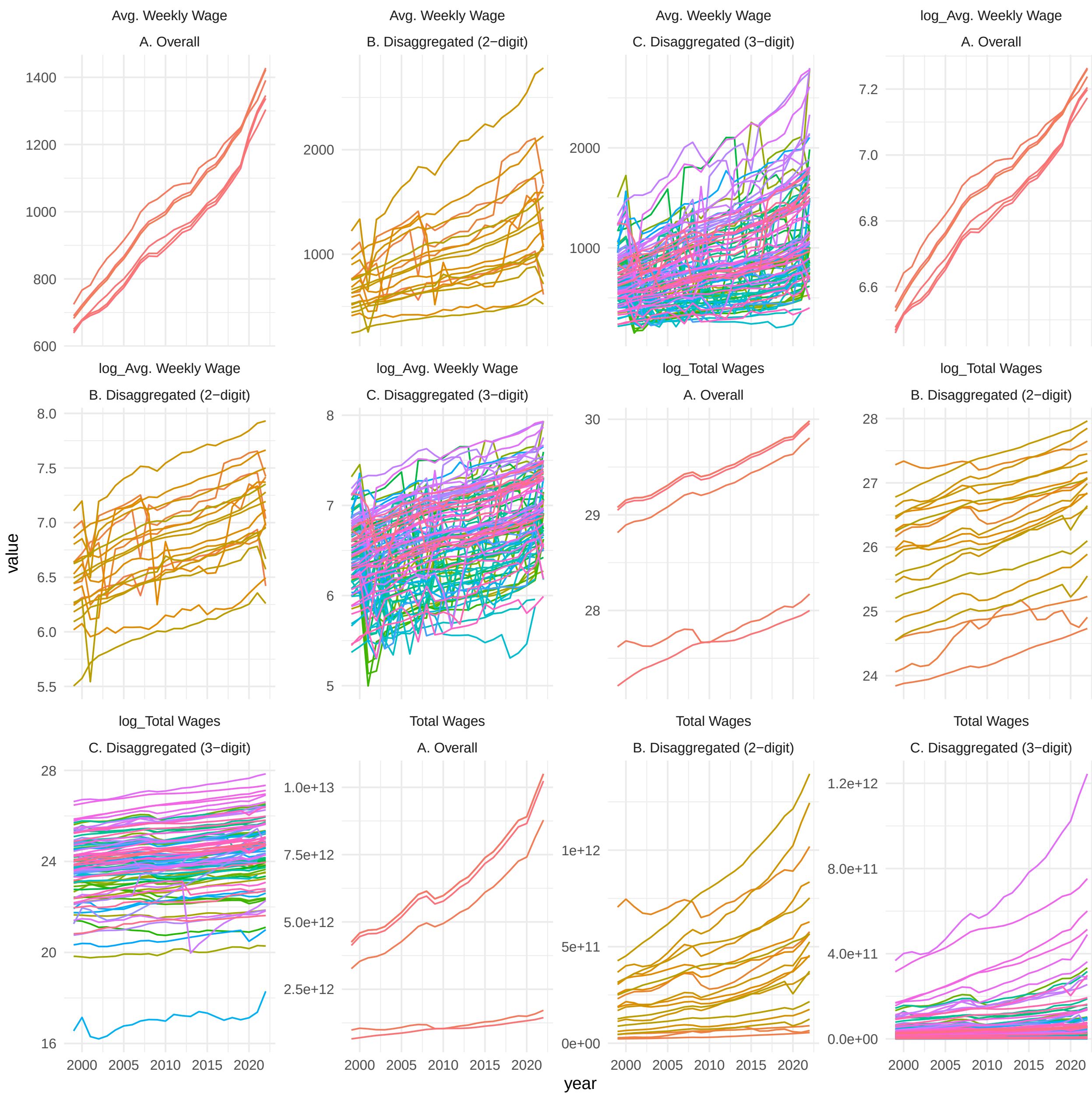
Dependent Variable:	(log) Elem.Ed.Exp.pp				
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
(log) Real GDP Priv. Industry pc			-0.2313***		
			(0.0743)		
(log,l1) Real GDP Priv. Industry pc			0.1026*		
			(0.0590)		
(log,l2) Real GDP Priv. Industry pc			0.3435***		
			(0.0794)		
Funding Share_state × (log) Real GDP Priv. Industry pc			0.4094***		
			(0.1201)		
Funding Share_state × (log,l1) Real GDP Priv. Industry pc			-0.1083		
			(0.0948)		
Funding Share_state × (log,l2) Real GDP Priv. Industry pc			-0.4455***		
			(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	0.7015	-0.4431	0.8906	0.7015	-0.4431
	(0.4808)	(0.3669)	(0.5640)	(0.4808)	(0.3669)
(log) Fed IG Rev. pp	-0.0014	-0.0016	-0.0030	-0.0014	-0.0016
	(0.0020)	(0.0020)	(0.0024)	(0.0020)	(0.0020)
(log) Enrollment	-0.3498***	-0.3723***	-0.3461***	-0.3498***	-0.3723***
	(0.0257)	(0.0264)	(0.0263)	(0.0257)	(0.0264)
Fixed-effects					
unit	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	13,356	12,588	12,084	13,356	12,588
R^2	0.85889	0.86831	0.86083	0.85889	0.86831
Within R^2	0.29018	0.31190	0.28372	0.29018	0.31190

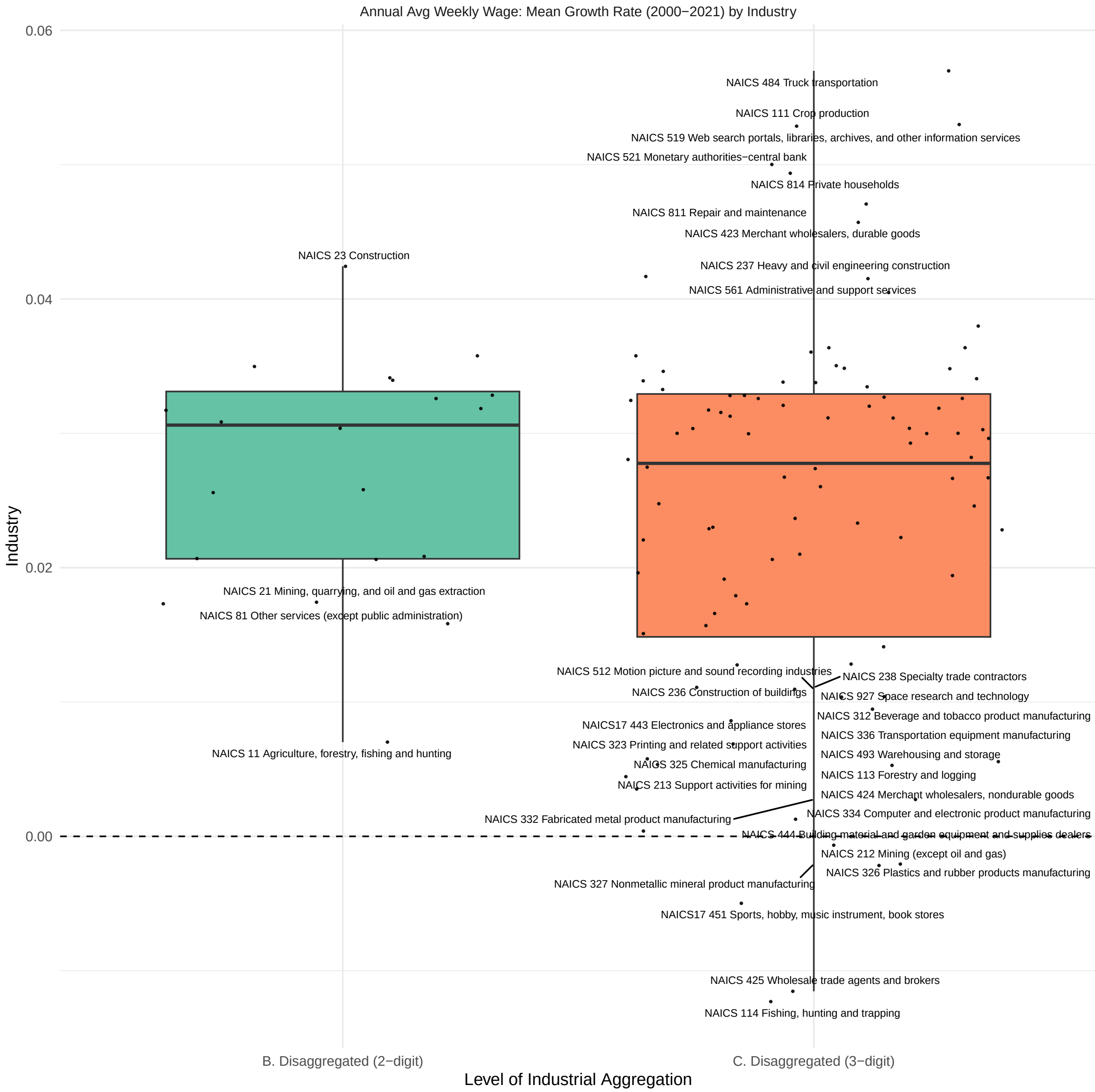
Clustered (unit) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

C.3 SS Construction

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




Avg. Weekly Wage
A. Overall
Avg. Weekly Wage
B. Disaggregated (2−digit)
Avg. Weekly Wage
C. Disaggregated (3−digit)
log_Avg. Weekly Wage
A. Overall
log_Avg. Weekly Wage
B. Disaggregated (2−digit)
log_Avg. Weekly Wage
C. Disaggregated (3−digit)
log_Total Wages
A. Overall
log_Total Wages
B. Disaggregated (2−digit)
log_Total Wages
C. Disaggregated (3−digit)
Total Wages
A. Overall
Total Wages
B. Disaggregated (2−digit)
Total Wages
C. Disaggregated (3−digit)
value
year



C.4 Wage-based Shift Share Instrument Results

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

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 extcolor{violet}{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 0.02-0.06% 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.

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.

C.4.1 Baseline Models for Growth Rate Sample Partitioning

C.4.2 Wage-based SS Instrument for Growth Rate Sample Partitioning

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.

Dependent Variable:	(log) Elem.Ed.Exp.pp				
	All	Hyper-Declining (GDP)	Declining (GDP)	Growing (GDP)	Hyper-Growing (GDP)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
(log) Annual Avg. Wkly. Wage	-1.974**	-1.095**	-1.333**	0.1730	0.1270
	(0.8249)	(0.4834)	(0.5584)	(0.4163)	(0.3819)
(l1, log) Elem.Ed.Exp.pp	0.6561***	0.6346***	0.6258***	0.4953***	0.4975***
	(0.0619)	(0.0435)	(0.0436)	(0.0301)	(0.0321)
(log) IG Revenue pp	0.2915***	0.2581***	0.2934***	0.2113***	0.1970***
	(0.0355)	(0.0420)	(0.0370)	(0.0328)	(0.0419)
(log) Real GDP Priv. Industry pc	0.3926***	0.2405***	0.2898***	0.0736	0.0895**
	(0.1315)	(0.0893)	(0.1027)	(0.0554)	(0.0440)
(log) Enrollment	-0.0092	-0.1380***	-0.0994**	-0.1925***	-0.2177***
	(0.0774)	(0.0392)	(0.0399)	(0.0474)	(0.0448)
Fixed-effects					
unit	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	12,084	3,021	5,016	7,068	3,021
F-test (IV only)	50.004	29.968	38.265	0.31864	0.19999
F-test (IV only), p-value	1.62×10^{-12}	4.76×10^{-8}	6.67×10^{-10}	0.57245	0.65476
Wu-Hausman	54.645	41.588	49.254	0.04377	9.88×10^{-6}
Wu-Hausman, p-value	1.54×10^{-13}	1.32×10^{-10}	2.57×10^{-12}	0.83429	0.99749
Wald (IV only)	5.7282	5.1323	5.6978	0.17264	0.11061
Wald (IV only), p-value	0.01671	0.02355	0.01702	0.67779	0.73948

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

Table 10: IV Estimation Using Wage-based Shift-share instrument (l0, l1, l2) in Levels varying state and CZ fixed effects and lags.

Dependent Variables:	(log) Annual Avg. Wkly. Wage	(log) Elem.Ed.Exp.pp	(log) Annual Avg. Wkly. Wage	(log) Elem.Ed.Exp.pp	(GR) Annual Avg. Wkly. Wage	(GR) Elem.Ed.Exp.pp
IV stages	First	Second	First	Second	First	Second
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Wage SS (lvl)	0.0352					
	(0.0522)					
Wage SS (lvl,l1)	-0.0671*					
	(0.0393)					
Wage SS (lvl,l2)	-0.0232					
	(0.0483)					
(l1, log) Elem.Ed.Exp.pp	0.0670***	0.6978***	0.0668***	0.6023***		
	(0.0086)	(0.1595)	(0.0086)	(0.0921)		
(log) IG Revenue pp	0.0310***	0.3106***	0.0305***	0.2669***		
	(0.0097)	(0.0762)	(0.0097)	(0.0464)		
(log) Real GDP Priv. Industry pc	0.1473***	0.4850	0.1482***	0.2733		
	(0.0154)	(0.3478)	(0.0153)	(0.1972)		
(log) Enrollment	0.0849***	0.0422	0.0822***	-0.0756		
	(0.0149)	(0.1981)	(0.0149)	(0.1120)		
(log) Annual Avg. Wkly. Wage		-2.598		-1.169		
		(2.293)		(1.329)		
Wage SS (GR)			0.0540		0.0484	
			(0.0498)		(0.0312)	
Wage SS (GR,l1)			-0.0012		-0.1204***	
			(0.0672)		(0.0464)	
Wage SS (GR,l2)			-0.0330		0.0111	
			(0.0508)		(0.0345)	
(GR, l1) Elem.Ed.Exp.pp					0.0064*	-0.0554***
					(0.0035)	(0.0159)
(GR) IG Revenue pp					0.0029	0.3071***
					(0.0028)	(0.0324)
(GR) Real GDP Priv. Industry pc					0.0582***	0.0341
					(0.0077)	(0.0400)
(GR) Enrollment					0.0116*	-0.5787***
					(0.0065)	(0.0429)
(GR) Annual Avg. Wkly. Wage						-0.6464
						(0.6585)
Fixed-effects						
unit	Yes	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	12,084	12,084	12,084	12,084	12,084	12,084
R2 (1st stage)	0.97749		0.97748		0.29798	
Adj. R2 (1st stage)	0.97619		0.97618		0.25742	
F-test (IV only)	3.4706	16.928	1.0551	1.0416	15.839	1.5779
F-test (IV only), p-value	0.01540	3.91×10^{-5}	0.36686	0.30746	2.82×10^{-10}	0.20909
Wu-Hausman		17.859		1.2358		1.5871
Wu-Hausman, p-value		2.4×10^{-5}		0.26630		0.20776
Wald (IV only)	1.3608	1.2840	1.4058	0.77442	5.1469	0.96375
Wald (IV only), p-value	0.25276	0.25719	0.23898	0.37887	0.00148	0.32626

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

Table 11: 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***	0.2398***	0.3356***	0.3469***	0.2110***	0.3340***	0.2902***	0.1987***	0.3088***	0.2444***	0.1662***
	(0.0555)	(0.0514)	(0.0340)	(0.0670)	(0.0625)	(0.0394)	(0.0361)	(0.0424)	(0.0286)	(0.0520)	(0.0609)	(0.0426)
(log) Enrollment	-0.2997***	-0.2890***	-0.2021***	-0.3433***	-0.3225***	-0.2202***	-0.2930***	-0.3075***	-0.2011***	-0.3250***	-0.3479***	-0.2280***
	(0.0374)	(0.0345)	(0.0214)	(0.0512)	(0.0469)	(0.0299)	(0.0317)	(0.0349)	(0.0226)	(0.0446)	(0.0543)	(0.0401)
(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			0.0862***			0.0590*			0.1063***			0.1252**
			(0.0231)			(0.0299)			(0.0283)			(0.0495)
(log, l1) House Price Index			0.0657**			0.1324***			0.0171			0.0312
			(0.0319)			(0.0408)			(0.0417)			(0.0679)
(log, l2) House Price Index			-0.0525			-0.0702			0.0229			0.0379
			(0.0375)			(0.0474)			(0.0340)			(0.0551)
(log, l3) House Price Index			0.0202			0.0064			-0.0223			-0.0625*
			(0.0263)			(0.0340)			(0.0218)			(0.0370)
(l1, log) Elem.Ed.Exp.pp			0.5445***			0.5558***			0.5144***			0.5279***
			(0.0208)			(0.0227)			(0.0264)			(0.0392)
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	5,238	3,021	3,339	3,138	7,068	7,812	6,815	3,021	3,339	2,708
R^2	0.85305	0.85739	0.90933	0.84306	0.84996	0.90675	0.86948	0.86249	0.90876	0.81211	0.79761	0.85273
Within R^2	0.29337	0.33568	0.57047	0.25448	0.31138	0.56253	0.29444	0.26447	0.46967	0.31424	0.25178	0.44548

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

Table 12: Baseline Regression Applied to Declining Wage vs. Growing Wage Regions

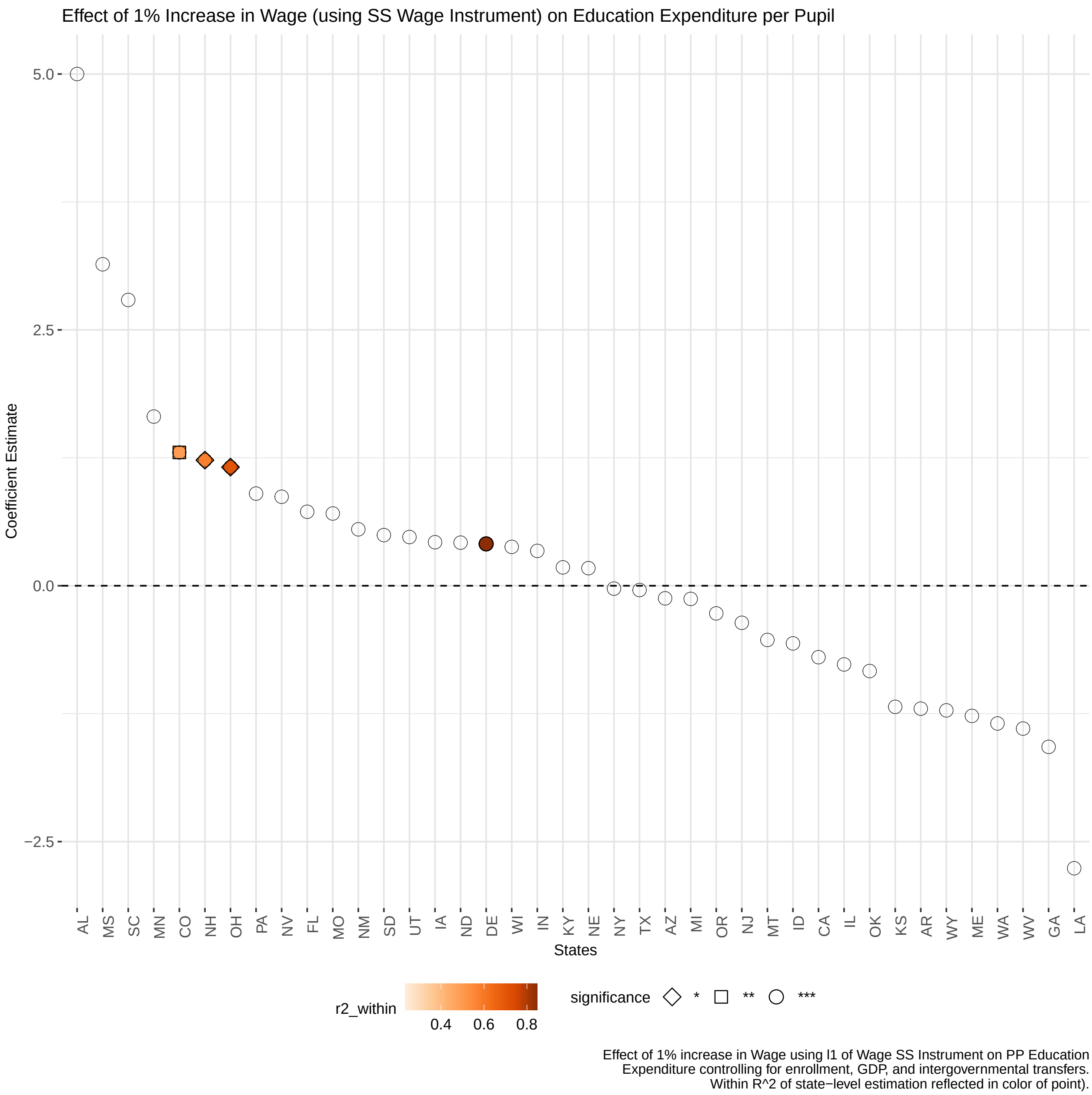
Dependent Variable:	(log) Elem.Ed.Exp.pp											
	Declining			Hyper-Declining			Growing			Hyper-Growing		
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
(log) Real GDP Priv. Industry pc	0.0304			0.0217			0.0099			-0.0134		
	(0.0535)			(0.0408)			(0.0199)			(0.0286)		
(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***	0.1908***	0.4039***	0.3996***	0.2521***	0.3543***	0.3228***	0.2154***	0.2938***	0.2230***	0.1369***
	(0.0670)	(0.0566)	(0.0284)	(0.0549)	(0.0482)	(0.0320)	(0.0324)	(0.0365)	(0.0253)	(0.0516)	(0.0663)	(0.0356)
(log) Enrollment	-0.3328***	-0.3399***	-0.2121***	-0.3460***	-0.3467***	-0.2426***	-0.2881***	-0.2956***	-0.2024***	-0.2625***	-0.2972***	-0.1527***
	(0.0615)	(0.0580)	(0.0349)	(0.0444)	(0.0417)	(0.0242)	(0.0262)	(0.0272)	(0.0171)	(0.0435)	(0.0576)	(0.0347)
(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			0.1072**			0.0718*			0.1077***			0.1204**
			(0.0432)			(0.0390)			(0.0239)			(0.0509)
(log, l1) House Price Index			0.0737			0.1179			0.0257			-0.0442
			(0.0754)			(0.0713)			(0.0354)			(0.0717)
(log, l2) House Price Index			-0.0857			-0.0931*			0.0131			0.0413
			(0.0702)			(0.0552)			(0.0299)			(0.0556)
(log, l3) House Price Index			0.0425			0.0347			-0.0059			-0.0051
			(0.0381)			(0.0263)			(0.0186)			(0.0357)
(l1, log) Elem.Ed.Exp.pp			0.5974***			0.5286***			0.5211***			0.5632***
			(0.0333)			(0.0268)			(0.0203)			(0.0410)
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	1,520	3,021	3,339	3,057	10,564	11,676	10,533	3,021	3,339	2,770
R^2	0.90202	0.90302	0.94226	0.87794	0.87767	0.92212	0.85732	0.85133	0.90135	0.86401	0.84668	0.90197
Within R^2	0.30517	0.34695	0.61530	0.33808	0.35539	0.59054	0.31081	0.29539	0.50863	0.31419	0.24337	0.44622

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

Dependent Variable:	(log) Elem.Ed.Exp.pp				
	All	Hyper-Declining (Wage)	Declining (Wage)	Growing (Wage)	Hyper-Growing (Wage)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
(log) Annual Avg. Wkly. Wage	-1.974**	-2.192*	0.1749	-1.197**	0.5982
	(0.8249)	(1.300)	(0.5914)	(0.5414)	(0.4487)
(l1, log) Elem.Ed.Exp.pp	0.6561***	0.6395***	0.5683***	0.5924***	0.5086***
	(0.0619)	(0.0791)	(0.0597)	(0.0414)	(0.0376)
(log) IG Revenue pp	0.2915***	0.3234***	0.2155***	0.2700***	0.1318***
	(0.0355)	(0.0476)	(0.0327)	(0.0295)	(0.0455)
(log) Real GDP Priv. Industry pc	0.3926***	0.4859**	0.0564	0.2712***	0.0192
	(0.1315)	(0.2240)	(0.1127)	(0.0837)	(0.0507)
(log) Enrollment	-0.0092	-0.0227	-0.1752***	-0.0754	-0.2309***
	(0.0774)	(0.1103)	(0.0615)	(0.0492)	(0.0572)
Fixed-effects					
unit	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	12,084	3,021	1,520	10,564	3,021
F-test (IV only)	50.004	12.043	0.13431	21.660	4.3224
F-test (IV only), p-value	1.62×10^{-12}	0.00053	0.71406	3.29×10^{-6}	0.03770
Wu-Hausman	54.645	13.959	0.01551	25.182	3.4490
Wu-Hausman, p-value	1.54×10^{-13}	0.00019	0.90091	5.31×10^{-7}	0.06340
Wald (IV only)	5.7282	2.8408	0.08749	4.8879	1.7771
Wald (IV only), p-value	0.01671	0.09200	0.76743	0.02707	0.18261

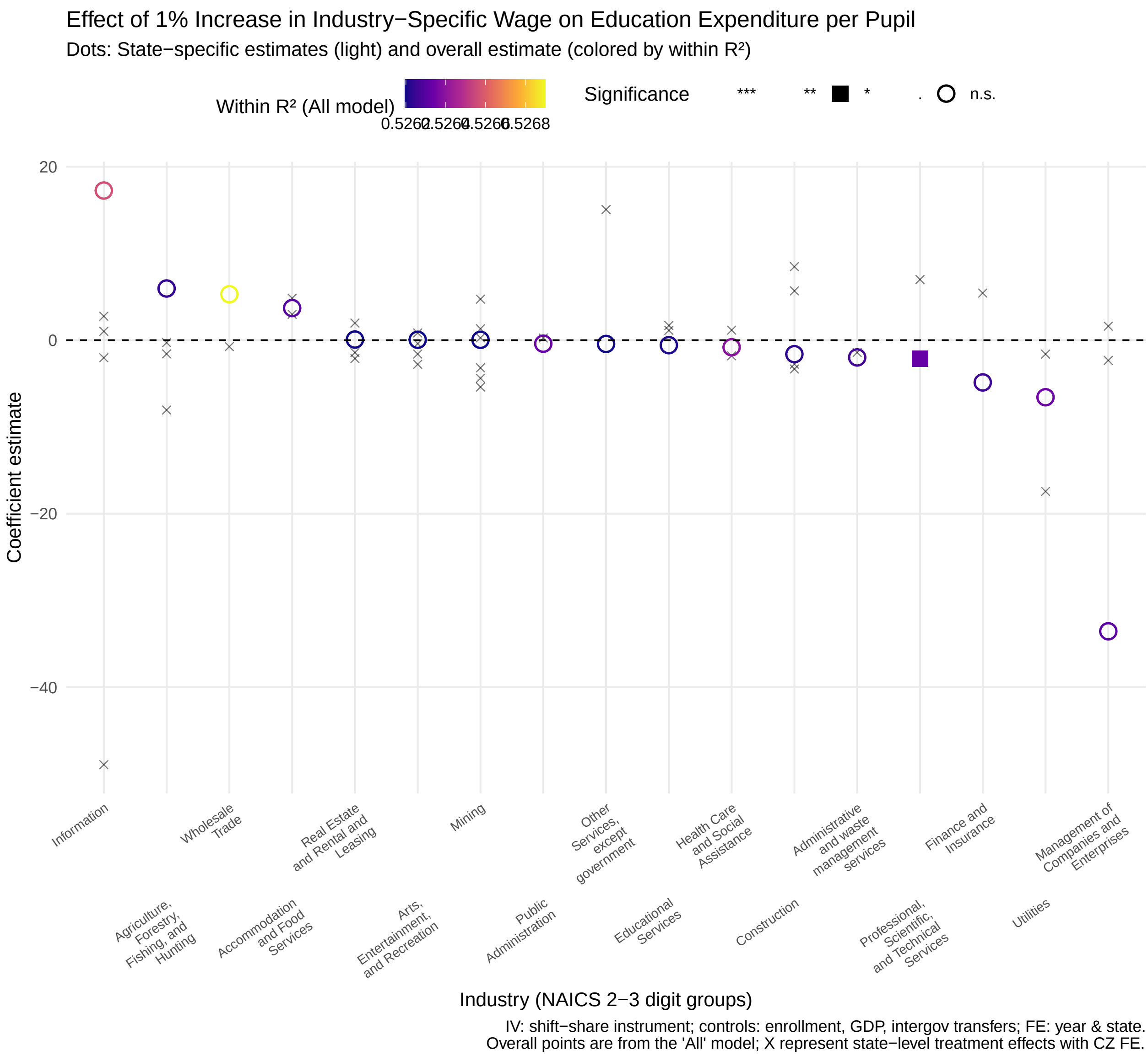
Clustered (unit) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

C.4.3 Wage-based SS State by State Estimation



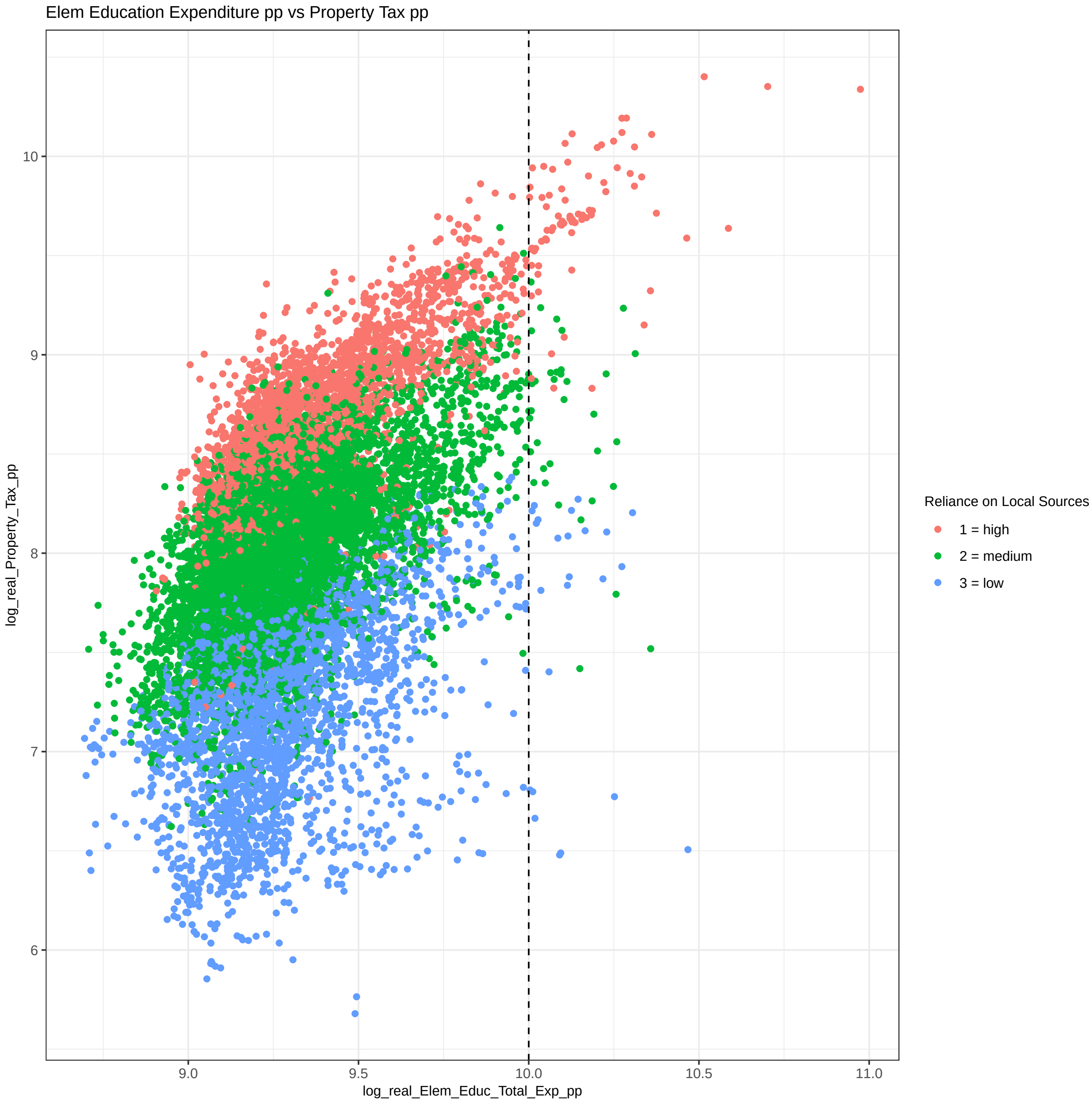
C.4.3.1 Wage-based SS in Industry-by-Industry Estimation

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



C.4.4 High-Income Outliers

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 VA-based instrument produces effects in line with earlier results. Overall, this robustness exercise confirms that the causal relationship between housing wealth and education spending is not confined to affluent areas but reflects a broader, generalizable pattern. These findings confirm that the main result is not driven solely by affluent jurisdictions, but reflects a more general relationship between local housing wealth and education spending.



Elem Education Expenditure pp vs Property Tax pp
log_real_Property_Tax_pp
10
9
8
7
6
9.0
9.5
10.0
10.5
11.0
log_real_Elem_Educ_Total_Exp_pp
Reliance on Local Sources
1 = high
2 = medium
3 = low

Dependent Variable:	(log) Elem.Ed.Exp.pp				
	Baseline 1	Baseline 2	Baseline 3	Wage-based SS	VA-based SS
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
(log) Real GDP Priv. Industry pc	0.0295**			-0.2421	0.3494***
	(0.0149)			(0.3792)	(0.1242)
(log,l1) Real GDP Priv. Industry pc	0.0529***				
	(0.0124)				
(log,l2) Real GDP Priv. Industry pc	0.1034***				
	(0.0154)				
(log) IG Revenue pp	0.3511***	0.3401***	0.2051***	0.1138**	0.2466***
	(0.0243)	(0.0242)	(0.0182)	(0.0506)	(0.0286)
(log) Enrollment	-0.2740***	-0.2794***	-0.1922***	-0.1195	-0.0084
	(0.0250)	(0.0226)	(0.0138)	(0.1515)	(0.0669)
(log) Annual Avg. Wkly. Wage		0.1900***		1.643	-1.874**
		(0.0520)		(2.172)	(0.8047)
(log, l1) Annual Avg. Wkly. Wage		0.1623***			
		(0.0472)			
(log, l2) Annual Avg. Wkly. Wage		0.2864***			
		(0.0533)			
(log) House Price Index			0.1087***		
			(0.0193)		
(log, l1) House Price Index			0.0394		
			(0.0293)		
(log, l2) House Price Index			-0.0175		
			(0.0269)		
(log, l3) House Price Index			0.0051		
			(0.0168)		
(l1, log) Elem.Ed.Exp.pp			0.5279***	0.5554*	0.6653***
			(0.0150)	(0.2970)	(0.0645)
Fixed-effects					
unit	Yes	Yes	Yes		Yes
year	Yes	Yes	Yes	Yes	Yes
state				Yes	
Fit statistics					
Observations	11,248	12,432	11,476	11,248	11,248
R2 (1st stage)	0.85852	0.85950	0.90345		
Adj. R2 (1st stage)	0.85035	0.85217	0.89810		
F-test (IV only)				51.687	40.106
F-test (IV only), p-value				6.93×10^{-13}	2.5×10^{-10}
Wu-Hausman				48.123	46.815
Wu-Hausman, p-value				4.22×10^{-12}	8.23×10^{-12}
Wald (IV only)				0.57200	5.4268
Wald (IV only), p-value				0.44948	0.01985

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

C.5 Panel VAR Specification

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

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

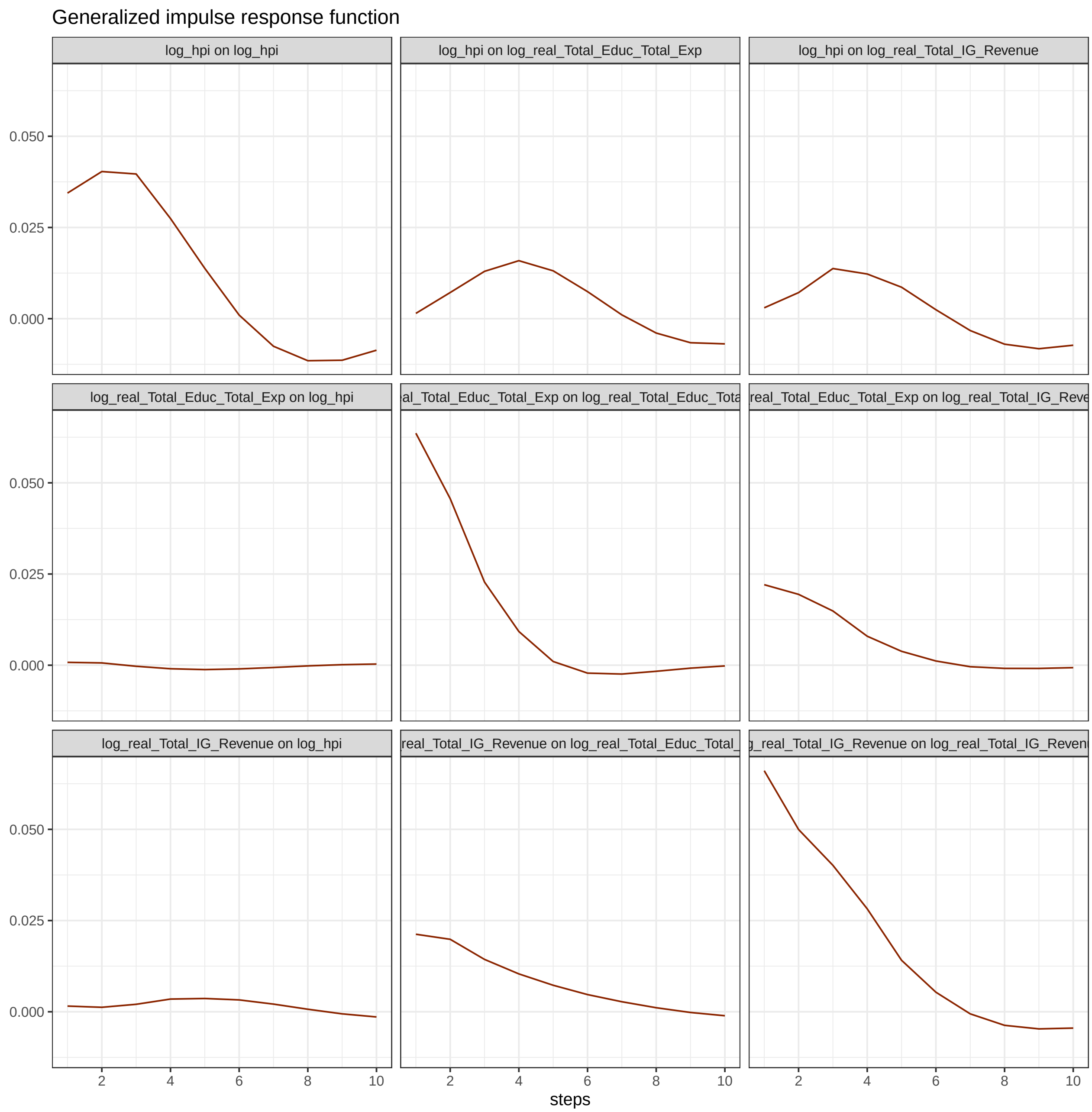
$$Y_{it} = \begin{bmatrix} \log(\text{real Total Educ. Exp.})_{it} \\ \log(\text{real Total IG Revenue})_{it} \\ \log(\text{HPI})_{it} \end{bmatrix}, \quad X_{it} = \begin{bmatrix} \log(\text{real GDP})_{it} \\ \log(\text{wage})_{it} \end{bmatrix}$$

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

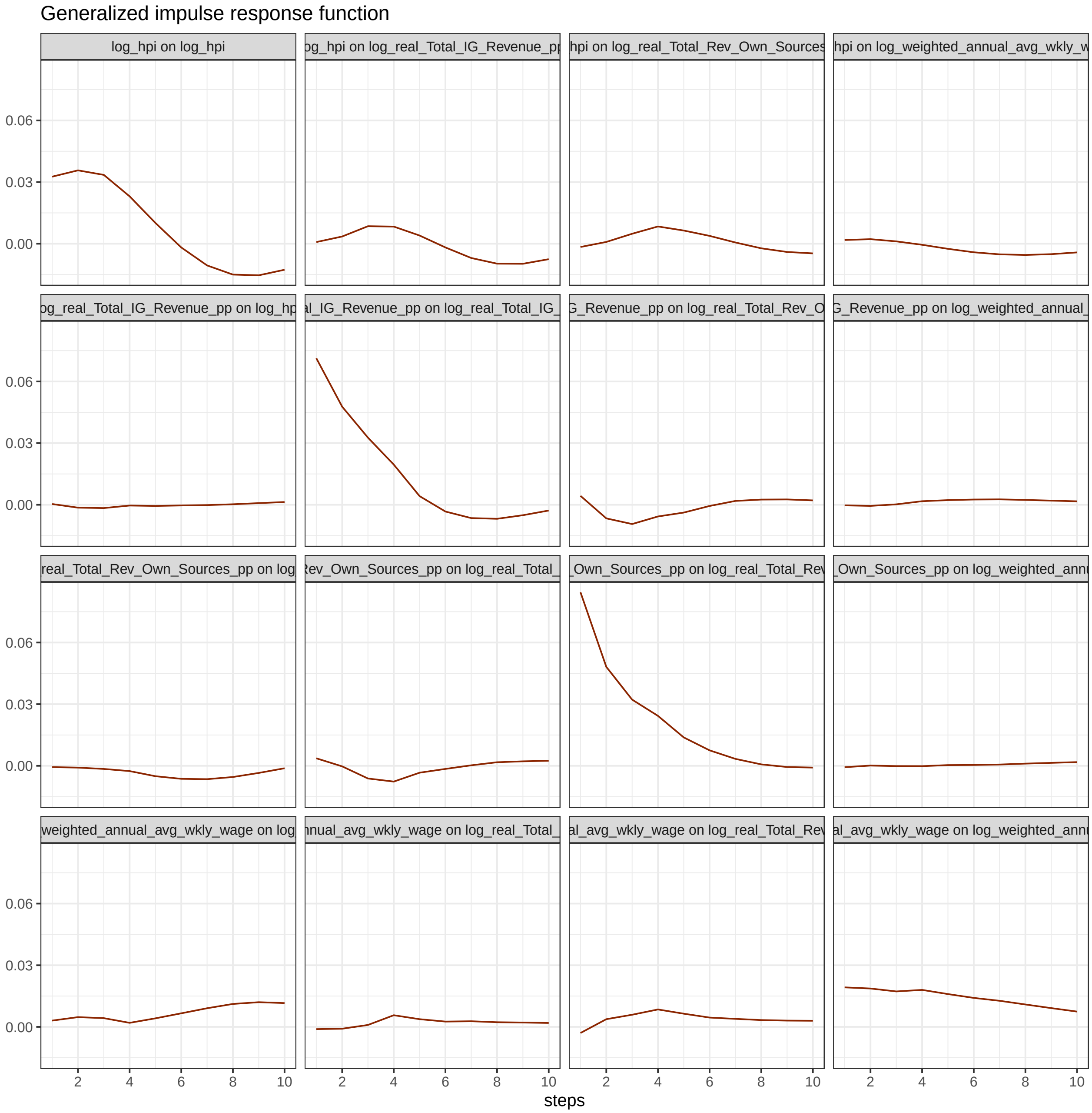
Where

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

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



Generalized impulse response function
log_hpi on log_hpi
log_hpi on log_real_Total_Educ_Total_Exp
log_hpi on log_real_Total_IG_Revenue
log_real_Total_Educ_Total_Exp on log_hpi
al_Total_Educ_Total_Exp on log_real_Total_Educ_Tota
real_Total_Educ_Total_Exp on log_real_Total_IG_Reve
log_real_Total_IG_Revenue on log_hpi
real_Total_IG_Revenue on log_real_Total_Educ_Total_
real_Total_IG_Revenue on log_real_Total_IG_Reven
0.050
0.025
0.000
2
4
6
8
10
steps



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