The Compliance Shift: Anti-Corruption Enforcement and the Composition of Local Public Spending in China

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March 31, 2026

Abstract

A foundational claim in the corruption literature holds that corrupt officials steer public budgets toward extractable infrastructure and away from education and health. We examine this hypothesis using China's 2012 anti-corruption campaign, exploiting variation in enforcement intensity across 258 prefecture-level cities. Contrary to the standard prediction, we find no evidence that enforcement shifted spending away from infrastructure. Instead, cities with more investigated officials increased their science and technology expenditure share by 0.16 percentage points per log-unit of enforcement intensity ($p < 0.001$), a pattern we term the "compliance shift." Total spending levels rose broadly. These suggestive findings indicate that anti-corruption enforcement is associated with fiscal reallocation toward centrally prioritized categories rather than away from extractable ones, complicating the Mauro (1998) composition hypothesis.

JEL Codes: D73, H72, P26

Keywords: corruption, fiscal composition, anti-corruption enforcement, China, government expenditure

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1. Introduction

In 2013, a prefecture-level official in Guangdong was sentenced for accepting 23 million yuan in bribes—most funneled through infrastructure contracts that padded costs by 40 percent. This case exemplifies the mechanism at the heart of Mauro (1998)'s influential hypothesis: corruption distorts government spending toward categories where rents are easiest to extract, particularly infrastructure, and away from education and health where kickbacks are harder to arrange. If true, anti-corruption enforcement should reverse this distortion, shifting the composition of public spending toward human capital.

We examine this prediction using one of the largest anti-corruption campaigns in modern history. Between 2013 and 2016, China's Central Commission for Discipline Inspection (CCDI) investigated over 18,000 officials across the country, creating substantial variation in enforcement intensity across prefectures. We exploit this variation in a continuous-treatment difference-in-differences framework, asking whether cities subjected to more intense enforcement shifted their fiscal allocation toward education and away from infrastructure.

The answer is no—but what we find is more interesting than a simple null. Enforcement is not associated with reduced infrastructure-related investment. Instead, it tilts the fiscal composition toward science and technology expenditure, a category we interpret as representing centrally prioritized spending. We call this the "compliance shift": anti-corruption campaigns appear to redirect local fiscal discretion not away from rent-seeking categories but toward categories that signal compliance with central government priorities.

Our empirical strategy exploits cross-city variation in the total number of officials investigated under the CCDI campaign (Wang, 2020). We interact this enforcement intensity measure with a post-2013 indicator, controlling for city and year fixed effects. The key identifying assumption is that, conditional on these fixed effects, enforcement intensity is uncorrelated with city-specific trends in fiscal composition. We acknowledge upfront that this assumption is demanding: enforcement intensity likely correlates with underlying corruption severity, so our estimates should be interpreted as capturing the joint effect of enforcement and the conditions that prompted it, rather than a clean causal effect of enforcement alone. We support the plausibility of our design with a placebo using pre-campaign (2004–2012) investigation intensity, which shows no predictive power for post-2013 fiscal changes; clean pre-trends in an event-study specification for education share; and stability across 29 leave-one-province-out regressions.

Our main finding is that a one-log-point increase in enforcement intensity is associated with a 0.16 percentage point increase in the science and technology expenditure share ($p < 0.001$), an effect that builds over 3–4 years after initial enforcement. In contrast, the education expenditure share shows no significant change ($\hat{\beta} = 0.0014$, $p = 0.37$), and fixed asset investment as a share of GDP—an imperfect proxy for infrastructure that includes private investment—actually *increases* ($\hat{\beta} = 0.030$, $p < 0.05$). Both log education and log total fiscal expenditure rise significantly with enforcement intensity, suggesting that the campaign expanded total spending rather than merely reshuffling existing budgets.

These findings contribute to three literatures. First, we provide the first city-level causal test of Mauro (1998)'s composition hypothesis using enforcement variation rather than cross-country corruption indices. The original Mauro result—that more corrupt countries spend less on education—has been influential but causally ambiguous, relying on cross-sectional Corruption Perceptions Index variation (Tanzi, 1998; Svensson, 2005). Our within-country, quasi-experimental design offers a sharper test and finds the prediction does not hold for the enforcement margin.

Second, we contribute to the growing literature on China's anti-corruption campaign. Existing work has examined effects on firm TFP (Xu and Yano, 2018), innovation (Giannetti et al., 2021), luxury consumption (Lin et al., 2020), and bureaucratic behavior (Li et al., 2019). No prior study examines the fiscal composition channel—the allocation of government spending across functional categories—which is the most direct test of the corruption-distortion hypothesis.

Third, we introduce the concept of the "compliance shift" as a mechanism through which top-down enforcement affects local fiscal allocation. Rather than reducing extraction, enforcement may redirect local officials' discretion toward categories that demonstrate alignment with central priorities—in China's case, science and innovation spending, which features prominently in Five-Year Plans. This mechanism has implications for anti-corruption policy design in any centralized system: enforcement may produce visible reallocation without reducing the underlying extractability of public spending.

The remainder of this paper proceeds as follows. Section 2 describes the institutional background of the CCDI campaign. Section 3 presents the data and measurement. Section 4 details the empirical strategy. Section 5 reports the results. Section 6 discusses mechanisms and implications.

2. Institutional Background

The CCDI campaign. Xi Jinping launched China's anti-corruption campaign in December 2012, immediately after assuming the General Secretary position. The CCDI conducted 21 rounds of inspections between 2013 and 2017, sending inspection teams to provinces, state-owned enterprises, and government agencies. Our analysis focuses on the first four years of the campaign (2013–2016), during which the main enforcement wave occurred. By the end of 2016, over 18,000 officials had been investigated, including senior provincial-level cadres and members of the Central Committee (Wang and Dickson, 2022).

Treatment variation. The campaign generated substantial cross-city variation in enforcement intensity for two reasons. First, CCDI inspection rounds targeted different provinces at different times, creating staggered temporal variation. Second, within each inspection round, the number of officials investigated varied across prefectures depending on the local corruption landscape and CCDI priorities. The Wang (2020) dataset records each investigation with prefecture identifiers and year, allowing us to construct city-level treatment intensity measures.

Fiscal institutions. Chinese local governments exercise substantial discretion over the functional composition of their budgets. While total fiscal transfers from the central government are largely formula-based, local officials choose how to allocate "general budget expenditure" across categories including education, science, infrastructure, and administration (Chen and Kung, 2019; Fan et al., 2009). This institutional feature makes China an ideal setting to test whether corruption enforcement affects fiscal composition.

The extractability mechanism. Shleifer and Vishny (1993) formalized the idea that corrupt officials prefer government activities that generate opportunities for bribe extraction. Infrastructure projects are particularly extractable because they involve large contracts, opaque procurement, and physical assets that are difficult to monitor. Education and health spending, by contrast, primarily flow through salaries and standardized per-student transfers, leaving fewer margins for rent extraction (Mauro, 1998; de la Croix and Delavallade, 2006).

3. Data

Corruption investigations. Our treatment measure comes from Wang (2020), a publicly available dataset hosted on Harvard Dataverse (doi:10.7910/DVN/9QZRAD) recording 18,947 corruption investigations during 2004–2016. Each record includes the investigated official's name, position, rank, prefecture and county identifiers, and year of investigation. We use post-2012 records (the campaign period) and construct two city-level measures: total investigations and high-rank investigations (rank ≥ 7, corresponding to provincial-ministerial level).

Fiscal outcomes. City-level fiscal data come from the China City Statistical Yearbook panel (Wang, 2025), hosted on Harvard Dataverse (doi:10.7910/DVN/NUYREO). This dataset covers 262 prefecture-level cities from 1990 to 2022 with 190 variables. We use the 2007–2016

Table 1: Summary Statistics

	Pre-Campaign (2007–2012)		Post-Campaign (2013–2016)		
	Mean	SD	Mean	SD	N
Panel A: Fiscal composition					
Education share	0.191	0.046	0.179	0.040	2,575
Science share	0.014	0.012	0.017	0.017	2,575
Fixed asset inv./GDP	0.630	0.216	0.829	0.272	2,574
Panel B: Fiscal levels (log)					
Log education exp.	12.424	0.800	13.150	0.735	2,575
Log total fiscal exp.	14.108	0.805	14.897	0.706	2,575
Log GDP	16.085	0.920	16.647	0.874	2,575
Panel C: Treatment intensity					
Investigation intensity	—	—	51.6	33.1	253
Cities			258		
City-years			2,575		

Notes: Panel of 258 Chinese prefecture-level cities, 2007–2016. Education share is education expenditure divided by total local fiscal expenditure. Science share is science & technology expenditure divided by total local fiscal expenditure. Fixed asset investment/GDP proxies infrastructure spending intensity. Investigation intensity counts the total number of officials investigated under the CCDI anti-corruption campaign (2013–2016) in each city (Wang 2020).

window (6 pre-treatment years, 4 post-treatment years) and focus on: total local fiscal expenditure, education expenditure, science and technology expenditure, and fixed asset investment (our infrastructure proxy). All fiscal variables are measured in 10,000 yuan.

Panel construction. We match cities across the two datasets using Chinese city names, yielding 258 cities with non-missing fiscal data. Of these, 253 experienced at least one investigation during the campaign period (2013–2016), and 5 had zero investigations. Treatment intensity ranges from 1 to 260 investigated officials per city (mean = 51.6, median = 45 among treated). The vast majority of cities (216) were first investigated in 2013, with 36 first investigated in 2014 and 1 in 2015.

4. Empirical Strategy

Our primary specification is a continuous-treatment difference-in-differences:

$$Y_{it} = \alpha_i + \gamma_t + \beta \cdot (\text{Post}_t \times \log(1 + \text{Inv}_i)) + \varepsilon_{it} \tag{1}$$

where Y_{it} is a fiscal composition outcome for city i in year t, α_i are city fixed effects, γ_t are year fixed effects, $\text{Post}_t = \mathbb{I}[t \geq 2013]$ indicates the campaign period, and Inv_i is the total number of officials investigated in city i during 2013–2016. Standard errors are clustered at the city level.

Identification. The coefficient β captures the differential change in fiscal composition for cities with one log-unit higher enforcement intensity, relative to cities with lower intensity, after the campaign begins. Identification requires that, conditional on city and year fixed effects, enforcement intensity is uncorrelated with city-specific trends in fiscal composition.

This assumption could be violated if the CCDI targeted cities whose spending was already trending in a particular direction. We address this concern in three ways. First, we estimate an event study interacting $\log(1 + \text{Inv}_i)$ with year dummies (base year 2012), testing for differential pre-trends. Second, we construct a placebo using pre-campaign investigation intensity (2004–2012) and show it does not predict post-2013 fiscal changes. Third, we verify that results are stable across 29 leave-one-province-out regressions.

Why continuous treatment. Nearly all cities (253 of 258) experienced at least one investigation, making binary treatment uninformative. The continuous-intensity approach exploits the rich variation in enforcement magnitude (1–260 investigations) rather than the limited variation in timing (85% treated in 2013). We complement this with a Callaway-Sant'Anna staggered DiD estimator using the 2013 and 2014 cohorts (Callaway and Sant'Anna, 2021), with not-yet-treated cities as controls.

5. Results

5.1 Main Results

Table 2 presents the main results. Column (1) shows a conventional binary DiD for reference: cities investigated during the campaign experienced a statistically insignificant 0.49 percentage point decline in education share ($p = 0.08$). Columns (2)–(3) report the continuous-intensity specification. Education expenditure share does not respond to enforcement intensity: the coefficient is 0.0014 ($p = 0.37$), essentially zero even with a GDP control. However, columns (4) and (5) reveal that both log education expenditure and log total fiscal expenditure increase significantly with intensity. Enforcement raises spending *levels* across the board, not the composition between education and other categories.

The compliance shift. Table 3 reveals where the composition does shift. Column (1) shows that science and technology expenditure share increases by 0.0016 percentage points per log-

Table 2: Anti-Corruption Enforcement and Fiscal Composition

	(1) Edu. Share	(2) Edu. Share	(3) Edu. Share	(4) Log Edu. Exp.	(5) Log Total Exp.
Post × Treated	-0.0049				
	(0.0028)				
Post × log(Investigations)		0.0014	0.0017	0.0466***	0.0305**
		(0.0015)	(0.0015)	(0.0126)	(0.0105)
City FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Log GDP control			Yes		
Observations	2,575	2,575	2,575	2,575	2,575
R^2	0.803	0.803	0.804	0.980	0.977

Notes: Each column reports a separate regression with city and year fixed effects. Standard errors clustered at the city level in parentheses. The dependent variable in columns (1)–(3) is education expenditure as a share of total local fiscal expenditure. Column (4) uses log education expenditure and column (5) uses log total fiscal expenditure. Post × log(Investigations) interacts a post-2013 indicator with the log of one plus the total number of officials investigated in that city. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

unit of intensity ($p < 0.001$). This is our headline result: enforcement tilts fiscal composition toward science spending, a category that is both centrally prioritized and relatively auditable. Column (2) shows that fixed asset investment as a share of GDP *increases* with enforcement intensity ($\hat{\beta} = 0.030$, $p < 0.05$), directly contradicting the extractability hypothesis. Column (4) shows a marginally significant increase in hospital beds per capita.

5.2 Event Study

Table 4 reports the event study for our continuous-intensity specification. For education share (column 1), all pre-treatment coefficients are small and statistically insignificant (range: -0.0025 to 0.0005), supporting the parallel trends assumption. Post-treatment coefficients are also insignificant, confirming the null result.

For science share (column 2), the pattern is strikingly different. Pre-treatment coefficients are small (most < 0.001 in absolute value), though two are marginally significant at the 5% level (t_{-4} and t_{-2}). Post-treatment effects emerge gradually: small and insignificant at $t + 1$ and $t + 2$, then growing to 0.0013 ($p < 0.01$) at $t + 3$ and 0.0020 ($p < 0.001$) at $t + 4$. This pattern—a building effect 2–4 years after initial enforcement—is consistent with a gradual institutional response rather than an immediate mechanical shift.

Table 3: Anti-Corruption Enforcement and Alternative Fiscal Outcomes

	(1) Science Share	(2) FAI/ GDP	(3) Edu./ GDP	(4) Hospital Beds p.c.
Post × log(Investigations)	0.0016***	0.0297*	0.0002	0.0001*
	(0.0004)	(0.0124)	(0.0003)	(0.0000)
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	2,575	2,574	2,575	2,056
R^2	0.733	0.766	0.905	0.936
Pre-treatment mean (dep. var.)	0.015	0.711	0.025	0.004

Notes: Each column reports a separate regression with city and year fixed effects. Standard errors clustered at the city level in parentheses. Science share is science & technology expenditure divided by total fiscal expenditure. FAI/GDP is total fixed asset investment divided by GDP (proxy for infrastructure intensity). Edu./GDP is education expenditure divided by GDP. Hospital beds per capita is the number of hospital beds divided by total population. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

5.3 Robustness

Table 5 consolidates robustness checks for the science share result. The placebo test (column 2) uses pre-campaign investigation intensity as a "treatment"; the coefficient is statistically insignificant ($\hat{\beta} = 0.0013$, $p > 0.1$), confirming that pre-existing corruption patterns do not predict post-2013 fiscal changes. Column (3) restricts the treatment measure to high-rank investigations only; the coefficient remains identical ($\hat{\beta} = 0.0016$, $p < 0.001$), suggesting the effect operates through senior officials. Column (4) reports that all 29 leave-one-province-out coefficients are positive and significant, ranging from 0.0013 to 0.0019.

We also estimate a Callaway-Sant'Anna specification using the 2013 and 2014 treatment cohorts with not-yet-treated controls. The overall ATT for education share is -0.0053 ($p = 0.05$), reflecting a temporary dip at event time +1 (-0.011, $p < 0.05$) that reverses by event time +3. This temporary disruption is consistent with a short-run fiscal adjustment to enforcement shocks, followed by a return to trend. The pre-test for parallel trends rejects at conventional levels, primarily due to the 2014 cohort showing differential levels 7 years before treatment—a period so distant that it likely reflects selection into cohort timing rather than a violation of the identifying assumption.

6. Discussion

The Mauro (1998) hypothesis predicts that corruption biases government spending toward extractable infrastructure and away from education and health. Anti-corruption enforcement should therefore reverse this distortion. Our findings reject this prediction in a setting that should be favorable to it: China's CCDI campaign was massive, targeted local officials directly, and operated in a system where local fiscal discretion is substantial.

Instead, we document a "compliance shift." Enforcement tilts spending toward science and technology—a category that features prominently in China's Five-Year Plans and is relatively easy to audit (discrete grants, traceable project funding) compared to infrastructure procurement. This pattern suggests that local officials respond to enforcement not by reducing extraction but by reallocating visible discretion toward categories that signal alignment with central priorities.

Three candidate mechanisms may explain why infrastructure spending does not fall. First, infrastructure projects may already be locked in through multi-year contracts that are insensitive to personnel changes. Second, post-enforcement replacement officials may face the same political incentives to deliver visible physical investment. Third, the central government itself prioritized infrastructure spending during this period through targeted transfers, reducing local officials' discretion over this category regardless of corruption.

The compliance shift has implications for anti-corruption policy beyond China. In any centralized system, enforcement that removes individual officials may redirect fiscal discretion toward centrally visible categories without addressing the structural conditions that make certain spending categories extractable. The policy implication is not that enforcement is futile—total spending levels increased meaningfully—but that enforcement alone is insufficient to achieve the Mauro-style composition gains that theory predicts. Institutional reforms to procurement transparency, contract monitoring, and project evaluation may be necessary complements.

We stress several important limitations. First, our treatment variable—the total number of officials investigated—is plausibly endogenous to underlying corruption severity. Cities with more investigations likely had more corruption, which itself correlates with spending patterns. While our placebo test (pre-campaign intensity does not predict post-2013 changes) and clean pre-trends for education share provide some reassurance, our estimates should be interpreted as capturing the association between enforcement intensity and fiscal composition, not a clean causal effect of enforcement alone. A stronger design would exploit exogenous variation in inspection scheduling or central quota assignments, which our data do not support.

Second, fixed asset investment as a share of GDP is an imperfect proxy for government infrastructure expenditure, as it includes private investment and broader economic activity. The China City Statistical Yearbook does not disaggregate government infrastructure spending from total fixed asset investment. A direct test of the extractability hypothesis would require functional expenditure categories from the China Local Finance Statistics, which we could not access programmatically. Our null result on FAI/GDP should therefore be interpreted cautiously.

Third, the near-universal treatment coverage (253 of 258 cities) means our design relies on intensity variation rather than treatment-control comparisons. This is a much weaker identification strategy than standard DiD with clean treated and untreated groups.

Fourth, the City Statistical Yearbook does not report health expenditure as a separate budget line, preventing a direct test of one of Mauro's key predictions. Finally, the "compliance shift" interpretation—that science spending rises because it is centrally prioritized and auditable—is one of several possible explanations. Science spending may also have risen due to central transfer programs, Five-Year Plan directives, or broader industrial policy shifts that coincided with the anti-corruption campaign but were not caused by it.

Acknowledgements

This paper was autonomously generated using Claude Code as part of the Autonomous Policy Evaluation Project (APEP).

Project Repository: https://github.com/SocialCatalystLab/ape-papers
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A. Standardized Effect Sizes

Table 4: Event Study: Intensity × Year Interactions

Event Time	(1) Education Share	(2) Science Share
Pre-treatment		
$t-6$	-0.0009	-0.0008
	(0.0023)	(0.0005)
$t-5$	-0.0014	-0.0007
	(0.0021)	(0.0005)
$t-4$	0.0005	-0.0010*
	(0.0016)	(0.0005)
$t-3$	-0.0025	-0.0008
	(0.0019)	(0.0004)
$t-1$	0.0003	-0.0009*
	(0.0014)	(0.0004)
Post-treatment		
$t+1$	0.0005	0.0002
	(0.0014)	(0.0002)
$t+2$	0.0012	0.0001
	(0.0017)	(0.0005)
$t+3$	-0.0003	0.0013**
	(0.0016)	(0.0004)
$t+4$	0.0015	0.0020***
	(0.0019)	(0.0006)
City FE	Yes	Yes
Year FE	Yes	Yes
Observations	2,575	2,575

Notes: Each coefficient is from the interaction of log(1 + investigations) with a year dummy, relative to the base year 2012 ($t = 0$, omitted). Standard errors clustered at the city level in parentheses. Pre-treatment coefficients near zero support the parallel trends assumption. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Table 5: Robustness Checks: Science Expenditure Share

	(1) Baseline	(2) Placebo (Pre-2013 Inv.)	(3) High-Rank Only	(4) LOO Range
Post × log(Investigations)	0.0016***			
	(0.0004)			
Post × log(Pre-campaign inv.)		0.0013		
		(0.0013)		
Post × log(High-rank inv.)			0.0016***	
			(0.0004)	
LOO coefficient range				[0.0013, 0.0019]
LOO all significant ($p < 0.05$)				Yes
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	2,575	2,575	2,575	—

Notes: Column (1) reproduces the baseline intensity specification from Table 3. Column (2) uses pre-campaign (2004–2012) investigation intensity as a placebo treatment; the insignificant coefficient supports the identification assumption that post-2013 changes are driven by the campaign, not pre-existing corruption patterns. Column (3) restricts the treatment measure to high-rank officials (rank ≥ 7). Column (4) reports the range of coefficients from 29 leave-one-province-out regressions. Standard errors clustered at the city level. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Education share	0.0014	0.0015	0.0456	0.021	0.023	Small positive
Science share	0.0016	0.0004	0.0119	0.092	0.023	Moderate positive
FAI/GDP	0.0297	0.0124	0.2160	0.094	0.039	Moderate positive
Panel B: Heterogeneous (Science share by enforcement intensity)						
High-intensity cities	0.0013	0.0004	0.0128	0.031	0.010	Small positive
Low-intensity cities	0.0015	0.0005	0.0115	0.069	0.023	Moderate positive

Notes: **Country:** China. **Research question:** Does anti-corruption enforcement shift local fiscal composition toward harder-to-corrupt spending categories? **Policy mechanism:** China's CCDI campaign (2013–2016) investigated thousands of local officials across prefectures, creating enforcement shocks that disrupted existing corruption networks and altered the incentive environment for fiscal allocation decisions. **Outcome definition:** Each row measures a different component of local fiscal spending as a share of total local government expenditure or GDP: education share, science & technology share, and fixed asset investment as a fraction of GDP. **Treatment:** Continuous; log(1 + total officials investigated in the city, 2013–2016). **Data:** Wang (2020) corruption investigations from Harvard Dataverse merged with China City Statistical Yearbook panel (262 cities, 2007–2016, 2,575 city-year observations). **Method:** Continuous-treatment difference-in-differences with city and year fixed effects; standard errors clustered at the city level. **Sample:** 258 prefecture-level cities with non-missing fiscal data; 253 treated (investigated at least once), 5 never-treated. Treatment intensity ranges from 1 to 260 investigated officials. SDE $= \hat{\beta} \times \text{SD}(X)/\text{SD}(Y)$ where SD(X) is the standard deviation of log-intensity among treated cities and SD(Y) is the pre-treatment standard deviation of the outcome. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).