Raising Grievances to the State: The Political Economic Effects of Anti-Corruption Crackdowns on Labor Activism in China

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Abstract

Government-firm corruption can affect workers' welfare and strike decisions. This paper presents a simple model examining a worker's decision to strike and the influence of corruption on that decision. The model predicts that higher corruption decreases workers' return to strike by increasing workers' perceived firm-government corruption. I then test the model by studying the impact of China's anti-corruption crackdown on strikes, leveraging temporal and geographical variations in corruption inspections. To test this, I built a unique dataset by combining city-level labor strikes and inspections. Following a first high-profile corruption inspection in a city, strikes doubled within one year and tripled within two years. The increase in strikes was mainly in private construction and manufacturing sectors with wage arrears as the primary reason, predominantly in cities with high prior corruption levels. The rise in strikes also saw network effects across cities; cities not yet inspected saw strike increase after other cities in the same province or neighboring cities underwent inspections and saw strike increase. Confirming the model's prediction, the inspections increased workers' expected return to strike, thereby revealing pre-existing grievances in a corrupt environment. A back-of-the-envelope calculation estimates a welfare loss to workers of up to 1.2 billion yuan ($170 million) without the anti-corruption crackdown.

Keywords — labor, public, political economy, collective bargaining, development

JEL Codes — P00, D70, D80, J81, J83, O10

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1 Introduction

Workers have historically used strikes to improve wages and working conditions, influencing landmark policies like the 1935 National Labor Relations Act and the 1938 Fair Labor Standards Act. Strikes can disrupt economic activities and incur social costs. In 2019, a General Motors strike involving $48,000$ workers caused 1.334 million lost days, according to the Bureau of Labor Statistics. Economic literature often portrays strikes as a bargaining process between unions and firms, in a democratic context with minimal government interference.[1] However, few studies explore the political environment of strikes: What is the government's role in negotiations between workers and firms?

This paper presents a conceptual framework predicting that stronger firm-government corruption[2] decreases workers' strike returns. This prediction is backed by previous evidence showing that politically connected firms weaken union power (Song et al., 2016) and receive favor from government in negotiations (Faccio, 2006; Brogaard et al., 2021). Such corruption can decrease workers' expected bargaining returns, elevating strike costs and reducing benefits due to quick suppression and unfavorable negotiation terms from authority-backed firms.

Empirically determining whether higher firm-government corruption *causes* lower strike returns is challenging. This is because corruption impacts firm productivity (Colonnelli and Prem, 2021), social stability (Fenizia and Saggio, 2021), and other economic activities,[3] all of which can in turn affect workers' welfare and strike returns.[4] Moreover, strikes can affect corruption. For example, collective actions like protests can serve as a check, reducing the influence of politically connected firms (Acemoglu et al., 2018).

This paper utilizes China's anti-corruption campaign to tackle endogeneity issues.[5] Launched in September 2012 by Xi Jinping, this campaign investigated Chinese Communist Party (CCP) officials nationwide.[6] From 2012 to 2017, hundreds of thousands of such officials were inspected across a variety of jurisdictions, including provinces, municipalities, universities, and state-owned enterprises (SOEs). These inspection decisions, managed by the Commission for Discipline Inspection (CCDI), remained undisclosed until execution, making the timing and location of the

[1] The major literature on the economic theory of labor strikes spans from the early 1980s to the late 1990s. Key references include Hicks (1963), Ashenfelter and Johnson (1969), Kennan (1980, 1986), Card (1988, 1990), Cramton and Tracy (1992), and Cramton et al. (1999).

[2] This paper primarily examines the impact of firm-government corruption on workers' decisions to strike, particularly in contexts where workers' rights are restricted. Although union-government corruption exists as seen in some U.S. cases, it is not the central focus of this study.

[3] A significant body of literature, including studies by Murphy et al. (1993), Shleifer and Vishny (1993, 1994, 1998), Mauro (1995), Johnson et al. (1997), Banerjee (1997), and Svensson (2005), explores the nexus between corruption, firm performance, and economic activity.

[4] Economic activities can influence strikes, such as Campante et al. (2022)'s analysis showing a decline in exports can escalate labor strikes in China.

[5] Studies such as Qian and Wen (2015), Lorentzen and Lu (2016), Dang and Yang (2016), Li et al. (2017), Zhu and Zhang (2017), Xi et al. (2018), Fang et al. (2018), Chu et al. (2019), Chen and Kung (2019), Ding et al. (2020), Hao et al. (2020), Xu et al. (2021), Huang et al. (2021), and Hong (2022), have used the 2012 anti-corruption campaign and the arrival of CCDI inspections as plausible exogenous events to examine diverse outcomes such as firm performance, work accidents, and human capital.

[6] The anti-corruption campaign initially targeted only CCP-affiliated officials. However, the scope broadened in 2018 with the Supervision Law, encompassing officials outside the CCP as well.

inspections largely unforeseeable for local governments, firms, and workers.[7] This setup serves as a natural experiment for studying the impact of varied corruption inspections on strikes.

To investigate the aforementioned causal relationship, I built a unique dataset linking CCDI corruption inspections to strike counts in each prefecture-level city in China, compiled both monthly and quarterly from 2011 to 2020. I then employed an event study methodology, leveraging both temporal and spatial variations, by comparing strike changes in cities based on different inspection timings. Specifically, an "event" is defined as the first "high-profile" inspection in a city, conditional on the inspection team's arrival in the city's province. I defined "high-profile" inspections as the inspections targeting CCP officials of Deputy-Bureau-Director level or above.[8] Typically, high-ranked inspections get broad media coverage, while low-ranked ones get local press. Hence, the former are termed "high-profile" inspections.

While the CCDI's inspection timings may not be entirely random due to potential unobservable factors, I demonstrated two points. Both underscore the assumption: Cities would have shown parallel trends in strikes without corruption inspections. First, inspection timings show no correlation with a city's economic characteristics, strike levels, or corruption levels prior to the anti-corruption campaign. Second, the baseline findings reveal no signs of anticipation in strikes before a city underwent inspections. Overall, there is no evidence to suggest inspection timings were predictable or that changes in strikes were due to other factors.

Before delving into the impact of inspections on strikes, I first established the CCDI inspection teams' effectiveness in enforcing the anti-corruption campaign. Specifically, following the inspection teams' provincial arrival and the first citywide inspection, the total inspections in a city tripled within a year and amplified six-fold within two years. Similarly, high-profile inspections quadrupled. These high-profile inspections also saw a surge in social media interest, with *Weibo* (a Chinese platform akin to *Twitter*) posts regarding them tripling within six months following the first city inspection.

My baseline analysis shows a sharp increase in strike incidents within three months following the first high-profile inspection in a city, conditional on CCDI team arrival in the province. This spike persisted for two years. One year post-inspection, average monthly strike incidents per city rose by 0.2, compared to pre-inspection 0.16, essentially doubling the original strike count. Two years post-inspection, strike incidents nearly tripled, increasing by almost 0.4. Despite a gradual decline after two years, strike incidents remained higher than the original count.

The baseline findings remain consistent when considering recent research on staggered treatments in difference-in-differences. I utilized the Callaway and Sant'Anna (2021) and Gardner (2021) estimators, with both yielding consistent strike outcomes. Furthermore, I addressed an alternative strike distribution where zero counts of strikes are predominant by implementing an inverse hyperbolic sine (IHS) transformation on strikes and a zero-inflated negative binomial (ZINB) model. Both approaches ensured outcomes that align with the baseline results.

[7] Willian Wan, "Secretive agency leads most intense anti-corruption effort in modern Chinese history," *The Washington Post*, July 2, 2014.

[8] Details on the CCP bureaucratic ranking will be provided in Section 4 as part of the introduction to the Corruption Investigation Dataset (CID). Moreover, Table C1 gives a description and examples for each CCP rank. Table 2 shows inspection counts by rank and category. High-profile inspections, ranked 6 or above, make up 7% of all inspections.

An analysis of strike characteristics reveals the majority of strikes occurred in the construction and manufacturing industries, primarily motivated by wage arrears and mostly in private firms rather than SOEs. These strikes were usually small-scale, involving fewer than 100 participants, and were carried out through protests and sit-ins. These findings suggest that the surge in strikes appears to be driven by low-skilled workers, likely migrant workers, seeking unpaid wages. These workers typically harbor substantial grievances, attributable to their non-residential status in the working city and a lack of protection from legal labor contracts.

I then explored whether network effects, or spillover effects, influenced the spread of strikes across cities and among migrant workers.[9] First, inspection timings' variation led to a rise in strikes in cities awaiting their first high-profile inspections, motivated by observing strikes in other cities in the same province undergoing such inspections. This spillover effect, where workers are influenced by strikes in other cities, differs from an anticipation effect where workers strike based on known inspection timings. Likewise, when a city underwent its first high-profile inspections, neighboring cities yet to face inspections saw a surge in strikes. Furthermore, cities with a higher ratio of migrant workers connected with *Laoxiang*, or fellow migrants from the same origin city, experienced a higher post-inspection strike increase.

The rest of the paper investigates the underlying mechanism behind my findings: Anti-corruption inspections increased workers' expected returns from striking. If this mechanism were true, cities with stronger pre-campaign firm-government connections should see a higher rise in strikes. To confirm this, I used Chen and Kung (2019)'s measure of princeling land transactions[10] as a proxy for firm-government connections. The results show that cities with stronger prior firm-government ties accounted for most of the strike surge, suggesting that the inspections increased workers' strike returns in previously more corrupt cities. This supports the hypothesis that corruption decreases expected strike returns, making this mechanism the most plausible explanation for the increase in strikes. I also ruled out alternative mechanisms including changes in workers' attitudes towards local governments, changes in media exposure, and changes in firms' behaviors after the inspections.

Given that anti-corruption inspections increased workers' expected returns, I estimated the potential welfare loss without these inspections, shedding light on corruption's economic impact. The debate about corruption's impact is divided. Despite some studies arguing corruption can benefit efficient firms and growth via rent-seeking, others believe it reduces firm productivity and growth.[11] The strike increase post-inspections resulted in forgone wages of up to 1.2 billion yuan, or \$170 million, equivalent to 1% of the median GDP of Chinese prefecture-level cities in 2013. This figure represents a lower bound estimate of welfare loss, or grievances, for workers who would not strike without inspections. Consequently, regardless of its effects on firms, corruption harms worker welfare, thereby impeding economic growth.

[9] Migrant workers are individuals relocating from typically rural birthplaces to work in non-agricultural sectors elsewhere.

[10] These are discounted land sales from the government to firms with board members connected to the CCP executive committee.

[11] The discussion over the economic impact of corruption is extensively covered in academic literature. While some research indicates that corruption has a detrimental effect on economic growth, other studies argue the opposite. This is further explored in the literature review.

My research is relevant to a broad range of concerns. First, it centers on labor activism within a high state-capacity authoritarian context, where grassroots organizations like labor unions wield limited political power. Economics literature, predominantly focused on labor strike theories in developed nations (Hicks, 1963; Ashenfelter and Johnson, 1969; Kennan, 1980, 1986; Card, 1988, 1990; Cramton and Tracy, 1992; Cramton et al., 1999),[12] scarcely explores labor activism in authoritarian and developing regimes. This gap exists despite prior studies in political science and sociology on how institutions shape labor practices and workers' political demands (Ngai, 2005; Lee, 2007; Gallagher, 2011, 2017). My work, therefore, offers empirical evidence on how an authoritarian environment impacts workers' strike decisions and policy-driven behaviors, maintaining relevance even to faltering or unstable democracies where politics influences worker welfare.

Second, my study contributes to the literature on corruption's impacts and anti-corruption policies. While some studies suggest corruption impedes growth (Murphy et al., 1993; Shleifer and Vishny 1993; 1994; 1998; Mauro, 1995; Johnson et al., 1997; Banerjee, 1997; Svensson, 2005; Sequeira and Djankov, 2014; Smith, 2016; Bobonis et al., 2016; Avis et al., 2018; Chen and Kung, 2019; Bai et al., 2019; Colonnelli and Prem, 2021; Fenizia and Saggio, 2021), others argue it can stimulate growth indirectly through efficient firm rent-seeking (Lui, 1985; Bardhan, 1997; Méon and Weill, 2010; Dreher and Gassebner, 2013). My study integrates into this discourse by demonstrating how corruption inhibits the expression of workers' grievances, thus negatively impacting workers' welfare and economic growth.

Furthermore, research indicates that anti-corruption policies like audits, inspections, and official turnovers can effectively reduce corruption and stimulate growth (Bobonis et al., 2016; Avis et al., 2018; Chen and Kung, 2019; Colonnelli and Prem, 2021; Fenizia and Saggio, 2021).[13] Previous research has studied China's anti-corruption campaign, particularly its impacts on firm-government connections (Chen and Kung, 2019; Chu et al., 2019; Ding et al., 2020; Hao et al., 2020; Xu et al., 2021), bribery reduction (Qian and Wen, 2015), and firm innovation and growth (Dang and Yang, 2016; Lorentzen and Lu, 2016; Xu and Yano, 2017; Fang et al., 2018; Hao et al., 2020; Huang et al., 2021).

Building on the aforementioned literature, I demonstrate how anti-corruption inspections reshape workers' perceptions of government-business ties, thereby influencing their behavior. Thus, these inspections influence not just governmental or firm behaviors, but also grassroots responses. In this way, my study extends the literature on grassroots responses to anti-corruption policies, building upon the works of Gingerich (2009), Zhu et al. (2019), and Wang and Dickson (2021).

Additionally, my research expands the discourse on collective actions in non-democratic regimes,[14] emphasizing the role of worker-organized actions and supplementing prior economic studies on protests in China (Qin et al., 2017, Beraja et al., 2021) and Russia (Enikolopov et al., 2020). Unlike previous studies that examined factors such as economic slowdown (Campante et al., 2022), surveillance technology (Beraja et al., 2021), and the media's role (Qin et al., 2022) in shaping col-

[12] Later works such as Glazer (1992) and Brunnschweiler et al. (2012) probe workers' strike decision behaviors but remain within democratic contexts.

[13] Not all studies have shown that anti-corruption policies are effective. For instance, Riaño (2021) demonstrates that an anti-nepotism reform in Colombia had limited success in combating nepotism within the bureaucracy.

[14] The majority of economic literature on collective action focuses on democratic settings. Zhuravskaya et al. (2020) thoroughly reviewed this, particularly the role of media in political participation in the West.

lective actions in China, my work offers a nuanced, worker-centric perspective on collective action decisions within shifting political contexts. Crucially, it illustrates how workers' collective actions can spread via spillover or network effects across regions, enhancing the existing literature on network effects in organizing collective action (Enikolopov et al., 2020). Thus, my findings highlight that workers, even under authoritarian regimes, actively engage in political participation, voicing their demands to the government.

2 Background

2.1 Labor Activism in China

China's economic boom has led to escalating labor disputes in recent years, particularly in the private sector (Chan, 2001). Workers are protesting violations of their rights,[15] like wage arrears, excessive hours, and substandard conditions (Leung, 2015; Li et al., 2016). The China Labour Bulletin (CLB) reported over $1,700$ labor protests in 2018 and over $1,200$ in 2017. Most collective actions aimed to draw media attention and seek government mediation.[16]

Migrant workers, largely unskilled individuals relocating from rural areas to urban locales for non-agricultural work, often suffer labor violations due to the absence of official work contracts and residency proof (*Hukou*) in their working cities (Chan, 2001). Between 2012 to 2015, these workers, averaging 270.8 million annually, constituted around 35.1% of China's workforce. Nearly half of these workers were employed in construction and manufacturing sectors, with the remaining in services, retail, wholesale, transportation, and hospitality industries.[17] They often migrate with kin or friends, forming connections with migrants from the same region (Xu and Palmer, 2011), which can significantly influence their collective decision to strike (Chan and Ngai, 2009).

In China, strikes primarily arise from wage arrears rather than demands for higher pay. From 1986-1999, 70% of labor disputes in Shenzhen, Guangdong, were due to unpaid wages (Lee, 2004). This trend continued into 2022, with CLB reporting wage arrears at the root of 87% of disputes.[18] Primarily driven by wage arrears, striking workers aim to secure compensation by impacting local governments and media (Leung, 2015). The central government, recognizing this issue, tolerates minor labor strikes and media coverage to keep a check on local governments (Qin et al., 2017).[19] [20] However, large-scale collective actions and social media discussions about strikes are censored (King et al., 2013), which results in most strikes being small, spontaneous acts often involving dozens of workers who stage rallies with banners and slogans.[21]

[15] Erik Eckholm, "Workers' Rights Are Suffering in China as Manufacturing Goes Capitalist." *New York Times*, August 22, 2001.
[16] China Labour Bulletin Workers' Calls-for-Help Map, 2020-2022.
[17] 2015 Report from National Bureau of Statistics.
[18] "Wage Arrears after Zero Covid and before Lunar New Year is Symptom of Systemic Problem." *China Labour Bulletin*, January 9, 2023.
[19] "Ministry of Human Resources and Social Security of the People's Republic of China: Comprehensive Solution to Wage Arrears for Migrant Workers in 2014." *Xinhua News*, April 1, 2014.
[20] "Government Launched Big Move to Rectify Wage Arrears: Number of Wage Arrears Decreased by 40% Since Last Year." *People's Daily*, December 18, 2017.
[21] "The Shifting Patterns of Labour Protests in China Present a Challenge to the Union." *China Labour Bulletin*, July 9, 2019.

China's limited union power also contributes to spontaneous small-scale strikes. The All-China Federation of Trade Unions (ACFTU), the country's sole national trade union, is purportedly responsible for protecting workers in industries like construction and manufacturing. However, its close ties with the CCP make it more of a political tool than an independent entity (Biddulph and Cooney, 1993; O'Leary et al., 2001; Clarke et al., 2004; Taylor and Li, 2007). Despite occasional aid to workers' rights (Yao and Zhong, 2013), the ACFTU's authority is undermined by firms' government connections (Song et al., 2016). Its main activities lean more towards propaganda, such as holiday gift distribution, rather than effectively addressing labor disputes.[22] Moreover, the formation of independent unions in China is prohibited, limiting alternative avenues for worker representation.[23]

Given the lack of a central platform to express grievances, workers assess their local political context before deciding to strike, aiming to attract government and media attention. This is exemplified in a case from Zigong, Sichuan, featured in the *People's Daily*, where 100 construction workers experiencing delayed wages in 2018 chose to strike:

> *"Our contractor manipulated our pay cards, transferring 6 million yuan into the workers' salary account, yet we received no payment from March 2017 to October 2018. Despite reporting this to various bureaus, no investigations were conducted. When we threatened to strike in September, the developer responded with indifference. We now hope your newspaper can help us reclaim our wages."*[24]

The example illustrates how workers, failing to recover wages through legal channels, only found success after gaining national media attention. This supports the study's hypothesis: workers strike more when they anticipate benefits from government and media attention, especially during an anti-corruption campaign which alters workers' perception of the government-firm relationship.

2.2 China's Anti-Corruption Campaign

Corruption has been rampant within the CCP since China's 1978 economic reform, involving bribery, public fund procurement, and nepotism. In November 2012, Xi Jinping initiated a major anti-corruption campaign, leading to the dismissal of significant officials like Sichuan Deputy Party Secretary Li Chuncheng.[25] While some scholars believe the campaign was a political move (Zhu and Zhang, 2017; Xi et al., 2018), others recognize its effects in cutting firm-government ties (Chen and Kung, 2019; Chu et al., 2019; Ding et al., 2020; Hao et al., 2020; Xu et al., 2021), reducing bribery (Qian and Wen, 2015), and promoting firm innovation and growth (Dang and Yang, 2016; Lorentzen and Lu, 2016; Xu and Yano, 2017; Fang et al., 2018; Hao et al., 2020; Huang et al., 2021). Also, a *New York Times* interview with Chinese residents reveals that Xi's anti-corruption campaign, which had significantly reduced unofficial fees and bribes, garnered him substantial popularity among Chinese citizens, despite skepticism towards the CCP.[26]

[22]Kaiser Kuo, "Labor unrest and how China balances repression and responsiveness." *SupChina*, September 20, 2021.
[23]"Workers' rights and labour relations in China." *China Labor Bulletin*, June 30, 2020.
[24]Yang Zhang and Yiqi Shi, "Accompanying Migrant Workers to Recover Wages." *People's Daily*, December 2, 2019.
[25]"China's Anti-Corruption Commission Investigates Senior Official." *New York Times*, December 5, 2012.
[26]Didi Kirsten Tatlow. "In Fighting Tigers, Xi Inspires the Masses." *The New York Times*. September 24, 2014.

In his campaign, Xi pledged to combat corruption among both high-ranked officials, known as "tigers," and low-ranked officials, known as "flies."[27] In this study, I categorized high-profile, or high-ranked officials as those ranked equal to or above deputy bureau director level, or those holding the position of deputy party secretary in prefecture-level cities. Low-ranked officials refer to those below deputy bureau director level. For example, a high-profile official can be a city mayor, while a low-ranked official can be a village party secretary. I categorized officials as high or low-profile based on the extent of media coverage and mentions on *Weibo* during inspections. Table C1 details CCP bureaucracy ranks and descriptions.

The anti-corruption campaign was primarily executed by the Commission for Discipline Inspection (CCDI), an entity within the CCP theoretically independent of party operations (Xu, 2014).[28] During 2013 and 2014, the CCDI conducted four rounds of inspections in May 2013, November 2013, March 2014, and July 2014 across 31 provinces, municipalities, and autonomous regions in mainland China. Given the CCDI's decision-making independence, the inspection timelines should be unpredictable to local governments, businesses, and residents.

According to Figure 1, CCDI inspection teams' arrival times do not seem clustered in regions with geographical closeness or similar economic performance. However, it is challenging to determine if inspections times were based on factors like corruption levels, business performance, or worker conditions. To overcome this, I performed a detailed geographical and temporal variation analysis in Section 5 using city instead of province variations. A treatment event is defined as the first high-profile investigation case in a city post-inspection team's arrival in its province. I then conducted a regression analysis to check if labor strikes and various city characteristics pre-campaign could predict inspection timings. Table C3 finds no statistically significant predictors.

The CCDI inspection team follows a specific process upon arrival in a province.[29] First, evidence of bribery, abuse of power, or scandal is secretly collected over one to two months. This evidence is then forwarded to a judicial body, either local or national, depending on the official's rank and the case's severity. If the evidence is sufficient, the CCDI either arrests the official or initiates an internal party investigation called *Shuanggui*, resulting in the official's removal from their position. The official is then charged in court using the collected evidence and their confession from the investigation. During the anti-corruption campaign, 99% of the investigated officials were convicted.[30]

The process from an official's inspection to conviction can take six months to a year. Local newspapers report on the inspection, including the official's name, position, and investigation reasons (Wang and Dickson, 2021). Despite limited information about the closed-door interrogations,[31] all inspection events are public. High-profile inspections attract significant media attention. For example, the 2014 investigation of Ding Xuefeng, former mayor of Lvliang, Shanxi, was covered by major news outlets such as *People's Daily*, *Economic Daily*, *Caixin News*, and *CCTV News*.

[27] "Xi Jining vows to fight 'tigers' and 'flies' in anti-corruption drive." *The Guardian*, January 22, 2013.

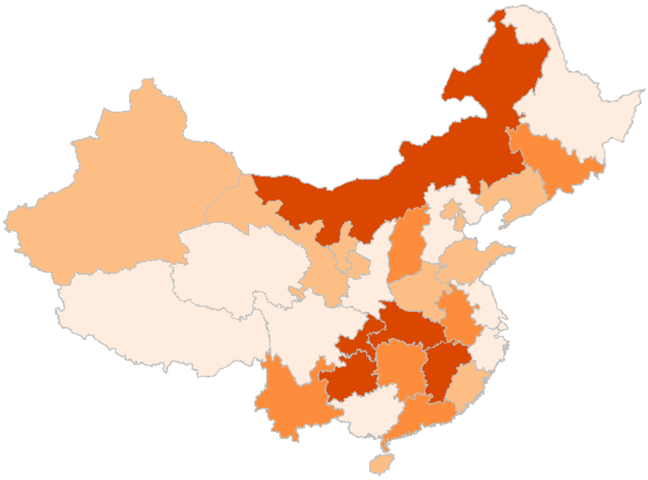
[28] In an article explained by *The Beijing News*, Xi and CCDI Secretary Wang Qishan institutionalised CCDI independence through the policy *Reform on CCDI (纪委改革方案)* the anti-corruption campaign.

[29] Ding Xuezhen, "CCDI reveals details of inspection process." *Global Times*, May 12, 2016.

[30] Andrew Jacobs and Chris Buckley, "Presumed Guilty in China's War on Corruption, Targets Suffer Abuses." *The New York Times*, December 8, 2016.

[31] Chris Buckley, "Confessions Made Under Duress Tarnish China's Graft Fight, Report Says." *The New York Times*, December 6, 2016.

Figure 1: CCDI Inspection Team Arrival Times by Province



Notes: The figure shows the first wave of the CCDI corruption inspection team's arrival in 31 provinces, municipalities, and autonomous regions in China between 2013 and 2014. This was in response to Xi Jinping's announcement of an anti-corruption campaign in 2012. It should be noted that the CCDI did not conduct inspections in the special administrative regions of Hong Kong and Macau.

3 Conceptual Framework

This section presents a simple model illustrating the impact of political environments on workers' strike decisions, primarily from the workers' viewpoint. Drawing from Passarelli and Tabellini (2017) concept of assessing costs and benefits for protest decisions, the model uses similar notation to outline workers' strike decisions.

3.1 A Simple Model

This model involves J industries and C cities, with an employment share $\lambda_{j,c}$ for each industry j and city c, where $\sum_{j=1}^{J}\sum_{c=1}^{C}\lambda_{j,c} = 1$.

A worker, i, in industry j and city c is owed wages, $\bar{w}_{jc} \geq 0$. The model uses wage arrears, common in China, as the strike cause, but $\bar{w}_{jc}$ may also symbolize sought pay raise or any other gains that strikes aim to achieve. Another way to view $\bar{w}_{jc}$ is as the worker's current grievance level, which they seek to address.

Every day, the worker weighs the costs and benefits of striking for owed wages. The market wage rate w_j remains uniform across all cities in industry j. By not striking, the worker retains their job or switch to a similar one at a daily wage of w_j.

If the worker chooses to strike, they bear an opportunity cost of $w_j d$, where d represents the strike's duration. The wage rate w_j and d are set exogenously, denoting a predetermined wage at a specific time. Since the worker decides daily whether to strike, their decisions are short-term, allowing me to treat w_j, d, and $\bar{w}_{jc}$ as exogenous.

Additionally, the worker encounters an idiosyncratic cost, ϵ_{ijc}. This means that workers may face personal strike sentiments or other unobserved factors affecting strikes. ϵ_{ijc} follows a uniform distribution with a mean of 0 and a density of $\frac{1}{2\sigma_j}$ for industry j within city c.

The strike success probability, $\rho_{j,c}$, is shared among workers in industry j and city c, and let $\rho_{j,c} \in (0,1)$. This probability depends on worker participation in strikes within their city and across cities. Higher participation within a city bolsters bargaining power and garners media attention, thereby increasing the likelihood of a successful strike. Additionally, $\rho_{j,c}$ can be swayed by participation in other cities. While increased strike activity elsewhere may inspire local workers to strike, it might also crowd out media attention, discouraging local participation by overshadowing local strike.

Let $p_{j,c} \in (0,1)$ denote the strike participation rate in industry j and city c, while $p_{j,-c}$ denotes participation in industry j within nearby cities.[32] Define

$$\rho_{j,c} = p_{j,c}\lambda_{j,c} + \alpha p_{j,-c}\lambda_{j,-c}, \tag{1}$$

where $\alpha \in (-\frac{p_{j,c}\lambda_{j,c}}{p_{j,-c}\lambda_{j,-c}}, \frac{1-p_{j,c}\lambda_{j,c}}{p_{j,-c}\lambda_{j,-c}})$ ensures $\rho_{j,c} \in (0,1)$. $\rho_{j,c}$ grows with increased local participation, but α can alter in either positive or negative direction $\rho_{j,c}$ based on strike activity in other cities.

3.2 Decision to Strike under No Corruption

Taking costs and benefits into account, worker i in industry j and city c will opt to strike if the expected return is positive, as depicted by the following inequality:

$$\begin{aligned} \rho_{j,c}\bar{w}_{jc} - w_j d - \epsilon_{ijc} &\geq 0 \\ (p_{j,c}\lambda_{j,c} + \alpha p_{j,-c}\lambda_{j,-c})\bar{w}_{jc} - w_j d - \epsilon_{ijc} &\geq 0. \end{aligned} \tag{2}$$

Thus, the probability that worker will strike is defined by:

$$\begin{aligned} p_{j,c} &= P[\epsilon_{ijc} \leq (p_{j,c}\lambda_{j,c} + \alpha p_{j,-c}\lambda_{j,-c})\bar{w}_{jc} - w_j d] \\ &= \frac{1}{2} + \frac{(p_{j,c}\lambda_{j,c} + \alpha p_{j,-c}\lambda_{j,-c})\bar{w}_{jc} - w_j d}{2\sigma_j}. \end{aligned} \tag{3}$$

Solving this, I obtain

$$p^*_{j,c} = \frac{\sigma_j + \alpha p_{j,-c}\lambda_{j,-c}\bar{w}_{jc} - w_j d}{2\sigma_j - \lambda_{j,c}\bar{w}_{jc}} \tag{4}$$

where $\sigma_j > \max[-\alpha p_{j,-c}\lambda_{j,-c}\bar{w}_{jc} + w_j d, \alpha p_{j,-c}\lambda_{j,-c}\bar{w}_{jc} - w_j d + \lambda_{j,c}\bar{w}_{jc}]$ so that $p^*_{j,c} \in (0,1)$.

Proposition 1 *$p^*_{j,c}$, the optimal participation rate for workers, increases with $\bar{w}_{jc}$.*

See Appendix A for the proof. Essentially, higher grievances motivate more workers to strike.

Proposition 2 *$p^*_{j,c}$ increases with $\lambda_{j,c}$, while its relationship with $p_{j,-c}$ and $\lambda_{j,-c}$ can be positive, negative, or zero.*

[32] $p_{j,-c}$ can signify the strike participation either in all neighboring cities of city c, or in all other cities within the same province, excluding c.

This can be directly drawn from Equation (4). $p^*_{j,c}$ grows with $\lambda_{j,c}$, indicating that larger groups of workers in the same industry and city are more likely to strike due to amplified bargaining power and a sense of community. Also, its relationship with $p_{j,-c}$ and $\lambda_{j,-c}$ demonstrates a network of strikes, where other cities' strikes influence local workers' decision to strike. The strike participation in other cities, $p_{j,-c}\lambda_{j,-c}$, can increase, decrease, or has no effect on $p^*_{j,c}$, depending on whether it encourages local workers ($\alpha > 0$), crowds out media attention ($\alpha < 0$), or if both or neither scenarios occur, resulting in $\alpha = 0$.

In addition, $p^*_{j,c}$ decreases with longer strike duration d and higher daily wages w_j as they boost the strike's opportunity cost. Furthermore, $p^*_{j,c}$'s sensitivity to the network effect, grievances $\bar{w}_{jc}$, and opportunity cost w_j and d intensifies as worker group homogeneity in industry j increases (i.e., σ_j decreases) since more workers are at the threshold of striking.

3.3 Decision to Strike under Corruption

Let θ denote the true firm-government connection, with 0 indicating no connections in the economy, and 1 indicating all firms connected to the government. Since workers can not directly observe θ, they form a perceived connection $\bar{\theta} \in [0, 1]$ regarding their firm's political links. Here, 0 represents no perceived link between their firm and local government, while 1 represents a strong connection. Workers build this perception based on indirect evidence, such as stories about firm executives' ties with CCP bureaucrats.[33]

This study emphasizes the change in perceived corruption, $\bar{\theta}$, rather than actual corruption, θ, following the anti-corruption campaign, due to unavailable firm-level data linked to worker strikes. It does not evaluate if the campaign truly reduced corruption or lessened firm-government ties, even with prior research indicating a weaker firm-government relationship post-campaign (Chen and Kung, 2019; Chu et al., 2019; Ding et al., 2020; Hao et al., 2020; Xu et al., 2021). Hence, the spotlight is on the workers' perceived political environment change and how it impacts their strike decisions, instead of assessing firms' or the government's perspective.

When deciding strike, a worker takes into account their perceived firm-government connection, $\bar{\theta}$. Their expected strike return is discounted by $1 - \bar{\theta}$. Strong firm-government connections can lead to reduced benefits for workers, such as a lack of support from the local labor bureau in arbitration, necessitating additional visits or gifts for support. Striking workers may also face added costs like suppression risks or potential police conflicts, which may require extra connections to circumvent. Therefore, $\bar{\theta}$ represents the perceived rate of worker returns transferring to corrupt firms and government bodies, with a higher $\bar{\theta}$ indicating a greater share perceived to be transferred to corrupt entities.

For simplicity, I assume $\bar{\theta}$ only applies to the worker's own firm in own city, with perceived corruption in other cities either unknown or zero. The strike participation rate in other cities is considered a given, meaning perceived corruption elsewhere does not influence workers' decisions in their own city.

[33]This study primarily explores the anti-corruption campaign's impact on workers' perceived firm-government connection, $\bar{\theta}$, not the actual connection, θ. This partial equilibrium approach offers a workers' perspective on striking, excluding government or firm viewpoints due to data limitations. Including these perspectives in a general equilibrium approach could be a subject for future exploration.

Taking into account the perceived connection $\bar{\theta}$, the worker will choose to strike if the expected strike return satisfies the following condition:

$$(1-\bar{\theta})\rho_{j,c}\bar{w}_{jc} - w_j d - \epsilon_{ijc} \geq 0. \tag{5}$$

The probability of this occurring is given by

$$p^{corr}_{j,c} = \frac{\sigma_j + (1-\bar{\theta})\alpha p_{j,-c}\lambda_{j,-c}\bar{w}_{jc} - w_j d}{2\sigma_j - (1-\bar{\theta})\lambda_{j,c}\bar{w}_{jc}} \tag{6}$$

where $\sigma_j > \max[-(1-\bar{\theta})\alpha p_{j,-c}\lambda_{j,-c}\bar{w}_{jc} + w_j d, (1-\bar{\theta})\lambda_{j,-c}\bar{w}_{jc} - w_j d + \lambda_{j,c}\bar{w}_{jc}]$ so that $p^{corr}_{j,c} \in (0,1)$.

Proposition 3 *$p^{corr}_{j,c}$, the optimal participation rate under corruption, decreases with $\bar{\theta}$.*

The proof parallels Proposition 1. It concludes that an increase in perceived corruption reduces strike participation as it lowers the expected strike return.

Given $\bar{\theta} > 0$, $p^{corr}_{j,c} < p^*_{j,c}$, holding everything else constant. Consequently, the grievance level $\bar{w}_{jc}$ must rise to $\bar{w}'_{jc}$ to make $p^{corr}_{j,c}(\bar{w}'_{jc};\bar{\theta}) = p^*_{j,c}(\bar{w}_{jc})$, holding other factors fixed. That is, workers need a higher level of grievances to strike with the same probability as in a non-corrupt context. Therefore, if an inspection team arrives to conduct anti-corruption measures, workers with grievances below $\bar{w}'_{jc}$ may be more likely to strike if they perceive a reduction in $\bar{\theta}$. Assuming perceived firm-government connections drop to 0 post-inspections, all workers with grievances between $\bar{w}_{jc}$ and $\bar{w}'_{jc}$ will strike, leading to a spike in strike incidents.

Proposition 3 implies that anti-corruption inspections can prompt a short-term increase in strikes, as workers perceive lower corruption levels. However, long-term strike predictions are uncertain due to potential changes in the local socio-economic environment post-inspection, hence beyond the coverage of this simple model. Factors like grievances and wages may become endogenous, subject to a new equilibrium as governments, firms, and workers adjust their long-term behaviors. For example, new government policies like enhanced surveillance could introduce unforeseen costs for workers.

4 Data

I developed a unique dataset, combining monthly strike and corruption inspection data from 362 prefectures in China between 2011 and 2020, totaling $43,440$ observations. Strike data was sourced from the China Labor Bulletin (CLB) Strike Map, while inspection data was from the Corruption Investigation Dataset (CID). For simplicity, a *city* will refer to a prefecture-level division in this study.

4.1 Primary Data

China Labour Bulletin Strike Map

The China Labour Bulletin (CLB) Strike Map records daily new strike incidents in prefecture-level cities from January 2011 onwards. This study utilizes data from 2011 to 2020, comprising $13,170$

strike incidents. I verified each observation by reading the strike description and cross-referencing it online. These observations came from posts on Chinese social media platform *Weibo* or news articles. Similar to Twitter, *Weibo* allows users to post short updates and interact with others. In 2012, it had 503 million registered users, with 220 million making at least one post,[34] a sizable proportion of China's 567 million internet users that year.[35] Despite *Weibo*'s censorship (King et al., 2013), CLB managed to record about one-tenth of all strikes in China.[36] The censorship issue will be discussed later in the section.

CLB records strike information such as the cause, industry, company type (private or state-owned enterprise (SOE)), scale, and outcome. Strike causes include wage arrears, pay raises, management supervision, and working environment issues. Industries range from construction, manufacturing, transportation, service,[37] education, mining, and others. Strike scale is categorized as small (1-100 participants), medium (101-1,000 participants), or large (1,001-10,000 participants). While the data indicates whether a strike took place in a private enterprise or SOE, it does not provide specific company details. Outcomes include negotiation, government or union intervention, police standby, and police suppression.

The CLB data has limitations. First, it is unable to record outcomes from 66.6% of observations due to limited follow-up information.[38] Also, it only captures short-term outcomes, not long-term outcomes like wage or working conditions. As a result, I can not assess workers' conditions post-strike. Second, it lacks firm information, making it impossible to link strikes to specific firms and their government connections. To mitigate the lack of firm information, I employed city-level corruption data from Chen and Kung (2019) in Section 7 for analyzing the mechanism behind corruption and strikes.

Table 1 presents a detailed summary of strike incidents from the CLB Strike Map. Construction and manufacturing industries account for nearly half of all strikes, and most strikes occur in private enterprises. Additionally, the majority of strikes are small-scale, involving fewer than 100 participants, with causes being wage arrears. Columns (1) to (4) summarize the count of strikes on a city-month level. Most cities witness less than one strike per month, averaging 0.3 strikes per city per month from 2011 to 2020.

Table C2 details the number of strikes, weighted by population, indicating that on average a city experiences 0.9 strikes per 10 million people each month. However, as the CCSY does not capture population data for some smaller cities and ethnic autonomous regions, only 297 out of 362 cities are included in the population-weighted sample.

The CLB Strike Map captures only 5-10% of all strikes in China, with a varying sampling rate over years due to censorship.[39] Prior to my study, Campante et al. (2022) compared the CLB Strike Map

[34] Paul Mozur, "How Many People Really Use Sina Weibo?" *The Wall Street Journal*, March 12, 2013.

[35] International Telecommunication Union (ITU) World Telecommunication/ICT Indicators Database.

[36] "An Introduction to China Labour Bulletin's Strike Map." *China Labour Bulletin*, May 2022.

[37] The service industry encompasses all service sector roles, excluding those in education and transportation. Examples of strikers in this industry include staff from restaurants, salons, karaoke bars, fitness trainers, and retail workers.

[38] "An Introduction to China Labour Bulletin's Strike Map." *China Labour Bulletin*, May 2022.

[39] According to CLB, the estimated capture of 5-10% of all strikes is based on "intermittent and partial statistics issued by national and local governments in China." Also, there is no evidence that *Weibo*, operated by the private company *Sina*, allows local governments to influence its censorship by region. King et al. (2013) indicates that censorship is topic-based, not location-specific.

Table 1: Summary Statistics on Strikes, 2010-2020

	Prefecture-Month Observations				Full Sample of Strikes	
Unit = Count of Strikes	Mean	SD	Max	Min	Total Count	Proportion
Overall	0.3	0.86	15	0	13,170	1.00
Industry						
Construction	0.11	0.43	10	0	4,714	0.36
Education	0.01	0.12	4	0	521	0.04
Manufacturing	0.08	0.43	13	0	3,487	0.26
Mining	0.01	0.11	5	0	394	0.03
Service	0.05	0.25	5	0	1,987	0.15
Transportation	0.04	0.23	4	0	1,920	0.15
Others	0.01	0.09	3	0	320	0.02
Firm Type						
Private	0.17	0.58	12	0	7,594	0.58
SOEs	0.04	0.21	6	0	1,619	0.12
Foreign or Joint Venture	0.02	0.21	7	0	1,019	0.08
Unknown	0.07	0.32	6	0	3,088	0.23
Strike Form						
Protest	0.18	0.58	11	0	7,667	0.58
Sit-In	0.08	0.38	12	0	3,484	0.26
Scale (number of participants)						
Small (<100)	0.25	0.74	13	0	10,653	0.81
Medium (101 - 1,000)	0.05	0.28	7	0	2,303	0.17
Large (1,001 - 10,000)	0.01	0.1	4	0	387	0.03
Reason to Strike						
Wage Arrear	0.22	0.7	13	0	9,520	0.72
Compensation	0.02	0.18	5	0	854	0.06
Pay Increase	0.02	0.16	5	0	951	0.07
Prefecture-Month Observations	43,440					
Number of Prefectures	362					
Year Span	2011-2020					

Notes: This table summarizes the mean, standard deviation, maximum, and minimum of the number of strikes that occurred on a prefecture-month basis from 2011 to 2020. Note that observations regarding the type and scale of industry firms are mutually exclusive, but observations regarding the form and reason for a strike are not. For instance, a strike can take both the form of a protest and a sit-in, or it can be for both wage arrears and compensation. The strikes were recorded by the China Labour Bulletin (CLB) Strike Map. The last two columns on the right provide an overview of the strikes by type across the entire sample, rather than on a prefecture-month basis.

with labor dispute data from the Ministry of Human Resources and Social Security (MOHRSS), finding similar sampling rates at the national level from 2012 to 2015. I further evaluated the representativeness of the CLB data by comparing it to labor disputes recorded on China Judgements Online (CJO). Established in July 2013, CJO archives all court cases and decisions in China. Despite CJO's comprehensive documentation of labor disputes, it can not be used for my main analysis as its records began only after the first CCDI inspections in May 2013.

Figure B1 compares the trends in monthly nationwide strike incidents recorded by CLB with labor disputes recorded by CJO from 2010 to 2020. From late 2013 to early 2016, both data show similar growth, aside from a brief CLB strike spike in December 2015 and January 2016. After 2016, CLB-recorded strikes decline, while CJO labor disputes continue to rise.[40] Even with post-2017 divergence, the CLB sampling rate from 2013 to 2016 should not be a major concern for my study,

[40]The divergence in trends between the CLB Strike Map and labor disputes tracked by CJO, especially post-2017, should not pose a significant concern. This discrepancy surfaced two years after the first CCDI inspection wave ended. It is likely that worker activism found a new equilibrium under the changed political environment post-inspections, while court-submitted labor disputes pursued a distinct equilibrium. For example, surveillance technology use for strike monitoring may have expanded in the long-term after the inspections.

as it primarily examines the aftermath from CCDI inspections, which largely occurred during 2013 to 2016. Moreover, treatment effect patterns using CJO labor disputes and the CLB Strike Map largely align, as explained in Section 6.5.

Corruption Investigation Dataset

The Corruption Investigation Dataset (CID), compiled and cleaned by Wang and Dickson (2021), is a collection of $18,947$ corruption investigations conducted between 2012 and January 2017. Each record denotes a corruption investigation involving a public official in a specific prefecture-level city during a certain month and year. It includes the official's name, position title, CCP rank, and the investigation reason. All records in the CID were obtained from a public records database managed by *Tencent*, a Chinese tech giant. Hence, every record represents a publicly recognized corruption investigation.

The CCP rank spans a scale from 1 to 10, with 1 indicating a state level official and 10 a deputy office level official, as outlined by China's Civil Servant Law. This study identifies high-profile officials as those with ranks 1 to 6, i.e., deputy-bureau-director level or higher, because they attracted both local and national media coverage as well as non-zero mentions on *Weibo* during inspections. Further details and position examples for each CCP rank are given in Table C1.

Table 2 provides a breakdown of corruption inspections by CCP rank and reason for investigation, which include bribery, embezzlement (procurement of public funds), rule violation, and others. Only 7% of inspections targeted officials at or above deputy-bureau-director (ranks 1 to 6), with the rest focusing on county-level officials or lower. Most investigations dealt with bribery and embezzlement. Rule violation investigations, including breaches of law and CCP discipline, constituted less than 20% of total inspections. The "other" category, encompassing negligence of duty and moral issues like adultery, accounted for 10% of inspections.

Figure 2, panel (a), displays the correlation between corruption inspections and strike incidents in China from 2011 to 2020. Corruption inspections notably surged from mid-2013 to mid-2014 and in 2015, peaking between 800-$1,000$. Strike incidents also had periodic peaks but experienced a significant increase in late 2015 and early 2016, a few months post the corruption inspection peak. Post 2016, corruption inspections declined nearly to zero, while strikes continued to peak seasonally. These strike peaks are tied to wage arrears in the construction industry shown in panel (b), as most strikes originate from this sector when workers demand annual payment after year-long construction projects.[41]

4.2 Complementary Data

China Judgements Online

China Judgements Online (CJO) is a public database containing extensive court decision records in China. Launched in July 2013, it mandates all court decision uploads since November 2013 and became a complete record of court decisions by June 2015. The CJO data has over $330,270$

[41] According to Pringle (2002), construction workers typically work for a year on a project and receive payment at the end. If the project underperforms financially or due to other reasons, they may not be paid. Consequently, workers often strike in January and February, around the Lunar New Year, when they need funds for family visits.

Table 2: Summary Statistics on Inspections, 2012-2017

Rank	Total	Bribery	Embezzlement	Rule Violation	Others
1 = National Leader	0	0	0	0	0
2 = Sub-National Leader	0	0	0	0	0
3 = Provincial-Ministerial Level	8	5	0	3	0
4 = Sub-Provincial (Ministerial) Level	71	34	2	34	13
5 = Bureau-Director Level	398	280	47	113	32
6 = Deputy-Bureau-Director Level	860	626	84	243	63
7 = Division-Head Level	2,211	1,492	337	485	202
8 = Deputy-Division-Head Level	1,998	1,335	314	413	200
9 = Section-Head Level	8,880	5,309	1,955	1,558	1,112
10 = Deputy-Section-Head Level	4,502	2,686	918	822	539
Total Inspections	18,947				
Years Span	2012-2017				

Notes: The table summarizes the corruption inspections carried out by the CCDI inspection team based on the reasons, such as bribery, embezzlement, violation of rules, and others, for each rank within the Communist Party of China (CCP). Note that the reasons for inspections are not mutually exclusive. For example, an official can be inspected for both bribery and rule violation. The information is obtained from the Corruption Investigation Database (CID) compiled by Wang and Dickson (2021), which provides a comprehensive record of corruption investigations from 2012 to February 2017.

distinct labor dispute records from July 2013 to 2020, complementing the CLB Strike Map. It was not used for primary analysis due to post-2013 data collection, lacking sufficient pre-inspection observations. The CJO data mainly serves to validate the CLB Strike Map's representation and test the baseline results' robustness.

China City Statistical Yearbook

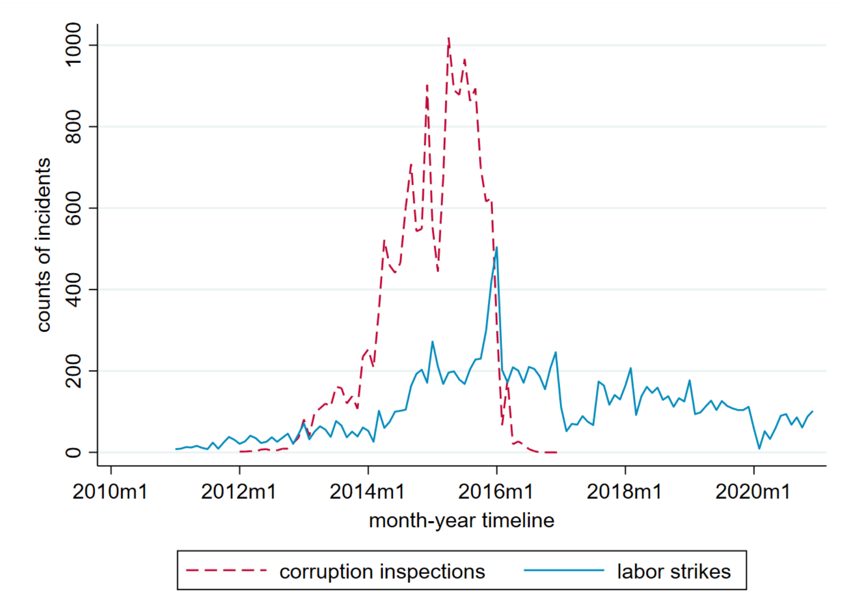
The China City Statistical Yearbook (CCSY) provides yearly data on city characteristics such as GDP, population, employment, wages, and government revenue from 2011 to 2020. It is mainly used for control variables and validating the identification assumption in Section 5.3. Its annual frequency limits its use as an outcome for anti-corruption inspections due to missing monthly fluctuations. Some small cities and cities with sizable minority populations either lack data or have inconsistent observations over time. Despite these constraints, the CCSY is the most extensive dataset for city-level attributes in China.

Social Media

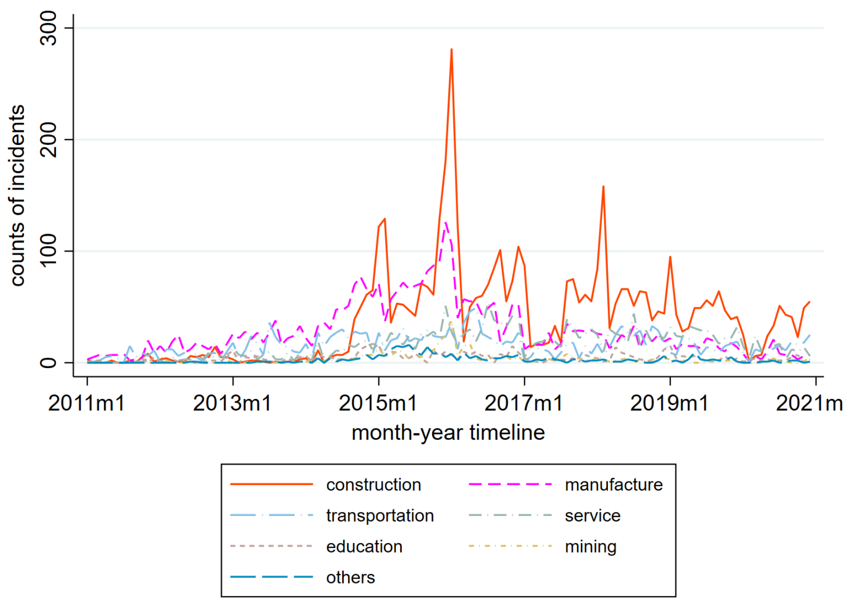
I scraped $53,197$ *Weibo* posts, each containing names and positions of high-profile officials from the CID records. Posts for low-ranked officials, with little social media mentions, were excluded. Each post's content, location, date, interaction statistics were recorded, as were sender's account details, followers/followings, and available demographic information. Each account was categorized as public (like news accounts) or private. Public accounts generated 44.6% of posts, with the rest from private users.

Figure 2: Monthly Corruption Inspections & Strikes, 2011-2020

(a) Inspections & Strikes by Month



(b) Strikes by Industry & Month



Notes: The figure displays the monthly trends in the number of strikes and corruption inspections from 2011 to 2020. The data for strikes is sourced from the China Labour Bulletin (CLB) strike map, covering the period from 2011 to 2020. The data for corruption inspections is taken from the Corruption Investigation Dataset (CID), which was compiled by Wang and Dickson (2021) and encompasses all corruption investigations carried out during Xi Jinping's anti-corruption campaign from 2012 to 2017.

China Migrants Dynamic Survey

The 2011 China Migrant Dynamic Survey (CMDS) is used to examine varied strike outcomes in cities with different migrant worker connections before the anti-corruption campaign. The CMDS offers surveyed information on $153,521$ migrant workers in 2011, sampling through a probability-proportional-to-size method.[42] It records variables on migrant workers' demographics, migration details, lifestyle, and quality of life.

5 Empirical Strategy

I conducted two event studies to investigate the relationship between anti-corruption inspections and strikes. The first event study analyzes the immediate, or first-stage effects of the anti-corruption campaign by observing the rise in corruption inspections and accompanying public attention in a city after the arrival of the CCDI inspection team. The second event study observes whether there is a rise in strikes after the CCDI inspection team starts their high-profile inspections. As previously defined, high-profile inspections target officials above the deputy-bureau-director rank.

The logic behind conducting two event studies is to first provide evidence that the inspections were indeed occurring and attracting public attention. Consequently, the increase in inspections had a reasonable basis to potentially impact workers' decisions to strike. The primary difference between the first stage and the baseline lies in the definition of the event: the first stage considers the initial inspection following CCDI's arrival in a city, while the baseline refers to the first high-profile inspection within the city. The first stage and baseline also differ in outcomes, which are further explained in sections 5.1 and 5.2.

The major specification for the two event studies is as follows:

$$\mathit{outcomes}_{cm} = \sum_{k=\underline{k}}^{-2} \lambda_k \eta_{cm}^k + \sum_{k=0}^{\bar{k}} \lambda_k \eta_{cm}^k + \boldsymbol{X}_{cy}\beta + \alpha_c + \alpha_m + \varepsilon_{cm} \tag{7}$$

where $\mathit{outcomes}_{cm}$ represents the outcomes of interest in city c during month-year m. The event indicator η_{cm}^k takes a value of 1 if it is the k^{th} month relative to the event in city c, for the month-year m. The months range from $\underline{k}$ months before the event to $\bar{k}$ months after the event. λ_k captures the average monthly change in the outcomes relative to those in one month prior to the event in city c. $\boldsymbol{X}_{cy}$ is a vector of city characteristics, including rate of natural increase (RNI), population, GDP per capita, and average wage in city c during year y. α_c and α_m are city and month-year fixed effects, respectively. The standard errors are clustered at the city level. The error term ε_{cm} captures any random noise.

Furthermore, to ensure the robustness of this primary specification, I employed the estimators of Callaway and Sant'Anna (2021) and Gardner (2021) as tested in Section 6.5. I also explored multiple alternative specifications, including the use of an inverse hyperbolic sine (IHS) transformation for the outcome variable and the zero-inflated negative binomial model (ZINB) alongside the main specification. All these approaches yielded similar patterns to the main results.

[42] The probability-proportional-to-size sampling method was used, with the sample size being proportional to the migration inflow of each province in the first quarter of 2011. Within each province, the sampling was further refined based on the migration inflow of each prefecture, county, and residential district.

To complement the major specification, I conducted a quarterly aggregated alternative specification to estimate parameters as follows:

$$outcomes_{cq} = \delta \times post\ event_{cq} + \boldsymbol{X}_{cy}\beta + \alpha_c + \alpha_q + \varepsilon_{cq} \tag{8}$$

where q represents a quarter of a year. *post event*$_{cq}$ is 1 if city c is in quarter-year q after the event and always 0 for cities that never experienced an event. δ measures the average quarterly change in outcomes in city c after the event compared to cities that not yet and never experienced an event. The analysis incorporates the Callaway and Sant'Anna (2021) estimator to ensure that all treated cities have a valid control group. In contrast to the graphical representation of dynamic outcome changes in Equation (7), Equation (8) presents the estimated parameters in a concise format.

The key identification assumption for the baseline is that, absent inspections, strike trends in cities with varying inspection times would be identical. In Section 5.3, I tested for the correlations between inspection timings and various city-level determinants. I also addressed potential anticipation effects and comparability between cities that have experienced inspections early, late, or not at all. Beforehand, I further explained how the first stage and baseline analyses differ in event design, outcome variables, and the assignment of treatment and control groups.

5.1 First Stage

The first event study examines the impact of the arrival of a CCDI inspection team on city-level corruption inspections. Outcomes include the numbers of total inspections, high-profile inspections, and the social media posts discussing these high-profile inspections. An event is defined as the first corruption inspection in a city after the CCDI inspection team arrives in the province.[43] Given the CCDI inspection team's presence in all provinces, all but 4 cities underwent inspections. Therefore, treatment groups include cities with earlier inspections, while control groups consist of cities inspected later and the four cities that had no inspections.

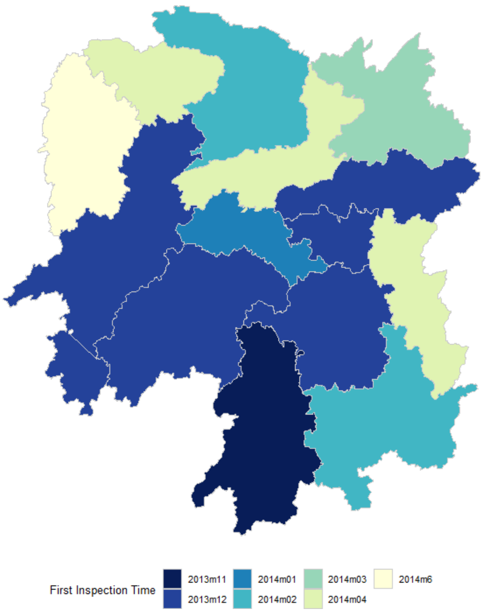
Figure 3, panels (a) and (b), illustrate the timings of the first inspection in cities across two Chinese provinces, Hunan and Anhui, where the CCDI team arrived in November 2013. In Hunan, the first inspection took place in Yongzhou within the same month, with first inspections in other cities between December 2013 and June 2014. In Anhui, Anqing, Hefei, Wuhu, and Xuancheng underwent their first inspections in November 2013, followed by other cities from December 2013 to July 2014. This visualization underscores that a city's first inspection could occur several months post the CCDI team's provincial arrival.

Based on Figure B2, panel (a), the timing of the first inspection by the CCDI team, conditional on their provincial arrival, varied across cities, ranging from May 2013 to August 2015, and spanning 26 distinct treatment timing cohorts. Notably, out of 362 cities or autonomous regions, 4 experienced no inspections between 2012 and 2017, even with the CCDI team's presence in their province.

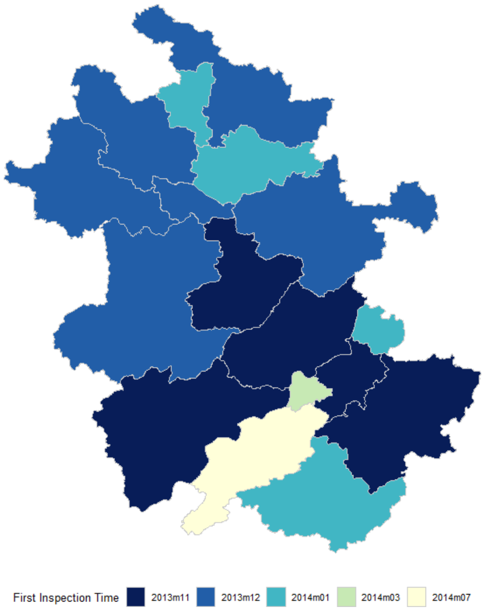
[43] Alternatively, I can use arrival times of the inspection teams at the provincial level as the treatment timing, which shows similar results. However, it is only through using city-level temporal variations that I am able to examine more precise responses to inspections.

Figure 3: Timing of First Corruption Inspections & High Profile Inspections

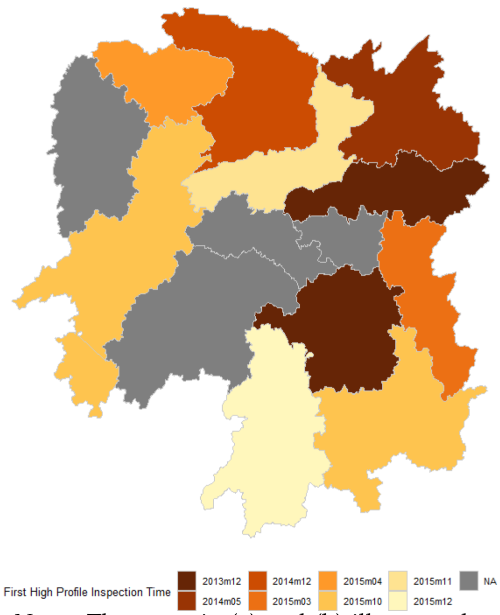
(a) First Inspections, Hunan Province



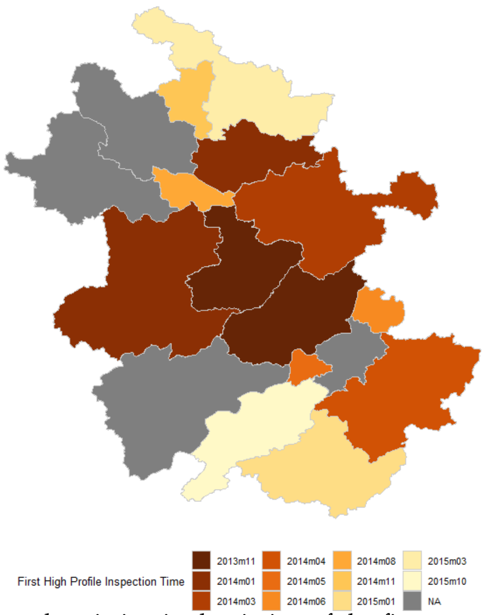
(b) First Inspections, Anhui Province



(c) First HP Inspections, Hunan Province



(d) First HP Inspections, Anhui Province



Notes: The maps in (a) and (b) illustrate the temporal variation in the timing of the first corruption inspection case in Hunan and Anhui respectively following the arrival of the CCDI inspection team within the province. The maps in (c) and (d) show the temporal variation in the timing of the first high-profile corruption inspection case in Hunan and Anhui respectively after the arrival of the CCDI inspection team within the province. It should be noted that the CCDI inspection team arrived in both Hunan and Anhui in November 2013. The information on corruption cases is obtained from the Corruption Investigation Dataset compiled by Wang and Dickson (2021), which provides a comprehensive record of corruption inspections from 2012 to the February 2017.

5.2 Baseline

The baseline specification analyzes changes in strikes before and after the first high-profile inspection in a city, given the arrival of the CCDI inspection team within the province. Outcomes include the number of strikes and strikes per 10 million people.[44]

I prefer the unweighted count of strikes as the primary outcome variable for two reasons. First, the population data from CCSY is inconsistent, missing population observations in less populated and ethnic minority areas. Of 362 cities, only 297 have population observations. Second, strike changes can occur rapidly, perhaps on a monthly basis following a city's high-profile inspection. My research focuses on short-term worker strike responses to corruption inspections, which could be obscured by long-term population growth.

Using high-profile inspections as the baseline event has several advantages. Since high-profile inspections garner media attention, workers can learn about these events through *Weibo*, news channels, or personal conversations. Moreover, the exposure of a high-profile inspection boosts workers' perception of Xi's anti-corruption campaign's authenticity, given the even-handed arrest of both upper and lower-tier officials (Wang and Dickson, 2021). Lastly, out of 362 cities, 128 never experienced high-profile inspections. Therefore, the treatment group comprises cities with earlier high-profile inspections, while the control group includes cities with later or no high-profile inspections, making it substantially larger than the first stage's control group.

It is crucial to note that interpreting the role of a first high-profile inspection goes beyond it being a mere inspection. Of the 234 cities that ever experienced a high-profile inspection during the anti-corruption campaign, nearly 60% experienced more than one such inspection. Even if many cities had multiple high-profile inspections, they were classified as the treated group from their first high-profile inspection onward. In this study, I used the first high-profile inspection as the only treatment instead of multiple events, believing it provides the cleanest shock. Yet, the presence of multiple inspections in many cities means one can not view the first inspection as the sole treatment influencing outcomes. In other words, the first high-profile inspection in a city signifies the moment when the anti-corruption campaign first gained local public visibility.

Figure 3, (b) and (c) display the timing of the first high-profile inspection in Hunan and Anhui after the arrival of the CCDI inspection team. Unlike the timings of the first inspections, the first high-profile inspection usually occurs between 0 and a few months after the first inspection within the city. Additionally, 4 out of 14 cities in Hunan and 4 out of 16 prefectures Anhui never experienced a high-profile inspection.

Figure B2, panel (b), demonstrates the timing of the first high-profile inspections across all prefecture-level cities and autonomous regions in China, encompassing 28 treatment cohorts. Compared to the first inspection scenario, this map indicates a larger control group, as several cities per province never underwent high-profile inspections. Despite some less populous regions not having any high-profile inspections, a few highly populated southeastern cities also did not experience such inspections.

[44] Appendix E further explores the external margin by presenting results for the outcome, defined as a binary variable set to 1 if a strike occurs.

Certainly, I can employ an alternative definition for the baseline event, which aligns with the first stage definition of the first inspection in the city. In Appendix D, I provided consistent results using this definition. However, a notable drawback arises: there is a delayed rise in strikes following a city's first inspection, indicating that the first inspection, without being high-profile, might lack adequate attention or authenticity to prompt immediate strikes.

5.3 Identification Assumption

The baseline event study assumes that in the absence of inspections, cities with different inspection timings would have exhibited the same trend in strikes over time. This does not require random inspection timings, instead, it presumes no link between inspection timings and other factors influencing strikes.

Two major scenarios could violate this identification assumption. First, if CCDI sets inspection timings based on the economic and political conditions of a province or city, which in turn may correlate with strikes. For instance, if inspection timings are tied to cities' unemployment levels or prior strike counts, this could create bias to the effects of the inspections. Second, workers and the local government can anticipate the inspection timings. Workers may plan to strike coinciding with the high-profile inspections. Similarly, the local government could strategically change media reporting of strikes around the inspection timings.

To address the first scenario, I conducted a regression analysis to investigate the link between inspection timings as outcomes and city characteristics before the anti-corruption campaign in 2011 as treatments. Table C3 shows that the majority of city characteristics are insignificant, with the exception of the Rate of Natural Increase (RNI) at a 5% significance level, relevant only to the timing of first inspections for unknown reasons. However, controlling for RNI does not significantly affect the main results. Furthermore, there is no correlation between inspection timings, the number of 2011 and 2012 strikes, and pre-campaign corruption levels — measured via land transactions by firms affiliated with senior CCP officials.

To check for an anticipation effect, I can visually examine if the monthly changes in strikes are significantly different from 0 before the first high-profile inspections. If workers anticipate inspections, they may strike preemptively. Therefore, I also controlled for the number of inspections in neighboring cities to mitigate anticipation effects when presenting the baseline results. Section 6.2 shows no pre-trend in strikes, and controlling for neighboring inspections does not significantly influence the main outcomes.

6 Main Results

6.1 First Stage Outcomes

Figure 4, panel (a), shows an immediate increase in a city's monthly total inspections following the first inspection post CCDI's provincial arrival. Within one year of the first inspection, monthly inspections in a city tripled, and within two years, it increased six-fold. In addition, high-profile inspections, as a fraction of total inspections, saw a nearly four-fold increase within six months of the first inspection, according to panel (b). Although the rate of increase slowed down after six months for total and high-profile inspections, it did not decline until two years after the first inspection. Overall, both total and high-profile inspections in a city increased three-fold per quarter post the CCDI's first inspection, as shown in Table 3, columns (1) and (2).

The high-profile inspections received significant public attention on *Weibo*. Figure 4, panel (c), illustrates a spike in monthly *Weibo* posts mentioning high-profile inspection officials' names two months after the first CCDI inspection in a city, and this spike persisted for nearly a year. In summary, the average number of *Weibo* posts per quarter in a city tripled following the first inspection, as indicated in Table 3, column (3).

6.2 Baseline Outcomes: Total Strikes

Figure 5 shows an immediate increase in monthly count of strikes following the first high-profile inspection in a city, given the CCDI inspection team's provincial arrival. Before the inspection, strike changes remained stable and insignificant, suggesting similar strike trends across all cities without inspections, thus indicating a low likelihood of anticipation effects. One year after the inspection, a city's monthly strike average doubled, increasing by 0.2 from a pre-inspection average of 0.16. Within two years, strikes tripled, but beyond that period, they receded to just above pre-inspection levels.[45]

Table 3 demonstrates a twofold increase in the quarterly number of strikes in a city following the first high-profile inspection. This finding remains consistent across all specifications, accounting for various controls such as city characteristics and neighboring inspections, as shown in columns (5)-(6) and (8)-(10). However, the effect is doubled in the specification from column (4) without city characteristics or neighboring controls, and with a restricted sample size from 2011-2016.[46]

Table 3, column (7), shows a short-term 70% increase in strikes per 10 million people post-inspection. However, the long-term population-weighted increase in strikes, though still positive, becomes insignificant in column (11), likely due to slower strike growth compared to population growth over years. Regardless, my key finding, as shown in Figure 5, is a rapid monthly short-term increase in strikes, lasting two years post-inspection.

In Appendix D, I presented consistent results with the main findings by using the first inspection (rather than the first high-profile inspection) as an alternative baseline event definition. Additionally, in Appendix E, I analyzed the extensive margin of strikes by changing the outcome to whether there is a strike in a city per quarter. The results demonstrate an 18 percentage point increase in the likelihood of strikes occurring in cities that had not previously encountered any strikes.

6.3 Baseline Outcomes: Strike Characteristics

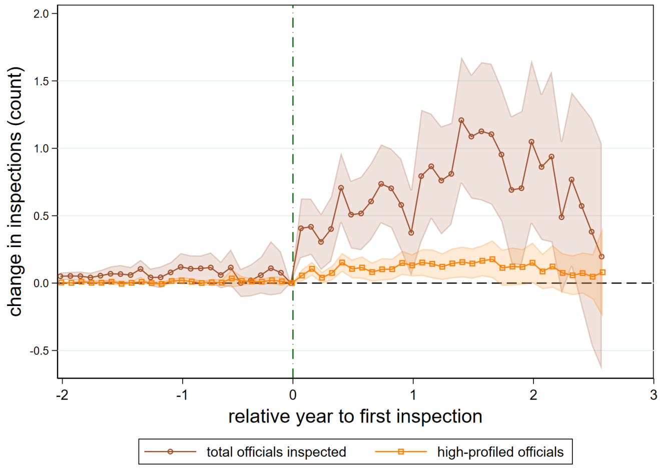
The CLB Strike Map's granular data facilitates an in-depth analysis of strike nature and the industries involved. Primarily, it reveals a notable increase in strikes within private construction and

[45] The results generally align with the baseline when the event changes to the first inspection post-CCDI's provincial arrival, despite a lagged strike increase and lack of never-treated cities in the control group - see Appendix D.

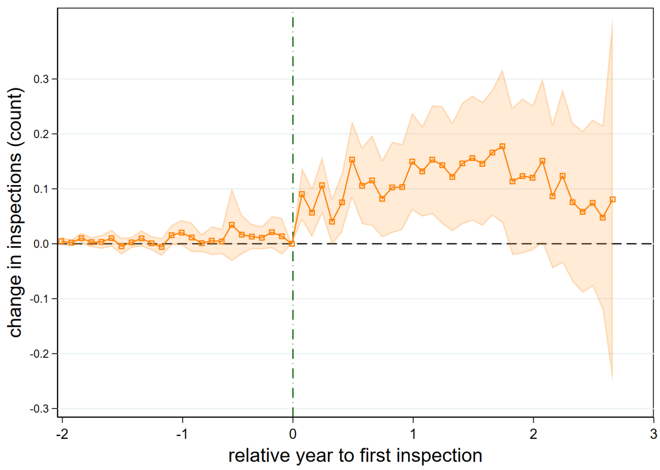
[46] Controlling for city characteristics results in a decrease in sample size, primarily due to inconsistent city characteristic observations from CCSY for many small cities and cities in ethnic autonomous regions. Nonetheless, CCSY remains the most comprehensive data source for Chinese city characteristics.

Figure 4: First Stage Outcomes

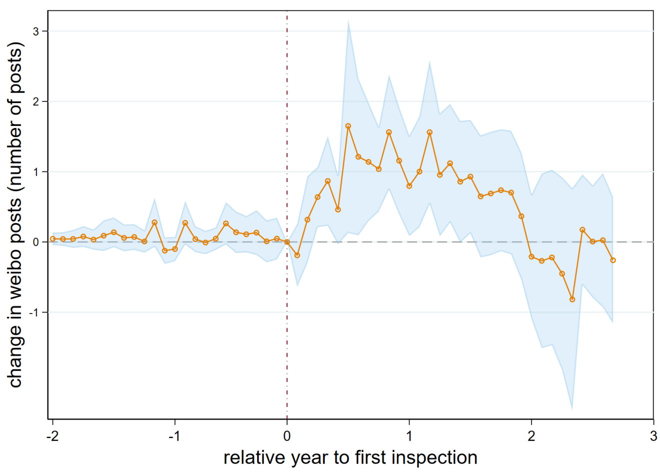
(a) Total & High-Profile Inspections



(b) High-Profile Inspections

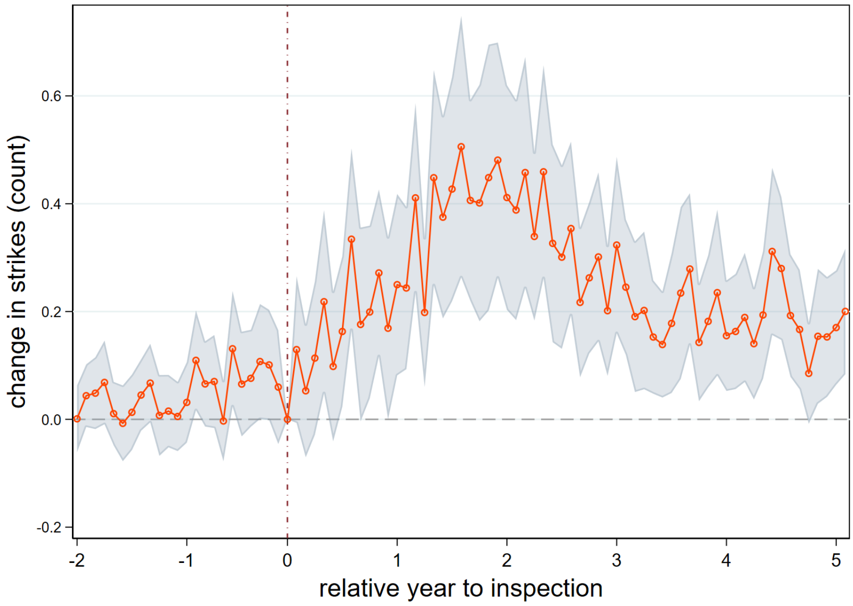


(c) *Weibo* Posts



Notes: The three graphs demonstrate the estimated λ_k from Equation (7). Panel (a) shows the change in total and high-profile inspections before and after the first inspection within city conditional on the arrival of CCDI inspection team within province. Panel (b) shows the change in high-profile inspections only. Panel (c) shows the change in *Weibo* posts mentioning the high-profile inspection within city. The sample contains $23,892$ observations from 362 prefectures from 2011 to July 2016.

Figure 5: Baseline Outcomes



Notes: The graph demonstrates the estimated λ_k from Equation (7), which shows the changes in the number of strikes in the months before and after the first high-profile inspection in a city. The hollow red dots represent the estimates in each month and the light blue region represents the standard errors. The dotted vertical line indicates the timing (one month prior) of the first high-profile corruption inspection case in a city, given the arrival of the CCDI inspection team in the same province. The sample includes $43,440$ observations from 362 prefectures from 2011 to 2020.

Table 3: First Stage and Baseline Outcomes of Inspections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	First Stage			Baseline							
	Total Insp.	HP Insp.	Weibo Posts	*Short Term*				*Long Term*			
				Strike Incidents			Strikes per 10M ppl	Strike Incidents			Strikes per 10M ppl
Post 1st Insp.	1.21***	0.11***	4.65***								
	(0.30)	(0.04)	(1.54)								
Post 1st HP Insp.				0.93***	0.43***	0.51***	0.90**	0.40***	0.45***	0.48***	0.39
				(0.16)	(0.16)	(0.17)	(0.38)	(0.13)	(0.16)	(0.17)	(0.33)
Average, Dep Var Before Insp.	0.68	0.06	2.00	0.48	0.48	0.48	1.33	0.48	0.48	0.48	1.33
SD, Dep Var Before Insp.	1.96	0.41	10.15	1.35	1.35	1.35	4.77	1.35	1.35	1.35	4.77
Observations	5,060	5,060	5,060	8,688	6,642	6,642	6,642	14,480	10,074	9,898	9,898
Year Span	2012-2016	2012-2016	2012-2016	2011-2016	2011-2016	2011-2016	2011-2016	2011-2020	2011-2020	2011-2020	2011-2020
City FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Quarter-Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
City Controls	YES	YES	YES	NO	YES	YES	YES	NO	YES	YES	YES
Neighbor Insp.	NO	NO	NO	NO	NO	YES	YES	NO	NO	YES	YES

Notes: The table presents the coefficients $\hat{\delta}$ from Equation (8) using the Callaway and Sant'Anna (2021) estimator. The table is divided into two sections: the first stage impacts in Columns (1) to (3) and the baseline impacts in Columns (4) to (9). The first stage impacts demonstrate the average change in the number of total inspections, high-profile inspections, and *Weibo* posts in the quarters after the first CCDI inspection, compared to the cities that have not yet experienced such an inspection. The baseline impacts show the average change in the number of strikes in the quarters after the first high-profile inspection in a city, conditional on the arrival of the CCDI in the province, compared to the strikes in cities that have not yet and never experienced a high-profile inspection. The variation in the observations is due to the absence of certain characteristics in the control variables in different cities. The standard errors are presented in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

manufacturing firms, with wage arrears as the primary drive. Figure 6 highlights that the construction sector led the rise in strikes, followed by manufacturing, while service, transportation, mining, and education sectors saw negligible strike changes. Figure 7, panels (a) and (b), show that the singular cause of the strike surge was wage arrears, not a demand for higher pay, aligning with the fact that over 70% of labor disputes in China are wage arrears-related (Lee, 2004). Furthermore, panels (c) and (d) illustrate that the majority of this increase in strikes originates from private firms, not state-owned enterprises (SOEs).

The aforementioned results indicate that construction and manufacturing workers harbor greater grievances, consistent with previous studies like Chan (2001) and Ngai (2005). This reinforces Proposition 1, which posits that higher grievances lead to increased strike participation. These workers struggle to secure promised wages, often due to the absence of valid labor contracts and legal understanding (Chan, 2001; Cheng et al., 2015), renders them more prone to strike when opportunities like corruption inspections arise, facilitating the voicing of complaints and securing payment. previous studies like Ngai (2005) has shown that workers from construction and manufacturing sectors had more grievances than others, hence my findings support previous studies.

A closer look at the CLB data, as illustrated in Figure 8, reveals most strikes were small-scale, engaging fewer than 100 participants, and primarily took the form of protests and sit-ins. This underscores Chinese workers' political involvement, who frequently arrange informal, small-group strikes instead of relying on union mobilization. It also suggests that media censorship in China may obscure larger strikes, making small-scale strikes more visible and recorded. However, a significant rise in medium-scale strikes, involving 101 to $1,000$ participants, is also evident.

Only a third of the strikes from the CLB data had recorded outcomes, predominantly involving police presence as Figure B3 indicates. This does not necessarily imply police disbanded the strikes through arrests or violence, but rather marks their presence at strike sites. There was no discernible change in the number of peaceful negotiations or violent incidents post-inspections.

6.4 Baseline Outcomes: Strike Network and Spillovers

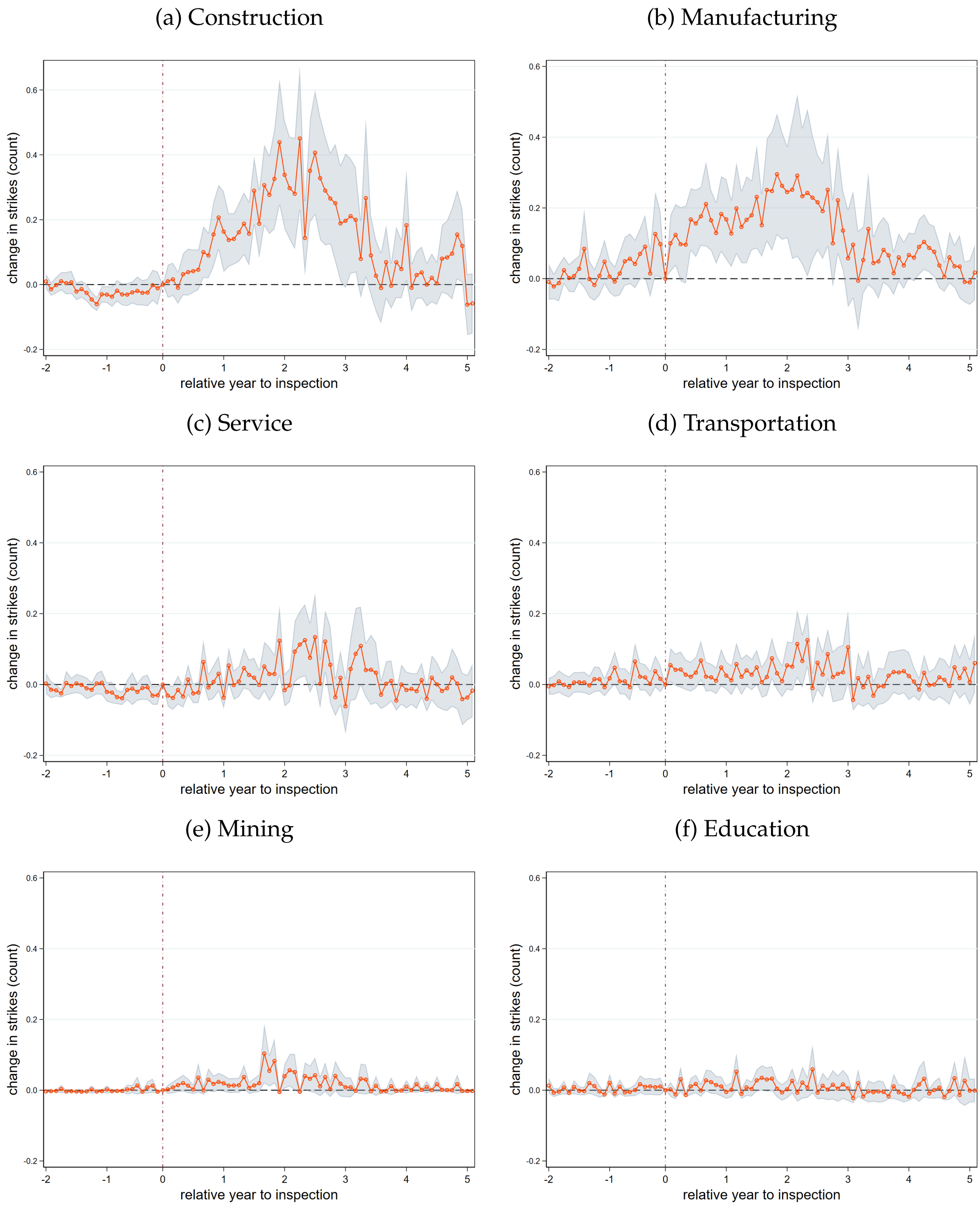
A crucial question emerging is the potential network and spillover among workers affecting strikes temporally and geographically. Proposition 2 shows that a stronger workers network within a city could encourage more strikes. However, a surge in strikes from other cities may either incentivize or crowd out strikes in the own city. For example, if city A sees a post-inspection strike surge, does it impact workers' decision to strike in city B, which has not yet been inspected? If so, do these workers in city B strike more or less?

This section aims to address three questions: First, does the different timings of first high-profile inspections across cities influence workers' decision to strikes differently, and in which direction? Second, does a strike increase in a city impact strikes in its neighboring cities, and in which direction? Last, how does the migrant network size influence the number of strikes within a city?

Inspection Timing

One may question if the staggered inspection timing differently affects cities inspected earlier versus later. Proposition 2 suggests intercity influences - workers in cities yet to undergo high-

Figure 6: Strikes by Industry

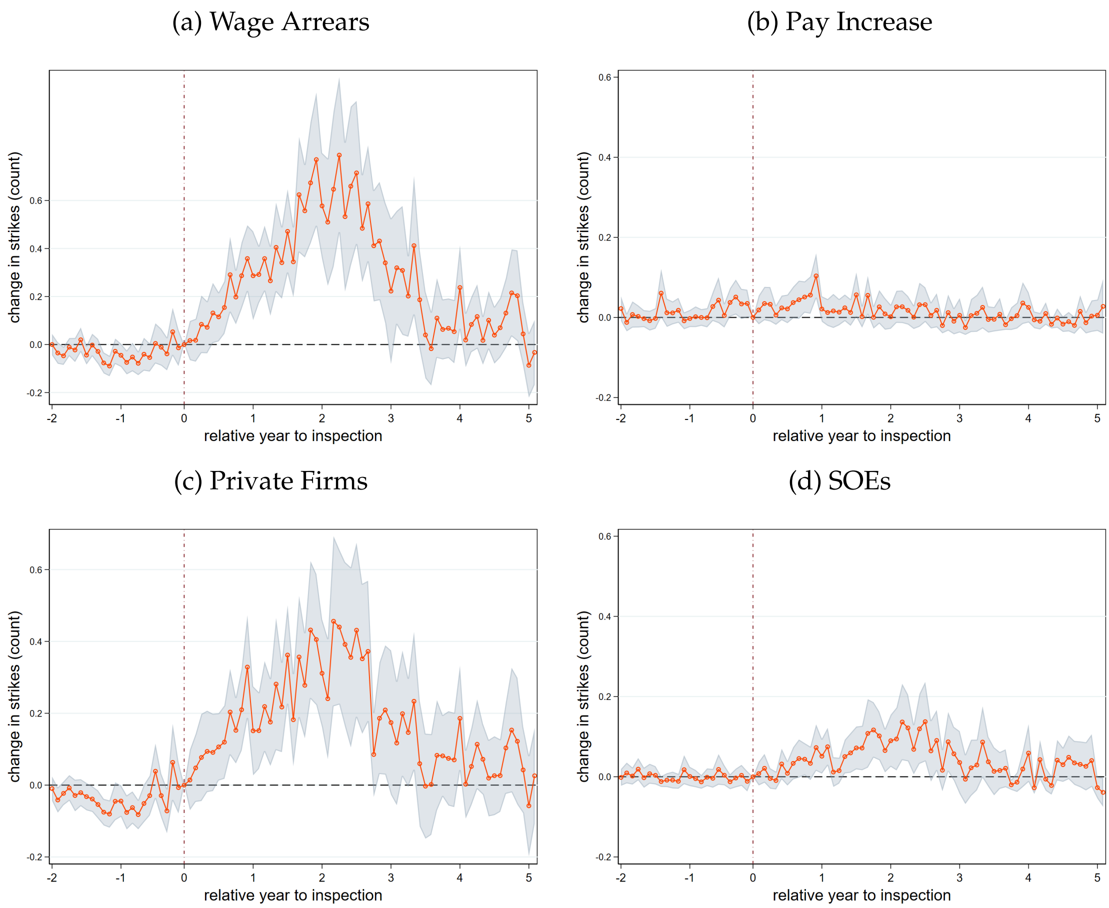


Notes: The series of graphs show the estimated λ_k from Equation (7) using strike observations from different industries. The hollow red dots depict the estimates and the light blue region signifies the standard errors. The dotted vertical line indicates the timing (one month prior) of the first high-profile corruption inspection case in a city, given the arrival of the CCDI inspection team in the same province. The sample includes 43,440 observations from 362 prefectures from 2011 to 2020.

profile inspections may reevaluate their decision to strike after witnessing strikes in an inspected city in their province. While such occurrences could embolden workers to strike, they could also discourage strikes due to potential media attention being crowded out by strikes in other cities.

Notably, this "network" or "spillover" effect differs from the anticipation effect. In the latter, workers strike in advance of inspections, anticipating the arrival of the CCDI inspection team. The

Figure 7: Strikes by Reason and Firm Type



Notes: The series of graphs demonstrate the estimated λ_k from Equation (7) based on the reasons for the strikes and the type of firms involved. The hollow red dots represent the estimates and the light blue region signifies the standard errors. The dotted vertical line indicates the timing (one month prior) of the first high-profile corruption inspection case in a city, given the arrival of the CCDI inspection team in the same province. The sample includes $43,440$ observations from 362 prefectures from 2011 to 2020.

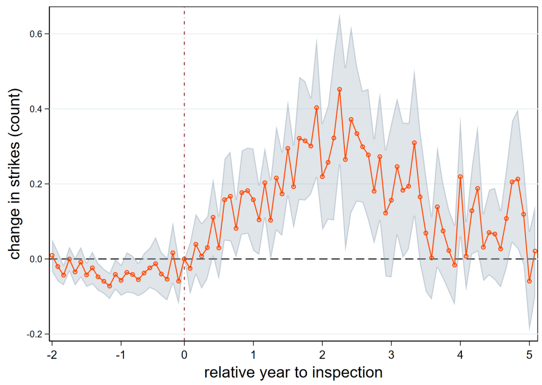
spillover effect, however, arises when workers strike after observing strikes in other cities within the province post-inspection, causing a spillover of strikes from inspected to yet-to-be-inspected cities. For example, an anticipation effect is present if workers preemptively strike in cities inspected early, but the network effect should not occur in these cities.

I divided cities in a province into four waves based on treatment timing. First-wave cities experienced their first high-profile inspections within 0 to 1 quarter of the CCDI inspection team's provincial arrival. The second wave had first high-profile inspections 2 to 3 quarters post-arrival, the third 4 to 5 quarters after, and the fourth or last wave more than 6 quarters later.

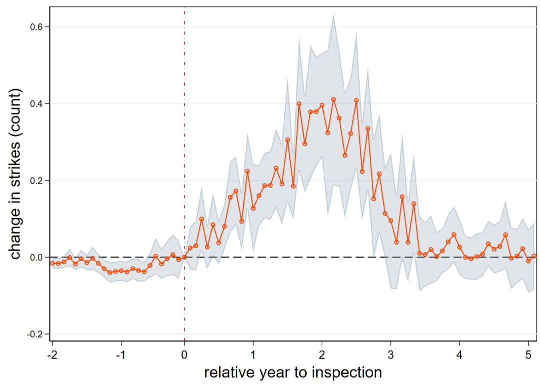
First, I analyzed the baseline treatment effects for cities in each wave. An event is a city's first high-profile inspection, with the treatment group being cities inspected in each wave, and the control group being cities without any high-profile inspections. Next, I investigated how high-profile inspections in earlier-wave cities impact the strikes in cities that are yet to be inspected. Here, an event is the first high-profile inspection in an early wave city, while the outcome is the strike count in cities inspected in later waves. In this case, the treatment group consists of later-wave inspected cities, and the control group remains the cities without high-profile inspections.

Figure 8: Strikes by Form and Size

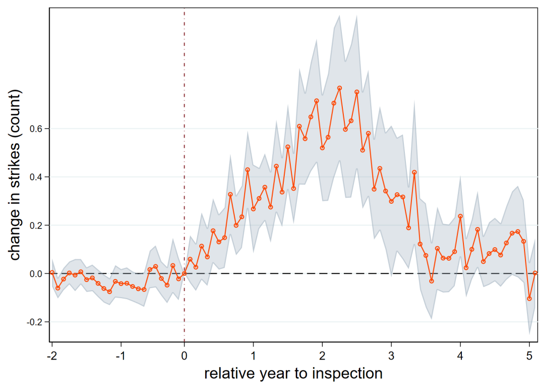
(a) Protests



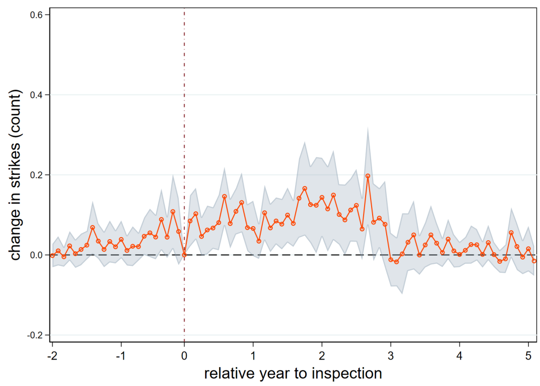
(b) Sit-Ins



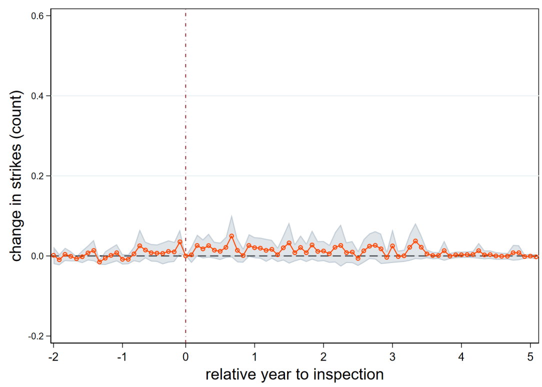
(c) Small (< 100)



(d) Medium ($101 - 1,000$)



(e) Large ($1,001 - 10,000$)



Notes: The series of graphs demonstrate the estimated λ_k from Equation (7) based on the form (protests or sit-ins) and size (small, median, large) of the strikes. The hollow red dots represent the estimates and the light blue region signifies the standard errors. The dotted vertical line indicates the timing (one month prior) of the first high-profile corruption inspection case in a city, given the arrival of the CCDI inspection team in the same province. The sample includes $43,440$ observations from 362 prefectures from 2011 to 2020.

Table 4 shows that strikes rose more in earlier wave cities, despite first-wave cities having more pre-inspection strikes than cities in the second, third, and fourth waves. First-wave cities had almost a 100% strike increase, second-wave cities around 50%, as shown in columns (1) and (5). Third and fourth-wave cities saw no significant rise, as shown in columns (8) and (10).

The diminished rise in strikes in the third and fourth waves could be due to workers in these cities striking earlier than own inspection times, influenced by earlier-wave cities' strikes. As Table 4 columns (6) and (7) show, third and fourth-wave cities started striking when second-wave cities were inspected. This suggests that strikes in early-wave cities encouraged rather than discouraged strikes in other cities. Also, this increase is likely not from an anticipation effect, as neighboring cities' inspections were controlled for. If an anticipation effect was at play, later-wave cities would have shown increased strikes during the first-wave inspections, which was not the case as shown in columns (2) and (3).

Both construction and non-construction sectors show similar patterns. Table C4 illustrates a four-fold construction strike increase in fourth-wave cities after second-wave cities' first high-profile inspections. In non-construction industries as shown in Table C5, third and fourth-wave cities saw strikes rise by 100% and 50% respectively, after the first high-profile inspections of second-wave cities.

It is important to acknowledge that even the control group, or cities never undergone high-profile inspections, may have seen strike increase when early-wave cities were inspected, reducing the effect size in later-wave cities. Therefore, the rise in strikes in later-wave cities should be seen as a lower bound. It is also important to stress that the identification assumption still holds - cities follow the same strike trend in the absence of high-profile inspections. This is because later-wave and control cities only experience spillover effects from high-profile inspections. Without first-wave cities being inspected, these later-wave and control cities would not see any strike increase.

Neighboring Cities

The previous section highlights a spillover effect of strikes in cities yet to undergo high-profile inspections, influenced by earlier-inspected cities within the same province. This raises the question - do similar effects occur in neighboring cities that share a border with city A if A undergoes a high-profile inspection? Table C6, column (1) shows no significant average strike change among all neighboring cities post-inspection. However, to accurately assess this, it is important to differentiate between neighboring cities that have undergone high-profile inspections and those that have not.

Table C6 shows that the largest strike increase happened in neighboring cities not yet inspected after city A's inspection, if city A was inspected 2 to 3 quarters post-CCDI's provincial arrival. However, when city A had inspections 0 to 1 quarter after CCDI arrival, its neighboring cities did not see a significant strike increase. These results again show that strikes in inspected cities positively influence strikes in yet-to-be inspected cities, as discussed in Proposition 2. Also, they suggest this increase is less likely from an anticipation effect, and more likely a neighboring spillover effect.

Lastly, to determine if the strike rise in neighboring cities was due to anticipation or spillover effects, I separately controlled for city A's inspection and strike counts. This would clarify if neighboring cities strike in response to city A's inspection surge, strike surge, or both. The results, shown in Table C7, demonstrate significant strike increase after controlling for inspections, but not for strikes. Thus, the strike increase in not-yet-inspected neighboring cities mainly stems from workers observing strikes in other cities - pointing to a spillover effect.

Table 4: Timings of Inspections and Network Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1st High Profile Inspection Taken Place in # of Quarters after CCDI Arrival in Province									
	0-1 Quarter				2-3 Quarters			4-5 Quarters		6+ Quarters
	Own City	Other Cities			Own City	Other Cities		Own City	Other Cities	Own City
		2-3 Qtrs	4-5 Qtrs	6+ Qtrs		4-5 Qtrs	6+ Qtrs		6+ Qtrs	
1st HP Insp.	0.98***	-0.31*	-0.15	0.17	0.53**	0.25**	0.28**	-0.31	0.18	-0.32
	(0.26)	(0.18)	(0.11)	(0.12)	(0.26)	(0.12)	(0.12)	(0.26)	(0.13)	(0.31)
Avg, Dep Var Before Insp.	0.72	0.34	0.37	0.37	0.34	0.37	0.37	0.37	0.37	0.37
SD, Dep Var Before Insp.	1.93	0.75	0.85	1.05	0.75	0.85	1.05	0.85	1.05	1.05
Observations	5,862	5,862	5,862	5,862	4,424	4,424	4,424	4,541	4,541	4,560
Year Span	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020
City FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Quarter-Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
City Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Neighbor Insp.	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: The table presents the coefficients $\hat{\delta}$ from Equation (8) using the Callaway and Sant'Anna (2021) estimator. The table presents two effects. First, columns (1), (5), and (8) present the treatment effects for the cities that experienced their first high-profile inspections that took place in $0-1$, $2-3$, $4-5$, and 6 or more quarters after the arrival of the CCDI inspection team in the respective province. Second, the table examines the impact that an earlier wave of high-profile inspections in a city has on strikes in other cities within the same province that have a later wave of high-profile inspections. Such network effects are presented in columns (2)-(4), (6)-(7), and (9). The standard errors are presented in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

Migrant Workers

Proposition 2 asserts that strike participation grows with the presence of more workers with similar traits in a city. To further explore if the strike increase is prevalent among migrant workers, I examined if cities with different levels of migrant worker networks saw different changes in strike numbers.

A migrant worker network is defined using the China Migrants Dynamic Survey (CMDS) data, specifically, if the surveyed migrant's primary connections are with *Laoxiang* - fellow migrants from their original city, now working in the same current city. I calculated the proportion of such connected migrant workers in a city in 2011 before the anti-corruption campaign, to exclude any inspection-induced changes in migrant decisions.

Figure B4 and Table C8 show that cities with stronger pre-existing migrant worker networks saw a significant strike increase, especially where over 25% of migrant workers had *Laoxiang* connections in 2011. However, it is possible that such workers faced integration issues in new cities, leading to isolation and a higher strike likelihood. To address this, I controlled for the proportion of migrants reporting unhappiness or feelings of local exclusion, but it did not change the results as seen in Table C8 columns (2), (5), and (8). To ensure the effects are not due to migrants mainly from neighboring cities, as such workers might have similar networks to locals, I accounted for the proportion of inner-province migrant workers in Table C8 columns (3), (6), and (9). However, this did not alter the results.

6.5 Robustness

Data Selection

There is a concern that the CLB Strike Map might disproportionately capture labor strikes after high-profile inspections, making treatment effects reflect CLB data collection patterns rather than actual changes in workers' strike decisions. To address this, I used China Judgements Online (CJO) labor dispute court cases as an alternative labor dispute data source. Since CJO only started recording cases from July 2013, this data can not be used in the baseline analysis due to a lack of pre-treatment observations. However, I can still utilize the sample after July 2013 and compare with the baseline results to see if inspections also led to an increase in court labor disputes.

Table C9 shows the treatment effects for various types of court labor disputes, including wage arrears, insurance, and work accidents in columns (1) to (4). Results reveal an overall labor dispute increase of 50% post-first high-profile inspection, a slightly smaller effect than the baseline. Of the dispute types, wage arrears saw the largest increase of just over 50%, in line with CLB causes, followed by insurance and work accidents disputes, which rose by 30% and 25% respectively. Figure B5 shows no labor dispute pre-trends prior to a city's first high-profile inspection. Thus, the CJO labor dispute increase pattern aligns with the CLB labor strike increase pattern.

A placebo test was conducted by randomly reassigning treatment times of the first high-profile inspection in each city to verify if the strikes' increase was not simply due to a surge in CLB data collection in 2015 and 2016. If the baseline effects arose solely due to CLB recording more strikes between 2015 and 2016, an increase in strikes would still occur after the reshuffled treatment times, mostly taken place in 2013 and 2014. However, as shown in Table C9, column (5), no significant change in strikes occurs under the rearranged treatment times, suggesting that the strike increase is not a result of data collection.

Alternative Estimators

In light of recent research on difference-in-differences with staggered treatments, it is crucial to compare early treated groups to later or never treated ones. This necessity arises because treatment effects can be biased if these groups exhibit distinct treatment effects over time. While the main analysis uses Callaway and Sant'Anna (2021)'s estimator to mitigate this issue, I also utilized Gardner (2021)'s estimator to ascertain the robustness of the results. If treatment effects are valid, I would not anticipate substantial variances between the outcomes of both methods, notwithstanding their differing tactics in comparing treated groups with those untreated or yet to be treated.

Table C9, column (6) shows almost a threefold surge in strikes post first high-profile inspections using Gardner (2021)'s estimator. This demonstrates a larger impact compared to the Callaway and Sant'Anna (2021) estimator results. A comparison of strike dynamics between both estimators, shown in Figure B6, reveals that their results follow similar trends. If anything, Gardner (2021)'s outcomes show a 20% larger increase in strikes and smaller standard errors.

Many cities reported no strikes in certain months or quarters, leading to a non-normal distribution of strikes. To address this without excluding cities with zero strikes, I applied an inverse hyperbolic sine (IHS) transformation on the strikes before re-running the baseline. As shown in Table C9, column (7), strikes increase by 15% post inspections after IHS transformation, a smaller increase than baseline results. Yet, due to the nonlinearity of the IHS, confirming the parallel trend assumption for strikes becomes challenging, making comparisons with level change estimates difficult. Furthermore, to handle the overdispersion in strike count data, I used a zero-inflated negative binomial model fitting Equation (8). As shown in column (8), this yields identical results with the baseline.

Active Cities

A concern is that the rise in strikes might largely stem from cities with the most strikes. The CLB Strike Map shows that the seven cities with the highest strike intensity are all in Guangdong province, each experiencing over 4 strikes per million people annually.[47] To confirm that the surge in strikes was not primarily driven by these cities, I excluded Guangdong province from the sample and reran the analysis. The new results in Table C9, column (8), show an almost threefold strike increase after inspections, aligning with the initial results. Consistent outcomes were also found when excluding China's four major metropolitan cities: Beijing, Shanghai, Guangzhou, and Shenzhen, as shown in column (9).

[47] In 2015, Guangdong, often referred to as the "world's factory," experienced a significant surge in strikes, as reported by Zheping Huang in a 2015 *Quartz* article titled, "China Has Seen a 13-fold Increase in Labor Strikes and Protests Since 2011—and It's Cracking Down."

7 Mechanisms

This section explains the rise in labor strikes after anti-corruption inspections. It provides evidence supporting Proposition 3, positing that workers perceived a loosening of ties between firms and the government post-inspections, thereby increasing their expected return from striking. To reiterate, this study does not assess if the anti-corruption campaign genuinely diminished corruption due to the absence of firm-level data, even with other studies suggesting weakened firm-government ties (Chen and Kung, 2019; Chu et al., 2019; Ding et al., 2020; Hao et al., 2020; Xu et al., 2021). Instead, it employs a partial equilibrium approach, focusing on workers' perception of political shifts and its influence on strike decisions, bypassing the viewpoints of firms or the government.

Section 7.1 validates the main hypothesis of this study explaining the strike increase post anti-corruption inspections. Concurrently, Sections 7.2 and subsequent sections rule out alternative explanations for this phenomenon.

7.1 Expected Return to Strike

The Rise of Strikes

To determine if high-profile inspections boosted workers' expected strike returns, I can study strike changes in cities with varying pre-inspection corruption levels. The rationale is, in a city with high corruption pre-inspection, workers would expect lower strike returns, resulting in fewer strikes than potentially possible. Post-inspection, these suppressed strikes would surface, implying that more corrupt cities should see a greater strike increase than less corrupt ones. This hypothesis emphasizes that highly corrupt cities harbored more grievances, which the inspections unveiled, as discussed in Proposition 3.

I adopted the count of princeling land transactions from 2004 to 2012 in a city as a measure for pre-campaign corruption levels. This data, acquired from Chen and Kung (2019), defines princeling purchases as discounted land transactions from the government to princeling firms, or companies with board members linked to the Politburo, the CCP's executive committee.

Table 5 shows that cities with more princeling land transactions prior to the anti-corruption campaign experienced a higher strike increase post-inspection. Columns (1)-(4) reveal that cities with 9 or more transactions saw over a twofold strike increase, whereas cities with fewer than 9 transactions did not significantly change. This pattern not only persists but also exhibits a larger effect size when labor disputes replace strikes as the outcome, as shown in Table C12, columns (1)-(4). Figure B7 contrasts the dynamic strike change in cities with transactions above and below the median cutoff of 8, displaying a substantial strike increase in cities with over 8 transactions.

A potential issue is that other factors like a city's GDP per capita or population size might correlate with transaction numbers, and a higher strike increase could simply reflect a city's size. To control for this, I performed an alternate analysis where transactions were weighted by a city's pre-campaign population and GDP per capita. Consistent with earlier analysis, Table 5, columns

(5)-(8), show that cities with more than 10 predicted transactions witnessed the most substantial strike increase.

One might also worry that cities with more transactions could have been targeted earlier by the CCDI inspection team, leading to a major strike increase merely due to early inspection. Yet, as mentioned in Section 5.3, transaction levels do not correlate with inspection timing. To further substantiate this, I assessed the effects using only cities that underwent their first high-profile inspections within 0 or 1 quarter post-CCDI's provincial arrival. Table C11 confirms prior findings, indicating the strike increase was confined to cities with higher land transactions among those inspected earlier.

A further concern is that cities with more transactions might have also faced more inspections, potentially prompting workers to react to inspection scale. To counter this, I controlled for city-wise inspection numbers, yielding consistent results in Table C10. This suggests that the announcement of high-profile inspections in corrupt cities, rather than the number of inspections, influenced workers' decisions.

Table C12, columns (5)-(8), present results using an alternative corruption measure: the proportion of inspected officials with business-related titles. The findings align with prior results. Cities in the top 25^{th} percentile, i.e., cities with over 15% business-related inspections, saw a threefold strike increase post-inspection. Cities in the 50^{th}-75^{th} percentile experienced a twofold increase, while cities in the bottom 50^{th} percentile saw no significant change.

All the findings and robustness checks above reinforce Proposition 3, suggesting that inspections revealed pre-existing grievances of workers in highly corrupt cities. These revelations changed workers' expected strike returns, thereby inciting a surge in strike numbers in these cities.

To emphasize once more, I did not compare the absolute number of strikes between cities with more or fewer transactions, but the increase in strikes. As Table 5 shows, cities with more transactions did have a higher average quarterly number of strikes prior to inspections. Factors such as lower firm productivity and less social stability, associated with corruption, could contribute to a greater number of observed strikes in highly corrupt cities. My identification assumption is that these factors are unconnected to the inspection team's arrival timing. The core observation is the more pronounced strike increase post-inspections in cities with more transactions, corroborating Proposition 3 that anti-corruption inspections revealed latent grievances in corrupt cities.

The Decline of Strikes

Referring to Figure 5, one may question why strikes declined two years post a city's first high-profile inspection. Proposition 3 suggests an explanation: The increased strikes were reflective of pre-existing grievances unveiled by the inspections, not new worker-firm conflicts post-inspections. Once these grievances were addressed, strike levels subsided and stabilized, reflecting a new equilibrium in the post-inspection environment.

Given that the economy likely transitioned to a new equilibrium post-inspection, pinpointing the exact cause behind the strike decline is beyond this study's scope. Possible explanations include genuine reduction in corruption levels post-inspection, leading to similar strike levels in previ-

Table 5: Treatment Effects by Princeling Land Transactions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Number of Princeling Land Transactions				Predicted Princeling Land Transactions			
	0 to 6	7 to 8	9 to 10	11 to 16	<= 6	(6,8]	(8,10]	>10
First HP Insp.	-0.24	0.03	0.78**	0.92***	-0.04	-0.18	0.10	1.92***
	(0.20)	(0.22)	(0.31)	(0.31)	(0.24)	(0.14)	(0.19)	(0.46)
Average, Dep Var Before Insp.	0.30	0.31	0.50	0.71	0.09	0.25	0.30	1.07
SD, Dep Var Before Insp.	0.80	0.73	1.39	1.83	0.34	0.63	0.78	2.17
Observations	5,016	4,359	4,632	5,426	3,735	5,172	5,132	5,398
Year Span	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020
City FE	YES	YES	YES	YES	YES	YES	YES	YES
Quarter-Year FE	YES	YES	YES	YES	YES	YES	YES	YES
City Controls	YES	YES	YES	YES	YES	YES	YES	YES
Neighbor Insp.	YES	YES	YES	YES	YES	YES	YES	YES

Notes: The table presents the coefficients $\hat{\delta}$ from Equation (8) using the Callaway and Sant'Anna (2021) estimator. The first four columns show treatment effects from cities with different numbers of princeling land transactions before the anti-corruption campaign, while columns (5) to (8) show results from an alternative classification that weights princeling land transactions by population and GDP per capita. The data for princeling land transactions is from Chen and Kung (2019). The standard errors are presented in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

ously corrupt and non-corrupt cities. Alternatively, the government may have enhanced surveillance or censorship after observing the strike increase, thereby suppressing further strikes.

In Appendix F, I explored the potential factors influencing the duration from rising to declining strikes using a duration analysis. Suggestive evidence indicates that strikes decline sooner when the return to strike is lower and later when it is higher. Specifically, strikes decline earlier in cities with higher average wages, suggesting higher opportunity costs for workers in these areas. Conversely, strikes tend to decline later in cities with a larger proportion of migrant workers, implying a higher return to strike as outlined in Proposition 2. Yet, this is the furthest extent to which I can address the strike decline within this study's scope.

7.2 Attitudes

The inspections may have shifted workers' attitudes or opinions of their local governments, driving a surge in strikes. As workers learned of the corruption involving local bureaucrats, their trust in the government's ability to settle labor disputes diminished. This idea is backed by a significant decline in local government approval, as found by a survey from Wang and Dickson (2021). The increasing distrust may prompt workers to bypass the government, seek public and media support, negotiate directly with firms, or even express their grievances through strikes. This mechanism differs from the previous one in that it suggests that workers' perception of corruption increased instead of decreased post-inspections.

This mechanism, if correct, should lead to a rise not just in strikes, but also in other forms of political protests due to decreased public trust. Yet, tracking political protests in China is challenging due to intense government censorship. To address this, I employed the Global Database of Events, Language, and Tone (GDELT) which uses machine learning to track global media coverage of protests, wars, and other social disturbances.

Despite the scarcity of protest records, GDELT's comprehensive coverage of international and domestic media makes it the most reliable source of China's protest data currently. After rigorous data cleaning, which included removing duplicate observations and validating actual protest events, I was left with 587 instances of political protests. These occurred in China's prefecture-level cities from 2011 to 2020, encompassing issues such as free speech, institutional reform, ethnic minority rights, environmental advocacy, and protests against specific policies.

Table C13, column (1), suggests that the count of political protests did not significantly rise following inspections. This evidence counters the hypothesis that inspections incited a surge in non-labor related political protests. Thus, it is implausible to conclude that decreased trust in the government induced an increase in unrest like strikes and protests. Moreover, the increase in labor-related court disputes, shown earlier in Table C9, reduces the likelihood that workers' trust in the judicial system declined post-inspections.

7.3 Media Exposure

Another hypothesis suggests local media started reporting more strikes following citywide anti-corruption inspections, rather than an actual rise in strikes. However, this appears implausible, since most strike reports originated from *Weibo*, a nationwide social media platform unlikely to adjust its censorship due to local political shifts. It is also perplexing why solely labor strikes experienced heightened media attention post-inspections, while other forms of protests did not. Additionally, Table C9 demonstrates an increase in court-reported labor disputes—many unpublicized by the media—following inspections. All these inconsistencies suggest that increased media exposure is likely not the primary driver of the surge in labor strikes.

7.4 Firms

An alternate hypothesis suggests that anti-corruption inspections indirectly increased strikes by influencing firm behaviors. For example, politically connected firms could experience profit losses post-inspections, leading to delayed wage payments and consequently, worker strikes. Although this hypothesis is difficult to be evaluated given the absence of firm-level data, it seems implausible as indicated in Figure 5, which demonstrates an immediate increase in strikes within a month or two post-inspections rather than a delayed rise as would be expected.

Moreover, qualitative evidence reveals that China's construction industry operates under a complex subcontracting system, involving numerous firms ranging from property developers and specialized constructors to labor subcontractors (Ngai and Huilin, 2010). As such, it is unclear how this subcontracting system might be impacted by anti-corruption inspections or how long it would take before workers are compelled to strike. Often, workers endure quarters or even years waiting for subcontractors to pay them (Wei and Chan, 2022). Such accounts further undermine the notion that inspections triggered a chain reaction within firms, leading to an immediate surge in strikes.

While I cannot fully rule out that politically connected firms faced profit losses post-inspections, leading workers to strike over potential unpaid wages, I can examine the patterns in CJO labor dispute cases using the keyword "firm debts" to determine if this narrative is plausible. Figure B8 panel (a) shows a slight rise in such cases post-inspection, yet it accounts for less than a fifth of the overall surge in labor disputes, suggesting other factors contribute to the post-inspection rise in labor disputes. Still, some evidence suggests that politically connected firms faced financial challenges post-inspection, given that the rise in these cases predominantly originates from cities with above-median princeling land transactions, as indicated in panels (c) and (e).

However, there is no strong evidence linking most wage arrears-related labor disputes to financial challenges of politically connected firms. Figure B8 panel (b) shows no increase in cases mentioning both "firm debts" and "wage arrears". Similarly, panels (d) and (f) indicate no such increase from either formerly more or less corrupt cities. By the same reasoning, this offers no evidence that post-inspection increase in strikes was due to firms' financial issues.

To ascertain whether firms mistreated workers post-inspections, I examined the changes in workplace accidents[48] before and after the inspections. Table C13, columns (2) to (6), indicates no significant change in workplace accidents or workplace fatalities in either construction or man-

[48]The workplace accident data used in this analysis comes from the China Stock Market & Accounting Research Database (CSMAR), which compiles data from China's State Administration of Work Safety (SAWS). The sample includes daily observations of workplace accidents in the construction and manufacturing industries from 2011 to 2016.

ufacturing industry during the inspection period. Hence, the evidence does not substantiate the claim of firms altering their treatment of workers in response to CCDI inspections.

8 Welfare

This section, drawing upon baseline results, explains the impact and extent of corruption on workers' welfare. My study, thus, contributes to the ongoing debate over the economic consequences of corruption. This discourse has two main perspectives: on one side, evidence suggests that corruption stifles growth.[49] On the flip side, some argue that corruption can create a suboptimal environment that facilitates efficient firms through rent-seeking, thus increasing economic growth.[50] Engaging with this debate, I argue that regardless of its impact on business, corruption can undermine workers' welfare, thereby exerting a negative influence on economic growth.

To assess the welfare impact of inspections on workers, I estimated the forgone wages resulting from the increase in strikes. A conservative back-of-the-envelope calculation places this figure at approximately 88 million yuan or $\$14$ million. This estimation leverages the 2013 average hourly wage of migrant workers from the China Labour Statistical Yearbook, which was 20 yuan. It also assumes, in the absence of detailed data, that a typical strike involves about 50 workers and lasts for 3 days. In other words, each worker is willing to forgo approximately 600 yuan ($85) to participate in a strike.

The back-of-the-envelope calculation is based on a conservative assumption, as I might have underestimated workers' grievances. Some studies suggest strikes often involve hundreds of participants and can last for more than 10 days,[51] hinting at grievances worth more than 600 yuan. Furthermore, since the CLB Strike Map captures only 5% to 10% of the strikes in China, the actual wage loss is likely substantially higher, potentially reaching 1.2 billion yuan ($170 million), equivalent to 1% of the median GDP of Chinese prefecture-level cities in 2013.

The $\$14$ to $\$170$ million in forgone wages represents the lower bound of worker welfare loss due to corruption, given that workers expect higher returns from striking when corruption inspections are present, as outlined in Proposition 3. In other words, corruption inspections prompt workers to strike, a decision they would not have made absent these inspections. Drawing from the framework in Section 3, the rank of a worker's expected returns to strike pre and post anti-corruption inspections can be depicted as follows:

$$\begin{gathered} \textit{strike if } \nexists \textit{ corruption inspections} < \textit{do not strike} < \textit{strike if } \exists \textit{ corruption inspections} \\ (1-\bar{\theta})\rho_{j,c}\bar{w}_{jc} - \epsilon_{ijc} < w_j d < \rho_{j,c}\bar{w}_{jc} - \epsilon_{ijc}. \end{gathered} \tag{9}$$

For simplicity, I assume the worker perceives $\bar{\theta} = 0$ post-inspections. Nonetheless, similar conclusions can be drawn even with a positive and declining $\bar{\theta}$ post-inspections. The inequality from (9)

[49] There is a substantial body of literature on corruption and growth. Although not exhaustive, noteworthy contributions include those by Murphy et al. (1993), Shleifer and Vishny (1993, 1994, 1998), Mauro (1995), Johnson et al. (1997), Banerjee (1997), and Svensson (2005), Sequeira and Djankov (2014), Smith (2016), Bobonis et al. (2016), Avis et al. (2018), Chen and Kung (2019), Bai et al. (2019), Colonnelli and Prem (2021), and Fenizia and Saggio (2021).

[50] These studies include Lui (1985), Méon and Weill (2010), and Dreher and Gassebner (2013).

[51] Weidong Xiao, "Investigation Report on the Cost of Rights Protection for Chinese Migrant Workers." *Beijing Teenagers Law Aid And Research Center.* May 23, 2005.

can be re-written as

$$\frac{w_j d + \epsilon_{ijc}}{\rho} < \bar{w}_{jc} < \theta \bar{w}_{jc} + \frac{w_j d + \epsilon_{ijc}}{\rho}. \tag{10}$$

Summing across all workers, industries, and cities, the range for the lost wages can be calculated as follows:

$$\sum_i \sum_j \sum_c w_j d < \sum_i \sum_j \sum_c \frac{w_j d}{\rho} < \bar{W} < \theta \bar{W} + \sum_i \sum_j \sum_c \frac{w_j d}{\rho} \tag{11}$$

where $\bar{W} = \sum_i \sum_j \sum_c \bar{w}_{jc}$ symbolizes the collective grievances among all workers in the economy under conditions wherein workers do not strike amid perceived corruption, but strike when perceived corruption lessens. Essentially, $\bar{W}$ signifies the loss in worker welfare if no inspections occur.

The inequality in (11) illustrates that the probability of a successful strike, ρ, influences the range of welfare loss - a higher success rate corresponds to lower welfare loss and vice versa. When the success rate approaches to 1, the summed opportunity cost $\sum_i \sum_j \sum_c w_j d$ across all workers and industries becomes the lower bound for welfare loss due to corruption.

9 Discussion

In 2022, Mr. Yao, Mr. Yang, and 15 others worked on a wastewater project in Zhoushan for a subcontractor under China State Construction Engineering but did not get paid for July to September. They lodged a wage complaint in January 2023, but the local labor bureau withdrew the case following a dismissal request from the accused company by March. In April, frustrated Mr. Yao and Mr. Yang complained to the Dinghai District Disciplinary Committee about their unpaid wages. The Committee confronted the officials who admitted to not confirming wage receipt or case withdrawal consent from workers. By June, the overdue wages of over $200,000$ yuan were finally paid, a year late.[52]

Rewinding to September 2014, Shao Jiaxi and 66 workers traveled from Sichuan to Taiyuan, working till November, but only received $2,000$ yuan each, with over 1.7 million yuan still unpaid. From January 2015, they stayed at the chilly worksite to claim wages, finding warmth in fire from waste wood. Despite multiple visits to various government offices, Shao's efforts were futile.[53]

This paper shows that after Xi's anti-corruption crackdown, workers' grievances like those of Shao Jiaxi, Mr. Yao, and Mr. Yang surfaced. It first reports a surge in corruption inspections and related *Weibo* posts following the first city inspection, given the arrival of a CCDI inspection team in the province. Then, worker strikes spiked after a city's first high-profile inspection, doubling within a year, and tripling within two.

Most strike increases were from construction and manufacturing private enterprises, mainly over wage arrears. This suggests a high level of grievances among workers in construction and manufacturing sectors. Strikes also had network effects, spiking in cities experiencing later inspections

[52] Dinghai District Disciplinary Committee, "Dinghai: Priority Supervision Ensures No More Wage Worries for Migrant Workers." *Zhejiang Provincial Discipline Inspection and Supervisory Commission*. July 17, 2023.

[53] Lei Feng, Liangquan Sun, Yanan Li, Renbin Sun, and Bingkun Wang, "Xinhua Reporter Experiences a Chilly Day with Wage-Claiming Migrant Workers: 'Every Day Feels Like a Year.'" *Xinhua News*. January 18, 2015.

after inspections in early-wave or neighboring cities. Cities with stronger networks among migrant workers saw a larger strike increase.

The surge in strikes stemmed from workers' heightened expected returns after witnessing corruption inspections, revealing previously suppressed grievances in a corrupt environment. Alternative explanations such as changes in attitudes, media exposure, or firm behaviors were unlikely to cause the strike increase. Without these inspections, an estimated welfare loss of up to 1.2 billion yuan ($170 million) would have been incurred in the context of a corrupt environment.

Some questions remain unanswered. Notably, what kind of new equilibrium emerged in cities long term post anti-corruption inspections? For instance, Mr. Yang and Mr. Yao successfully addressed their wage complaints to the local disciplinary committee, unlike Shao Jiaxi eight years ago - were they under a different political environment?

One scenario is that the inspections effectively curbed corruption, leading to fewer grievances and reduced strikes in the years following, as evidenced by Mr. Yang and Mr. Yao's case where the disciplinary committee promptly addressed their complaints. Alternatively, local governments may have increased strike crackdowns, leveraging advanced surveillance technologies.[54] It could also be a mix of both, with new officials aiming for less corruption while maintaining social stability. Answering these questions is beyond the paper's scope, requiring comprehensive data on bureaucracy and firm-level information.

This paper posits that workers strike based on the political environment, with the inspection-induced strike surge indicating perceived shifts in firm-government ties. Nevertheless, China's lack of firm-level data still poses a challenge. Future research should aim to improve access to data on firms and government, facilitating deeper exploration into the effects of the anti-corruption campaign on these relationships, allowing a comprehensive examination of policy shifts that encompasses governments, firms, and workers.

[54] According to a study by Beraja et al. (2021), the Chinese government's procurement and application of AI technologies have been effective in quelling dissidents and social unrest.

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Appendix

A Proof of Proposition 1

This section proves that $p^*_{j,c}$, the optimal participation rate, increases with owed wages $\bar{w}_{jc}$, given a non-zero probability of strike success.

First, I know that

$$p^*_{j,c} = \frac{\sigma_j + \alpha p_{j,-c}\lambda_{j,-c}\bar{w}_{jc} - w_j d}{2\sigma_j - \lambda_{j,c}\bar{w}_{jc}}. \tag{12}$$

Also, the probability of strike success lies between 0 and 1, or $\rho_{j,c} \in (0,1)$, meaning that

$$\alpha \in \left(-\frac{p_{j,c}\lambda_{j,c}}{p_{j,-c}\lambda_{j,-c}}, \frac{1 - p_{j,c}\lambda_{j,c}}{p_{j,-c}\lambda_{j,-c}}\right). \tag{13}$$

If $\alpha \geq 0$, then it is obvious to tell that $\frac{\partial p^*_{j,c}}{\partial \bar{w}_{jc}} > 0$.

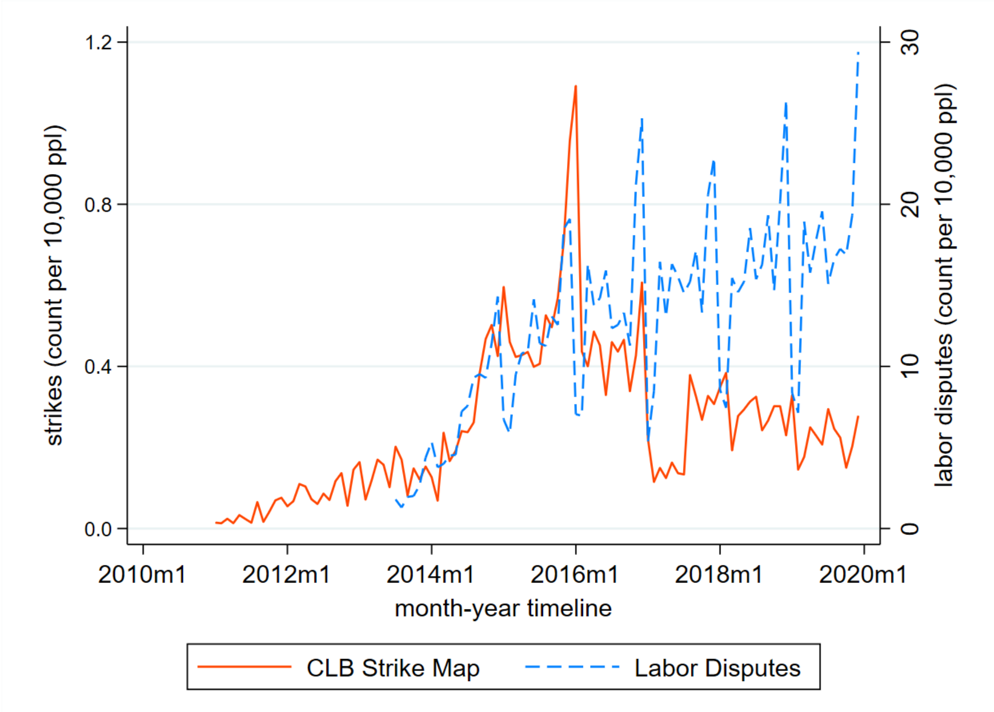
If $\alpha < 0$, I let $\alpha = -\frac{p^*_{j,c}\lambda_{j,c}}{p_{j,-c}\lambda_{j,-c}} + \delta$ for some $\delta > 0$. Substitute this in Equation (3), I have

$$\begin{aligned} p^*_{j,c} &= \frac{1}{2} + \frac{(p^*_{j,c}\lambda_{j,c} - p^*_{j,c}\lambda_{j,c} + \delta p_{j,-c}\lambda_{j,-c})\bar{w}_{jc} - w_j d}{2\sigma_j} \\ &= \frac{1}{2} + \frac{\delta p_{j,-c}\lambda_{j,-c}\bar{w}_{jc} - w_j d}{2\sigma_j}. \end{aligned} \tag{14}$$

Therefore, Equation (14) shows that $\frac{\partial p^*_{j,c}}{\partial \bar{w}_{jc}} > 0$.

B Additional Figures

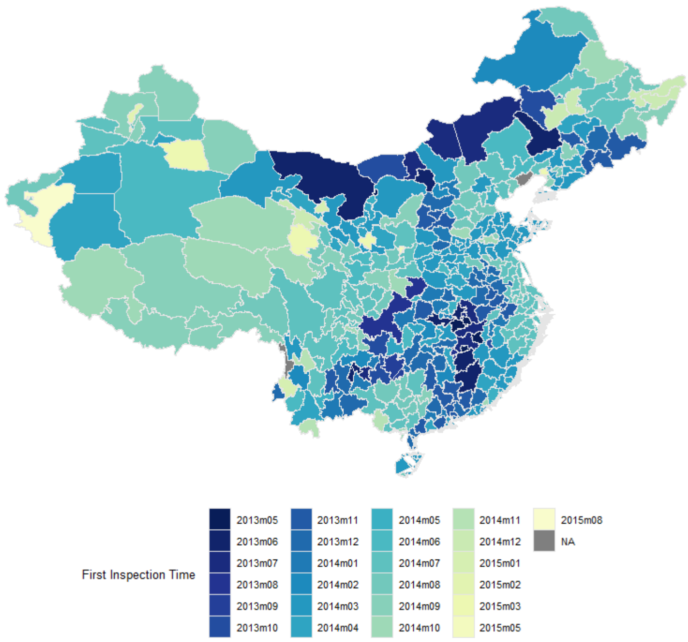
Figure B1: Comparing CLB Strike Map and Labor Disputes from CJO



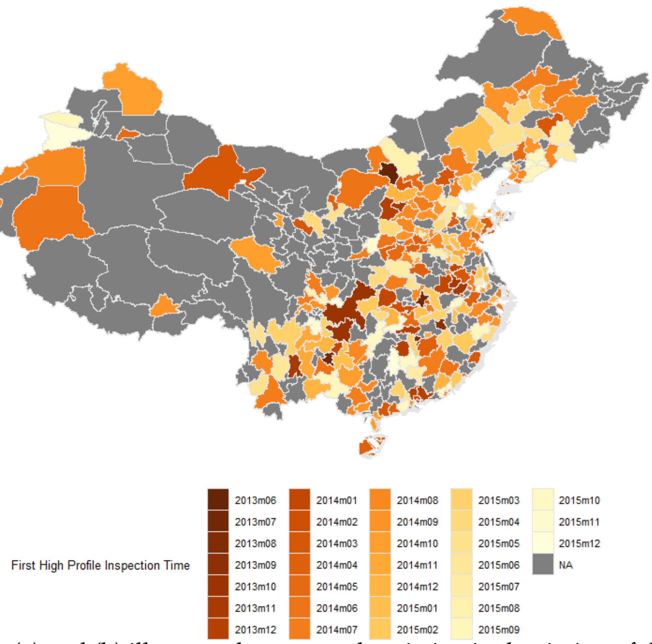
Notes: The graph compares the labor strikes recorded by the China Labor Bulletin (CLB) and the labor disputes recorded by China Judgements Online (CJO) on a national level and on a monthly basis. According to the graph, the patterns of strikes and labor disputes are similar from 2013 to 2017. After 2017, there is a decrease in the number of strikes while the number of labor disputes continues to rise steadily. As a result, the data on labor disputes from CJO can serve as a robustness check for the main findings, given that the main estimates cover the period from 2013 to 2017. The discrepancy in the trends between the CLB Strike Map and labor disputes recorded by CJO should not be a great concern, as it has risen since 2017, two years after the conclusion of the first wave of CCDI inspections. A probable explanation is that workers' activism reached a new equilibrium in the long run under a different political environment following the inspections, while the number of labor disputes brought to court followed a separate equilibrium. For instance, the use of surveillance technology to monitor strikes may have increased in the long run after the inspections. Although labor strikes and labor disputes exhibit different trends after 2017, the dynamic pattern of the main outcome, using the primary specification from Equation (7), closely follows the same pattern.

Figure B2: Timing of First Corruption Inspections & First High Profile Inspections

(a) First Inspections



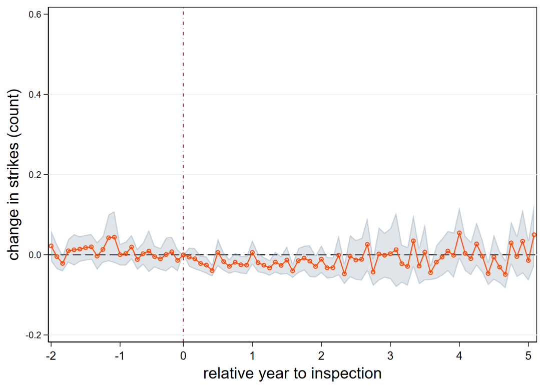
(b) First High Profile Inspections



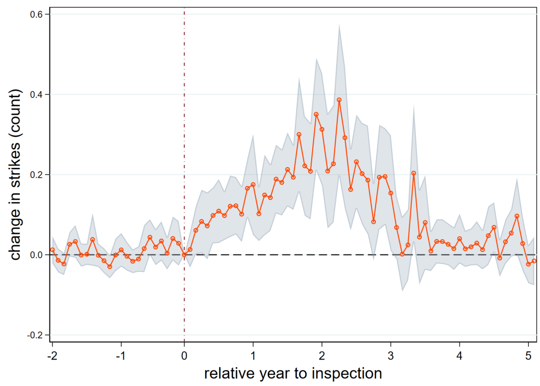
Notes: The maps in (a) and (b) illustrate the temporal variation in the timing of the first inspection and the first high-profile inspection across prefectural cities in China, conditional upon the provincial arrival of the CCDI inspection team. Information on corruption cases is obtained from the Corruption Investigation Dataset compiled by Wang and Dickson (2021). This dataset provides a comprehensive record of corruption inspections from 2012 to February 2017.

Figure B3: Strikes by Consequence

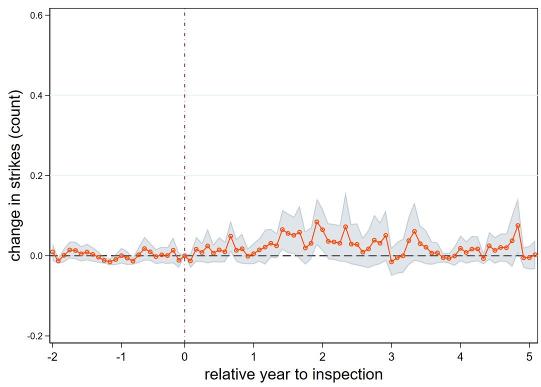
(a) Peaceful Negotiations



(b) Police Involvement



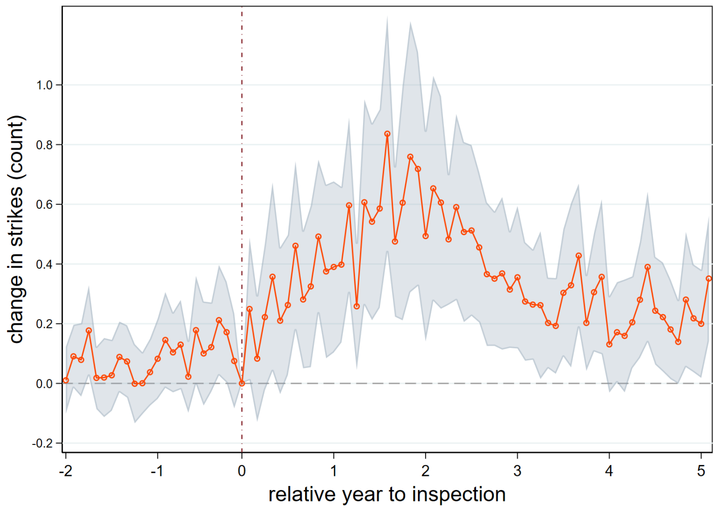
(c) Suppression or Violence



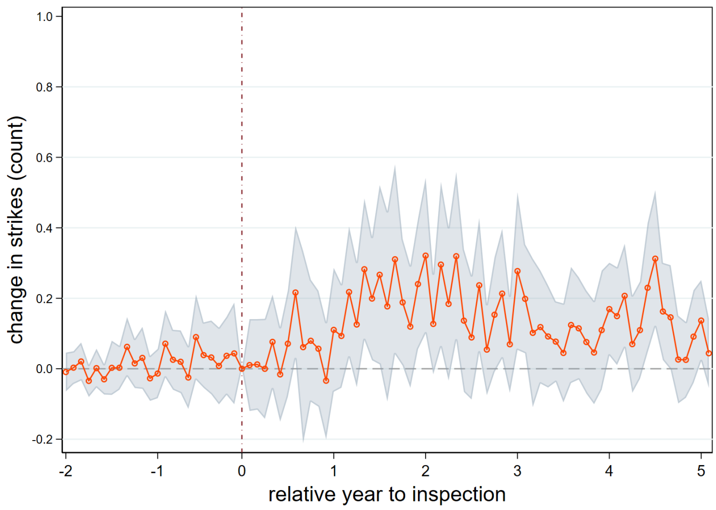
Notes: The series of graphs demonstrate the estimated λ_k from Equation (7) based on the outcome of the strikes. The hollow red dots represent the estimates and the light blue region signifies the standard errors. The dotted vertical line indicates the timing (one month prior) of the first high-profile corruption inspection case in a city, given the arrival of the CCDI inspection team in the same province. The sample includes $43,440$ observations from 362 prefectures from 2011 to 2020.

Figure B4: Strikes by Migrant Workers Connections

(a) Higher Migrant Workers Connections



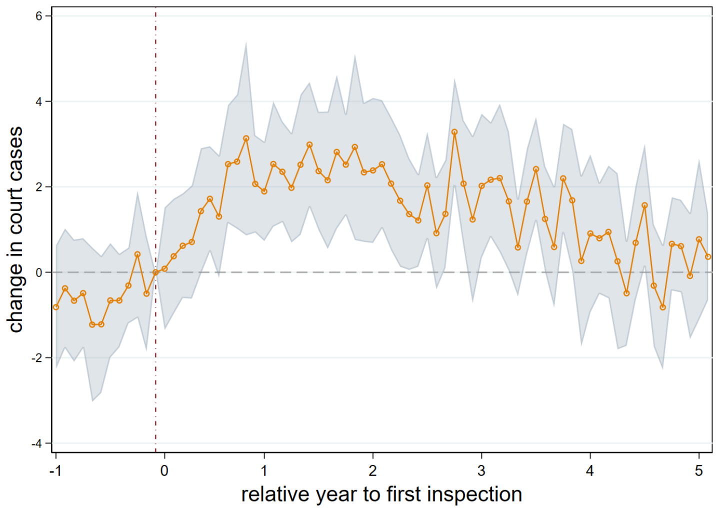
(b) Lower Migrant Workers Connections



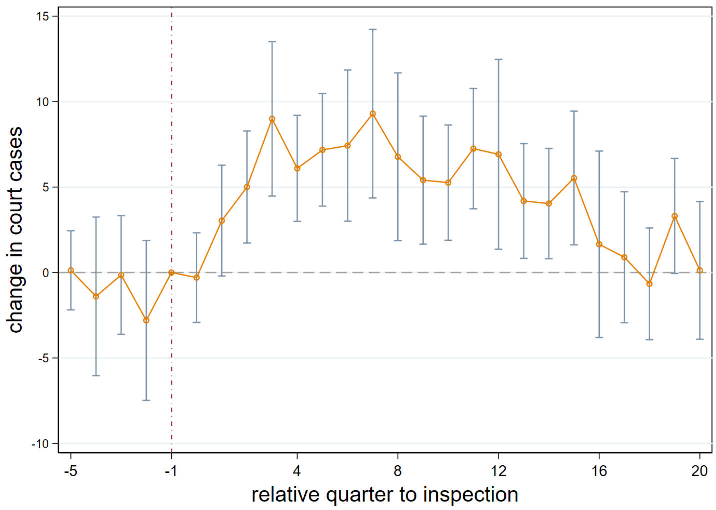
Notes: The two graphs present the estimated λ_k from Equation (7). Panel (a) displays the cities where more than 50% of the migrant workers have primary connections with *Laoxiang*, or fellow migrants from their city of origin, who now work in the same current city in 2011, while panel (b) shows the cities otherwise. The purpose of the figure is to demonstrate the heterogeneous effects of strikes across cities with varying levels of migrant worker networks before the 2012 anti-corruption campaign. The hollow red dots represent the estimates, and the light blue region indicates the standard errors. The dotted vertical line signifies the timing of the first high-profile corruption inspection case in a city, given the arrival of the CCDI inspection team in the province. The sample consists of 43,440 observations from 362 prefectures from 2011 to 2020, and information on the migrant workers was obtained from the 2011 China Migrants Dynamic Survey (CMDS).

Figure B5: Baseline Outcomes Using Labor Disputes from CJO

(a) Monthly Level



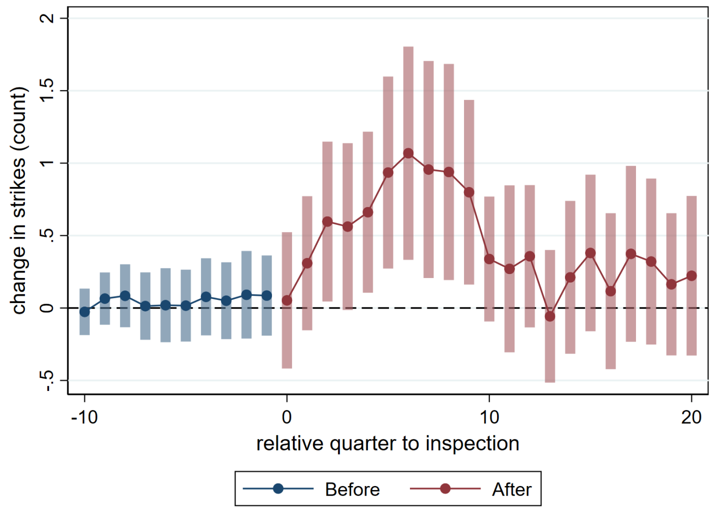
(b) Quarterly Level



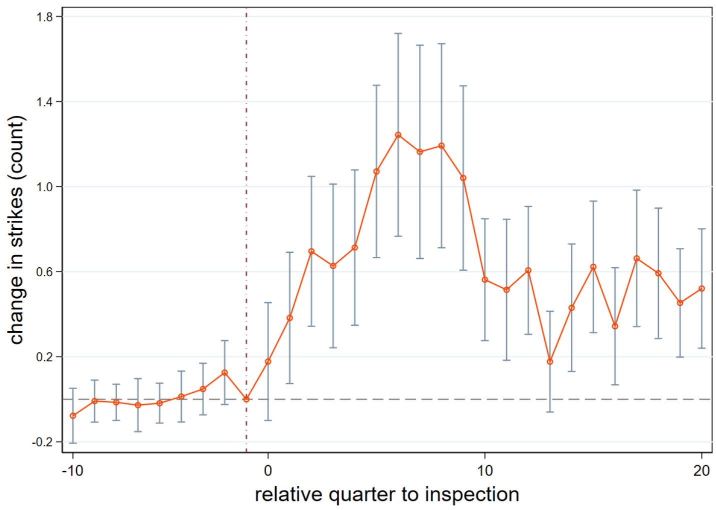
Notes: This graph depicts the changes in court labor disputes before and after the first high-profile inspection in a city, given the arrival of the CCDI inspection team within the province. Specifically, the points are the estimated λ_k from Equation (7). Panel (a) presents λ_k at the monthly level, while panel (b) presents λ_k at the quarterly level. The vertical dashed line represents one month (quarter) prior to the first high-profile corruption inspection case in a city, given the arrival of the CCDI inspection team in the same province. The monthly sample includes $43,440$ observations from 362 prefectures from 2011 to 2020. The quarterly sample includes $14,480$ observations from 362 prefectures from 2011 to 2020.

Figure B6: Comparing Baseline Results b/t Callaway and Sant'Anna (2021) & Gardner (2021)

(a) Callaway and Sant'Anna (2021)



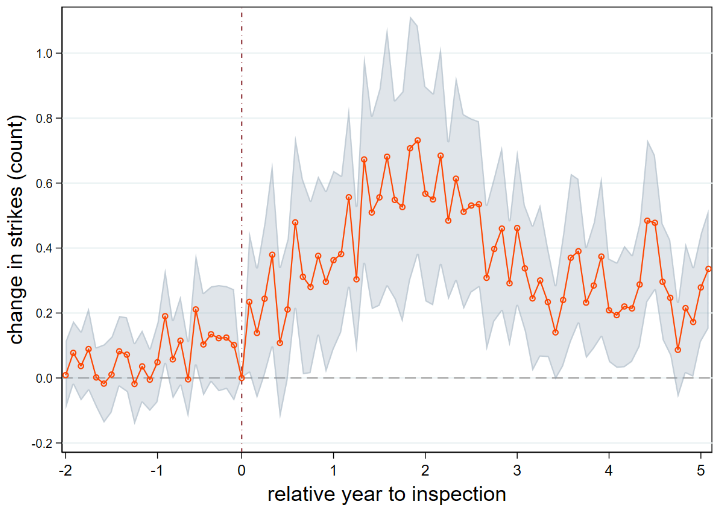
(b) Gardner (2021)



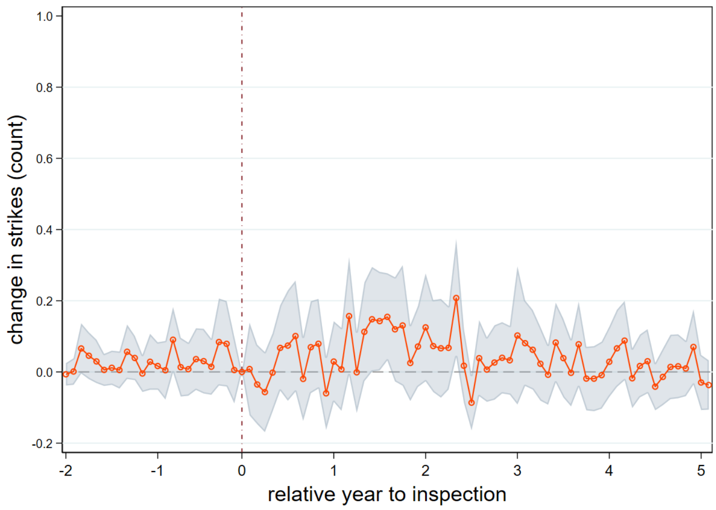
Notes: The graphs demonstrate the estimated λ_k from Equation (7) based on the specifications from Callaway and Sant'Anna (2021) and Gardner (2021). I changed the temporal observational level from months to quarters of the year to expedite the estimation process. The vertical dashed line represents one quarter prior to the first high-profile corruption inspection case in a city, given the arrival of the CCDI inspection team in the same province. The sample includes $14,480$ observations from 362 prefectures from 2011 to 2020.

Figure B7: Strikes in Cities with Different Princeling Land Transactions

(a) Princeling Purchases ≥ 8



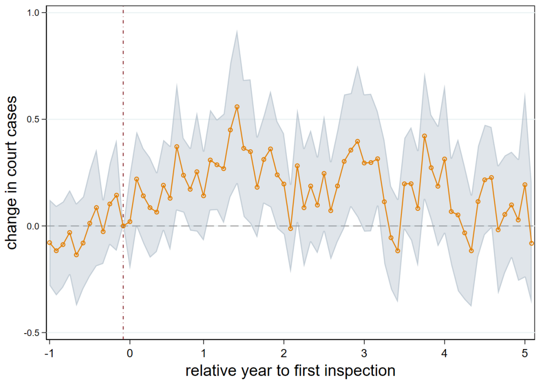
(b) Princeling Purchases < 8



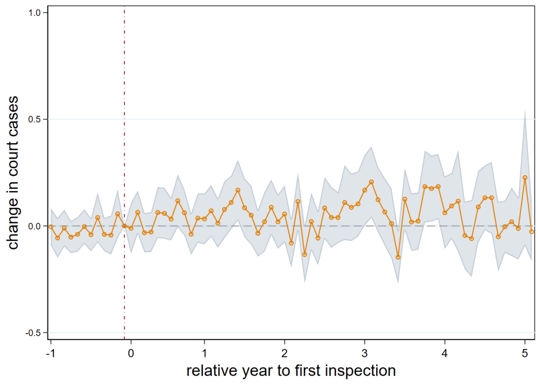
Notes: The two graphs demonstrate the estimated λ_k from Equation (7). Graph (a) displays cities where there were more than 8 princeling land transactions prior to Xi Jinping announcing the anti-corruption campaign in 2012, while graph (b) shows the cities where there were less than 8 princeling land transactions. The hollow red dots represent the estimates and the light blue region signifies the standard errors. The dotted vertical line indicates the timing (one month prior) of the first high-profile corruption inspection case in a city, given the arrival of the CCDI inspection team in the same province. The sample includes 43,440 observations from 362 prefectures from 2011 to 2020. Information on the prefecture-level princeling land transactions is sourced from Chen and Kung (2019).

Figure B8: CJO Cases, Firm Debts and Wage Arrears

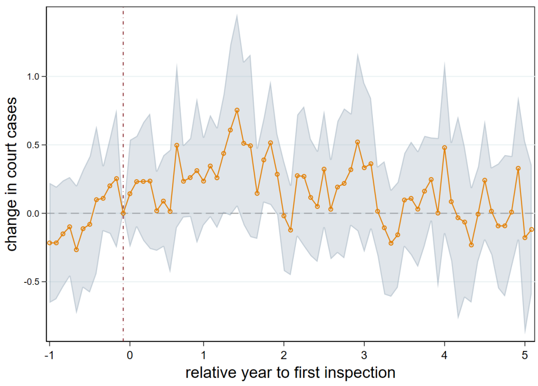
(a) Cases with "Firm Debts"



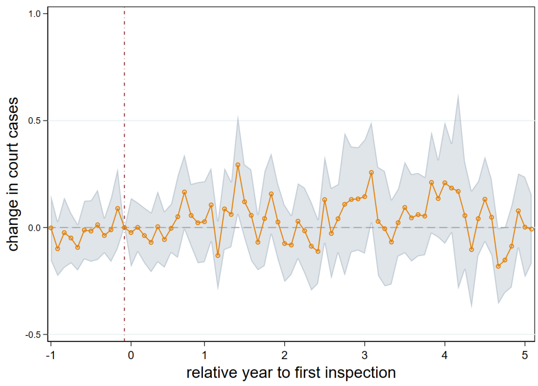
(b) Cases with "Firm Debts" & "Wage Arrears"



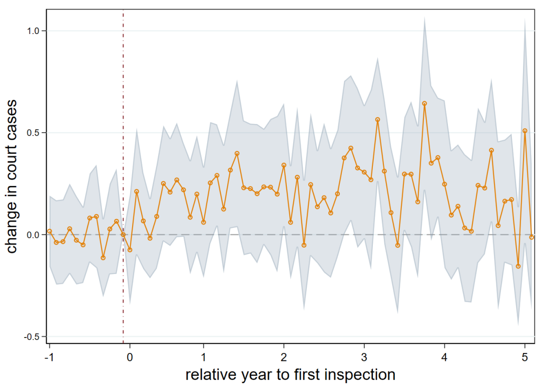
(c) "Firm Debts", High PLT



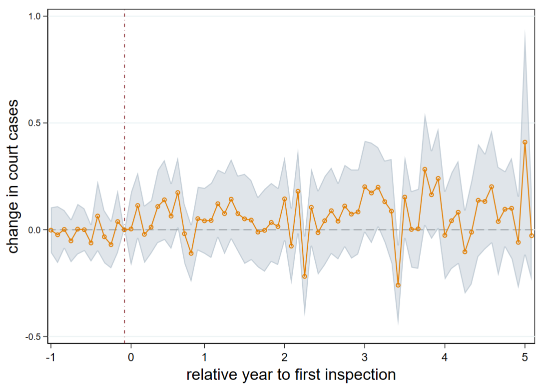
(d) "Firm Debts" & "Wage Arrears", High PLT



(e) "Firm Debts", Low PLT



(f) "Firm Debts" & "Wage Arrears", Low PLT



Notes: This graph depicts the changes in court labor disputes before and after the first high-profile inspection in a city, given the arrival of the CCDI inspection team within the province. Specifically, the points are the estimated λ_k from Equation (7). Panel (a) presents the estimates for cases where the keyword "firm debts" is mentioned in the case document, while panel (b) presents the estimates for cases where both the keywords "firm debts" and "wage arrears" are mentioned in the document. Panels (c) to (f) respectively show cases with "firm debts" and with both "firm debts" and "wage arrears" in cities with princeling land transactions per capita (PLT) above and below the median prior to the campaign. The vertical dashed line represents one month prior to the first high-profile corruption inspection case in a city, given the arrival of the CCDI inspection team in the same province. The sample includes $43,440$ observations from 362 prefectures from 2011 to 2020.

C Additional Tables

Table C1: Chinese Communist Party (CCP) Rank Descriptions

Rank	Level	Positions
1	National Leader	General Secretary of the Communist Party of China (CCP) Central Committee, Chairman of the CCP Central Military Commission, Chairman of the National Committee of the Chinese People's Political Consultative Conference (CPPCC)
2	Sub-National Leader	Members of the Political Bureau of the CCP Central Committee, Secretary of the CCP Central Commission for Discipline Inspection (CCDI)
3	Provincial-Ministerial Level	Secretary of Party Committees of Provinces, Autonomous Regions and Municipalities, etc.
4	Sub-Provincial (Ministerial) Level	Deputy Secretary of Party Committees of Provinces, Autonomous Regions and Municipalities, Standing Committee Members of Provincial People's Congress
5	Bureau-Director Level	Party Secretary of Prefecture-Level Cities and Divisions
6	Deputy-Bureau-Director Level	Deputy Party Secretary of Prefecture-Level Cities and Divisions, Standing Committee Members of Party Committees of Prefecture-Level Cities
7	Division-Head Level	Party Secretary of Counties or County-Level Cities
8	Deputy-Division-Head Level	Deputy Party Secretary of Counties or County-Level Cities
9	Section-Head Level	Party Secretary of Towns or Townships
10	Deputy-Section-Head Level	Deputy Party Secretary or Standing Committee Member of Towns or Townships

Notes: The table presents the classification of the bureaucratic ranks within the Chinese Communist Party (CCP) as specified by the Civil Servant Law of the People's Republic of China, which was established in 2005. The rank is a 1 to 10 scale, with 1 being equivalent to the national level and 10 being equivalent to the deputy office level. Note that the "positions" column only provides examples of the positions within a specific rank, and therefore, the positions listed are not exhaustive.

Table C2: Summary Statistics on Population-Weighted Strikes, 2010-2020

	Prefecture-Month Observations	
Unit = Count of Strikes per 10M ppl	Mean	SD
Overall	0.89	2.71
Industry		
Construction	0.30	1.25
Education	0.03	0.34
Manufacturing	0.27	1.68
Mining	0.03	0.45
Service	0.11	0.74
Transportation	0.13	0.86
Others	0.02	0.31
Firm Type		
Private	0.50	1.76
SOEs	0.10	0.69
Strike Form		
Protest	0.47	1.64
Sit-In	0.25	1.28
Scale (number of participants)		
Small (<100)	0.70	2.15
Medium (101 - 1,000)	0.17	1.08
Large (1,001 - 10,000)	0.03	0.42
Reason to Strike		
Wage Arrear	0.62	2.11
Compensation	0.06	0.61
Pay Increase	0.07	0.59
Observations	31,164	
Number of Prefectures	297	
Year Span	2011-2020	

Notes: This table summarizes the mean and standard deviation of the number of strikes every 10 million people that occurred on a prefecture-month basis from 2011 to 2020. Note that observations regarding the type and scale of industry firms are mutually exclusive, but observations regarding the form and reason for a strike are not. For instance, a strike can take both the form of a protest and a sit-in, or it can be for both wage arrears and compensation. The strikes were recorded by the China Labour Bulletin (CLB) Strike Map.

Table C3: Correlations between Inspection Timings and City Characteristics before 2012

	First Inspection		First High Profile Inspection	
	Coefficient	Standard Error	Coefficient	Standard Error
Number of Strikes	-0.29	0.18	-0.02	0.25
Number of Princeling Land Transactions	-0.12	0.07	-0.17	0.11
GDP, Current Year Price (10,000 Yuan)	-0.38	0.71	0.09	0.96
RNI (‰)	-0.20	0.07	-0.23	0.12
Population, End of Year (10,000 People)	0.02	0.06	0.00	0.10
Employment in Finance (Count)	1.03	0.65	-0.67	0.98
Employment in Real Estate (Count)	0.21	0.65	-0.27	1.02
Highway Traffic Flow (10,000 people)	0.08	0.24	0.14	0.38
Particulate Matter (PM)	0.18	0.14	0.06	0.20
Employment in Mining (Count)	-0.42	0.37	-0.12	0.62
Employment in Manufacturing (Count)	-1.94	2.51	-0.67	4.31
Employment in Private Sector (Count)	0.21	0.24	0.04	0.34
Employment in Local Firms (10,000 People)	9.43	7.62	1.71	13.28
Population Ratio, Primary Sector (Percentage)	75.76	44.88	15.22	56.80
Population Ratio, Secondary Sector (Percentage)	161.43	95.64	32.88	121.03
Unemployment (Count)	-0.16	0.26	-0.02	0.35
Population Ratio, Tertiary Sector (Percentage)	152.18	89.92	30.86	113.81
Employment in Sales (Count)	-1.60	0.88	-0.52	1.37
GDP Growth (Percentage)	-0.05	0.07	0.04	0.12
Employment in Transportation (Count)	-0.14	0.83	-0.19	1.32
Employment in Energy (Count)	-0.10	0.20	-0.06	0.31
Employment in Construction (Count)	-0.93	1.16	-0.55	1.98
Employment in Rental and Business Service (Count)	0.60	0.77	-0.10	1.10
Employment in Research (Count)	0.60	0.65	-0.93	0.99
Employment in Public Infrastructure (Count)	-0.26	0.26	0.01	0.40
Employment in Accomodation and Catering (Count)	-0.55	0.75	-0.34	1.07
Employment in Information and Technology (Count)	-1.05	0.90	1.39	1.25
Employment in Community Service (Count)	0.01	0.83	0.79	1.11
Employment in Education (Count)	-0.31	0.73	-0.05	1.17
Employment in Health (Count)	-1.05	0.55	0.89	0.77
Employment in Entertainment (Count)	-0.96	0.65	-0.70	0.89
Employment in Public Service (Count)	-0.60	0.51	-0.31	0.85
Land for Administrative Purposes (km^2)	-0.08	0.08	-0.13	0.12
GDP Ratio, Secondary Sector (Percentage)	131.15	180.22	14.13	279.26
GDP Ratio, Tertiary Sector (Percentage)	113.46	156.08	12.17	241.86
GDP Ratio, Primary Sector (Percentage)	102.13	140.10	10.86	217.10
Large-Scale Firms (Count)	-217.48	1413.79	1628.20	2252.62
Large-Scale Private Domestic Firms (Count)	152.68	994.59	-1145.18	1584.69
Large-Scale Firms, Hong Kong, Macau, or Taiwan (Count)	38.27	251.25	-289.12	400.32
Large-Scale Firms, Foreign (Count)	51.19	330.68	-380.78	526.87
Value-Added Tax, Large-Scale firms (10,000 Yuan)	0.54	0.28	-0.24	0.39
Real Estates Investments (10,000 Yuan)	-0.06	1.02	0.53	1.48
Real Estates Investments, Residential (10,000 Yuan)	0.46	0.94	-0.52	1.40
Commodity Sales (10,000 Yuan)	-0.24	0.67	-0.09	0.92
Large-Scale Retail Firms (Count)	0.34	0.45	0.21	0.61
Foreign Investments (10,000 USD)	-0.36	0.26	0.14	0.37
Local Government Revenue (10,000 Yuan)	1.54	1.31	-1.44	1.94
Local Government Spending (10,000 Yuan)	-2.99	1.35	1.52	1.84
Education Spending (10,000 Yuan)	-0.06	0.66	-1.00	0.88
Research Spending (10,000 Yuan)	1.26	0.75	-0.25	1.10
Borrowing, Financial (10,000 Yuan)	0.40	1.07	2.16	1.53
Savings, Financial (10,000 Yuan)	-1.21	0.61	-1.46	0.83
Average Employment (10,000 People)	-1.74	1.84	-1.95	2.46
Total Wages (10,000 Yuan)	-0.94	2.39	4.64	3.25
Average Wage (Yuan)	0.06	0.21	-0.27	0.29
Retirement Insurance Enrollment (Count)	-0.50	0.53	-0.43	0.80
Medical Insurance Enrollment (Count)	-0.27	0.24	-0.37	0.37
Unemployment Insurance Enrollment (Count)	1.21	0.58	-0.22	0.80
Telecom Revenue (10,000 Yuan)	-0.67	0.35	-0.22	0.46
Mobile Phone Users (10,000 People)	0.92	0.44	0.09	0.58
Internet Users (10,000 Households)	0.28	0.22	0.11	0.29
Observations	228		160	

Notes: The coefficients and standard errors are obtained from regression analyses using the first inspections and first high-profile inspections as the outcome variables and prefecture-level characteristics as the explanatory variables. All variables are standardized to have a mean of 0 and a standard deviation of 1. The prefecture characteristics are sourced from the 2011 China City Statistical Yearbook (CCSY). The timing of inspections and the number of strikes are obtained from the Corruption Investigation Dataset (CID) and the China Labour Bulletin (CLB) strike map, respectively. The princeling land transactions data is taken from Chen and Kung (2019).

Table C4: Network Effects, Construction Industry

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Outcome = Construction Strikes									
	1st High Profile Inspection Taken Place in # of Quarters after CCDI Arrival in Province									
	0-1 Quarter				2-3 Quarters			4-5 Quarters		6+ Quarters
	Own City	Other Cities			Own City	Other Cities		Own City	Other Cities	Own City
		2-3 Qtrs	4-5 Qtrs	6+ Qtrs		4-5 Qtrs	6+ Qtrs		6+ Qtrs	
1st HP Insp.	2.43***	-0.17	-0.21*	0.07	0.60**	0.01	0.34***	-0.06	0.13	0.03
	(0.66)	(0.19)	(0.13)	(0.15)	(0.30)	(0.14)	(0.11)	(0.26)	(0.09)	(0.46)
Avg, Dep Var Before Insp.	0.12	0.04	0.12	0.11	0.04	0.12	0.11	0.12	0.11	0.11
SD, Dep Var Before Insp.	0.86	0.27	0.66	0.67	0.27	0.66	0.67	0.66	0.67	0.67
Observations	5,862	5,862	5,862	5,862	4,417	4,424	4,424	4,541	4,541	4,560
Year Span	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020
City FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Quarter-Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
City Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Neighbor Insp.	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: The table presents the coefficients $\hat{\delta}$ from Equation (8) using the Callaway and Sant'Anna (2021) estimator, with the strikes in the construction industry as the outcomes. The table presents two effects. First, columns (1), (5), and (8) present the treatment effects for the cities that experienced their first high-profile inspections that took place in $0-1$, $2-3$, $4-5$, and 6 or more quarters after the arrival of the CCDI inspection team in the respective province. Second, the table examines the impact that an earlier wave of high-profile inspections in a city has on strikes in other cities within the same province that have a later wave of high-profile inspections. Such network effects are presented in columns (2)-(4), (6)-(7), and (9). The standard errors are presented in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

Table C5: Network Effects, Excluding Construction Industry

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Outcome = Other Strikes Excluding Construction									
	1st High Profile Inspection Taken Place in # of Quarters after CCDI Arrival in Province									
	0-1 Quarter				2-3 Quarters			4-5 Quarters		6+ Quarters
	Own City	Other Cities			Own City	Other Cities		Own City	Other Cities	Own City
		2-3 Qtrs	4-5 Qtrs	6+ Qtrs		4-5 Qtrs	6+ Qtrs		6+ Qtrs	
1st HP Insp.	2.36**	-0.56	-0.26*	0.15	1.04*	0.34**	0.29*	-0.13	0.57*	-1.03
	(0.93)	(0.38)	(0.14)	(0.16)	(0.62)	(0.17)	(0.16)	(1.18)	(0.30)	(0.86)
Avg, Dep Var Before Insp.	1.75	0.40	0.42	0.58	0.40	0.42	0.58	0.42	0.58	0.58
SD, Dep Var Before Insp.	8.99	1.13	1.29	2.80	1.13	1.29	2.80	1.29	2.80	2.80
Observations	5,862	5,862	5,862	5,862	4,417	4,424	4,424	4,541	4,541	4,559
Year Span	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020
City FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Quarter-Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
City Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Neighbor Insp.	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: The table presents the coefficients $\hat{\delta}$ from Equation (8) using the Callaway and Sant'Anna (2021) estimator, with strikes from the manufacturing, service, mining, education, and transportation industries serving as the outcomes, excluding any outcomes from the construction industry. The table presents two effects. First, columns (1), (5), and (8) present the treatment effects for the cities that experienced their first high-profile inspections that took place in $0-1$, $2-3$, $4-5$, and 6 or more quarters after the arrival of the CCDI inspection team in the respective province. Second, the table examines the impact that an earlier wave of high-profile inspections in a city has on strikes in other cities within the same province that have a later wave of high-profile inspections. Such network effects are presented in columns (2)-(4), (6)-(7), and (9). The standard errors are presented in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

Table C6: Timings of Inspections and Network Effects in Neighboring Cities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All Quarters	1st High Profile Inspection Taken Place in # of Quarters after CCDI Arrival in Province					
	All Neighboring Cities	0-1 Quarter			2-3 Quarters		4-5 Quarters
		Neighboring Cities			Neighboring Cities		Neighboring Cities
		2-3 Qtrs	4-5 Qtrs	6+ Qtrs	4-5 Qtrs	6+ Qtrs	6+ Qtrs
1st HP Insp.	-0.15	0.02	0.09	-0.05	0.21	0.45***	0.25
	(0.47)	(0.28)	(0.12)	(0.16)	(0.22)	(0.17)	(0.21)
Avg, Dep Var Before Insp	2.41	0.19	0.15	0.07	0.17	0.21	0.25
SD, Dep Var Before Insp	4.25	0.60	0.51	0.45	0.51	0.86	0.88
Observations	9,898	5,862	5,862	5,862	4,413	4,413	4,541
Year Span	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020
City FE	YES	YES	YES	YES	YES	YES	YES
Quarter-Year FE	YES	YES	YES	YES	YES	YES	YES
City Controls	YES	YES	YES	YES	YES	YES	YES
Neighbor Insp.	YES	YES	YES	YES	YES	YES	YES
# of Neighboring Cities	YES	YES	YES	YES	YES	YES	YES

Notes: The table presents the coefficients $\hat{\delta}$ from Equation (8) using the Callaway and Sant'Anna (2021) estimator. Column (1) presents the average treatment effect for the neighboring cities of city c if city c experienced its first high-profile inspections for the entire sample. Columns (2) to (7) present the treatment effects for neighboring cities (that did not yet experience high-profile inspections until several quarters after) of city c, if city c experienced its first high-profile inspections $0-1$, $2-3$, or $4-5$ quarters after the arrival of the CCDI inspection team in the respective province of city c. The number of quarters until neighboring cities experienced their first high-profile inspections are indicated in the "Neighboring Cities" section of the table. The standard errors are clustered at city-level and presented in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

Table C7: Decomposing Network Effects of Neighboring Cities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	1st High Profile Inspection Taken Place in # of Quarters after CCDI Arrival in Province											
	Own City											
	0-1 Quarter						2-3 Quarters				4-5 Quarters	
	Neighboring Cities						Neighboring Cities				Neighboring Cities	
	2-3 Qtrs		4-5 Qtrs		6+ Qtrs		4-5 Qtrs		6+ Qtrs		6+ Qtrs	
	Control Insp.	*Control Strikes*	*Insp.*	*Strikes*	*Insp.*	*Strikes*	*Insp.*	*Strikes*	*Insp.*	*Strikes*	*Insp.*	*Strikes*
1st HP Insp.	-0.02	-0.05	0.23*	0.09	-0.07	0.06	0.16	0.16	0.54***	0.32	0.07	0.25
	(0.33)	(0.31)	(0.14)	(0.14)	(0.19)	(0.19)	(0.26)	(0.26)	(0.16)	(0.22)	(0.17)	(0.25)
Avg, Dep Var	0.19	0.19	0.15	0.15	0.07	0.07	0.17	0.17	0.21	0.21	0.25	0.25
SD, Dep Var	0.60	0.60	0.51	0.51	0.45	0.45	0.51	0.51	0.86	0.86	0.88	0.88
Observations	5,862	5,860	5,862	5,858	5,862	5,858	4,421	4,409	4,421	4,406	4,536	4,536
Year Span	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020
City FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Quarter-Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
City Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Neighbor Insp.	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
# of Neighboring Cities	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Control Strikes	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Control Inspections	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO

Notes: The table presents the coefficients $\hat{\delta}$ from Equation (8) using the Callaway and Sant'Anna (2021) estimator. The table presents the treatment effects for the neighboring cities of city c if city c experienced its first high-profile inspections that took place in $0-1$, $2-3$, $4-5$, and more than 6 quarters after the arrival of the CCDI inspection team in the respective province of city c. The neighboring cities are defined as those that have not undergone high-profile inspections until several quarters ($2-3$, $4-5$, and more than 6 quarters) later, as indicated under the "Neighboring Cities" section on the table. Under each group of neighboring cities, I include or exclude inspections and strikes from the cities that have previously experienced inspections, to determine whether it was the inspections or strikes that caused the increase in strikes in neighboring cities. The standard errors are clustered at city-level and presented in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

Table C8: Treatment Effects by Migrant Workers Connections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Proportion of MWs with *Laoxiang* as Primary Contacts in Current City								
	Less Than 25%			25% - 50%			More than 50%		
1st HP Insp.	0.22	0.16	0.23	0.56**	0.50**	0.46**	0.70***	0.69**	0.68**
	(0.21)	(0.21)	(0.21)	(0.22)	(0.23)	(0.22)	(0.27)	(0.27)	(0.28)
Average, Dep Var Before Insp.	0.53	0.53	0.53	0.87	0.87	0.87	1.35	1.35	1.35
SD, Dep Var Before Insp.	1.00	1.00	1.00	1.69	1.69	1.69	2.85	2.85	2.85
Observations	3,957	3,778	3,753	5,874	5,694	5,694	6,100	5,920	5,920
Year Span	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020
City FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Quarter-Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
City Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
Neighbor Insp.	YES	YES	YES	YES	YES	YES	YES	YES	YES
Happiness Controls	NO	YES	YES	NO	YES	YES	NO	YES	YES
Inter-Province Controls	NO	NO	YES	NO	NO	YES	NO	NO	YES

Notes: The table presents the coefficients $\hat{\delta}$ from Equation (8) using the Callaway and Sant'Anna (2021) estimator. The table presents the impact of treatment on cities with different proportions of migrant workers whose primary contacts were *Laoxiang*, other migrants from their city of origin and who are now working in the same current city. The measures of *Laoxiang* are from the 2011 China Migrants Dynamic Survey (CMDS). The purpose of the table is to demonstrate the heterogeneous effects of strikes across cities with varying levels of migrant worker networks before the 2012 anti-corruption campaign. The standard errors are clustered at city-level and presented in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

Table C9: Robustness Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Labor Dispute Cases				Strikes					
	Total	Wage Arrears	Insurance	Accidents	Shuffle Times	Gardner (2021)	IHS	ZINB	Drop Guangdong	Drop Big Cities
1st HP Insp.	14.19***	3.46***	0.96**	1.08**	-0.03	0.68***	0.14**	0.42***	0.64***	0.45***
	(3.17)	(0.80)	(0.40)	(0.47)	(0.13)	(0.10)	(0.06)	(0.06)	(0.17)	(0.16)
Average, Dep Var Before Insp.	32.54	6.69	3.54	4.37	0.44	0.48	0.48	0.48	0.35	0.41
SD, Dep Var Before Insp.	72.21	16.27	6.33	8.24	1.64	1.35	1.35	1.35	0.81	1.06
Observations	7,046	7,046	7,046	7,046	9,944	9,967	9,898	9,984	9,158	9,754
Year Span	2013-2020	2013-2020	2013-2020	2013-2020	2010-2020	2010-2020	2010-2020	2010-2020	2010-2020	2010-2020
City FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Quarter-Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
City Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Neighbor Insp.	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: The table presents the coefficients $\hat{\delta}$ from Equation (8), and columns (1) to (5) use the Callaway and Sant'Anna (2021) estimator. Columns (1) to (4) display the treatment effects of first high-profile inspections on labor dispute cases. The labor disputes are recorded by China Judgements Online from 2013 to 2020. Column (5) shuffles the timing of treatment effects on strikes. Column (6) estimates Equation (8) using the Gardner (2021) estimator. Column (7) transforms the strikes using the inverse hyperbolic sine (IHS) function and estimates Equation (8) using this transformed variable. Column (8) fits Equation (8) using a zero-inflated negative binomial model. Columns (9) and (10) estimate Equation (8) by excluding all cities in Guangdong province and excluding four major cities: Beijing, Shanghai, Guangzhou, and Shenzhen. The standard errors are clustered at city-level and presented in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

Table C10: Treatment Effects by Princeling Land Transactions, Controlling Own Inspections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Number of Princeling Land Transactions				Predicted Princeling Land Transactions			
	0 to 6	7 to 8	9 to 10	11 to 16	<= 6	(6,8]	(8,10]	>10
First HP Insp.	-0.29	-0.06	0.76**	0.95***	-0.08	-0.22	0.12	1.92***
	(0.21)	(0.22)	(0.32)	(0.31)	(0.28)	(0.15)	(0.19)	(0.48)
Average, Dep Var Before Insp.	0.30	0.31	0.50	0.71	0.09	0.25	0.30	1.07
SD, Dep Var Before Insp.	0.80	0.73	1.39	1.83	0.34	0.63	0.78	2.17
Observations	5,016	4,355	4,632	5,424	3,736	5,172	5,132	5,398
Year Span	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020
City FE	YES	YES	YES	YES	YES	YES	YES	YES
Quarter-Year FE	YES	YES	YES	YES	YES	YES	YES	YES
City Controls	YES	YES	YES	YES	YES	YES	YES	YES
Neighbor Insp.	YES	YES	YES	YES	YES	YES	YES	YES
Own Insp.	YES	YES	YES	YES	YES	YES	YES	YES

Notes: The table presents the coefficients $\hat{\delta}$ from Equation (8) using the Callaway and Sant'Anna (2021) estimator. The first four columns show treatment effects from cities with different numbers of princeling land transactions before the anti-corruption campaign, while columns five to eight show results from an alternative classification that weights princeling land transactions by population and GDP per capita. The data for princeling land transactions is from Chen and Kung (2019). The standard errors are presented in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

Table C11: Treatment Effects by Princeling Land Transactions, First Wave

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Number of Princeling Land Transactions				Predicted Princeling Land Transactions			
	0 to 6	7 to 8	9 to 10	11 to 16	<= 6	(6,8]	(8,10]	>10
First HP Insp.	-0.10	-0.02	1.03*	1.74***	0.29	-0.05	0.35	2.13***
	(0.21)	(0.30)	(0.62)	(0.52)	(0.42)	(0.19)	(0.24)	(0.47)
Average, Dep Var Before Insp.	0.19	0.29	0.82	1.13	0.01	0.11	0.29	1.35
SD, Dep Var Before Insp.	0.52	0.64	2.01	2.57	0.09	0.31	0.59	2.67
Observations	3,740	3,612	3,860	4,180	3,388	3,632	3,924	4,450
Year Span	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020
City FE	YES	YES	YES	YES	YES	YES	YES	YES
Quarter-Year FE	YES	YES	YES	YES	YES	YES	YES	YES
City Controls	YES	YES	YES	YES	YES	YES	YES	YES
Neighbor Insp.	YES	YES	YES	YES	YES	YES	YES	YES
Own Insp.	YES	YES	YES	YES	YES	YES	YES	YES
First Wave?	YES	YES	YES	YES	YES	YES	YES	YES

Notes: The table presents the coefficients $\hat{\delta}$ from Equation (8) using the Callaway and Sant'Anna (2021) estimator. This table presents the treatment effects only for the cities that experienced their first high-profile inspections either 0 or 1 quarter after the arrival of the CCDI in their province. The first four columns show treatment effects from cities with different numbers of princeling land transactions before the anti-corruption campaign, while columns five to eight show results from an alternative classification that weights princeling land transactions by population and GDP per capita. The data for princeling land transactions is from Chen and Kung (2019). The standard errors are presented in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

Table C12: Robustness for Workers' Expected Returns Mechanism

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Outcome = Labor Disputes				Outcome = Strikes			
	Number of Princeling Land Transactions				Precentage of Inspections Related to Businesses			
	0 to 6	7 to 8	9 to 10	11 to 16	(0%, 5%]	(5%, 10%]	(10%, 15%]	>15%
First HP Insp.	2.90*	2.34	12.75**	27.40***	0.04	0.19	0.91**	1.01***
	(1.48)	(2.41)	(6.30)	(7.04)	(0.21)	(0.12)	(0.40)	(0.35)
Average, Dep Var Before Insp.	5.67	8.65	14.00	17.35	0.39	0.31	0.92	0.44
SD, Dep Var Before Insp.	7.45	15.51	22.99	45.35	0.89	0.88	2.81	1.16
Observations	2,142	1,334	1,454	1,996	3,524	4,316	3,085	3,496
Year Span	2013-2020	2013-2020	2013-2020	2013-2020	2011-2020	2011-2020	2011-2020	2011-2020
City FE	YES	YES	YES	YES	YES	YES	YES	YES
Quarter-Year FE	YES	YES	YES	YES	YES	YES	YES	YES
City Controls	YES	YES	YES	YES	YES	YES	YES	YES
Neighbor Insp.	YES	YES	YES	YES	YES	YES	YES	YES
Own Insp.	YES	YES	YES	YES	YES	YES	YES	YES

Notes: The table presents the coefficients $\hat{\delta}$ from Equation (8) using the Callaway and Sant'Anna (2021) estimator. The first four columns show treatment effects for labor disputes from cities with different numbers of princeling land transactions before the anti-corruption campaign, while columns five to eight show results from an alternative measure of corruption level using the percentage of officials inspected with a position title related to businesses, including both private and public enterprises. The data for princeling land transactions is from Chen and Kung (2019). The data for labor disputes is from China Judgements Online (CJO) 2013-2020. The standard errors are presented in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

Table C13: Other Possible Mechanisms

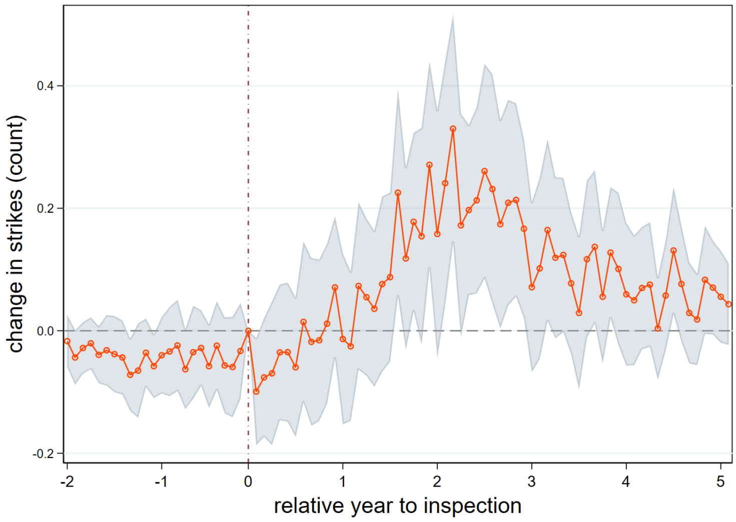
	(1)	(2)	(3)	(4)	(5)	(6)
	Protests	Workplace Accidents				
			Accident Counts		Deaths	
		Total	Construction	Manufacturing	Construction	Manufacturing
1st HP Insp.	0.01	-0.09	0.00	0.03	0.03	0.32
	(0.03)	(0.08)	(0.03)	(0.03)	(0.10)	(0.45)
Average, Dep Var Before Insp.	0.02	0.76	0.06	0.06	0.27	0.32
SD, Dep Var Before Insp.	0.26	1.14	0.27	0.26	1.47	2.67
Observations	9,898	9,898	9,898	9,898	9,898	9,898
Year Span	2011-2020	2011-2016	2011-2016	2011-2016	2011-2016	2011-2016
City FE	YES	YES	YES	YES	YES	YES
Quarter-Year FE	YES	YES	YES	YES	YES	YES
City Controls	YES	YES	YES	YES	YES	YES
Neighbor Insp.	YES	YES	YES	YES	YES	YES

Notes: The table presents the coefficients $\hat{\delta}$ from Equation (8) using the Callaway and Sant'Anna (2021) estimator. The first column shows treatment effects for the outcome of protest counts, using data from the Global Database of Events, Language, and Tone (GDELT). Columns (2) to (6) show treatment effects for the outcome of workplace accidents counts, using data from the China Stock Market & Accounting Research Database (CSMAR), which collects and compiles data sources from China's State Administration of Work Safety (SAWS). * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

D Alternative Event: First Inspection

In this section, the baseline event is defined as the first inspection, instead of high-profile inspection, following the CCDI inspection team's arrival in the province. The dynamic impact of this first inspection on strike changes is shown in Figure D1. A lagged strike increase post-inspection suggests that the first inspection might not draw enough media attention or sufficiently prove CCDI inspections' efficacy.

Figure D1: Baseline Outcomes with Alternative Event



Notes: The graph demonstrates the estimated λ_k from Equation (7), which shows the changes in the number of strikes in the months before and after the first inspection in a city. The hollow red dots represent the estimates in each month and the light blue region represents the standard errors. The dotted vertical line indicates the timing (one month prior) of the first inspection case in a city, given the arrival of the CCDI inspection team in the same province. The sample includes $43,440$ observations from 362 prefectures from 2011 to 2020.

Table D1 reveals a short-term threefold increase in strikes consistent with the baseline analysis once all relevant variables are controlled. However, the long-term effects become insignificant after accounting for inspections in neighboring cities, although it remains significant without this and when city characteristics are controlled. This could be due to the different control group used in the comparison to the baseline analysis. Almost all cities underwent at least one inspection post-CCDI's provincial arrival, leaving a control group composed mainly of later-treated cities. As discussed in Section 6.4, the effects of these cities dissipate over time, making the long-term effects insignificant.

Importantly, the CCDI team's arrival time in a province is not used as the event due to insufficient temporal variation when grouping all cities into four treatment cohorts in May 2013, November 2013, March 2014, and July 2014. This approach is problematic as it is unlikely that workers would respond immediately to the provincial arrival of the CCDI team, especially if most cities had not been inspected yet. Moreover, cities with few or no high-profile inspections could be misleadingly categorized as part of the earlier treated group, leading to biased results.

Table D1: First Stage and Baseline Outcomes of Inspections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Event: First Inspection after CCDI Arrival in Province							
	Short Term				*Long Term*			
	Strike Incidents			Strikes per 10M ppl	Strike Incidents			Strikes per 10M ppl
Post 1st Insp.	0.99***	1.18***	0.55***	1.08***	0.59***	0.67***	0.08	-0.30
	(0.11)	(0.14)	(0.15)	(0.37)	(0.11)	(0.12)	(0.13)	(0.35)
Average, Dep Var Before Insp.	0.29	0.29	0.29	0.91	0.29	0.29	0.29	0.91
SD, Dep Var Before Insp.	1.06	1.06	1.06	4.04	1.06	1.06	1.06	4.04
Observations	8,688	6,053	5,995	5,995	14,480	9,013	8,919	8,919
Year Span	2011-2016	2011-2016	2011-2016	2011-2016	2011-2020	2011-2020	2011-2020	2011-2020
City FE	YES	YES	YES	YES	YES	YES	YES	YES
Quarter-Year FE	YES	YES	YES	YES	YES	YES	YES	YES
City Controls	NO	YES	YES	YES	NO	YES	YES	YES
Neighbor Insp.	NO	NO	YES	YES	NO	NO	YES	YES

Notes: The table presents the coefficients $\hat{\delta}$ from Equation (8) using the Callaway and Sant'Anna (2021) estimator. The baseline impacts show the average change in the number of strikes in the quarters after the first inspection in a city, conditional on the arrival of the CCDI in the province, compared to the strikes in cities that have not yet and never experienced a high-profile inspection. The variation in the observations is due to the absence of certain characteristics in the control variables in different cities. The standard errors are presented in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

E Additional Results

E.1 Prior Strikes

This section investigates treatment effects across cities based on different average quarterly strike numbers before their first high-profile inspection. Roughly 10% of cities had no strikes per quarter, while the median city recorded a quarterly average of 0.25 strikes prior to the first high-profile inspection. I explored whether strike increases were more prevalent in cities with higher or lower prior strike frequencies, and if cities with no prior strikes experienced strikes after their first high-profile inspection.

As shown in Table E1, columns (1) to (3), cities with no previous strikes or a quarterly strike average less than 0.25 saw an increase of 0.3 strikes per quarter post the first high-profile inspection. Given the sample's average of 0.48 strikes per quarter before treatment, this is a significant rise for cities that never had a strike before. For cities with a quarterly strike average of less than 0.25, strikes increased threefold. Cities with a quarterly average over 0.25 saw nearly a 50% increase in strikes. Thus, corruption inspections impacted all cities, regardless of their prior strike levels, with the effect size increasing particularly for cities with fewer pre-inspection strikes.

E.2 Extensive Margin

In this analysis, the outcome variable is the occurrence of a strike in a city within a quarter, rather than the number of strikes. This extensive margin is only relevant for cities that had not experienced a strike before the high-profile inspections, as most cities had. As per Table E1, columns (4) to (6), there was an 18% increase in the probability of witnessing a strike after the first high-profile inspections in cities without any prior strikes. Conversely, cities with previous strikes did not show any change in strike likelihood post-inspection.

Table E1: Strike Intensity & Compare with Extensive Margin

	(1)	(2)	(3)	(4)	(5)	(6)
	Percentile, Average Number of Strikes per Quarter Before First HP Insp.					
	Zero Strike	Bottom 50th (exclude 0 strike)	Top 50th	Zero Strike	Bottom 50th (exclude 0 strike)	Top 50th
	Outcome = Number of Strikes			*Outcome = Whether there is a strike*		
Post 1st HP Insp.	0.30***	0.30***	0.52**	0.18***	0.01	0.00
	(0.09)	(0.11)	(0.25)	(0.07)	(0.06)	(0.06)
Average, Dep Var Before Insp.	0.00	0.14	0.87	0.00	0.13	0.40
SD, Dep Var Before Insp.	0.00	0.39	1.82	0.00	0.34	0.49
Observations	7,040	9,160	10,040	3,718	5,760	6,748
Year Span	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020	2011-2020
City FE	YES	YES	YES	YES	YES	YES
Quarter-Year FE	YES	YES	YES	YES	YES	YES
City Controls	YES	YES	YES	YES	YES	YES
Neighbor Insp.	YES	YES	YES	YES	YES	YES

Notes: The table presents the coefficients $\hat{\delta}$ from Equation (8) using the Callaway and Sant'Anna (2021) estimator. Columns (1)-(3) present the average change in the number of strikes in the quarters following the first inspection in a city, conditional on the arrival of the CCDI in the province, compared to the strikes in cities that have not yet experienced, and never will experience, a high-profile inspection. Columns (4)-(6) present the average change in the occurrence of strikes in the quarters following the first inspection in a city, conditional on the arrival of the CCDI in the province, compared to the strikes in cities that have not yet experienced, and never will experience, a high-profile inspection. Within columns (1)-(3), column (1) shows the results for cities that experienced an average of 0 strike per quarter before the first high-profile inspection, column (2) shows the results for cities that experienced an average number of strikes per quarter in the bottom 50th percentile of the distribution before the inspection, and so on. The variation in the observations is due to the absence of certain characteristics in the control variables in different cities. The standard errors are presented in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

F Duration Analysis on Strikes Decline

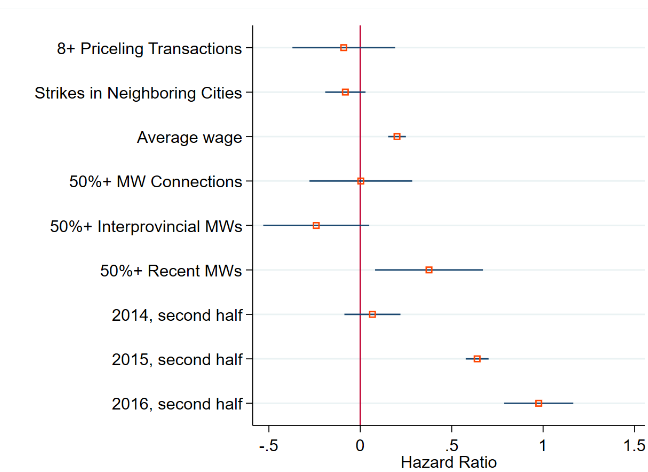
This section explores how various factors related to workers' strike returns can affect the duration from a city's strike increase to decrease after its first high-profile inspection. To analyze this, I employ a duration analysis model:

$$h(t|x, \beta) = h_0(t)e^{\boldsymbol{X}\beta + \boldsymbol{Z}\alpha}. \tag{15}$$

In this model, $h(t|x, \beta)$ denotes the hazard of strike decrease at month t following the first high-profile inspection in a city. The baseline hazard rate is represented by $h_0(t)$. The vector $\boldsymbol{Z}$ includes variables relating to strike returns, such as princeling land transactions, average wage, the percentage of migrant workers (both inter-provincial and recent arrivals within two years), and the percentage of migrant workers linked mainly to *Laoxiang*. $\boldsymbol{X}$ represents city-level controls (number of strikes, inspections, employment rate, population, GDP per capita, and GDP growth) to account for possible confounders due to city scale variations affecting strike duration. The coefficients vector α depicts the relationships between predictors in $\boldsymbol{Z}$ and the hazard of strike decrease.

Figure F1 suggests that strikes decline faster as strike returns diminish. In cities with higher average wages (indicating higher strike opportunity costs), strike decline was faster. Conversely, strikes decreased more slowly in cities with a higher proportion of inter-provincial migrant workers, implying these workers had more grievances prompting them to strike, and their strong network fostered strike participation. However, the presence of recent migrant workers (those in the city for less than two years) led to a quicker strike decrease, indicating their lower bargaining power, fewer connections, and higher strike costs.

Figure F1: Estimates of Duration Analysis on Decreasing Strikes



Notes: The graph demonstrates the estimates $\hat{\beta}$ from Equation (15). The covariates have been standardized. Positive coefficients indicate that the indicator has a positive correlation with a quicker decrease in strikes, while negative coefficients indicate the opposite. The data on migrant workers is taken from the 2011 China Migrants Dynamic Survey (CMDS). Information on princeling land transactions is obtained from Chen and Kung (2019), and information on wages is sourced from the China City Statistical Yearbook (CCSY). The sample includes $43,440$ observations from 362 prefectures from 2011 to 2020.