

Education Budget Shares and District Economic Activity in Beijing: Evidence from 2016-2023

Le Jin*

Independent Researcher, Beijing 100000, China

*Corresponding email: 1450484218@qq.com

Abstract

Education expenditure is often regarded as an instrument for long-term development, yet the empirical link between district-level fiscal education allocations and contemporaneous local economic activity remains ambiguous. While public education spending can boost future productivity via human-capital accumulation, its benefits are time-lagged and geographically spill over across administrative boundaries. The education expenditure share reflects budget composition rather than the absolute magnitude or effectiveness of educational investment. Using a balanced panel dataset of Beijing's 16 districts from 2016 to 2023, this study adopts two-way fixed-effects regressions. Across different specifications, no statistically significant contemporaneous association is found between the education budget share and nominal gross domestic product (GDP) per resident; coefficient signs shift conditional on fiscal control variables. This does not imply education spending is economically valueless. It suggests contemporaneous district GDP is an inadequate metric for evaluating education budget allocation. Fiscal assessment should consider spending scale, demographic demands and long-run outcomes, and it underscores the importance of small-cluster inference and clear fiscal definitions in sub-municipal panel research.

Keywords

Public education, Fiscal composition, Education expenditure, Education budget

Introduction

Public investment in education can contribute to long-run productivity by developing skills and supporting the diffusion of knowledge [1]. Yet an annual education budget share is not equivalent to the stock of human capital, the quality of schooling, or the economic return to education [2]. These distinctions matter when researchers use government expenditure data to investigate the relationship between education and economic development [3]. The economic rationale for education investment is well established. Even so, the empirical link between fiscal allocations and contemporaneous economic activity is conditional on several factors. These include when benefits materialize, how spending is structured, the geographic scope used for analysis, and the institutional setting governing budget formulation [4].

This paper asks a narrow empirical question: Do years in which a Beijing district allocates a larger proportion of its general public budget to education also exhibit higher nominal GDP per resident, after controlling for district

and year fixed effects? The analysis covers Beijing's 16 administrative districts between 2016 and 2023. The outcome is an indicator of district-level economic production per resident, while the principal explanatory variable measures the relative priority assigned to education within the district's general public budget.

The question matters because a change in the share of education may reflect either increased education expenditure or a change in the denominator, including contractions or expansions in other budget functions. Economic activity may also influence fiscal capacity and expenditure choices, producing reverse causality. Consequently, a contemporaneous regression coefficient should not be interpreted as the economic return to education investment.

Beijing provides an unusually sharp illustration of the difference between economic production and the fiscal responsibilities of a district government. The location at which GDP is recorded need not coincide with the residence of workers or with the location where they

received their education. Education expenditure is also shaped by public-service obligations, student populations, fiscal transfers, and administrative arrangements. A district with substantial economic output may not necessarily have the same educational expenditure requirements as a residential district with a large school-age population.

The metropolitan setting therefore introduces a potential mismatch between the geographical allocation of fiscal expenditure and the geographical measurement of economic benefits. Students educated in one district may subsequently work in another. Firms may recruit workers from across the municipality or beyond. Public education may generate social and economic returns that are not captured by the current-year GDP of the district financing the expenditure.

The paper makes three deliberately bounded contributions. First, it assembles a district-year framework in which the fiscal variable is a composition measure, not an education investment stock. This distinction is important because expenditure shares are frequently interpreted as direct indicators of policy effort even though they are influenced by movements in other budget categories. Second, it contrasts the allocation-share relationship with specifications that account for the broader fiscal envelope and student enrollment intensity. These comparisons help assess whether the observed association is sensitive to differences in the scale of public spending or the demand for educational services. Third, it examines the sensitivity of inference to a small number of geographical clusters. With only 16 districts, conventional large-cluster approximations may be unreliable. The analysis therefore reports cluster-adjusted tests and wild cluster bootstrap results alongside the principal estimates.

The findings indicate no statistically robust contemporaneous association between the education expenditure share and nominal GDP per resident. The baseline coefficient is negative but statistically insignificant. Its direction changes when total fiscal expenditure per resident is introduced as an additional control. These results do not imply that public education spending is economically unproductive. Instead, they demonstrate the difficulty of identifying education-related economic returns from short-run changes in fiscal composition within a single metropolitan area.

This paper proceeds as follows. First, it reviews the relevant economic mechanisms. Second, it develops a conceptual framework. Third, it describes district-year-level measures as well as their limitations. Fourth, it lays out the estimation strategy. Fifth, it presents the empirical results. Finally, it discusses implications for fiscal evaluation and future research.

Literature review

Theoretical models of human-capital accumulation motivate a link between education and longer-run productive capacity. Gust et al. showed how skill formation underpins sustained economic growth, even if spending increases do not translate into immediate output gains [5]. This perspective provides a rationale for public education investment, but it does not imply that every increase in annual education expenditure must generate an immediate increase in measured economic output. Empirical research also emphasizes the distinction between educational attainment and the quality of skills acquired. Hanushek and Woessmann argued that cognitive skills provided important information about the relationship between education and economic development [6]. Their work suggests that expenditure measures alone may be insufficient to evaluate the productive contribution of education systems. The relationship between resources, educational outcomes, and economic performance involves multiple intermediate processes that are not necessarily observable in annual fiscal accounts.

A separate literature examines the composition of public expenditure. Andersson Järnberg and Värja demonstrated that the growth implications of government spending depended on how public resources were allocated across different functions [7]. Their framework cautions against assuming that expenditure categories commonly regarded as productive will necessarily exhibit positive coefficients in empirical growth regressions. The economic contribution of a spending category depends partly on the existing allocation structure and the relative productivity of alternative expenditures.

This consideration is especially relevant when education spending is expressed as a proportion of total government expenditure. An increase in the ratio does not uniquely identify an increase in educational resources. It can also result from reductions in other spending categories,

including infrastructure, public administration, health, or social protection. A coefficient on the education share therefore reflects a relationship involving the wider fiscal composition, rather than an isolated change in education expenditure.

Two issues limit identification in the present setting. First, district budget allocations may respond to changing economic conditions, school-age population, transfers, and administrative responsibilities. Second, Beijing districts form an integrated metropolitan economy rather than independent regional labor markets. These features create potential endogeneity and spillovers that cannot be eliminated simply by including district and year fixed effects.

Inference presents a separate challenge. Cameron et al. examined bootstrap-based improvements for clustered errors, while Colin Cameron and Miller discussed the limitations of conventional cluster-robust inference when the number of clusters was small [8,9]. Their work motivates additional caution when evaluating statistical significance in a panel containing only 16 geographical units.

Education expenditure, skills, and the timing of returns

The education-growth relationship has several distinct temporal dimensions. Expenditure on primary and secondary education purchases services during the budget year, whereas many potential economic returns arise only when students complete their education, enter the labor market, and accumulate professional experience. The delay between public expenditure and productive outcomes may extend over many years [10]. Consequently, the absence of a statistically significant relationship between current education spending and current economic output does not necessarily contradict theories of human-capital accumulation. It may instead reflect a mismatch between the observation period and the relevant economic mechanism.

Educational inputs are heterogeneous. Salaries, school maintenance, infrastructure, learning materials, inclusion services, and administrative expenditures need not generate comparable outcomes or operate over the same horizon. A budget category aggregates these expenditures without directly measuring their quality or effectiveness.

Furthermore, the economic value of education depends

on the institutional environment in which skills are developed and utilized. Labor-market conditions, industrial composition, innovation systems, and mobility opportunities influence whether educational achievements translate into productivity gains. Even effective educational expenditure may not produce measurable growth within the district responsible for financing it.

The literature on education and growth therefore offers reasons to investigate these mechanisms without implying that every public expenditure increase must generate immediate local growth. One theoretical tradition emphasizes the role of human-capital accumulation for long-run growth, while another line of work highlights the primacy of measured cognitive skills. Both perspectives suggest that fiscal expenditure and economic outcomes should ideally be connected through intermediate measures of educational performance and labor-market development.

Fiscal composition and the denominator problem

Public budgets are systems of competing commitments. An increase in the education share may be associated with higher education spending, lower spending on other functions, or a combination of the two. This is a compositional-data problem rather than a simple measurement inconvenience.

Let education expenditure be represented by E , total general public budget expenditure by B , and the education share by $s = \frac{E}{B}$. A proportional change in the share can be expressed as:

$$d\ln s = d\ln E - d\ln B \quad (1)$$

This identity shows that the education share may increase even when education expenditure remains unchanged, provided total expenditure decreases. Similarly, education expenditure may rise without an increase in its budget share if other expenditures grow more rapidly.

The interpretation of a regression coefficient on the education share is therefore conditional on the behavior of the fiscal denominator. A negative coefficient could arise because economically stronger districts allocate a relatively larger proportion of their budgets to other functions. A positive coefficient could reflect a different expenditure structure rather than the independent productivity of education.

Andersson Järnberg and Värja motivate attention to the composition of public expenditure while illustrating why

the productivity of a spending category depends on the wider allocation structure. Their argument is particularly relevant to a metropolitan panel in which fiscal obligations and expenditure priorities differ across administrative districts.

For this reason, the present study includes an additional specification controlling for total fiscal expenditure per resident. This adjustment is not a complete solution to the compositional problem, nor does it establish causality. It provides a diagnostic test of whether the estimated education-share association is sensitive to the scale of the district's fiscal budget.

Metropolitan geography and fiscal responsibility

Administrative districts are not closed economic systems. In a large metropolitan area, workplaces, schools, residential neighborhoods, and transport networks can span district boundaries. A district's GDP records production attributed to economic activity within its boundaries, while its education expenditure primarily reflects public-service responsibilities associated with the local administrative system.

The resulting geographical mismatch complicates interpretation. Education spending in one district may benefit workers employed elsewhere. Conversely, the productive activity recorded in a district may depend on human capital developed through educational expenditure in other districts or regions.

Beijing also contains districts with very different economic structures and fiscal responsibilities. The empirical design uses district fixed effects to remove stable level differences, but fixed effects cannot make the districts identical or eliminate time-varying institutional and demographic differences.

The study therefore treats the district as a fiscal and statistical observation unit, not as an independent economy. This distinction limits the interpretation of the results and informs the discussion of potential spillovers and longer-run effects.

Conceptual framework

The central distinction is between a fiscal allocation decision and the accumulation of productive skills. Public education spending may improve access, teacher quality, facilities, or learning outcomes. Those intermediate outcomes may influence employment, innovation, and productivity over time. The present data observe expenditure classifications and district economic

activity, not the intermediate educational outcomes needed to test the full mechanism.

The denominator of the fiscal ratio creates a second mechanism. Let the education share be education expenditure divided by total general public budget expenditure. A higher share may accompany a lower total budget even if nominal education spending is unchanged. Conversely, a district may increase education spending while its education share declines because other expenditure categories expand more rapidly.

The observable relationship may also run in reverse. Stronger local economic activity can affect fiscal revenues, expenditure capacity, and demand for public services. Municipal transfers and administrative responsibilities can simultaneously influence district budgets and economic outcomes. These relationships create endogeneity that cannot be eliminated by district and year fixed effects alone.

A framework of competing channels

Three channels link district education expenditure and measured economic activity. The human-capital channel operates through learning, future employment, and productivity. The public-demand channel operates through the current purchase of educational services and associated economic activity. The fiscal-composition channel operates through the relative allocation of public resources among education and other expenditure categories.

These channels need not generate the same contemporaneous relationship. Human-capital effects may be delayed, demand effects may be more immediate, and fiscal-composition effects depend on the productivity of alternative public expenditures. The direction of the estimated coefficient is therefore theoretically ambiguous.

A fourth channel runs from economic conditions to fiscal choices. Districts experiencing changes in production, property activity, population, or fiscal transfers may revise expenditure priorities. If budget decisions respond to economic performance, an observed association cannot be attributed exclusively to the effect of education expenditure on economic activity.

The empirical analysis is designed to describe these relationships rather than to distinguish all underlying mechanisms causally.

Methods and data

The analysis covers the 16 administrative districts of Beijing; the Beijing Economic-Technological Development Area (BDA) is not treated as a separate unit, because it is not a separate administrative district. The 2025 Beijing Regional Statistical Yearbook provides historical district series for 2015-2024, while education-category expenditure is assembled from annual district fiscal tables supplied in separate yearbook exports. Detailed source-file mapping for each fiscal-year district observation is reported in Table A1 (see Appendix).

Nominal GDP per resident is district GDP divided by resident population, with published units reconciled before division. The outcome is its natural logarithm. Because district-specific GDP deflators were not established from the source materials, the dependent variable is not described as real GDP per capita. Education expenditure share is district education expenditure divided by district general public budget expenditure. The education line is recorded in units of 10,000 yuan and the historical total expenditure series in units of 100 million yuan. The relevant units are reconciled before calculating the ratio.

All 128 district-year records have nonmissing values for the variables used in the principal regressions. Repeated fiscal-year entries found in the supplied exports were

checked for numerical discrepancies. However, full source-note verification remains important because identical numerical entries do not necessarily establish that different annual publications employ identical statistical definitions.

The unit of analysis is the administrative district, not the economic-development zone. The Beijing Economic-Technological Development Area is therefore not added as a seventeenth observation unit. The 2016-2023 window is covered by the historical district series, with annual education-category expenditures matched to the corresponding district and fiscal year. The dependent variable is logging nominal GDP per resident. Nominal output divided by resident population is an economic-activity indicator, not a measure of residents' wages or welfare. Year fixed effects absorb shocks shared by all districts, including common price-level movements, but cannot remove district-specific price changes or differences in sectoral deflators. Enrollment intensity is constructed as primary plus secondary students per 1,000 residents. It helps account for education-service demand but may itself respond to district population movements. Total fiscal spending per resident is included in an additional specification to examine sensitivity to the overall budget scale. Table 1 defines all key variables, their units, and important measurement limitations.

Table 1. Variables and measurement.

Measure	Definition	Unit/Limitation
Nominal GDP per resident	District GDP/resident population	Yuan per resident; not deflated
Education budget share	Education-category budget expenditure/total general public budget expenditure	Ratio from 0 to 1
Enrollment intensity	Primary + secondary enrollment / resident population × 1,000	Students per 1,000 residents
Fiscal spending per resident	Total general public budget expenditure/resident population	Yuan per resident
District fixed effects	District-specific intercepts	Capture time-invariant district characteristics

Note: The fiscal category is a district-budget classification, not all-sources education financing. Data are the author's calculations based on table exports from the Beijing Regional Statistical Yearbook.

Estimation and small-cluster inference

The principal estimating equation is:

$$\ln(GDPpc_{it}) = \beta EduShare_{it} + \gamma EnrollIntensity_{it} + \alpha_i + \lambda_t + \varepsilon_{it} \quad (2)$$

Where $GDPpc_{it}$ denotes nominal GDP per resident in

district i and year t , $EduShare_{it}$ denotes the education budget share, α_i represents district fixed effects, λ_t represents year fixed effects, and ε_{it} is the error term. The baseline specification omits enrollment intensity. District fixed effects absorb stable differences between

districts, including persistent geographical and institutional characteristics. Year fixed effects account for shocks common to the entire municipality. The coefficient β is identified from within-district changes in the education share relative to common annual changes.

Standard errors are clustered by district to accommodate arbitrary within-district heteroskedasticity and serial correlation. Because the sample contains only 16 clusters, conventional asymptotic inference may be unreliable. The analysis therefore reports p-values using 15 cluster degrees of freedom and supplements them with wild cluster bootstrap-t inference based on 3,999 replications. Additional specifications examine the exclusion of 2020, a one-year lag of the education share, adjustment for total budget spending per resident, and the sensitivity of estimates to individual districts. These checks evaluate robustness but do not eliminate potential reverse causality or omitted-variable bias.

Measurement, comparability, and accounting scope

The fiscal series must be interpreted within the institutional accounting framework from which it is drawn. General public budget education expenditure is not equivalent to total educational resources available in a district. Other financing channels, municipal-level spending, private expenditure, and expenditures administered through different institutional arrangements may contribute to education provision.

Cross-edition statistical revisions present another risk. A later yearbook can restate historical GDP or population series, while an earlier publication may report expenditure categories under the classification used at that time. Combining series requires attention to the yearbook edition, statistical definitions, accounting units, and geographical coverage.

Nominal GDP per resident is intentionally retained as the principal outcome. Converting it to real terms requires an appropriate price adjustment that reflects district economic activity. Applying a municipality-wide consumer price index would not necessarily reproduce district-level real GDP growth because consumer prices and output prices measure different concepts.

The analysis therefore avoids labeling nominal GDP as real GDP and interprets the findings as relationships involving reported nominal economic activity.

Interpretation of the fixed-effects estimator

The district fixed-effects estimator identifies the

coefficient from within-district changes after subtracting annual citywide movements. This design reduces confounding from stable district characteristics but does not identify a causal education-spending effect.

Time-varying local policies, demographic changes, fiscal transfers, sectoral shifts, and district-specific economic shocks may simultaneously influence fiscal composition and economic output. Such factors can remain in the regression error term and bias the estimated coefficient. A further consideration is the meaning of the coefficient's unit. The education share is expressed as a fraction, not as a percentage measured from zero to 100. A one-percentage-point increase corresponds to a change of 0.01 in the explanatory variable. Accordingly, a coefficient of -0.941 corresponds to an approximate change of -0.941% in nominal GDP per resident for a one-percentage-point increase in the education share, holding the included controls and fixed effects constant. This is an association, not an estimated causal return.

Inference with sixteen district clusters

Cluster-robust standard errors permit observations within a district to share shocks and serial dependence, but their familiar asymptotic justification relies on having a sufficiently large number of independent clusters. With only 16 districts, conventional normal approximations may understate uncertainty.

The analysis uses cluster-adjusted t-tests and wild cluster bootstrap procedures as complementary approaches. The bootstrap resamples disturbances at the district level rather than treating individual district-year observations as independent. This preserves the clustered structure of the panel during inference.

Bootstrap inference is not a substitute for a credible research design. Its purpose is to improve the approximation of the sampling distribution under clustered errors, not to resolve endogeneity, measurement error, or the limited external validity of a single-city study.

Results and discussion

Table 2 reports regression estimates across our five main specifications, with data derived from the author's calculations using the compiled district-year panel covering all 16 Beijing administrative districts over the 2016-2023 sample window. All models include district and year two-way fixed effects, and statistical inference accounts for district-level clustered errors alongside wild-cluster bootstrap adjustments to address the small-cluster concern raised in the methodology section.

Table 2. Education budget share and log nominal GDP per resident.

Specification	n	Coefficient	Cluster SE	t(15) p	Wild p
Baseline	128	-0.941	0.732	0.218	0.203
Enrollment adjusted	128	-0.824	0.820	0.331	0.329
Fiscal scale adjusted	128	+0.543	0.790	0.790	0.517
Excluding 2020	112	-0.942	0.877	0.300	0.301
One-year lag	112	-0.535	0.845	0.536	0.554

Notes: District and year fixed effects are included throughout. Clustered standard errors use the standard finite-sample adjustment. The wild cluster bootstrap uses 3,999 replications. Reported coefficients are expressed as a fraction for the education budget.

The baseline coefficient is -0.941, and the enrollment-adjusted coefficient is -0.824. Both are imprecisely estimated. The corresponding cluster-adjusted and bootstrap p-values exceed conventional significance thresholds.

Across the leave-one-district-out specifications, the enrollment-adjusted coefficient ranges from -1.531 to -0.128. This variation illustrates that the point estimate is sensitive to the composition of a relatively small metropolitan sample.

The estimates do not establish a statistically precise contemporaneous relationship between the share of district public expenditure allocated to education and nominal GDP per resident. The absence of significance is not evidence that education expenditure has no economic value. Rather, the regression does not isolate a reliably measurable association within the specified setting.

Several alternative explanations cannot be distinguished here. District output may depend on commuter inflows, sectoral composition, enterprise concentration, and economic activity unrelated to local educational provision. Education expenditure may reflect the needs of resident students, while the economic benefits of education may arise elsewhere or after substantial delays. The sensitivity to total fiscal expenditure per resident is substantively important. Once the budget scale is included, the education-share coefficient changes from negative to positive. Although the positive coefficient is also statistically insignificant, the sign reversal indicates that the direction of the association depends on the fiscal specification.

The year 2020 may represent an exceptional shock, but excluding it does not materially change the inference. Likewise, lagging the education share by one year does

not produce a statistically significant estimate. A single-year lag, however, is too short to capture many plausible human-capital mechanisms.

It is also possible that the economically relevant outcome is not district GDP in the same year. Education benefits may be more visible in student achievement, school completion, employment outcomes, labor productivity, or earnings over a longer period.

What the baseline association does and does not show

The baseline coefficient is negative, but its cluster-adjusted uncertainty is large enough that the null hypothesis of no association cannot be rejected. The estimate should therefore not be described as evidence that education expenditure reduces economic activity.

The negative point estimate has several plausible explanations that the present data cannot distinguish. Districts facing weaker economic conditions may protect education budgets while reducing other expenditures. Districts with stronger economic activity may devote a greater proportion of their budgets to infrastructure or other functions. Changes in the fiscal denominator may also affect the education share independently of changes in education spending.

These explanations are consistent with the accounting properties of expenditure shares, but they are not directly tested by the baseline regression. The empirical finding is limited to an imprecise conditional association.

Sensitivity to fiscal scale and educational demand

Adding student enrollment intensity offers a partial check on observable education-service demand. The resulting education-share coefficient remains negative and statistically insignificant. This suggests that the baseline inference is not driven solely by the inclusion or exclusion of the enrollment measure.

A more consequential sensitivity check adds per-capita

total general public budget expenditure. Under that specification, the education-share coefficient becomes positive. The estimate remains imprecise, but the sign change indicates that budget scale and fiscal composition should not be treated as interchangeable concepts.

The finding also cautions against interpreting the education share as a direct measure of education investment. When the fiscal denominator changes, the same level of education spending can correspond to different expenditure shares. The empirical relationship may therefore reflect changes in other budget categories rather than changes in education provision.

Temporal sensitivity and the limits of lagged regressions

Excluding 2020 examines whether the exceptional economic environment associated with the pandemic year dominates the estimated relationship. The coefficient remains negative and statistically insignificant, indicating that the baseline inference does not depend entirely on that single year.

Replacing the contemporaneous education share with its one-year lag is another limited diagnostic. The lagged coefficient is also negative and statistically insignificant. This result does not rule out delayed education effects, because the relevant adjustment period may be considerably longer than one year.

Human-capital accumulation typically involves several stages, including educational participation, skill acquisition, graduation, employment, and workplace learning. A short panel cannot fully trace these processes. Consequently, the lagged specification should be interpreted as a sensitivity check rather than a comprehensive dynamic model of educational investment.

Economic significance, precision, and null results

Economic significance should be considered separately from statistical significance. A coefficient may be economically meaningful in magnitude while remaining too imprecisely estimated to support a reliable conclusion. In the present study, uncertainty is substantial, and the coefficient changes sign under an alternative fiscal specification.

Null results are often treated as disappointing, but they can help refine a research question when reported transparently. Here, the evidence suggests that contemporaneous district GDP is not a sufficiently informative outcome for evaluating the economic

contribution of education budget shares over a short period.

The analysis therefore supports a narrower conclusion: the specified data and models do not establish a robust contemporaneous association. It does not support a broader conclusion about the effectiveness or inefficiency of public education expenditure.

Threats to identification and external validity

Several identification threats remain. First, time-varying local economic policies may influence output and expenditure decisions simultaneously. Second, changes in population and economic structure may affect both education-service demand and measured GDP per resident. Third, fiscal transfers and administrative responsibilities may create variation in district expenditure shares unrelated to locally generated economic activity.

Measurement issues also matter. The education expenditure category aggregates different types of spending, while nominal GDP per resident does not directly measure educational outcomes or household welfare. District-level price differences and statistical revisions may introduce additional comparability concerns.

The findings should not be generalized mechanically beyond Beijing. Other cities differ in fiscal decentralization, school-financing arrangements, labor mobility, and administrative boundaries. The relationship between fiscal composition and economic activity may therefore vary across institutional settings.

Policy implications

Three practical implications follow from the limitations of the evidence. First, fiscal performance dashboards should report both the education share and education expenditure per pupil or resident. A share alone cannot distinguish increased investment from a contraction in other spending categories. Second, budget evaluation should align the horizon of the outcome with the policy mechanism. Short-run GDP is a poor stand-alone criterion for an intervention intended to affect skills over many years. Third, cross-district comparisons should adjust for service responsibilities, enrollment demand, intergovernmental transfers, and population mobility before attributing economic differences to fiscal priorities.

The estimates do not justify reducing education budgets,

nor do they quantify the growth return to an additional yuan of education expenditure. They instead support better measurement and longer-horizon evaluation. For a city with integrated labor markets and concentrated business activity, education policy assessment should explicitly allow benefits to spill across administrative boundaries.

Implications for fiscal monitoring and evaluation

The results favor a multidimensional approach to monitoring education expenditure. Fiscal authorities can report the education share alongside absolute real expenditure, expenditure per pupil, enrollment, and indicators of service quality.

Such a dashboard would make it easier to distinguish genuine resource expansion from denominator-driven changes. It would also discourage comparisons that reward a high share without examining the adequacy, efficiency, or distribution of services. These recommendations are consistent with the measurement problems identified in the analysis, but they should not be presented as policies proven to increase GDP by the estimated coefficient.

Evaluation horizons should match policy mechanisms. Routine annual budget monitoring is useful for fiscal discipline and service delivery, while educational outcomes and labor-market returns require longer follow-up. Where feasible, administrative education records can be linked to later employment and earnings outcomes under appropriate privacy safeguards. This would shift attention from an aggregate fiscal ratio to the channels through which spending may matter. A complementary approach is to document changes in specific budget categories, such as facilities, teacher support, or student assistance, before testing outcomes that plausibly respond to those interventions.

For applied economic research, the case underscores the value of clearly defining the treatment, the outcome, and the counterfactual. A budget share is not the same intervention as an additional unit of spending, and a district's nominal GDP per resident is not the same outcome as the welfare of its students. Better identification could come from transparent allocation-rule changes or credible policy discontinuities, but such designs should only be claimed when the relevant institutional evidence and data exist. In the absence of that evidence, a disciplined descriptive panel analysis

remains useful provided its limitations are made central rather than hidden.

Conclusion

Using 128 observations for Beijing's 16 districts during 2016-2023, this paper finds no statistically precise within-district contemporaneous association between the education share of general public budget expenditure and nominal GDP per resident. The evidence is descriptive, limited by the short panel, few clusters, and potential endogeneity. The results motivate longer-horizon research with educational quality, commuting patterns, fiscal transfers, and credible sources of exogenous policy variation. They do not support a causal recommendation to increase or decrease district education budgets.

The analysis is best read as evidence about the limits of contemporaneous fiscal-share regressions in a highly integrated metropolitan economy. Its main finding is the absence of a statistically precise and specification-robust association, not proof of a zero return to education. Future work should combine budget levels, education quality, fiscal transfers, student cohorts, and mobility data and should seek plausibly exogenous changes in fiscal assignments or educational policy.

Funding

This work was not supported by any funds.

Acknowledgements

The author would like to show sincere thanks to those technicians who have contributed to this research.

Conflicts of Interest

The author declares no conflict of interest.

References

- [1] Ma, C., Wu, H., Li, X. (2023) Spatial spillover of local general higher education expenditures on sustainable regional economic growth: a spatial econometric analysis. *PLoS One*, 18(11), e0292781.
- [2] Abraham, K. G., Mallatt, J. (2022) Measuring human capital. *Journal of Economic Perspectives*, 36(3), 103-130.
- [3] Oyebowale, A. Y., Babatunde, S. A., EL Fakir, A. (2026) Government expenditures on education and economic growth in the UK. *Applied Economics Letters*, 33(16), 3086-3092.
- [4] Awuna, J. M., Malkina, M. Y., Adejo, M. A. (2026) The nexus between government investment in

- human capital and economic growth in Nigeria: when institutions matter. *BRICS Journal of Economics*, 7(2), 81-99.
- [5] Gust, S., Hanushek, E. A., Woessmann, L. (2024) Global universal basic skills: current deficits and implications for world development. *Journal of Development Economics*, 166, 103205.
- [6] Hanushek, E. A., Woessmann, L. (2008) The role of cognitive skills in economic development. *Journal of Economic Literature*, 46(3), 607-668.
- [7] Andersson Järnberg, L., Värja, E. (2023) The composition of local government expenditure and income growth: the case of Sweden. *Regional Studies*, 57(9), 1784-1797.
- [8] Cameron, A. C., Gelbach, J. B., Miller, D. L. (2008) Bootstrap-based improvements for inference with clustered errors. *The Review of Economics and Statistics*, 90(3), 414-427.
- [9] Colin Cameron, A., Miller, D. L. (2015) A practitioner's guide to cluster-robust inference. *Journal of Human Resources*, 50(2), 317-372.
- [10] Contreras, D., Galindo, A. J., Lepe, I. (2026) Education spending and economic growth: short- and long-term effects in a global panel. *Review of Development Economics*, 1-22.

Appendices

The district education expenditure series was assembled from the following supplied statistical tables, as summarized in Appendix Table A1.

Table A1. Source mapping and replication qualifications.

Fiscal year	Source file	Districts
2016	Beijing regional statistical yearbook - local fiscal expenditure	16
2017		
2018		
2019		
2020		
2021		
2022		
2023		

Note: The source mapping records the supplied filenames, not independently verified publication-year metadata.