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Enforcing discipline in China: Countering internal misinformation with public disclosure

Handi Li [a,*], Shengqiao Lin [b], Minh D. Trinh [c]

[a] University College London, Rubin Building, 29-31 Tavistock Square, London, WC1H 9QU, United Kingdom
[b] University of Toronto, Sidney Smith Hall, Room 3018, 100 St. George Street, Toronto, ON M5S 3G3, Canada
[c] The University of Texas at Austin, Batts Hall 3.152, 158 W 21st ST STOP A1800, Austin, TX 78712-1704, United States

HIGHLIGHTS

- Public disclosure without punishment can effectively discipline bureaucrats in authoritarian systems by credibly signaling commitment to curbing wrongdoing.
- Using China's crackdown on local GDP falsification, we show that disclosure of manipulation significantly reduces future data misreporting across affected regions, even when implicated officials face no meaningful career sanctions.
- Disclosure helps break cycles of ongoing falsification, lowers administrative costs of maintaining false data, and generates modest spillover effects to nearby or similar regions.
- Despite reputational risks, transparency improves economic confidence: real activity rises and new business formation increases in areas exposed to disclosure.

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ABSTRACT

Governments around the world need internal discipline to ensure the implementation of policies and delivery of governance outputs, and this need is more severe in authoritarian systems that govern top-down and without bottom-up electoral pressures. Conventional wisdom suggests that autocrats rely on punishment to discipline subordinates who fail to obey orders, but we argue that public disclosure of such malfeasance can also be effective, even when unaccompanied by punishment. In the case of internal misinformation—the tampering of government data and statistics by local agents to distort central authorities' decisions—we find that the Chinese government successfully adopted public disclosure to combat the manipulation of local GDP numbers by lower-level governments. Our evidence indicates that publicly disclosing a prior act of falsification reduced future manipulation and did so at various levels of local governments, even as officials implicated by the disclosure faced no career setbacks. Hinting at downstream impacts, such disclosure also improved confidence among economic actors, evidenced by increased business activity in localities involved in public disclosures.

How may government leaders prevent rule-breaking within the regime's ranks? Internal discipline is important under most governments, but it is critical in non-democracies, where top-down control replaces bottom-up accountability in ensuring the enforcement of government policies and delivery of governance outcomes. Existing literature, primarily informed by research on accountability, emphasizes monitoring and sanctioning as solutions to maintain discipline (see Newson & Trebbi, 2018). This paper, however, argues that authorities at the highest level of government may enforce compliance among their agents with a different strategy, specifically by *publicly disclosing* acts of wrongdoing these agents have committed.

In this study, we investigate the effectiveness of public disclosure in curbing a particularly egregious and intractable form of rule-breaking by regime insiders, specifically the misreporting of internal information within China's government hierarchy. Government agents are frequently tasked with gathering information about society's material well-being or subjective opinions, but they may distort this information before reporting it upward, undermining regime leaders' authority and disrupting downward accountability. In China, internal misinformation manifests through the falsification of routine internal government statistics. Broadly condemned by the Chinese Communist Party (CCP) (e.g., National Bureau of Statistics of China, 2017), statistical misreporting

* Corresponding author.
Email addresses: handi.li@ucl.ac.uk (H. Li), shengqiao.lin@utoronto.ca (S. Lin), mdtrinh@austin.utexas.edu (M.D. Trinh).

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distorts the regime's understanding of government officials' performance and of the local conditions facing citizens, both which interfere with its model of top-down, performance-driven governance. In recent years, the CCP has responded to this transgression by publicly broadcasting high-profile incidents it has detected, pivoting away from a tradition of dealing with rule-breaking behind the opaque shroud of court politics. Why would an authoritarian regime like China disclose internal misbehavior—when such disclosure can expose rifts within the regime, reveal its weaknesses, and cast doubt on all other messages it sends to the public?

Against the grain of three strands of literature—one that highlights the reputational cost facing government authorities if they let the public learn about regime officials' rule-breaking (Della Porta et al., 2013; Dickson, 2016; Sun et al., 2022; Wang & Dickson, 2022; Zhang & Kim, 2018; Zhu et al., 2013), one that doubts the effectiveness of public disclosure in authoritarian contexts where electoral sanctions are absent (Malesky et al., 2012; Seligsohn et al., 2018; Tan, 2014), and one that extols the utility of disciplining elites with punishment (Cai, 2014; Landry, 2008; Tsai, 2021; Tsai et al., 2022)—we argue that public disclosure of malfeasance by government agents can be effective in curbing this malfeasance, and that it can be effective even when unaccompanied by any significant punishment. Precisely because public disclosure results in reputational cost, it allows rulers to credibly signal resolve, to project strength, and—in the case of internal misinformation—to restructure regime actors' incentives away from misbehavior.

Focusing on the fight against local data manipulation in China as a prototypical case of this disciplinary strategy, we show that public disclosure succeeded in curbing future manipulation. Specifically, we use multiple data sources and empirical approaches to demonstrate that the central government's public disclosure of Gross Domestic Product (GDP) misreporting in several localities caused this form of statistical manipulation to decline rapidly in those same localities. Notably, this effect manifested even when local leaders implicated in these disclosures suffered no subsequent sanctions or career setbacks. Yet it was absent when the regime detected misreporting without disclosing it or when the offenders voluntarily revealed themselves. Internal misinformation declined only when the regime both detected such malfeasance and forced public disclosure against the offenders' will.

In addition, we find that the effectiveness of public disclosure is far-reaching. First, it reduces the need for misreporting localities to continuously maintain their lies, pulling them out of a vicious cycle in which each year's exaggeration must be revised downward the next year. Second, tackling wrongdoing through public means may generate positive spillovers, though the evidence is only tentative and the spillover effect is weaker than the direct treatment effects. Finally, although public knowledge of GDP manipulation may damage the regime's reputation among citizens, the commitment signaled by public disclosure may improve confidence among economic actors, as demonstrated by new businesses in localities involved in the disclosures.

Our paper makes three broad contributions. First, to the literature on authoritarian agent control,[1] we unpack the logic behind monitoring and sanctioning and show that the latter may not be necessary as long as the former takes place in plain view of the public. Second, more broadly, we identify another way transparency and disclosure may benefit autocracies (see Egorov et al., 2009; Hollyer et al., 2019; Li, 2026). Finally, our evidence speaks to a discourse on internal data manipulation in non-democracies, which recognizes the extent of this problem (e.g., Carlitz & McLellan, 2021; Holz, 2014; Martinez, 2022; Rawski, 2001; Trinh, 2022; Wallace, 2015) but does not scrutinize whether and how the authorities may tackle it (exceptions include Gao, 2016, 2021).

In the next section, we discuss our theory of government authorities improving discipline through public disclosure. Then, we present the case of falsification in economic statistics in China. The following sections introduce our empirical design and present results that confirm public disclosure of statistical misreporting truly helps reduce this behavior. Finally, we discuss ancillary outcomes of countering manipulation through public disclosure.

1. Theory

In all countries, democratic and authoritarian alike, lower-level officials maintain direct control over statistical bureaucrats whose expertise makes possible both the production and the manipulation of government data (Boräng et al., 2018). In developmental authoritarian states, government leaders build their legitimacy on their ability to make well-informed policy decisions that advance outcomes the public finds desirable (Bueno De Mesquita et al., 2005; Huntington, 1968). However, effective policy decisions require reliable information, and, because autocratic governments tightly restrict the information environment, the data informing these decisions come only from subordinates on lower rungs of the authoritarian ladder. Internal information manipulation occurs when these subordinates distort or even outright falsify data and statistics produced under their supervision before they can reach regime leaders. It is a common issue in central-planning regimes of the past, most notably the Soviet Union (Bergson, 1953; Herrera, 2010). In modern times, it is especially well-reported in China (e.g., Cai, 2000; Rawski, 2001), where the government pursues development outcomes through apparently technocratic practices, such as meritocratic promotion (e.g., Landry, 2008; Li & Zhou, 2005; Zhou, 2007), growth targeting (e.g., Li et al., 2019), or policy experimentation (Heilmann, 2008). Outside China, information manipulation proliferates in developmental autocracies that embrace a top-down, data-driven approach to socioeconomic management, a category that includes cases as diverse as Vietnam (Trinh, 2022) and Rwanda (Heinen, 2022; Sandefur & Glassman, 2015). In other autocracies, information manipulation still manifests the moment dictators seek information from below to make policy decisions from above, such as when they try to contain the COVID-19 pandemic using case and death figures (Adiguzel et al., 2020). Ultimately, and perhaps ironically, it is when autocrats are most keen to govern with data that the quality of that data becomes most suspect.

Internal misinformation is a thorny problem, particularly in non-democracies. Because they have shut down the independent media, suppressed opposition parties, and neutered civil society, autocrats have few reliable information providers besides their subordinates. Inaccurate data exacerbates center-periphery tensions, preventing the central leadership from monitoring and supervising local officials. Sham numbers can also yield real governance consequences, producing flawed economic policies, loss of market confidence, and declining trust in the regime. The possibility that government data may be falsified can alone undermine government authority. Thus, even when dictators have little interest in communicating truthful numbers to their citizens (see Guriev & Treisman, 2019; Hollyer et al., 2019), they may still be keen to ensure that their subordinates are honest with internal data and statistics.

1.1. The logic of public disclosure

Existing literature suggests that government leaders attempt to discipline elite wrongdoings that can bring harm to the regime.[2] Often, discipline goes hand in hand with punishment: by firing, demoting, or halting the promotion of misbehaving officials, the regime can filter these violators out of the system while simultaneously deterring others from future violations (Shotts & Wiseman, 2010). We argue, however, that there are other tools to enforce discipline. In particular, the public disclosure of wrongdoing, even when issued without punishment to the

[1] See Newson and Trebbi (2018) for an overview of the theoretical models of elite control.

[2] Unless the regime may have something to gain from tolerating these wrongdoings, such as enabling corruption as an informal reward for officials (Darden, 2008).

offender, can curb malfeasance and is especially effective in the fight against internal misinformation.

Public disclosure is the act of informing broader audiences, including both government actors and the public, of internal events that have transpired. It is an official act, carried out through channels commonly associated with the authority, such as government websites or state media, as part of routine news cycles, or by extraordinary means such as formal announcements or press releases. Regarding malfeasance by government elites, public disclosure means broadcasting that the leaders have identified the act of wrongdoing, revealing the identity of those who committed it, and what punishment has befallen these violators.

How may public disclosure reduce misbehavior? Related work on exposing corruption suggests it hurts rule-breaking officials at the polls (Ferraz & Finan, 2008, 2011) or increases the expectation of being caught (Avis et al., 2018). But these are unlikely to be the reasons autocrats choose public disclosure to fight data manipulation. First, in non-democracies, electoral channels are muted, the media is controlled, and the separation of powers between auditors, prosecutors, and courts is farcical. The regime leadership needs no elections or prosecutions to remove an official and, if it wishes, can do so with fabricated vote counts or kangaroo courts without revealing any wrongdoing. Second, because it can control information through propaganda and censorship, the regime can communicate a heightened risk of detection to offending officials via nonpublic internal channels. Ultimately, while the importance of publicly exposing wrongdoing is well understood in democracies, it is less so in our case.

Indeed, the disclosure of wrongdoing curbs misbehavior in other ways. Consider the reputational costs of public disclosure: revealing an act of wrongdoing establishes a public knowledge of internal disobedience and undermines government authority (Sun et al., 2022; Wang & Dickson, 2022); it may lead observers to suspect that even more malfeasance is occurring (Zhu et al., 2013).[3] With regard to government data, public knowledge of *some* misinformation may reduce public trust in *all* official messaging. However, these immense reputational costs can send a costly signal of its commitment to combat misbehavior. Indeed, a ruler willing to accept the bad optics of internal problems must also be keen to resolve them; deploring some malfeasance in public also commits the government to not reverse its stance in the future.

The costly signals of public disclosure make it a credible substitute for punishment, but, in the case of information misreporting, they may become a complement to leniency. Note that manipulating a certain number benefits an official only when the regime uses this data to distribute power and resources, and it benefits only those who manipulate it better than the rest. Individual officials have limited choices over which data to manipulate and how much or little to manipulate it; instead, they face diminishing marginal returns and increasing marginal costs in an arms race of increasingly outrageous lies that require increasingly sophisticated techniques to conceal. Second, once an official has been falsifying a number for many years, an abrupt halt would disrupt the time series, betraying the extent of prior manipulation even before the higher authority could detect it. Thus, even when marginal costs exceed marginal returns, officials afraid of being found out may still find it hard to stop. Contrast this with other forms of wrongdoing, such as bribe-taking: a corrupt official can accept any amount of bribe he or she deems safe to accept, still secure a discrete benefit from even a small offer, and may always stop accepting bribes without exposing past dealings.

A promise of temporary leniency negates these consequences of stopping and offers an off-ramp for those who no longer find the spiraling costs of misinformation worth the uncertain gains.[4] And yet, such a promise would not be credible unless the regime leadership makes clear that it chooses not to punish rather than fails to do so. Publicly broadcasting wrongdoing incidents even when few wrongdoers face retribution signals that the ruler has nothing to hide, indicating a deliberate decision to forbear rather than an inability to discipline.

Ultimately, even though the literature has established that punishing wrongdoing and broadcasting such punishment generates utility for government leaders (e.g., Shotts & Wiseman, 2010; Tsai, 2021; Tsai et al., 2022), we argue that broadcasting alone can reduce violations by communicating a complex temporal discontinuity: although violators may enjoy immediate leniency, future offenders must expect severe sanctions from an authority committed to fighting malfeasance. In the case of internal misinformation, this message motivates violators to cease lying immediately.

2. Local GDP manipulation in China

Like other bureaucratic agencies, the statistical reporting system in China is subject to both vertical and horizontal accountability. Each level of government, from the county onward, has a bureau of statistics that, until 2018, collected, analyzed, and disseminated data for its jurisdiction, while the National Bureau of Statistics (NBS) performed the same function at the national level. Vertically, each statistical bureau supervises all its counterparts one level below. Horizontally, each level of government, headed by the local CCP secretary, controls the budgets and personnel of local bureaucratic agencies, including the statistical bureau (National Bureau of Statistics of China, 2015). This "dual leadership" allows local governments to manipulate the work of statistical agencies under their jurisdiction (Holz, 2014).

Statistical figures, especially subnational GDP figures, matter greatly to local government officials. The Chinese central government has been known to use a "promotion tournament" to identify, select, and promote local officials who are competent in promoting the local economy (Landry, 2008; Li & Zhou, 2005; Xi et al., 2018; Yao & Zhang, 2015; Zhou, 2007) and compliant with developmental goals handed down from above. GDP numbers serve as a key measure for this tournament. Indeed, existing empirical evidence from various levels of administration shows that local officials presiding over higher GDP growth are more likely to be promoted (Landry, 2008; Landry et al., 2017; Li & Zhou, 2005).

Given this, it is unsurprising that empirical evidence finds local officials routinely tampering with government statistics (Cai, 2000). A frequently observed tell-tale sign of such manipulation is that the sum of the provincial GDP—calculated under provincial governments' supervision—always greatly exceeds the national GDP—calculated by the NBS. Furthermore, by comparing reported GDP with critical economic indicators, such as nightlights, electricity consumption, and freight, scholars have also shown that statistical manipulation is prevalent and highly correlated with local leaders' promotion incentives (Chen et al., 2021; Jiang & Wallace, 2017; Wallace, 2015). In addition, the Chinese central government's inspections have identified specific instances of artificially inflated numbers. Many local governments have been found to force firms to report false data, double-count economic activities in calculations, or intentionally misattribute products produced across jurisdictional boundaries to themselves (Wang, 2019).

Although some autocrats falsify *national* GDP figures at the central level for their own benefit (see Martinez, 2022), the fabrication of

[3] Even in democracies, exposing corruption may reduce corruption (Ferraz & Finan, 2011) and improve the economy (Colonnelli & Prem, 2022) but it may also undermine political participation (Chong et al., 2015).

[4] Like forbearance (Holland, 2016), public acknowledgment of forgiveness convinces regime elites that the ruler is empathetic with their struggles. Yet unlike forbearance, which generates net electoral gains (Holland, 2016), forgiving elite malfeasance in an authoritarian context is ultimately costly, and this allows the authority to credibly commit to *not* remain forgiving forever, thereby deterring continued violations.

local GDP numbers by lower-level officials presents vexing governance problems. Statistical misreporting exacerbates information asymmetries between the central leadership and local actors, undermining the authority's ability to monitor its subordinates' competence and compliance. Where central planning still determines resource allocation, falsified statistics can have fatal economic consequences, as occurred in China during the catastrophic Great Leap Forward. Even beyond governance decisions, perceptions of misleading government statistics can generate distrust in the government and its public messaging. The Chinese government recognizes all these costs. Indeed, an NBS Deputy Commissioner publicly noted that "the irregularities of local GDP data […] harm the understanding of local economic status, impede the regulation of the macroeconomy, and damage people's confidence in the government's statistics" (National Bureau of Statistics of China, 2017).

Tampering with government statistics is not cost-free for local governments either. Fake numbers can lead to real economic consequences. For example, a locality that exaggerates its economic well-being may end up having to collect more taxes. The misreporting activities also distort local economies. To falsify stellar economic achievements, local governments must inflate the economic performance of certain firms or sectors in the local economy. The low-hanging fruit can come in two forms: underperformers whose numbers leave ample room for upward adjustment, or state-owned entities dependent on the local government's goodwill and thus vulnerable to its pressure. Statistical manipulation can make some firms or sectors appear to perform much better than reality, leading to unfair competition for investments or contracts. This distortion hurts better-performing firms and private businesses, impeding local economic development.

Another cost is the significant administrative expenditure needed to maintain the lies. Numbers like GDP are reported at regular intervals. Consecutive numbers form trajectories and patterns from which deviations may attract attention: if a locality were exaggerating its GDP for some periods and then decided to stop, the resulting drop would appear as a failing performance, or worse, draw attention to the entire time series and betray the past misreporting. Thus, if they lied in the past, local governments must continue to do so in the future. Left unchecked over the long run, even a few percentage points of exaggeration each year can cause reported numbers to outpace reality by an absurd amount.

To mitigate this long-term problem, many local governments expend significant administrative efforts to revise downward the GDP numbers they had previously falsified upward. Starting from 2004, the calculation and reporting of local GDP statistics in China occur in three rounds: an initial "preliminary calculation" (*chubu hesuan*) round in late January, producing numbers for government and media reports; a "preliminary verification" (*chubu heshi*) round taking place months after and producing revised numbers for local statistical yearbooks; and a "final verification" (*zuizhong heshi*) round finalizing the numbers for the official NBS database (National Bureau of Statistics of China, 2003). Numbers for the first two rounds come out early in the year and receive public attention. They are the basis for official performance evaluation and for media headlines comparing and even ranking the GDP levels and growth rates across sub-national units. In contrast, figures from the final round rarely make news, yet are entered into the official database as the baseline for growth rate calculations. Local officials would therefore exaggerate GDP figures in the earlier rounds but quietly adjust them downward later. This is consistent with the pattern that figures in later rounds of calculation are systematically smaller than those in earlier rounds, and with reported cases of such adjustments (BBC News Chinese, 2024). This entire charade requires painstaking efforts throughout the entire year, comes at the expense of improving data quality, and, because it would not have been necessary without the lies, is a deadweight loss that the provinces themselves must absorb.

3. Fighting statistical manipulation with public disclosures

Given the significant challenges posed by statistical falsification, the Chinese regime unsurprisingly attempts to keep it under control. A viable but crude option was to reduce manipulation incentives by removing GDP from the official evaluation criteria. This tactic has been discussed in the central government's official statements, including several made by President Xi Jinping himself. Ultimately, however, the regime cannot commit to ignoring economic statistics because its legitimacy requires maintaining top-down control over economic performance (Gao, 2021). As another solution, the central government carried out nationwide inspections of local statistical work in the early 2000s, which revealed plenty of local statistical misreporting but still failed to stop the problem. The inspections were also fiscally costly (Zhu, 2023). More recently, the NBS launched another reform in 2008, establishing an online system for firms to report raw data and bypass tampering by local officials. Although this measure helped increase the reliability of the raw data (Gao, 2016), it still left room for manipulation during the calculation of statistics. Furthermore, local governments can exert pressure on firms to input figures favorable to the officials, which have been widely reported in media and by policy observers.

After previous reform attempts failed to eliminate GDP manipulation and inspections failed to deter ongoing violations, the Chinese central government took steps to publicize cases of manipulation it had previously detected. Previously, inspections and sanctions were essentially closed-door affairs; the only exceptions were delayed publications that anonymized the offending localities and the officials involved. This changed in the mid-2010s, when data manipulation incidents began to make national news.

The case of Liaoning illustrates China's new public disclosure strategy. In 2014, after an inspection detected severe "irregularities" consistent with data falsification in the province's statistical reports, the central leadership responded by privately reprimanding the provincial leaders, who then promised to rectify the problem, only for inspectors to find irregularities again in 2016. Unlike in 2014, however, Beijing publicly announced the inspectors' findings in the state media. The announcement takes the form of a January 2017 declaration by the provincial governor stating that his government had overreported GDP and other local economic statistics for years. This marked a pivot from its typical practice of expressing displeasure only internally.

Proving that it was not a one-off decision, the CCP repeated this strategy in Inner Mongolia and Tianjin. Reports about Inner Mongolia were published in January 2018, with the provincial government acknowledging that it had inflated its 2016 GDP, particularly its industrial production figures. Around the same time, state media reported that a district in Tianjin had admitted to falsifying its 2016 GDP. In all three cases, Beijing relied on the state media to disseminate news of the falsification. As the central government's mouthpiece, the official state media reaches a wider audience than any other platform, and its stories are frequently republished by mainstream media outlets and social media, thus reaching significant swaths of audience. Importantly, although they appear to be proactive "confessions" by the local governments, all three disclosures were a result of pressure from the central government. In Liaoning, the misbehavior was publicized not when it was initially discovered but only after pressure from the central government. Similarly, the government of Inner Mongolia had been named internally by the inspection group in 2017, prior to its formal "confession."

And yet, despite all the publicity given to these cases, throughout the 2010s and early 2020s, the central government issued virtually no substantial punishment to any local official found guilty of statistical falsification. The most straightforward punishment for a rule-breaking official would be firing, dismissal, forced retirement, or demotion. Practically none of this happened to the leaders—defined as party secretaries or governors/mayors—who presided over localities implicated in

publicized misinformation incidents. Indeed, we detail the subsequent career paths of these 13 officials in Table A6 in Appendix A and find that seven were promoted, five were transferred laterally, and only one was dismissed—for a different offense.[5] More formal analysis of every provincial official from the entire 2013–2018 period, presented in Table A5 in Appendix A, further confirms that being named an offender in internal misinformation cases influenced neither the probability of promotion nor the desirability of subsequent postings, even after controlling for official-level characteristics. Among rank-and-file bureaucrats, we also found most penalties to be light, ranging from warnings (*jinggao*) to admonitory talks (*jiemiantanhua*) or records of demerit (*jiguo*). Even the media pointed out that the punishments are "soft, delayed, or not actually implemented" (Zhu, 2023). Ultimately, although we cannot rule out unobservable sanctions, the fact that local leaders continued to climb the hierarchy unhindered and local bureaucrats remained unscathed suggests that such sanctions were inconsequential at best. In any case, to the public and regime officials alike, it is clear that the Chinese leadership was content for the public to know about the uncovering of statistical falsification even without any major disciplinary responses.

4. Empirical design

Our theory posits that revealing rule-breaking within an authoritarian regime sends a credible signal that the authority is committed to preventing future offenses, and even when such a revelation comes without evident sanction upon identified offenders, it can still persuade regime subordinates to stop their violations. It leads us to predict that the public disclosure of internal statistical manipulation incidents, such as those issued by the Chinese central government, would reduce such misbehavior. We verify this prediction using an empirical approach similar to the difference-in-differences (DiD) method, drawing on data from China at both the provincial and prefectural levels. This approach seeks to demonstrate that *treated* localities targeted by public disclosures experienced a drop in statistical falsification compared to *control* localities, but only after such disclosures occurred.

Our primary empirical challenge is selection bias in treatment assignments: whether the central government detects an act of statistical manipulation and subsequently discloses it depends on various characteristics of the localities, including the prevailing extent of manipulation. The timing of treatment also depends on time-varying factors, e.g., the authority may intervene once a locality has reached a certain level of development or when statistical falsification in that locality has exceeded a certain threshold. To account for non-random assignment to disclosure, we rely on fixed effects and a carefully chosen set of time-varying pretreatment covariates that justify the assumption of conditional independence. Effectively, we use localities that are similar in terms of the level of misreporting and pretreatment covariates but did not experience disclosures as counterfactuals for localities implicated in actual disclosures, all while accounting for all unobserved time-invariant differences and common shocks. Meanwhile, time-varying covariates capture within-unit, over-time variations that may affect the timing of public disclosure; they also improve precision by absorbing some variation in the outcome.

We run parallel analyses at the province and prefecture levels. In both, we estimate the following model:

$$Misreporting_{it} = \beta * Disclosure_{it} + X_{it}^{T}\gamma + \mu_i + \epsilon_t + \sigma_{it}$$

where $Misreporting_{it}$ refers to the level of GDP misreporting in a province (or a prefecture) i in year t, $Disclosure_{it}$ denotes whether the place receives a disclosure of GDP manipulation in year t, and X_{it}, μ_i, and ϵ_t are a set of control variables, province (prefecture) fixed effects, and year fixed effects, respectively. Because manipulation disclosures did not occur in the same year across the targeted localities, our estimation is equivalent to a staggered DiD.

Our control variables X_{it} include time-varying covariates that might affect the structure of local economic growth and reported statistics (Wallace, 2015), including the pretreatment annual growth of GDP (for provincial analysis only), and GDP per capita (logged). Because many governments that manipulate GDP numbers also tamper with their fiscal revenues, we also control for fiscal revenue per capita (logged). In addition, we control for the share of the primary industry to account for the reliance on agriculture versus industry and service. These control variables are chosen because they may affect the timing of treatment, but also because they may absorb variation in statistical falsification independent of what public disclosure may influence, thereby improving precision. Appendix B.8 justifies in greater detail our choice of these control variables. Data for these variables is derived from provincial statistical yearbooks. We cluster standard errors at the provincial level for the province model and at the prefecture level for the prefecture model to ensure the cluster levels match the sampling and treatment levels of our models (Abadie et al., 2023).

The DiD framework helps address the selection of public disclosure, but it may not be perfect. To enhance confidence in our parallel trends assumption, we adopt a wide range of DiD estimation methods. Specifically, we employ various estimators introduced by Sun and Abraham (2021) (henceforth SA), Callaway and Sant'Anna (2021) (henceforth CS), De Chaisemartin and d'Haultfoeuille (2020) (henceforth dCdH), Borusyak et al. (2024) (henceforth BJS), plus the fixed-effects counterfactual estimator (FECT) by Liu et al. (2023).

We further bolster our analysis with the Generalized Synthetic Control (GSC; Xu, 2017) and Panel Matching (Imai et al., 2021). The GSC analysis uses a weighting strategy to construct synthetic counterfactuals, with weights computed based on pre-treatment outcomes and covariates. Leveraging pre-treatment outcomes is helpful in two ways. First, the prevailing level of GDP misreporting in a locality is probably the most important determinant of whether and when the central government responds to such misreporting. Second, if any other unobserved time-varying covariate could have influenced treatment assignment, it must have also influenced the observed outcomes in the pre-treatment period, such that accounting for the latter sufficiently accounts for the former. Ultimately, properly constructed synthetic controls match treated units exactly on all pre-treatment outcomes, which strongly implies parallel trends, if not exact matching, in post-treatment potential outcomes. Meanwhile, the panel-matching approach matches treatment and control cases with the same treatment history, with refinement using pretreatment covariates. The intuition is that if non-random but unobserved factors drive selection into treatment, their impact should manifest in when treatment happened and when it did not, allowing matching on entire treatment histories to balance their influence.[6]

4.1. Measuring data manipulation

We measure statistical misreporting in two ways. Our first measure is the gap between a province's reported GDP and the combined GDP of its prefectures. Although a higher-level locality's GDP need not equal the sum of its lower-level constituent units' GDPs, large discrepancies between the two figures are often attributed to perverse manipulation incentives that differ across administrative levels. This metric is widely cited by scholars, the media, China observers, and even Chinese authorities (e.g., Chen et al., 2019; China Daily, 2013). Specifically, we calculate the difference between the provincial GDP published by the NBS (from

[5] In this dismissal, that of Xinguo Huang, then-governor of Tianjin, the disciplinary statement explicitly cited more serious corruption charges he had separately committed and made no mention of economic data manipulation.

[6] Like the GSC, panel-matching does not rely on time-varying covariates and is not vulnerable to post-treatment bias, allowing both to complement our other approaches.

the third "final verification" round) and the sum of prefectural GDPs obtained from provincial statistical yearbooks (in the second "preliminary verification" round), and then divide the difference by the provincial GDP. This measure captures misreporting as a proportion of the provincial economy. We choose the final verification as the baseline for two reasons: first, this version of GDP numbers is scrutinized and released by the NBS, so it is less subject to local manipulation than earlier versions that are self-reported by local governments. Second, it comes out a year later than other versions and receives relatively less public attention, giving local governments no incentive to manipulate. We calculate this measure for 24 provincial-level units over the 2004–2019 period.[7]

$$\text{Misreport}_{\textbf{GDP gap}} = \frac{\sum \text{prefectural GDP} - \text{provincial GDP}}{\text{provincial GDP}}$$

Our second measure benchmarks reported GDP growth against nighttime luminosity, an objective indicator of actual economic activity. Because nightlight data is produced by U.S. satellites and thus untouched by local actors, it is useful as an independent proxy for economic activity (Chen & Nordhaus, 2011; Henderson et al., 2012; Min, 2015) and for detecting manipulation in official statistics (Chen et al., 2021; Jiang & Wallace, 2017; Martinez, 2022; Trinh, 2022).[8]

Researchers have used nightlight to measure GDP falsification in several ways. The most straightforward approach compares levels to produce time-varying measures. For instance, Chen et al. (2021) uses nightlight levels to predict the "true" GDP levels for each unit-year, then takes the difference between the official numbers and this prediction. The prediction step is necessary because nightlight and GDP are measured in different units and on different scales, so one must be converted to the other for any comparison. However, such conversion requires identifying training units with little to no manipulation, plus assuming that external factors influencing the GDP-nightlight conversion—even the use of LED vs. incandescent bulbs (Elvidge et al., 2022)—are consistent across units and over time. A second approach models the mapping between nightlight and GDP, then identifies manipulation as an abnormal influence on top of this mapping (e.g., Martinez, 2022; Trinh, 2022); this influence could either be multiplicative or additive, with different modeling implications (Martinez, 2022). This approach is agnostic about and does not directly estimate the determinants of nightlight-GDP mapping; instead, it identifies an honest baseline, estimates a reduced-form (often linear) mapping for this baseline, and then calculates how much the suspected liars' reduced-form mapping diverges from it.[9] It allows for factors linking nightlight and GDP to fluctuate, but this fluctuation must not overlap with the treatment of interest. A bigger constraint still is that it cannot produce unit-year-level measures, as multiple data points are needed to estimate each relevant mapping. Finally, the need for an honest baseline makes it inapplicable to Chinese prefecture-level cities, none of which can be a priori acquitted of falsification.

Our measure follows a third middle-ground approach taken by others (e.g., Zeng & Zhou, 2024) and captures GDP misreporting by comparing GDP and nightlight growth rates. Growth rates are substantively meaningful targets for manipulation and enable unit-year-level measures useful for our analysis. It is also intuitive and directly comparable with our GDP gap measure. Technically, comparing growth rates implicitly assumes a linear mapping between nightlight and GDP; given two-way fixed effects, this mapping is allowed to fluctuate so long as it remains consistent within the same unit or across all units over time. As explained in Appendix B.7, this approach is actually similar to Martinez (2022), with the primary difference being additive rather than multiplicative falsification. Considering GDP falsification as scalars added to the real number, rather than as factors proportional to the true growth, is important in China, where baseline growth is often high, and the lies are often calculated with regard to the scalar gap between each locality and its growth target. More importantly, as Appendix B.7 demonstrates, when no honest officials can be assumed, only a model based on additive lies can identify a treatment effect. Ultimately, the risk of non-classical measurement errors remains nontrivial, but it is not substantially more severe than under alternative approaches and is entirely acceptable given the need for a time-varying measure of GDP misreporting. Additionally, in Table B8, we show that alternative derivations yield similar results without the interpretable intuition of our nightlight gap measure:

$$\text{Misreport}_{\textbf{nightlight gap}} = \text{prefectural GDP growth} - \text{prefectural nightlight growth}$$

4.2. Public disclosure of manipulation

We identify public disclosure of statistical misreporting by looking at whether a place was named and criticized for manipulating GDP data in government announcements or news disseminated by central government-operated official media. We searched through government newsletters, news reports, and editorials from two major central official news agencies, Xinhua and People's Daily. An Internet search found no additional cases beyond these government sources, and we confirmed that the identified disclosure news had been widely disseminated by other popular mainstream media outlets. Ultimately, there are three provinces and three prefectural cities for which statistical falsification incidents were published: Liaoning (2017), Inner Mongolia (2018), Jilin (2018), Hengyang and Changsha in Hunan Province (2015), as well as Shuozhou in Shanxi Province (2018).[10] For the province-level analysis, we treat the three provinces that publicly disclosed data manipulation as the treated group. For the prefecture-level analysis, the treated group includes all prefectures within these three provinces as well as three additional prefectures with publicly disclosed data manipulation cases (i.e., Hengyang, Changsha, and Shuozhou). All other provinces and prefectures serve as the control groups.

5. Main result: disclosure reduces manipulation

At a quick glance, the public disclosure of GDP misreporting incidents appears to have quite a significant impact on the pattern of statistical falsification. Prior to the disclosures, the sum of the GDPs of all the prefectures within a province adds up to, on average, 8 percent higher than the province's GDP; this gap is particularly severe—43 percent—in the three treated provinces, perhaps explaining why these

[7] We exclude the four centrally administered municipalities (Beijing, Tianjin, Chongqing, and Shanghai), as well as provincial-level units such as Xinjiang, Tibet, and Hainan, where lower-level GDP data are not systematically disclosed using comparable measures.

[8] Nightlight data comes from two primary series: DMSP-OLS, which runs from 1992 to 2013, and VIIRS, available from 2012 onward (Elvidge et al., 2013; Gibson et al., 2021). The VIIRS series has a higher quality, but we intercalibrated it with DMSP-OLS to match the latter's standards, arriving at a consistent series covering the entire 2004–2019 study window (for other intercalibration efforts, see Li et al., 2021; Nechaev et al., 2021; Tang et al., 2025; Tu et al., 2020; Zheng et al., 2019). Both the input and output data have been processed to account for cloud cover and filter out incidental lights such as gas flares. Our robustness check in Appendix B.6 verifies that alternative intercalibration methods yield similar results.

[9] Martinez (2022) sets democracies as the baseline to measure falsification by non-democracies; Trinh (2022) sets Vietnam's capital as the baseline to measure falsification by the remaining provinces.

[10] Jiang and Wallace (2017) covers three additional cases. The first, Heihe, was reported to have manipulated its investment numbers but not its GDP. Since the news could still have led people to infer there was GDP manipulation, we included this case in our robustness check but found no change in our results. The second, Qujing, was reported by nonstate media but could not be found in any original article or reprint. The third, Lianyungang, was also reported by nonstate media but subsequently refuted by state media. We excluded these two latter cases.

Table 1
Main effects of disclosure across Estimators.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Estimator	TWFE		SA	CS	dCdH	BJS	FECT
Panel A: City-Province Gap (SD)							
Disclosure	−1.212***	−1.179***	−1.399***	−1.348***	−1.338*	−0.522***	−1.164***
	(0.140)	(0.284)	(0.216)	(0.260)	(0.521)	(0.148)	(0.287)
Observations	384	384	384	384	384	384	384
Pre-treatment Outcome Mean (pp)							
Control	3.09 (SD: 10.14)						
Treated	43.04 (SD: 15.12)						
Panel B: Nightlight Gap (SD)							
Disclosure	−0.611***	−0.538**	−0.485**	−0.580**	−0.698***	−1.225***	−0.508**
	(0.171)	(0.176)	(0.147)	(0.182)	(0.199)	(0.323)	(0.170)
Observations	4497	4479	4479	4479	4479	4479	4479
Pre-treatment Outcome Mean (pp)							
Control	0.66 (SD: 31.68)						
Treated	8.44 (SD: 18.14)						
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Province/City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: All outcomes are standardized (z-scored over the pooled sample), so coefficients are in standard-deviation units. City-province gap = 100 × (city GDP sum − provincial GDP)/provincial GDP (1 SD ≈ 16.2 pp). Nightlight gap = official GDP growth − nightlight growth (1 SD ≈ 38.9 pp). Column (1) reports TWFE without controls; columns (2)–(7) include controls (province: GDP growth, log GDP per capita, log revenue per capita, log population; city: log GDP per capita, log revenue per capita, log population). Column (5) reports the dCdH average ATT across effects 0–2. Pre-treatment outcome means are reported for never-treated (control) and eventually-treated units (treated). Standard errors clustered at province/city level in parentheses. Signif. codes: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

provinces were selected for the public disclosure treatment. Following the public disclosure, this gap shrank to 12 percent in the treated provinces and 2.5 percent across the entire sample. The nightlight-based measure paints a similar picture. Starting from an average of 3 percentage points across all prefectures in our sample and 8 percentage points in the treated ones, the difference between GDP and nightlight growth shrank to −1 and −35 percentage points, respectively, following the disclosures. That the gap turned negative is highly indicative of overcorrection, which is necessary if a city wants to bring its reported GDP closer to reality after long periods of exaggeration. Less straightforward is the fact that GDP misreporting also decreased in the untreated localities, suggesting either a global downward trend or potential spillover effects, both of which can complicate a before-and-after comparison. That falsification was more serious in the treated provinces alone further calls for the inclusion of pre-treatment information in our analysis. We thus turn to our difference-in-differences models to obtain more accurate estimates of the effects of public disclosures.

Table 1 presents our main results. Panel A reports results from our province-level analysis, for which the unit of analysis is the province-year and the measure of misreporting is the discrepancy between the province's GDP number and the sum of its prefectures' GDPs. Panel B reports city-level results, with misreporting captured by our nightlight-based measure. All outcomes are standardized as z-scores over the pooled sample, so coefficients are in standard-deviation units. We employ seven different difference-in-difference estimators, each with its own strengths, to maximize robustness. Confirming our theory, estimates for the effect of public disclosure on GDP misreporting are consistently negative and statistically significant across both panels and across all seven estimators.

Specifically, in both panels, Columns (1) and (2) present TWFE estimates without and with controls (log GDP per capita, log revenue per capita, and log population for cities; plus GDP growth for provinces). Following a public disclosure of falsification, misreporting shrank by 1.18–1.21 standard deviations (SD) in treated provinces and by 0.54–0.61 SD in treated cities. Column (3) uses the SA interaction-weighted estimator designed for staggered treatment adoption, and finds a 1.40 SD drop in treated provinces and a 0.49 SD drop in treated cities. Column (4), using the CS estimator, yields similar magnitudes: −1.35 SD for provinces and −0.56 SD for cities.

Columns (5)–(7) present additional heterogeneity-robust estimators. Column (5) uses the dCdH approach and finds effects of −1.34 SD (province) and −0.70 SD (city). Column (6) applies the BJS imputation estimator, yielding −0.52 SD for provinces and −1.23 SD for cities. Column (7) reports the FECT estimator from Liu et al. (2023), finding −1.16 SD (province) and −0.51 SD (city).[11] We also use the GSC (Xu, 2017) for the province panel and Panel Matching (Imai et al., 2021) for the city panel; results are qualitatively similar and reported in Appendices B.1 and B.2. They are also robust to various alternative specifications, including those that exclude under-reporting localities (Appendix B.3), use different city-level controls (B.4), exclude extreme outliers (B.5), or leverage different misreporting measures and nightlight intercalibration methods (B.6).

Overall, the results from Table 1 paint a consistent picture of negative and statistically significant estimates. That the estimates agree despite the diverse range of estimation procedures is reassuring, but for them to truly reflect the causal effect of public disclosure on GDP falsification, the key identification assumption—the parallel trends assumption—must be validated. The threat to identification is certainly non-trivial, as our assessment of pre-treatment levels of misreporting at the beginning of this section shows—and Appendix B.8 validates—that treated localities differ in key baseline characteristics, including the level of misreporting. And yet, Fig. 1, which presents an event-study for our analysis, demonstrates that the parallel trends assumption remains uncompromised. It shows estimated treatment effects separately for each of the three years before and after treatment has actually taken place—using the pre-treatment year as the baseline. The effects are estimated from five staggered DiD estimators: TWFE, SA, CS, dCdH, and BJS. If

[11] To address concerns that GDP had originally been underestimated, we exclude observations with extreme outliers ($|z| > 3$ and $|z| > 2.5$). Results remain significant (Appendix Table B4).

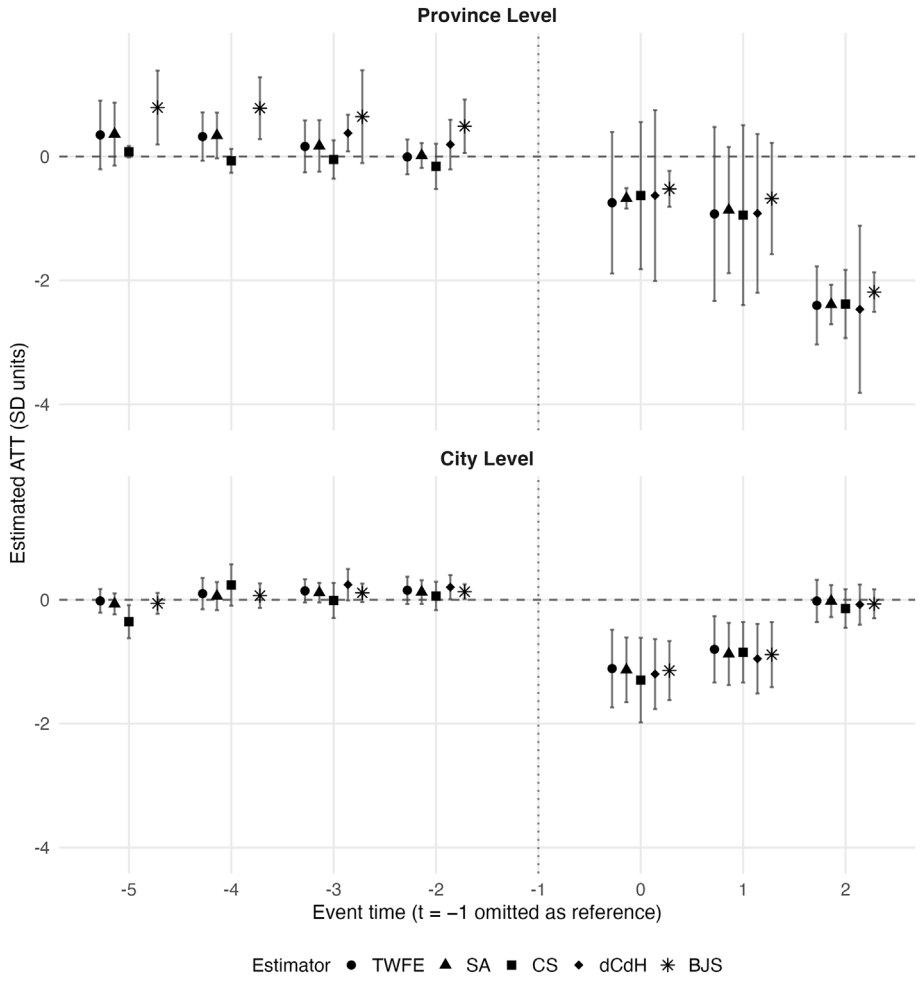


Fig. 1. Province and city-level event studies across five estimators.

the parallel trends assumption holds for an estimator, we should observe zero treatment effects in all pre-treatment years, suggesting no evidence of a pre-trend. This is largely what we find in Fig. 1, which shows that most pre-treatment effects are small and close to zero, in contrast to negative and statistically significant post-treatment effects. Two estimators, specifically dCdH and BJS, exhibit pre-treatment deviations that, although small, support our decision to prioritize the TWFE, SA, and CS estimators in our main models.[12]

5.1. *Did misreporting really decrease? closer look at industrial statistics*

Results from Table 1 show a marked narrowing in the gap between benchmarks for a locality's GDP and the sum of reported GDPs from its constituent parts as well as between a prefecture's nightlight intensity and its reported GDPs. To validate our interpretation of this narrowing as evidence of declining GDP misreporting, we examine the "ground zero" of statistical manipulation in China—the industrial sector. Widely recognized by the Chinese public, Chinese local governments often falsify industrial statistics and GDP figures. Not only are industrial figures questioned by scholars (e.g, Ma et al., 2014), but cases of manipulation have also been widely reported by the media. Indeed, almost all the news reports on internal misinformation we have identified involve charges of manipulating industrial statistics. Much less criticism has been leveled against numbers from the agricultural or service sectors. In this sense, most of the decline in GDP manipulations must entail a reduction in industrial statistics. In other words, if the narrowing of the GDP gap we observed in Table 1 truly reflects a decrease in GDP manipulation, public disclosure should also lead to decreases in figures for the industrial sector.

We test this prediction using granular statistics from our prefecture-level dataset. Table 2 presents the effects of public disclosure of manipulation on the proportion that each of the primary, secondary, and tertiary sectors—corresponding to agriculture, industry and construction, and services—contributes to the city's official GDP figures. Consistent with our hypothesis, the results show that public disclosure triggered slight increases in the shares of the primary sector and the tertiary sector and, most importantly, a substantive decrease in the share of the secondary sector.

Further validating this finding, we present, in Appendix C, evidence of a downward revision of industrial *statistics* but not a corresponding decline in industrial *activities*. Table C1 shows the impact of disclosing incidents of manipulation on various statistics related to the activities of large industrial enterprises.[13] We find public disclosure leads to significant declines in these enterprise statistics, which should have been stable during our time of study. In contrast, we find no evidence that the level of local production activities is affected by misreporting disclosure. Resource usage figures provide reliable proxies for industrial activity;

[12] Additional event studies using other outcome variables, presented in Appendix B.9, offer even further validation, specifically for the CS estimator.

[13] Industrial enterprises with revenue greater than 20 million RMB are required to report statistics about their environmental impacts or supply chain security.

Table 2
Reduced falsification at the city level.

	(1) First Ind. Share (%)	(2) Second Ind. Share (%)	(3) Third Ind. Share (%)
Disclosure	1.947***	−5.279***	3.327***
	(0.576)	(0.906)	(0.849)
Observations	4399	4399	4399
City & Year FE	Yes	Yes	Yes
Pre-treatment Mean			
Control	13.71	47.64	38.65
	(8.91)	(11.33)	(9.77)
Treated	12.69	50.42	36.89
	(7.84)	(8.51)	(7.49)
Pre-treatment diff.	1.02	−2.78	1.75
	(11.87)	(14.18)	(12.31)

Note: All columns use TWFE with city and year fixed effects. City sample covers 2004–2019, excluding four municipalities. Columns (1)–(3) report the effect of disclosure on industry composition (percentage-point shares; 2017 values use district-level data as the city-level yearbook table was not published that year). Pre-treatment outcome means are reported for never-treated (control) and eventually-treated units (treated). Standard errors clustered at city level are in parentheses. Signif. codes: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

importantly, local governments have no incentive to over-report these figures. We find no impact of disclosure on multiple main resource usage figures (Table C2). In short, disclosure reduces the reporting of industrial activities, but not the activities themselves. This confirms that the primary impact of disclosure is on misreporting behaviors.

5.2. How disclosure reduces misreporting

The public disclosure of internal misinformation in China may be considered a bundled treatment with three elements. First, the treatment is a disclosure—it involves the public identification of those found by the central government to have violated statistical integrity. Second, for a locality to be disclosed as manipulating government statistics, it must have been inspected by the central government before the manipulation was published. Finally, some local governments were pushed to admit to their manipulation before the central government would go public with the results of its inspection. Whether the reduction in statistical misreporting comes from the disclosure itself, the central inspection, or the admission of guilt warrants closer examination but is difficult to disentangle. In Appendix D, we probe this problem with two suggestive tests.

In the first test, we examine whether the observed effect is attributable solely to the discovery of fraudulent activities during inspection. We collect reports from an NBS-run platform detailing instances of local economic data falsification. Although the platform is publicly accessible, it was established primarily for internal use. Many of the reported cases were not published by any official or other mainstream media and were therefore not widely disseminated to the public. We also included cases discovered between 2013 and 2019 that were published in mainstream media only after 2021. Overall, we identified seven prefectures falling into these two categories. Table D1 compares the effects of inspection and disclosure. Across specifications, the disclosure coefficient remains negative and statistically significant, whereas the indicator for undisclosed discoveries is not consistently negative. These patterns suggest—tentatively, because the endogeneity between disclosure and discovery cannot be entirely disentangled—that discovery alone may not be sufficient.

Second, our result may reflect that only local officials with the strongest motivation to correct their wrongdoing would have allowed themselves to be identified in the public disclosure. To address this question, we note that, in our treated provinces, only Liaoning and Inner Mongolia admitted to tampering with their economic figures, while the rest neither admitted to nor publicly responded to criticism from the official media, suggesting that these two provinces may have been more forthcoming about their misbehavior. However, Table D2 shows that excluding cities in these two provinces and limiting the analysis only to localities that appeared less voluntarily in the public disclosure does not eliminate the negative post-disclosure effect. Again, these findings are far from conclusive, but they may tentatively suggest that the main effect is not entirely due to voluntary admission.

6. Broader impacts of public disclosure

In this section, we document the additional impact of public disclosure beyond the immediate reduction in falsification in the localities involved.

6.1. Reduction in administrative costs

Above, we alluded to the marginally increasing cost that statistical falsification brings to the local governments that commit it. A major component of this cost comes from the administrative efforts needed to maintain the charade. Local governments are bound by their past behavior, such that if they had exaggerated one year's GDP, they would have to inflate the following year's, too, perhaps by an even greater extent; otherwise, a sudden drop in GDP would betray their lies. Increasingly outrageous exaggeration could get out of hand quickly, but local statisticians in China preempted this by engaging in an elaborate process of statistical adjustment, escalating the degree of upward manipulation during the first round of statistical reporting but quietly following up with downward revisions in later rounds. This charade certainly entails significant administrative efforts and generates little substantial improvement in data quality.

The disclosure of manipulation eliminates the need for additional statistical adjustment. Fig. 2 shows that the degree of revision to provincial GDPs between the primary calculation and the final verification rounds had been large prior to 2017, but it shrank to zero as the disclosures took place in 2017 and 2018. Specifically, Fig. 2 shows the average gap between provincial GDP numbers from the primary calculation and final verification rounds, separately for treated provinces implicated in public disclosures of GDP misreporting (dark line), and control provinces that were not implicated (light grey line). For further clarity, dotted lines represent each of the three treated provinces, Liaoning (red), Inner Mongolia (green), and Tianjin (blue). Each year corresponds to the year of the statistic and not the year it was released (e.g., 2016 GDPs were released only in 2017).

As clearly observable in Fig. 2, revisions of provincial GDPs intensified throughout the 2000s, coinciding with the rise of the "promotion tournament" model of meritocratic promotion in China (Li & Zhou, 2005). A reversal began with the 2016 GDPs, which Liaoning admitted to exaggerating in the 2017 public disclosure. Following subsequent disclosures in Inner Mongolia and Tianjin, this gap shrank rapidly to zero.

Notably, the reduction in revision is observable only for provinces implicated in public disclosure. Previously, the gap between initial and revised numbers had been attributed to the timing of data availability, specifically, more accurate data that emerged only after the first round of reporting. That public disclosure would have an impact on this timing is implausible; instead, the shrinking gap reflects only that the provinces no longer had to cover up their lies once they stopped telling them. This gap shrank not only for the manipulating provinces but also for other provinces, echoing our earlier finding about the spillover effects of public disclosure. We know that the process is administratively burdensome for provincial statistical officials, but we cannot quantify its exact cost, yet it clearly stopped after the disclosure.

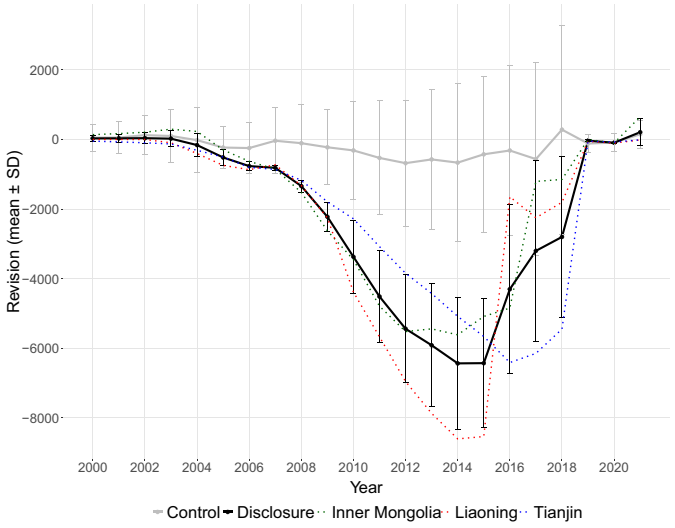


Fig. 2. Revision of GDP statistics by group (2000–2021).

6.2. Spillover reduction in misreporting

The public nature of internal misinformation disclosure may also influence the behavior of actors beyond those working in the target localities. For example, seeing their colleagues implicated in public disclosures, local governments that are manipulating statistics but have yet to be caught may realize they must be more cautious. This can lead to a spillover effect in which statistical falsification decreases even beyond the original targets of public disclosure.

We estimate this spillover effect among two types of localities: those that share borders and those that share similar economic structures with the original targets. Shared borders likely imply shared economic conditions; adjacent localities are also more likely to be compared against each other, but they also facilitate close informal ties between leaders. Meanwhile, similar economic structures, specifically the relative importance of the industrial sector to the economy, likely expose localities to similar manipulation incentives and capacities. Both sets of factors are likely to make local officials sensitive to news about misinformation being addressed in other provinces.

Table 3 presents estimates of this spillover effect, obtained by adding indicators for spillover units to our TWFE models. The results indicate a plausible but ultimately limited spillover effect. In particular, estimates from both the province and city models, and across both definitions of spillover units, are negative but weaker than the direct treatment effects, and statistical significance is found only for those pertaining to adjacent neighbors of the treated cities. Additional analyses in Appendix E find similar results.

6.3. Downstream economic benefits

The public disclosure of internal misinformation may reduce misbehavior and reinforce the regime's control over its subordinates, but it nevertheless brings significant reputational cost. We find that this strategy has additional economic benefits that may have made this trade-off easy for Chinese leaders to accept. In particular, the problem of statistical manipulation has long been an open secret to many economic actors, specifically firms and investors. A common understanding that economic statistics are unreliable and government information is untrustworthy has likely reduced trust in the government and confidence in the business environment. The public disclosure of data manipulation, as a signal that the regime is aware of the problem and willing to fight it even at great cost, may improve the situation, boosting business confidence and encouraging investment in the economy.

Table 3
Spillover effects of public disclosure.

Dependent Variables:	City-Province Gap		Nightlight Gap	
Sample	Province		City	
Spillover Definition	Border	Industry	Border	Industry
Model:	(1)	(2)	(3)	(4)
Variables				
Disclosure (treated units)	−1.199***	−1.240***	−0.6946***	−0.5794**
	(0.2913)	(0.2877)	(0.1716)	(0.1760)
Spillover units	−0.0532	−0.2025	−0.5222***	−0.1413
	(0.1602)	(0.1215)	(0.1305)	(0.1021)
GDP growth	−0.0755	−0.0743		
	(0.0451)	(0.0446)		
GDPPC (log)	−1.385*	−1.494*	−0.0419	−0.0033
	(0.6516)	(0.6001)	(0.1104)	(0.1138)
Revenue PC (log)	1.684**	1.725**	0.0029	0.0355
	(0.5582)	(0.5571)	(0.0449)	(0.0437)
Population (log)	0.0568	−0.0127	0.5609**	0.7469**
	(0.6937)	(0.7506)	(0.2107)	(0.2276)
Pre-treatment Outcome (pp)				
Control	3.09 (10.14)		1.39 (31.68)	
Treated	43.04 (15.12)		8.44 (18.14)	
Fixed-effects				
province	Yes	Yes		
year	Yes	Yes		
city			Yes	Yes
Year			Yes	Yes
Fit statistics				
Observations	384	384	4479	4479
R^2	0.86925	0.87016	0.49864	0.49246

*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05. Clustered (province/city) standard-errors in parentheses.*

Consistent with this expectation, Table 4 presents the effects of disclosure on a range of city-level economic indicators, providing evidence of both the downstream economic impact of disclosure and the mechanisms by which it manifests. First, Columns (1) and (2) show that disclosure has opposite effects on official GDP growth and nightlight growth. The decrease in official GDP growth is no surprise and only confirms that localities implicated in the disclosures no longer report falsely exaggerated economic performance, such that their reported numbers are now much lower than before. The increase in nightlight, however, indicates that real economic activity —for which nightlight is a reliable proxy—continues to grow, and does so more in cities that experienced the disclosure, even with a relatively modest amount. In other words, disclosure slows down reported growth but does the opposite for real growth.

Columns (3)–(6) offer a tentative explanation, showing that public disclosure increases the entry of new businesses. Using data from the China Business Registration Database, we find that the share of new manufacturing enterprises has increased in prefectures involved in statistical falsification disclosures, while cancellations of old-industry firms also rise. This result is consistent with the broader pattern that disclosure narrows measured manipulation without evidence of a broad decline in real activity. Finding this impact concentrated among manufacturing firms is important because the industrial sector is most subject to manipulation by local governments and is thus most likely to suffer damage to business confidence.[14]

[14] In Table F1 in Appendix F, we find heterogeneous interaction patterns between disclosure and pre-treatment industrial-reliance measures, rather than a uniformly stronger reduction in misreporting for all reliance dimensions.

Table 4
Economic benefits of public disclosure.

	(1) Official Growth (SD)	(2) Nightlight Growth (SD)	(3) Manufacturing New Entry Share	(4) Hi-tech Mfg. New Entry Share	(5) Old Industry New Entry Share	(6) Old Industry Cancellations (log)
Disclosure	−0.817***	0.478***	0.008**	0.001**	−0.001*	0.317*
	(0.125)	(0.129)	(0.003)	(0.000)	(0.000)	(0.133)
Observations	4479	4479	4529	4529	4529	3557
Dep. Var.	11.07	9.97	0.066	0.002	0.003	1.72
	(4.42)	(32.55)	(0.045)	(0.003)	(0.005)	(1.23)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
City & Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: Cols. (1)–(2): official GDP growth and nightlight growth, standardized as z-scores over the pooled sample; controls include log GDP per capita, log revenue per capita, and log population. Cols. (3)–(5): share of new firm registrations in the specified industry sector. Old industry (Col. 5): mining, smelting (ferrous/non-ferrous), chemicals, chemical fibres, and petrochemicals. Col. (6): log number of cancelled firms in old-industry categories. All columns use TWFE with city and year FE. Standard errors clustered at city level in parentheses. Signif. codes: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

Improving business confidence may not be the only way public disclosure—and its subsequent impact on data quality—may have spurred local economic activities. The public disclosures, by drawing local officials' attention to top-down monitoring, may have also reduced corruption, which then improved the economy. Similarly plausible, the improved data quality may have enabled better growth-promoting policies. Although Appendix J provides tentative evidence suggesting that these alternative channels are unlikely to be at work, our theory is agnostic about *how* public disclosures brought about the economic benefits. Ultimately, without claiming that it had directly motivated the public disclosure decisions, we are confident that public disclosure has led to unmistakable economic benefits.

7. Discussion & conclusion

Governments that pursue a top-down developmental agenda create both the incentive and the capacity for local officials to manipulate internal information, which, in turn, undermines that agenda. Government leaders in these countries thus face debilitating cycles of misinformation, a problem that requires navigating a difficult trade-off. The issue of local GDP manipulation in China exemplifies this problem. While the central authority attempts to use government statistics to evaluate officials, local officials manipulate these numbers to improve their career prospects—giving rise to the adage "*numbers produce officials, but officials produce numbers*." Consequently, both the central leader and the local agents fall into a trap in which the leader cannot compel the agents to stop manipulating, and the agents must maintain the charade by inflating the numbers ever more, incurring high administrative costs and jeopardizing the actual local economy.

Existing literature focuses on monitoring and sanctioning as primary methods to discipline subordinates in autocracies, but we show that the public disclosure of manipulation cases—even when issued without punishment—still reduces future misinformation. Such disclosure may generate greater reputational costs but is effective at reducing misbehavior. It also brings secondary benefits, such as alleviating the administrative burden of maintaining continued falsehoods, reviving market confidence, and spurring new business activities.

Our theory attributes the effectiveness of public disclosure in fighting internal rule-breaking to its reputation costs, which credibly signal the regime's willingness to tackle malfeasance without revealing the limits of its capacity. This self-inflicted reputation cost, plausibly unsustainable, also communicates to rule breakers a closing window of opportunity in which they could and should correct their misbehavior before more severe sanctions return.

To support this argument, we present evidence from various data sources, combining multiple empirical approaches at several levels of analysis. In our analyses, we recognize the problem of nonrandom selection into treatment, that is, localities implicated in public disclosures of statistical misreporting are chosen for some reasons that may matter for their misreporting behaviors both before and after the disclosure. As validated in Appendix B, this approach appears to have mitigated this problem, lending confidence to our findings.

Robustness aside, pondering where and when higher authorities may be most interested in tackling malfeasance with public disclosure is nevertheless instructive. Although a thorough investigation lies beyond the scope of this project, our probing in Appendix G shows that the three provinces implicated in the public disclosures of GDP manipulation have a greater share of "zombie companies"—essentially large and inefficient state-owned enterprises presumably kept afloat as vessels for false statistics. In addition, Appendix H shows that personal political connections between local leaders and higher-level authorities, long understood as a central force in Chinese politics (Jiang, 2018; Landry et al., 2017), did not shield localities from public disclosure. Together with our findings about public disclosure's economic benefits, these pieces of evidence suggest an economic imperative behind publicly disclosing the falsification of economic statistics. This may explain why we observe public disclosure working so effectively in a developmental authoritarian regime like China.

At the same time, the logic behind our theory also sheds light on when and how public disclosure may be most appropriate beyond the case of internal misinformation in China. First, regime leaders must be truly sensitive to public perception—or the reputational cost would not be credible, but not too sensitive—or they would not dare to risk such cost even in the short term. The former qualification may have little practical import even in autocracies, but the latter rules out weak authoritarian regimes, especially electoral ones. Second, the type of wrongdoing also matters. Outside of reputational considerations, the violation must carry sufficient long-term costs for the short-term sacrifices to be worthwhile, yet autocrats may not be able to extend leniency to offenses that threaten their survival or violate their principles. Thus, public disclosure may work for a wide range of violations from bureaucratic slack to grand corruption, but may not be appropriate against open defection or ideological disenchantment. Finally, the conflict of interests underlying the wrongdoing cannot be strictly zero-sum, meaning that a viable path must exist where the offenders can become better off by not breaking rules. For example, environmental violations also reduce the quality of life of the very officials who allow them, but the same cannot be said about embezzlement.

Ultimately, this paper is among the few studies examining the fight against internal misinformation in authoritarian countries. We demonstrated the economic incentives for autocrats to reduce manipulation

and the effectiveness of public disclosure as a solution to this problem; nevertheless, more open questions can be explored in the future. For instance, we find that the effect of disclosure on future misreporting is not enduring. Although this could occur because of the irregularity of the statistical reporting system during the pandemic, it might also indicate that the disclosures alone are not sufficient for clean statistics to remain the dominant status quo in equilibrium.

Finally, leaders of authoritarian regimes may not want to offer amnesty to their agents forever. While intensifying the public disclosure of internal data manipulation, China also began to impose serious punishment on some offenders in 2020. In 2024, the central government adopted internal rules that appeared to be stricter in punishing the misreporters, and cases emerged in which heads of the local statistical bureaus were demoted. Meanwhile, despite questions about the true intention, the country has claimed to gradually abandon "the high-growth mode." Once regime priority has shifted from growth to information accuracy, we may observe both less leniency for agents and greater transparency.

CRediT authorship contribution statement

Handi Li: Writing – review & editing, Writing – original draft, Project administration, Methodology, Formal analysis, Data curation, Conceptualization. **Shengqiao Lin:** Writing – review & editing, Writing – original draft, Methodology, Formal analysis, Data curation, Conceptualization. **Minh D. Trinh:** Writing – review & editing, Writing – original draft, Methodology, Formal analysis, Data curation, Conceptualization.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary data

Supplementary data for this article can be found online at doi:10.1016/j.worlddev.2026.107498.

Data availability

The data and replication materials necessary to reproduce the analyses reported in this article are publicly available at: https://doi.org/10.7910/DVN/EI3FBU.

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