Journal of Economic Behavior and Organization 189 (2021) 657–671

Contents lists available at ScienceDirect

Journal of Economic Behavior and Organization

journal homepage: www.elsevier.com/locate/jebo

Chasing or cheating? Theory and evidence on China's GDP manipulation

Shuo Chen[a], Xue Qiao[b,*], Zhitao Zhu[c]

[a] School of Economics, Fudan University, Shanghai, China
[b] School of Economics, Renmin University of China, Beijing, China
[c] Department of Economics, Chinese University of Hong Kong, Hong Kong

ARTICLE INFO

Article history:
Received 15 September 2020
Revised 2 June 2021
Accepted 6 July 2021

JEL:
D73
H11
P26

Keywords:
GDP manipulation
Promotion incentive
Political turnover
Accountability

ABSTRACT

This paper studies the effect of promotion incentive on county officials' GDP manipulation in China, both theoretically and empirically. We employ satellite nightlight data to estimate a proxy for GDP manipulation. Our difference-in-difference estimation explores the variation in political turnovers across counties during the local party congresses between 2006 and 2007. We find that county officials who are in the early period of their tenure and thus expect a higher chance of being promoted, undertake more cheating effort to manipulate GDP than those in the later period. Furthermore, we find that the impact of promotion incentive on GDP manipulation is smaller in counties where local officials are subject to stronger accountability, either from their superiors or from the grassroots. These results are not only consistent with the model predictions, but also state that the lauded merit-based promotion system in China could lead to systematic GDP manipulation, which may induce information distortion problem.

1. Introduction

China's GDP statistics have long been plagued by data manipulation. There are considerable anecdotal evidences showing that local officials falsify GDP data, and the "Keqiang Index" has become a popular alternative statistic used to complement the official GDP.[1] The recent news that three provinces (Liaoning, Inner Mongolia, and Tianjin) publicly admitted falsifying their fiscal and industrial data has confirmed that such problems still persist.[2] This paper tries to explore China's data manipulation behavior by connecting it to the merit-based promotion system.

China's merit-based promotion system has been recognized as an essential reason behind its remarkable economic growth. The logic is that local government officials had done a great deal of effort to improving local economy growth (Maskin et al., 2000; Xu, 2011; Bai et al., 2020), because they believe that stronger economic performance improves their chance of being promoted (Li and Zhou, 2005; Jia et al., 2015; Chen and Kung 2019). Following this logic, one cannot help wonder why officials would not cheat to increase the local economic performance to get themselves promoted more likely. Indeed, the aim of this paper is to validate such concern and identify the systematic pattern of the GDP manipulation at the county level.

* Corresponding author.
E-mail addresses: cs@fudan.edu.cn (S. Chen), qiaoxue@ruc.edu.cn (X. Qiao), zhuzhitao@link.cuhk.edu.hk (Z. Zhu).

[1] Anecdotal evidence and case studies demonstrate that economic data are manipulated in certain areas of China (Cao 2000).

[2] *Financial Times*, "China Fake Data Mask Economic Rebound." Jan 16, 2018.

https://doi.org/10.1016/j.jebo.2021.07.010
0167-2681/

In the paper, we first build a simple model to examine how promotion incentive affects official's effort decisions. Officials have two types of effort: real effort (which we refer to as "chasing effort") and cheating effort (i.e., data manipulation). Chasing effort improves the true performance of the economy, but it is costly. Cheating effort increases the reported economic statistics only, but there comes a danger to the official in term of utility loss if such behavior is caught. The model distinguishes between two types of officials: those in their first term (which we call "new" officials) and those in their later term ("old" officials). Based on existing evidence, we assume that "new" officials expect a higher likelihood of being promoted than "old" officials (Chen and Kung, 2016; Jia et al., 2015; Opper, Nee and Brehm, 2015). Our model predicts that "new" officials engage in both higher chasing effort and higher cheating effort than their "old" counterparts.

We then conduct a difference-in-differences (DID) analysis to test the predictions. To measure GDP manipulation, we first use satellite nightlight data to estimate the "true" GDP by follow the method in Henderson et al. (2012).[3] GDP manipulation is then calculated as the difference between official GDP and estimated "true" GDP. To distinguish between "new" and "old" officials, we make use of the large-scale political turnovers generated at the local Party Congress between 2006 and 2007, during which the number of political turnovers generated is the highest among all sample periods. Based on whether there was a change in the political leadership in the local party congresses, counties are divided into two groups: "new" official group-the *treatment* group, and "old" official group-the *control* group.

Our baseline estimation result supports the model prediction. In particular, we find that GDP manipulation is higher in the treatment group than in the control group, that is, stronger promotion incentives lead to more cheating effort.[4] We then explore which kind of officials is more inclined to cheat from the perspective of accountability, local information advantage and opportunity cost. Our findings show that the impact is smaller for officials subject to strong accountability, regardless whether it is from their superiors or from the grassroots. However, we do not find significant effect for local information advantage and opportunity costs. All these results remain robust when concerns of omitted variable and measurement errors are taken into account.

This paper is closely related to the political economy literature that studies how officials respond to various incentives. Studies on democratic regimes found that electoral incentive leads to cycles on police force, employment, dropout rates and so on (Levitt, 1997; Burgess et al., 2012; Janvry, Finan and Sadoulet.2012; Labonne, 2016). There is also a strand of literature that argues high-powered incentive contracts could have distortionary effect in a multitask setting, pioneered by the seminal work of Holmstrom and Milgrom (1991), Baker (1992), and Dixit (1997).[5] Studies on China focus on promotion incentive instead, and found that it has impacts on governance quality, fiscal expenditure and extractions, IPO, FDI and urban expansion (Guo, 2009; Lichtenberg and Ding, 2009; Kung and Chen, 2011; Lü and Landry, 2014; Piotroski and Zhang, 2014; Chen and Li, 2016; Wang et al., 2020; Wang et al., 2021). This paper focuses on China and finds promotion incentive could cause officials to manipulate GDP, i.e., unintended "false positives", which echoes the distortionary effects of high-powered incentives. Nevertheless, it is also found that promotion incentives increase officials' real effort toward improving GDP.

This paper is related to the literature on impacts of political turnovers. Many studies have characterized economic fluctuations associated with political turnovers (Guo, 2009; Labonne, 2016; Alesina and Paradisi, 2017; Corvalan et al., 2018; Xi et al., 2018). It has been established that political turnovers could bring uncertainties and lead to economic distortions (Fang, Xu and Yan, 2018; Shi et al., 2018; Zhong et al., 2019). Our paper adds to this literature by adding a new channel, that is, political turnover incentivizes officials to manipulate statistical information. Meanwhile, the finding that political turnover has positive impacts through the incentive channel again,[6] reveals a complex nature of the impacts of political turnover.

Our paper is certainly related to the literature on GDP reliability issues. There are many works that point out problems with GDP statistics and develop methods to deal with them (Henderson et al., 2012; Wallace 2014; Magee and John 2015; Clark et al., 2018; Lyu et al., 2018; Chen et al., 2019; Martinez 2019). Among them, Henderson et al. (2012) shows that satellite nightlight data can be used to correct unreliable GDP statistics caused by insufficient statistical ability. There are also many studies that assess the accuracy of China's GDP figures.[7] Although some argue that the figures contain numerous inconsistencies and are sometimes falsified (Rawski 2001a, 2001b, 2002; Li 2004), most of these studies focus on macro-level GDP data, and the conclusions are still mixed.[8] Wallace (2016) finds that the difference between GDP (sensitive to political turnover) and electricity growth (non-sensitive to political turnover) is higher when there is a provincial leadership turnover (sensitive time). Clark et al. (2018) argues that China's aggregate GDP figures contain numerous inconsistencies and are sometimes falsified. Lyu et al. (2018) applies a discontinuity methodology to examine the distribution of the difference between actual and target GDP growth rates. In a recent paper, Chen et al. (2019) uses value-added tax to re-estimate

[3] We also apply the approach in Clark, Pinkovskiy and Sala-i-Martin (2018) and machine learning technology (XGBoost) for robustness.

[4] This, however, does not mean that stronger promotion incentives lead to lower real efforts in promoting the economy, as real effort and cheating effort are not necessarily mutually exclusive. In fact, we find that promotion incentive leads to higher real effort too.

[5] Evidences of such distortionary effects have been found in teachers (Levitt and Jacob, 2003), managers (Oyer, 1998), and military officials (Acemoglu *et al.*, 2020).

[6] Another dimension that differs our paper from these studies (e.g. Fang, Xu and Yan, 2018; Shi *et al.*, 2018; Zhong *et al.*, 2019) is that these studies explore the "redistribution" effect of political turnover, while our paper focuses on "growth" effect. The combination of the aforementioned negative "redistribution" and positive "growth" effects suggest that some agents may benefit more due to higher productivity or patronage, but this is beyond the scope of this paper.

[7] Magee and John (2015) and Martinez (2019) argue that GDP manipulation is more severe in authoritarian regimes.

[8] There are optimistic views about the quality of China's GDP data as well (Mehrotra and Pääkkönen 2011; Holz 2004, 2014). However, many researchers, and even senior policy makers, are skeptical of the figures' credibility (see section 2.1).

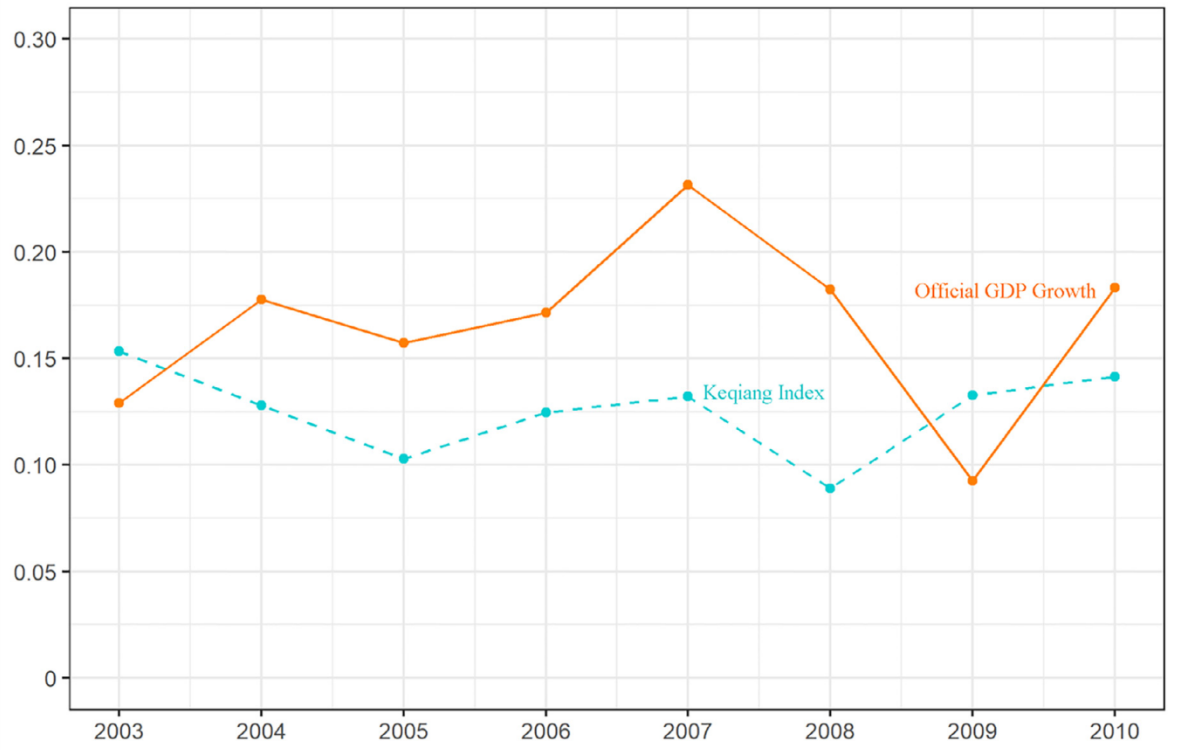

Fig. 1. Official GDP Growth vs. Keqiang Index Note: Official GDP growth is calculated as the mean value of the growth rates of the county official GDP, deflated by provincial CPI in 1999. *Source:* China Data Online, http://chinadataonline.org/.

China's provincial and national GDP, and argues that the adjusted GDP growth from 2008 to 2016 is 1.7% lower than the official figure. Xiong (2018) employs a tournament model to rationalize provincial governor's behavior toward developing economy and manipulating economic statistics, and tests the model predictions on overleverage. This paper is different in that it focuses on the very root level of the government hierarchies. We think that the most basic level ought to be considered when it comes to the impact of promotion incentive, as performance matters more in one's promotion at county level than at higher levels (Landry et al., 2018; Chen, Fan and Zhu, 2020). Our analysis suggests that such a system might induce and aggravate information distortion problems at the most basic level.[9]

The rest of the paper is structured as follows. Section 2 provides background information. Section 3 describes a simple model and derives the predictions. Section 4 discusses data, variables and the empirical specifications. Section 5 presents the baseline results and robustness results. Section 6 presents the heterogeneity results, and Section 7 concludes.

2. Background

2.1. China's GDP reliability and manipulation

Along with the country's rapid economic growth, the credibility of China's GDP statistics has received increasing attention. Aside from the scholarly work described earlier in the introduction, policy makers in China are aware of the reliability problem and have sometimes admitted it publicly. The most influential example is the statement made by the current premier, Li Keqiang, when he was party committee secretary of Liaoning province (2004–2007). He claimed that Liaoning's GDP figure was unreliable, and noted that he personally used three other indicators – electricity consumption, railway cargo volumes and bank loans disbursed – to evaluate the economic situation (WikiLeaks, 2007). Clark et al. (2018) created what they call the "Keqiang Index", which averages these three indicators. Fig. 1 plots the "Keqiang Index" alongside official GDP growth. A simple comparison clearly reveals the discrepancies between the two, which to some extent confirms the doubts about the reliability of official GDP statistics.

Suspected data manipulation was verified after three provinces, Liaoning, Inner Mongolia, and Tianjin, publicly admitted falsifying their fiscal and industrial data, which are essential components of GDP. In January 2017, Chen Qiufa, the governor

[9] This phenomenon is also evident in the Former Soviet Union and some Eastern European countries (Powers, 1992; Koen, 1996; Bartholdy, 1997). We estimate the correlation between GDP and nighttime illumination across countries and find that the correlation for autocratic countries is systematically lower than for democratic counties.

of Liaoning, admitted that the province's fiscal revenue figures were inflated by at least 20 percent between 2011 and 2014; the governments of Inner Mongolia Autonomous Region and Tianjin then acknowledged that their fiscal and economic numbers for 2016 had also been overstated. All three provinces responded by revising their economic figures: Inner Mongolia cuts its industrial output figure for 2016 by 40 percent, and the 2016 GDP of Tianjin's Binhan New Area was reduced by about a third.

2.2. Political promotion and GDP manipulation

GDP manipulation can be attributed to various factors, including promotion incentive.[10] In practice, China's political selection is often described as a performance-based evaluation system, probably to better accord with the mission of "pursuing economic development as our central task."[11] Among the indicators used in the evaluation system, GDP is assigned the largest weight and thus plays a critical role.[12] Furthermore, relative evaluations could formalize severe competition among local officials (Chen et al., 2005; Yu et al., 2016). These officials thus have strong incentives to utilize all means – including falsification – to improve local GDP figures (Guo, 2009; Lichtenberg and Ding, 2009; Kung and Chen, 2011; Lü and Landry, 2014; Piotroski and Zhang, 2014; Chen and Li, 2016; Wang et al., 2020; Wang et al., 2021).

China's statistical system also makes data manipulation possible (see Figure A.1 for a visual illustration). Local party leaders exert complete control over personnel appointments in local statistical bureaus, *and* must approve external reports of statistical data (Pan and An, 2003). As a consequence, local party secretaries have the discretion to manipulate economic data.

Finally, the GDP growth rate targets set by the central and local governments reinforce the incentives for data manipulation. For instance, in 2005 the target for GDP growth was set as 8 percent for the national level, 10.27 percent for the provincial level on average, and 12.81 percent at city level on average (Li et al., 2019).[13] Once local officials realize the difficulty of reaching these amplified targets simply by working hard, falsification of data naturally follows (Cao, 2000).[14]

3. A simple model

To model different promotion incentives, we consider a scenario where official's expected probability of being promoted depends on his terms, ceteris paribus.[15] There are studies showing that terms affect political officials' incentives. For example, it has been found that incumbents have lower incentive than newly elected politicians to act in voters' interests (Rogoff, 1990; Besley and Case, 1995; Johnson and Crain, 2004). In China, despite that political officials are *de facto* appointed rather than elected, the variation in incentives due to term difference still exists. In order to achieve as much political success as possible before they reach the designated retirement age, local officials must minimize their tenure at every position and keep moving up the political ladder. If an official fails to be promoted at the end of his first term at any position, then he is already lagged behind his peers for the next round of competition, as he may be too old to be qualified.[16] Bearing these facts in mind, an official would expect his chance to be promoted to be lower, as he stays longer in the same position.[17] As a consequence, promotion incentive becomes smaller as one's tenure increases.

For simplicity, we consider a two-period economy. In each period, there are many officials who make effort to provide public good. The true amount of the public good is private information. Therefore there is room for officials to manipulate data. Officials can make "chasing" effort (e) or/and "cheating" effort (m). One unit of chasing effort converts to α units of

[10] Some argue that the root of the problem lies in underdeveloped statistical institutional arrangements, and predict that the reliability problem can be gradually resolved by improving the statistical collection process. For example, industry classifications have been adjusted three times since 1984 to bring them more in line with international standards (Holz, 2014); the National Bureau of Statistics conducts regular censuses and sample surveys, and uses them as references to evaluate and adjust the reported data. The pessimistic view, however, attributes the problem to local officials' manipulation, and thus predicts that the reliability issue will remain a problem indefinitely (Cai, 2000; Tsai, 2008).

[11] Many empirical studies have found that officials' political promotions depend on the local area's reported economic performance (Li and Zhou, 2005; Jia, Kudamatsu and Seim, 2015).

[12] For example, in "Indicator System for Finish Building a Moderately Prosperous Society in Jiangsu Province" (revised in 2013, for trial implementation), the weight for GDP per capita is 6%, which is higher than all other individual indicators. Likewise, in "General Performance Evaluation System of Guangzhou and Shenzhen" (2007), the weight for GDP-related indicators (GDP growth rate, GDP per capital) is 4.8%, the highest of all 50 indicators (Chen, Ma and Bao, 2011: 263).

[13] Another example is Fujian province. The GDP growth rate target was set as 9.5 percent for the whole province, 11.4 percent at the city level on average, and 13.75 percent for the county level on average (Li *et al.* 2019).

[14] Such vertical amplification implies that county-level targets would be higher than those proposed by provincial and prefectural officials, which indicates it is almost impossible to complete the task, and that there is a greater potential to manipulate GDP at the county than the provincial or prefecture levels. Therefore, applying county-level data to this research is appropriate.

[15] We compared the promotion outcomes of officials in their first vs. second terms. Table A.1 reports the actual promotion outcomes, and demonstrates that this assumption is satisfied.

[16] Such a pattern has been justified by considerable evidence generated by comparing the promotion likelihood of newly appointed vs. incumbent leaders (Chen and Kung, 2016; Jia, Kudamatsu and Seim, 2015; Opper, Nee and Brehm, 2015). For example, Chen and Kung (2016) calculated that 86% of promotions occurred among first-term leaders. In other words, the probability of promotion plummets after an official's first term in office.

[17] This difference in promotion incentive can also be indirectly justified by a posteriori comparison between the promotion likelihood of incumbents and that of newly appointed officials with our county level data, which would be described later. We present the *posteriori* comparison in Table A.1, revealing that newly appointed officials' promotion likelihood is indeed significantly higher than that of incumbents.

public good whereas one unit of cheating effort converts to 0 unit. However, one unit of cheating effort transforms into 1 unit of the *reported* amount of public good. Here, higher α means that an official is more capable in producing pubic good.

There is cost associated with each type of effort. Chasing effort incurs a direct cost $c(e)$, and it has the usual convex property. Cheating effort incurs no direct cost, but there is an opportunity cost: with a probability $q\ (m, \lambda)$, the official is caught cheating, and he faces a punishment, *loss*, that represents a utility loss that may include jail time, or the forgone future political career. Here, q, the probability of "cheating" behavior being caught upon inspection, is increasing in the reported amount of public good m, and the intensity of inspections undertaken by officials' supervisors, λ. That is, $q_m > 0$, $q_\lambda > 0$. To mimic a cost function, we assume that $q_{mm} >$ 0and $q_{m\lambda} > 0$. Moreover, $q(0, \lambda) = 0$, $\lim_{m\to\infty} q(., \lambda) = \lim_{\lambda\to\infty} q(m, .) = 1$. It is reasonable to think that officials may vary in their ability to manipulate data. For example, a native official or an official who is quite familiar with the local economy could manipulate the data with a lower probability being caught. In that sense, q will negatively depend on official's *native/local* identity.

At the end of each period, officials face a chance to be promoted to a higher position, depending on their performance. Let g and $\hat{g}$ represent the *true* amount and the *reported* amount of public good respectively. As the true amount of public good is private information, the chance of being promoted will depend on the reported amount of public good. Let $F_t(\hat{g})$ represent an official's expected probability of being promoted at the end of period t.[18] To ensure the existence and uniqueness of the optimal solution, it is assumed that F_t is concave and continuously differentiable. If he is promoted, he receives a payoff V; if he is not promoted, then he stays at the same position for another period and earns nothing. Here, V represents the *net* gain in official's discounted lifetime utility by moving one ladder up along the political career path.[19] It is assumed that $V>0$ so that officials desire to be promoted to a higher position.

To capture the notion that the marginal benefit of an additional increase in observed performance (i.e., an additional increase in the probability of being promoted) declines with tenure, we make the following assumption:

Assumption 1: $F_2'(\hat{g}) < F_1'(\hat{g})$.

We are now ready to solve officials' problem. In period t, officials choose (e_t, m_t) to maximize their utility as follows

$$F_t(\alpha e_t + m_t)V - q(m_t, \lambda)loss - c(e_t) \tag{1}$$

Optimal efforts are determined by the two FOCs below:

$$\alpha F_t'(\alpha e_t^* + m_t^*)V = c'(e_t^*), \tag{2}$$

$$F_t'(\alpha e_t^* + m_t^*)V = q_m(m_t^*, \lambda)loss. \tag{3}$$

It is straightforward to see that $e_1^* > e_2^*$, and $m_1^* > m_2^*$. That is, when the marginal benefit of making an additional effort decreases with tenure, regardless of chasing or cheating, officials choose to reduce both kinds of efforts.

PREDICTION: *When a "new" (first-term) official perceives a higher probability of being promoted than an "old" (second-term) official,* ceteris paribus*, the former engages in more chasing and cheating efforts than the latter.*

We can explore some comparative static results on cheating effort. The parameters of interest are λ, *local/native* identity, and *loss*.

λ captures the idea of accountability. As a higher accountability increases the probability of being caught, officials now face a higher marginal cost of cheating. Clearly, an official cheats less as accountability λ rises.

Local/native identity captures whether an official has local information advantage. As mentioned earlier, a native official or an official who is familiar with the local area has an advantage of being caught less often when engaging into data manipulation. This implies a lower q_m. However, such local information advantage could also entitle an official being more capable in providing public good, i.e., a higher α. Mathematically, more local information advantage implies a lower F_t' and a lower q_m. Therefore, it is ambiguous about the effects of local information advantage on official's cheating effort.

We finally explore the effect of a higher forgone political career (higher *loss*) on official's cheating effort. The analysis, however, has some subtlety here, as it would depend on where the higher political career comes from. The source of a higher political career could come from a higher ability α, revealed by officials past good performance. In this case, F_t' declines and *loss* increases, which causes a lower cheating effort. The source of a higher political career could also come from a better connection to superior, which reduces one's risk of being caught. In that case, although *loss* increases, the risk of being caught could be reduced, which leaves the effect on official's cheating effort ambiguous.

[18] Several studies mentioned earlier in the introduction (Li and Zhou, 2005; Jia, Kudamatsu and Seim, 2015) have shown that China's political selection largely depends on economic performance, especially GDP records.

[19] To simplify analysis, we do not consider different level of positions, and thus the return from being promoted to a higher position is not explicitly modeled.

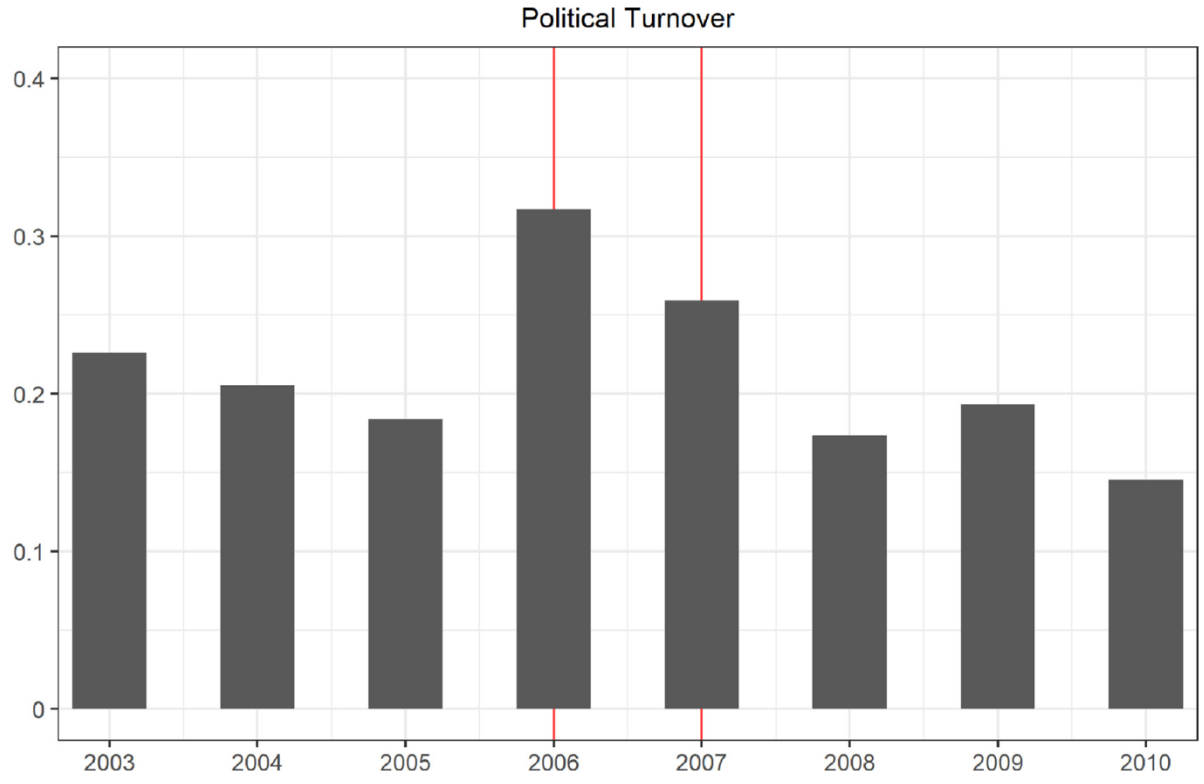

Fig. 2. Fraction of Counties with Political Turnovers. Notes: The vertical axis represents the fraction of counties experiencing turnovers in party secretaries for each year.

4. Research design, data and variables

4.1. Research design

A simple comparison between officials with different incentives arising from term difference suffers from two problems. First, since the nominal term limit is usually not strictly followed in practice, using any tenure length as the actual term limit will be arbitrary. The second one is endogeneity problem due to omitted variables that may correlate with term and GDP both. To deal with these, we employ a difference-in-differences approach using the time of the local party congresses (*di fang dang dai hui*) - the period between 2006 and 2007, right before the 17th National Congress of the Communist Party of China (CPC).[20] We make use of the information on whether there was a new appointment of the county-level party secretary in those meetings to separate counties into two groups: the counties receiving new appointment of party secretaries are categorized into the treatment group, and these new secretaries are undoubtedly in their first terms; the other counties are categorized into the control group, where the incumbent party secretaries are treated as reaching the "second" term. Justification is provided later that these two groups share the common trend before the 17th party congress, which alleviates the endogeneity concern. We pick the 17th National Congress for two reasons. First, this is the only National Congress of the CPC that our dataset (introduced in the next paragraph) covers. Second, the number of the county-level party secretaries newly appointed in the local congresses during this particular period is higher than that in the rest of the data period (see Fig. 2).[21] Therefore, it creates enough variation for us to distinguish "new" officials from "old" officials and construct the treatment and control groups accordingly.

[20] The National Congress of the Communist Party of China is held once every five years. The 17th National Congress of the CPC was held in Beijing from 15 to 21, October 2007, and the local party congresses were convened around 2006 and 2007, before the National Congress. Local party congresses elect the party committees, which in turn elect party secretaries and deputy secretaries (Chapter IV, Party Constitution, Articles 25 and 27). This arrangement can also be found in the Article 33 of The Regulation on the Selection and Appointment of the Party and Government Leaders (*Dangzheng Ganbu Xuanba Renyong Gongzuo Tiaoli, Di 33 Tiao*).

[21] There are local party congresses before each National Congress. The sessions of the local party congress vary across counties. For simplicity, we will interchangeably use the terminology "local party congresses" and "the 17th Party Congress" to represent all the sessions of the local party congresses before the 17th National Congress in the rest of the paper.

We have hand-collected biographic information of county-level party secretaries covering the period from 2003 to 2010. The original sample includes 5724 county-level party secretaries from 2746 counties.[22] During 2006 and 2007, 57.62 percent of the counties experienced turnovers in their party secretaries.[23] The treatment group consists of these counties. For each county in this group, there are two secretaries, the one who was replaced and the newly appointed one during the 17th Party Congress. The newly appointed secretaries start their first term after the 17th Party Congress and therefore are considered as "new" officials. The control group consists of counties experiencing no change in their party secretaries. For these counties, the same party secretaries span the whole period before and after the 17th Party Congress. Thus, they are considered as "old" officials who stayed for another term after the party congress. After these considerations, our final sample consists of 3019 county-level party secretaries from 2412 counties.

Our main independent variables are constructed as follows: *Change* is a county dummy that is coded 1 if a county experienced a change in its party secretary in the local party congress, and 0 otherwise.[24] *After* is a time dummy equal to 1 if the time is after the local party congress, and 0 otherwise.

4.2. How to measure cheating?

Our main dependent variable is *Cheating GDP* (i.e., GDP manipulation), which is measured as the difference between the official GDP and the "true" GDP. For "true" GDP estimation, we first follow Henderson et al. (2012) and make use of satellite data on nighttime illumination in the baseline analysis. In the robustness check section, we follow Clark et al. (2018) and combine both nighttime illumination and "Keqiang Index" to predict "true" GDP,[25] and also apply a machine learning technology (XGBoost) for such prediction. Equation (4) shows that the estimated "true" GDP is a weighted average of the official GDP and predicted GDP. Here, the weights (λ) are selected by minimizing the variance of difference between the unobservable true GDP and the estimated "true" GDP, as illustrated by equation (5).

$$"true"\ GDP = \lambda \times official\ GDP + (1-\lambda) \times predicted\ GDP, \tag{4}$$

$$\lambda = argmin\{var(true\ GDP - "true"\ GDP)\}. \tag{5}$$

To estimate the weight λ, we first obtain the predicted GDP for each county in each year by regressing official GDP (ln) on nighttime illumination (ln).[26] Solving the minimization problem gives λ as:

$$\lambda = \frac{\frac{var(w)}{var(w)+var(z-w)}var(z)var(l) - cov(z,l)^2}{var(z)var(l) - cov(z,l)^2}, \tag{6}$$

where w and z denote "true" GDP and official GDP, respectively, and l denotes nighttime illumination. Since "true" GDP w is unobservable, following Henderson et al. (2012), we will assume the values of $\frac{var(w)}{var(w)+var(z-w)}(\equiv\phi)$ for each county in each year. Instead of setting an arbitrary value for ϕ, we first specify ϕ for areas that are believed to have good-quality data based on some third-party information,[27] and then solve the values of ϕ for the rest of the areas.[28] The third-party information comes from Clark et al. (2018) that uses an independent method to estimate China's true provincial-level GDP. After applying

[22] Officially, the county-level administrative division includes counties, districts, county-level cities, autonomous counties, banners, autonomous banners, forestry districts, and special districts. For convenience, we classified all of these together into a single category of "county." Appendix B explains the data collection procedures in more detail.

[23] Since the local party congresses were convened at different times in 2006 and 2007, we tried an alternative way to look at the trend by plotting the fraction of counties experiencing turnovers for the t-th year posterior to their party congresses (t=−5, …−1, 0, 1, … 5). The patterns are similar.

[24] The "2006/2007" indicates the year 2006 for counties that held their local party congress in 2006, and 2007 for those that held them in 2007. This notation applies throughout the whole paper.

[25] In addition to Henderson, Storeygard and Weil (2012), several recent studies have justified that nightlight data contains reliable information concerning true economic status, and can be used as a proxy for economic development or to predict the true income of an area (Chen and Nordhaus, 2011; Pinkovskiy and Sala-i-Martin, 2016; Clark, Pinkovskiy and Sala-i-Martin, 2018; Vogel *et al.*, 2019).

[26] The natural logarithm of "predicted GDP" is the predicted value from the regression of official GDP (ln) on nighttime illumination (ln).

[27] This approach is preferable because ϕ itself reflects the reliability of GDP data: here we assume $z-w$ is the classical measurement error in official GDP (that is, w and $z-w$ are uncorrelated), as in Henderson, Storeygard and Weil (2012). Thus ϕ represents the ratio of the variance of true GDP to the variance of official GDP. If an area's GDP is believed to be more accurate, then its ϕ is closer to 1. Thus, assigning a specific value of ϕ such as 0.9 to areas with good-quality GDP data is a reasonable and convincing choice.

[28] The solving method is described as follows: as w and $z-w$ are uncorrelated, we have:

$$var(z) = var(w) + var(z-w).$$

Let g denote areas with good quality and b denote other areas, then we have the following system of equations:

$$var(z_g) = var(w) + var(z_g - w),$$
$$var(z_b) = var(w) + var(z_b - w),$$
$$\phi_g = var(w)/[var(w) + var(z_g - w)],$$
$$\phi_b = var(w)/[var(w) + var(z_b - w)].$$

With four equations and four unknown variables ($var(w)$, $var(z_g - w)$, $var(z_b - w)$, and ϕ_b), ϕ_b can be calibrated successfully.

Table 1
Summary Statistics.

	Observation	Mean	Std. Dev.	Data Source
Official GDP (log)	11,364	12.704	1.095	A
Satellite Nightlight (log)	12,852	0.783	1.769	B
Estimated "True" GDP (log)	11,279	12.703	1.075	C
After	13,033	0.328	0.470	D
Change	13,033	0.415	0.493	D
Investment	10,850	11.741	1.220	E
Employment	10,181	9.809	0.812	A
Human capital	10,136	10.824	0.853	A
Age	10,129	46.676	3.979	D
Tenure	12,952	3.030	1.732	D
Education	9667	17.400	2.163	D

Sources:
.A: China Data Online, http://chinadataonline.org/;
B: Version 4 DMSP-OLS Nighttime Lights Time Series, https://ngdc.noaa.gov/eog/dmsp/downloadV4composites.html#AVSLCFC;.
C: Calculated by authors, as described in Section 4.2;.
D: Collected by authors, as described in Appendix B;.
E: China County Statistical Yearbook (*Zhongguo Xian(Shi) Shehui Jingji Tongji Nianjian*);.

China's official provincial data to their structural model, and adopting the justification provided in Chen et al. (2019) for good-quality areas, we pick Jiangsu and Jiangxi as the ones believed to have good-quality GDP data, and assign a value of 0.9 to their ϕs.[29] This method generates a weight (λ) of 0.79 for these two provinces, and 0.54 for the rest of the areas in our baseline analysis.

We then calculate the "true" GDP according to equation (4). The main dependent variable, *Cheating GDP*, is obtained as the difference of the log value of official GDP and the log value of the estimated "true" GDP.[30] To justify our measurement of GDP manipulation, we compare it with the "Keqiang Index" and the three provincial cases of falsified economic statistics exposed by the news media. We regress both official GDP and the estimated "true" GDP on the "Keqiang Index" and find that the estimated "true" GDP fits better than the official GDP (see Figure A.8).[31] Also, our measurement of GDP manipulation is consistent with the exposed cases of falsified economic statistics for Yunnan Province around 2005 and Liaoning Province after 2009 (see Figure A.7).[32]

4.3. Summary statistics

Table 1 provides summary statistics for the dependent variables, the three independent variables, and leader- and county-specific control variables. For leader-specific controls, we consider age, tenure and education.[33] The squared terms of these variables are also considered to address the nonlinearity effects (Li and Zhou, 2005; Chen and Kung, 2016). For county-specific controls, following the standard growth literature (Acemoglu, 2008: 85–89), we consider investment, employment, and human capital.[34] Investment is measured as total investment in fixed assets (*Quanshehui Gudingzichan Touzi*), employment is measured as the number of employed persons at year end (*Nianmo Congye Renyuan*), and human capital is measured as the number of students enrolled in primary and regular secondary schools (*Zhongxiaoxue Ruxue Renshu*).

[29] As a robustness check (Appendix D2.2), we conducted the same analyses using alternative values of ϕ_g and alternative good-quality areas. The results remain unchanged.

[30] Specifically, $cheating\ GDP(log) = official\ GDP\ (log) - "true"\ GDP\ (log)$. We also use the normalized difference as an alternative for *cheating GDP*, and report the results in Table A.8. This additional exercise is motivated by the positive correlation between the difference and economic size, regardless whether the latter is measured by official GDP or "true" GDP (see Figure A.6).

[31] Usually, the "Keqiang Index" is composed of loan, railway freight, and electricity production data. Unfortunately, the latter two are unavailable at the county level. Thus, here we use county-level loan and rural consumption of electricity to construct the index.

[32] Bai Enpei, the party secretary of Yunnan Province, manipulated the GDP growth rate from 7.5% to 12% in 2005; Wang Min also manipulated GDP data when he served as the party secretary of Liaoning Province from 2009 to 2015. We focus on these two cases, instead of others like those from Inner Mongolia or Tianjin, because the latter cannot be covered by our sample period (2003–2010).

[33] Age is regarded as one of most crucial factors correlated with promotion (Chen and Kung, 2016). The effect of tenure duration on promotion has also been highly justified by the political selection literature (Guo, 2009), and education is an important consideration in promotion decisions (Shih, Adolph and Liu, 2012).

[34] According to standard growth theory, capital, instead of investment, is an important input for production. However, we do not have access to county-level capital data (Total Fixed Capital Stock, *Quan Shehui Guding Ziben Cunliang*). If we follow the perpetual inventory method to derive capital from investment, too many missing variables would be generated. Thus, as a second-best choice, we directly employ investment as a control variable.

Table 2
Baseline Results.

	Dependent Variable					
	Cheating GDP		*"True" GDP*		*Official GDP*	
	(1)	(2)	(3)	(4)	(5)	(6)
Interaction	0.010**	0.011**	0.011**	0.012**	0.021**	0.023**
	(0.005)	(0.005)	(0.006)	(0.006)	(0.010)	(0.011)
After	−0.012***	−0.006	−0.009*	−0.002	−0.021**	−0.009
	(0.004)	(0.004)	(0.005)	(0.005)	(0.009)	(0.009)
Personal characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Socioeconomic characteristics		Yes		Yes		Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture specific year trend	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7725	6344	7725	6344	7766	6350
Within R^2	0.145	0.220	0.539	0.651	0.307	0.400

Notes: Standard errors in parentheses are clustered at the prefecture level.
* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$. The constant term is not reported. Personal characteristics include party secretaries' age, tenure, education and their squared terms; socioeconomic characteristics include investment (ln), employment (ln), and human capital (ln).

5. Promotion incentives and gdp manipulation: empirical results

5.1. Econometric specification

We examine the relationship between GDP manipulation and promotion incentive using the following specification:

$$y_{it} = \beta_1 After_{it} \times Change_i + \beta_2 After_{it} + \beta_3 Change_i + \alpha Constant + \gamma_i + \theta_t + \delta X_{it} + pref \times t + \varepsilon_{it}, \quad (7)$$

where i denotes county and t the year, and y_{it} are the dependent variables defined in Section 4.2. $After_{it}$ and $Change_i$ are dummy variables defined in Section 4.1. $After_{it}$ is a dummy variable indicating whether year t is after county-i's local party congress. $After_{it}$ varies across counties as some counties held their party congresses in 2006 while others in 2007. The dummy variable $Change_i$ is 1 if a new party secretary was appointed to county i in the local party congress, and 0 otherwise.[35] We are mainly interested in the interaction term of these two dummy variables, and expect the coefficient β_1 to be significantly positive. γ_i indicates county fixed effects to control for all time-invariant factors, θ_t represents year fixed effects to capture the common shocks to all counties at each t, and $pref \times t$ is included to control for prefecture specific year trend. X_{it} represents other control variables that are described in Section 4.3.

5.2. Baseline results

Table 2 reports the baseline results, which are consistent with Prediction 1. The first two columns of the table show that the coefficient of the interaction term for *Cheating GDP* is positive and significant, implying that "new" party secretaries engage in more GDP data manipulation than "old" secretaries. Columns (3)-(4) show that "new" secretaries also engage in more chasing efforts to improve the economy than their "old" counterparts, which is consistent with existing evidence on China's merit-based promotion system (Li and Zhou, 2005; Landry et al., 2018). Finally, the last two columns of the table show that *Official GDP* is also positively significantly affected by promotion incentives, which coincides with previous findings on China's political business cycle (Guo, 2009; Xi et al., 2018).[36]

To validate our difference-in-differences approach, we conduct an event study by employing a year-by-year specification as follows to check the common trend assumption:

$$y_{it} = \sum_{\tau=-3+}^{3+} \beta_{1,\tau} \times After_{\tau i} \times Change_i + \sum_{\tau=-3+}^{3+} \beta_{2,\tau} \times After_{\tau i} + \beta_3 Change_i + \alpha Constant + \eta X_{it} + \gamma_i + \theta_t + pref \times t + \varepsilon_{it}. \quad (8)$$

[35] Since $Change_i$ is time invariant, β_3 will be absorbed by the county fixed effect.
[36] We also cluster the errors at different levels (province, year, as well as individual). Moreover, we restrict the same analysis to the balanced panel between 2003 and 2010. The results for these replications are reported in Tables A.9 and A.10, respectively. The baseline results remain the same for both replications.

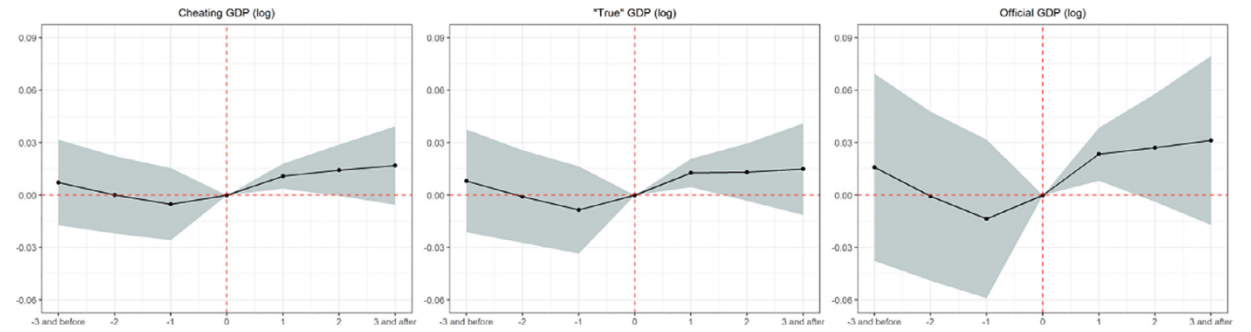

Fig. 3. *Common Trend Check. Notes:* The horizon axis indicates the periods before and after 2006/2007, and the vertical axis indicates the coefficient estimates. Period 0 (2006/2007) is used as the reference year.

Here, the previous "$After$" dummy variable is replaced by a series of time dummy variables indicating the τth year away from 2006/2007.[37] Fig. 3 plots the trend of the estimated coefficients for three dependent variables, and shows that there is no pre-trend in *Cheating GDP*, *"True" GDP*, and *Official GDP* between the two groups until the year after 2006/2007. These results support the common trend assumption and validate the difference-in-differences estimation.[38]

5.3. IV results and robustness

Existing literature on political turnover has regarded three alternative variables – predicted turnover, age, and tenure – as "exogenous" enough and eligible to be instrument variables. In this subsection, we report instrument results stemming from these variables to further overcome the potential endogeneity problem, caused by events that could have been correlated with the 17th Party Congress.

Ru (2018) asserts that a predecessor's turnover can be used to predict a current official's turnover. Based on this, our *first* instrument is a county dummy indicating whether there is a predicted turnover during 2006 and 2007. The dummy is constructed as follows. Consider a county-i party secretary who starts her office before the 17th Party Congress, and her starting year is y. Then the predicted turnover for this county occurs at year $y + 5$. If year $y + 5$ coincides with the year of the realized political turnover, i.e., 2006 or 2007, then the dummy is equal to 1 for county i. If not, the dummy is 0. The exogeneity comes from the staring year of the party secretary before the 17th Party Congress (or, the leaving year of the secretary before this one) and the fixed term duration. Also, the latter is only "loosely" followed in reality, which ensures the relevance of the instrument.

According to Fang et al. (2018), the older an official is, or the longer an official has stayed in her current position, the more likely she is to be replaced. Therefore, our *second* and *third* instruments are age dummy and tenure dummy of the county-level party secretary one year before 2006/2007. The age dummy is equal to 1 if the age of this secretary is no smaller than the median value, and 0 otherwise. Similarly, the tenure dummy is equal to 1 if the tenure of this secretary is no smaller than the median value, and 0 otherwise. The exogeneity comes from the assertation that the age and tenure before the 17th Party Congress are not likely to affect the GDP performance after the party congress.

The specifications are given below:

$$First\ Stage:\ Change_i = b_1 IV_1 + b_2 IV_2 + b_3 IV_3 + b_4 After_{it} + \alpha Constant + \gamma_i + \theta_t + \delta X_{it} + pref \times t + \epsilon_{it}, \tag{9}$$

$$Second\ Stage:\ y_{it} = \beta_1 After_{it} \times \widehat{Change}_i + \beta_2 After_{it} + \beta_3 \widehat{Change}_i + \alpha Constant + \gamma_i + \theta_t + \delta X_{it} + pref \times t + \varepsilon_{it}, \tag{10}$$

The IV estimation results are reported in Table 3. Column (1) reports the first-stage result given by specification (9) and shows that the three instruments are positively related to the actual turnover. The large F values reported in the remaining columns alleviated the weak IV concerns. Columns (2), (3), and (4) report the second-stage results given by specification (10) for cheating GDP, "true" GDP, and official GDP, respectively. We still find that the interaction term has very significant (and somewhat larger) effects on the three dependent variables, which further confirms the robustness of our baseline results.

Furthermore, a series of robustness-check exercises have been carried out for concerns on measurement errors and potential omitted variables.[39] These include further controlling political connections and rival competitions, conducting same

[37] For example, $\tau = 0$ refers to 2006/2007. $\tau = -3+$ refers to "at least three years before 2006/2007", and $\tau = 3+$ refers to "at least three years after 2006/2007".

[38] Appendix D.1 provides a detailed description of the event study.

[39] Robustness check results are presented in Appendix D.2.

Table 3
Instrument Variable Estimation.

	First Stage	Second Stage		
		Cheating GDP	*"True" GDP*	*Official GDP*
	(1)	(2)	(3)	(4)
After × predicted turnover dummy	0.090***			
	(0.027)			
After × age dummy	0.036*			
	(0.022)			
After × tenure dummy	0.459***			
	(0.029)			
After × turnover		0.021**	0.025***	0.045**
		(0.008)	(0.009)	(0.018)
After	0.058***	−0.010*	−0.006	−0.016
	(0.020)	(0.005)	(0.006)	(0.011)
Personal characteristics	Yes	Yes	Yes	Yes
Socioeconomic characteristics	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes
Prefecture-specific year trend	Yes	Yes	Yes	Yes
Observations	6094	6001	6001	6007
Within R^2	0.820	0.235	0.967	0.899
Cragg-Donald Wald F statistic		693.86	693.86	694.87
Kleibergen-Paap rk Wald F statistic		126.47	126.47	126.44

Notes: Standard errors in parentheses are clustered at the prefecture level.
* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$. The constant term is not reported. Personal characteristics include party secretaries' age, tenure, education and their squared terms; socioeconomic characteristics include investment (ln), employment (ln), and human capital (ln).

analysis for various subsamples, running placebo tests, redoing the DID estimation with a Propensity Score Matching approach and employing alternative methodologies to estimate for "true" GDP and GDP manipulations. Our basic results are not changed.

6. Who is more likely to cheat?

This section explores the heterogeneity effect and examines how the impact of promotion incentive on officials' GDP manipulation behavior is affected. We consider three categories of factors that all relate to the cost of such behavior. The first one is *accountability*. Stronger accountability, either from superiors or from grassroots, increases the risk of being caught. Therefore the impact might be smaller in counties with stronger accountability. The second one is *local information advantage*. Officials with more local information advantage can either manipulate GDP in a less risky way, or become more effective in promoting real economic growth that in fact dampens the demand for data manipulation. As a consequence, the ultimate effect on the impact is uncertain. The last one is *opportunity cost*. A higher forgone political career may either increase or reduces official's incentive to cheat, depending on the source of such forgone political career. Again, how the impact is affected is uncertain. Our empirical results confirm the importance of *accountability*, but we do not find significant effects for *information advantage* and *opportunity cost*.

6.1. Accountability

Under China's regionally decentralized regime, local officials' political prospects are mainly determined by their superiors who are in charge of personnel decisions (Li and Zhou, 2005; Xu, 2011). As a consequence, local officials are accountable to their superiors, and their behavior will be affected by the degree of supervision exerted by the superiors. When the degree of supervision increases, local officials tend to manipulate GDP data less and it is expected that the aforementioned impact might be smaller. One way to measure this is to proxy the degree of supervision from superiors by the number of economic affair visits made by provincial/central leaders.[40] Note that since visiting activities are reported by newspapers and such information are exposed to citizens, this proxy would capture public attention to some extent. We also standardize

[40] We collect data on inspections carried out by provincial and central officials in each prefecture from local official newspapers. Admittedly, the best measure would be inspections carried out in each county. However, such information is unavailable. Thus we regard inspections in each prefecture as the second-best choice and the empirical exercise mainly relies on the cross-prefecture variation.

Table 4
Accountability, Information Advantage, and Opportunity Cost.

	Accountability		Information Advantage		Opportunity Cost	
	X = Inspection (1)	X = GDP Search (2)	X = Native (3)	X = Local (4)	X = Ability (5)	X = Age (6)
$X \times$ After $\times$ Change	−0.006*	−0.005**	0.012	0.007	−0.004	−0.007
	(0.004)	(0.002)	(0.008)	(0.009)	(0.007)	(0.004)
After $\times$ Change	0.010**	0.010	0.004	0.005	0.015**	0.011**
	(0.005)	(0.006)	(0.007)	(0.005)	(0.007)	(0.005)
$X \times$ After	0.008**	−0.003***	−0.014***	0.009	−0.004	0.002
	(0.003)	(0.000)	(0.005)	(0.006)	(0.004)	(0.003)
$X \times$ Change	0.002	−0.012	−0.001	−0.006	0.006*	0.011
	(0.003)	(0.013)	(0.006)	(0.005)	(0.003)	(0.009)
After	−0.012***	−0.007	0.001	−0.011**	−0.009	−0.006
	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)	(0.004)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture specific year trend	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5199	4021	6344	5782	3617	6344
R^2	0.242	0.225	0.222	0.222	0.233	0.221

Notes: Standard errors in parentheses are clustered at prefecture level.
* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$. The constant term is not reported. All constitutive terms of the triple interaction terms are included: in addition to those reported in the table, "Change" is omitted because it is time invariant and thus absorbed by county fixed effects, and the "X"s are also controlled but not reported as in column (3)-(6) they are omitted due to the collinearity with the fixed effects and trends. Inspection, GDP search, ability, and age (which is the taking-office age) are all standardized by first subtracting their means and then divided by their standard deviations. Local and native are dummy variables. The control variables include party secretaries' age, tenure, education, their associated squared terms, investment (ln), employment (ln), and human capital (ln).

the number by subtracting the mean and dividing it by the standard deviation. The result is reported in the column (1) of Table 4. The coefficient for the triple interaction term is negative and significant, which implies more inspection curbs GDP data manipulation.

Although reporting to superiors almost dominates their daily work, officials are also required to respond to public opinions (Chen and Li, 2017; Jiang and Zeng, 2020). If the local public pays more attention to local GDP, the more likely GDP manipulation will be exposed, and local officials tend to manipulate GDP data less. Thus the impact might be smaller in county with higher public attention. To test this, we proxy the degree of public attention by the searching frequency for "GDP" in the Baidu search engine (http://www.baidu.com) in each sample year and county,[41] which is also standardized. The finding is reported in the column (2) of Table 4. The coefficient of triple interaction term is negative and significant, which is consistent with the prediction.

6.2. Local information advantage

Previous studies have shown that the extent to which officials have access to local information affects their behavior (Svolik, 2009; Gehlbach et al., 2016; Fan et al., 2016). Following Persson and Zhuravskaya (2016), we lay out two characteristics that could capture official's access to local information. The first one is whether an official is native to the county she serves. We use a dummy "native" to measure this. It is 1 if official's hometown and the county she currently serves are located in the same prefecture and 0 otherwise. The second one is whether an official is familiar to the county she currently serves. If she has worked in the same county before, then she is familiar and has some local information advantage. We use a dummy "local" to measure this. It is 1 if an official has previously worked in the county she currently serves and 0 otherwise.[42] The results are reported in column (3) and (4) of Table 4. Both coefficients of the triple interaction terms are insignificant, suggesting that we do not find evidence supporting the role of local information.

Persson and Zhuravskaya (2016) argued that there are both positive and negative effects involved with being a "local". The positive side means that "locals cater to low-level provincial elites", and locals spend a higher share of budgetary resources on education and health care. The "negative" side means that locals could utilize their local information advantage

[41] Baidu is a widely used internet search engine in China. We do not use Google as it is blocked by the censorship wall and thus is unavailable to most people in mainland China. In our empirical analysis, we normalize the searching frequencies by electricity consumption.

[42] According to Article 5 of "Measures on Implementation of Withdrawal System for Party and Government Leaders" (*Dangzheng Lingdao Ganbu Renzhi Huibi Zanxing Guiding*), leaders cannot serve in their hometowns directly. Thus, we use "sharing the same prefecture" instead of "sharing the same county" to define "native" or "non-native".

to exercise more data manipulation. In other words, the ultimate effect of being a "local" could be ambiguous, and our insignificant finding for "local" could serve as a complement to Persson and Zhuravskaya (2016) in this sense. Moreover, the insignificant effect of "native" also suggests local officials' desire of applying local information advantage to data cheating may be completely dominated by hometown favoritism (Alder and Kondo, 2018; Fisman et al., 2018; Chu et al., 2020).

6.3. Opportunity cost – forgone political career

The opportunity cost for an official to manipulate GDP data refers to the forgone political career if she was caught, i.e., the career prospect she would have if not caught. We consider two important factors that could affect one's future political career: ability and age. Higher personal ability usually brings a higher likelihood of being promoted (Yao and Zhang, 2015; Xi et al., 2018) and therefore implies a higher opportunity cost of GDP manipulation. Thus the impact might be smaller for party secretaries associated with higher ability. We use the standardized number of the years an official takes to become a member of the standing committee of the local party congress as a proxy for ability (fewer years implies higher ability).[43] The result is reported in column (5) of Table 4. Coefficient of the triple interaction term is not significant and we do not find supportive evidence for the role of forgone political career measured by ability.

Age is another important factor considered in China's political selection. Prior studies have provided considerable evidence that younger officials enjoy better promotion prospects (Bo, 1996; Li and Zhou, 2005; Wang et al., 2020). Accordingly, younger officials would face of a higher opportunity cost than older one for GDP manipulation. We use the standardized age when an official became party secretary as a proxy. Result of the triple difference estimation is reported in column (6) of Table 4. Again, we do not find supporting evidence for the role of forgone political career measured by it.

7. Conclusion

There are many case studies and a significant amount of anecdotal evidence on data manipulation in China. This paper conducts a theoretical and empirical analysis to identify a systematic pattern of GDP manipulation at the county level.

Our simple model makes several predictions, and the empirical results are consistent with these predictions. We find that county-level party secretaries who are in the first term of their tenure undertake more cheating effort to manipulate GDP than those in their second term. Meanwhile, we find that the impact of promotion incentive on GDP manipulation is smaller in counties where party secretaries are subject to stronger accountability. The findings not only indicate that higher promotion incentives could lead to greater data manipulation that adds nothing to the real economy, but also suggest that strengthening accountability might be able to curb such behavior.

These results could considerably enrich our understanding of political business cycles and information distortions. On the one hand, political business cycles are prevalent around the world (Guo, 2009; Labonne, 2016; Alesina and Paradisi, 2017; Corvalan et al., 2018) and China is no exception. Moreover, our finding of the cyclical behavior of GDP manipulation due to promotion incentives is an addition to existing indicators like fiscal extraction and expenditure, effective tax rate or coal mine deaths (Guo, 2009; Nie et al., 2013; Xi et al., 2018). On the other hand, information friction exists everywhere and generates non-negligible consequences (Cheng et al., 2014; Allen, 2014; Fan et al., 2016; Steinwender, 2018). By identifying the existence of GDP manipulation, this paper shows that information distortion could be induced and aggravated by promotion incentives.

At last, it deserves to point out that China has taken several actions to deal with these problems. For example, the central government has used "Keqiang Index" as a complementary statistic to make public policy, has built a "Direct Online Report System" (*lianwangzhibao*) to prevent potential data manipulation, has conducted stricter audits on certain provinces to force them to squeeze water from official data. Future studies shall systematically summarize these actions, and explore their impacts on data manipulations and economic growth.

Declaration of Competing interest

None

Funding

This work was supported by the National Natural Science Foundations of China (72073133, 71473141, 71773021, 71933002), Innovation Program of Shanghai Municipal Education Commission (2017-01-07-00-07-E00002), Beijing Municipal Fund for Building World-Class Universities (Disciplines) of Renmin University of China, Zhuoyue Talent Project and 2018-21 Theoretical Economics Type I Peak Program of Fudan University.

[43] The standardization in this section is similar to the standardization in section 6.1. Educational degree is often considered as a plausible proxy for ability. However, in China, reported education degree of an official contains much noise, as it is extremely difficult to distinguish full-time education before an official started working from the degree she obtained at the CPC party school or though part-time education. The former represents the actual education level while the latter is degree only. Therefore, we consider alternative proxy for ability in our analysis.

Acknowledgments

We are grateful to Hongbin Li, Ruixue Jia, Zheng Michael Song, and conference participants at the CUHK Macro and History Workshop for helpful discussions, comments and suggestions.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jebo.2021.07.010.

References

Acemoglu, Daron., 2008. Introduction to Modern Economic Growth. Princeton University Press.
Acemoglu, Daron, Fergusson, Leopoldo, Robinson, James A., Romero, Dario, Vargas, Juan F., 2020. The perils of high-powered incentives: evidence from Colombia's false positives. Am. Econ. J.: Econ. Policy 12 (3), 1–43.
Alder, Simon, Kondo, Illenin, 2018. Political Distortions and Infrastructure Networks in China: A quantitative Spatial Equilibrium Analysis. working paper.
Alesina, Alberto, Paradisi, Matteo, 2017. Political budget cycles: evidence from Italian Cities. Econ. Pol. 29 (2), 157–177.
Allen, Treb., 2014. Information frictions in trade. Econometrica 82 (6), 2041–2083.
Bai, Chong-En, Hsieh, Chang-Tai, Song., Zheng Michael, 2020. Special deals with Chinese characteristics. NBER Macroecon. An. 34 (1), 341–379.
Baker, George., 1992. Incentive contracts and performance measurement. J. Polit. Econ. 100, 598–614.
Bartholdy, Kasper., 1997. Old and new problems in the estimation of national accounts in transiton economies. Economics of Transition 5 (1), 131–146.
Besley, Timothy, Case, Anne, 1995. Does electoral accountability affect economic policy choices? Evidence from gubernatorial term limits. Q. J. Econ. 110 (3), 769–798.
Bo, Zhiyue., 1996. Economic performance and political mobility: chinese provincial leaders. J. Contemporary China 5 (12), 135–154.
Burgess, Robin, Hansen, Matthew, Olken, Benjamin A., Potapov, Peter, Sieber, Stefanie, 2012. The political economy of deforestation in the tropics. Q. J. Econ. 127 (4), 1707–1754.
Cai, Yongshun., 2000. Between state and peasant: local cadres and statistical reporting in rural China. China Q. 783.
Cao, Jinqing., 2000. China Alongside the Yellow River: Investigation and Thoughts On Countryside Society of a Sociologist. Shanghai Literature and Art Press (in Chinese).
Chen, Shuo, Li, Yiran, 2016. Secrets of Ghost towns: Career incentives and Politically Driven Urbanization in China. working paper.
Chen, Ting, Kung, James Kai-Sing, 2016. Do land revenue windfalls create a political resource curse? Evidence from China. J. Dev. Econ. 123, 86–106.
Chen, Ting, Kung, James Kai-sing, 2019. Busting the "Princelings": the campaign against corruption in China's primary land market. Q. J. Econ. 134 (1), 185–226.
Chen, Wei, Chen, Xilu, Hsieh, Chang-Tai, Song., Zheng Michael, 2019. A forensic examination of China's national accounts. In: Brookings Papers on Economic Activities. Spring, pp. 77–127.
Chen, Ye, Li, Hongbin, Zhou, Li-An, 2005. Relative performance evaluation and the turnover of provincial leaders in China. Econ. Lett. 88 (3), 421–425.
Chen, Xi, Nordhaus, William D., 2011. Using luminosity data as a proxy for economic statistics. Proc. Natl. Acad. Sci. 108 (21), 8589–8594.
Cheng, Ing-Haw, Sahil, Raina, Xiong, Wei, 2014. Wall street and the housing bubble. Am. Econ. Rev. 104 (9), 2797–2829.
Chu, Jian, Fisman, Raymond, Tan, Songtao, Wang, Yongxiang, 2020. Hometown favoritism and the quality of government monitoring: evidence from rotation of Chinese auditors. Am. Econ. J: Appl. Econ. Forthcoming.
Clark, Hunter, Pinkovskiy, Maxim, Sala-i-Martin, Xavier, 2018. China's GDP growth may be understated. China Econ. Rev. doi:10.1016/j.chieco.2018.10.010.
Corvalan, Alejandro, Cox, Paulo, Osorio, Rodrigo, 2018. Indirect political budget cycles: evidence from Chilean municipalities. J Dev Econ 133, 1–14.
Dixit, Avinash., 1997. Power of incentives in private versus public organizations. Am. Econ. Rev. 87 (2), 378–382.
Fan, Ziying, Xiong, Wei, Zhou, Li-An, 2016. Information Distortion in Hierarchical organizations: A study of China's great Famine. working paper.
Fang, Hanming, Li, Zhe, Xu, Nianhang, Yan, Hongjun, 2018. the Shadows of the government: Relationship building During Political Turnovers. NBER working paper #25300.
Fisman, Raymond, Shi, Jing, Wang, Yongxiang, Xu, Rong, 2018. Social ties and favoritism in Chinese science. J. Polit. Econ. 126 (3), 1134–1171 2018.
Gehlbach, Scott, Sonin, Konstantin, Svolik, Milan W., 2016. Formal models of nondemocratic politics. Ann. Rev. Pol. Sci. 19, 565–584.
Guo, Gang., 2009. China's local political budget cycles. Am. J. Pol. Sci. 53 (3), 621–632.
Henderson, J.Vernon, Storeygard, Adam, Weil, David N., 2012. Measuring economic growth from outer space. Am. Econ. Rev. 102 (2), 994–1028.
Holmstrom, Bengt, Milgrom, Paul., 1991. Multitask principal-agent analyses: incentive contracts, asset ownership, and job design. J. Law, Econ. Organization 7 (0), 24–52.
Holz, Carsten A., 2004. China's statistical system in transition: challenges, data problems, and institutional innovations. Rev. Income and Wealth 50 (3), 381–409.
Holz, Carsten A., 2014. The quality of China's GDP statistics. China Econ. Rev. 30, 309–338.
Jia, Ruixue, Kudamatsu, Masayuki, Seim, David, 2015. Political selection in China: the complementary roles of connections and performance. J. Eur. Econ. Assoc. 13 (4), 631–668.
Jiang, Junyan, Zeng, Yu, 2020. Countering capture: elite networks and government responsiveness in China's land market reform. J. Politics 82 (1), 13–28.
Johnson, Joseph M., Crain, W.Mark, 2004. Effects of term limits on fiscal performance: evidence from democratic nations. Public Choice 119 (1–2), 73–90.
Kung, James Kai-Sing, Chen, Shuo, 2011. The tragedy of the nomenklatura career incentives and political radicalism during China's Great Leap Famine. Am. Pol. Sci. Rev. 27–45.
Koen, V., 1996. Russian macroeconomic data: existence, access, interpretation. Communist Econ. Econ.Transform 8 (3), 321–333.
Labonne, Julien., 2016. Local political business cycles: evidence from Philippine municipalities. J. Dev. Econ. 121, 56–62.
Landry, Pierre F., Lü, Xiaobo, Duan, Haiyan, 2018. Does performance matter? Evaluating political selection along the Chinese administrative ladder. Comp. Polit. Stud. 51 (8), 1074–1105.
Levitt, Steven D., 1997. Using electoral cycles in police hiring to estimate the effect of police on crime. Am. Econ. Rev. 87 (3), 270–290.
Levitt, Steven D., Jacob, Brian, 2003. Rotten apples: an investigation of the prevalence and predictors of teacher cheating. Q. J. Econ. 118 (3), 843–877.
Li, Deshui., 2004. Some thoughts about GDP. Econ. Res. J. 4, 26–28 in Chinese.
Li, Hongbin, Zhou, Li-An, 2005. Political turnover and economic performance: the incentive role of personnel control in China. J. Public Econ. 89 (9), 1743–1762.
Li, Xing, Liu, Chong, Weng, Xi, Zhou, Li-An, 2019. Target setting in tournaments: theory and evidence from China. Econ. J. doi:10.1093/ej/uez018.
Lichtenberg, Erik, Ding, Chengri, 2009. Local officials as land developers: urban spatial expansion in China. J. Urban Econ. 66 (1), 57–64.
Lü, Xiaobo, Landry, Pierre F., 2014. Show me the money: interjurisdiction political competition and fiscal extraction in China. Am. Pol. Sci. Rev. 108 (03), 706–722.
Lyu, Changjiang, Wang, Kemin, Zhang, Frank, Zhang, Xin, 2018. GDP management to meet or beat growth targets. J. Account. Econ. 66 (1), 318–338.
Martinez, Luis. (2019). How much should we trust the dictator's GDP growth estimates? Available at SSRN: https://ssrn.com/abstract=3093296.
Maskin, Eric, Qian, Yingyi, Xu, Chenggang, 2000. Incentives, information, and organizational form. Rev. Econ. Stud. 67 (2), 359–378.
Mehrotra, Aaron, Pääkkönen, Jenni, 2011. Comparing China's GDP statistics with coincident indicators. J. Comp. Econ. 39 (3), 406–411.

Nie, Huihua, Jiang, Minjie, Wang, Xianghong, 2013. The impact of political cycle: evidence from coalmine accidents in China. J. Comp. Econ. 41 (4), 995–1011.
Opper, Sonja, Nee, Victor, Brehm, Stefan, 2015. Homophily in the career mobility of China's political elite. Soc. Sci. Res. 54, 332–352.
Oyer, Paul., 1998. Fiscal year ends and nonlinear incentive contracts: the effect on business seasonality. Q. J. Econ. 113 (1), 149–185.
Pan, Zhenwen, An, Yuli, 2003. Where does the difference of one trillion yuan come from? Some thoughts about the difference between national and provincial statistics. Statistical Policy 11, 8–9 in Chinese.
Persson, Petra, Zhuravskaya, Ekaterina, 2016. The limits of career concerns in federalism: evidence from China. J. Eur. Econ. Assoc. 14 (2), 338–374.
Pinkovskiy, Maxim, Sala-i-Martin, Xavier, 2016. Lights, camera… income! Illuminating the national accounts-household surveys debate. Q. J. Econ. 131 (2), 579–631.
Piotroski, Joseph D., Zhang, Tianyu, 2014. Politicians and the IPO decision: the impact of impending political promotions on IPO activity in China. J. Financ. Econ. 111 (1), 111–136.
Powers, S., 1992. Statistical needs in Eastern Europe. Mon. Labor Rev. 115 (3), 18–28.
Rawski, Thomas G., 2001. China by the numbers: how reform affected Chinese economic statistics. China Perspectives 33, 25–34.
Rogoff, Kenneth, 1990. Equilibrium political budget cycles. Am. Econ. Rev. 21–36 LXXX.
Ru, Hong., 2018. Government credit, a double-edged sword: evidence from the China Development Bank. J. Fin. 73 (1), 275–316.
Shi, Xiangyu, Xi, Tianyang, Zhang, Xiaobo, Zhang, Yifan, 2018. Moving "umbrella": Bureaucratic transfers, collusion, and Rent-Seeking in China. The National School of Development Working Paper #E2018011.
Shih, Victor, Adolph, Christopher, Liu, Mingxing, 2012. Getting ahead in the communist party: explaining the advancement of central committee members in China. Am. Pol. Sci. Rev. 106 (1), 166–187.
Steinwender, Claudia., 2018. Real effects of information frictions: when the states and the Kingdom became united? Am. Econ. Rev. 108 (3), 657–696.
Svolik, Milan W., 2009. Power sharing and leadership dynamics in authoritarian regimes. Am. J. Pol. Sci. 53 (2), 477–494.
Tsai, Lily L., 2008. Understanding the falsification of village income statistics. China Q. 196, 805–826.
Vogel, Kathryn Baragwanath, Goldblatt, Ran, Hanson, Gordon H., Khandelwal, Amit K., 2019. Detecting urban markets with satellite imagery: an application to India. J. Urban Econ. doi:10.1016/j.jue.2019.05.004.
Wallace, Jeremy L., 2016. Juking the stats? Authoritarian information problems in China. Br. J. Polit. Sci. 46 (01), 11–29.
Wang, Zhi, Zhang, Qinghua, Zhou, Li-An, 2020. Career incentives of city leaders and urban spatial expansion in China. Rev. Econ. Stat. 102 (5), 897–911.
Wang, Danqing, Zhu, Zhitao, Chen, Shuo, Luo, Xiaowei Rose, 2021. Running out of steam? A political incentive perspective of FDI inflows in China. J. Int. Bus Stud. 52, 692–717.
WikiLeaks. (2007). Fifth generation star Li Keqiang discusses domestic challenges, trade relations with ambassador. https://wikileaks.org/plusd/cables/07BEIJING1760_a.html.
Xi, Tianyang, Yao, Yang, Zhang, Muyang, 2018. Capability and opportunism: evidence from city officials in China. J. Comp. Econ. 46 (4), 1046–1061.
Xiong, Wei., 2018. The Mandarin Model of Growth. NBER working paper, p. 25296.
Xu, Chenggang, 2011. The fundamental institutions of China's reforms and development. J. Econ. Lit. 49 (4), 1076–1151.
Yao, Yang, Zhang, Muyang, 2015. Subnational leaders and economic growth: evidence from Chinese cities. J. Econ. Growth 20 (4), 405–436.
Yu, Jihai, Zhou, Li-An, Zhu, Guozhong, 2016. Strategic interaction in political competition: evidence from spatial effects across Chinese cities. Reg. Sci. Urban Econ. 57, 23–37.
Zhong, Weiguo, Lin, Ya, Gao, Danxue, Yang, Haibin, 2019. Does politician turnover affect foreign subsidiary performance? Evidence in China. J. Int. Bus Stud. 50 (7), 1184–1212.