# Incentives, Manipulation, and Growth: Performance Metrics and Bureaucratic Behavior in China

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**Abstract**

This paper study how promotion incentives shape both reported and real economic performance in Chinese prefectures. The design exploits the age rule that governs eligibility for promotion and estimates a sharp regression discontinuity at the threshold. To separate reporting from substance, We validate reported growth with night time lights and construct lights implied real growth. Leaders just below the threshold report higher GDP and also raise real activity. The discontinuity implies about a four percentage point increase in reported growth and a similar rise in real growth. Event study estimates around leadership turnover show these effects fade when incentives weaken. Firm level tests find higher total factor productivity and more entry where incentives are strong, with larger effects for collectively owned, foreign owned, and small or micro firms. Manipulation is concentrated among unconnected officials and in economically lagging places.

**JEL:** D73, P35, O29
**Keywords:** GDP Manipulation, Promotion incentive, Economic growth

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# 1 Introduction

Optimal incentive design in public bureaucracies faces a basic tension. Promotion systems that reward measured performance can raise effort and state capacity, yet imperfect monitoring creates scope for strategic manipulation. Tournament style promotions sharpen this trade off because officials compete for a limited number of slots based on observable metrics. In theory such tournaments can align individual incentives with organizational goals (Banerjee et al., 2012; Besley et al., 2022). In practice their success hinges on the measurability and verifiability of performance in complex states.

The incentive compatibility problem is most severe when evaluation uses self reported statistics that are costly to verify. In that environment the principal cannot write a contract that fully disciplines misreporting. Agents then choose how to allocate effort between real production and reporting management (Tirole, 1986; Laffont and Martimort, 2002). When expected manipulation costs are low relative to career benefits, rational agents may increase recorded output without proportional real activity. The prediction is not pure fabrication but a reallocation of effort across two margins.

China provides a useful setting to examine these predictions. Since the 1980s local officials have advanced largely on the basis of measured economic performance, with gross domestic product growth a central criterion Li and Zhou (2005). Information asymmetries between the center and localities limit verification. Mandatory retirement ages create an age cutoff rule that induces a discrete change in the return to measured performance. Officials just below the cutoff face stronger incentives to signal performance than officials just above the cutoff. This institutional feature allows us to test whether promotion pressure shifts effort toward reporting management, toward real production, or both.

The remainder of the paper is organized as follows. Section 2 discusses institutional background of China's high school education system and the ideological oirented curriculum reform. Section 3 describes the survey data I use and identification strategy. Section 4 shows empirical results of long term effect of curriculum reform on financial literacy and behavior. Section 5 shows robustness check of main results. Section 6 concludes.

# 2 Institutional Background

## 2.1 China's GDP statistics: constitution and critique

Since the initiation of reform and opening in 1978, China has experienced a period of rapid economic growth. Despite this widely recognized economic miracle, concerns remain over the accuracy and reliability of official gross domestic product statistics. Rawski (2001) shows that between 1997 and 2001 official data significantly overstated growth, with actual growth potentially as low as two percent. Using expenditure-side data from 1998 to 1999, Shiau (2005) similarly estimates that official growth may have been overstated by 3.4 to 4 percentage points. In related work, Wu (2007) and Perkins and Rawski (2008) highlight the coarseness and limitations of the statistical system. The incompleteness of China's national accounts framework and the hierarchical compilation structure are primary sources of these inconsistencies.

China's statistical system mirrors its five-tier administrative hierarchy—central, provincial, city, county, and township governments. The National Bureau of Statistics (NBS), a direct organ of the State Council, is responsible for organizing, coordinating, and leading national statistical work. Each level of local government maintains its own statistical bureau, overseen by the bureau at the higher administrative level. In addition, some central audit departments authorized by the NBS conduct data collection through their local branches. This nested system inherently limits the NBS's supervisory power over local statistical bureaus, leaving room for interference by local administrative leaders.

These institutional weaknesses create conditions for strategic data manipulation. Although the NBS directly supervises provincial bureaus and provides part of their funding, its authority over local officials remains weaker than that of provincial leaders. Local officials have strong incentives to report higher local GDP in order to improve their promotion prospects. In practice, key statistics—such as provincial GDP—must be approved by provincial leaders before being jointly audited by the NBS. As Holz (2005) documents, provincial governments control the appointment, removal, and transfer of key staff in provincial bureaus. When provincial leaders pressure bureaus to report higher growth figures, it is unrealistic for bureau staff to resist such demands, even if they violate statistical laws.

Opportunities for manipulation also vary across sectors. In the industrial sector, output data for enterprises above a designated size are collected and compiled by local authorities. Because value-added data for these enterprises are not further disaggregated, they are more vulnerable to numerical adjustments by local statistical bureaus (Ma et al., 2014). These structural limitations—combined with weak vertical oversight—undermine the accuracy and reliability of economic data and set the stage for the manipulation incentives explored in the following sections.

## 2.2 Promotion system and bureaucratic competition

Officials' private interests are closely tied to their career prospects. Promotion is a central incentive for local officials to supply effort to economic development. Because gross domestic product is a key input in evaluation and promotion, and because yardstick style relative evaluation is common (Besley and Case, 1992; Yao and Zhang, 2015), competition across jurisdictions is intense. In this environment officials may engage in reporting management, including strategic inflation of measured performance, to strengthen their political standing.

Since 1978 China has built a comprehensive cadre evaluation and promotion system. Compared with the early years of the People's Republic, when advancement often reflected merit based rewards or privileged personal ties, the post reform bureaucracy is more formalized and performance oriented. Party cadres are grouped into roughly ten ranks and are managed by corresponding party organs, with promotion decisions made by higher level authorities. In this study we focus on municipal officials, and references to higher level government denote the provincial tier.

In 1993 the Organization Department issued a nationwide policy for civil service evaluation. The policy lists four criteria: political integrity, competence, diligence, and practical achievements. Practical achievements account for more than sixty percent of the total weight and are typically proxied by local gross domestic product growth (Edin, 2003). In parallel, a policy of leadership rejuvenation favors younger cadres and limits opportunities for older ones. According to the *Temporary Provisions on the Term of Office and Age Limitations of Leading Cadres in Party and Government* and Kou and Tsai (2014), promotion age thresholds are displayed at Table 1

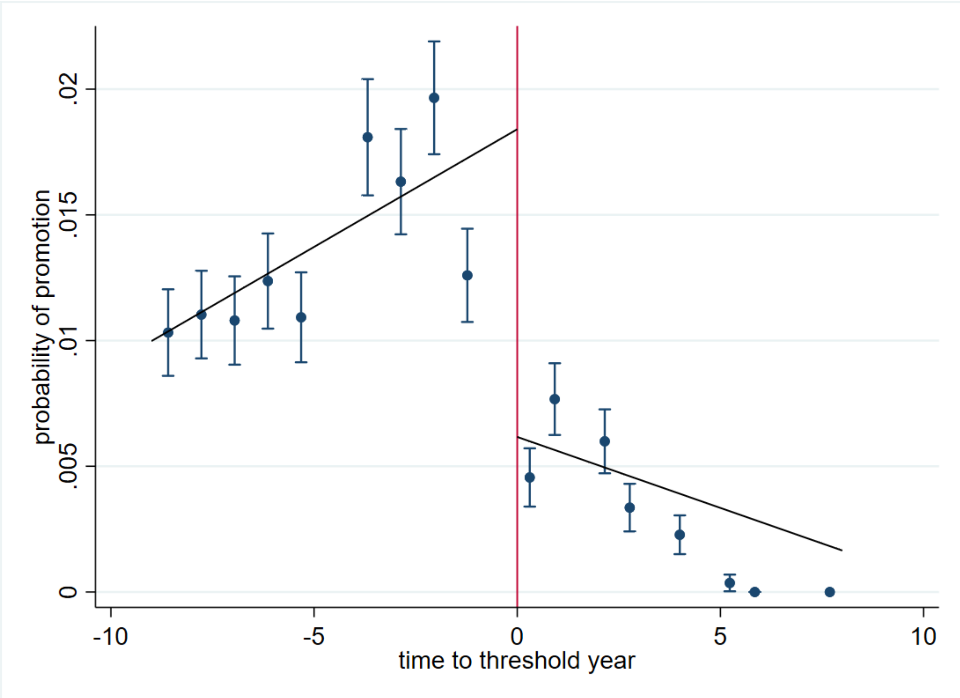
Table 1: Chinese officials' ranks and age limits for promotion

| Position rank | Promotion age threshold |
|---|---|
| Bureau director | $< 58$ |
| Deputy minister | $< 58$ |
| Minister | $< 63$ |
| Deputy state leader | $< 67$ |
| State leader | $< 67$ |

Notes. This table lists the maximum promotion age thresholds by rank in China.

These thresholds shape promotion probabilities, especially for deputy department level or bureau level officials at the municipal level. Incentives to demonstrate strong performance peak as officials approach the cutoff age, around 57. Failure to advance before the threshold sharply reduces the chance of further promotion and implies a lower pension at retirement. Appendix 3 reports the difference in promotion probabilities on the two sides of the threshold. Promotion incentives therefore vary with age: for younger officials higher measured performance yields larger expected returns, whereas for officials beyond the threshold the career return to additional effort is limited. Consistent with evidence on retirement related shirking among legislators in democracies (Smart and Sturm, 2013; Besley and Case, 2003), officials with diminished prospects may rationally reduce real effort and disengage from politically visible projects.

Figure 1: Promotion probability by age




Notes: This figure shows promotion probabilities across ages. The probability falls sharply after age 57.

Figure 1 illustrates the estimated probability of promotion as a function of years from the promotion age cutoff. The x-axis measures the number of years relative to the threshold (with zero marking the cutoff age, approximately 57), and the y-axis shows the probability of promotion. The fitted lines reveal a sharp discontinuity at the threshold: promotion probabilities rise steadily as officials approach the cutoff but decline precipitously once they pass it. This pattern reinforces the interpretation that age constraints sharply weaken promotion incentives after the threshold, consistent with our assumption that incentive intensity peaks just before the cutoff.

# 3 Data and descriptive statistics

## 3.1 Prefecture leader data

Every Chinese prefecture level city has two leading officials, the Municipal Party Secretary and the Mayor. By legal rules and political convention the Municipal Party Secretary is the top local leader (Li and Zhou, 2005). We assemble personal characteristics for Municipal Party Secretaries, including name, gender, age, education, tenure in office, and departure time. The source is the China Political Elites Database, which we manually cleaned. The database collects demographic and career information on more than four thousand leading officials at city, provincial, and national levels since the nineteen nineties (Jiang, 2018). We supplement missing values using official resumes.

Our panel covers one thousand three hundred twenty eight officials from 1994 to 2013. Women account for two point nine percent and ethnic minority officials account for eight point one percent. The average age is fifty one point nine four years and the average tenure in a post is two point eight two years. About seven point three five percent were born in the city where they serve. Promotions and demotions follow Chen and Kung (2019). The overall promotion probability is sixteen point four percent.

## 3.2 Night time lights data

Night time lights are recorded by the Defense Meteorological Satellite Program of the United States Air Force. We use the stable lights product from the National Oceanic and Atmospheric Administration for 1992 to 2013 and match it to Chinese administrative boundaries using the WGS 84 coordinate system. The data provide grayscale values that we aggregate to total luminosity for each prefecture year. A potential concern is the upper bound of the sensor, which caps the digital number at sixty three. For most developing economies this ceiling does not bind (Baum-Snow et al., 2017). In our data the highest value for Shanghai is fifty six point nine, which is well below the ceiling and therefore not saturated. After 2013 we also use the VIIRS series. We describe the cross calibration of the two series in the appendix and verify that it does not affect identification.

## 3.3 Other prefecture level variables

We compile city level socio economic variables from the China City Statistical Yearbooks. Targets for gross domestic product growth come from city or provincial government work reports. Due to data availability we exclude a small number of autonomous prefectures, leagues, and their subordinate cities. The final city panel covers two hundred fifty seven cities from 1994 to 2013.

## 3.4 Firm level data

To study the link between promotion incentives and real growth at a finer level we use two firm data sources. The first is the Annual Survey of Industrial Firms for 1998 to 2013, which includes all state owned industrial firms and non state firms with sales above five million Chinese Yuan. The National Bureau of Statistics conducts the annual survey. The second is the Industrial and Commercial Enterprise Registration Database from the State Administration for Industry and Commerce. For entry analysis we build a panel of prefecture cities by two digit manufacturing industries from 1998 to 2006. The dependent variable is the entry growth rate in a city industry cell, defined as new registrations divided by the stock of incumbent firms. The registration data are comprehensive and cover most registered firms in China. For consistency with the baseline we retain the 1994 to 2013 window where possible.

## 3.5 Summary statistics and intuitive result

Table 2 summarizes the main variables across three dimensions: prefecture leaders, city level outcomes, and firm level outcomes. The leader panel documents low female shares and limited ethnic minority representation. City level growth rates display substantial dispersion, and firm level moments show wide heterogeneity across enterprises.

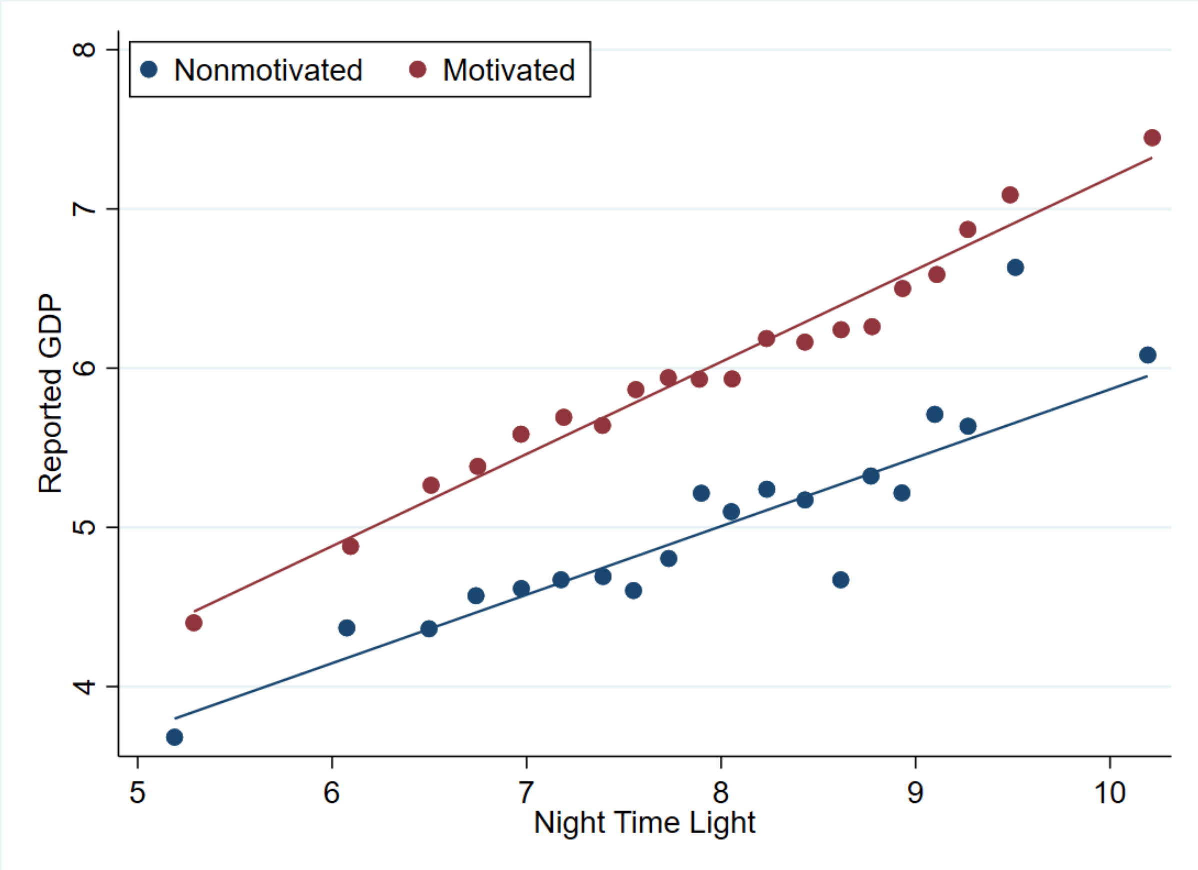
To provide intuitive evidence on incentives and reporting, Figure 2 plots annual gross domestic product growth against night time lights growth, split by whether the city leader faces promotion incentives. Leaders beyond the promotion age threshold are labeled non incentivized. Both groups exhibit a positive association between the two measures, but the slope is steeper for the incentivized group. For a given change in lights, incentivized cities report larger gross domestic product growth, which is consistent with stronger reporting management under promotion pressure.

Table 2: Descriptive Statistics

| Variables | Mean | SD | Min | Max |
|---|---|---|---|---|
| *Panel A: Prefecture Leaders* (N = 4,057) | | | | |
| Ethnicity | 0.064 | 0.245 | 0.000 | 1.000 |
| Female | 0.027 | 0.162 | 0.000 | 1.000 |
| Age | 50.710 | 3.996 | 37.000 | 61.000 |
| Connection | 0.574 | 0.495 | 0.000 | 1.000 |
| *Panel B: City-Level Variables* (N = 5,314) | | | | |
| Population | 5.783 | 0.690 | 2.434 | 8.110 |
| Investment | 11.910 | 1.913 | 3.850 | 16.570 |
| GDP Growth | 0.151 | 0.104 | -0.113 | 1.256 |
| NTL Growth | 0.063 | 0.110 | -0.654 | 1.324 |
| *Panel C: Firm-Level Variables* (N = 191,377) | | | | |
| Employment | 0.745 | 3.205 | 0.008 | 193.100 |
| Output | 0.187 | 1.310 | 0.000 | 161.000 |
| Assets | 0.227 | 1.584 | 0.000 | 135.800 |
| Profit | 0.012 | 0.515 | -6.898 | 119.000 |
| Asset Liability | 0.614 | 0.294 | 0.000 | 15.120 |
| TFP | 8.678 | 1.081 | 1.255 | 15.620 |

Notes. Panel A reports characteristics of Municipal Party Secretaries. Panel B reports city–year variables. Panel C reports firm–year variables. Values are rounded to three decimals. Sample sizes appear in parentheses. Variable definitions and units are documented in the appendix.

Figure 2: GDP growth and night time lights




Notes: This figure shows separate binned scatter plots of yearly growth in reported GDP and Night time light for- prefectures-years classified as motivated prefecture leaders and nonmotivated prefecture leaders. Classification based on the binary measure of prefecture leaders' age that whether it has been crossed the age threshold of promotion. Separate lines of best fit are displayed. The sample include 281 prefectures, time period is from 1993-2013.

# 4 Empirical results

## 4.1 Baseline estimation

### 4.1.1 Model setting

We quantify the share of over reporting by comparing official gross domestic product with a lights based proxy of real activity, following Martinez (2022). Let $y^e_{i,t}$ denote the unobserved real growth of prefecture $i$ at year $t$. Officials may face an incentive $a_{i,t} \in \{0,1\}$ that is one when the leader is below the promotion age threshold and zero otherwise. Decompose real growth into a baseline component $y_{i,t}$ and an incremental response to incentives:

$$y^e_{i,t} = y_{i,t} + \alpha\, a_{i,t}. \tag{1}$$

Measured gross domestic product $g_{i,t}$ is an unbiased but noisy measure of real growth:

$$g_{i,t} = y^e_{i,t} + \varepsilon_{i,t}. \tag{2}$$

If incentivized officials inflate reports, the released series $\hat{g}_{i,t}$ scales up $g_{i,t}$ by a factor that depends on incentives. Let $\sigma$ be the proportional manipulation:

$$\hat{g}_{i,t} = \left(1 + \sigma\, a_{i,t}\right) g_{i,t}. \tag{3}$$

Lights provide an external proxy that is not subject to local manipulation. Let $l_{i,t}$ be night time lights. Following Henderson et al. (2012) and Martinez (2022), allow lights to load on the baseline and the incentive component with possibly different elasticities:

$$l_{i,t} = \gamma\, y_{i,t} + \gamma^a \alpha\, a_{i,t} + u_{i,t}. \tag{4}$$

Combining eqs. (1) to (4) yields a relationship between the reported series and lights:

$$\hat{g}_{i,t} = \frac{1}{\gamma} l_{i,t} + \frac{\sigma}{\gamma}\left(l_{i,t} \times a_{i,t}\right) + \left[\lambda + \sigma\, \varepsilon_{i,t} - \frac{\sigma}{\gamma} u_{i,t}\right] a_{i,t} + \sigma \lambda\, a^2_{i,t} + v_{i,t}, \tag{5}$$

where $\lambda \equiv \left(1 - \gamma^a / \gamma\right)\alpha$ and $v_{i,t}$ collects higher order error terms. The coefficient on the interaction $l_{i,t} \times a_{i,t}$ equals $\sigma/\gamma$, while the coefficient on $l_{i,t}$ equals $1/\gamma$. Hence the manipulation share is identified as

$$\sigma = \frac{\mathrm{coef}[l_{i,t} \times a_{i,t}]}{\mathrm{coef}[l_{i,t}]}. \tag{6}$$

Add prefecture and year effects and an idiosyncratic error to obtain the estimating equation:

$$\hat{g}_{i,t} = \mu_i + \delta_t + \frac{1}{\gamma} l_{i,t} + \frac{\sigma}{\gamma}\left(l_{i,t} \times a_{i,t}\right) + \left[\lambda + \sigma\, \varepsilon_{i,t} - \frac{\sigma}{\gamma} u_{i,t}\right] a_{i,t} + \sigma \lambda\, a^2_{i,t} + \xi_{i,t}. \tag{7}$$

#### 4.1.2 Empirical results

We estimate a log linear specification that maps eq. (7) into observables. Let $\mathrm{Inc}_{i,t}$ be the binary incentive indicator defined by the age threshold. The baseline regression is

$$\ln \mathrm{GDP}_{i,t} = \mu_i + \delta_t + \phi_0 \ln \mathrm{NTL}_{i,t} + \phi_1 \mathrm{Inc}_{i,t} + \phi_2 \left(\ln \mathrm{NTL}_{i,t} \times \mathrm{Inc}_{i,t}\right) + \mathbf{Z}'_{i,t}\theta + \xi_{i,t}, \tag{8}$$

where $\mathbf{Z}_{i,t}$ includes controls as needed. Fixed effects absorb time invariant city factors and common year shocks. The manipulation share is estimated as

$$\hat{\sigma} = \frac{\hat{\phi}_2}{\hat{\phi}_0}. \tag{9}$$

Table 3 reports the results. Column one includes only lights and fixed effects and identifies the elasticity $\hat{\phi}_0$. Column two adds the incentive dummy and the interaction. Columns three and four absorb leader and province effects and include leader covariates such as local origin and superior connection. In the preferred specification, $\hat{\phi}_0 = 0.194$ and $\hat{\phi}_2 = 0.050$, which implies $\hat{\sigma} = 0.050/0.194 = 0.258$. Interpreting the ratio, an incentivized leader who reports a 10% growth is associated with a lights implied true growth of roughly 7.5%. A zero interaction would be consistent with no over reporting.

## 4.2 Regression discontinuity design

The baseline specification in Section 4.1 provided a parsimonious structural framework, following the spirit of Martinez (2022), to gauge the extent of local GDP manipulation induced by promotion incentives. Its main strength lies in simplicity: the ratio $\hat{\sigma} = \hat{\phi}_2/\hat{\phi}_0$ directly measures the degree of exaggeration. However, that approach imposes strong assumptions. It treats officials above the promotion age cutoff as fully unincentivized, ignores heterogeneity within groups, and assumes that the "control group" provides a clean counterfactual without any manipulation.

To relax these assumptions, we implement a regression discontinuity (RD) design that exploits the sharp age cutoff embedded in the cadre evaluation system. Unlike the baseline approach, the RD only requires continuity of potential outcomes at the cutoff. The running variable is the distance, in years, from the official's age to the promotion threshold. The outcome variable is the annual rate of GDP over-reporting—the gap between reported GDP growth and the lights-implied real growth rate—and, in a separate panel, the real GDP growth rate after decomposition.

Following Henderson et al. (2012) and Pinkovskiy and Sala-i Martin (2016), we construct the over-reporting rate as the percentage difference between reported and real GDP growth. For example, a prefecture reporting 10% growth when the lights-based proxy implies 8% true growth yields an over-reporting rate of 2%. Estimation uses local linear regressions with mean squared error (MSE) optimal bandwidth selection and robust bias correction as in Calonico et al. (2014), with robustness checks at fractions and multiples of the optimal bandwidth.

Table 3: Incentive gradient in the elasticity of GDP to NTL

| | (1) GDP | (2) GDP | (3) GDP | (4) GDP | (5) GDP |
|---|---|---|---|---|---|
| NTL | 0.169*** | 0.175*** | 0.183*** | 0.194*** | 0.198** |
| | [0.063] | [0.062] | [0.061] | [0.064] | [0.090] |
| Incentive | | -0.064** | -0.484*** | -0.442*** | -0.561*** |
| | | [0.029] | [0.139] | [0.142] | [0.146] |
| Log NTL × Incentive | | | 0.055*** | 0.050*** | 0.060*** |
| | | | [0.017] | [0.017] | [0.018] |
| Population | | | | 0.000*** | 0.000*** |
| | | | | [0.000] | [0.000] |
| Investment | | | | | 0.000 |
| | | | | | [0.000] |
| Prefecture fixed effects | Yes | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | Yes | Yes | Yes | Yes |
| Observations | 5,655 | 5,655 | 5,655 | 5,655 | 5,655 |
| Prefectures | 281 | 281 | 281 | 281 | 281 |
| $R^2$ | 0.932 | 0.932 | 0.932 | 0.931 | 0.971 |
| $\hat{\sigma} = \hat{\phi}_2 / \hat{\phi}_0$ | | | 0.303 | 0.258 | 0.306 |
| s.e. for $\hat{\sigma}$ | | | [0.137] | [0.122] | [0.165] |

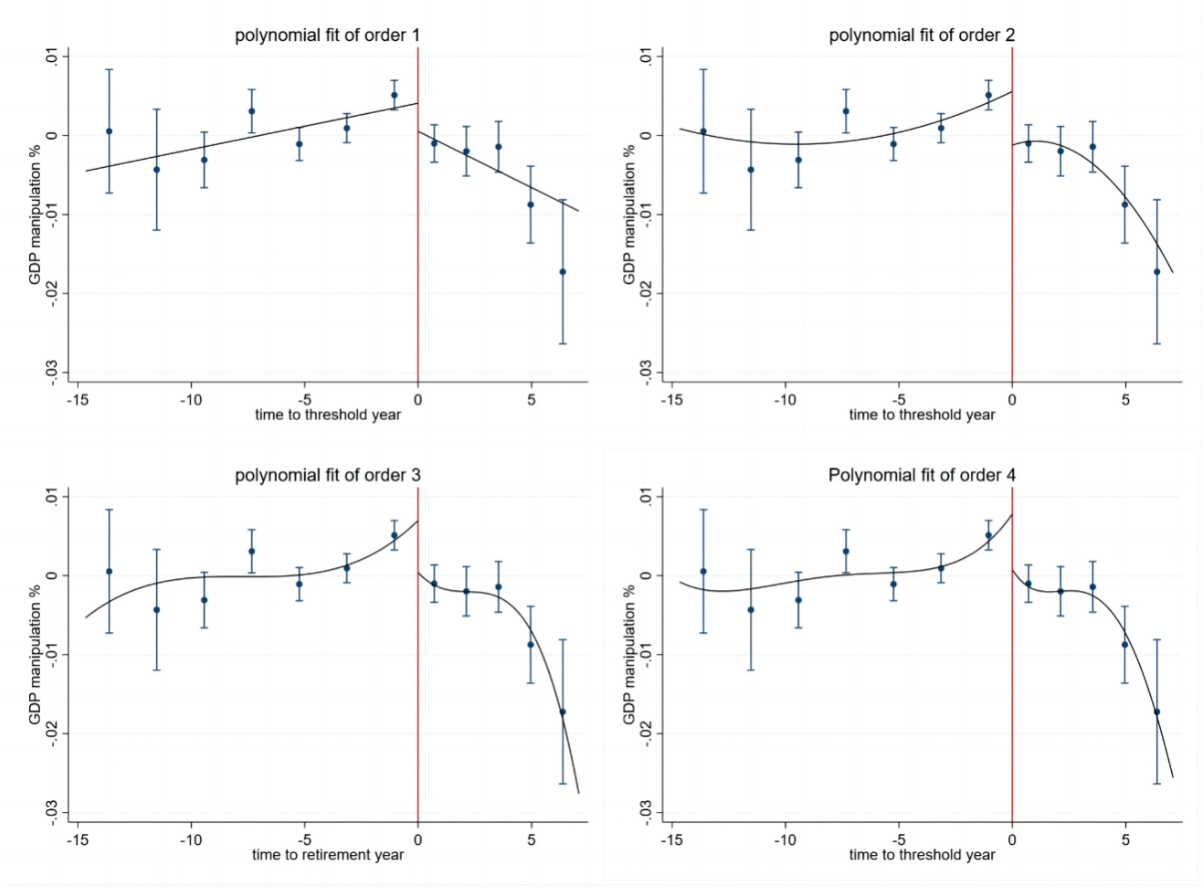
Notes: This table presents a structural estimation of the extent to which prefecture leaders with promotion incentives exaggerate reported GDP growth relative to leaders without such incentives, following the method of Martinez (2022). The dependent variable is the natural log of reported GDP. ln(NTL) is the natural logarithm of the average prefecture-level night-time light intensity (digital number). *Incentive* equals one if the prefecture leader is below the promotion age threshold of 57 years. All regressions include prefecture and year fixed effects. Robust standard errors clustered at the prefecture level are shown in brackets. The estimated value of $\hat{\sigma}$—the structural parameter capturing the proportional exaggeration of GDP growth—and its standard error are reported at the bottom of columns (3)–(5). Statistical significance is denoted as: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: RD estimates for real GDP growth and GDP manipulation

| | (1) Optimal bw | (2) 1/2 bw | (3) 3/4 bw | (4) 3/2 bw | (5) 2 bw |
|---|---|---|---|---|---|
| *Panel A: GDP growth manipulation* | | | | | |
| Estimate | -0.042** | -0.044** | -0.040* | -0.033* | -0.027* |
| s.e. | [0.020] | [0.022] | [0.021] | [0.017] | [0.014] |
| *Panel B: Real GDP growth* | | | | | |
| Estimate | -0.038** | -0.037* | -0.033* | -0.030* | -0.026* |
| s.e. | [0.018] | [0.019] | [0.019] | [0.016] | [0.013] |
| Prefecture FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| Observations | 2,934 | 2,934 | 2,934 | 2,934 | 2,934 |
| Prefectures | 281 | 281 | 281 | 281 | 281 |
| Bandwidth | 0.608 | 0.330 | 0.498 | 1.000 | 1.350 |
| Effective obs left | 42 | 17 | 33 | 94 | 130 |
| Effective obs right | 38 | 26 | 31 | 43 | 55 |

Notes. Each cell reports a separate RD regression. The running variable is the age distance to the promotion cutoff. Panel A outcomes are the annual GDP over-reporting rate, defined as the percentage gap between reported and lights-implied growth. Panel B outcomes are lights-implied real GDP growth rates. Estimation uses local linear regressions with robust bias correction and MSE-optimal bandwidth selection (Calonico et al., 2014); Columns (2)–(5) scale the optimal bandwidth for robustness. Standard errors clustered at the prefecture level are in brackets. Negative coefficients indicate that once officials pass the promotion age, manipulation intensity and real growth contributions decline. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure 3: Regression Discontinuity Plot of GDP Manipulation

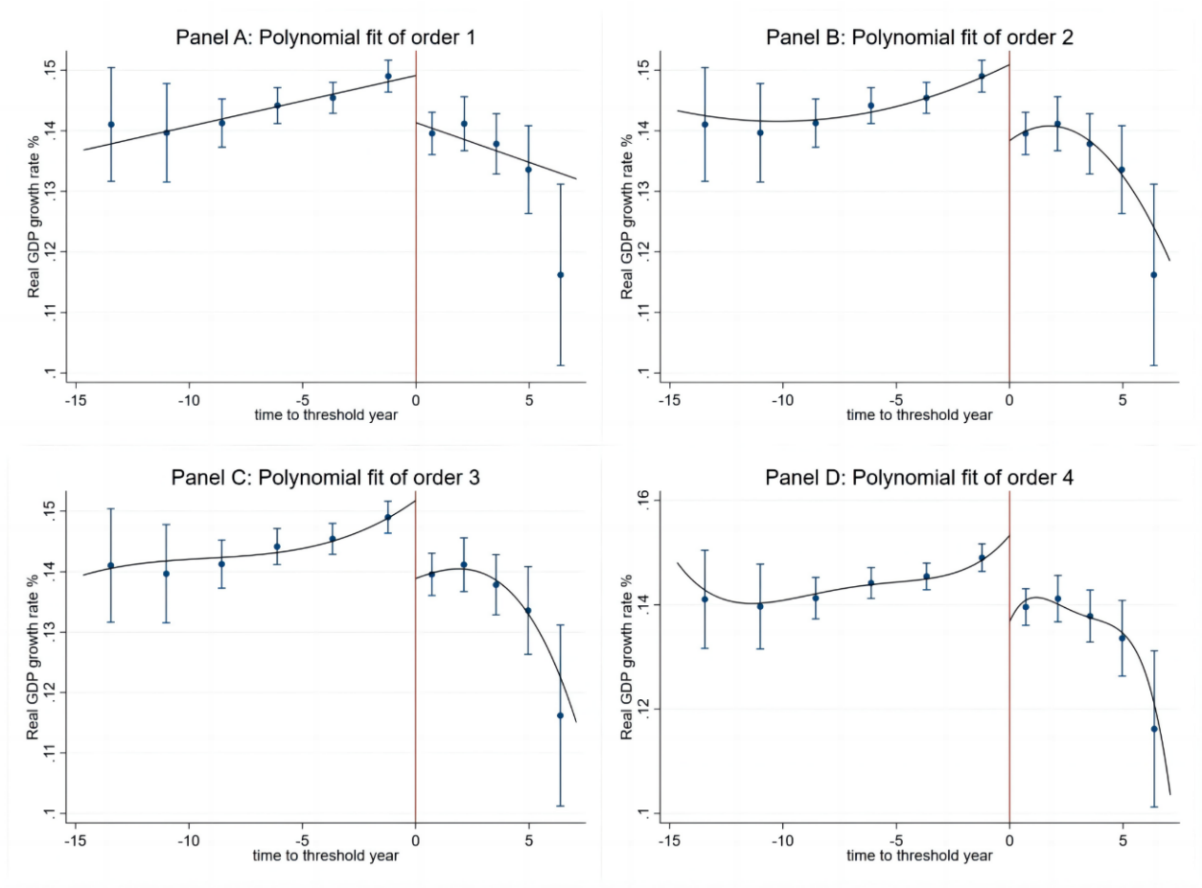



Notes: This figure shows the local linear regression fit of GDP over-reporting around the promotion age cutoff. The vertical dashed line indicates the age threshold for promotion eligibility. Points are binned local averages of the manipulation rate; fitted curves are estimated using MSE-optimal bandwidth with robust bias correction. Shaded areas represent 95% confidence intervals. Estimates correspond to Panel A of Table 4.

Panel A of Table 4 reports the estimated discontinuity in the manipulation rate. At the optimal bandwidth, officials facing promotion incentives exaggerate reported GDP growth by roughly 4.2 percentage points relative to those beyond the age cutoff. Panel B shows the effect on real growth: incentivized officials raise actual local growth by about 3.8 percentage points. Columns (2)–(5) confirm robustness under alternative bandwidth choices. Negative coefficients throughout indicate that once officials pass the promotion age, both manipulation intensity and real growth contributions decline sharply. These findings suggest that promotion incentives shape not only reported statistics but also real economic outcomes.

Figures 3 and 4 visualize the regression discontinuity estimates reported in Table 4. Figure 3 shows the discontinuity in the degree of GDP over-reporting around the promotion age threshold, while Figure 4 presents the corresponding discontinuity in real economic growth.

Figure 4: Regression Discontinuity Plot of Real GDP Growth




Notes: This figure shows the local linear regression fit of real GDP growth around the promotion age cutoff. The vertical dashed line marks the promotion threshold. Points are binned averages of the lights-implied growth rate; fitted curves are based on local linear regressions with MSE-optimal bandwidth and robust bias correction. Shaded areas denote 95% confidence intervals. Estimates correspond to Panel B of Table 4.

In Figure 3, there is a clear upward shift in reported GDP manipulation as officials approach the promotion cutoff, consistent with the estimates in Panel A of Table 4. This indicates that leaders nearing the threshold inflate GDP more aggressively, whereas officials beyond the cutoff show significantly lower manipulation.

Figure 4 illustrates a similar discontinuity in genuine economic performance. Local growth rates rise for officials close to the promotion age cutoff, while growth declines once the incentive fades. The magnitude of the discontinuity is comparable across the two figures, highlighting that promotion incentives simultaneously shape both reported statistics and underlying economic activity.

## 4.3 Dynamic impacts of prefecture leader turnover

The regression discontinuity results in Section 4.2 identify a local average treatment effect, showing that officials below the promotion age limit are more likely to inflate GDP and stimulate genuine economic growth. However, these effects are inherently local to the threshold neighborhood.

To capture dynamic adjustment over time, we construct an event study design based on turnover of prefecture-level leaders. The treatment group consists of cities where a political turnover replaces an incentivized leader (below 57) with a non-incentivized leader (above 57). The control group consists of cities experiencing turnover but maintaining incentivized leaders in place. This staggered difference-in-differences (DID) framework uses turnover as the shock, and estimates dynamic impacts on both exaggerated GDP growth and real GDP growth.

Figure 5 plots the event study coefficients. Panels A to D show results from alternative estimators, including Cengiz et al. (2019), De Chaisemartin and d'Haultfoeuille (2020), Sun and Abraham (2021), and Borusyak et al. (2024). Blue markers trace genuine GDP growth, while red markers trace GDP over-reporting. Coefficients prior to turnover are small and statistically insignificant, consistent with the parallel trends assumption. After turnover, coefficients turn negative and significant, indicating that replacing an incentivized leader with a non-incentivized one reduces both manipulation and real economic growth.

## 4.4 Mechanism analysis of officials' contribution to real economic growth

We study how promotion incentives affect real activity at the enterprise level. We focus on total factor productivity in manufacturing and estimate local effects using a regression discontinuity in the distance to the promotion age cutoff.

### 4.4.1 Total factor productivity of manufacturing enterprises

We compute firm level total factor productivity using the method of Levinsohn and Petrin (2003) on the Annual Survey of Industrial Firms. We then implement local linear RD where the running variable is the leader's age distance to the promotion threshold. We report three estimators for each outcome: conventional, bias corrected, and robust, and absorb year and firm fixed effects.

Table 5 reports RD estimates of log total factor productivity by ownership. Column (1) uses the full sample. Columns (2) to (5) split firms into state owned, collectively owned, privately owned, and foreign owned. Each panel reports three estimators in rows, namely

Table 5: RDD estimation for TFP by ownership type

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Variables | | | ln(TFP) | | |
| Firm type | Total firm | SOE | COE | PE | FOE |
| Conventional | -0.638** | -0.439 | -0.570*** | -0.038 | -1.522*** |
| | [0.275] | [0.659] | [0.211] | [0.250] | [0.300] |
| Bias corrected | -0.795*** | -0.443 | -0.638*** | -0.193 | -1.871*** |
| | [0.275] | [0.659] | [0.211] | [0.250] | [0.300] |
| Robust | -0.795** | -0.443 | -0.638** | -0.193 | -1.871*** |
| | [0.366] | [0.800] | [0.307] | [0.464] | [0.473] |
| Year fixed effects | Yes | Yes | Yes | Yes | Yes |
| Firm fixed effects | Yes | Yes | Yes | Yes | Yes |
| Observations | 127,234 | 15,023 | 19,942 | 67,310 | 24,170 |
| Bandwidth | 0.629 | 0.886 | 0.852 | 0.533 | 0.593 |
| Effective obs left | 2,186 | 406 | 700 | 619 | 337 |
| Effective obs right | 2,566 | 260 | 482 | 1,426 | 368 |

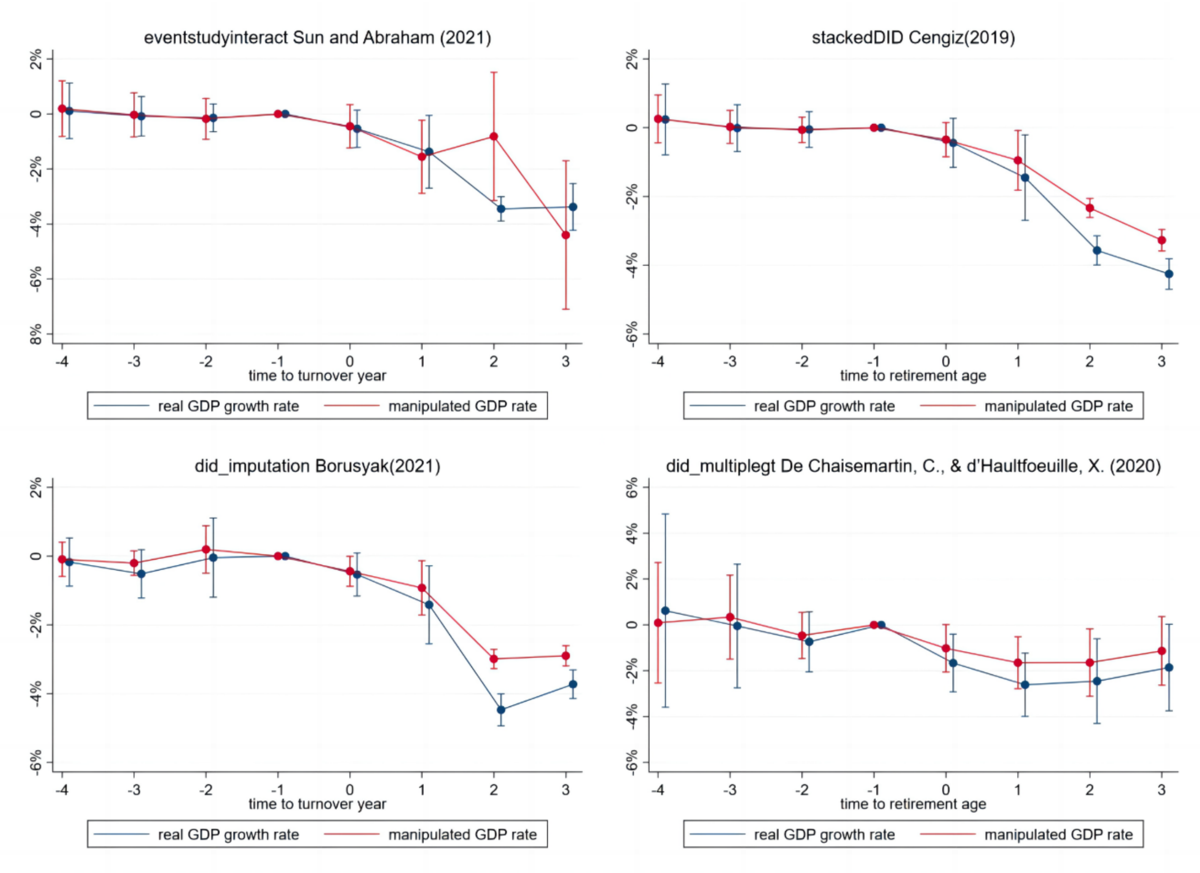
Notes. Outcome is the natural log of firm level total factor productivity estimated by Levinsohn and Petrin (2003). The running variable is the age distance to the promotion cutoff for the municipal leader. A negative coefficient indicates a drop at the cutoff, consistent with higher productivity under active promotion incentives. Estimation uses local linear RD with robust bias correction and MSE optimal bandwidth (Calonico et al., 2014). Year and firm fixed effects are included. Standard errors in brackets are clustered at the prefecture level. SOE is state owned, COE is collectively owned, PE is privately owned, FOE is foreign owned. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6: RDD estimation for TFP by enterprise size

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Firm scale | Total | Large | Medium | Small and micro |
| Variables | | | ln(TFP) | |
| Conventional | -0.638** | 0.165 | 0.185 | -0.635*** |
| | [0.275] | [0.812] | [0.256] | [0.214] |
| Bias corrected | -0.795*** | 0.000 | 0.071 | -0.753*** |
| | [0.275] | [0.812] | [0.256] | [0.214] |
| Robust | -0.795** | | | -0.753** |
| | [ ] | [1.071] | [0.461] | [ ] |
| Year fixed effects | Yes | Yes | Yes | Yes |
| Firm fixed effects | Yes | Yes | Yes | Yes |
| Observations | 127,234 | 7,782 | 55,827 | 63,625 |
| Bandwidth | 0.629 | 0.534 | 0.530 | 0.902 |
| Effective obs left | 2,186 | 80 | 445 | 1,543 |
| Effective obs right | 2,566 | 145 | 971 | 1,463 |

Notes. Outcome is the natural log of firm level total factor productivity estimated by Levinsohn and Petrin (2003). Firm size follows the definition in the national classification by employment and revenue. The running variable is the age distance to the promotion cutoff. Estimation uses local linear RD with robust bias correction and MSE optimal bandwidth (Calonico et al., 2014). Year and firm fixed effects are included. Standard errors in brackets are clustered at the prefecture level. In the robust row for Large and Medium columns, point estimates are not available in the current output; robust standard errors from the same routine are reported for completeness. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure 5: Dynamic effects of prefecture leader turnover on GDP manipulation and real growth




Notes: This figure illustrates the dynamic effects of prefecture leader turnover on the degree of GDP exaggeration and the contribution to real economic growth. The treatment group includes turnovers replacing incentivized leaders (aged under 57) with non-incentivized leaders (aged over 57); the control group includes turnovers where the successor remains incentivized. The x-axis represents years relative to turnover; the y-axis measures GDP manipulation (red) or genuine growth (blue). Panels A to D report staggered DID estimates from Cengiz et al. (2019), De Chaisemartin and d'Haultfoeuille (2020), Sun and Abraham (2021), and Borusyak et al. (2024). All models absorb prefecture and year fixed effects. Standard errors are clustered at the prefecture level.

conventional, bias corrected, and robust. All regressions include year and firm fixed effects.

At the promotion age cutoff the discontinuity is negative in the full sample, which implies higher productivity when leaders are incentivized. The pattern is concentrated among collectively owned and foreign owned firms, where the drops at the cutoff are large and statistically precise. For state owned and privately owned firms the point estimates are negative but not statistically different from zero, consistent with weaker direct influence of local leaders on centrally managed state firms and on private firms.

Table 6 examines heterogeneity by firm size based on the national classification by employment and revenue. The discontinuity is strongest for small and micro firms, and the full sample also shows a significant negative jump. Estimates for large and medium firms are close to zero. These results indicate that promotion incentives translate into productivity gains mainly where administrative support and local coordination matter most.

#### 4.4.2 Firm entry

New firms drive innovation and local growth. We study whether promotion incentives affect entry using the Industrial and Commercial Enterprise Registration Database. The outcome is the log number of newly registered firms in a prefecture year. The running variable is the distance in years from the leader's age to the promotion cutoff. Estimation follows the local linear RD with robust bias correction and MSE optimal bandwidth selection (Calonico et al., 2014).

Table 7 reports RD estimates for total entry and by sector. On the incentivized side of the cutoff cities experience more entry. Sector results show larger and statistically meaningful increases in catering, finance, and infrastructure. Other sectors are small and not statistically different from zero. The pattern is consistent with promotion pressure shifting administrative support toward consumer services and investment related activities that are malleable to authorities.

Table 7: RDD estimation for firm entry by sector

| | (1) Total | (2) Catering | (3) Finance | (4) Health | (5) Infrastructure |
|---|---|---|---|---|---|
| Conventional | -0.416 | -0.806** | -1.149*** | -0.168 | -1.282* |
| | [0.407] | [0.409] | [0.416] | [0.536] | [0.715] |
| Bias corrected | -0.353 | -0.846** | -1.288*** | -0.076 | -1.512** |
| | [0.407] | [0.409] | [0.416] | [0.536] | [0.715] |
| Robust | -0.353 | -0.846 | -1.288*** | -0.076 | -1.512* |
| | [0.477] | [0.580] | [0.494] | [0.622] | [0.814] |
| Observations | 4,442 | 4,442 | 4,442 | 4,442 | 4,442 |
| Bandwidth | 0.726 | 0.476 | 0.595 | 0.482 | 0.595 |
| | (6) Manufacturing | (7) Retail | (8) Transport | (9) Education | (10) Entertainment |
| Conventional | 0.048 | -0.612 | -0.235 | 0.196 | 0.292 |
| | [0.460] | [0.419] | [0.318] | [0.616] | [0.716] |
| Bias corrected | 0.004 | -0.523 | -0.129 | 0.254 | 0.737 |
| | [0.460] | [0.419] | [0.318] | [0.616] | [0.716] |
| Robust | 0.004 | -0.523 | -0.129 | 0.254 | 0.737 |
| | [0.547] | [0.479] | [0.417] | [0.675] | [1.169] |
| Observations | 4,442 | 4,442 | 4,442 | 4,442 | 4,442 |
| Bandwidth | 0.707 | 0.694 | 0.557 | 0.772 | 0.374 |

Notes. Outcome is the natural log of newly registered firms in a prefecture year from the registration database. The running variable is the distance in years from the leader's age to the promotion cutoff. Each cell reports the discontinuity from a local linear RD with robust bias correction and MSE optimal bandwidth (Calonico et al., 2014). Sectors follow the national industry classification. Standard errors in brackets are clustered at the prefecture level. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

# 5 Further Analysis

## 5.1 Heterogeneity analysis of officials' exaggeration of GDP

We study who inflates local gross domestic product. First, we split leaders by connections to provincial superiors and re estimate the baseline specification in Martinez (2022). Officials without connections display a larger elasticity wedge and a positive interaction between lights and incentives, consistent with stronger manipulation. For connected officials the interaction is small and not statistically different from zero, suggesting weaker gains from reporting management.

Second, we split cities by economic development. We classify a city as developed if its gross domestic product exceeds the national or provincial median in the same year. Manipulation concentrates in lagging areas, where genuine growth is harder to produce and promotion pressure is stronger.

Table 8: Heterogeneity in baseline manipulation estimates

| | (1)<br>With<br>Connections | (2)<br>Without<br>Connections | (3)<br>Top 50 %<br>in China | (4)<br>Last 50 %<br>in China | (5)<br>Top 50 %<br>in province | (6)<br>Last 50 %<br>in province |
|---|---|---|---|---|---|---|
| NTL | 0.224** | 0.132* | 0.180* | 0.137* | 0.192** | 0.164** |
| | [0.097] | [0.072] | [0.094] | [0.075] | [0.091] | [0.080] |
| Incentive | -0.131 | -0.389** | -0.397* | -0.403** | -0.200 | -0.712*** |
| | [0.240] | [0.170] | [0.237] | [0.167] | [0.201] | [0.199] |
| Log NTL × Incentive | 0.009 | 0.043* | 0.040 | 0.047** | 0.016 | 0.088*** |
| | [0.027] | [0.022] | [0.029] | [0.022] | [0.023] | [0.026] |
| Prefecture fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 3,265 | 2,390 | 3,107 | 2,548 | 2,944 | 2,711 |
| $R^2$ | 0.948 | 0.925 | 0.948 | 0.871 | 0.940 | 0.903 |
| Prefectures | 270 | 266 | 155 | 126 | 148 | 133 |
| $\hat{\sigma} = \hat{\phi}_2/\hat{\phi}_0$ | 0.040 | 0.326 | 0.221 | 0.345 | 0.086 | 0.538 |
| s.e. for $\hat{\sigma}$ [a] | [0.122] | [0.244] | [0.199] | [0.247] | [0.126] | [0.306] |

Notes. Dependent variable is ln(GDP). ln(NTL) is the log of average prefecture night time lights. *Incentive* equals one if the leader is below the promotion age threshold. Estimates follow the baseline in Martinez (2022). Standard errors in brackets are clustered at the prefecture level. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Connections may also shape assignment. We test whether connected leaders are placed in better cities. The dependent variable is the within province rank of city gross domestic product. The key regressor is the connection indicator in the spirit of Francois et al. (2023). Coefficients are negative across specifications, consistent with connected officials serving in stronger locations.

Table 9: Connections and placement within province

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Dependent variable | GDP rank | GDP rank | GDP rank | GDP rank | GDP rank |
| Connection | -48.737*** | -57.379*** | -16.327*** | -1.887** | -19.842*** |
| | [2.686] | [3.212] | [1.513] | [0.836] | [1.866] |
| Prefecture fixed effects | No | Yes | No | No | Yes |
| Year fixed effects | No | No | Yes | No | Yes |
| Official fixed effects | No | No | No | Yes | No |
| Observations | 5,655 | 5,655 | 5,655 | 5,655 | 5,655 |
| Prefectures | 281 | 281 | 281 | 281 | 281 |
| $R^2$ | 0.126 | 0.224 | 0.486 | 0.769 | 0.566 |

Notes. Dependent variable is the within province rank of city gross domestic product. *Connection* equals one if the prefecture leader shares hometown, school, work history, or faction with the provincial leader, following Francois et al. (2023). Standard errors in brackets are clustered at the prefecture level. Lower ranks denote stronger economies, so negative coefficients imply assignment to better cities. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Finally, we re visit the regression discontinuity by development tier. Manipulation and real growth both drop after the cutoff in lagging cities. In developed cities the discontinuities are small and imprecise.

## 5.2 Robustness checks

This section probes the sensitivity of our results along three dimensions that are standard in the regression discontinuity literature: placebo cutoffs, continuity of covariates at the threshold, and bandwidth choice. We also repeat these exercises for the firm productivity outcomes used in the mechanism analysis.

Table 11 shifts the promotion cutoff to placebo ages and re estimates the discontinuities. Only the institutional threshold delivers a statistically and economically meaningful jump. At placebo ages the estimates are close to zero and imprecise for both manipulation and real

Table 10: Heterogeneity in RD estimates by development tier

| | (1) Top 50 percent in China | (2) Last 50 percent in China | (3) Top 50 percent in province | (4) Last 50 percent in province |
|---|---|---|---|---|
| GDP growth manipulation | -0.026* | -0.101** | -0.023 | -0.127*** |
| | [0.016] | [0.041] | [0.015] | [0.033] |
| Real GDP growth | -0.027 | -0.084** | -0.022 | -0.112*** |
| | [0.016] | [0.039] | [0.016] | [0.031] |
| Prefecture fixed effects | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | Yes | Yes | Yes |
| Observations | 1,626 | 1,185 | 1,501 | 1,310 |
| Bandwidth | 0.703 | 0.621 | 0.823 | 0.632 |

Notes. Each cell reports a local linear regression discontinuity with robust bias correction and MSE optimal bandwidth (Calonico et al., 2014). The running variable is the age distance to the promotion cutoff. Outcomes are the annual manipulation rate and the lights implied real growth rate. Development tiers split cities by the national or provincial median in the same year. Standard errors in brackets are clustered at the prefecture level. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 11: Cutoff placebo tests for RD on GDP manipulation and real growth

| | (1) Cutoff age 57 | (2) 58 | (3) 56 | (4) 55 | (5) 54 |
|---|---|---|---|---|---|
| GDP growth manipulation | -0.042** | 0.002 | 0.008 | 0.020 | -0.019 |
| | [0.020] | [0.016] | [0.019] | [0.022] | [0.014] |
| Real GDP growth | -0.038** | 0.003 | 0.005 | 0.017 | -0.014 |
| | [0.018] | [0.015] | [0.018] | [0.021] | [0.013] |
| Prefecture fixed effects | Yes | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | Yes | Yes | Yes | Yes |
| Observations | 2,934 | 2,934 | 2,934 | 2,934 | 2,934 |
| Bandwidth | 0.608 | 0.665 | 0.665 | 0.665 | 0.665 |

Notes. Each column reports a local linear RD with robust bias correction and mean squared error optimal bandwidth (Calonico et al., 2014). The running variable is the age distance to the stated cutoff. Outcomes are the annual manipulation rate and the lights implied real growth rate constructed in the spirit of Henderson et al. (2012). Standard errors in brackets are clustered at the prefecture level. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

growth. This pattern is consistent with a discrete change in incentives at the institutional rule rather than a smooth age trend.

Table 12: Covariate continuity at the cutoff for RD

| | (1) Ethnicity | (2) Connection | (3) Male | (4) Investment | (5) Population |
|---|---|---|---|---|---|
| Conventional | -0.010 | -0.017 | 0.000 | -156.096 | 0.696 |
| | [0.169] | [0.026] | [0.000] | [152.515] | [68.899] |
| Bias corrected | -0.004 | -0.020 | -0.001 | -166.549 | 1.805 |
| | [0.169] | [0.026] | [0.000] | [152.515] | [68.899] |
| Robust | -0.004 | -0.020 | -0.001 | -166.549 | 1.805 |
| | [0.191] | [0.038] | [0.001] | [160.629] | [82.345] |
| Observations | 2,934 | 2,934 | 2,934 | 2,934 | 2,934 |
| Bandwidth | 0.570 | 0.650 | 1.820 | 0.446 | 1.383 |

Notes. Each cell is a separate local linear RD with robust bias correction and mean squared error optimal bandwidth (Calonico et al., 2014). The running variable is the age distance to the promotion cutoff. Covariates are leader ethnicity, connection to provincial leaders, gender, prefecture investment, and population. Standard errors in brackets are clustered at the prefecture level.

Table 12 tests whether observables change discretely at the threshold. We examine leader ethnicity, connection status, gender, and two city covariates, investment and population. Point estimates are small and not statistically different from zero across estimators, supporting the identifying assumption that potential outcomes are continuous at the cutoff.

Table 13 varies the bandwidth around the optimal value and reports conventional, bias corrected, and robust RD estimators for log TFP. Our preferred specification uses the mean squared error optimal bandwidth with robust bias correction and yields a negative and significant discontinuity in productivity when promotion incentives fade. Estimates at very small or very large bandwidths move as expected with the bias variance trade off but do not overturn the main conclusion.

Table 14 repeats the placebo exercise for TFP. The largest and most precisely estimated effect occurs at the institutional threshold. Estimates at placebo ages attenuate and lose precision, which is consistent with an incentive driven discontinuity at the true rule.

Table 15 examines asset liability ratio, return on assets, and assets per worker controlled for table 14 . We do not find systematic discontinuities at the cutoff. The magnitudes are economically small, and the estimates do not display a consistent pattern across variables,

Table 13: Bandwidth robustness for RD on firm total factor productivity

| | (1) One half bw | (2) Three fourths bw | (3) Optimal bw | (4) Three halves bw | (5) Two bw |
|---|---|---|---|---|---|
| Conventional | -1.307*** | 0.234 | -0.638** | -0.521*** | -0.270* |
| | [0.188] | [0.468] | [0.275] | [0.165] | [0.160] |
| Bias corrected | 1.272*** | 0.129 | -0.795*** | -0.812*** | -0.704*** |
| | [0.188] | [0.468] | [0.275] | [0.165] | [0.160] |
| Robust | 1.272*** | 0.129 | -0.795** | -0.812*** | -0.704*** |
| | [0.103] | [0.642] | [0.366] | [0.289] | [0.271] |
| Year fixed effects | Yes | Yes | Yes | Yes | Yes |
| Firm fixed effects | Yes | Yes | Yes | Yes | Yes |
| Observations | 127,234 | 124,360 | 127,234 | 127,234 | 124,357 |
| Bandwidth | 0.330 | 0.470 | 0.629 | 0.940 | 1.140 |

Notes. Outcome is log firm total factor productivity estimated by Levinsohn and Petrin (2003). Local linear RD with robust bias correction and bandwidth choices as labeled (Calonico et al., 2014). Standard errors in brackets are clustered at the prefecture level.

Table 14: Cutoff placebo tests for RD on firm total factor productivity

| | (1) 57 | (2) 58 | (3) 56 | (4) 55 | (5) 54 |
|---|---|---|---|---|---|
| Conventional | -0.415*** | -0.638** | 0.098 | 0.149 | 0.090 |
| | [0.084] | [0.275] | [0.141] | [0.098] | [0.089] |
| Bias corrected | -0.428*** | -0.795*** | 0.131 | 0.146 | 0.077 |
| | [0.084] | [0.275] | [0.141] | [0.098] | [0.089] |
| Robust | -0.428*** | -0.795** | 0.131 | 0.146 | 0.077 |
| | [0.071] | [0.366] | [0.179] | [0.107] | [0.100] |
| Year fixed effects | Yes | Yes | Yes | Yes | Yes |
| Firm fixed effects | Yes | Yes | Yes | Yes | Yes |
| Observations | 127,234 | 127,234 | 127,234 | 127,234 | 127,234 |
| Bandwidth | 0.629 | 0.629 | 0.629 | 0.629 | 0.629 |

Notes. Each column reports a local linear RD with robust bias correction at the stated cutoff age. Outcome is log firm total factor productivity from the Levinsohn and Petrin (2003) method. Standard errors in brackets are clustered at the prefecture level. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 15: Covariate continuity at the cutoff for RD on firm productivity

| | (1) Asset liability ratio | (2) Return on assets | (3) Assets per worker |
|---|---|---|---|
| Conventional | -0.000 | -0.109 | 0.000 |
| | [0.045] | [0.117] | [0.000] |
| Bias corrected | 0.008 | -0.165 | 0.000 |
| | [0.045] | [0.117] | [0.000] |
| Robust | 0.008 | -0.165* | 0.000 |
| | [0.065] | [0.095] | [0.000] |
| Observations | 173,146 | 173,146 | 173,146 |
| Bandwidth | 0.932 | 1.483 | 0.318 |

Notes. Each column is a local linear RD with robust bias correction and mean squared error optimal bandwidth (Calonico et al., 2014). The running variable is the age distance to the promotion cutoff. Variables are defined as follows: Asset liability ratio equals total liabilities over total assets; Return on assets equals net profit over total assets; Assets per worker equals total assets over employment. Standard errors in brackets are clustered at the prefecture level.

which supports the view that composition around the threshold is stable.

# 6 Conclusion

This paper studies how promotion incentives shape both reported and real economic performance in Chinese prefectures. Using a regression discontinuity around the age rule for promotion eligibility, linked to an external validation based on night time lights, we document a clear pattern. Leaders just below the eligibility cutoff report higher gross domestic product than comparable leaders just above the cutoff, and they also raise real activity. The implied over reporting is on the order of four percent, and the real growth response is of similar magnitude.

The evidence points to a trade off at the core of the promotion tournament. When monitoring is imperfect, incentives induce both genuine effort and strategic reporting. We show this dual response in three ways. First, city level discontinuities appear for both manipulation and real growth. Second, dynamics around leadership turnover confirm that effects emerge at the time of incentive changes rather than as smooth trends. Third, mechanisms at the firm level indicate higher total factor productivity and more entry when incentives are strong, especially among collectively owned and foreign owned firms and among small and micro firms.

Heterogeneity supports an information and access channel. Officials without connections exaggerate more, and manipulation concentrates in economically lagging places where genuine growth is harder to produce quickly. Connected leaders are more often placed in stronger locations, which helps explain weaker manipulation on their side.

The paper contributes by providing causal evidence that links a concrete rule in the cadre system to both misreporting and real outcomes, by quantifying the wedge between reported and lights implied growth, and by tracing firm level channels. Methodologically, the design combines a sharp institutional discontinuity with external measurement to separate manipulation from real activity.

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# A Appendix

## A.1 Appendix A. Recovering real growth and the manipulation measure

We combine reported gross domestic product growth with lights implied growth to recover actual growth and the manipulation wedge (Henderson et al., 2012).

**Measurement equations.** Let $y_{it}$ denote unobserved true growth in city $i$ year $t$. Let $\hat{g}_{it}$ be reported growth and $l_{it}$ the lights index. Assume

$$\hat{g}_{it} = y_{it} + \varepsilon^{g}_{it}, \tag{A1}$$

$$l_{it} = \theta\, y_{it} + \varepsilon^{\ell}_{it}, \tag{A2}$$

with $\mathbb{E}[\varepsilon^{g}_{it}] = \mathbb{E}[\varepsilon^{\ell}_{it}] = 0$. Estimate $\hat{\theta}$ from a regression of $l_{it}$ on $\hat{g}_{it}$ or from lights on income in external data, and form the lights implied growth

$$\hat{g}^{L}_{it} \equiv \hat{\theta}^{-1} l_{it}. \tag{A3}$$

**Minimum mean squared error combination.** Construct

$$\hat{y}_{it}(\lambda) \equiv \lambda\, \hat{g}_{it} + (1-\lambda)\, \hat{g}^{L}_{it}. \tag{A4}$$

Let $\sigma^2_g \equiv \mathrm{Var}(\varepsilon^{g}_{it})$, $\sigma^2_\ell \equiv \mathrm{Var}(\varepsilon^{\ell}_{it})$, and $\rho \equiv \mathrm{Corr}(\varepsilon^{g}_{it}, \varepsilon^{\ell}_{it})$. The mean squared error is

$$\mathrm{MSE}(\lambda) = \lambda^2 \sigma^2_g + (1-\lambda)^2 \frac{\sigma^2_\ell}{\theta^2} - 2\lambda(1-\lambda)\frac{\rho\,\sigma_g \sigma_\ell}{\theta}. \tag{A5}$$

The optimal weight is

$$\lambda^* = \frac{\dfrac{\sigma^2_\ell}{\theta^2} - \dfrac{\rho\,\sigma_g\sigma_\ell}{\theta}}{\sigma^2_g + \dfrac{\sigma^2_\ell}{\theta^2} - 2\dfrac{\rho\,\sigma_g\sigma_\ell}{\theta}} = \frac{1-\rho\sqrt{\phi}}{1+\phi-2\rho\sqrt{\phi}}, \qquad \phi \equiv \frac{\sigma^2_g}{\sigma^2_\ell/\theta^2}. \tag{A6}$$

A common benchmark assumes independent errors, $\rho = 0$, which gives $\lambda^* = 1/(1+\phi)$. Following Henderson et al. (2012), we set $\phi = 0.594$ in the baseline and later vary it in robustness checks.

**Constructed outcomes.** Define the lights validated real growth and the manipulation wedge as

$$\widehat{\mathrm{RealGrowth}}_{it} \equiv \hat{y}_{it}(\lambda^*), \tag{A7}$$

$$\widehat{\mathrm{Manipulation}}_{it} \equiv \hat{g}_{it} - \hat{y}_{it}(\lambda^*). \tag{A8}$$

These variables are used as outcomes in the regression discontinuity and event study analyses.

## A.2 Appendix B. Additional heterogeneity tables

Table A1: Heterogeneity in RD estimates for GDP manipulation

| | (1)<br>Top 50%<br>in China | (2)<br>Last 50%<br>in China | (3)<br>Top 50%<br>in province | (4)<br>Last 50%<br>in province |
|---|---|---|---|---|
| Conventional | -0.026* | -0.101** | -0.023 | -0.127*** |
| | [0.016] | [0.041] | [0.015] | [0.033] |
| Bias corrected | -0.024 | -0.110*** | -0.019 | -0.143*** |
| | [0.016] | [0.041] | [0.015] | [0.033] |
| Robust | -0.024 | -0.110* | -0.019 | -0.143*** |
| | [0.019] | [0.056] | [0.017] | [0.047] |
| Prefecture fixed effects | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | Yes | Yes | Yes |
| Observations | 1,626 | 1,185 | 1,501 | 1,310 |
| Bandwidth | 0.703 | 0.621 | 0.823 | 0.632 |

Notes. Each cell reports a local linear regression discontinuity with robust bias correction and mean squared error optimal bandwidth (Calonico et al., 2014). The running variable is the age distance to the promotion cutoff. Outcome is the annual manipulation rate. Cities are split by the national or provincial median of gross domestic product in the same year. Standard errors in brackets are clustered at the prefecture level. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A2: Heterogeneity in RD estimates for real GDP growth

| | (1) Top 50 percent in China | (2) Last 50 percent in China | (3) Top 50 percent in province | (4) Last 50 percent in province |
|---|---|---|---|---|
| Conventional | -0.027 | -0.084** | -0.022 | -0.112*** |
| | [0.016] | [0.039] | [0.016] | [0.031] |
| Bias corrected | -0.014 | -0.113*** | -0.012 | -0.121*** |
| | [0.016] | [0.039] | [0.016] | [0.031] |
| Robust | -0.014 | -0.113*** | -0.012 | -0.121*** |
| | [0.021] | [0.027] | [0.019] | [0.025] |
| Prefecture fixed effects | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | Yes | Yes | Yes |
| Observations | 1,626 | 1,185 | 1,501 | 1,310 |
| Bandwidth | 0.703 | 0.621 | 0.823 | 0.632 |

Notes. Each cell reports a local linear regression discontinuity with robust bias correction and mean squared error optimal bandwidth (Calonico et al., 2014). The running variable is the age distance to the promotion cutoff. Outcome is the lights validated real growth rate. Cities are split by the national or provincial median of gross domestic product in the same year. Standard errors in brackets are clustered at the prefecture level. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.