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

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Abstract

This paper studies how government actions shape workers' expected returns to striking by exploiting China's anti-corruption campaign as a natural experiment. Using a city-level dataset linking corruption inspections to strikes from 2011 to 2020, I show that strike incidents rise sharply after a city's first high-profile inspection, driven largely by wage arrears from private firms and concentrated in the construction and manufacturing sectors. Strikes also spread through network channels, with later-treated and neighboring cities responding to inspections elsewhere. Evidence indicates that inspections weakened firm–government ties and shifted government attention, raising workers' expected returns and revealing pre-existing grievances among workers.

Keywords — political economy, collective bargaining, corruption

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1 Introduction

Workers have long used strikes to improve wages and working conditions, contributing to major policy reforms such as the 1935 National Labor Relations Act and the 1938 Fair Labor Standards Act (Bernstein, 1970). Traditional economic models treat strikes primarily as a bargaining process between workers and firms, with minimal government involvement (Ashenfelter and Johnson, 1969; Kennan 1980, 1986; Card 1988, 1990; Cramton and Tracy, 1992; Cramton et al., 1999). Yet in many institutional contexts, both workers and firms depend on government support during labor disputes, and government alignment can significantly shape their relative bargaining power. This raises a central question: what role does government play in negotiations between workers and firms?

This paper argues that government plays a crucial role in shaping workers' expected returns to striking. Prior evidence shows that politically connected firms face weaker union power (Song et al., 2016) and receive preferential treatment from the government in disputes with workers (Faccio, 2006; Brogaard et al., 2021). As a result, when workers expect limited government support, they anticipate lower potential gains and higher risks of government suppression.

Empirically identifying how government involvement affects workers' strike returns is challenging. Government actions shape not only labor relations but also a wide range of economic outcomes. For example, they can influence firm productivity (Colonnelli and Prem, 2021), market structure (Fenizia and Saggio, 2024), and broader economic activity. These channels can in turn shape workers' returns to striking. Moreoever, strikes themselves can also influence government and firm behavior. For instance, collective actions such as protests may limit the influence of politically connected firms (Acemoglu et al., 2018).

This paper utilizes China's anti-corruption campaign to address endogeneity concerns. Launched in September 2012 under Xi Jinping, the campaign investigated Chinese Communist Party (CCP) officials nationwide.[1] Between 2012 and 2017, hundreds of thousands of officials were inspected across provinces, municipalities, universities, and state-owned enterprises (SOEs). Inspection decisions, managed by the Commission for Discipline Inspection (CCDI), were not disclosed in advance, making the timing and location of inspections largely unforeseeable for local governments, firms, and workers.[2] This setting provides a natural experiment for examining how unexpected inspections generate shocks to local governments and, in turn, shape workers' strike responses.

To examine this causal relationship, I construct a unique dataset linking CCDI corruption inspections to strike counts in each prefecture-level city in China, aggregated both monthly and quarterly from 2011 to 2020. A natural strategy would be to study firm-level responses, but suitable data linking firms to their workers are not available. I therefore rely on city-level variation, which captures the aggregate impact of inspections on local labor markets. This approach remains informative because large-scale corruption inspections often signal broader institutional shifts that influence firms and workers beyond those directly targeted.

[1] The campaign initially targeted only CCP-affiliated officials. Its scope expanded in 2018 with the Supervision Law, which extended oversight to officials outside the CCP.

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

I then employ an event study approach that leverages both temporal and spatial variation by comparing strike changes across cities with different inspection timings. An event is defined as the first high-profile inspection in a city, conditional on the inspection team's arrival in the province. I classify an inspection as high-profile if it targets CCP officials at the Deputy-Bureau-Director level (level 6) or above. High-ranking inspections typically receive broader media coverage and are therefore considered high-profile, whereas lower-ranking inspections attract minimal attention and are considered low-profile.[3]

While CCDI inspection timings may not be fully random due to potential unobserved factors, I establish two points that support the key assumption: cities would have followed parallel strike trends in the absence of inspections. First, inspection timings are uncorrelated with a city's economic characteristics, strike activity, or corruption levels before the campaign. Second, the baseline results show no evidence of anticipation in strike patterns prior to inspections. Taken together, these findings indicate that inspection timings were not predictable and that changes in strike activity were unlikely driven by confounding factors.

Before examining the impact of inspections on strikes, I first establish that CCDI inspection teams were effective in enforcing the anti-corruption campaign. After a team arrived in a province and conducted its first citywide inspection, the total number of inspections in a city tripled within one year and increased six-fold within two years. High-profile inspections also quadrupled. Social media attention rose as well: *Weibo* posts mentioning the inspected official's name tripled within six months of the first city inspection. In contrast, no *Weibo* discussions were found regarding low-profile inspections.

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 the pre-inspection average of 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 results remain robust under recent methods for staggered treatments in difference-in-differences. Using the estimators of Callaway and Sant'Anna (2021) and Gardner (2021) yields consistent strike effects. I also address the prevalence of zero strike counts in city-quarter observations by applying an inverse hyperbolic sine (IHS) transformation and estimating a zero-inflated negative binomial model. Both approaches produce results that align closely with the baseline findings.

Most of the increase in strikes occurred in the construction and manufacturing industries, driven primarily by wage arrears and concentrated in private firms rather than SOEs. These strikes were typically small-scale, involving fewer than 100 participants, and were carried out through protests and sit-ins. The evidence suggests that the surge was led by low-skilled workers, likely migrant workers, seeking unpaid wages. These workers often face significant grievances due to their non-

[3] Details on CCP bureaucratic rankings are provided in Section 4 as part of the introduction to the Corruption Investigation Dataset (CID). Table B1 summarizes each rank with examples, and Table 2 reports inspection counts by rank and category. High-profile inspections, defined as rank 6 or above, account for about 7% of all inspections. Additional evidence supporting the use of high-profile inspections as the event is presented in Appendix C.

residential status in the cities where they work and limited protection from formal labor contracts (Chan, 2001).

I then examine whether network effects contribute to the spread of strikes across cities and among migrant workers, who are individuals relocating from rural birthplaces to work in non-agricultural sectors elsewhere. First, I find that strikes increase in cities awaiting their first high-profile inspections when other cities in the same province are undergoing such inspections. Similarly, when a city experiences its first high-profile inspection, nearby cities that have not yet been inspected also see a rise in strikes. These results show that workers react to strikes elsewhere by striking proactively after observing first-wave high-profile inspections in related cities. In addition, cities with a higher share of migrant workers connected through *Laoxiang* networks, or ties among migrants from the same origin city, experience larger post-inspection increases in strike activity.

The last step is to uncover the mechanisms through which inspections increase strike activity. I examine several potential channels, guided by a simplified conceptual framework of how workers decide to strike. These channels propose that inspections (1) disrupt or weaken firm–government ties, (2) shift government attention from being hostile or indifferent toward workers to becoming neutral or supportive, (3) alter local economic conditions, or (4) affect other institutional factors. My analysis evaluates the extent to which the available data support each of these channels. The goal is not to fully confirm or dismiss any single mechanism, and it is possible that multiple channels operate simultaneously.

The existing evidence most strongly supports the first channel, that inspections disrupted or weakened firm–government connections. Strikes rose most sharply in cities where firms had closer political ties before the anti-corruption campaign, as measured by pre-campaign entertainment expenses relative to sales, the frequency of princeling land transactions, and the post-campaign share of inspected officials with business-related titles. These patterns indicate that inspections reduced political protection for connected firms and increased workers' expected returns from striking. I also provide additional evidence by matching firm information with labor dispute court records, and find that workers' winning rates in wage-arrear cases against more corrupt firms increase after inspections, further supporting the view that politically linked firms lose influence once inspections occur.

There is also some evidence supporting the second channel: inspections appear to have created a temporary window of opportunity for workers to strike in environments where local governments previously had strong incentives to suppress labor unrest to maintain economic performance and stability. Strike increases were concentrated in cities led by mayors nearing the end of their promotion window, consistent with inspections shifting local priorities away from economic growth and toward political compliance. By contrast, there is little evidence supporting the remaining channels that propose inspections altered local economic conditions, firms' financial conditions, bureaucratic morale, official turnover, or workers' trust in government. Taken together, the results suggest that CCDI inspections amplified labor activism primarily by dismantling entrenched political–business ties and by redirecting government attention.

My research speaks to several broader concerns. First, it examines labor activism in a high state-capacity authoritarian setting, where grassroots organizations such as labor unions hold limited political influence. Much of the economics literature on strikes focuses on developed democracies (Hicks, 1963; Ashenfelter and Johnson, 1969; Kennan, 1980, 1986; Card 1988, 1990; Cramton and Tracy, 1992; Glazer, 1992; Cramton et al., 1999; Brunnschweiler et al., 2012) and analyzes workers' strike decisions in institutional environments that differ markedly from authoritarian or developing contexts. This stands in contrast to research in political science and sociology, which highlights how institutions shape labor practices and workers' political demands (Ngai, 2005; Lee, 2007; Gallagher, 2011, 2017). My work contributes by providing empirical evidence on how an authoritarian environment, where government plays a significant role in labor disputes, affects workers' strike decisions. These insights remain relevant even for fragile or unstable democracies in which political dynamics influence worker welfare.

Second, my study joins the debate on whether corruption greases or sands the wheels of the economy. On one hand, a large body of work argues that corruption hinders 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, 2024). On the other hand, some studies contend that corruption can indirectly promote growth through more efficient rent-seeking by firms (Lui, 1985; Bardhan, 1997; Méon and Weill, 2010; Dreher and Gassebner, 2013). Prior research has also examined the effects of anti-corruption policies such as audits, inspections, and official turnovers, with many studies finding that these measures reduce corruption and stimulate economic growth (Bobonis et al., 2016; Avis et al., 2018; Chen and Kung, 2019; Colonnelli and Prem, 2021; Fenizia and Saggio, 2024). However, not all evidence points to success; for example, Riaño (2021) shows that an anti-nepotism reform in Colombia had limited impact on reducing nepotism within the bureaucracy.

My findings contribute to the literature that uses China's anti-corruption campaign to empirically study corruption's effects. Existing work shows that the campaign weakened firm–government connections (Chen and Kung, 2019; Chu et al., 2019; Ding et al., 2020; Hao et al., 2020; Xu et al., 2021), reduced bribery (Qian and Wen, 2015), promoted firm innovation and growth (Dang and Yang, 2016; Lorentzen and Lu, 2016; Xu and Yano, 2017; Fang et al., 2023; Hao et al., 2020; Huang et al., 2023), and improved human capital (Hong, 2024). Yet grassroots responses, particularly from workers rather than firms or bureaucrats, remain understudied. My work fills this gap by examining how workers respond to China's anti-corruption campaign, building on Zhu et al. (2019) and Wang and Dickson (2021). I show that most of the rise in strikes occurs in cities with higher pre-campaign corruption, offering a new perspective by demonstrating that corruption suppresses workers' ability to voice grievances and ultimately harms worker welfare.

Additionally, my research expands the discourse on collective actions in non-democratic regimes, whereas much of the economic literature on collective action focuses on democratic contexts. Zhuravskaya et al. (2020) provides a comprehensive review of this literature, particularly on the role of media in political participation in Western settings. My study complements prior economic research on protests in China (Qin et al., 2017, Beraja et al., 2023). Unlike earlier studies that examine factors such as economic slowdowns (Campante et al., 2023), surveillance technology (Beraja et al., 2023), or social media influence (Qin et al., 2024) in shaping collective action in China, my work offers a worker-centered perspective on how political environments shape collective behavior. It also shows that workers' collective actions can spread across regions through spillover and network effects, contributing to the literature on network dynamics in mobilization (Enikolopov et al., 2020; Qin et al., 2024). Together, these findings highlight that workers, even under authoritarian regimes, actively engage in political participation and voice 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,[4] like wage arrears, excessive hours, and substandard conditions (Leung, 2015; Li et al., 2016). The China Labour Bulletin (CLB) reported over 1,200 labor protests in 2017 and over 1,700 in 2018. Most collective actions aimed to draw media attention and seek government mediation.[5]

Migrant workers, typically unskilled individuals who move from rural areas to cities for non-agricultural employment, often face labor violations because they lack formal work contracts and official residency status (*Hukou*) in their destination cities (Chan, 2001). Between 2012 and 2015, these workers averaged 270.8 million annually and accounted for about 35.1% of China's workforce. Nearly half were employed in construction and manufacturing sectors, with the rest working in service, retail, wholesale trade, transportation, and hospitality sectors.[6] Many migrate with kin or friends and maintain ties with others from the same home region (Xu and Palmer, 2011), which can shape their collective decisions to strike (Chan and Ngai, 2009).

In China, most strikes arise from wage arrears rather than demands for higher pay. In 2022, CLB reported that wage arrears accounted for 87% of labor disputes.[7] Wage arrears are widespread because many firms, especially in construction and manufacturing industries, operate in multi-tier subcontracting chains and face liquidity constraints. When cash is tight, firms often prioritize bank loans, supplier payments, and tax or land-use fees before wages, leaving workers as residual claimants with the weakest enforcement power (Wei and Chan, 2022). Workers therefore seek compensation by drawing attention from local governments and the media, hoping that escalation can prompt government intervention (Leung, 2015). The central government, aware of this systemic problem, tolerates small-scale strikes and limited media coverage to discipline local authorities (Qin et al., 2017).[8] [9] However, large collective actions and widespread online discussions are censored (King et al., 2013), so most strikes remain small and spontaneous, typically involving only a few dozen workers with banners and slogans.[10]

[4]Erik Eckholm, "Workers' Rights Are Suffering in China as Manufacturing Goes Capitalist." *New York Times*, August 22, 2001.
[5]China Labour Bulletin Workers' Calls-for-Help Map, 2020-2022.
[6]2015 Report from the National Bureau of Statistics.
[7]"Wage Arrears after Zero Covid and before Lunar New Year is Symptom of Systemic Problem," *China Labour Bulletin*, January 9, 2023.
[8]"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.
[9]"Government Launched Big Move to Rectify Wage Arrears: Number of Wage Arrears Decreased by 40% Since Last Year," *People's Daily*, December 18, 2017.
[10]"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 the prevalence of small, spontaneous strikes. The All-China Federation of Trade Unions (ACFTU), the country's only national trade union, is nominally responsible for protecting workers' rights. In practice, its close ties to the CCP make it a political instrument rather than an independent representative of workers' interests (Biddulph and Cooney, 1993; O'Leary et al., 2001; Clarke et al., 2004; Taylor and Li, 2007). Although the ACFTU occasionally helps improve workers' treatment (Yao and Zhong, 2013), its authority is weakened by firms' political connections (Song et al., 2016). Its activities often center on propaganda-oriented tasks, such as distributing holiday gifts, rather than resolving labor disputes.[11] Independent unions are prohibited, leaving workers without alternative channels for representation.[12]

Given the lack of a legal platform to voice their grievances, when workers perceive their firms as corrupt and their bargaining power diminished, they resort to strikes to attract 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."*[13]

This example illustrates how workers, unable to recover wages through legal channels, turned to the media to attract national attention and prompt public intervention on their behalf. Therefore, changes in the political environment can influence workers' decisions to strike once they perceive a shift in the expected return to collective action.

2.2 China's Anti-Corruption Campaign

Corruption has been widespread within the CCP since China's 1978 economic reforms, taking forms such as bribery, misuse of public funds, nepotism, and others. In November 2012, Xi Jinping launched a major anti-corruption campaign. Some scholars view the campaign mainly as a political strategy to sideline Xi's rivals (Zhu and Zhang, 2017; Xi et al., 2025), while others acknowledge its broader economic effects, including the weakening of firm–government ties (Chen and Kung, 2019; Chu et al., 2019; Ding et al., 2020; Hao et al., 2020; Xu et al., 2021), reductions in bribery (Qian and Wen, 2015), and increases in firm innovation and growth (Dang and Yang, 2016; Lorentzen and Lu, 2016; Xu and Yano, 2017; Fang et al., 2023; Hao et al., 2020; Huang et al., 2023). Fang (2024) provides a comprehensive review of these debates, situating the political and economic consequences of the campaign within the broader literature. Media reports, such as a *New York Times* interview with Chinese citizens, similarly noted that curbing unofficial fees and bribes earned Xi substantial popular support.[14]

[11] Kaiser Kuo, "Labor unrest and how China balances repression and responsiveness." *SupChina*, September 20, 2021.
[12] "Workers' rights and labour relations in China." *China Labor Bulletin*, June 30, 2020.
[13] Yang Zhang and Yiqi Shi, "Accompanying Migrant Workers to Recover Wages." *People's Daily*, December 2, 2019.
[14] 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-ranking officials, referred to as "tigers," and lower-level officials, referred to as "flies."[15] Table B1 summarizes CCP bureaucratic ranks and their descriptions. In this study, I define "high-profile" inspections as those targeting officials at or above the deputy bureau director level in prefecture-level cities (rank 6 or above), and "low-profile" inspections as those targeting officials below that rank. In other words, high-profile inspections occur at the city level or above, whereas low-profile inspections take place at the district, town, or rural level. This distinction matches the paper's focus on prefecture-level analysis. For example, a city mayor qualifies as high-profile, while a village party secretary is classified as low-profile. This categorization is also consistent with *Weibo* activity during inspections: high-profile officials received at least some *Weibo* mentions, while low-profile officials received none.

The anti-corruption campaign was primarily carried out by the Commission for Discipline Inspection (CCDI), an entity within the CCP that is theoretically independent of other party operations (Xu, 2014).[16] 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 institutional independence, the inspection schedule should be unpredictable to local governments, businesses, and residents.

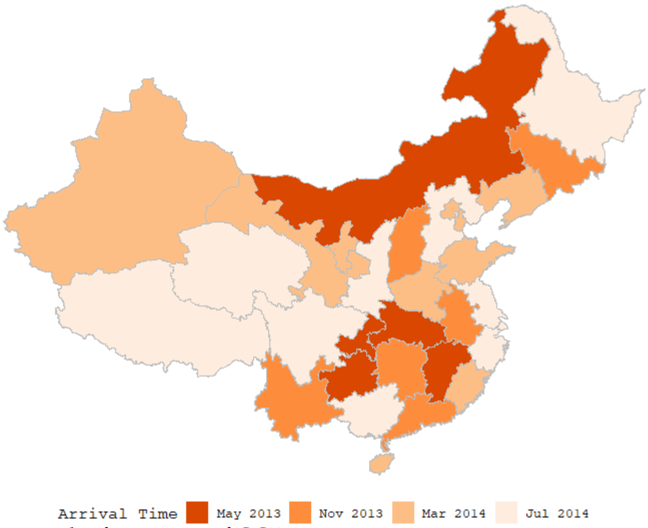
According to Figure 1, the arrival times of CCDI inspection teams do not appear clustered in regions with geographical proximity or similar economic conditions. However, it remains difficult to determine whether inspection timing was influenced by factors such as corruption levels, business performance, worker conditions, or other unobserved characteristics. To address this concern, I conduct a detailed balance test in Section 5 to examine whether pre-campaign labor strikes or city characteristics predict inspection timing. Table B3 shows that none of these variables are statistically significant predictors.

The CCDI inspection team follows a specific process upon arrival in a province.[17] First, evidence of bribery, abuse of power, or misconduct 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 severity of the case. If the evidence is sufficient, the CCDI either arrests the official or initiates an internal party investigation called *Shuanggui*, which results in the official's removal from office. The official is then charged in court using the collected evidence and their confession obtained during the investigation. During the anti-corruption campaign, 99% of investigated officials were convicted.[18]

At the moment an official is arrested or placed under *Shuanggui*, local newspapers report the event, including the official's name, position, and the stated reasons for the investigation (Wang and Dickson, 2021), making the information public. The process from inspection to conviction can take six months to a year. Although little information is available about the closed-door interro-

[15]"Xi Jinping vows to fight 'tigers' and 'flies' in anti-corruption drive." *The Guardian*, January 22, 2013.
[16]According to reporting by *The Beijing News*, Xi and CCDI Secretary Wang Qishan strengthened CCDI independence through the policy *Reform on CCDI (纪委改革方案)* during the anti-corruption campaign.
[17]Ding Xuezhen, "CCDI reveals details of inspection process." *Global Times*, May 12, 2016.
[18]Andrew Jacobs and Chris Buckley, "Presumed Guilty in China's War on Corruption, Targets Suffer Abuses." *The New York Times*, December 8, 2016.

Figure 1: CCDI Inspection Team Arrival by Province



Notes: The figure depicts the first wave of CCDI corruption inspection teams arriving in 31 provinces, municipalities, and autonomous regions across China between 2013 and 2014, following Xi Jinping's launch of the anti-corruption campaign in 2012. Note that the CCDI did not conduct inspections in the special administrative regions of Hong Kong and Macau.

gations,[19] all inspection events are publicly announced. High-profile inspections typically attract substantial media attention. For example, the 2014 investigation of Ding Xuefeng, former mayor of Lvliang, Shanxi, was covered by major outlets such as *People's Daily*, *Economic Daily*, *Caixin News*, and *CCTV News*.

3 Conceptual Framework

Building on the background on workers and the political environment in China, this section presents a simple conceptual outline that illustrates how workers, firms, local governments, and CCDI inspection teams interact, and how these relationships shape workers' decisions to strike.

3.1 Institutional Setting

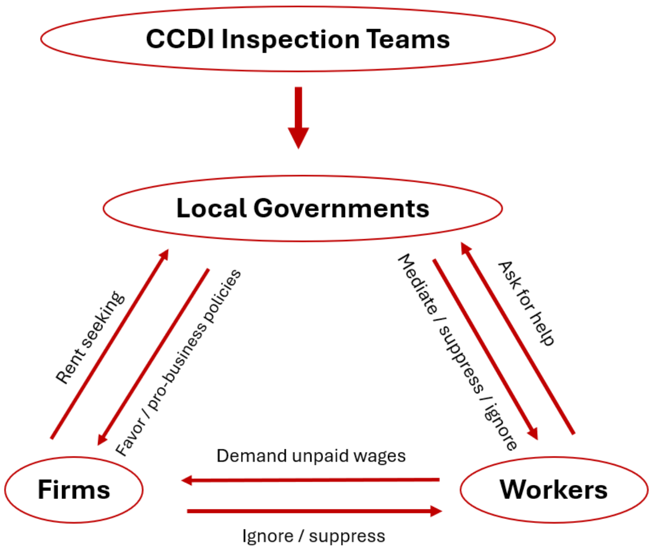
Figure 2 illustrates the triangular relationship among workers, firms, and local governments. Workers often experience wage arrears or related grievances and initially seek payment directly from firms. As discussed in Section 2.1, when firms face liquidity constraints, they may prioritize other obligations, such as supplier payments or bank loans, over paying workers. When firms refuse or fail to pay, disputes can escalate. Workers may then turn to local governments by filing petitions (*Shangfang*), initiating lawsuits, or going on strike. Strikes generally occur as a last resort, after other channels have failed or proved inaccessible (Lee, 2007).

Local governments can respond to worker grievances in several ways, including mediating disputes, suppressing collective action, or ignoring complaints. When firms face liquidity constraints, governments can compel them to prioritize wage repayment over other obligations. Government

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

responses reflect competing priorities such as economic growth, social stability, and relationships with local firms. When firms and officials are closely aligned through political ties, authorities often side with firms, which weakens workers' bargaining power (Song et al., 2016). Even without explicit corruption, local officials may still prioritize social order and economic performance over workers' rights (Li and Zhou, 2005), since strikes can signal instability.

Figure 2: Institutional Relationships among Key Actors



Notes: This figure illustrates the simplified relationships among workers, firms, local governments, and CCDI inspection teams. Workers may demand unpaid wages directly from firms or seek help from local governments through petitions, lawsuits, or strikes. Local governments can mediate, suppress, or ignore these grievances while maintaining pro-business policies or rent-seeking ties with firms. The arrival of CCDI inspection teams can weaken firm–government connections, shift government attention toward worker grievances, and alter economic conditions or other political factors.

3.2 Theoretical Framework

Having established the basic interactions among workers, firms, and local governments, the next step is to consider why strikes occur. In the classic labor strike literature, such as Card (1990), strikes should not arise under full information because they are costly for both workers and firms, and strikes arise only when workers face uncertainty about firms' ability to pay.[20] For example, if workers knew with certainty that firms could not repay wage arrears, they would have no reason to strike given the opportunity cost.

Yet strikes do occur in China. They arise when workers face uncertainty about the outcome, in this case whether wages will be paid. Two forms of uncertainty are especially important. Political uncertainty stems from workers not knowing how willing or able local officials are to intervene labor disputes and compel firms to pay. Economic uncertainty arises because workers cannot ob-

[20] Card (1990) develops a one-sided asymmetric information model in which firms privately observe their profitability, while workers know only the distribution of firm productivity. From the workers' perspective, the optimal strike decision balances the expected gain from inducing high-productivity firms to concede higher wages against the expected cost of a failed or prolonged strike, including foregone earnings during the stoppage. A key implication of the model is that uncertainty is captured by the dispersion of firm productivity: greater dispersion increases both the likelihood and expected duration of strikes.

serve firms' repayment priorities or timing, even when they know firms are financially distressed; firms may repay banks, suppliers, and other obligations before paying workers.

Therefore, I empirically test whether the arrival of CCDI inspection teams constitutes a new institutional shock that affects workers' political and economic uncertainty. Prior studies have used institutional shocks to examine how changes in uncertainty influence labor disputes. For example, Chau and Kanbur (2013), building on the framework of Card (1990), show that trade liberalization as an institutional shock can alter the dispersion of firm profitability and generate heterogeneous bargaining outcomes for workers. In my setting, inspections may shape workers' strike decisions by shifting their beliefs about government intervention and firms' ability to repay wages. Specifically, inspections can alter political and economic uncertainty through four main mechanisms:

1. *Change in firm–government connections*. This mechanism primarily affects political uncertainty, with secondary effects on economic uncertainty. It implies that, in the absence of inspections, workers in cities with stronger ties between firms and local officials expect the government to side with firms. As a result, workers believe that the government is unlikely to intervene on their behalf in labor disputes, which reduces firms' incentives to repay wages and ultimately lowers workers' expected likelihood of recovering wages through striking.

   The arrival of inspection teams reshapes workers' beliefs about government behavior. Workers update their beliefs along two dimensions: inspections increase political uncertainty by expanding the range of possible government responses, and they shift the expected level of government intervention toward more neutral or even worker-favorable outcomes. Workers may also update their beliefs about firms' repayment priorities and timing, as firms become less protected by the local government. If this mechanism is operative, cities with stronger pre-existing firm–government ties should experience a more pronounced surge in strikes following inspections than cities with weaker ties.

2. *Change in government attention*. This mechanism also primarily affects political uncertainty, with secondary effects on economic uncertainty. Without inspections, workers in cities where officials place greater priority on economic performance and social stability than on workers' grievances, regardless of officials' connections to firms, expect less government support than workers in cities where governments place higher priority on addressing labor grievances. Similar to the firm-government connection mechanism, workers believe that the government will tend to not intervene on their behalf, thereby lowering their expected likelihood of recovering wages through striking.

   The arrival of inspection teams reshapes workers' beliefs about government attention to labor grievances. In cities where officials previously paid little attention to wage disputes, inspections expand the range of possible government responses, for example by introducing direct oversight from inspection teams or by inducing local officials to respond more actively to workers' claims. This, in turn, also updates workers' beliefs on firm repayment priorities and timing. As a result, cities with historically low government attention to labor grievances should experience a larger increase in strikes following inspections than cities where officials were already more responsive.

3. *Change in economic conditions*. This mechanism operates through economic uncertainty. Inspections can disrupt local economies and firms' operations and profitability, generating uncertainty about local labor markets, firm cash flow, and repayment capacity. As a result, higher economic uncertainty may increase workers' perceived risk in the local labor market and of firms' nonpayment, thereby raising the likelihood of strikes.

4. *Change in other political factors*. The arrival of inspection teams can also affect political uncertainty through additional channels. For example, inspections may influence bureaucratic morale, official turnover, or public trust in local governments. These factors can shape workers' beliefs about the government's response to labor grievances, which in turn affect the expected payoff to striking.

Section 7 evaluates the plausibility of each channel discussed above. Multiple mechanisms may operate simultaneously when CCDI inspections occur, so the objective is not to isolate a single definitive channel but to assess which ones receive stronger empirical support. Some channels can be tested more directly given available data, while others are more speculative. The analysis therefore examines which mechanisms align most closely with the evidence and which lack sufficient empirical support.

4 Data

I compile a unique dataset combining monthly strike and corruption inspection data from 362 geographic units in China between 2011 and 2020, yielding 43,440 observations. Strike data come from the China Labour Bulletin (CLB) Strike Map, and inspection data come from the Corruption Investigation Dataset (CID). For simplicity, a *city* refers to a geographic unit in this study. The analysis is conducted at the city level because firm-level data are unavailable, which prevents linking strikes directly to specific firms.

The sample includes 362 geographic units: 333 prefecture-level cities, 4 direct-administered municipalities (Beijing, Shanghai, Chongqing, and Tianjin), 24 province-administered county-level regions, and 1 city, Laiwu in Shandong Province. The province-administered county-level regions are parallel to prefecture-level divisions and are included because both the CID and CLB record inspections and strikes in these jurisdictions, which cannot be assigned to any existing prefecture-level city. Their inclusion is for completeness, and excluding them does not affect the results qualitatively. Laiwu, which remained a prefecture-level city until it was merged into Jinan in 2019, is treated separately since the study period spans 2011 to 2020.

4.1 Primary Data

China Labour Bulletin Strike Map

The CLB Strike Map records daily strike incidents in cities from January 2011 onward. This study uses data from 2011 to 2020, comprising 13,170 incidents, and I manually verified each observation by reading the strike description and cross-checking the information online. These reports originate from posts on the social media platform *Weibo* or from news articles. Similar to Twitter, *Weibo* allows users to post short updates and interact with other users. In 2012, it had 503 million registered users, with 220 million making at least one post,[21] a sizable share of China's 567 million internet users that year.[22] Despite *Weibo*'s censorship (King et al., 2013), which is discussed later in this section, CLB still records roughly one-tenth of all strikes in China.[23]

CLB records information on strike causes, industries, firm type (private or state-owned enterprise (SOE)), scale, and outcomes. Strike causes include wage arrears, pay disputes, management issues, and working conditions. Industries include construction, manufacturing, transportation, services,[24] education, mining, and others. Strike scale is classified as small (1–100 participants), medium (101–1,000 participants), or large (1,001–10,000 participants), without exact headcounts. The data indicate whether a strike occurred in a private firm or an SOE, but do not identify specific companies. Outcomes include negotiation, government or union intervention, police presence, and police suppression, though not all strikes have a recorded outcome.

The CLB data have limitations. First, 66.6% of strikes have no recorded outcome, and even when an outcome is reported, there is little information on whether workers' welfare improved in either the short or long run.[25] As a result, I cannot assess how anti-corruption inspections affect workers' welfare using strike data alone, so I later complement the analysis with labor dispute data from China Judgement Online (CJO).

Second, the dataset does not include firm identifiers, making it impossible to link strikes to specific firms. This study therefore relies on city-level variation, which still provides meaningful insights because high-profile corruption inspections often trigger broader institutional responses that affect the local labor market as a whole.

Third, the CLB Strike Map captures only 5–10% of all strikes in China, with sampling rates that vary over time due to censorship.[26] This is not a major concern as long as censorship is uniform nationwide and unrelated to inspection timing. There is also no evidence that *Weibo*, operated by the private company *Sina*, allows local governments to influence censorship by region; King et al. (2013) show that censorship is topic-based rather than location-specific. Prior to my study, Campante et al. (2023) compared the CLB Strike Map with labor dispute data from the Ministry of Human Resources and Social Security (MOHRSS) and found similar strike patterns across the two datasets. This paper further addresses concerns about strike reporting bias in Section 7.5.

I further evaluate the representativeness of the CLB data by comparing it with labor disputes recorded on CJO. Established in July 2013, CJO archives all court cases and decisions in China. Although it provides comprehensive coverage of labor disputes, it cannot be used for the main analysis because its records begin only after the first wave of CCDI inspections in May 2013. Figure A1 compares monthly nationwide strikes recorded by CLB with labor disputes recorded by CJO from 2010 to 2020. From late 2013 to early 2016, the two series move closely together, aside from a brief CLB spike in December 2015 and January 2016. After 2016, CLB-recorded strikes de-

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

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

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

[24] The service industry includes all service-sector roles except education and transportation. Examples include workers in restaurants, salons, karaoke bars, fitness centers, and retail.

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

[26] According to CLB, the estimated 5–10% capture rate is based on "intermittent and partial statistics issued by national and local governments in China."

cline while CJO labor disputes continue to rise.[27] Because the analysis focuses on the aftermath of CCDI inspections, which occurred mainly between 2013 and 2016, the post-2016 divergence between CLB and CJO counts is unlikely to affect the main results. Moreover, the treatment effect patterns using CJO labor disputes and CLB strikes largely align, as shown in Section 6.5.

Table 1 presents a detailed summary of strike incidents from the CLB Strike Map. Construction and manufacturing account for the majority of all strikes, and most strikes occur in private enterprises rather than SOEs. The majority are small-scale, involving fewer than 100 participants, and wage arrears are the most common cause. Columns (1) to (4) summarize strike counts at the city-month level. Most cities experience fewer than one strike per month, with an average of 0.3 strikes per city per month from 2011 to 2020.

Table B2 reports the population-weighted number of strikes, showing that a city experiences on average 0.9 strikes per 10 million people each month. Because the China City Statistical Yearbook (CCSY) does not report population data for some smaller cities and ethnic autonomous regions, only 297 of the 362 cities are included in the population-weighted sample. Combined with the fact that the CLB does not capture all strikes, the per capita strike measure may be imprecise. For this reason, the analysis primarily focuses on the absolute number of strikes in each city.

Corruption Investigation Dataset

The Corruption Investigation Dataset (CID), compiled and cleaned by Wang and Dickson (2021), contains 18,947 corruption investigations conducted between 2012 and January 2017. Each record identifies an investigation involving a public official in a specific city and month, and includes the official's name, position, CCP rank, and the stated reason for investigation. All records were obtained from a public database managed by *Tencent*, meaning each corresponds to a publicly acknowledged corruption case.

The CCP rank ranges from 1 to 10, where 1 is the highest rank, indicating a national-level official, and 10 is the lowest, indicating a deputy office–level official, as defined by China's Civil Servant Law. This study classifies officials with ranks 1 to 6, that is, deputy bureau director level or higher and therefore at least city-level officials, as high-profile because they attracted both local and national media coverage and received nonzero mentions on *Weibo* during inspections. In contrast, officials ranked 7 or below are district-, town-, or county-level officials and receive few *Weibo* mentions when inspected. Table B1 provides further details and examples for each rank.

Table 2 summarizes corruption inspections by CCP rank and investigation reason, which include bribery, embezzlement of public funds, rule violations, and other categories. Only 7% of inspections targeted officials at or above the deputy bureau director level (ranks 1 to 6), with the remainder involving county-level officials or lower. Most investigations concerned bribery and embezzlement. Rule violations, which include breaches of law and CCP discipline, made up less than 20% of inspections, while the "other" category, covering negligence of duty and moral issues such as adultery, accounted for about 10%.

[27] The post-2017 divergence between CLB strikes and CJO labor disputes emerges two years after the first CCDI inspection wave ended and likely reflects different long-run equilibria: worker activism may have stabilized under the post-inspection political environment, while court-submitted cases followed a distinct trajectory. For instance, surveillance technology used to monitor protests may have expanded over years.

Table 1: Summary Statistics on Strikes, 2010-2020

	City-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
City-Month Observations				43,440		
Number of Cities				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 city-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 across the entire sample, rather than on a city-month basis.

Figure 3, panel (a), shows the correlation between the timing of corruption inspections and strike incidents from 2011 to 2020. Corruption inspections rose sharply beginning in 2014 and peaked at roughly 800 to 1,000 cases in 2015. Strike incidents also exhibit periodic peaks, with a pronounced surge in late 2015 and early 2016, occurring a few months after the inspection peak. After 2016, inspections declined to nearly zero, while strikes continued to follow a seasonal pattern. As shown in panel (b), these seasonal peaks are driven mainly by wage arrears in the construction industry, where most strikes occur when workers demand payment at the end of project cycles.[28]

4.2 Complementary Data

China Judgements Online

I use labor dispute data from China Judgements Online (CJO) to complement the CLB Strike Map. CJO is a public database that contains extensive court decision records in China. Launched in July

[28] According to Pringle (2002), construction workers typically work for a year on a project and receive payment at the end. If the project underperforms or for other reasons, they may not be paid. As a result, 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.

2013, it mandated the upload of all court decisions beginning in November 2013 and became fully comprehensive by June 2015. The CJO data include more than 330,270 distinct labor dispute cases from July 2013 to 2020. CJO is not used for the main analysis because its data begin only after 2013, leaving too few pre-inspection observations. Instead, it is used to assess the representativeness of the CLB data and to test the robustness of the baseline results.

China City Statistical Yearbook

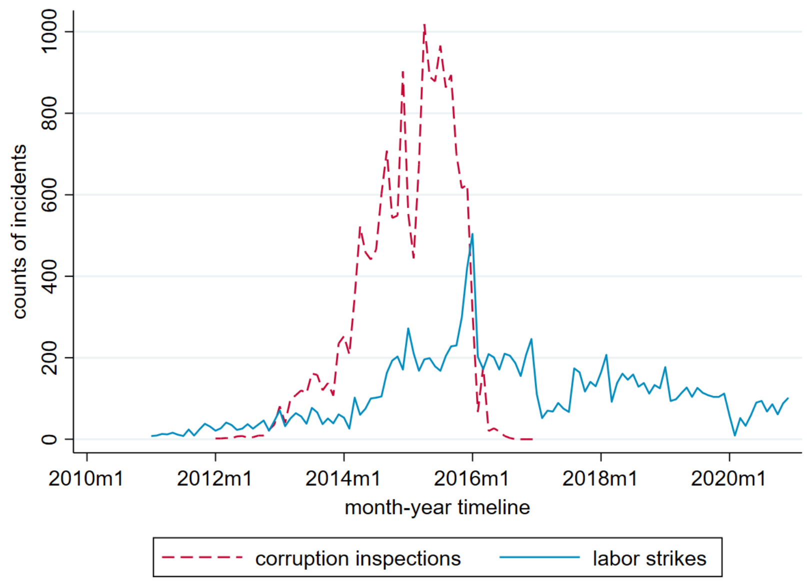
The China City Statistical Yearbook (CCSY) provides annual data on city characteristics such as GDP, population, employment, and wages from 2011 to 2020. It is used primarily for city-level control variables and for validating the identification assumption in Section 5.3. Its annual frequency limits its ability to capture immediate monthly fluctuations around inspections. Some less populated or remote cities lack data or have inconsistent observations over time; for example, only 297 of the 362 cities have consistent population records. Despite these limitations, the CCSY remains the most comprehensive source of city-level attributes in China.

Social Media

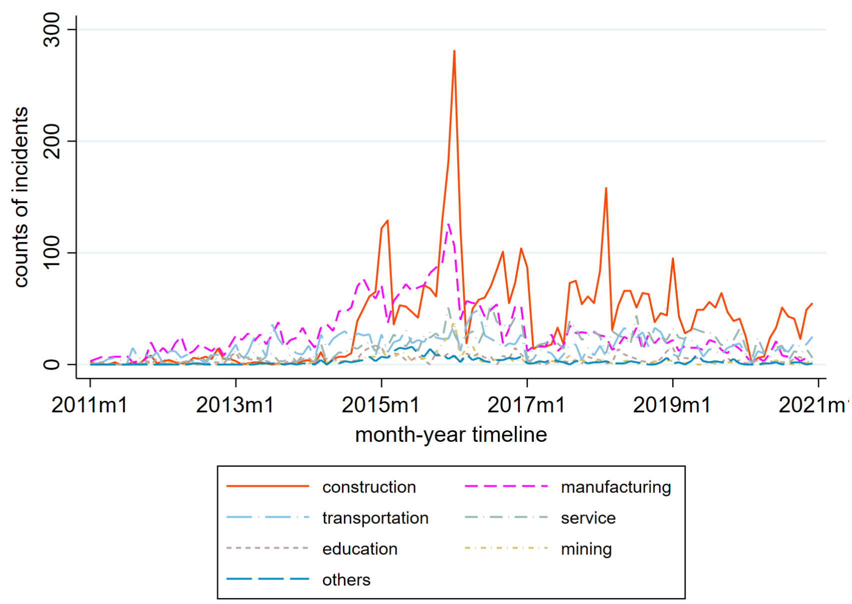
I scraped 53,197 *Weibo* posts containing the names and positions of high-profile officials from the CID records. Posts related to low-profile officials, which had few or no social media mentions, were excluded. For each post, I recorded the content, location, date, and interaction statistics, along with the sender's account details, follower counts, and any available demographic information. Each account was classified as either public (such as local news outlets or local government accounts) or private (individual users). Public accounts generated 44.6% of all posts, with the remainder coming from private users.

Figure 3: 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 differences in strike outcomes across cities with varying migrant worker networks prior to the anti-corruption campaign. The CMDS provides information on 153,521 migrant workers surveyed in 2011 using a probability-proportional-to-size sampling method.[29] It includes detailed variables on migrant workers' demographics, migration histories, living conditions, and quality of life.

Additional Data

Beyond the datasets described above, I also draw on additional sources, including the National Tax Survey, the land transaction data from Chen and Kung (2019), the Global Database of Events, Language, and Tone (GDELT), and the China Stock Market & Accounting Research (CSMAR) Database. Further details on these datasets are provided in Section 7, where the potential channels are examined.

5 Empirical Strategy

I conduct two event studies to examine the relationship between anti-corruption inspections and strikes. The first event study captures the immediate, or *first-stage*, effects of the campaign by tracking the rise in corruption inspections in a city after the arrival of the CCDI inspection team. The second event study, referred to as the *baseline* effect, assesses whether strikes increase after the CCDI team begins conducting high-profile inspections. As defined earlier, high-profile inspections target officials above the deputy bureau director rank.

The logic for using two event studies is as follows. The *first stage* verifies that inspection activity increased after the CCDI team arrived in a province, confirming that the campaign was active on the ground. This provides a basis for interpreting changes in workers' strike behavior in the *baseline* results. The key difference between the two studies is the definition of the event: the first stage uses the first inspection recorded in a city after the CCDI's provincial arrival, which may not have attracted public attention, while the baseline uses the first high-profile inspection in the city, which plausibly attracted public attention, informed workers, and could influence their decision to strike.

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

$$\textit{outcomes}_{cm} = \sum_{k=\underline{k}}^{-2} \lambda_k \eta_{cm}^k + \sum_{k=0}^{\bar{k}} \lambda_k \eta_{cm}^k + \boldsymbol{X}_{cy}\beta + \alpha_c + \alpha_m + \varepsilon_{cm} \tag{1}$$

where $\textit{outcomes}_{cm}$ 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,

[29] The sampling followed a probability-proportional-to-size design, with sample sizes proportional to provincial migration inflows in the first quarter of 2011 and further stratified by prefecture, county, and residential district.

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.

To ensure the robustness of the primary specification, I also apply the estimators of Callaway and Sant'Anna (2021) and Gardner (2021), as reported in Section 6.5. In addition, I estimate alternative models, including an inverse hyperbolic sine (IHS) transformation of the outcome and a zero-inflated negative binomial (ZINB) model, to account for the count nature of strike incidents and the large number of zeros across cities. All approaches produce patterns consistent with the main results.

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

$$outcomes_{cq} = \delta \times post\ event_{cq} + \boldsymbol{X}_{cy}\beta + \alpha_c + \alpha_q + \varepsilon_{cq} \tag{2}$$

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 0 before, and is 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 applies the Callaway and Sant'Anna (2021) estimator to ensure that all treated cities have a valid control group. Unlike the graphical depiction of dynamic outcome changes in Equation (1), Equation (2) presents the estimated parameters in a concise format.

The key identification assumption for the baseline is that, in the absence of inspections, strike trends would have evolved similarly across cities with different inspection dates. Section 5.3 tests for correlations between inspection timing and various city-level characteristics, and also examines potential anticipation effects and the comparability of cities inspected early, late, or not at all. Before that, I clarify how the first-stage and baseline analyses differ in event definitions, outcome variables, and the construction of treatment and control groups.

5.1 First Stage

The first event study serves as a sanity check, examining whether the arrival of a CCDI inspection team increases city-level corruption inspections. Outcomes include the number of total inspections, high-profile inspections, and *Weibo* posts discussing these high-profile cases. The event is defined as the first corruption inspection in a city after the CCDI team arrives in the province. Because CCDI inspection teams were deployed widely, all but 4 cities experienced an inspection. Treated cities are those inspected earlier, while the control group consists of cities inspected later and the four cities that were never inspected.

Figure 4, panels (a) and (b), show the timing of the first inspection in cities in two provinces, Hunan and Anhui, where the CCDI team arrived in November 2013. These provinces are shown for illustration, and the same pattern applies elsewhere. In Hunan, the first inspection occurred in Yongzhou in November, with other cities receiving their first inspections between December 2013 and June 2014. In Anhui, Anqing, Hefei, Wuhu, and Xuancheng were inspected in November 2013, followed by other cities from December 2013 to July 2014. This visualization shows that a city's first inspection can occur immediately or several months after the CCDI team arrives in the province.

Broader national patterns are shown in Figure A2, panel (a). The timing of the first inspection after the CCDI team's provincial arrival varied widely across cities, ranging from May 2013 to August 2015 and spanning 26 distinct treatment cohorts. Notably, of the 362 cities and autonomous regions, 4 experienced no inspections between 2012 and 2017 despite the CCDI team's presence in their province.

5.2 Baseline

The baseline specification analyzes changes in strikes before and after the first high-profile inspection in a city, conditional on the CCDI team's arrival in the province. Outcomes include the number of strikes and strikes per 10 million people. Appendix D also examines the extensive margin using a binary indicator equal to 1 if a strike occurs.

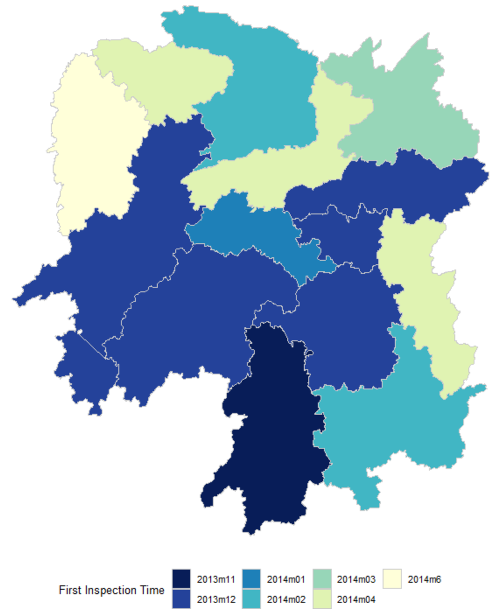
Using high-profile inspections as the baseline event is guided by institutional considerations. Intuitively, this suggests that workers observe media reports of high-profile inspections, interpret them as signs of institutional change, and may decide to strike in response. Several factors support this view. First, investigations targeting officials at rank 6 or above correspond to prefecture-level leadership, which aligns with the city-level focus of the empirical analysis, while inspections below this rank involve county or district officials and affect only sub-city jurisdictions. Second, *Weibo* data show that inspections below rank 6 generate little or no media attention, whereas high-profile investigations consistently attract public discussion and are therefore more likely to be visible to workers. Third, because these inspections are both citywide and salient, they signal a meaningful shift in the local political environment and can plausibly affect workers' expected returns to striking. Finally, of the 362 cities in the sample, 128 never experienced a high-profile inspection, allowing a clear comparison between cities treated earlier and those treated later or not at all.

Although using the first high-profile inspection as the event has the advantages discussed above, it is important to note that it is not the only inspection that may affect strikes. Of the 234 cities that experienced a high-profile inspection during the campaign, nearly 60% had more than one. Even so, cities are classified as treated from the date of their first high-profile inspection onward. I use the first high-profile inspection as the treatment because it provides the cleanest shock, marking the point at which the campaign first became locally visible. However, the accumulation of inspections over time may also shape strike activity, so the first event should not be viewed as the only relevant treatment.

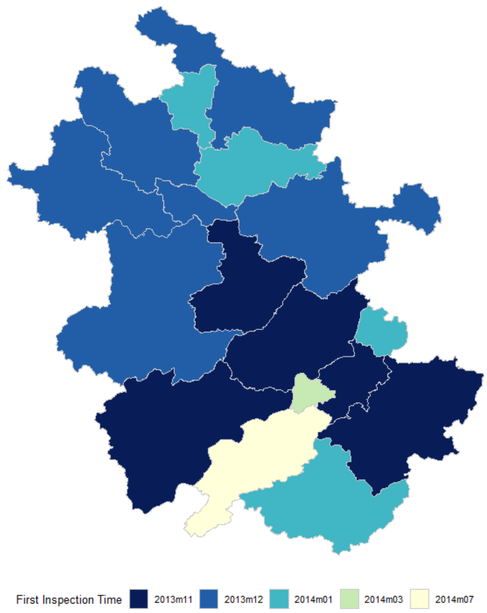
For this reason, I use alternative events to test whether workers' strike behavior responds to different triggers. Appendix C compares four alternatives: (a) the baseline event; (b) the CCDI team's arrival to the province; (c) the first inspection of any rank in a city after CCDI arrival; and (d) the first high-profile inspection in a city after the national anti-corruption campaign announcement in November 2012, regardless of CCDI arrival. Across these definitions, the baseline event provides the cleanest and most reasonable outcome. In Section 6.2, I also show that cities with more accumulated inspections experience more strikes, reinforcing the interpretation that additional inspections can further influence workers' behavior.

Figure 4: Timing of First Corruption Inspections & High Profile Inspections

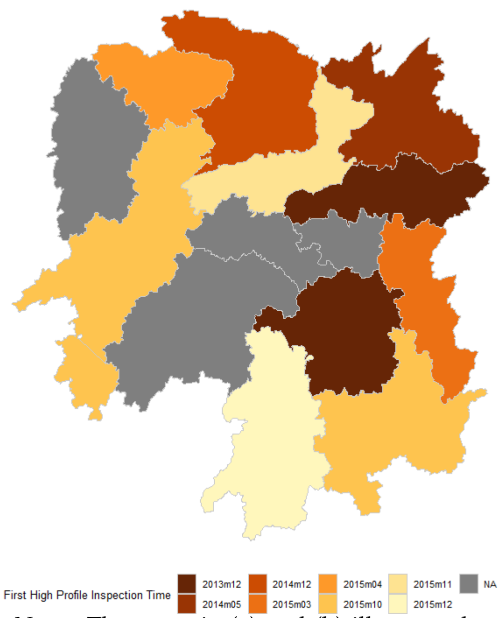
(a) First Inspections, Hunan Province



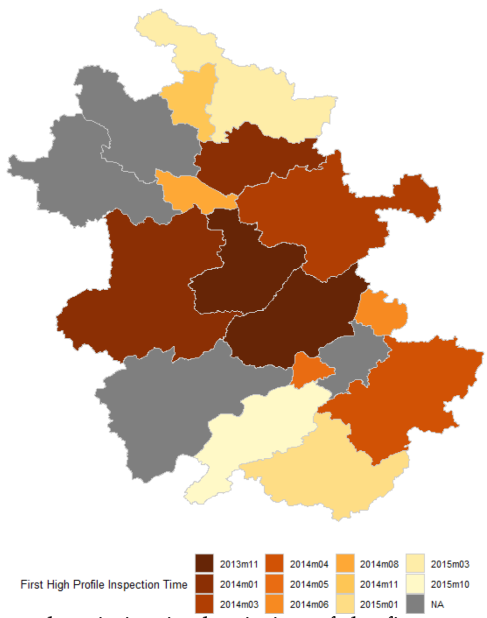
(b) First Inspections, Anhui Province



(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.

Figure 4, panels (c) and (d), show the timing of the first high-profile inspections across cities in Hunan and Anhui after the CCDI team's arrival. Unlike the timing of the first inspection, the first high-profile inspection occurs within the same month in a few cities but in most cases appears several months later. In Hunan, 4 of 14 cities never experienced a high-profile inspection, and in Anhui, 4 of 16 prefectures did not experience one.

Figure A2, panel (b), shows the timing of the first high-profile inspection across all cities nationwide, covering 28 treatment cohorts. Compared with the first-inspection map, this figure produces a larger control group because several cities in each province never received a high-profile inspection. Although these cities may be less populated or more remote, excluding them does not alter the results qualitatively, and including them is beneficial because it provides a larger control group.

5.3 Identification Assumption

The main concern of the empirical strategy is whether the sequencing of CCDI inspection waves is exogenous. The baseline event study assumes that, in the absence of inspections, cities with different inspection timings would have followed the same trend in strikes over time. However, this does not require inspection timings to be random; it only requires that their timing is not correlated with other factors that influence strike activity.

The sequencing of CCDI inspections has been discussed in the literature. Existing studies agree that the timing and coverage of inspections were centrally determined and not disclosed in advance to provincial or city officials (Wang and Dickson, 2021; Fang, 2024). Other empirical studies also find little evidence that provincial or city characteristics predicted inspection timing (Hong, 2024), supporting the view that these events can be treated as plausibly exogenous shocks to local governments and firms.

Two scenarios could potentially violate this identification assumption. First, CCDI may schedule inspections based on a province's or city's economic or political conditions, which could also relate to strike activity. For example, if inspection timing depends on unemployment levels or prior strike counts, the estimated effects may be biased. Second, workers or local governments may anticipate inspection timing; for instance, workers might time strikes to coincide with high-profile inspections. A direct check for both scenarios is to examine pre-trends in the main results, and there is no evidence of such pre-trends, although further discussion is still necessary.

To address the first scenario, I regress inspection timings on a series of pre-campaign city characteristics using 2011 data. Table B3 shows that most characteristics are insignificant, except for RNI, which is significant only for the timing of the first inspection of any rank. I therefore control for RNI in the main analysis, and the results remain qualitatively unchanged. Moreover, inspection timing is uncorrelated with the number of strikes prior to the inspections.

Regarding the possibility that workers anticipate inspections, I mitigate this concern by controlling for the number of inspections in neighboring cities, which helps limit cross-city anticipation effects. Including or excluding this control does not change the results qualitatively, as shown in Section 6.2. I also adopt a moderate assumption by requiring no anticipation only before a city experiences its first high-profile investigation either before or within the first two quarters after the CCDI team's provincial arrival. In other words, workers would have had little basis for anticipation before and during the early waves of events. After this period, workers in not-yet-inspected cities may reasonably adjust their strike decisions due to spillover effects, as discussed in Section 6.4.

A final concern is that an unobserved shock may have triggered both the first wave of inspections and the rise in labor strikes. If such a shock existed, it would be difficult to explain why strikes increase only after inspections, rather than simultaneously. As shown in Figure 3, the rise in strikes occurs roughly one year after the initial increase in inspections. Moreover, as shown later, strikes respond more promptly to the first high-profile inspections, whereas using any-rank inspections as the treatment produces a delayed increase in strikes of about one year. Therefore, although I cannot completely dismiss the possibility of such a shock, it is difficult to reconcile why it would affect inspections and strikes at such different times.

6 Main Results

6.1 First Stage Outcomes

Figure 5, panel (a), shows an immediate rise in a city's monthly inspections following the first inspection after the CCDI's provincial arrival. Within one year, monthly inspections tripled, and within two years, they increased six-fold. Panel (b) shows that high-profile inspections, a subset of total inspections, rose nearly four-fold within six months of the first inspection. Although the pace of increase slowed after six months for both measures, neither declined until two years after the initial inspection. Overall, total and high-profile inspections increased about three-fold per quarter after the CCDI's first inspection, as reported in Table 3, columns (1) and (2).

The high-profile inspections received significant public attention on *Weibo*. Figure 5, 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).

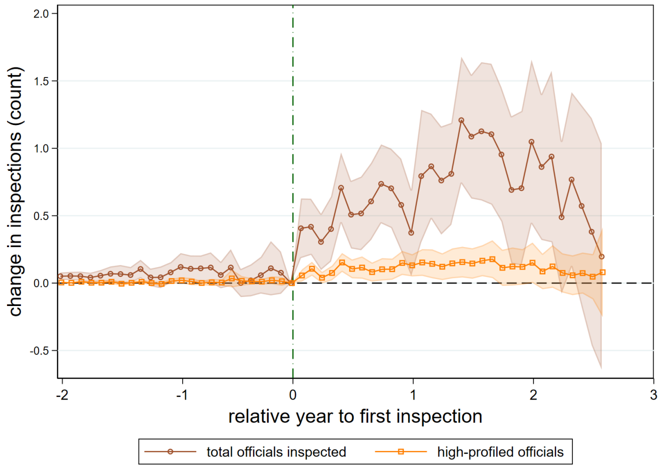
6.2 Baseline Outcomes: Total Strikes

Figure 6 shows an immediate rise in monthly strikes following the first high-profile inspection in a city after the CCDI team's provincial arrival. Before the inspection, strike patterns were stable and statistically insignificant, indicating similar strike trends across cities and a low likelihood of anticipation. One year after the inspection, the monthly strike average doubled, increasing by 0.2 from a pre-inspection mean of 0.16. Within two years, strikes tripled, after which they gradually returned to slightly above pre-inspection levels.

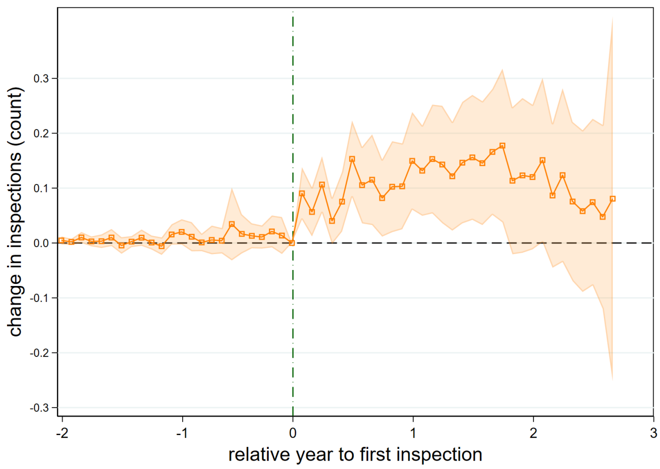
Table 3 shows that, on average, the quarterly number of strikes in a city doubled after the first high-profile inspection. This result holds across all specifications, including those that split the sample into 2011–2016 for short-term effects and 2011–2020 for overall effects, and those that control for city characteristics and neighboring inspections, as reported in columns (5)–(6) and

Figure 5: First Stage Outcomes

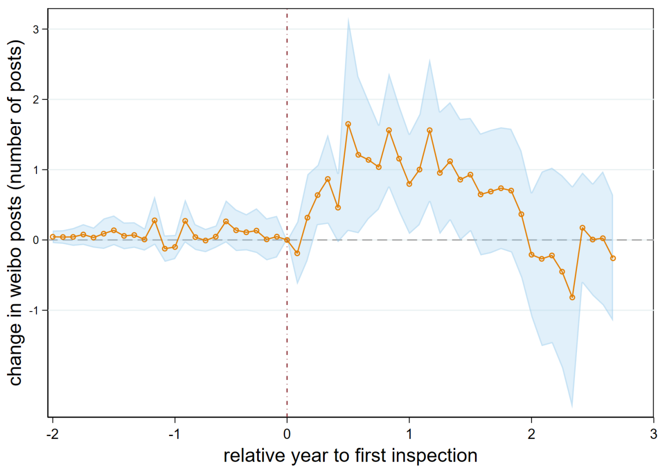
(a) Total & High-Profile Inspections



(b) High-Profile Inspections



(c) *Weibo* Posts



Notes: The three graphs demonstrate the estimated λ_k from Equation (1). 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.

Table 3: First Stage and Baseline Outcomes of Inspections

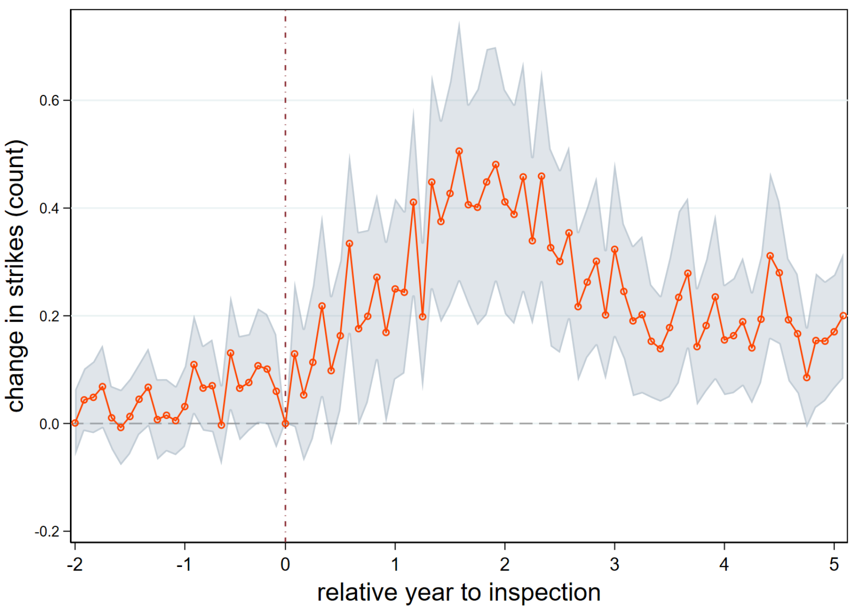
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	First Stage			Baseline							
	Total Insp.	HP Insp.	Weibo Posts	*Short Term*				*Long Term*			
				Strike Incidents			Strikes per 10M ppl	Strike Incidents			Strikes per 10M ppl
Post 1st Insp.	1.21***	0.11***	4.65***								
	(0.30)	(0.04)	(1.54)								
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 (2) 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 (11). 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 $p < 0.01$.

(8)–(10). Without these controls, the estimated short-term effect is even larger, nearly doubling once more, as shown in column (4).

To examine population-weighted outcomes, Table 3, column (7), shows a short-term increase of about 70% in strikes per 10 million people following an inspection. In the long term, the effect remains positive but becomes statistically insignificant in column (11). Two factors likely contribute to this. First, using CCSY population data excludes many less populated cities that may have meaningful strike responses, since only 297 of 362 cities have observations. Second, strike activity often shifts within months after a high-profile inspection, so combining monthly strike variation with slow-moving annual population data may obscure these dynamics. Nevertheless, the main event-study pattern remains clear: strikes rise sharply in the months after high-profile inspections.

Figure 6: Baseline Outcomes



Notes: The graph demonstrates the estimated λ_k from Equation (1), 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.

A follow-up question is whether strikes respond more strongly to cities with more high-profile inspections or to those where higher-ranking officials were inspected. To assess this heterogeneity, I examine variation across cities with different characteristics of high-profile inspections. Figure A3 reports estimates by (i) the total number of high-profile inspections a city experienced and (ii) the bureaucratic rank of the first inspected official, which in the sample includes rank 6 (deputy bureau director), rank 5 (bureau director), and rank 4 (sub-provincial or ministerial); no city in the sample had an inspected official above rank 4. Panels (a)–(c) present results by the total number of high-profile inspections: cities with only one inspection (about the 25th percentile) show little change, whereas those with five (around the median) or more show a clear and persistent rise in strike activity.

Panels (d)–(f) present results by the rank of the first high-profile inspected official. Cities whose first inspection targeted a rank 6 official show the clearest and most stable effects, while those with rank 4 or rank 5 officials display noisier patterns, which is plausible given that only four cities experienced a rank 4 inspection, forty-two experienced rank 5, and the others were at rank 6. Even so, the magnitude of the effects for rank 5 inspections exceeds that for rank 6. Overall, these patterns indicate that both the scale and the hierarchical prominence of inspections intensify strike activity.

6.3 Baseline Outcomes: Strike Characteristics

The CLB Strike Map's granular data allows a detailed examination of strike types and the industries involved. Figure 7, panels (a)–(f), show that the construction sector experienced the largest increase, followed by manufacturing, while the service, transportation, mining, and education sectors saw little change. Panels (g) and (h) indicate that the surge in strikes is driven almost entirely by wage arrears rather than demands for higher pay, consistent with evidence that more than 70% of labor disputes in China involve wage arrears (Lee, 2004). Panels (i) and (j) further show that this rise is concentrated in private firms rather than SOEs.

These results suggest that construction and manufacturing workers face greater grievances, consistent with the literature on China's labor market (Ngai, 2005). These workers often struggle to obtain promised wages, partly due to the lack of valid labor contracts and limited legal awareness (Chan, 2001; Cheng et al., 2015). As a result, they are more likely to strike as a last resort to voice their grievances (Lee, 2007).

A closer look at strike size and form, shown in Figure 7, panels (m)–(o), shows that most strikes were small-scale, involving fewer than 100 participants and typically taking the form of protests or sit-ins. This pattern aligns with evidence that Chinese workers often rely on informal, small-group actions to voice grievances rather than on unions (Chan and Hui, 2014). The limited response among larger strikes is also consistent with the fact that Chinese censors target content related to collective action, which may reduce the visibility of large-scale events (King et al., 2013). Even so, there was a notable rise in medium-scale strikes involving 101 to 1,000 participants.

There is limited evidence on what these strikes achieved. Only about one third of the strikes in the CLB data have recorded outcomes, most of which involve police presence, as shown in Figure A4. This does not imply that police disbanded strikes through arrests or violence, but simply indicates their presence at the scene. There is no discernible change in the frequency of peaceful negotiations or violent incidents after inspections.

6.4 Baseline Outcomes: Strike Network and Spillovers

It is reasonable to assume that workers could not anticipate the first waves of high-profile inspections because they were conducted by the CCDI without prior disclosure. However, after several waves of inspections, especially in nearby cities, workers may begin to strike preemptively through network or spillover effects. For instance, if city A experiences a post-inspection surge in strikes, workers in city B, which has not yet been inspected, may adjust their strike decisions in response.

There are three main approaches to studying strike networks. First, how do differences in the timing of a city's first high-profile inspection affect workers' decisions to strike? Second, does an increase in strikes in one city influence strike activity in neighboring cities? Third, how does the size of the migrant network shape the number of strikes within a city?

Inspection Timing

I begin by examining whether staggered inspection timing differentially affects cities inspected earlier versus later. Workers in cities that have not yet experienced a high-profile inspection may update their strike decisions after observing strike activity in an inspected city within the same province. This spillover or network effect is not an anticipation effect. Anticipation implies that workers strike even before the first wave of inspections takes place. On the other hand, network effects arise only when workers in not-yet-inspected cities respond to strikes that occur after other cities in the same province or nearby areas are inspected.

To study this, I divide cities within each province into four waves based on treatment timing. First-wave cities experienced their first high-profile inspection within 0 to 1 quarter of the CCDI team's provincial arrival. The second wave was inspected 2 to 3 quarters after arrival, the third wave 4 to 5 quarters after, and the fourth wave more than 6 quarters later.

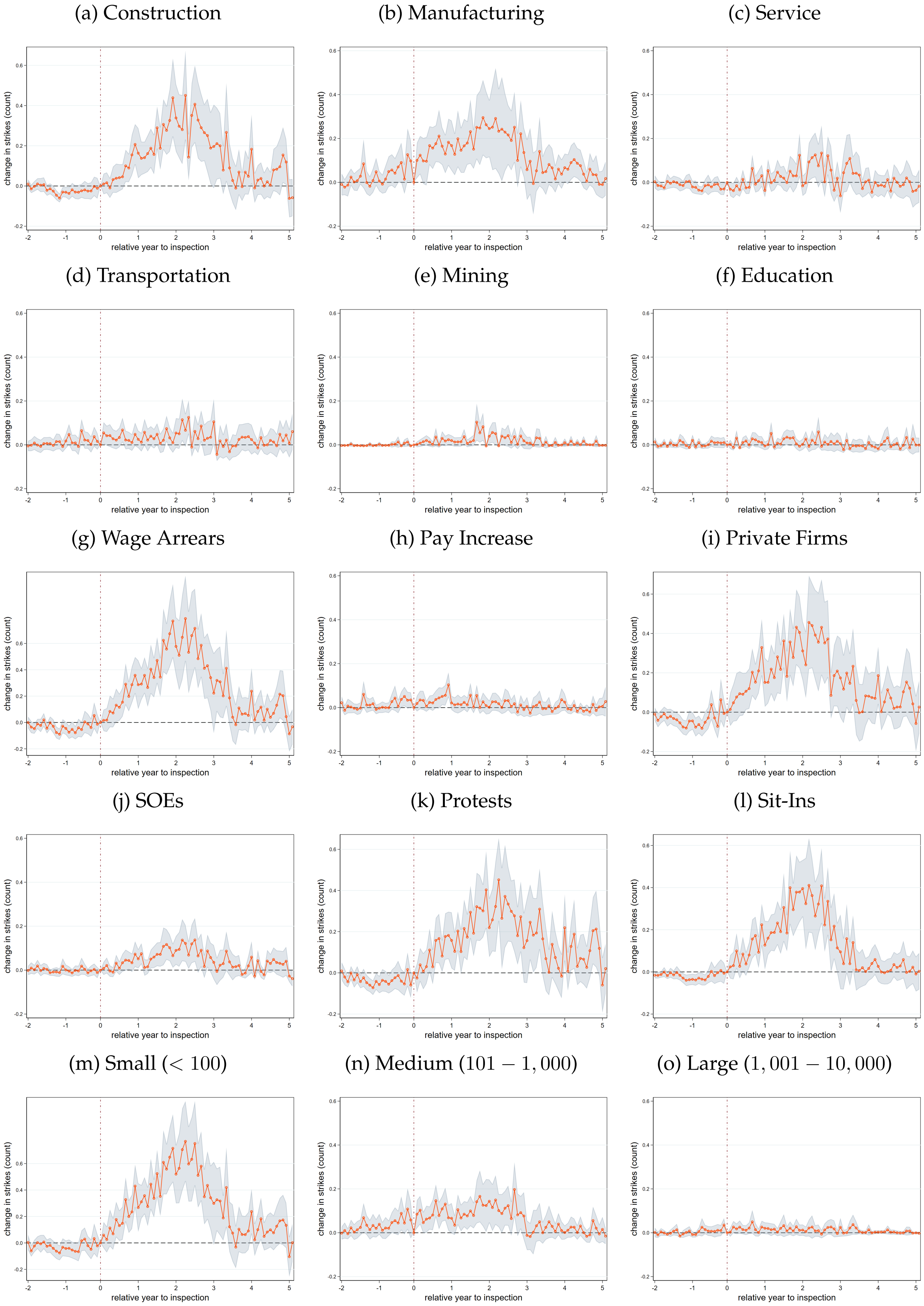
Specifically, I examine two aspects. First, I estimate baseline treatment effects separately for each wave. The event is a city's first high-profile inspection, with the treatment group defined by cities in each wave and the control group consisting of cities that never experience a high-profile inspection. Second, I study how inspections in earlier-wave cities affect strike activity in cities that have not yet been inspected. In this case, the event is the first inspection in an early-wave city, and the outcome is the strike count in later-wave cities. The treatment group is later-wave cities, and the control group remains the never-inspected cities.

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 may reflect workers in these cities striking earlier than their own inspection times in response to inspections and strike activity in earlier-wave cities within the same province. Columns (6) and (7) of Table 4 show that third- and fourth-wave cities began striking when second-wave cities were inspected. This pattern is unlikely to be driven by anticipation, since inspections in neighboring cities are controlled for, and if anticipation were the main channel, later-wave cities would have shown increased strikes during the first-wave inspections, which is not supported by columns (2) and (3).

It is important to note that the presence of network effects implies that even the control group, consisting of cities that never underwent high-profile inspections, may have experienced strike increases when early-wave cities were inspected, which reduces the estimated effect size for later-wave cities. Thus, the rise in strikes in later-wave cities should be viewed as a lower bound. This does not violate the identification assumption that cities would follow the same strike trend in the absence of high-profile inspections. Later-wave and control cities are only affected through

Figure 7: Heterogeneous Effects Across Strike Characteristics



Notes: The series of graphs show the estimated λ_k from Equation (1) using strike observations categorized by different characteristics, including industry, reason for the strike, type of firm involved, form (protest or sit-in), and size (small, medium, or large). 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.

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 (2) 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 $p < 0.01$.

spillovers, and without inspections in first-wave cities, neither group would exhibit any increase in strike activity.

Neighboring Cities

The previous section shows that strikes can spill over to cities that have not yet undergone high-profile inspections, influenced by earlier-inspected cities within the same province. A related question is whether similar effects arise in neighboring cities that share a border with an inspected city. The evidence is consistent with this pattern. As shown in Table B4, the largest increase in strikes occurred in neighboring not-yet-inspected cities when the inspected city received its first high-profile inspection 2 to 3 quarters after the CCDI team's provincial arrival. In contrast, inspections within 0 to 1 quarter did not lead to a significant rise in neighboring cities, again indicating the presence of network spillovers rather than anticipation.

Migrant Workers

Finally, I examine whether the increase in strikes is concentrated among migrant workers who share similar traits and are likely to interact with one another, as suggested by Chan and Ngai (2009) and Xu and Palmer (2011). I test whether cities with different sizes of migrant worker networks experienced different changes in strike activity. The migrant network measure is constructed using the CMDS. A migrant is coded as connected if their primary social ties[30] are with *Laoxiang*, meaning fellow migrants from their original city who now work in the same destination city. I calculate the proportion of such connected migrants in each city in 2012, prior to the anti-corruption campaign, to avoid capturing any inspection-induced shifts in migrant behavior.

Table B5 shows that cities with stronger pre-existing migrant worker networks experienced a significant rise in strikes, particularly where more than 25% of migrant workers had *Laoxiang* connections in 2012. One concern is that these workers may have faced integration difficulties in destination cities, leading to isolation and a greater likelihood of striking. To address this, I control for the share of migrants reporting unhappiness or feelings of local exclusion, which does not change the results, as shown in columns (2), (5), and (8). I also account for the share of inner-province migrants, since migrants from nearby cities may have networks similar to locals, but this adjustment likewise leaves the results unchanged, as shown in columns (3), (6), and (9).

6.5 Robustness

Data Selection

One major concern is that the CLB Strike Map may disproportionately capture labor strikes after high-profile inspections, causing the estimated treatment effects to reflect CLB reporting patterns rather than actual changes in workers' strike behavior. To address this, I use CJO labor dispute court cases as an alternative measure of labor conflict. Because CJO only began recording cases in

[30] The survey asked, "Who do you interact with most frequently during your leisure time (excluding customers)?" with options: (1) *Laoxiang* with household registration in their hometown, (2) *Laoxiang* with household registration in the current location, (3) other local residents, (4) other non-local residents, and (5) rarely interact with others. Responses 1 and 2 indicate that a migrant's primary social connection is with *Laoxiang*.

July 2013, these data cannot be used in the baseline analysis due to limited pre-treatment observations. However, I can still use the post–July 2013 sample to compare with the baseline results.

Figure A5, which estimates Equation (1) using the CJO data, shows a dynamic pattern that closely mirrors the CLB results in Figure 6. This similarity is particularly compelling given the different long-term trends in CLB and CJO data shown in Figure A1, providing further evidence that the treatment effect reflects real changes in worker behavior rather than data-collection patterns. Table B6 further reports the treatment effects for wage arrears, insurance, and work accident disputes in columns (1) to (4). Overall, labor disputes increased by about 50% after a city's first high-profile inspection. Wage arrears show the largest rise at just over 50%, followed by insurance and work accident disputes, which increased by roughly 30% and 25% respectively.

To further verify that the observed increase in strikes was not simply driven by the surge in CLB data collection in 2015 and 2016, I conduct a placebo test by randomly reassigning the treatment status and timing of the first high-profile inspection for each city. If the baseline effects were driven solely by increased CLB reporting during this period, we would still observe a rise in strikes after the reshuffled treatment times, which mostly fall in 2013 and 2014. However, after 100 simulations of randomly assigning treatment status and timing, the resulting distribution in Figure A6 centers around zero, in sharp contrast to the baseline treatment effect of 0.4. This indicates that the observed rise in strikes is not driven by CLB data-collection patterns.

Alternative Event Definitions

As discussed in Section 5.2, I explore which event timing is most closely associated with the rise in strikes. In Appendix C, I use alternative definitions of the baseline event to test whether the CCDI team's provincial arrival, the announcement of high-profile inspections, or both shaped workers' strike behavior. I compare four events: (a) the first high-profile inspection in a city after the CCDI team's provincial arrival (baseline); (b) the CCDI team's arrival to the province; (c) the first inspection of any rank in a city after CCDI arrival (first stage); and (d) the first high-profile inspection in a city after the national anti-corruption campaign announcement in November 2012, regardless of CCDI arrival.

Across these alternative definitions, the rise in strike activity is most immediate and persistent when the event is defined as the first high-profile inspection following CCDI arrival. Under the other definitions, the responses are more delayed or statistically insignificant. This pattern suggests that the baseline event captures both *local salience* and *credibility*: the high-profile inspection announcement is highly visible to workers, and the CCDI team's presence signals genuine central oversight, together prompting an immediate and sustained behavioral response.

Alternative Specifications

I include additional time-varying controls because the CCDI team's arrival may act as a province-wide shock that affects strike activity in all cities. Province-level changes such as leadership turnover or new regulations, as well as city-specific trends, may also influence the estimates. To address these concerns, I re-estimate Equation (1) using alternative specifications with stricter controls, shown in Figure A7. Panel (a) adds province-year fixed effects; panel (b) replaces them with province-time fixed effects; panel (c) includes city-specific linear trends to capture heterogeneous pre-trends; and panel (d) adds both city trends and province-time fixed effects. These controls help ensure that the estimated treatment effects are not driven by province-wide shocks or underlying local dynamics.

Across all specifications, the estimated treatment effects remain robust. The magnitude and timing of the effects closely match the baseline, indicating that the results are not driven by province-level shocks or city-specific confounders.

Alternative Estimators

Recent research on difference-in-differences with staggered treatments highlights the importance of comparing early-treated groups to later- or never-treated groups, since treatment effects may be biased if these groups exhibit different trends over time. While the main analysis uses the estimator in Callaway and Sant'Anna (2021) to address this concern, I also apply the estimator in Gardner (2021) to assess robustness. If the treatment effects are valid, the results from both methods should be broadly similar.

A comparison of strike dynamics using the estimators in Callaway and Sant'Anna (2021) and Gardner (2021), shown in Figure A8, indicates that both produce similar results. If anything, the Gardner (2021) estimates show roughly a 20% larger increase in strikes and smaller standard errors. Consistent with this, Table B6, column (5), reports almost a threefold rise in strikes after the first high-profile inspection when using the Gardner (2021) estimator, a larger effect than that obtained with the Callaway and Sant'Anna (2021) approach.

I further evaluate the choice of estimator given the nature of the strike data. Many cities report zero strikes in certain months or quarters, creating a highly non-normal distribution. To address this without excluding zero-strike cities, I apply an inverse hyperbolic sine (IHS) transformation to the strike counts and re-estimate the baseline. As shown in Table B6, column (6), strikes increase by about 15% after inspections under the IHS specification, smaller than the baseline effect. I also estimate a zero-inflated negative binomial model to address overdispersion in strike counts, following Equation (2). Column (7) shows that this specification yields results nearly identical to the baseline.

Active Cities

I further tighten the sample to ensure that the rise in strikes is not driven disproportionately by a few high-strike cities. The CLB Strike Map shows that the seven cities with the highest strike intensity are all located in Guangdong, a region widely known as the "world's factory."[31] To confirm that the overall surge is not driven by these cities, I exclude Guangdong and re-estimate the model. The results in Table B6, column (8), remain consistent with the baseline. Similar results are obtained when excluding China's four major metropolitan cities—Beijing, Shanghai, Guangzhou, and Shenzhen—as shown in column (9).

[31] Zheping Huang, "China Has Seen a 13-fold Increase in Labor Strikes and Protests Since 2011—and It's Cracking Down," *Quartz*, 2015.

7 Mechanisms

Having established the baseline effects of CCDI inspections on strike activity in Section 6, I now examine the mechanisms that may explain these results. The conceptual framework in Section 3 suggests that inspections can influence strikes by reshaping the political and economic environment in which workers, firms, and local governments interact. To evaluate these channels, I assess whether the evidence supports each proposed mechanism: (1) disruption of firm–government connections, (2) shifts in government attention, (3) changes in local economic conditions, and (4) other institutional factors. The goal is not to isolate a single dominant channel but to identify which mechanisms receive stronger empirical support and which appear less plausible. Multiple mechanisms may operate simultaneously, and the relative strength of each is what this section aims to clarify.

7.1 Firm–Government Connections

The first mechanism examines how CCDI inspections disrupt or weaken connections between firms and local governments. As outlined in Section 3, inspections can undermine the political protection that connected firms previously relied on. For workers, these shifts alter perceptions of firm–government relations and may prompt a reassessment of the expected returns to striking.

A key empirical challenge for evaluating this mechanism is the lack of firm-level data that would allow strikes to be matched to specific firms. As a result, this study cannot directly assess whether the anti-corruption campaign reduced firm–government connections at the firm level. Although prior research shows that the campaign weakened political ties between firms and officials (Chen and Kung, 2019; Chu et al., 2019; Ding et al., 2020; Hao et al., 2020; Xu et al., 2021), the analysis here instead examines how workers may have *perceived* these changes at the *city level* and how such perceptions influenced their decision to strike.

To overcome the data limitation as much as possible, I approximate the strength of firm–government connections using city-level indicators. Specifically, I use three proxies: (1) the average ratio of firms' entertainment expenses to sales in each city, based on National Tax Survey data collected before the anti-corruption campaign; (2) the frequency of princeling-related land transactions in each city prior to the campaign, following Chen and Kung (2019); and (3) the proportion of inspected officials in a city who held business-related titles. Together, these measures capture complementary dimensions of local government–business connections.

If this mechanism is correct, cities with stronger firm–government connections prior to CCDI inspections should experience a larger *increase* in strike activity afterward. This prediction concerns relative changes in strike incidence rather than absolute levels. Cities with tighter political–business ties may already exhibit higher prior strike counts for economic or institutional reasons, such as corruption exacerbating worker grievances, but the inspection-induced rise in strikes should be more pronounced in these environments.

Entertainment Expenses to Sales

The first proxy for firm–government connections is the widely used ratio of firms' entertainment and travel expenses to total sales. This measure reflects firms' informal efforts to cultivate relationships with local officials. In China, expenditures recorded as "entertainment and travel costs" often include not only legitimate hospitality but also gifts, banquets, and other forms of cultivating bureaucratic favor (Cai et al., 2011). A higher city-level ratio therefore signals stronger rent-seeking activity and deeper political ties between firms and government. As noted in Fang (2024), this ratio is a standard proxy in studies of China's corruption environment. Using firm-level data from the National Tax Survey, I compute each city's average entertainment-to-sales ratio, averaged across 2007 and 2011, to capture pre-campaign firm–government connectedness.

To examine heterogeneity by the strength of firm–government connections, I divide cities into quartiles based on their pre-campaign entertainment-to-sales ratios: below 2%, 2–4%, 4–9%, and above 9%, as shown in Table 5. Columns (1)–(4) indicate that the effect of the first high-profile inspection on strike incidence is concentrated in cities in the top quartile, with ratios above 9%. In contrast, the estimated effects for the lower three quartiles are small and statistically insignificant. This pattern suggests that CCDI inspections triggered a pronounced rise in strikes only in cities where firms were previously most politically connected, consistent with the mechanism in which weakening strong firm–government ties increases workers' expected returns to striking.

Because more politically connected cities may have experienced more frequent inspections or reacted more strongly to inspections in neighboring cities, all results in Table 5, columns (1) to (4), follow the specifications of Equation (2) but additionally control for the number of inspections within the city and in adjacent cities. The findings therefore indicate that the post-inspection rise in strikes is not driven by differences in inspection intensity or regional spillovers, but by the weakening of pre-existing firm–government connections.

Princeling Land Transactions

The second proxy for firm–government connections uses the frequency of princeling land transactions between 2004 and 2011 as a measure of pre-campaign corruption. The data from Chen and Kung (2019) define princeling purchases as discounted land transfers from local governments to "princeling firms," whose board members have ties to the Politburo. I measure this frequency at the city level by recording whether the mayor or party secretary was observed conducting a princeling land transaction in a given year. Because I only observe whether a transaction occurred in a given year—not the number or value of transactions—this proxy captures the extensive margin of political–business interactions. For example, if both officials were observed in each year from 2004 to 2011, the city's frequency equals 16; if the mayor appeared in four years and the party secretary in three, the frequency equals 7.

Table 5, columns (5)–(8), shows that cities with more frequent princeling land transactions prior to the anti-corruption campaign experienced substantially larger increases in strikes after inspections. Cities with nine or more transactions saw more than a twofold rise in strike incidence, with the largest effects in cities with more than eleven transactions. In contrast, cities with fewer than nine transactions show no significant change.

Table 5: Treatment Effects by Entertainment Expenses Over Sales and Princeling Land Transactions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Entertainment Expenses Over Sales, Pre-Campaign				Frequency of Princeling Land Transactions Observed, Pre-Campaign			
	<2%	[2%, 4%]	(4%, 9%]	>9%	0 to 6	7 to 8	9 to 10	11 to 16
First HP Insp.	-0.14	0.22	0.15	1.04***	-0.29	-0.06	0.76**	0.95***
	(0.21)	(0.19)	(0.23)	(0.35)	(0.21)	(0.22)	(0.32)	(0.31)
Average, Dep Var Before Insp.	0.29	0.41	0.35	0.77	0.30	0.31	0.50	0.71
SD, Dep Var Before Insp.	0.73	1.33	0.79	1.88	0.80	0.73	1.39	1.83
Observations	4,684	4,728	4,549	5,472	5,016	4,355	4,632	5,424
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. Over Time	YES	YES	YES	YES	YES	YES	YES	YES
Total Inspections Over Time	YES	YES	YES	YES	YES	YES	YES	YES

Notes: The table reports the estimated coefficients $\hat{\delta}$ from Equation (2), using the estimator of Callaway and Sant'Anna (2021). Columns (1)–(4) present results by quartiles of the pre-campaign ratio of entertainment expenses to sales, where the groups correspond to below 2%, 2–4%, 4–9%, and above 9%. Columns (5)–(8) report results by the frequency of princeling land transactions observed between 2004 and 2011, following the definition in Chen and Kung (2019). Each specification includes city fixed effects, quarter-year fixed effects, city-level controls, and additional controls for both neighboring inspections and total inspections over time. Robust standard errors are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Inspected Officials with Business-Related Titles

The third proxy is constructed directly from CCDI inspection cases and measures the proportion of inspected officials holding business-related titles out of all officials inspected from 2012 to 2017, regardless of inspection timing. This measure captures the extent to which local corruption was intertwined with firm–government relations, as a higher share of business-connected officials indicates closer ties between political and economic elites.

Table B7 presents findings that align with the earlier results. Cities in the top quartile, where more than 15% of inspected officials held business-related titles, experienced roughly a threefold increase in strike incidence following inspections. Cities in the 50th–75th percentile range saw about a twofold increase, while those in the bottom half show no significant change.

Robustness: Measurement

I conduct robustness checks to address two common concerns. One concern is that cities with stronger firm–government ties may have been inspected earlier by the CCDI, so the rise in strikes could reflect inspection timing rather than the underlying mechanism. To address this, I restrict the sample to cities whose first high-profile inspection occurred within zero or one quarter after the CCDI team's provincial arrival. Columns (1) to (4) of Table B8 show that the positive effects remain concentrated in early-wave cities with higher entertainment-to-sales ratios, indicating that the results are not driven by inspection timing. Second, if the mechanism reflects changes in worker expectations about government–firm relations, a similar pattern should arise when using labor disputes as an alternative outcome. Columns (5) to (8) of Table B8 show even larger effects for labor dispute cases.

Taken together, these robustness checks show that the observed strike responses are not driven by inspection priority or by the choice of outcome measure. Instead, the evidence supports the view that CCDI inspections exposed underlying grievances in more corrupt cities and raised workers' expected returns to striking.

Robustness: Labor Dispute Outcomes

A further implication of this mechanism is that if CCDI inspections altered workers' expectations about government–firm relations, these shifts should also appear in labor outcomes. When workers perceive a higher return, they should be more willing to strike and more likely to secure favorable outcomes against more corrupt firms, consistent with their expectations.

However, the CLB strike data cannot be used to test this implication because strikes cannot be matched to specific firms, and even when they can, detailed outcomes are rarely reported. To address this limitation, I turn to labor dispute cases from the CJO database. For each wage-arrears case filed by workers, I match firm names to the National Tax Survey, which reports entertainment expenses relative to sales. This linkage allows me to examine whether workers' likelihood of winning increased after inspections, particularly in disputes involving firms with higher pre-campaign entertainment-to-sales ratios. If workers are more likely to win cases against firms that were previously more politically connected, it would provide direct evidence that CCDI inspections reduced firms' political protection and shifted the balance of power toward labor.

Building on this idea, the following regression tests whether workers became more likely to win court cases against firms with higher pre-campaign entertainment-to-sales ratios after CCDI inspections:

$$WorkerWin_{jcq} = \sum_{g \in \{G\}} \Big[\delta_g (Post_{cq} \times ETS_{jg}) + \gamma_g ETS_{jg} \Big] + \alpha_c + \alpha_q + \varepsilon_{jcq} \tag{3}$$

where $WorkerWin_{jcq}$ is a dummy equal to 1 if workers won the case against firm j in court c and quarter q. A win is defined as the worker securing the delayed payment from the firm. ETS_{jg} denotes the pre-campaign entertainment-to-sales ratio of firm j, and g indexes its percentile group. I classify firms into four groups: the 50th–75th percentile, 75th–95th percentile, 95th–99th percentile, and above the 99th percentile. The corresponding cutoffs are approximately 0.23% for the 50th percentile, 0.56% for the 75th, 2.88% for the 95th, and 14.66% for the 99th. Thus, most firms in the matched sample have low entertainment-to-sales ratios, while only the top one percent exhibit very high levels.

$Post_{cq}$ is a dummy equal to 1 if court c is in a quarter q after the first high-profile inspection in its city. The coefficients δ_g capture how post-inspection changes in workers' success rates vary across firms with different pre-campaign entertainment-to-sales levels, relative to firms below the 50th percentile. In contrast, γ_g captures how workers' success rates varied with firms' entertainment-to-sales ratios before inspections. α_c and α_q denote court and quarter fixed effects, and ε_{jcq} is the error term.

The results in Table B9 show that workers' likelihood of winning wage-arrears cases increased significantly after CCDI inspections when disputes involved firms with the highest pre-campaign entertainment-to-sales ratios. Workers were about 38 percentage points more likely to win against firms in the top one percent after inspections, even though they were 28 percentage points less likely to win against these firms before inspections. No significant effects appear for firms in lower percentiles. This strong post-inspection effect for the most politically connected firms supports the mechanism that CCDI inspections improved workers' bargaining position by weakening firm–government ties.

7.2 Government Attention

The second mechanism considers how CCDI inspections may have shifted workers' perceptions of government attention. Because local officials had incentives to prioritize social stability and economic performance (Li and Zhou, 2005), workers' concerns often received limited attention or were suppressed when they threatened stability (Friedman, 2014). The arrival of CCDI inspectors therefore created a temporary window of opportunity for workers, as external oversight increased monitoring and raised the political costs of ignoring or repressing grievances.

Testing this mechanism is challenging because there is no direct measure of government attention or administrative priorities across cities. I therefore use an indirect strategy that exploits variation in mayors' promotion incentives. Chinese mayors become ineligible for promotion after age 58, giving them strong incentives to boost economic performance and maintain stability, including preventing conflicts such as strikes, when they are close to this cutoff. Prior work supports this pattern: Zeng and Zhou (2024) show that city-level GDP growth peaks when mayors approach age 57 and declines sharply afterward. Similarly, Hu and Yang (2024) find that workplace accidents fall once mayors turn 58, suggesting that administrative attention shifts away from GDP growth toward areas such as safety and regulation.

Following this logic, I test whether the effects of CCDI inspections differ across cities led by mayors in four age groups at the time of inspection: below 50, 51–56, 57–58, and above 58. I hand-collected mayors' ages from official government websites and local news reports. The key idea is that mayors aged 57–58 were nearing the end of their promotion window and, in the year before inspection (when they were 56–57), had the strongest incentives to prioritize GDP growth and suppress labor unrest that signaled instability. Thus, the prediction should be that inspections generate the largest increase in strikes in cities whose mayors were close to age 57 before the inspection.

Table B10 shows results consistent with the promotion incentive mechanism. The estimated effects of CCDI inspections on strike activity are strongest in cities led by mayors aged 57–58 at the time of inspection, with a smaller and less significant effect for mayors aged 51–56. No significant effects appear for the other age groups. This pattern aligns with the idea that mayors nearing the end of their promotion window, who in the year before inspections had the strongest incentives to maintain stability and suppress unrest, experienced the largest post-inspection increase in strikes. These results support the channel that the presence of CCDI inspectors shifted workers' beliefs that government attention would change once an external agent arrived, prompting them to strike during this temporary opportunity window.

7.3 Economic Conditions

This section examines whether CCDI inspections altered local economic conditions and labor markets in ways that contributed to the rise in strikes. A straightforward first step is to treat key economic indicators as outcome variables and re-estimate Equation (1) using yearly data. I focus on six city-level measures: employment per capita, unemployment per capita, construction employment per capita, manufacturing employment per capita, the number of private firms per capita, and real estate investment as a share of city-level GDP. The results, shown in Figure A9, reveal no statistically significant post-inspection changes in any of these outcomes.

The mechanism also suggests that politically connected firms may have suffered profit losses after inspections, leading to delayed wage payments or higher layoffs that could trigger more worker unrest. However, this hypothesis is difficult to test because firm-level high-frequency data are unavailable. Without such data, it is unclear how quickly inspections might disrupt firms' payment flows or how long these disruptions would take to translate into worker strikes.

To work around this limitation, I first examine CJO labor dispute cases containing the keyword "firm debts." Figure A10, panel (a), shows a modest increase in such cases after inspections, but they account for less than one-fifth of the overall rise in labor disputes. There is some indication of short-term financial distress among politically connected firms, as the increase in debt-related cases comes mainly from cities with above-median frequencies of princeling land transactions, shown in panels (c) and (e). Second, I show that there is no strong evidence linking most wage-arrears disputes to the financial difficulties of politically connected firms. Panel (b) shows no increase in cases mentioning both "firm debts" and "wage arrears," and panels (d) and (f) show no corresponding patterns for either more or less corrupt cities. Overall, these findings do not support the view that post-inspection increases in labor disputes were driven by firms' financial problems.

To further assess whether firms mistreated workers after inspections, I examine changes in workplace accidents using data from the China Stock Market & Accounting Research (CSMAR) Database, which covers daily accidents in the construction and manufacturing sectors from 2011 to 2016. Columns (2) to (6) of Table B11 show no significant changes in workplace accidents or fatalities in either sector during the inspection period. Overall, there is no evidence that CCDI inspections altered firms' treatment of workers.

7.4 Other Institutional Factors

So far, the evidence provides strong support for the disruption of firm–government connections as the primary mechanism, some support for shifts in government attention, and limited evidence for changes in local economic conditions. Building on these findings, this section considers several alternative institutional channels through which CCDI inspections may have influenced strike activity. Although many institutional factors could be relevant, I focus on three plausible ones that may shift after inspections: (1) leadership turnover, (2) bureaucratic morale, and (3) public trust in local governments.

Leadership Turnover

This mechanism posits that the anti-corruption campaign triggered leadership turnover, creating uncertainty about future political environments. Such uncertainty can reshape workers' expectations about the returns to striking, as existing research links political turnover to greater perceived risk (Julio and Yook, 2012) and temporary contract fragility (An et al., 2016). This mechanism is also closely related to those discussed earlier. By removing key patrons, turnover can weaken firm–government ties, and new leaders may recalibrate relationships with firms and redirect administrative attention. In this sense, it reinforces rather than replaces the firm–government connection and government attention mechanisms.

Directly testing the turnover channel is difficult because treated cities often experienced multiple inspections, making it hard to identify which specific personnel change might have affected workers. There is, however, suggestive evidence consistent with this channel. In Section 6.2, I show that cities exposed to more inspections experience larger increases in strikes, implying that workers may respond to the accumulation of turnover. In Section 7.1, I also show that strike increases are larger in cities where a higher share of inspected officials held business-related positions, linking turnover to firms and, in turn, to workers' responses.

However, it is important to note that workers do not respond to personnel changes per se, but to the environment implied by them. Turnover may signal weakened protection for firms, a shift in government attention, or changes in economic conditions, all of which provide more informative cues about future conditions than turnover alone. Consequently, even if the turnover mechanism is plausible, the strike response is more directly explained by the firm–government corruption mechanism or shifts in government attention.

Bureaucratic Morale

Another potential mechanism concerns bureaucratic morale. Recent research shows that anti-corruption inspections created a chilling effect among local officials, making them more risk-averse and less willing to pursue development projects, especially those involving private enterprises (Wang, 2022; Fang et al., 2022). If this channel were operative, the rise in strikes should display a more delayed rather than immediate pattern, since reduced project approvals and investment would affect workers gradually through slower economic activity. The implications of this channel also parallel those of the economic conditions mechanism, as both operate through weaker local economies and heightened worker grievances. Given the lack of empirical support for the economic channel, there is likewise little evidence consistent with a bureaucratic morale effect.

Trust in Local Government

Another possible channel operates through changes in workers' trust in local governments. Anti-corruption inspections may have weakened this trust by revealing corruption among local bureaucrats (Wang and Dickson, 2021), reducing workers' confidence in the government's ability or willingness to mediate labor disputes. As distrust grew, workers may have been more inclined to bypass official negotiation channels, such as courts or formal grievance filings, and instead use strikes as an informal means to press for negotiation. This channel differs from the firm–government connection and government attention mechanisms in the direction of belief change: in those channels, workers initially viewed the government as unhelpful but became more optimistic after inspections, whereas here workers initially had greater confidence in local governments but lost trust once corruption was exposed.

If this channel were valid, one would expect opposite movements in strikes and labor dispute cases: strikes would rise while formal disputes would fall. However, the evidence in previous sections shows that both strikes and disputes increased after inspections, providing little support for this mechanism.

In addition, this channel should generate more political protests beyond strikes, reflecting a broader decline in public trust. Tracking political protests in China is challenging due to strict censorship. To address this limitation, I draw on the Global Database of Events, Language, and Tone (GDELT), which uses machine learning to monitor global media coverage of protests and other forms of social unrest. After rigorous data cleaning, including the removal of duplicate entries and validation of actual protest events, I obtain a sample of 587 political protests in China's prefecture-level cities from 2011 to 2020. These events span a wide range of issues, including free speech, institutional reform, ethnic minority rights, environmental advocacy, and opposition to specific government policies. Although scarce, this dataset represents the best available source for tracking political protest in China. Table B11, column (1), shows no significant change in political protests following inspections. This evidence counters the hypothesis that inspections triggered a surge in political protests driven by declining trust in local governments.

7.5 Other Concerns

After examining several mechanisms, this final section considers two additional concerns not directly captured by the theoretical outline in Section 3. The first is whether the observed rise in strikes reflects reporting bias, as media outlets may have covered more incidents after inspections. The second is the decline in strike activity two years after inspections, for which I discuss possible explanations.

Media Exposure

This hypothesis suggests that the observed rise in strikes reflects a mechanical effect driven by increased media reporting after citywide anti-corruption inspections, rather than a genuine increase in strike activity. Several factors make this unlikely. First, most strike records come from *Weibo*, a nationwide platform with centrally managed censorship (Vuori and Paltemaa, 2015), so local political changes should not influence censorship adjustments in any specific city. Second, if inspections had prompted broader media attention, other types of protest documented in the GDELT data should have risen as well, yet they did not as in Table B11.

Third, court-reported labor disputes, many of which never reached the media, also increased after inspections. Even if one argues that inspections simply led courts to relax acceptance standards or speed up processing, one would expect a more uniform increase across categories. However, the increase is not uniform across labor dispute types: wage-arrear cases rise by about 50%, whereas other types of labor disputes, such as those related to insurance or workplace accidents, increase by less than 30%, as shown in Table B6. This differential pattern is more consistent with a mechanism in which inspections disproportionately increase wage-related disputes. Taken together, these points suggest that heightened media exposure or generalized court processing changes are unlikely to explain the observed rise in strikes.

The Decline of Strikes

One remaining puzzle is the decline in strike activity that begins roughly two years after a city's first high-profile inspection, as shown in Figure 6. A plausible explanation is that the initial surge in strikes reflected the release of pre-existing grievances that inspections brought to light rather than the emergence of new worker–firm conflicts. Once these accumulated disputes were addressed, strike activity naturally subsided and stabilized, suggesting that the local labor environment had reached a new post-inspection equilibrium.

Given that the economy likely transitioned to a new equilibrium after inspections, identifying the precise cause of the subsequent decline in strikes is beyond this study's scope. Possible explanations include a genuine reduction in corruption or a shift toward a more labor-favorable environment, both of which could reduce strike activity. Alternatively, the government may have strengthened surveillance or censorship following the surge in strikes, thereby suppressing further unrest.

I have also explored potential factors shaping the duration from rising to declining strike activity using a duration analysis, presented in Appendix E. The suggestive evidence shows that strikes decline sooner when the return to striking is lower and later when it is higher. In particular, strikes fall earlier in cities with higher average wages, consistent with higher opportunity costs for workers in these areas. By contrast, strikes decline later in cities with a larger share of migrant workers, suggesting more persistent grievances or longer strike coordination. This analysis represents the furthest extent to which I can examine the decline in strikes within this study's scope.

8 Discussion

In September 2014, Mr. Shao and 66 coworkers from Sichuan undertook short-term construction work in Taiyuan. By November, they had received only 2,000 yuan each, with more than 1.7 million yuan unpaid. Beginning in January 2015, they remained at the worksite to pursue their wages despite severe winter conditions. Repeated visits to government offices were unsuccessful.[32]

This paper shows that China's anti-corruption crackdown brought long-standing worker grievances, such as those faced by Shao, to the surface. Following the arrival of a CCDI inspection team in a province, corruption investigations and related *Weibo* discussions rose sharply. After a city's first high-profile inspection, worker strikes also increased, doubling within one year and tripling within two. Most of this rise occurred from private firms in construction and manufacturing sectors and was driven by wage arrears. Strikes also exhibited network effects: strike activity began to rise in cities even before their own first high-profile inspection, following inspections and strike surges in early-wave or neighboring cities. Cities with stronger migrant networks experienced larger increases in strikes. Overall, the surge in strikes reflected grievances previously suppressed in environments with higher firm–government corruption and limited government attention to workers' concerns.

Some questions remain unanswered. In particular, what new equilibrium emerged in cities in the longer term after anti-corruption inspections? Did inspections reshape the local political environment in ways that enabled worker complaints to be addressed more promptly in subsequent years? Or did local governments respond by strengthening strike suppression, supported by advances in surveillance technologies (Beraja et al., 2023)? Addressing these questions lies beyond the scope of this paper and would require detailed bureaucratic and firm-level data.

Over the years, regulatory reforms appear to have moved the system toward a more favorable outcome for workers. The first dedicated national regulation, the *Regulation on Ensuring Wage Payment to Migrant Workers*, adopted by the State Council in December 2019 and implemented on May 1, 2020, codifies the responsibilities of employers and local governments and introduces enforcement tools such as blacklists and designated wage accounts. Yet how much has truly changed on the ground?

Consider a recent example. In 2022, 17 workers, including Mr. Yang and Mr. Yao, worked on a wastewater project in Zhoushan for a subcontractor under China State Construction Engineering but were not paid for three months of work. They filed a wage complaint in January 2023, which

[32] 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.

the local labor bureau withdrew in March at the company's request. After the workers reported the case to the Dinghai District Disciplinary Committee, officials acknowledged that they had neither verified wage payments nor obtained consent for withdrawing the complaint. The overdue wages, totaling more than 200,000 yuan, were ultimately paid in June.[33]

Even though the representativeness of cases like those of Yang and Yao is unclear, they reflect the broader institutional evolution unfolding in China over the years. Taken together, this paper shows that shifts in local political incentives can bring long-standing worker grievances to the surface. It offers a first step toward understanding how political oversight can transform labor activism and illuminate the grievances embedded in a nation's evolving political economy.

[33]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.

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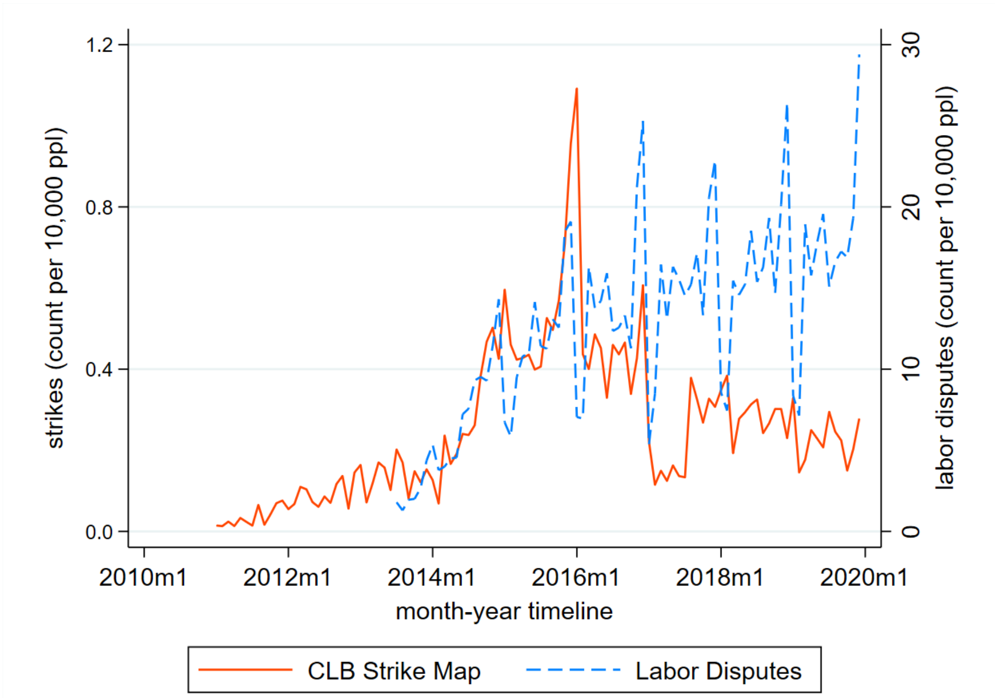
Appendix

A Additional Figures

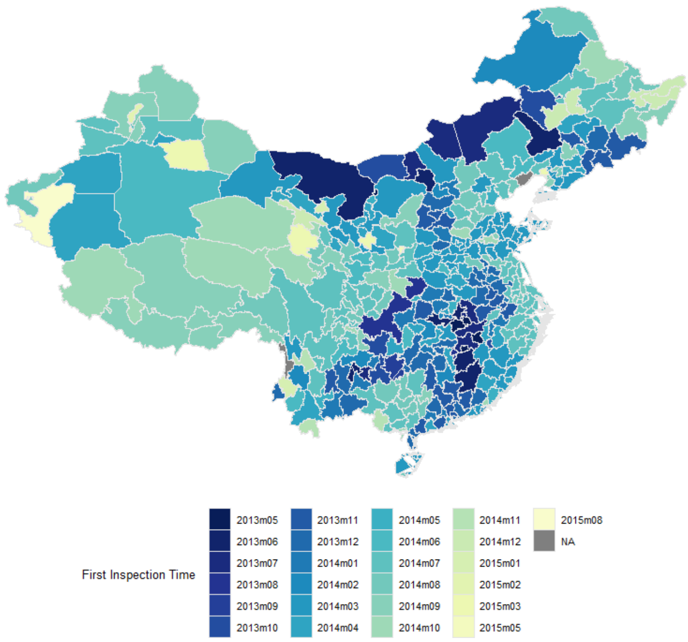
Figure A1: Comparing CLB Strike Map and Labor Disputes from CJO



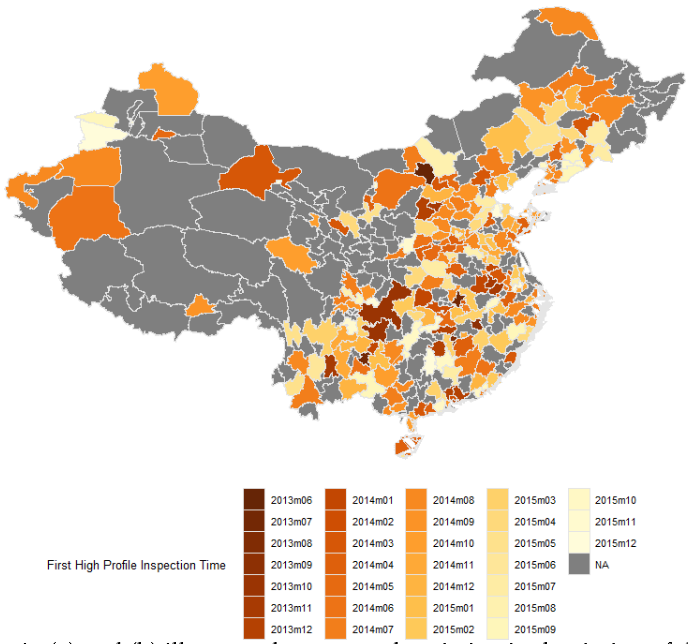
Notes: The graph compares the labor strikes recorded by the China Labor Bulletin (CLB) and the labor disputes recorded by China Judgements Online (CJO) on a national level and on a monthly basis. According to the graph, the patterns of strikes and labor disputes are similar from 2013 to 2017. After 2017, 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 (1), closely follows the same pattern.

Figure A2: Timing of First Corruption Inspections & First High Profile Inspections

(a) First Inspections

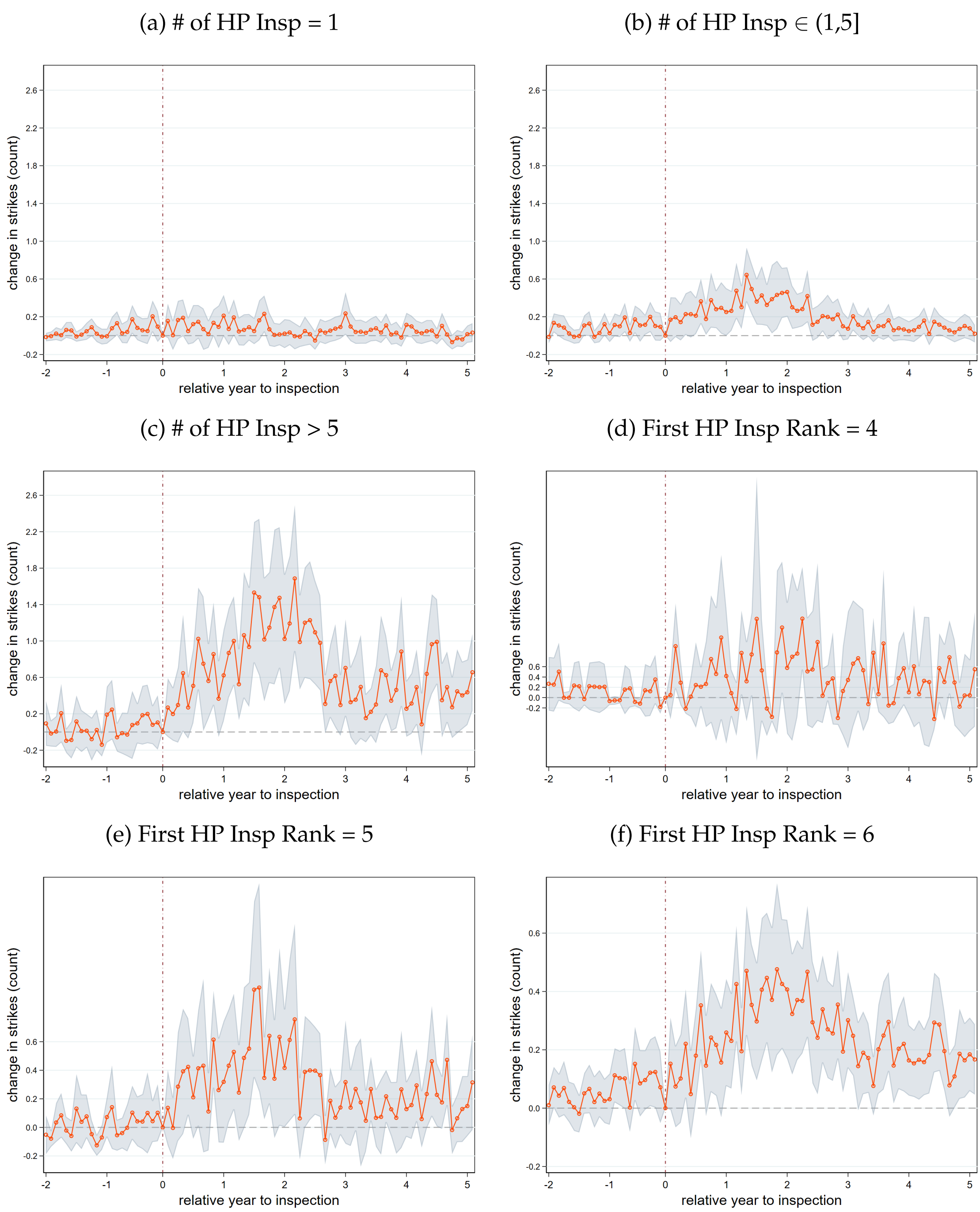


(b) First High Profile Inspections



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 A3: Heterogeneous Effects by Intensity and Rank of High-Profile Inspections



(a) # of HP Insp = 1

(b) # of HP Insp ∈ (1,5]

(c) # of HP Insp > 5

(d) First HP Insp Rank = 4

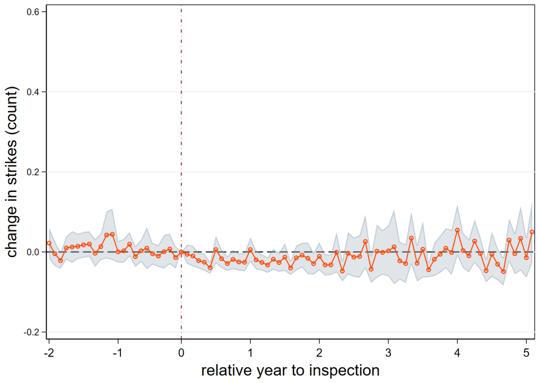
(e) First HP Insp Rank = 5

(f) First HP Insp Rank = 6

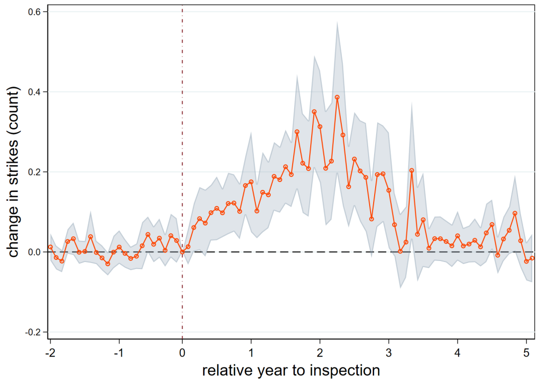
Notes: The series of graphs demonstrate the estimated λ_k from Equation (1) based on the total number of high-profile inspections experienced and by the bureaucratic rank of the first inspected official. Panels (a)–(c) divide cities by the cumulative count of high-profile inspections: one, two to five, and more than five. Panels (d)–(f) divide cities by the rank of the first inspected official, where rank 4 corresponds to sub-provincial (ministerial) level, rank 5 to bureau-director level, and rank 6 to deputy–bureau-director level. The red line plots the estimated coefficients λ_k from Equation (1), and the shaded area represents 95% confidence intervals based on standard errors clustered at the city level. The vertical dashed line marks the month immediately preceding the first high-profile inspection in the city.

Figure A4: Strikes by Consequence

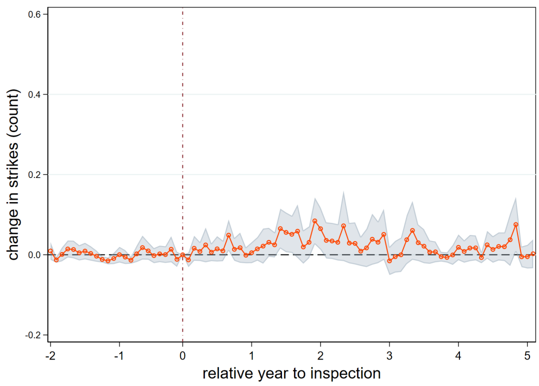
(a) Peaceful Negotiations



(b) Police Involvement



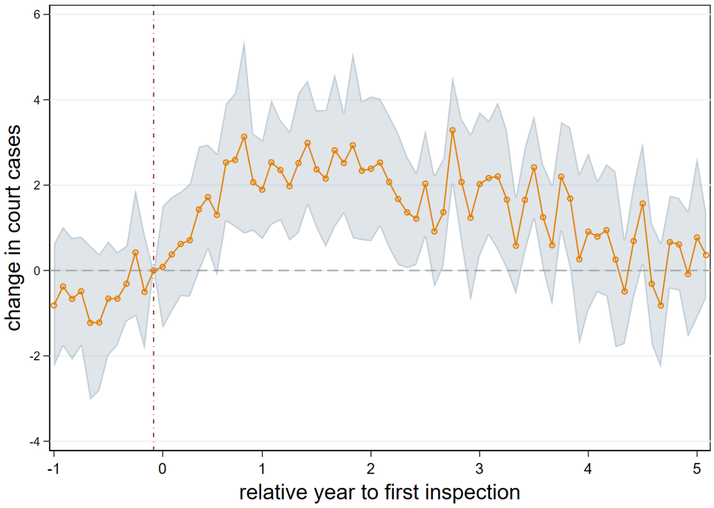
(c) Suppression or Violence



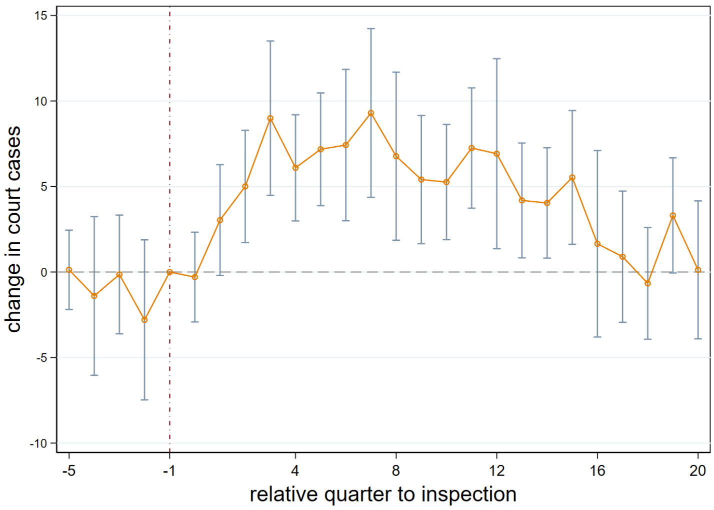
Notes: The series of graphs demonstrate the estimated λ_k from Equation (1) based on the outcome of the strikes. The hollow red dots represent the estimates and the light blue region signifies the standard errors. The dotted vertical line indicates the timing (one month prior) of the first high-profile 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 cities from 2011 to 2020.

Figure A5: Baseline Outcomes Using Labor Disputes from CJO

(a) Monthly Level

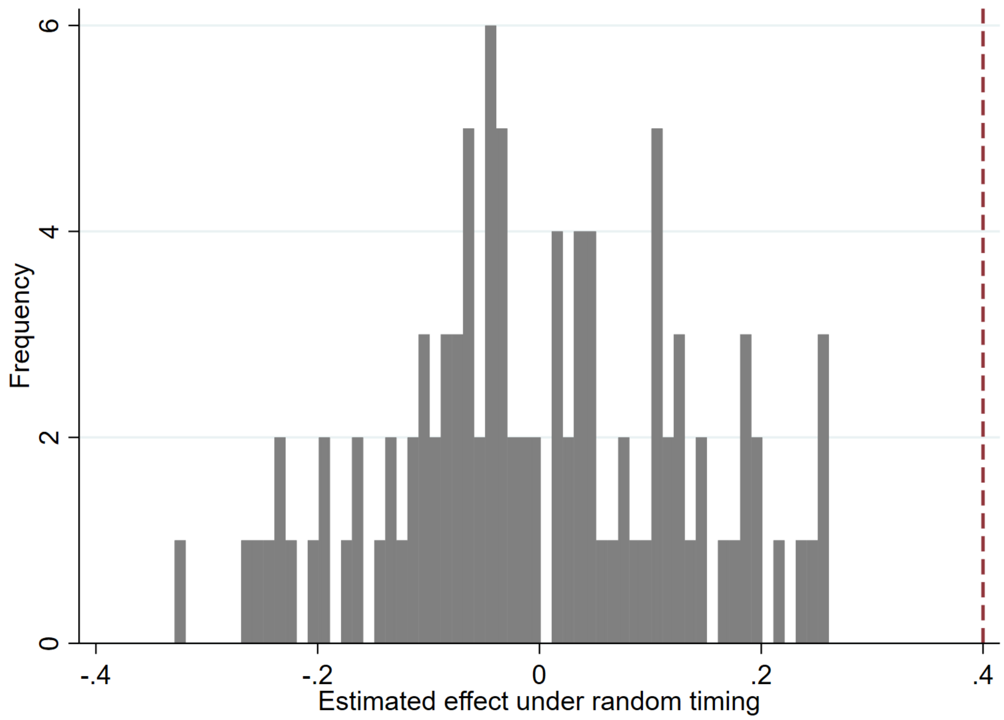


(b) Quarterly Level



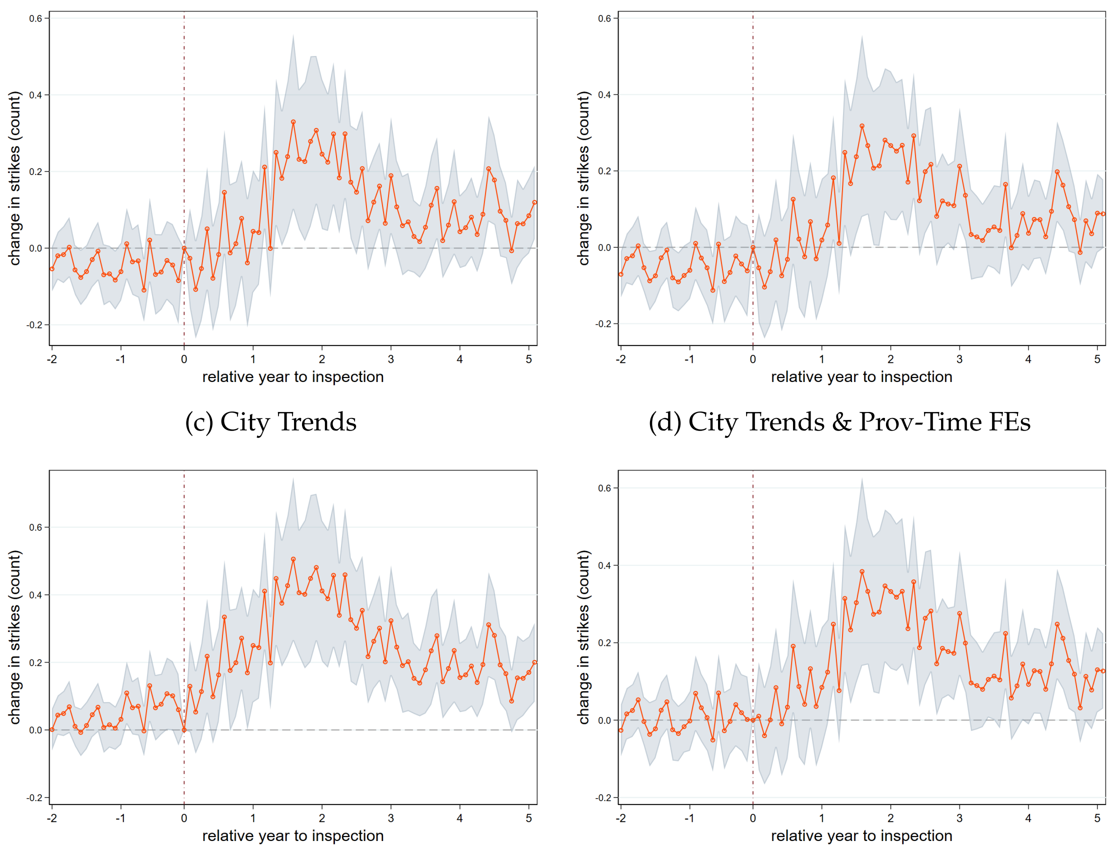
Notes: This graph depicts the changes in court labor disputes before and after the first high-profile inspection in a city, given the arrival of the CCDI inspection team within the province. Specifically, the points are the estimated λ_k from Equation (1). Panel (a) presents λ_k at the monthly level, 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 cities from 2011 to 2020. The quarterly sample includes 14,480 observations from 362 cities from 2011 to 2020.

Figure A6: Placebo Distribution of Treatment Effects



Notes: This figure plots the distribution of estimated treatment effects, $\hat{\delta}$, from Equation (2) based on 1,000 placebo simulations, in which both the timing of the first high-profile inspection and the treatment status were randomly reassigned across cities. The dashed vertical line indicates the actual estimated effect from the baseline specification reported in Table 3.

Figure A7: Alternative Specifications Controlling for Province and City-Level Shocks



(a) Prov-Year FEs

(b) Prov-Time FEs

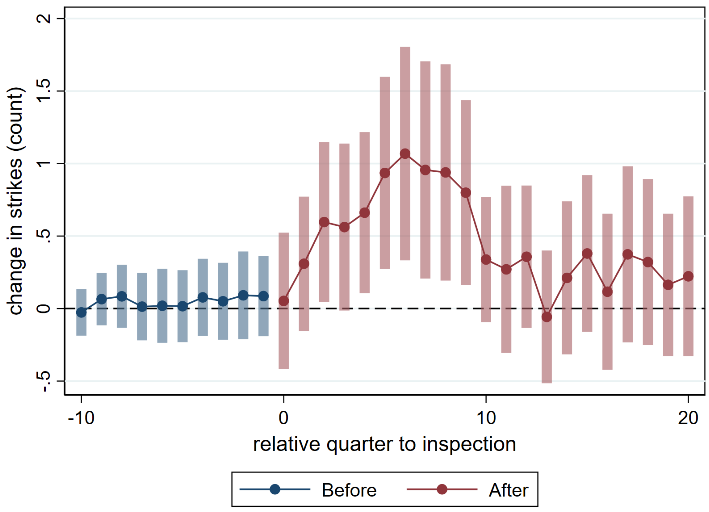
(c) City Trends

(d) City Trends & Prov-Time FEs

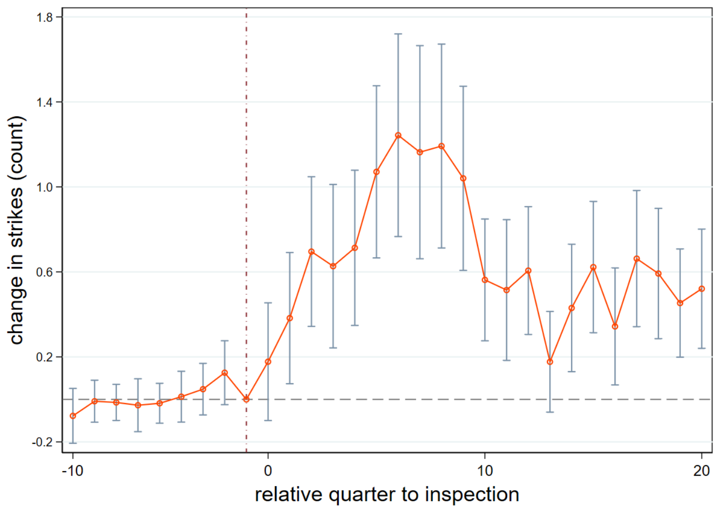
Notes: The four graphs present the estimated λ_k from Equation (1) under alternative specifications. Panel (a) controls for province-year fixed effects to account for province-level annual shocks such as leadership turnover or policy changes. Panel (b) replaces these with province-month fixed effects to capture finer temporal variations within provinces. Panel (c) adds city-specific linear trends to control for heterogeneous local dynamics, while Panel (d) combines city trends and province-month fixed effects, forming the most restrictive specification. The hollow red dots represent the estimates, and the light blue region indicates the standard errors. The dotted vertical line marks the month of the first high-profile corruption inspection in a city, conditional on the CCDI inspection team's arrival in the same province. The sample includes 43,440 observations from 362 cities covering 2011–2020.

Figure A8: Comparing Baseline Results b/t Callaway and Sant'Anna (2021) & Gardner (2021)

(a) Callaway and Sant'Anna (2021)



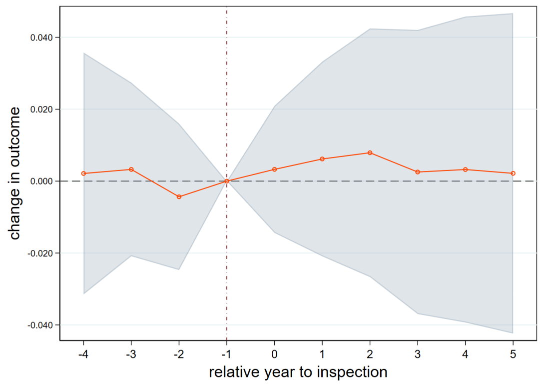
(b) Gardner (2021)



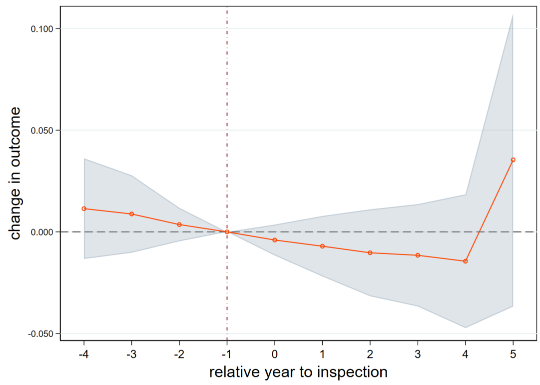
Notes: The graphs demonstrate the estimated λ_k from Equation (1) based on the specifications from Callaway and Sant'Anna (2021) and Gardner (2021). I changed the temporal observational level from months to quarters of the year to expedite the estimation process. The vertical dashed line represents one 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 cities from 2011 to 2020.

Figure A9: Economic Outcomes Post High-Profile Inspections

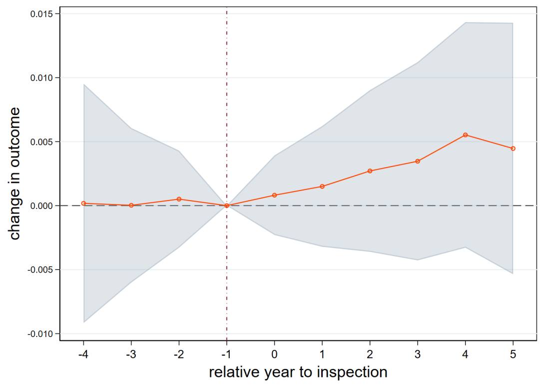
(a) Employment per Capita



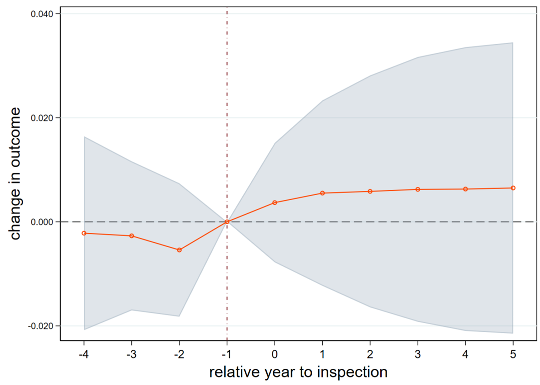
(b) Unemployment per Capita



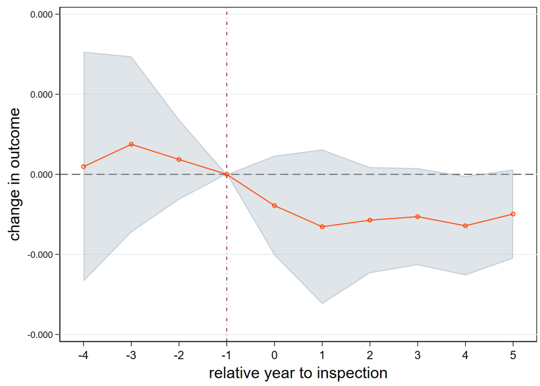
(c) Construction Employment per Capita



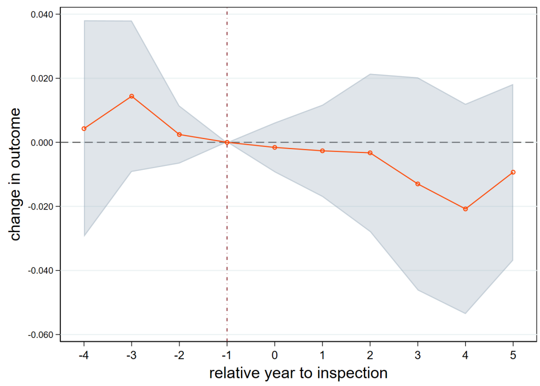
(d) Manufacturing Employment per Capita



(e) Number of Private Firms per Capita

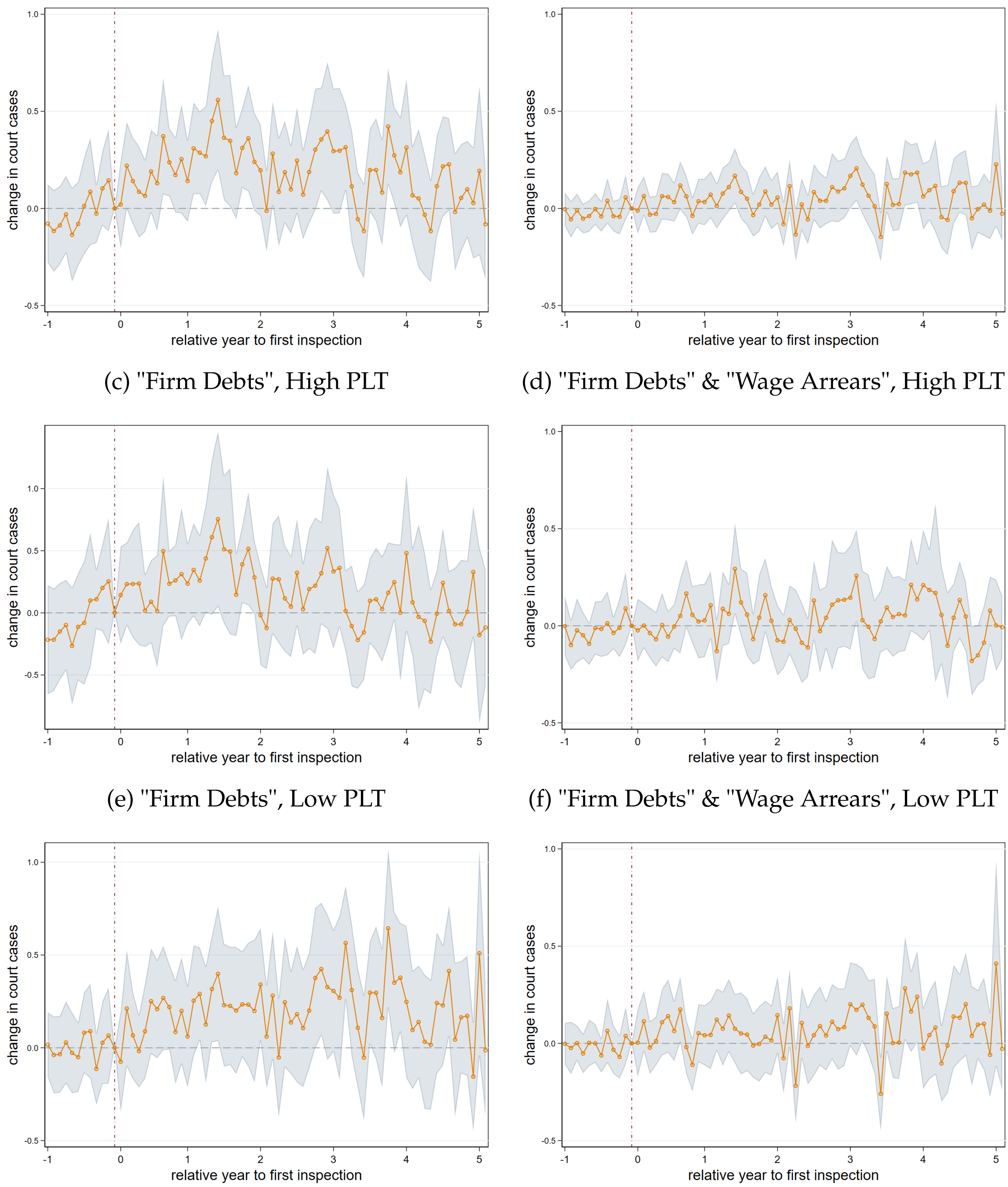


(f) Real Estate Investment / GDP



Notes: The six graphs report the estimated λ_k from Equation (1) when the dependent variable is replaced by alternative city-level economic outcomes with yearly observations. Panel (a) plots employment per capita; panel (b) unemployment per capita; panel (c) construction employment per capita; panel (d) manufacturing employment per capita; panel (e) the number of private firms per capita; and panel (f) real estate investment as a share of GDP. Hollow red dots represent point estimates and the light blue region denotes the 95% confidence interval. The dotted vertical line marks the timing of a city's first high-profile anti-corruption inspection case, following the arrival of the provincial CCDI inspection team. All economic outcome variables are drawn from the China City Statistical Yearbook (CCSY).

Figure A10: 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 (1). 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 cities from 2011 to 2020.

B Additional Tables

Table B1: 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 B2: 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 Cities	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 B3: 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 B4: 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 (2) 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 B5: 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 (2) 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 B6: Robustness Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Labor Dispute Cases				Strikes				
	Total	Wage Arrears	Insurance	Accidents	Gardner (2021)	IHS	ZINB	Drop Guangdong	Drop Big Cities
1st HP Insp.	14.19***	3.46***	0.96**	1.08**	0.68***	0.14**	0.42***	0.64***	0.45***
	(3.17)	(0.80)	(0.40)	(0.47)	(0.10)	(0.06)	(0.06)	(0.17)	(0.16)
Average, Dep Var Before Insp.	32.54	6.69	3.54	4.37	0.48	0.48	0.48	0.35	0.41
SD, Dep Var Before Insp.	72.21	16.27	6.33	8.24	1.35	1.35	1.35	0.81	1.06
Observations	7,046	7,046	7,046	7,046	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
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

Notes: The table presents the coefficients $\hat{\delta}$ from Equation (2), and columns (1) to (4) 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) estimates Equation (2) using the Gardner (2021) estimator. Column (6) transforms the strikes using the inverse hyperbolic sine (IHS) function and estimates Equation (2) using this transformed variable. Column (7) fits Equation (2) using a zero-inflated negative binomial model. Columns (8) and (9) estimate Equation (2) 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 B7: Treatment Effects by the Share of Business-Connected Officials

	(1)	(2)	(3)	(4)
	Precentage of Inspections Related to Businesses			
	(0%, 5%]	(5%, 10%]	(10%, 15%]	>15%
First HP Insp.	0.04	0.19	0.91**	1.01***
	(0.21)	(0.12)	(0.40)	(0.35)
Average, Dep Var Before Insp.	0.39	0.31	0.92	0.44
SD, Dep Var Before Insp.	0.89	0.88	2.81	1.16
Observations	3,524	4,316	3,085	3,496
Year Span	2011-2020	2011-2020	2011-2020	2011-2020
City FE	YES	YES	YES	YES
Quarter-Year FE	YES	YES	YES	YES
City Controls	YES	YES	YES	YES
Neighbor Insp.	YES	YES	YES	YES
Own Insp.	YES	YES	YES	YES

Notes: The table reports the estimated coefficients $\hat{\delta}$ from Equation (2), using the estimator of Callaway and Sant'Anna (2021). Cities are divided into quartiles based on the share of inspected officials whose positions were business-related prior to the anti-corruption campaign: (0%, 5%], (5%, 10%], (10%, 15%], and above 15%. Each specification includes city fixed effects, quarter-year fixed effects, city-level controls, as well as controls for both neighboring and own inspections over time. Robust standard errors are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B8: Robustness on First Wave and Labor Disputes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Outcome: Labor Strikes				Outcome: Labor Disputes			
	Entertainment Expenses Over Sales, Pre-Campaign				Entertainment Expenses Over Sales, Pre-Campaign			
	<2%	[2%, 4%]	(4%, 9%]	>9%	<2%	[2%, 4%]	(4%, 9%]	>9%
First HP Insp.	0.15	0.47*	0.23	1.58***	1.51	0.92	10.48***	29.35***
	(0.19)	(0.28)	(0.32)	(0.50)	(1.76)	(2.48)	(4.09)	(6.53)
Average, Dep Var Before Insp.	0.15	0.78	0.40	0.99	7.48	6.61	7.66	22.63
SD, Dep Var Before Insp.	0.38	2.40	0.76	2.27	13.38	12.82	8.90	48.58
Observations	3,580	3,640	3,673	4,504	3,364	3,402	3,294	3,910
Year Span	2011-2020	2011-2020	2011-2020	2011-2020	2013-2020	2013-2020	2013-2020	2013-2020
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	NO	NO	NO	NO

Notes: The table reports the estimated coefficients $\hat{\delta}$ from Equation (2), using the estimator of Callaway and Sant'Anna (2021). Columns (1)–(4) present results for labor strikes, comparing cities in different quartiles of pre-campaign entertainment expenses to sales. These estimates are based only on the first-wave sample, which includes cities that experienced their first high-profile inspection within one quarter of the CCDI team's arrival in the province. Columns (5)–(8) show results for labor dispute cases obtained from the China Judgments Online (CJO) database. All specifications include city fixed effects, quarter-year fixed effects, city-level controls, and controls for both neighboring and own inspections over time. Robust standard errors are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B9: Treatment Effects on Workers' Court Outcomes by Firms' Entertainment-to-Sales Ratios

	Outcome = 1 if Workers Win in Wage Arrear Disputes
Post × ETS[50–75]	0.02
	(0.03)
Post × ETS[75–95]	0.02
	(0.04)
Post × ETS[95–99]	0.00
	(0.07)
Post × ETS[>99]	0.38**
	(0.16)
ETS[50–75]	0.01
	(0.03)
ETS[75–95]	-0.02
	(0.04)
ETS[95–99]	0.01
	(0.06)
ETS[>99]	-0.28*
	(0.15)
Post	-0.03
	(0.04)
Constant	0.60***
	(0.03)
Observations	9,555
Year Span	2013-2020
Court FE	YES
Quarter-Year FE	YES

Notes: The dependent variable equals one if workers won the case against a firm in a given court and quarter. The table reports the estimates $\hat{\delta_g}$ and $\hat{\gamma_g}$ of Equation (3). The sample includes wage-arrears disputes initiated by workers as plaintiffs from the China Judgements Online (CJO) database between 2013 and 2020. Firm names were matched to the National Tax Survey to obtain firms' pre-campaign entertainment-to-sales (ETS) ratios. Court and quarter-year fixed effects are included. Robust standard errors clustered at the court level are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table B10: Treatment Effects by Mayor's Age

	(1)	(2)	(3)	(4)
	Mayor's Age at the Time of Inspections			
	Below 50	51 to 56	57 to 58	Above 58
First HP Insp.	0.05	0.33*	1.94**	-0.14
	(0.20)	(0.19)	(0.88)	(0.42)
Average, Dep Var Before Insp.	0.27	0.54	1.28	1.12
SD, Dep Var Before Insp.	0.65	1.55	2.14	1.35
Observations	4,680	7,290	3,510	3,430
Year Span	2011-2020	2011-2020	2011-2020	2011-2020
City FE	YES	YES	YES	YES
Quarter-Year FE	YES	YES	YES	YES
City Controls	YES	YES	YES	YES
Neighbor Insp.	YES	YES	YES	YES
Own Insp.	YES	YES	YES	YES

Notes: The table reports the estimated coefficients $\hat{\delta}$ from Equation (2), using the estimator of Callaway and Sant'Anna (2021). Cities are grouped by the mayor's age at the time of the first high-profile CCDI inspection: below 50, 51–56, 57–58, and above 58. Each specification includes city fixed effects, quarter-year fixed effects, city-level controls, as well as controls for both neighboring and own inspections over time. Robust standard errors clustered at the city level are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B11: 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 (2) 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 $p < 0.01$.

C Alternative Event Definitions

This section examines alternative definitions of the timing of the anti-corruption shock. Specifically, I re-estimate the event-study specifications using four alternative event definitions to distinguish between the potential roles of CCDI inspection team arrivals and high-profile (HP) inspection announcements. Figure C presents the dynamic effects under each definition.

Panel (a) corresponds to the baseline specification, where the treatment is defined as the first HP inspection in a city following the CCDI team's arrival in the province. Strikes increase sharply after the event and remain elevated for several years, indicating a strong and persistent response to locally salient HP inspections.

Panel (b) defines the event as the CCDI team's *arrival* to each province, capturing a province-level announcement shock. The results show little to no change in strike activity around the arrival date, likely because the province-level timing is too coarse and fails to capture the more localized salience of subsequent inspections. Panel (c) defines the event as the city's first inspection of any rank (high or low profile) after CCDI arrival in the province, which produces a delayed, roughly one-year-lagged increase in strikes. Finally, Panel (d) defines the treatment as the city's first HP inspection after the national anti-corruption campaign announcement in November 2012, regardless of CCDI arrival. This specification also reveals a roughly one-year-lagged rise in strikes following the event.

Taken together, the results suggest that only the first definition—the first HP inspection occurring in a city after the CCDI team has arrived in the province—generates an immediate and sustained increase in strike activity. The alternative definitions in Panels (c) and (d) may also affect workers' behavior, but their impacts appear more gradual and mediated through broader awareness of the campaign. In other words, while early announcements or low-profile investigations may have raised general expectations about heightened government scrutiny, the most salient and credible signal for workers appears to occur when an HP inspection is announced locally after the CCDI team's arrival in the province has made the campaign more tangible and visible. The arrival of the CCDI inspection team likely served as a *credible* signal of central oversight, while the subsequent HP case provided the *locally salient* evidence that enforcement was real and relevant to workers' own environment.

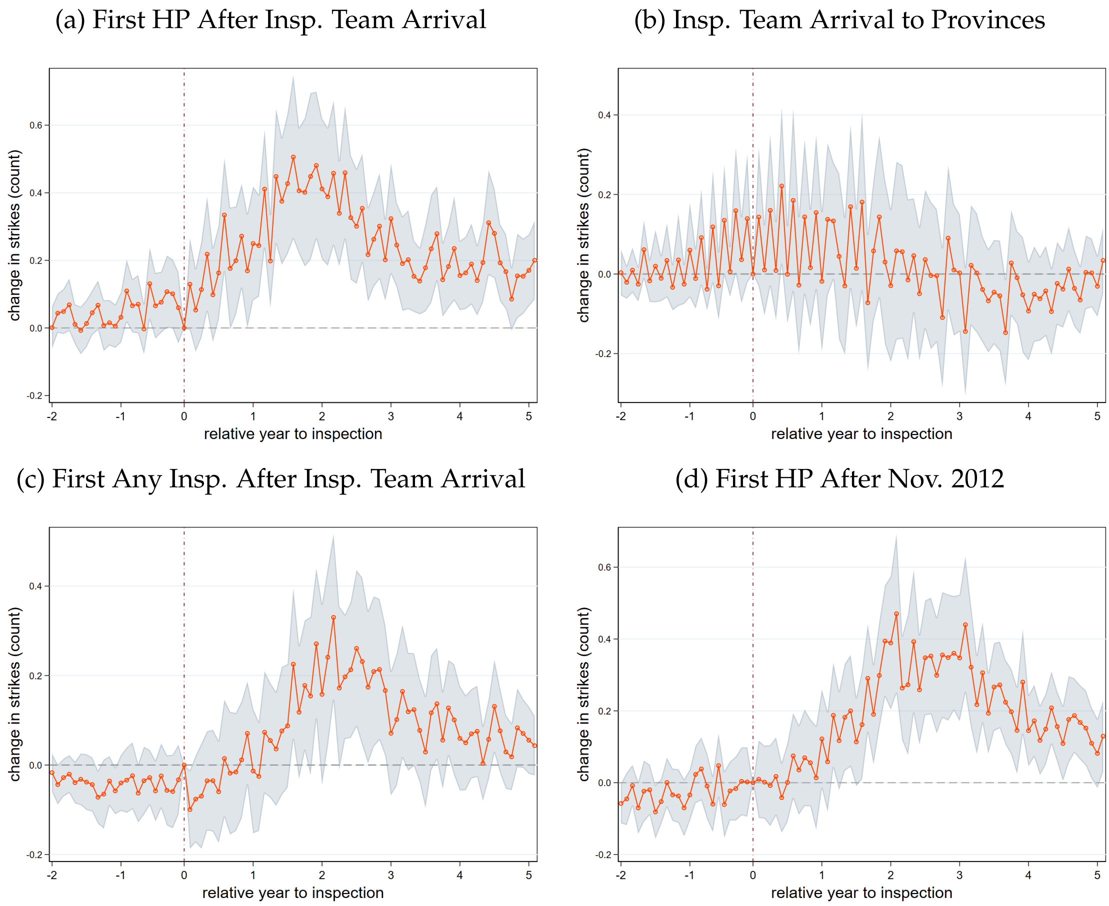
D Additional Results

D.1 Prior Strikes

This section examines treatment effect heterogeneity across cities based on their average quarterly strike activity before the first high-profile inspection. About 10% of cities reported no strikes per quarter, and the median city recorded an average of 0.25 strikes. I investigate whether inspection-induced increases in strikes were concentrated in cities with higher or lower prior strike frequencies, and whether cities with no prior strikes began to experience strikes after their first high-profile inspection.

As shown in Table D, columns (1) to (3), cities with no prior strikes or a quarterly average below 0.25 experienced an increase of 0.3 strikes per quarter after their first high-profile inspection.

Figure C: Baseline Outcomes with Alternative Events



Notes: The figure plots the estimated coefficients λ_t from Equation (1), showing changes in the number of strikes before and after each alternative event definition. Each panel corresponds to a different treatment timing: (a) the first high-profile (HP) inspection in a city after the CCDI team's arrival in the province (baseline); (b) the CCDI team's arrival to provinces; (c) the first inspection of any rank (high or low profile) in a city after CCDI arrival; and (d) the first HP inspection in a city after the national anti-corruption campaign announcement in November 2012, regardless of CCDI arrival. Red dots represent monthly point estimates, and the shaded blue regions denote 95% confidence intervals based on province-clustered standard errors. The dotted vertical line marks the event month ($t = 0$). The sample includes 43,440 city-month observations across 362 prefectures from 2011 to 2020.

Given the sample average of 0.48 strikes per quarter before treatment, this represents a sizable rise for cities that had never recorded a strike. For cities with a quarterly average below 0.25, strike incidence increased roughly threefold. Cities with averages above 0.25 saw an increase of nearly 50%. Overall, corruption inspections raised strike activity across all cities, with the largest proportional effects in cities with fewer pre-inspection strikes.

D.2 Extensive Margin

In this analysis, the outcome is whether a city experienced at least one strike in a given quarter, rather than the number of strikes. This extensive margin is relevant mainly for cities with no strike history before the first high-profile inspection. As shown in Table D, columns (4) to (6), cities without prior strikes experienced an 18% increase in the probability of a strike after the first inspection. In contrast, cities with previous strikes show no change in strike likelihood following inspections.

Table D: 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 (2) 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 $p < 0.01$.

E Duration Analysis on Strikes Decline

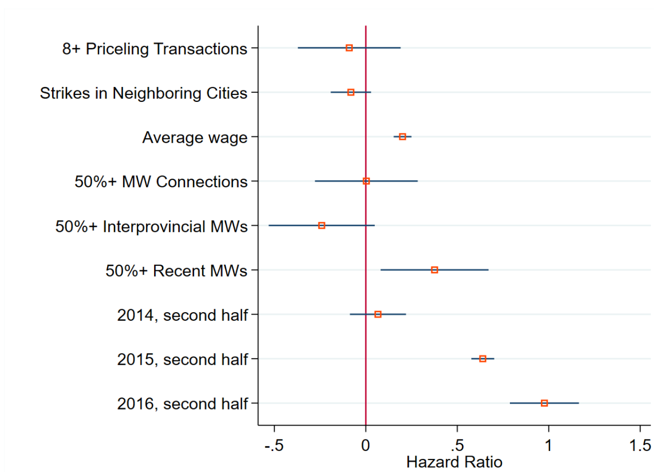
This section explores how various factors related to workers' strike returns can affect the duration from a city's strike increase to decrease after its first high-profile inspection. To analyze this, I employ a duration analysis model:

$$h(t|x, \beta) = h_0(t)e^{\boldsymbol{X}\beta + \boldsymbol{Z}\alpha}. \tag{4}$$

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 E1 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 E1: Estimates of Duration Analysis on Decreasing Strikes



Notes: The graph demonstrates the estimates $\hat{\beta}$ from Equation (4). 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 cities from 2011 to 2020.