Corruption and the Allocation of Subsidies in China: The Role of Hometown Preference *

Jingcheng Jiang†

Abstract

Can anti-corruption enforcement improve allocative efficiency when political connections distort industrial policy? Exploiting China's 2013 anti-corruption inspections as a plausibly exogenous shock to political connections, I show that enforcement reduced subsidies to hometown-connected firms. Effects are concentrated on opaquely described allocations, while transparent subsidies remain unaffected. This pattern suggests that politicians curtail corrupt rather than legitimate transfers when monitoring intensifies. Subsidy reductions operate primarily through behavioral change among continuing politicians rather than elite turnover, as hometown favoritism shifts from career asset to career liability, disrupting clientelistic equilibria. As resources shift from politically connected to more productive firms, regional productivity increases, with gains concentrated among smaller enterprises that had been crowded out by larger firms in politicians' hometowns. These findings demonstrate how strengthened enforcement can restore competitive dynamics by disrupting the *quid pro quo* arrangements between politicians and firms that generate substantial economic misallocation.

JEL Codes: D73, H25, O17, O25, P26, R58

Keywords: Anti-corruption, Government Subsidies, Hometown Favoritism, Public Spending

*I would like to thank the editor, Xinzheng Shi, and two anonymous referees for their insightful comments and suggestions. I would like to thank my Ph.D. supervisors Kensuke Teshima, Chiaki Moriguchi, Meradee Tangvatcharapong, and Atsushi Yamagishi for their guidance. I also thank Yoichi Sugita, Haishan Yuan, Guojun He, Jun Nakabayashi, Yutaro Izumi, Charles Siriban, Wenwei Peng, Yukichi Mano, Takashi Kurosaki, Yutaka Arimoto, Eiichi Tomimura, Yuri Sugiyama, Hiroshi Kumanomido, Erina Nakai, and comments from seminar participants from Hitotsubashi University, Waseda University, Seoul National University, National Taiwan University, and comments from participants from the 17th Applied Econometrics Conference, 18th EAEA Conference, 2023 EWMES Conference, 23rd AASLE Conference, and 6th JADE Conference.

†University of Tokyo. Email: jingcheng.jiang@uqconnect.edu.au

1 Introduction

Government subsidies are a key tool for boosting firm performance and driving economic growth. Widely implemented across both developed and developing economies, these financial incentives can significantly influence firms' productivity, survival rates, and innovation capacity.[1] For example, China's industrial policy spending and direct subsidies are substantial, amounting to at least 1.73 percent of GDP (approximately $248 billion) (Kennedy et al., 2022), emphasizing the substantial economic significance of subsidies.[2]

However, the very nature of government subsidies implies that they are not always guided by economic efficiency and are particularly vulnerable to corruption. The allocation of subsidies is often discretionary, allowing for potential manipulation by political actors (Fang et al., 2023). Additionally, evaluating the effectiveness of subsidies *ex-post* is challenging, and there are no clear criteria to determine the optimal allocation of subsidies. These factors combine to make subsidy allocation a fertile ground for corrupt practices, which can ultimately diminish the intended economic benefits of subsidies.

One specific channel through which corruption can distort subsidy allocation is politician's hometown favoritism, a phenomenon where politicians show preference for their places of origin. This has been documented in various contexts (Besley et al., 2012; Hodler and Raschky, 2014; Carozzi and Repetto, 2016; Do et al., 2017).[3] In the realm of subsidies, hometown favoritism may manifest as preferential allocation to firms in a politician's birthplace. The shared cultural background and reduced information asymmetry between hometown-connected politicians and firms can lower the transaction costs of corruption, thereby making it an attractive avenue for illicit practices.

Establishing a causal link between hometown favoritism and government subsidy allocation is challenging due to competing explanations. Specifically, politicians may favor local firms due to corruption or simply because they have better knowledge of

[1] Specifically, the effect of subsidies includes firm productivity (Bernini and Pellegrini, 2011; Cin et al., 2017; Li et al., 2022; Dvouletỳ et al., 2021); firm survival rate (Désiage et al., 2010; Zhang and Xu, 2019; Ehrl, 2021); firm innovation (González et al., 2005; Clausen, 2009; Herrera and Sánchez-González, 2013; Howell, 2017).

[2] The industrial policy spending in the United States and Japan was 0.39 percent and 0.50 percent of GDP in 2019, respectively.

[3] Extensive literature has examined the role of hometown favoritism in various contexts, including intergovernmental transfers (Carozzi and Repetto, 2016; Kung and Zhou, 2021); government investment (Öhler and Nunnenkamp, 2014; Fiva and Halse, 2016; Do et al., 2017; Shi, 2021); elections (Fisman et al., 2018, 2020); auditing quality of regional government spending (Chu et al., 2021); and the allocation of foreign aid received (Hodler and Raschky, 2014; Bommer et al., 2022).

these businesses.[4] This study addresses this challenge by focusing on China's 2013 anti-corruption inspections, which were sudden and uniquely uncompromised, coinciding with the political turnover under Xi Jinping.[5] The research investigates whether these robust anti-corruption inspections curtailed the misallocation of subsidies to hometown firms. If subsidies were indeed being allocated to local firms through corrupt politician-firm practices, rather than due to politicians' intimate knowledge of the local firms, a reduction in subsidies should be observed following the anti-corruption crackdown. This research aims to shed light on the mechanisms of hometown favoritism in subsidy allocation and assess the effectiveness of anti-corruption inspections in disrupting such preferential treatment.

China provides a useful setting to study corrupt subsidies in politicians' hometowns. First, the aforementioned anti-corruption efforts can be exploited as a plausibly exogenous and negative shock to the value of political connection, thus allowing me to infer the role of hometown favoritism in facilitating corruption.[6] Second, China's extensive industrial policy spending, combined with the significant discretionary power regional political leaders hold over subsidy allocation—with limited oversight from higher political levels—creates fertile ground for corrupt practices (Pei, 2017; Fang et al., 2023). This discretionary power has often led to widespread *quid pro quo* arrangements between politicians and firms in their hometowns, resulting in the misallocation and misuse of subsidies.

In this paper, I analyze two types of hometown favoritism among political leaders in China: (1) "intra-city favoritism" and (2) "inter-city favoritism." For (1), the intra-city favoritism occurs when a city party secretary or mayor serves in their native prefecture city (hereafter "Hometown Leader").[7] [8] The local-born leader favoritism is a typical measure of

[4]The primary concern in studying hometown favoritism and subsidy allocation arises from the potential simultaneity between a politician's hometown connections and their knowledge of local firms. This creates a challenge in distinguishing whether subsidies are allocated based on corrupt favoritism or legitimate, non-corrupt reasons, such as a deeper understanding of the firms' capacities and needs.

[5]President Xi announced his anti-corruption campaign in 2012. The campaign is widely used in the literature to examine corruption in China. On an unprecedented scale, more than 392 province-level political leaders, 22,000 prefecture-level leaders, and 170,000 at the county-level were investigated and inspected by the central government after the announcement of the anti-corruption campaign. In addition to politicians, firms were also inspected by the central inspection team.

[6]Xi's campaign is considered more sustained and far-reaching than those of his predecessors. Previous anti-corruption efforts, such as those under Jiang Zemin and Hu Jintao, were generally seen as less ambitious and more focused on specific cases or periods. Furthermore, the scale and intensity of Xi's campaign are generally considered to be greater than anti-corruption efforts in most other countries in that time.

[7]A hometown leader refers to an individual who was born, raised, and eventually appointed as the main city-level leader of their native city.

[8]In China's governance structure, (1) the city mayor heads the local government, focusing on administrative and executive functions, day-to-day operations, and public administration; (2) the city party secretary, a high-ranking position within the Communist Party of China (CPC), holds considerable power and primarily ensures the implementation of CPC policies and directives. While the city party secretary's authority often supersedes that of the mayor, decisions on firm subsidy allocation typically involve both officials.

localized hometown favoritism, as it allows for direct observation of preferential treatment within a leader's jurisdiction of origin.[9] For (2), the inter-city favoritism, a novel measure introduced in this study, is based on the number of city party secretaries and mayors a prefecture city produces who serve in other prefecture cities (hereafter "Leader #"). I include this second type to account for the fact that Chinese politicians' influence on government subsidy allocation often extends beyond their current jurisdictions.[10]

I find that the anti-corruption inspection leads to a 20.2% decrease in subsidy allocations for firms located in prefecture cities with at least one local-born leader, while each additional leader a prefecture city produced who served elsewhere corresponds to a 2.2% decrease in subsidies. Back-of-the-envelope calculations indicate the inspection reduced overall firm subsidies by 5.55%, with inter-city favoritism accounting for 4.09% and costing approximately $2.2 billion (0.02% of GDP), demonstrating its quantitative importance. These reductions operate primarily through behavioral change among continuing officials rather than leadership turnover, as politicians respond to fundamentally altered career incentives: hometown favoritism transforms from predicting career advancement to becoming a career liability, revealing disrupted clientelistic equilibria.

Several mechanisms confirm that corruption rather than superior information drives hometown favoritism. First, inspection effects concentrate entirely on opaquely described subsidies while leaving transparent, well-documented subsidies unaffected. This pattern is inconsistent with politicians possessing better local knowledge but consistent with curtailing corrupt discretionary transfers. Machine learning textual analysis confirms subsidies with greater ambiguity decline significantly in affected regions. Second, firms' entertainment and travel costs, widely recognized as disguised bribes (Cai et al., 2011; Fang et al., 2023), predict subsidy receipt in hometown-connected cities before inspections, but this correlation disappears afterward. Effects concentrate among inter-city networks where monetary payments substitute for geographic proximity. Third, longer subsidy descriptions mitigate negative inspection effects, indicating transparency protects allocations from scrutiny. Finally, inspections increase public spending in cities with local-born leaders, implying corrupt subsidies previously crowded out legitimate fiscal expenditure.

Despite significant subsidy reductions, average productivity increases in hometown-

[9] Intra-city favoritism refers to the tendency of politicians to disproportionately benefit their place of origin when assigned leadership roles within their native city or prefecture. This can manifest through preferential allocation of resources, government subsidies, or policy benefits to specific areas or entities within the leader's hometown.

[10] Firms in China can receive government subsidies from various sources, including their local city government and the governments of other cities. This allows for an examination of subsidies allocated by politicians to their hometowns, even when they are not the current leaders of those hometowns. This phenomenon is analogous to "pork barrel" politics in electoral systems, despite the absence of explicit electoral incentives in China.

favored regions, demonstrating that curtailed subsidies were misallocated rather than productivity-enhancing. This productivity improvement operates through resource reallocation: subsidy allocation shifts from larger, politically connected firms toward smaller enterprises, and smaller firms experience disproportionately larger productivity gains. This pattern is consistent with anti-corruption enforcement restoring competitive dynamics by removing advantages that had protected inefficient incumbents. Additional heterogeneity reveals network-based favoritism emerges only among politicians with strong pre-existing ties (geographic proximity, shared workplace experience) who face limited direct promotion competition, while elite networks in the top 1% of leader production experienced the largest reductions, having generated the most substantial corrupt transfers. These patterns collectively demonstrate that politician-firm collusion generates substantial resource misallocation, while credible enforcement restores allocative efficiency.

This paper addresses three strands of literature. First, it relates to the literature on the role of hometown favoritism. As already mentioned, a large amount of literature has analyzed the role of hometown favoritism in various outcomes.

I contribute to this strand of literature in three ways. First, while most existing literature relates to hometown favoritism and outcomes in the public sector, I focus more on private-sector firms, providing novel evidence on the reciprocity between political leaders and hometown-connected firms. Second, I introduce a new measure of hometown favoritism that goes beyond the typical local-born leader metric. By quantifying how many leaders were born in each prefecture-level city while serving in other cities, I find that corruption in a prefecture city increases as it produces more city party secretaries and mayors who influence resource allocation elsewhere. This suggests that hometown connections encourage politician-firm reciprocity and corruption even without geographical proximity. Third, in the context of China, where there are no explicit electoral incentives for politicians to favor local voters, as political leaders are only appointed by their superiors and peers, I find that political leaders still favor their hometown firms, suggesting a role of non-election-related reciprocity between political leaders and hometown-connected firms.

It is important to note that this paper is not the first to examine negative shocks to political connections based on a politician's hometown. In particular, recent work by Chu et al. (2021) employs a similar approach to investigate the effect of hometown favoritism on the number of suspicious government expenditures. Their groundbreaking work focused primarily on public sector interactions. This paper extends and complements their paper by examining the dynamics of public-private sector interactions, thereby offering a more comprehensive understanding of hometown favoritism's effects across diverse economic domains.

The second strand of literature this paper contributes to concerns the effects of anti-

corruption efforts. Previous studies have examined how anti-corruption measures impact firm R&D expenditures and subsidies (Xu and Yano, 2017; Fang et al., 2023); land resource discounts (Chen and Kung, 2019); regional entrepreneurship (Kong and Qin, 2021; Colonnelli and Prem, 2022). My causal evidence demonstrates that firms connected to political leaders suffer from anti-corruption inspections, which is consistent with a large body of literature. The study most closely related to this paper in this literature is Fang et al. (2023), which finds that the allocation of R&D subsidies became more merit-based and less influenced by corruption following the anti-corruption campaign. However, this paper differs by focusing on a crucial yet understudied aspect: the hometown favoritism of political leaders. To the best of my knowledge, no existing paper has examined the relationship between corruption induced by politicians' hometown favoritism and government subsidies.

The third strand of literature to which this paper contributes is the effect of political connection. This body of research has consistently shown that political ties can substantially influence various aspects of firm operations and outcomes: firm returns (Fisman, 2001; Faccio, 2006); access to bank loans and capital markets (Li et al., 2008; Piotroski and Zhang, 2014; Cull et al., 2015); regulatory oversight on firms (Yu and Yuan, 2011; Correia, 2014). My paper extends this literature by examining hometown ties as a significant form of political connection, particularly in the allocation of government subsidies. I introduce two measures of hometown favoritism that captures both direct and indirect political connections, including those of leaders serving outside their birthplace. By focusing on China and examining the effects of anti-corruption efforts, I demonstrate how political favoritism persists in non-democratic contexts and how institutional changes can affect the value of political ties. This paper offers a subtle understanding of the mechanisms through which political connections benefit firms and how these benefits may be vulnerable to anti-corruption efforts.[11]

The paper is organized as follows: Section 2 provides background information on anti-corruption inspections, government subsidies, and the conceptual framework of hometown favoritism. Section 3 describes the data. Section 4 outlines the empirical strategy. Section 5 presents the main results of the paper. Section 6 discusses additional analyses. In appendix, I have a set of robustness checks.

[11]This paper builds upon the seminal work of Fisman (2001) by using a sudden and uniquely uncompromised shock to the value of political connections. While Fisman (2001) relies on news about President Suharto's health as a source of variation, this paper leverages the staggered anti-corruption inspections, which may provide a direct and targeted shock to the value of political connections. This approach may allow for a relatively more precise estimation of the effects of political connections on firm outcomes, addressing some limitations in previous literature.

2 Background

First, I explain the institutional background of the hometown favoritism of political leaders in China. Second, I discuss the allocation process of subsidies in China, which may be linked to potential corruption and rent-seeking activity. Third, I explain President Xi's anti-corruption campaign and the accompanying anti-corruption inspections.

2.1 Hometown Favoritism in China

The hometown favoritism in China stems from two main aspects and has influenced the interactions between bureaucracies and firms in China for decades. The first pertains to social identity and self-categorization that arise from cultural proximity and geographical affinity, facilitating information flows between Chinese political leaders and firms. Less Information asymmetry is among individuals born in the same origin with similar beliefs, languages, and ethnicities, as it is simpler to observe peers' behavior and preferences.[12] A large amount of literature also supports the role of affinity and proximity.[13]

The second aspect, which is the main focus of this study, concerns the reduced transaction costs of corruption in politicians' hometowns. If political leaders currently serve in their hometown city, the cost of colluding with local firms could be lower than that of other non-hometown political leaders. This aspect stems primarily from less information asymmetry, which fosters trust between political leaders and firms in prefecture cities with hometown leaders. As corruption-related deals are always costly (Shleifer and Vishny, 1994; Lambsdorff, 2002; Dong and Torgler, 2013; Kwong, 2015), reducing transaction costs could encourage collusion between political leaders and firms and provide preferential treatment to in-group individuals for personal benefits.

In a parallel vein, if a prefecture city could produce more leaders, firms headquartered in that prefecture may receive preferential treatment over counterparts for two reasons. First, leaders from other cities influence the allocation decision of the leader serving in their hometown. Local leaders are responsible for allocation decisions; however, the influence of leaders from other cities on these decisions is noteworthy due to the interplay of local leaders' social and political incentives. For example, the Chinese leader's promotion partially depends on connections amongst political leaders because local People's Congresses

[12]For example, Shi (2021) finds that Chinese political leaders reduce the entry barriers for investors from their hometown due to lower information asymmetry.

[13]For example, effects on labor market outcomes (Goldin and Rouse, 2000; Bertrand and Mullainathan, 2004; Charles and Guryan, 2008); cultural proximity on loan outcomes (Fafchamps, 2000; Blanchflower et al., 2003; Fisman, 2003; Pope and Sydnor, 2011; Du et al., 2017; Fisman et al., 2017). For example, Fisman et al. (2017) find that cultural proximity decreases information friction between lenders and borrowers. As a natural result, lenders tend to lend money to borrowers who are close to them.

hold the political elections in leaders' jurisdictions. Semi-retired political leaders mainly fill the seats of members of the local People's Congresses, e.g., the person who served other cities as a leader and is now a member of the congress in her hometown prefecture city. Favor exchange amongst political leaders is thus reasonable and can affect public resource allocation. Second, firms linked to a larger cohort of generated leaders from the hometown city could expand their resource access. This extends beyond resources allocated solely by their headquarters' prefecture city, encompassing resources derived from other cities as well.

Hometown favoritism has also drawn the attention of the Chinese government. Wang Qishan, a loyalist of President Xi, reshuffled the Discipline Inspection Commissions (DIC) leadership in most prefecture cities and made the DICs have fewer hometown directors after 2013.[14]

2.2 Subsidy Allocation in China

China adopts large-scale subsidy policies to improve firm performance. In 2018, the total allocated subsidies to Chinese listed firms were RMB 153.8 billion ($23.6 billion), which suggests that, on average, each listed firm could receive more than RMB 38.5 million ($5.92 million) in government subsidies.

Nearly all subsidy allocations to listed firms are originally lodged at the prefecture city level.[15] The granting of subsidies is mainly decided by a prefecture city's finance department, especially the amount of each distribution and the receiving firms. However, as subsidies for firms are a type of discretionary government spending, the allocation decision made by the department must have the local government's stamp of approval. This approval, however, is contingent on the city's party secretary and mayor, who hold prominent positions at the prefecture city level. These political leaders wield the authority to sanction or dismiss subsidy applications from the finance department. The practical power given to political leaders leads to a situation where they are directly involved in the selection process for the subsidy beneficiaries, which creates sufficient incentives for firms to bribe and cultivate a good relationship with those political leaders.

Furthermore, local governments' decentralization and high degree of autonomy have further intensified corruption in subsidy allocation. According to Pei (2017), the economic reform that started at the end of the 1970s led the local governments to obtain more administrative power over public resources, e.g., government funds. While the local government must submit the decision of subsidy allocation to the higher-level government, e.g., the

[14]Discipline Inspection Commissions (DIC) is the regional subordinate of CCDI, which plays a role similar to CCDI.

[15]Figure C.1 shows the organizational chart of the allocation process of subsidy.

provincial government, the lenient upper-level oversight, and the regional autonomy make it almost impossible for submitted applications to be rejected by the provincial government. [16] The approval process for subsidy applications follows a pyramidal structure, but city-level leaders play a role as gatekeepers of applications. Fang et al. (2023) corroborate the preceding point by noting that the higher-level government rarely denies resource allocation decisions made by the lower-level government. Therefore, the criteria for getting government subsidies or the selection of beneficiaries could fluctuate among different regional governments and be altered to suit a leader's preferences.

2.3 Anti-corruption Inspections Under Xi Jinping

Coinciding with the political transition that brought President Xi Jinping to leadership, China initiated a far-reaching anti-corruption campaign in late 2012. This campaign rapidly became a defining characteristic of his administration and was distinguished by its "sudden and uniquely uncompromised" nature, setting it apart from previous efforts. The design and execution of these inspections have been recognized as providing a significant exogenous shock, valuable for analyzing corruption dynamics; this perspective is integral to the present study and reflects the campaign's extensive use within the academic literature that examines corruption in China. The Central Commission for Discipline Inspection (CCDI), the Communist Party's apex internal control institution, served as the primary enforcement body, responsible for identifying and addressing corruption among party members and government officials.

Beginning in late 2012, the CCDI started to publicly announce its investigations into prominent corruption cases that involved both high-ranking government political leaders and directors of various firms. The operational vigor of the campaign significantly intensified from May 2013, when the CCDI mobilized numerous inspection teams to perform comprehensive audits and inspections across diverse provinces and state-owned enterprises. These inspections were systematically implemented in a staggered sequence, ensuring that all provinces were subjected to this exacting scrutiny by the close of 2015.[17] This methodical oversight was also extended to the corporate domain, where business firms, in addition to political figures, were directly investigated by these centrally dispatched inspection teams.

The sheer scale and meticulousness of these anti-corruption inspections were without

[16] Government in China consists of four separate levels: (1) central government; (2) provincial government; (3) prefectural (city) government; (4) county government. A prefecture city is a smaller administrative division of a province.

[17] Detailed information concerning the anti-corruption inspections, including the specific timeline and the various phases of inspections, is provided in Table D.4.

precedent in China's contemporary history. From the time President Xi assumed office, the central authorities initiated probes into an extraordinary count of officials across all echelons of government: 392 provincial-level leaders, 22,000 prefecture-level leaders, and 170,000 county-level leaders were subject to investigation. This undertaking represented a far more sustained and deeply penetrating effort than the anti-corruption initiatives of his predecessors, Jiang Zemin and Hu Jintao. Those prior campaigns were generally regarded as less ambitious in their scope and frequently concentrated on particular, isolated cases or were confined to limited durations, rather than pursuing systemic reforms. The intensity and comprehensive reach of Xi's campaign were also widely considered to surpass those of anti-corruption efforts undertaken in most other nations during the same period.

Furthermore, Xi's anti-corruption campaign imposes stringent oversight on local government accounting practices in China. The heightened scrutiny implies that any suspicious subsidy allocation to a firm could trigger investigations by the CCDI. This increased probability of investigation and subsequent expulsion for political leaders fundamentally alters the risk calculus associated with such fiscal practices at the sub-national level.

3 Data

In this section, I present the data construction and source as well as the most salient trends in my data.

3.1 Subsidy Allocation Data

The main data set used in this paper consists of subsidy allocation information merged with publicly listed Chinese firm-level data from 2009 to 2018.[18] These data sets are sourced from the China Stock Market & Accounting Research Database (CSMAR). I excluded firms flagged for special status or financial abnormalities, such as those designated as ST and PT.[19]

For each subsidy allocation, the data includes information on the recipient firm, a description of the subsidy, the amount allocated, and the year of allocation. I further refine the subsidy allocation data by excluding allocations described as "tax refund." In China, tax refunds for listed firms are considered a type of government subsidy, so they were removed from the data set. Finally, I compute the log value of the firm's allocated subsidies in year t, denoted as Log(Subsidy).

[18] Chinese listed firms only began disclosing detailed information on their government subsidies after 2007. Therefore, this study focuses on the period from 2009 to 2018.

[19] An ST firm is under "special treatment" by the authorities due to financial abnormalities. PT stands for "particular transfer," indicating that a firm is nearing exit.

The sample is restricted to listed firms due to the availability of reliable financial data and detailed disclosure regarding subsidy sources. Listed firms differ systematically from non-listed firms: they are generally larger, more productive, and subject to stricter regulatory oversight including mandatory information disclosure and independent auditing. Despite this limitation, listed firms represent an economically significant sector that accounts for approximately 27.8% of total direct government subsidies allocated to Chinese firms (Branstetter et al., 2023). Moreover, the institutional setting provides a demanding test of the hometown favoritism hypothesis, as the high-transparency environment makes detecting favoritism more difficult and engaging in overt rent-seeking more costly. Whether the findings generalize to non-listed firms remains an open question, and I caution against extrapolating the precise magnitudes to the broader population of Chinese firms.

Additionally, the main data set includes firm-level information such as firm address, age, assets, state ownership status, return on assets, and leverage. I use firm-level data prior to the inspections to avoid changes in these variables resulting from anti-corruption inspections. These variables are also obtained from CSMAR. Figure C.3b shows the distribution of Log(Subsidy) by two periods: (1) 2009–2013 (pre-inspection); (2) 2014–2018 (post-inspection).[20]

3.2 Hometown Favoritism Status from Political Leader's Biography

In this section, I discuss the two variables measuring the degree of hometown favoritism by politicians.

3.2.1 Intra-city Hometown Favoritism by Political Leaders

I include an indicator variable for a prefecture city that had a local-born city party secretary or mayor, denoted as Hometown Leader$_{i,c,2009-2013}$, indicating firm i is located in prefec-

[20]The key difference between the two periods lies in the peak region (Log(Subsidy) values 10–12), where the 2014–2018 distribution (red line) displays more pronounced spikes and variability compared to the smoother 2009–2013 distribution (blue line). This increased "bumpiness" in the later period, particularly noticeable around Log(Subsidy) value of 11 where the red line reaches slightly higher density peaks, suggests a shift toward more structured or discretized subsidy allocation, with funds becoming concentrated at specific threshold amounts. Additionally, there is a subtle increase in density on the left side of the distribution (around values 8–10) in the 2014–2018 period, potentially indicating a small rise in the proportion of lower-value subsidies being awarded compared to the earlier period.

ture city c that had at least one local-born party secretary or mayor during 2009 to 2013.[21] To construct the variable, I manually matched leaders' serving places and their birthplaces in the period 2009-2013, following the leaders' biographies.[22] The variable takes the value of one for a prefecture city if a leader's jurisdiction aligns with her birthplace, and she held leadership in her hometown from 2009 to 2013. Suppose some firms headquartered in that prefecture city, the firm is hometown-preferred by the hometown leader, and the indicator for hometown favoritism applies to those firms.[23] As firms and leaders were in the same city, the proximity may foster stronger personal connections and potential favoritism, hence the term "intra-city hometown favoritism." Figure 2a illustrates the prefecture cities that had a hometown leader in China. Figure C.2 shows the distribution for this type of hometown favoritism.

3.2.2 Inter-city Hometown Favoritism by Political Leaders

I further consider the intensity of hometown favoritism by calculating the average number of party secretaries and mayor who were born in prefecture city c and serving in other cities during 2009 to 2013, denoting as Leader $\#_{i,c,2009-2013}$.[24][25] While the measure, Leader $\#_{i,c,2009-2013}$, is different from Hometown Leader$_{i,c,2009-2013}$, it captures the degree of hometown favoritism within each prefecture. It is natural to assume that a prefecture city may receive more preferential treatment from political leaders if it produces more political leaders, as discussed in the Background section. Figure 2 illustrates whether prefecture c housed a local-born leader who served within that prefecture, alongside the average leader count produced by prefecture c from 2009 to 2013. Since firms and politicians have no geographical proximity in this type of hometown connection, it is termed "inter-city hometown favoritism." Figure 2b illustrates the number of city-level leaders that each prefecture city produced. Figure C.2 shows the distribution for this type of hometown favoritism.

[21] Figure 1 illustrates the trend of city-level leaders serving in their hometowns from 2008 to 2019, which shows a decreasing trend (about 40% from 2013 to 2016) after the anti-corruption inspection in 2013. Therefore, I employ the pre-existing hometown favoritism status before anti-corruption inspections, serving as a strategy to mitigate potential endogeneity concerns. This approach helps address potential confounding factors arising from the association between changes in hometown leaders and changes in anti-corruption efforts.

[22] Some biographies can be accessed from https://ldzl.people.com.cn, a website for political leader information in China. For the missing information, I manually obtained it from Baidu, which is the Chinese version of Google.

[23] Regarding the firm address, the database provides two addresses: (1) where the firm is registered and (2) where the firm is currently located. In this paper, the firm's headquartered address is used.

[24] For example, if prefecture city c produced 5 city-level leaders serving in other cities from 2009-2013, then Leader $\#_{i,c,2009-2013}$ is 1, as $\frac{5\,\text{leaders}}{5\,\text{years}}$.

[25] The variable construction of Leader $\#_{i,c,2009-2013}$ excludes hometown leaders, as I focus on the generated leaders who were serving in other cities.

3.3 Anti-corruption Inspections

The other main variable is the staggered anti-corruption inspection. As the inspection is at the province-level, I have the variable $\text{Inspection}_{p,t}$, which takes 1 in affected province p for both the investigation year t and the following years, and 0 for all other years.

3.4 Other Regional Level Variables

I use the following time-varying provincial control variables: (1) GDP per capita, (2) Log(Fiscal Income); (3) Log(Population). The Chinese Statistics Year Books provide the data.

3.5 Summary Statistics and Balance Tests

Panel A of Table 1 and Table 2 presents the summary statistics for the main variables used in this study. Panel B of these tables provides descriptive statistics for the firm and regional controls.

In Table 1, I compare firm-year and province-year attributes between observations with a connection to a hometown leader and those without. Interestingly, the value of Log(Subsidy) for firms connected to a hometown leader is lower than for firms not connected to a hometown leader. This finding might seem counterintuitive to the hypothesis that firms receive greater subsidies from hometown leaders, but these statistics are not conditional on firm size, nor is the sample divided into periods before and after the inspection. Additionally, I find that firms connected to a hometown leader have a higher probability of undergoing anti-corruption inspections, suggesting that regions with hometown leaders might be inspected earlier by the central government. This raises a concern about the exogeneity assumption of the inspections. To address this, I conduct a balance test (see Table D.6) for the early-inspected provinces, which suggests no significant regional differences between early-inspected provinces and late-inspected provinces.[26]

Furthermore, in Table 2, I compare statistics of firms based in prefecture cities that generated higher leader counts versus those that did not. There is a natural concern that the political turnover of a leader to serve their hometown may be associated with regional patterns. To address this, I conduct a balance test for hometown favoritism status, examining whether the assignments of Hometown Leader and Leader # correlate with regional characteristics.[27] The results are shown in Table D.5. In columns (1)-(3),

[26] I investigate the impact of province-level attributes in 2012 on the likelihood of CCDI inspection in 2013. Notably, none of the province-level attributes from 2012 appear to significantly influence the occurrence of anti-corruption inspections in 2013. Therefore, the concern about the inspection may not be severe here.

[27] In the balance test, I did not use time-invariant hometown favoritism status, e.g., $\textit{Leader \#}_{i,c,2009-2013}$.

the dependent variable is **1**(Hometown Leader), and the independent variables include regional attributes such as GDP per capita, number of firms, population, fiscal income, and others. The only statistically significant coefficient is the employment at the city level using samples before the anti-corruption inspection, but it is only significant at the 10 percent level. In columns (4)-(6), Leader # is regressed on the regional attributes, and after including all controls and fixed effects, no variables are found to be significantly correlated with the assignment of Leader #.

3.6 Correlation between Hometown Favoritism and Allocated Subsidies

Before examining the anti-corruption effect, I first establish that hometown-connected firms received preferential treatment in subsidy allocation prior to the inspection. Table D.3 presents correlations between hometown connections and subsidies in the pre-inspection period (2009–2012), controlling for firm and regional characteristics.

Column (1) examines intra-city hometown favoritism. Firms headquartered in cities where a local-born leader served received 16.8% more subsidies than non-connected firms ($\exp(0.155) - 1 = 0.168$), after controlling for firm size, profitability, leverage, ownership, and regional economic conditions. Column (2) examines inter-city hometown favoritism. Each additional leader produced by the firm's city and serving elsewhere is associated with a 0.9% increase in subsidies. These results confirm that hometown favoritism existed before the anti-corruption campaign.

4 Empirical Strategy

I use the Difference-in-Differences method to estimate the effect of anti-corruption inspections on firms' subsidies. The main regression equation takes the following forms:

$$\begin{aligned} Log(Subsidy_{a,i,c,t}) = {} & \alpha_1 Hometown_{c,2009-2013} + \alpha_2 Inspection_{p,t} \\ & + \alpha_3 Hometown_{c,2009-2013} \times Inspection_{p,t} \\ & + X'\beta + \delta_i + \tau_t + \epsilon_{a,i,c,t} \end{aligned} \tag{1}$$

where the subscripts a, i, p, c, and t denote subsidy allocation, firm, province, prefecture city, and year, respectively. Hometown $\text{Leader}_{i,c,2009-2013}$denotes one of the two hometown favoritism status variables: (1) Hometown $\text{Leader}_{i,c,2009-2013}$ denotes whether prefecture city c had at least one local-born city party secretary or mayor during 2009-2013; (2) Leader $\#_{i,c,2009-2013}$ denotes the average count of city-level leader produced by prefecture c during 2009-2013. $\text{Inspection}_{p,t}$ is the staggered anti-corruption inspection, which

takes 1 in affected province p for both the investigation year t and the following years, and 0 for all other years. α_3 is the coefficient of interest, capturing the effects of curbing corruption on hometown-connected firms' subsidy allocation. The vector of the control variables $X'\beta$ encompasses firm scale, whether state-owned enterprise, firm age, leverage, return on asset, province-level GDP per capita, and fiscal income. δ_i denotes firm fixed effects; τ_t denotes year fixed effects; $\epsilon_{i,t}$ is the error term. Standard errors are clustered at the province-year.

5 Main Results

In this section, I report the baseline results on subsidies for firms with hometown favoritism under anti-corruption efforts.

5.1 Baseline Results

5.1.1 Intra-city Hometown Favoritism: Hometown Leader

I now examine whether the anti-corruption inspection reduced subsidies for firms headquartered in prefecture cities that had at least one local-born city mayor or party secretary from 2009 to 2013 (intra-city hometown favoritism).

Panel A of Table 3 presents the results. In column (1), I include pre-firm controls interacting with anti-corruption inspection, firm fixed effects, and year fixed effects. In column (2), I add time-varying regional controls: GDP per capita and fiscal income. In column (3), I further include 2-digit industry-year fixed effects. Across all specifications, the coefficients on Hometown Leader $\times$ Inspection are negative and statistically significant at the 1 percent level.

In the full specification (column 3), the estimated coefficient is -0.226, indicating that firms headquartered in cities with a local-born leader experienced a 20.2% reduction in allocated subsidies after the anti-corruption inspection. Compared to the pre-inspection premium of 16.8% documented in Table D.3, the post-inspection reduction exceeds the original advantage, suggesting that the anti-corruption campaign fully eliminated the preferential treatment that hometown-connected firms previously enjoyed.

5.1.2 Inter-city Hometown Favoritism: Leader

I next examine the anti-corruption effect on subsidies for firms headquartered in prefecture cities that produced more leaders serving elsewhere from 2009 to 2013 (inter-city hometown favoritism).

Panel B of Table 3 presents the results using Leader # interacting with Inspection. Across all specifications, the coefficients are negative and statistically significant. In the full specification (column 3), I find that each additional city-level leader produced by a prefecture city is associated with a 2.2% reduction in subsidies for firms headquartered in that city after the anti-corruption inspection.

Compared to the pre-inspection premium of 0.9% per leader documented in Table D.3, the post-inspection reduction is approximately 2.4 times the original advantage, indicating that the anti-corruption campaign fully eliminated the subsidy premium associated with inter-city political networks.

Both sets of results demonstrate that the anti-corruption inspection substantially reduced hometown favoritism in subsidy allocation. The magnitude of the reductions, which exceeds the pre-existing premiums in both cases, suggests that the campaign did not merely attenuate favoritism but effectively eliminated it. These baseline findings are consistent with Proposition 2 of the conceptual framework in the Appendix.

5.2 Event-study

The differential trends across firms with hometown favoritisms may bias the estimates of the anti-corruption-related analyses. To address this concern, I visualize the dynamic effects of the interaction between Hometown Leader (Leader #) interacting with the relative year to 2013, with the outcome variable Log(Subsidy) and the 95 percent confidence intervals. I follow Roth et al. (2023) to assess unconditional parallel trend assumption (exclude covariates). The following is the event-study equation:

$$Log(Subsidy_{a,i,c,t}) = \sum_{\tau=-4}^{5} Hometown_{i,c,2009-2013} \times \beta_{\tau}(Periods\,since\,the\,Inspection_{t}) + \delta_i + \tau_t + \epsilon_{a,i,c,t} \tag{2}$$

where the subscripts and definition of variables are the same as above. I set the year of inspection as the reference year, and all coefficients are relative to this year. As plotted in Figure 3, the estimated coefficients β_{τ} are fairly constant throughout 2009-2013. It observes a sudden decrease in the estimated coefficients after the anti-corruption inspection, which is a natural consequence of the anti-corruption campaign.

5.3 Firm-level Subsidies

The data reveals that following anti-corruption inspections, firms with connections to local government officials received smaller individual subsidy amounts. While these findings are conditional on firms that continued to receive subsidies, a broader question emerges: What was the overall impact of these inspections on the total subsidy distribution across all firms, including those that may have stopped receiving subsidies altogether?

It is possible that while politicians reduced the size of each subsidy disbursement, they may have increased the frequency of allocations. In other words, firms could have received multiple smaller subsidies over a given period, resulting in a comparable or even higher total subsidy amount compared to before the inspections.[28]

To explore this possibility, I collapse the data on subsidy allocation levels to the firm level and re-estimate Equation 1.[29] Table 4 reports the results, displaying the effect of anti-corruption inspections on the aggregate amount of subsidies received by firms.

Panel A focuses on intra-city hometown favoritism, examining firms headquartered in a prefecture city that had a hometown leader. The key interaction term here is Hometown Leader $\times$ Inspection. In column (3), the specification that includes all the fixed effects and controls, I find that firms headquartered in cities with a hometown leader experience a significant reduction in total subsidies, approximately 13.1% (exp(-0.14) - 1 = 0.8694 - 1 = -0.1306 = -13.06%), following anti-corruption inspections. Although the estimated coefficients are slightly smaller than those in the baseline specification using subsidy allocation level data (a reduction of 20%), it still suggests that firms received fewer subsidies from the hometown leader.

Panel B examines the effect on firms headquartered in cities that produced more leaders serving in other cities. In column (3), I find that firms in cities that produced one additional leader serving in other cities experienced a 2.9% (exp(-0.029) - 1 = 0.9714 - 1 = -0.0286 = -2.86%) reduction in total subsidies following inspections, which is a similar estimated coefficient to the coefficients using subsidy allocation level data. Overall, the firm-level results align with the earlier findings on individual subsidy allocations, indicating a

[28] For example, if a firm was previously receiving an annual subsidy of $100,000 from local officials with whom they had connections. After the anti-corruption inspections, that single $100,000 subsidy may have been split into four separate subsidies of $25,000 each, disbursed quarterly, in order to mitigate the adverse effect of allocating subsidies. While the individual subsidy amounts decreased, suggesting a reduction in favoritism, the firm could have ultimately received the same $100,000 total through multiple smaller allocations. This would allow politicians to maintain support for connected firms while creating an appearance of propriety.

[29] In the baseline specification, the analysis of anti-corruption effects on hometown-connected firms focuses on the amount of each individual subsidy allocation. However, this effect is conditional on the subsidy allocation amounts for firms without any political connections. When examining the firm-level data, the comparison group is further expanded to include firms that did not receive any subsidies over the entire data period being analyzed.

notable impact of anti-corruption inspections on reducing subsidies linked to hometown favoritisms by politicians.

5.4 Aggregate Impact of Inspections on Corrupt Subsidies

5.4.1 Aggregate Corrupt Subsidies

In this subsection, I analyze the aggregate impact of the anti-corruption inspection on subsidies, considering how many government subsidies to firms are affected through the channels of two types of hometown favoritism using the firm-level results.

First, based on back-of-the-envelope calculations, the anti-corruption inspection reduces subsidies by 1.46% (13.1% × 0.177, the probability of being connected to a hometown leader, × 0.629, the probability of inspections) through the channel of intra-city hometown favoritism.

Second, similar calculations indicate that the anti-corruption inspection reduces subsidies by 4.43% (2.9% × 2.428 × 0.629) through the inter-city hometown favoritism channel. The anti-corruption inspection reduces 5.88% corrupt subsidies for firms.

Overall, these findings underline the more pronounced overall impact of anti-corruption inspection on subsidies associated with the inter-city hometown favoritism by politicians, highlighting its significant role in shaping corruption dynamics between politicians and firms.

5.4.2 The Costs of Ex-ante Corrupt Subsidies

The estimates on the aggregate reduction in corrupt subsidies indicate a significant decline of over 5.55% following anti-corruption inspections. This reduction has substantial financial implications. Based on the actual subsidy amounts provided in 2018, the cost associated with corrupt subsidies to these firms is calculated to be approximately 14.39 billion yuan (around $2.2 billion and 0.02% of China's GDP).[30] This highlights the considerable financial burden imposed by corrupt practices in subsidy allocation, specifically through the channel of politicians' hometown favoritisms.

[30]The calculation is as follows: (1) Intra-city hometown favoritism (Hometown Leader): 13.1% × 0.177 × 153.8 billion yuan (subsidies in 2018) ≈ 3.56 billion yuan; (2) Inter-city hometown favoritism (Leader #): 2.9% × 2.428 × 153.8 billion yuan (subsidies in 2018) ≈ 10.83 billion yuan.

5.5 Heterogeneity in Corrupt Subsidy Allocation

5.5.1 Potential Corrupt Subsidies vs Potential Legitimate Subsidies

In this subsection, I investigate the correlation between the level of transparency in subsidy descriptions and the allocation amounts based on hometown favoritism. The findings are categorized into four types of subsidies: (1) descriptions with fewer than 15 Chinese characters (mean value), (2) descriptions with more than the mean value, (3) descriptions without specific program information, and (4) descriptions with specific program information. Table D.2 provides an example of subsidy allocation to a firm.

I categorize subsidies with shorter descriptions as opaque or potentially "corrupt," while those with longer descriptions are considered more transparent. The rationale for this classification is that detailed descriptions typically indicate higher levels of transparency and accountability. Subsidies with minimal descriptions are deemed opaque or potentially "corrupt" due to their lack of sufficient information to justify the allocation of funds. Such opaque descriptions suggest that these subsidies may not be tied to specific projects but are rather the result of corruption between politicians and firms. Politicians may allocate corrupt subsidies by fabricating a short description, but cannot fabricate a detailed description that includes a specific program.

I estimate Equation 1, restricting the sample to the four subgroups mentioned above. Panel A of Table 5 reports the results for intra-city hometown favoritism (Figure 4 shows the event-study results). In Column (1), the coefficient on the interaction term Hometown Leader $\times$ Inspection is -0.289 for subsidies with less than the average length of description, suggesting that hometown leaders reduce subsidies to firms by 25.1% after the anti-corruption inspection when the subsidy descriptions are opaque. However, as shown in Column (2), this anti-corruption effect becomes smaller and statistically insignificant for subsidies with more transparent descriptions. In Column (3), I restrict the sample to subsidies without descriptions containing specific program information, and the estimated coefficient is -0.259, suggesting a 22.8% decrease in those subsidies after the anti-corruption inspection, which is a similar pattern to the results in Column (1). In Column (4), I find no significant effect of anti-corruption inspections on specific subsidies.

Panel B of Table 5 reports the results for inter-city hometown favoritism. A similar pattern emerges, where the interaction term Leader # $\times$ Inspection is -0.029, suggesting a 2.9% reduction in subsidies for firms in prefectures that produced one additional city-level leader when the description length is below the mean. Consistent with the results for Hometown Leader, there is no significant effect of anti-corruption inspections on subsidies with longer descriptions. In Column (3), the coefficient for subsidies without specific program information is -0.027. In Column (4), no significant effect of anti-corruption

inspections is found on subsidies with specific program information.

To validate that the 15-character threshold is not arbitrary, I conduct two complementary tests. First, I use a machine learning-based corruption score to independently assess subsidy opacity. Figure C.8 demonstrates that description length strongly predicts ML-based corruption risk: subsidies with shorter descriptions exhibit systematically higher corruption scores, and this relationship holds even after controlling for firm fixed effects. Second, I test the sensitivity of results to alternative cutoffs using thresholds of 10, 15, and 20 characters. Figure C.7 shows that the pattern is consistent across all specifications: anti-corruption effects are concentrated among subsidies with shorter descriptions regardless of the exact cutoff chosen, while subsidies with longer descriptions show no significant effects.

Overall, the findings support the notion that prior to the anti-corruption inspection, thsoe subsidies with vague description may have been utilized as a means for corruption and nepotism benefiting hometown-connected firms. The anti-corruption inspections appear to have curbed this practice by reducing opaquely described subsidies to such firms.

5.5.2 Mitigating Effect of Length of Subsidy Description

I further test the linear relationship between the length of subsidy description and the amount allocated to politicians' hometowns after the anti-corruption inspection, for the purpose of examining whether increased transparency directly influences the allocation amounts. That is, whether politicians allocate more transparent subsidies in terms of amount to their hometown firms. To explore this, I expand upon the previous findings by considering not only the binary categorization of description length but also the continuous variable of description length. I estimate the following equation:

$$\begin{aligned} Log(Subsidy_{a,i,c,t}) =& \alpha_1 Hometown_{c,2009-2013} + \alpha_2 Inspection_{p,t} + \alpha_3 Description\ Length_{a,i,c,t} \\ &+ \alpha_4 Hometown_{c,2009-2013} \times Inspection_{p,t} \\ &+ \alpha_5 Hometown_{c,2009-2013} \times Description\ Length_{a,i,c,t} \\ &+ \alpha_6 Inspection_{p,t} \times Description\ Length_{a,i,c,t} \\ &+ \alpha_7 Hometown_{c,2009-2013} \times Inspection_{p,t} \times Description\ Length_{a,i,c,t} \\ &+ X'\beta + \gamma_c + \tau_t + \epsilon_{a,i,c,t} \end{aligned} \tag{3}$$

where subscripts, control variables, and fixed effects are defined in the same way as the baseline equation. Description Length denotes the length (in Chinese characters).

In Table D.8, I interact both Hometown Leader (Leader #) and Inspection with Length of Subsidy Description. In columns (1) - (3) of Panel A, I find a significant mitigating effect of the length of the subsidy description on the amount of allocated subsidies in the hometown of a hometown leader. In column (3), where I include all the control variables and fixed effects, the estimated coefficient suggests that for each additional Chinese character present in the subsidy description, the amount of that subsidy increases by 0.9%.

In columns (1) - (3) of Panel B, which report the results for inter-city hometown favoritism, I find a similar pattern to that for intra-city hometown favoritism, though the estimated coefficient becomes insignificant after controlling for industry-year fixed effects.

Overall, these findings suggest that increased transparency in subsidy descriptions, measured by the length of the description, is associated with higher subsidy amounts allocated to hometown firms after the anti-corruption inspection. Transparency in subsidy allocation is an important factor influencing the allocated amount because allocating larger subsidies to hometown firms via less transparent descriptions may pose a risk to politicians, especially during periods of heightened anti-corruption efforts.

5.5.3 Textual Analysis: Machine Learning in Subsidy Description

To further investigate the nature of subsidies allocated to firms in politicians' hometowns, I employ machine learning techniques to analyze the textual descriptions accompanying these subsidies. This analysis aims to identify linguistic patterns that may indicate corrupt practices, such as vague or ambiguous descriptions.

The subsidy descriptions range from brief, generic phrases such as "Government Subsidy" to more detailed explanations that include specific projects, timelines, or funding sources. This variation in detail provides a basis for distinguishing between potentially corrupt and legitimate subsidies.[31]

I utilize a machine learning model to analyze the textual descriptions of subsidies.[32] The model incorporates features such as the length of the description, the presence of specific keywords related to subsidy purposes, and semantic analysis to assess the clarity and specificity of the text. The model identifies each subsidy allocation description, and

[31]For example, in the training process, I consider that subsidy descriptions were scored on a continuous scale ranging from high corruption risk (higher score) to low corruption risk (lower score). Vague descriptions lacking project details, funding amounts, named recipients, or oversight mechanisms (e.g., "Government subsidy") received higher scores. Descriptions with partial specificity about timeline, limited funding disclosure, or generic recipients (e.g., "Regional development subsidy of $2M for business growth next fiscal year") received middle-range scores. Descriptions with clear purpose, defined timeline, full funding disclosure, named recipients, and explicit oversight (e.g., "Rural Electrification Project grant of $4.2M for solar panel installation in villages, 2015") received lower scores.

[32]Specifically, I use Bidirectional encoder representations from transformers (BERT) and the above examples to generate the score.

generate a score for each subsidy. Then I aggregate the score at the firm-level by taking the average value of each subsidy allocation score. The output subsidy score with higher values indicate greater ambiguity or potential corruption. Importantly, this score is calculated for each subsidy based solely on its description, without weighting by the subsidy amount.

After obtained the score, I re-estimate the equation at the firm-level but Log(Corrupt Subsidy Score) as the main outcome variable. Table D.9 reports the results. The analysis reveals that for intra-city favoritism, anti-corruption inspections lead to a significant reduction in subsidy scores, suggesting a decrease in the prevalence of ambiguous descriptions by 5.8% for all subsidies that a firm receive in one year. This aligns with the hypothesis that direct political control facilitates overt corruption, which is mitigated by inspections.

In contrast, for inter-city favoritism, where leaders influence subsidies from outside their hometowns, the subsidy scores do not exhibit a significant decrease post-inspection, as shown in Panel B Table D.9, despite an overall reduction in subsidies. However, the heterogeneity analysis in Table 5 reveals a nuanced effect: the amount of corrupt subsidies–those with shorter, more ambiguous descriptions–decreases after inspections. This discrepancy between the score and the amount prompts further investigation.

The key to this puzzle lies in the subsidy score's construction. Since the score is an unweighted measure based only on textual ambiguity, it does not account for the monetary value of each subsidy. For inter-city favoritism, inspections may reduce the amounts of the largest, most corrupt subsidies (those with high scores) without significantly altering the number or average ambiguity of remaining subsidies.

For example, if a firm previously received several large subsidies with ambiguous descriptions and smaller ones with varying clarity, post-inspection reductions in the large subsidies' amounts would lower the total corrupt subsidy amount. However, if smaller subsidies persist with similar scores, the average subsidy score across all allocations might not decrease significantly. This pattern is particularly plausible in the context of inter-city favoritism, where the transactional costs of arranging subsidies across jurisdictional boundaries may make small-value allocations economically inefficient. Unlike intra-city corruption where leaders can easily distribute numerous small subsidies with minimal overhead, inter-city influence likely focuses on securing fewer but larger corrupt allocations to justify the additional coordination required. Consequently, when inspections occur, these large, ambiguous subsidies are precisely the ones reduced or eliminated, while the remaining smaller subsidies (which may serve legitimate purposes) continue with similar descriptive patterns, explaining the observed decrease in amounts without significant change in average ambiguity scores.

For intra-city favoritism, the reduction in corrupt subsidies appears more uniform, affecting both the number and nature of ambiguous subsidies, leading to a clear drop

in the average score. In contrast, intercity favoritism's indirect influence may result in corruption that is less consistently tied to textual ambiguity, with inspections trimming the largest corrupt allocations while leaving smaller ones intact. This differential impact explains why the amount decreases but the score does not.

Despite these limitations, the textual analysis provides valuable insights into the nature of subsidy allocations. The significant reduction in ambiguous descriptions for intra-city favoritism following anti-corruption inspections supports the notion that such favoritism is associated with detectable corrupt practices. The lack of a similar effect for inter-city favoritism suggests that corruption in this context may operate through different channels, highlighting the need for multifaceted approaches to studying and combating corruption.

5.6 Firm Rent-Seeking Expenditures and Subsidy Allocation

I now examine whether firm-level corruption-related expenditures help generate government subsidies, and whether this relationship weakens after anti-corruption inspections. Following the literature, I use "Entertainment and Travel Costs (ETC)" as a proxy for rent-seeking expenditures. ETCs are publicly reported in firms' accounting books and are commonly used to disguise bribes to officials (Cai et al., 2011; Fang, 2023).[33] While ETCs contain legitimate business expenses, firms have considerable latitude to use them for corrupt purposes, making this measure widely adopted in the corruption literature (Cai et al., 2011; Huang et al., 2017; Giannetti et al., 2021; Fang et al., 2023).

To validate this measure, I first examine whether ETCs decreased in hometown-favored regions after anti-corruption inspections. Table D.10 shows that for inter-city favoritism, ETC (scaled by firm revenue and profit) declined significantly after inspections, suggesting that corrupt money is a very important mechanism in generating subsidies from other cities. The null results for intra-city favoritism suggest that geographical proximity enables alternative forms of corruption not captured by ETCs.

I then estimate the following specification to test whether ETCs generate subsidies pre-inspection and post-inspection:

[33] According to Fang et al. (2023), some hotels in China provide *baijiu* (Chinese liquor) or cigarettes that can be added to room bills as business trip expenses—items widely used as currency in bribes.

$$
\begin{aligned}
Log(Subsidy_{a,i,c,t}) =& \alpha_1 Hometown_{c,2009-2013} + \alpha_2 Inspection_{p,t} + \alpha_3 ETC_{i,c,t} \\
&+ \alpha_4 Hometown_{c,2009-2013} \times Inspection_{p,t} \\
&+ \alpha_5 Hometown_{c,2009-2013} \times ETC_{i,c,t} + \alpha_6 Inspection_{p,t} \times ETC_{i,c,t} \\
&+ \alpha_7 Hometown_{c,2009-2013} \times Inspection_{p,t} \times ETC_{i,c,t} \\
&+ X'\beta + \gamma_i + \delta_{jt} + \epsilon_{a,i,c,t}
\end{aligned} \tag{4}
$$

where $ETC_{i,c,t}$ denotes firm entertainment and travel costs scaled by profit.[34] The coefficient of interest is α_4, capturing changes in the ETC-subsidy relationship in hometown-favored regions after inspections.

Table 6 presents results across different subsidy types. Panel A (intra-city favoritism) shows no significant relationship between ETCs and subsidies for any subsidy type. However, Panel B (inter-city favoritism) reveals that ETCs significantly influence subsidy allocation, particularly for opaque subsidies. In Column (1), the coefficient on Leader # $\times$ ETC is 0.035, indicating that higher ETCs generate more subsidies in cities producing more leaders. The triple interaction coefficient of -0.035 shows this effect completely disappears after inspections. Columns (2) and (4) confirm this pattern is concentrated among subsidies with shorter descriptions and those lacking specific program information—precisely the subsidies most susceptible to manipulation.

The contrasting results between intra-city and inter-city favoritism provide important insights into corruption mechanisms. For inter-city favoritism, firms lack geographical proximity to leaders serving elsewhere, necessitating corrupt expenditures to build connections and facilitate cross-jurisdictional resource transfers. In contrast, when leaders serve in their hometowns, geographical proximity enables alternative corruption channels—such as land transactions (Chen and Kung, 2019; Fang et al., 2022)—that would not appear in firm ETCs. These findings suggest that the nature of corrupt interactions varies systematically with the spatial relationship between politicians and connected firms.

This interpretation is consistent with the network heterogeneity results in Table 8. Panel A showed that anti-corruption effects on inter-city favoritism concentrate among politicians with strong ties—those serving within the same province or sharing workplace connections. The ETC findings provide a complementary mechanism: strong network ties facilitate the transfer of resources across jurisdictions, but maintaining these relationships still requires firm-level investments in rent-seeking. ETCs serve as the monetary channel through which firms access and cultivate these political networks. This also aligns with Table 9, which showed that cities with elite networks (top 1%) experienced the largest subsidy reductions.

[34] Results are similar using unscaled or log-transformed ETCs.

Cities producing many leaders likely developed more extensive rent-seeking channels involving higher ETCs to coordinate resource flows across multiple jurisdictions, making them particularly exposed when the anti-corruption campaign disrupted these monetary transfers.

6 Additional Analyses

In this section, I explore the potential mechanisms underlying the reduction in subsidies for hometown-connected firms.

6.1 Intensive vs. Extensive Margin: Behavioral Change or Politician Turnover?

Did the anti-corruption inspection reduce hometown favoritism through behavioral change among incumbent leaders (intensive margin) or through leader removal (extensive margin)? This distinction is crucial for understanding the mechanism of anti-corruption enforcement. If effects operate through the extensive margin, campaigns work primarily by purging corrupt officials. Conversely, if effects operate through the intensive margin, campaigns succeed by altering the incentives and perceived detection risk of continuing leaders (Olken, 2007).

To distinguish these channels, I identify politicians in office during 2011–2012 (immediately before the inspection) and classify them into two mutually exclusive groups: *Stayers* (politicians who remained in office through 2014) and *Leavers* (politicians who departed during the inspection). I then re-estimate the baseline specifications separately for each group, restricting the sample to 2011–2018 to focus on the period most relevant for this identification.

Table 7 presents the results. For intra-city favoritism (Panel A), both Stayer (-0.266, $p < 0.05$) and Leaver (-0.209, $p < 0.05$) coefficients are negative and statistically significant. However, with only 12 Stayer cities and 25 Leaver cities, the sample is too small to draw strong inferences about the relative magnitudes. The point estimates suggest the Stayer effect is somewhat larger, consistent with behavioral change being an important channel, though both the intensive and extensive margins may have operated for intra-city favoritism given the direct connection between leaders and local firms.

For inter-city favoritism (Panel B), where statistical power is sufficient (169 Stayer cities, 84 Leaver cities), the decomposition provides clear evidence. The Stayer coefficient is -0.067 and highly significant ($p < 0.01$), while the Leaver coefficient is 0.015 and statistically insignificant. This demonstrates that the reduction in network-based favoritism

operated through behavioral change among continuing politicians. When hometown leaders remained in office, they reduced favoritism in response to the anti-corruption campaign. When hometown leaders left office, there was no effect because replacement leaders typically did not have hometown connections to the same cities.

These findings demonstrate that China's anti-corruption campaign succeeded primarily by inducing behavioral change among incumbent officials. The evidence is particularly clear for inter-city favoritism, where subsidy reductions occurred exclusively among cities where hometown leaders remained in office. This indicates that the enforcement changed official behavior even in the absence of leadership turnover, suggesting that deterrence effects extend beyond the punishment of specific individuals. The dominance of the intensive margin parallels Olken (2007), who shows that increased monitoring leads to substantial reductions in missing expenditures among incumbent officials. The results carry important policy implications: anti-corruption campaigns can be effective even when political constraints limit the removal of connected officials, provided that enforcement credibly alters the risk-return calculus facing incumbents (Becker, 1968).

6.2 Political Reciprocity and Competition in Political Network

The baseline results indicate that inter-city hometown favoritism operates through political networks that span jurisdictional boundaries. Yet such reciprocity is not guaranteed. China's promotion tournament generates strong competitive incentives among local officials (Li and Zhou, 2005; Xu, 2011; Fang et al., 2025), which may crowd out cooperation. This section examines the conditions under which network ties sustain reciprocity despite these competitive pressures.

I study heterogeneity along two dimensions. First, I assess whether strong pre-existing ties facilitate reciprocal exchange by lowering coordination costs and fostering trust: (1) geographic proximity; (2) shared workplace experience; (3) Shanghai Gang factional affiliation; and (4) Communist Youth League of China (CYLC) membership.[35] If reciprocity is network-based, the anti-corruption inspection should induce larger declines in hometown favoritism where such ties are stronger, reflecting the dismantling of previously active reciprocal arrangements. Second, I examine whether direct promotion competition inhibits cooperation. If politicians are close rivals, little reciprocity should arise ex-ante, and the inspection should have limited effects.

[35]Factional affiliation is proxied by membership in the Shanghai Gang or the Communist Youth League of China (CYLC). For the Shanghai Gang, I classify city leaders as members if they have work or birth ties to Shanghai and documented political connections to former President Zemin Jiang (Francois et al., 2023). For CYLC, I identify politicians who served in leadership positions within the Communist Youth League, a major pathway for political advancement in China.

For each dimension, I decompose the leader network measure (Leader #) into mutually exclusive continuous components. Panel A distinguishes strong from weak ties based on geographic proximity (within- versus cross-province appointments), prior workplace connections to city c, Shanghai Gang membership, and CYLC affiliation. Panel B stratifies politician pairs by the intensity of promotion competition, measured by predicted promotion probabilities following Wang et al. (2020); Fang et al. (2025), age cohort similarity, and tenure overlap.[36] Age cohorts and tenure are defined using a five-year window, reflecting institutional features of China's cadre management system in which promotion eligibility, rotation cycles, and evaluation horizons are typically organized in multi-year intervals. Politicians within five years of age or tenure are therefore more likely to compete for the same promotion opportunities. Each component is interacted with the inspection indicator and estimated using the baseline specification in Equation (1).

Table 8 reports the results. Panel A shows that reciprocity operates only through strong ties. Column 1 indicates that the inspection effect is concentrated among within-province networks, with no effect for cross-province ties. Column 2 reveals an even larger effect for shared workplace connections, while leaders without prior professional links exhibit no response. These patterns underscore the central role of repeated interaction and embedded professional networks in sustaining reciprocal favoritism. Column 3 yields a negative but imprecise estimate for Shanghai Gang ties, providing suggestive but inconclusive evidence due to limited variation (approximately 5% of the sample). Column 4 examines CYLC membership, another important factional network in Chinese politics. The point estimate suggests stronger effects for CYLC-affiliated leaders compared to non-CYLC leaders, though the point estimates are not statistically significant, likely due to limited variation in CYLC membership within the sample.

Panel B shows that direct promotion competition suppresses reciprocity. Column 1 demonstrates that the inspection effect is driven by leader pairs with sufficiently different promotion prospects, while pairs with similar predicted promotion probabilities exhibit no statistically significant response. This suggests that politicians competing for the same promotion do not engage in reciprocal subsidy allocation ex ante, and thus show no inspection effect. Column 2 reveals a similar pattern across age cohorts: reductions in hometown favoritism arise only among leaders from different cohorts who are unlikely to compete for the same positions. Column 3 provides the strongest evidence: leaders with dissimilar tenure lengths experience a substantial decline in favoritism, whereas leaders

[36]Predicted promotion probability is estimated following Wang et al. (2020) and Fang et al. (2025) by regressing next-term promotion on politician characteristics (gender, starting age interacted with deputy provincial status, and education) with city and year fixed effects. For each pair of exported and hometown leaders, I compute the absolute difference in predicted promotion probability and split pairs at the sample median into similar-prospect (below median) and different-prospect (above median) groups.

with comparable tenure show no effect.

Taken together, these results reconcile political reciprocity with promotion competition. Network-based favoritism emerges only when leaders share strong pre-existing ties—particularly through workplace connections and geographic proximity—and face limited direct rivalry for career advancement. When either condition is absent, competitive incentives dominate behavior. Accordingly, the anti-corruption campaign reduced hometown favoritism precisely in settings where reciprocity had previously been operative, namely among non-competing leaders embedded in strong political networks.

6.3 Non-Linear Effects of Political Network Strength

In the baseline specification, I treat political network strength (Leader #) as a linear predictor of hometown favoritism. However, theoretical literature suggests that political influence often exhibits threshold effects. Models of collective action indicate that a group must reach a "critical mass" to effectively coordinate and exert pressure (Esteban and Ray, 2001). Similarly, Acemoglu et al. (2008) argue that loose networks must coalesce into "winning coalitions" of sufficient size to enforce reciprocal exchanges. In this context, inter-city favoritism likely requires a minimum threshold of network strength to operate effectively.

To test for such non-linearities, I divide cities into four groups based on the number of leaders they produced during the pre-inspection period: Bottom 33% (reference group), Mid Network (33rd to 66th percentile), High Network (66th to 99th percentile), and Elite Network (top 1%). I then re-estimate the baseline specification with each category interacted with the inspection indicator. Table 9 presents the results. Column 3 reveals a clear threshold pattern: relative to the weakest-network cities, mid-network cities experienced a 12% increase in subsidies, high-network cities experienced 10% reductions, and elite-network cities experienced the largest reductions.

This pattern reveals three distinct regimes. Cities below the 66th percentile lacked sufficient network strength for effective inter-city favoritism, while those above had crossed the threshold for effective coordination. Among high and elite network cities, anti-corruption effects increase monotonically with network strength, demonstrating that stronger networks had engaged in more extensive favoritism and thus experienced larger disruptions when the campaign curtailed these practices.

Figure C.9 assesses robustness to the choice of binning cutoffs by replicating the analysis using terciles, quartiles, and quintiles, each retaining the top 1% as an elite group. The same qualitative pattern emerges across all specifications: coefficients are near zero or positive for lower network strength groups and turn increasingly negative above approximately

the 60th to 70th percentile, confirming that the threshold pattern is not an artifact of the specific cutoffs chosen.

6.4 Anti-corruption Effects on Firm Productivity and Market Competition

The preceding analyses document that hometown favoritism distorted subsidy allocation and that the anti-corruption campaign reduced this distortion. A natural question follows: did these reductions in corrupt subsidies harm or help regional economic performance? If hometown-connected subsidies were productively employed, their reduction should decrease firm productivity. However, if these subsidies were misallocated to inefficient connected firms at the expense of more productive competitors, their removal could improve aggregate productivity by restoring market competition. This section tests these competing hypotheses.

6.4.1 Regional Productivity Effects

To investigate the productivity consequences of anti-corruption efforts, I re-estimate the city-level Equation 6 using city-level average firm revenue, employment, and labor productivity (revenue per worker) as dependent variables. Table 10 presents the results.

Contrary to what one might expect from subsidy reductions, I find that anti-corruption inspections had a statistically significant *positive* effect on regional firm productivity. Panel A shows that in municipalities where a hometown leader was present, average firm productivity (revenue per worker) increased by 10.3% ($\exp(0.098) - 1 = 0.103$) following the inspection, driven by higher firm revenue without significant employment reductions. Panel B extends this finding to inter-city favoritism: each additional leader originating from a prefecture city is associated with a 1.9% increase in productivity post-inspection.

These results are striking because hometown-connected regions experienced substantial subsidy reductions yet saw productivity improvements rather than declines. This pattern aligns with Proposition 3 from the conceptual framework, which predicts that productivity can increase following anti-corruption inspections if the efficiency gains from curtailing misallocated subsidies outweigh the direct resource loss. The evidence suggests that subsidies channeled through hometown connections were not enhancing firm productivity but rather sustaining inefficient allocation. By disrupting these corrupt flows, the anti-corruption campaign improved resource allocation and spurred more productive economic activity.

6.4.2 Market Competition: Firm Heterogeneity in Subsidy Allocation

The regional productivity gains documented above raise a question about the underlying mechanism: how did the anti-corruption campaign affect subsidy allocation across different types of firms? Table 11 addresses this by examining heterogeneity along three dimensions: firm size, ownership type, and pre-inspection subsidy receipt.

Columns 1 and 2 interact the baseline specification with firm scale. For both intra-city and inter-city favoritism, the triple interaction with firm scale is negative and significant, indicating that smaller firms in politically connected regions received relatively more subsidies following the inspection. To illustrate the distribution of these effects across the firm size distribution, Figure C.6 plots the implied treatment effect at each percentile of firm scale, computed as the linear combination of the two-way and triple interaction coefficients. The implied effect is positive for firms below approximately the 90th percentile of the size distribution and turns negative for the largest firms, confirming that subsidy reallocation was concentrated among smaller and less politically connected firms.

Columns 3 and 4 examine SOE status. The triple interaction is negative for both intra-city and inter-city specifications, though statistically insignificant for intra-city favoritism, suggesting that the reallocation was not systematically driven by ownership type. Columns 5 and 6 interact the baseline with an indicator for firms that received above-median subsidies in the pre-inspection period. The triple interaction is negative and significant in both specifications, confirming that firms which had previously benefited from politically connected subsidy allocation experienced the largest reductions after the inspection.

Overall, these findings indicate that the anti-corruption campaign leveled the playing field in subsidy allocation. Before the inspection, larger and previously connected firms captured disproportionate subsidies through rent-seeking. The inspection disrupted these channels, redistributing resources toward smaller and less politically connected firms that had previously been crowded out. This pattern is consistent with the reduction in entertainment and travel costs documented in Table D.10.

6.4.3 Firm Heterogeneity in Productivity Gains

The regional productivity gains documented above raise a question about the underlying mechanism: which firms benefited from the anti-corruption campaign? If larger firms previously used political connections to maintain market positions despite lower efficiency, smaller firms should experience the largest productivity gains once these advantages are removed.

Table 12 tests this prediction using firm-level TFP estimated via five different meth-

ods.[37] The findings strongly support the competition restoration hypothesis. Because the specification includes a triple interaction with firm scale (measured by log total assets), the two-way interaction coefficients represent the effect for a hypothetical firm with zero log assets, which is outside the data range. The economically relevant effects at typical firm sizes combine both the two-way and triple interaction coefficients. Using the OLS estimates from Panel A, at the sample mean of log assets (21.6), the implied productivity increase is approximately 9%, consistent with the regional productivity effects documented in Table 10.

The triple interaction with firm scale is consistently negative and significant across all five TFP estimation methods in both panels, revealing substantial heterogeneity by firm size. In Panel A (intra-city favoritism), the productivity effect becomes zero at approximately the 90th percentile of the firm size distribution, with smaller firms experiencing substantial gains while the largest firms experienced zero or negative effects. Panel B (inter-city favoritism) shows a consistent pattern. These results demonstrate that smaller, less politically connected firms benefited most from the anti-corruption campaign, while large firms that had previously maintained market positions through political networking lost their artificial advantages.

Table D.13 examines heterogeneity by ownership type. The triple interaction with the SOE indicator is consistently insignificant across all specifications in both panels. This finding reflects the well-documented productivity disadvantages of state-owned enterprises in China relative to private firms, which stem from softer budget constraints, weaker managerial incentives, and non-commercial objectives that persistently distort resource allocation (Hsieh and Klenow, 2009; Song et al., 2011). Given these structural differences, the productivity effects of the anti-corruption campaign operated primarily through firm size rather than ownership type.

7 Conclusion

This paper provides systematic evidence that hometown favoritism operates as a pervasive channel for corrupt subsidy allocation in China, and that credible anti-corruption enforcement can restore allocative efficiency. Exploiting the 2013 anti-corruption inspections as a plausibly exogenous shock, I document that firms in cities with local-born leaders experience a 20.2% reduction in subsidies, while each additional city-level leader a prefec-

[37] Firm-level TFP is estimated using five approaches: ordinary least squares (OLS), Levinsohn and Petrin (2003) (LP), Ackerberg et al. (2015) (ACF), Wooldridge (2009) (WRDG), and industry-specific production functions (IND). All methods use revenue as output and capital and labor as inputs. The multiple estimation approaches address well-known concerns about simultaneity and selection bias in production function estimation.

ture produces corresponds to a 2.2% decline in subsidies to local firms. These reductions concentrate entirely on opaque, discretionary subsidies while leaving transparent allocations unaffected, demonstrating that corruption rather than superior information drives hometown favoritism. Back-of-envelope calculations indicate these corrupt transfers cost approximately $2.2 billion annually, with inter-city political networks accounting for three-quarters of misallocated resources. The campaign operates primarily through behavioral change among continuing officials rather than leadership turnover, as politicians respond to fundamentally altered career incentives: hometown favoritism transforms from a career asset into a career liability post-inspection.

Importantly, aggregate productivity increases in hometown-favored regions following subsidy reductions, as anti-corruption enforcement disrupts clientelistic relationships that had protected inefficient politically-connected firms at the expense of more productive competitors. Smaller firms experience disproportionate gains in both subsidy access and productivity, revealing that corrupt allocations had systematically distorted competition. These findings carry important implications for industrial policy effectiveness: discretionary resource allocation creates opportunities for corruption that misallocate resources toward politically connected but inefficient firms, particularly when informal institutions rooted in hometown networks enable reciprocal exchange across bureaucratic boundaries.

Credible enforcement demonstrates the capacity to correct these distortions. These findings show both the mechanisms through which political connections distort resource allocation and the institutional remedies that restore efficiency—providing insights for harnessing industrial policy's potential while safeguarding against political capture.

References

Acemoglu, Daron, Georgy Egorov, and Konstantin Sonin, "Coalition formation in non-democracies," *The Review of Economic Studies*, 2008, *75* (4), 987–1009.

Ackerberg, Daniel A, Kevin Caves, and Garth Frazer, "Identification properties of recent production function estimators," *Econometrica*, 2015, *83* (6), 2411–2451.

Becker, Gary S, "Crime and punishment: An economic approach," *Journal of political economy*, 1968, *76* (2), 169–217.

Bernini, Cristina and Guido Pellegrini, "How are growth and productivity in private firms affected by public subsidy? Evidence from a regional policy," *Regional Science and Urban Economics*, 2011, *41* (3), 253–265.

Bertrand, Marianne and Sendhil Mullainathan, "Are Emily and Greg more employable than Lakisha and Jamal? A field experiment on labor market discrimination," *American economic review*, 2004, *94* (4), 991–1013.

Besley, Timothy, Rohini Pande, and Vijayendra Rao, "Just rewards? Local politics and public resource allocation in South India," *The World Bank Economic Review*, 2012, *26* (2), 191–216.

Blanchflower, David G, Phillip B Levine, and David J Zimmerman, "Discrimination in the small-business credit market," *Review of Economics and Statistics*, 2003, *85* (4), 930–943.

Bommer, Christian, Axel Dreher, and Marcello Perez-Alvarez, "Home bias in humanitarian aid: The role of regional favoritism in the allocation of international disaster relief," *Journal of Public Economics*, 2022, *208*, 104604.

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess, "Revisiting event-study designs: robust and efficient estimation," *Review of Economic Studies*, 2024, p. rdae007.

Branstetter, Lee G, Guangwei Li, and Mengjia Ren, "Picking winners? Government subsidies and firm productivity in China," *Journal of Comparative Economics*, 2023, *51* (4), 1186–1199.

Cai, Hongbin, Hanming Fang, and Lixin Colin Xu, "Eat, drink, firms, government: An investigation of corruption from the entertainment and travel costs of Chinese firms," *The Journal of Law and Economics*, 2011, *54* (1), 55–78.

Callaway, Brantly and Pedro HC Sant'Anna, "Difference-in-differences with multiple time periods," *Journal of econometrics*, 2021, *225* (2), 200–230.

Carozzi, Felipe and Luca Repetto, "Sending the pork home: Birth town bias in transfers to Italian municipalities," *Journal of Public Economics*, 2016, *134*, 42–52.

Cavallo, Eduardo, Sebastian Galiani, Ilan Noy, and Juan Pantano, "Catastrophic natural disasters and economic growth," *Review of Economics and Statistics*, 2013, *95* (5), 1549–1561.

Chaisemartin, Clément De and Xavier d'Haultfoeuille, "Two-way fixed effects estimators with heterogeneous treatment effects," *American Economic Review*, 2020, *110* (9), 2964–2996.

Charles, Kerwin Kofi and Jonathan Guryan, "Prejudice and wages: an empirical assessment of Becker's The Economics of Discrimination," *Journal of political economy*, 2008, *116* (5), 773–809.

Chen, Ting and James Kai sing Kung, "Busting the "Princelings": The campaign against corruption in China's primary land market," *The Quarterly Journal of Economics*, 2019, *134* (1), 185–226.

Chu, Jian, Raymond Fisman, Songtao Tan, and Yongxiang Wang, "Hometown ties and the quality of government monitoring: evidence from rotation of Chinese auditors," *American Economic Journal: Applied Economics*, 2021, *13* (3), 176–201.

Cin, Beom Cheol, Young Jun Kim, and Nicholas S Vonortas, "The impact of public R&D subsidy on small firm productivity: evidence from Korean SMEs," *Small Business Economics*, 2017, *48* (2), 345–360.

Clausen, Tommy H, "Do subsidies have positive impacts on R&D and innovation activities at the firm level?," *Structural change and economic dynamics*, 2009, *20* (4), 239–253.

Colonnelli, Emanuele and Mounu Prem, "Corruption and firms," *The Review of Economic Studies*, 2022, *89* (2), 695–732.

Correia, Maria M, "Political connections and SEC enforcement," *Journal of accounting and Economics*, 2014, *57* (2-3), 241–262.

Cull, Robert, Wei Li, Bo Sun, and Lixin Colin Xu, "Government connections and financial constraints: Evidence from a large representative sample of Chinese firms," *Journal of corporate finance*, 2015, *32*, 271–294.

Désiage, Lionel Jacques, Richard Duhautois, and Dominique Redor, "Do public subsidies have an impact on new firm survival? An empirical study with French data," *An Empirical Study with French Data (March 26, 2010)*, 2010.

Do, Quoc-Anh, Kieu-Trang Nguyen, and Anh N Tran, "One mandarin benefits the whole clan: hometown favoritism in an authoritarian regime," *American Economic Journal: Applied Economics*, 2017, *9* (4), 1–29.

Dong, Bin and Benno Torgler, "Causes of corruption: Evidence from China," *China Economic Review*, 2013, *26*, 152–169.

Du, Qianqian, Frank Yu, and Xiaoyun Yu, "Cultural proximity and the processing of financial information," *Journal of Financial and Quantitative Analysis*, 2017, *52* (6), 2703–2726.

Dvouletỳ, Ondřej, Stjepan Srhoj, and Smaranda Pantea, "Public SME grants and firm performance in European Union: A systematic review of empirical evidence," *Small Business Economics*, 2021, *57* (1), 243–263.

Ehrl, Philipp, "Live large or die young: subsidized loans and firm survival in Brazil," *Empirical Economics*, 2021, *61* (6), 3479–3503.

Esteban, Joan and Debraj Ray, "Collective action and the group size paradox," *American political science review*, 2001, *95* (3), 663–672.

Faccio, Mara, "Politically connected firms," *American economic review*, 2006, *96* (1), 369–386.

Fafchamps, Marcel, "Ethnicity and credit in African manufacturing," *Journal of Development economics*, 2000, *61* (1), 205–235.

Fang, Hanming, "Measurements, Determinants and Causes of Corruption: Lessons from China's Anti-Corruption Campaign," 2023.

_ , Jing Wu, Rongjie Zhang, and Li-An Zhou, "Understanding the Resurgence of the SOEs in China: Evidence from the Real Estate Sector," Technical Report, National Bureau of Economic Research 2022.

_ , Ming Li, and Zenan Wu, "Tournament-style political competition and local protectionism: theory and evidence from China," *Journal of Public Economics*, 2025, *248*, 105421.

Fang, Lily, Josh Lerner, Chaopeng Wu, and Qi Zhang, "Anticorruption, government subsidies, and innovation: Evidence from China," *Management Science*, 2023, *69* (8), 4363–4388.

Ferraz, Claudio and Frederico Finan, "Electoral accountability and corruption: Evidence from the audits of local governments," *American Economic Review*, 2011, *101* (4), 1274–1311.

Fisman, Raymond, "Estimating the value of political connections," *American economic review*, 2001, *91* (4), 1095–1102.

_ , "Ethnic ties and the provision of credit: Relationship-level evidence from African firms," *Advances in Economic Analysis & Policy*, 2003, *3* (1), 1211.

_ **, Daniel Paravisini, and Vikrant Vig**, "Cultural proximity and loan outcomes," *American Economic Review*, 2017, *107* (2), 457–92.

_ **, Jing Shi, Yongxiang Wang, and Rong Xu**, "Social ties and favoritism in Chinese science," *Journal of Political Economy*, 2018, *126* (3), 1134–1171.

_ **,** _ **,** _ **, and Weixing Wu**, "Social ties and the selection of China's political elite," *American Economic Review*, 2020, *110* (6), 1752–81.

Fiva, Jon H and Askill H Halse, "Local favoritism in at-large proportional representation systems," *Journal of Public Economics*, 2016, *143*, 15–26.

Francois, Patrick, Francesco Trebbi, and Kairong Xiao, "Factions in nondemocracies: Theory and evidence from the Chinese Communist Party," *Econometrica*, 2023, *91* (2), 565–603.

Giannetti, Mariassunta, Guanmin Liao, Jiaxing You, and Xiaoyun Yu, "The externalities of corruption: Evidence from entrepreneurial firms in China," *Review of Finance*, 2021, *25* (3), 629–667.

Goldin, Claudia and Cecilia Rouse, "Orchestrating impartiality: The impact of" blind" auditions on female musicians," *American economic review*, 2000, *90* (4), 715–741.

González, Xulia, Jordi Jaumandreu, and Consuelo Pazó, "Barriers to innovation and subsidy effectiveness," *RAND Journal of economics*, 2005, pp. 930–950.

Goodman-Bacon, Andrew, "Difference-in-differences with variation in treatment timing," *Journal of Econometrics*, 2021, *225* (2), 254–277.

Herrera, Liliana and Gloria Sánchez-González, "Firm size and innovation policy," *International Small Business Journal*, 2013, *31* (2), 137–155.

Hodler, Roland and Paul A Raschky, "Regional favoritism," *The Quarterly Journal of Economics*, 2014, *129* (2), 995–1033.

Howell, Sabrina T, "Financing innovation: Evidence from R&D grants," *American economic review*, 2017, *107* (4), 1136–1164.

Hsieh, Chang-Tai and Peter J Klenow, "Misallocation and manufacturing TFP in China and India," *The Quarterly journal of economics*, 2009, *124* (4), 1403–1448.

Huang, Zhangkai, Lixing Li, Guangrong Ma, and Lixin Colin Xu, "Hayek, local information, and commanding heights: Decentralizing state-owned enterprises in China," *American Economic Review*, 2017, *107* (8), 2455–78.

Jiang, Junyan and Muyang Zhang, "Friends with benefits: Patronage networks and distributive politics in China," *Journal of Public Economics*, 2020, *184*, 104143.

Kennedy, Scott, Gerard DiPippo, and Ilaria Mazzocco, *Red ink: estimating Chinese industrial policy spending in comparative perspective*, Center for Strategic & International Studies, 2022.

Kitschelt, Herbert and Steven I Wilkinson, *Patrons, clients and policies: Patterns of democratic accountability and political competition*, Cambridge University Press, 2007.

Kong, Dongmin and Ni Qin, "China's anticorruption campaign and entrepreneurship," *The Journal of Law and Economics*, 2021, *64* (1), 153–180.

Kwong, Julia, *The political economy of corruption in China*, Routledge, 2015.

Lambsdorff, Johann Graf, "Corruption and rent-seeking," *Public choice*, 2002, *113* (1), 97–125.

Levinsohn, James and Amil Petrin, "Estimating production functions using inputs to control for unobservables," *The Review of Economic Studies*, 2003, *70* (2), 317–341.

Li, Hongbin and Li-An Zhou, "Political turnover and economic performance: the incentive role of personnel control in China," *Journal of public economics*, 2005, *89* (9-10), 1743–1762.

_ , Lingsheng Meng, Qian Wang, and Li-An Zhou, "Political connections, financing and firm performance: Evidence from Chinese private firms," *Journal of development economics*, 2008, *87* (2), 283–299.

Li, Mingyang, Man Jin, and Subal C Kumbhakar, "Do subsidies increase firm productivity? Evidence from Chinese manufacturing enterprises," *European Journal of Operational Research*, 2022.

Öhler, Hannes and Peter Nunnenkamp, "Needs-based targeting or favoritism? The regional allocation of multilateral aid within recipient countries," *Kyklos*, 2014, *67* (3), 420–446.

Olken, Benjamin A, "Monitoring corruption: evidence from a field experiment in Indonesia," *Journal of political Economy*, 2007, *115* (2), 200–249.

Pei, Minxin, *China's crony capitalism*, Harvard University Press, 2017.

Piotroski, Joseph D and Tianyu Zhang, "Politicians and the IPO decision: The impact of impending political promotions on IPO activity in China," *Journal of Financial Economics*, 2014, *111* (1), 111–136.

Pope, Devin G and Justin R Sydnor, "What's in a Picture? Evidence of Discrimination from Prosper. com," *Journal of Human resources*, 2011, *46* (1), 53–92.

Rambachan, Ashesh and Jonathan Roth, "A more credible approach to parallel trends," *Review of Economic Studies*, 2023, p. rdad018.

Roth, Jonathan, "Pretest with caution: Event-study estimates after testing for parallel trends," *American Economic Review: Insights*, 2022, *4* (3), 305–322.

_ , Pedro HC Sant'Anna, Alyssa Bilinski, and John Poe, "What's trending in difference-in-differences? A synthesis of the recent econometrics literature," *Journal of Econometrics*, 2023.

Shi, Xiangyu, "Hometown favoritism and intercity investment networks in China," *Available at SSRN 3745064*, 2021.

_ , Tianyang Xi, Xiaobo Zhang, and Yifan Zhang, ""Moving umbrella": Bureaucratic transfers and the comovement of interregional investments in China," *Journal of Development Economics*, 2021, *153*, 102717.

Shleifer, Andrei and Robert W Vishny, "Politicians and firms," *The quarterly journal of economics*, 1994, *109* (4), 995–1025.

sing Kung, James Kai and Titi Zhou, "Political elites and hometown favoritism in famine-stricken China," *Journal of Comparative Economics*, 2021, *49* (1), 22–37.

Song, Zheng, Kjetil Storesletten, and Fabrizio Zilibotti, "Growing like china," *American economic review*, 2011, *101* (1), 196–233.

Stokes, Susan C, Thad Dunning, and Marcelo Nazareno, *Brokers, voters, and clientelism: The puzzle of distributive politics*, Cambridge University Press, 2013.

Sun, Liyang and Sarah Abraham, "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects," *Journal of Econometrics*, 2021, *225* (2), 175–199.

Wang, Zhi, Qinghua Zhang, and Li-An Zhou, "Career incentives of city leaders and urban spatial expansion in China," *Review of Economics and Statistics*, 2020, *102* (5), 897–911.

Wooldridge, Jeffrey M, "On estimating firm-level production functions using proxy variables to control for unobservables," *Economics Letters*, 2009, *104* (3), 112–114.

Xu, Chenggang, "The fundamental institutions of China's reforms and development," *Journal of economic literature*, 2011, *49* (4), 1076–1151.

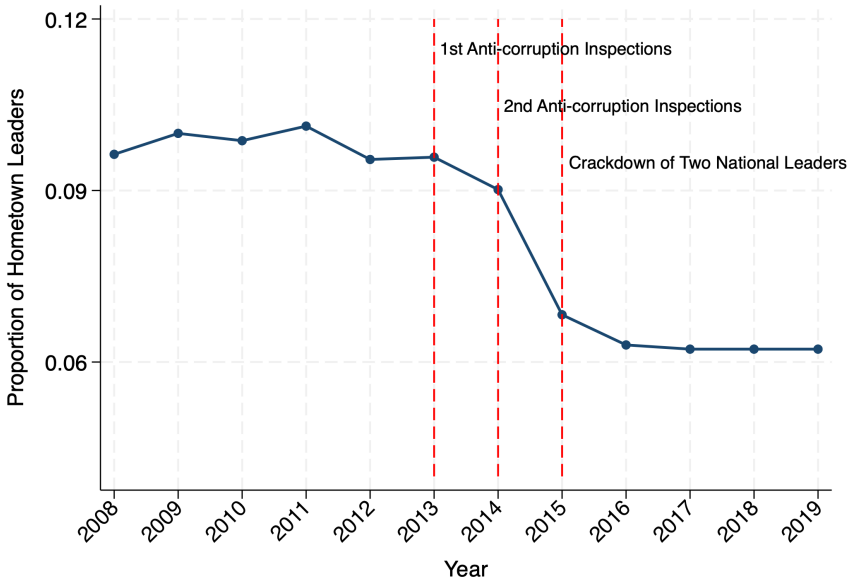
Xu, Gang and Go Yano, "How does anti-corruption affect corporate innovation? Evidence from recent anti-corruption efforts in China," *Journal of comparative Economics*, 2017, *45* (3), 498–519.

Yu, Jianfeng and Yu Yuan, "Investor sentiment and the mean–variance relation," *Journal of Financial Economics*, 2011, *100* (2), 367–381.

Zhang, Dongyang and Gang Xu, "Does government subsidy affect firm survival? Evidence from Chinese manufacturing firms," *Emerging Markets Finance and Trade*, 2019, *55* (11), 2628–2651.

8 Figures

Figure 1: Hometown Status and Anti-corruption: Whether a City has a Local-born Party Secretary or Mayor over the Years

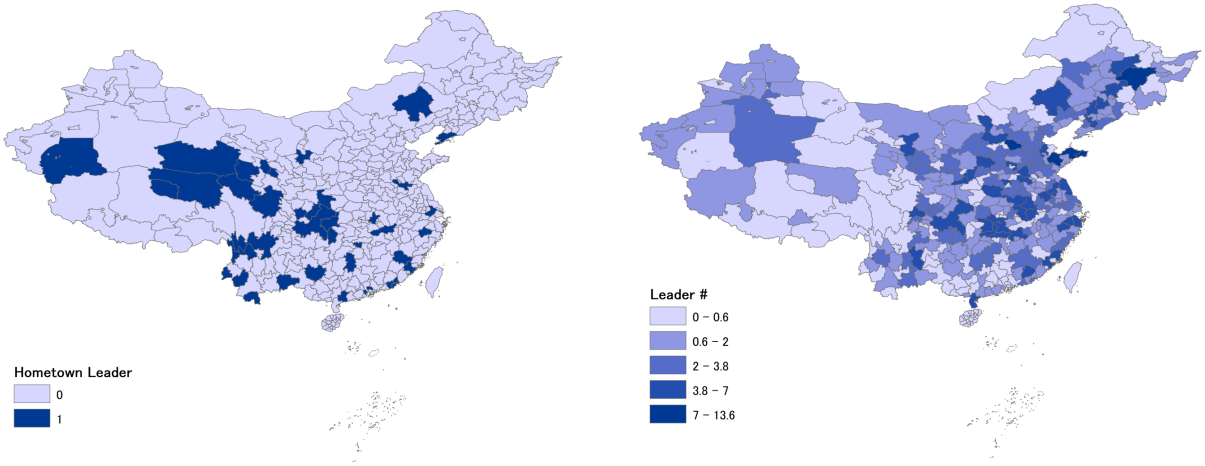


Notes: The figure illustrates the changes in the presence of political leaders serving in their hometowns.

Figure 2: The Prefectures with a Hometown Leader & the Birthplace Counts of Chinese Leaders in 2009

(a) Prefectures with a Hometown Leader from 2009-2013

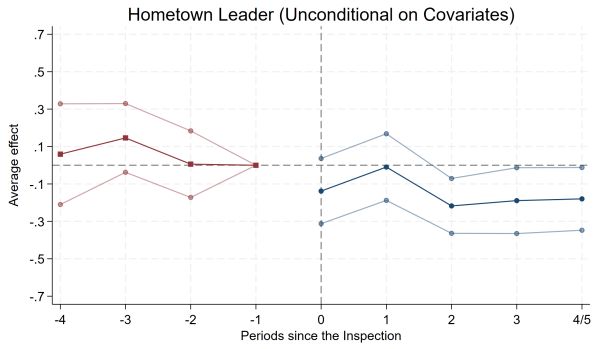
(b) Average Number of Leaders Produced by Each Prefecture from 2009-2013



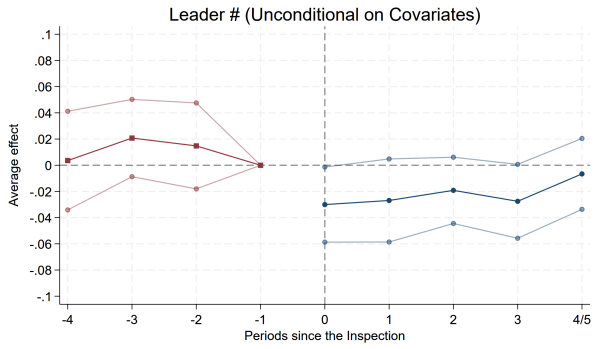
Notes: Panel A of this Figure displays the prefectures that had local-born city party secretaries or mayors from 2009 to 2013. Panel B shows the average number of Chinese party secretaries and city mayors produced by each prefecture during the same period.

Figure 3: Event-study: Politicians' Hometown Firms and Subsidy (Unconditional on Covariates Parallel Trends)

(a) Hometown Leader



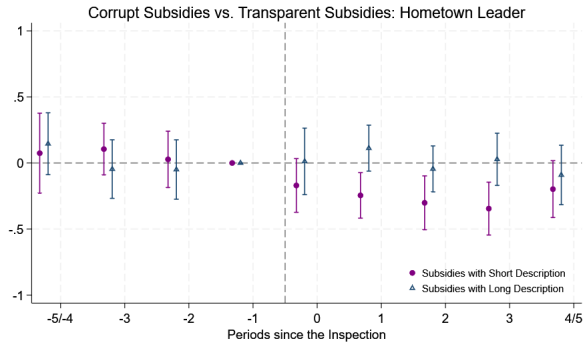
(b) Leader #



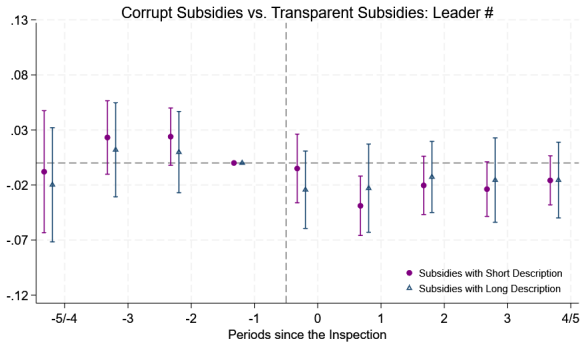
Notes: Figure illustrates the event-study results for Hometown Leader and Leader #. Panel A and Panel B are estimated by $Log(Subsidy_{a,i,c,t}) = \sum_{\tau=-4}^{5} Hometown_{i,c,2009-2013} \times \beta_\tau \text{Periods since the Inspection}_t + X'\beta + \delta_i + \tau_t + \epsilon_{a,i,c,t}$. Where *Hometown* denotes one of the two hometown favoritism statuses: (1) Hometown Leader is a dummy denoting prefecture city c that had local-born city party secretaries or mayors from 2009 to 2013; Leader # is a continuous variable denoting the average number of leaders (mayor and party secretary) produced by each prefecture city from 2009 to 2013. δ_i is the firm fixed effects. τ_t is the year fixed effects. I also control for the 2-digit industry year fixed effects. The year of anti-corruption inspection is set as the reference year, and all other coefficients are relative to inspection.

Figure 4: Event-study: Corrupt Subsidies vs. Transparent Subsidies

(a) Hometown Leader



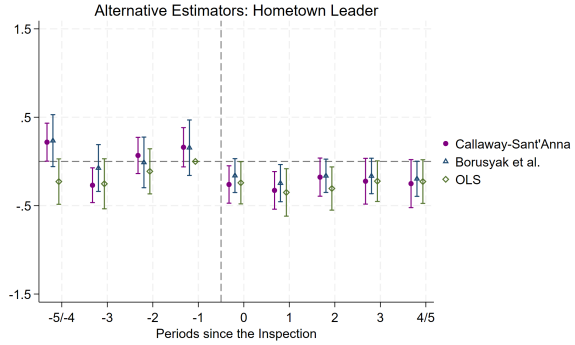
(b) Leader #



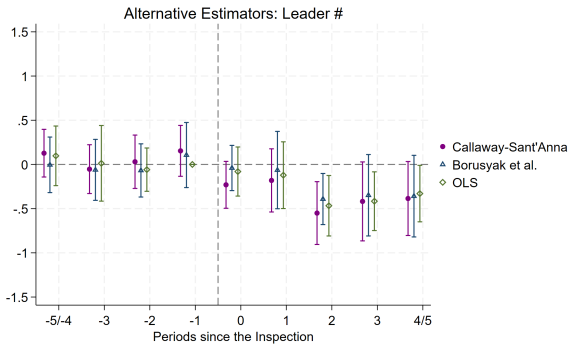
Notes: Figure illustrates the event-study results for the effect of the anti-corruption inspection on two types of subsidies: (1) Subsidies with a short description: subsidies with descriptions of no more than 15 Chinese characters (mean value); (2) Subsidies with a long description: subsidies with descriptions exceeding 15 Chinese characters. Panel A and Panel B are estimated by $Log(Subsidy_{a,i,c,t}) = \sum_{\tau=-4}^{5} Hometown_{i,c,2009-2013} \times \beta_\tau Periods\,since\,the\,Inspection_t + \delta_i + \tau_t + \epsilon_{a,i,c,t}$ and restricting samples to the three aforementioned subsidies. Where *Hometown* denotes one of the two hometown favoritism statuses: (1) Hometown Leader is a dummy denoting prefecture city c that had local-born city party secretaries or mayors from 2009 to 2013; Leader # is a continuous variable denoting the average number of leaders (mayor and party secretary) produced by each prefecture city from 2009 to 2013. δ_i is the firm fixed effects. τ_t is the year fixed effects. I also control for the 2-digit industry year fixed effects. The year of anti-corruption inspection is set as the reference year, and all other coefficients are relative to inspection.

Figure 5: Event-study: Alternative Estimators Robust to Staggered Difference-in-Differences (Firm-level Analyses)

(a) Hometown Leader



(b) Leader # (Dummy indicating 95 percentile Leader #)



Notes: Figure illustrates the event-study results for Hometown Leader and Leader #, using alternative estimators: (1) OLS; (2) Callaway and Sant'Anna (2021); (3) Borusyak et al. (2024). I use firm-level data here. Where *Hometown* denotes one of the two hometown favoritism statuses: (1) Hometown Leader is a dummy denoting prefecture city c that had local-born city party secretaries or mayors from 2009 to 2013; Leader # (Dummy) is a dummy variable taking 1 if the average number of leaders (mayor and party secretary) produced by each prefecture city from 2009 to 2013 is above the 95 percentile, approximately 15% of the sample. The reason of using a dummy variable is that both new estimators robust to staggered Difference-in-Differences may not deal with continuous treatment variable. $X'\beta$ is the vector of controls. δ_i is the firm fixed effects. τ_t is the year fixed effects. I also control for firm-level characteristics interacting with the anti-corruption inspection and 2-digit industry year fixed effects (Callaway and Sant'Anna (2021) allow only for time-invariant control variables). The year of anti-corruption inspection is set as the reference year, and all other coefficients are relative to inspection.

9 Tables

Table 1: Summary Statistics: Sort by Hometown Leader

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Hometown Leader = 0					Hometown Leader = 1				
VARIABLES	N	mean	sd	min	max	N	mean	sd	min	max
Panel A. Main variables										
Log(Subsidy)	123,537	12.666	2.157	-3.507	23.112	18,911	12.557	2.065	3.189	21.778
Anti-corruption Inspection	123,537	0.643	0.479	0.000	1.000	18,911	0.724	0.447	0.000	1.000
Panel B. Other variables										
State-owned Enterprises	123,537	0.609	0.488	0.000	1.000	18,911	0.433	0.496	0.000	1.000
Firm Age	123,537	12.064	4.440	1.000	29.000	18,911	12.385	4.874	1.000	24.000
Log(Asset)	123,537	21.620	1.237	15.418	29.895	18,911	21.430	1.018	17.769	25.338
Leverage	123,537	0.542	1.315	0.018	55.409	18,911	0.471	0.375	0.014	7.350
ROA	123,537	0.043	0.092	-3.001	2.058	18,911	0.058	0.079	-0.416	0.703
Log(GDP per capita)	123,537	10.147	0.812	6.100	11.512	18,911	10.466	0.601	8.791	11.512
Log(Fiscal Income)	123,537	7.995	0.829	3.404	9.401	18,911	8.212	0.620	6.365	9.401
Log(Population)	123,537	8.418	0.693	5.690	9.223	18,911	8.642	0.417	7.701	9.223

Notes: The data is at the firm-level. This table illustrates the descriptive statistics, which are sorted by $Hometown\ Leader_{i,c,2009-2013}$. $Hometown\ Leader_{i,c,2009-2013}$ is a dummy denoting prefecture city c that had at least one local-born city party secretary or mayor from 2009 to 2013.

Table 2: Summary Statistics: Sort by Leader #

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
		Leader # Below Median					Leader # Above Median			
VARIABLES	N	mean	sd	min	max	N	mean	sd	min	max
Panel A. Main variables										
Log(Subsidy)	61,025	12.690	2.157	-3.507	22.236	81,423	12.624	2.136	-1.897	23.112
Anti-corruption Inspection	61,025	0.664	0.472	0.000	1.000	81,423	0.645	0.478	0.000	1.000
Panel B. Other variables										
State-owned Enterprises	61,025	0.557	0.497	0.000	1.000	81,423	0.608	0.488	0.000	1.000
Firm Age	61,025	12.277	4.606	1.000	29.000	81,423	11.978	4.418	1.000	28.000
Log(Asset)	61,025	21.607	1.225	15.418	28.828	81,423	21.585	1.201	17.551	29.895
Leverage	61,025	0.521	0.596	0.025	55.409	81,423	0.541	1.546	0.014	41.939
ROA	61,025	0.045	0.099	-0.999	2.058	81,423	0.044	0.083	-3.001	0.703
Log(GDP per capita)	61,025	10.237	0.956	6.100	11.512	81,423	10.154	0.645	8.092	11.512
Log(Fiscal Income)	61,025	8.121	0.978	3.404	9.401	81,423	7.951	0.642	5.658	9.401
Log(Population)	61,025	8.445	0.777	5.690	9.223	81,423	8.450	0.571	7.113	9.223

Notes: The data is at the firm-level. This table illustrates the descriptive statistics, which are sorted by $Leader\#_{i,c,2009-2013}$. $Leader\#_{i,c,2009-2013}$ is a continuous variable denoting the average number of leaders (mayor and party secretary) produced by each prefecture city from 2009 to 2013.

Table 3: The Effect of Anti-corruption Efforts on Subsidy Allocation (Subsidy Allocation Level Data)

VARIABLES	(1)	(2)	(3)
	Log(Subsidy)		
Panel A. Intra-city Hometown Favoritism			
Hometown Leader × Inspection	-0.207***	-0.213***	-0.226***
	(0.047)	(0.047)	(0.047)
Observations	142,442	142,442	142,442
R-squared	0.264	0.264	0.274
Panel B. Inter-city Hometown Favoritism			
Leader # × Inspection	-0.024***	-0.023***	-0.022***
	(0.008)	(0.008)	(0.008)
Observations	142,442	142,442	142,442
R-squared	0.264	0.264	0.274
Firm FEs	✓	✓	✓
Year FEs	✓	✓	
Regional Controls		✓	✓
Pre-firm Controls × Inspection FEs	✓	✓	✓
2-Digit Industry × Year FEs			✓

Notes: This table illustrates the effect of anti-corruption inspections on subsidies for firms located in the hometown of politicians. The sample includes all publicly listed Chinese firms from 2009 to 2018. Standard errors are clustered at the province-year level, indicated in parentheses, due to the anti-corruption inspections. Across all columns, the main dependent variable is $Log(Subsidy)$, denoting the logarithm value of the amount of subsidy allocation to firm i located in city c in year t. This table reports the coefficients of the interaction term between Hometown Leader (Leader #) × Inspection. Hometown Leader is a dummy denoting prefecture city c that had local-born city party secretaries or mayors from 2009 to 2013. Leader # is a continuous variable indicating the average number of Chinese leaders (city mayor and city party secretary) that prefecture c produces from 2009 to 2013. Inspection denotes the staggered anti-corruption inspections, which takes 1 in affected province p for both the investigation year t and the following years, and 0 for all other years. The analysis includes firm fixed effects, year fixed effects, pre-firm controls interacting with the anti-corruption inspection, and 2-digit industry-year fixed effects. Significance level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: The Effect of Anti-corruption on Subsidy Allocation (Firm Level Data)

VARIABLES	(1)	(2)	(3)
	Log(Subsidy)		
Panel A. Intra-city Hometown Favoritism			
Hometown Leader × Inspection	-0.085	-0.083	-0.140**
	(0.065)	(0.065)	(0.069)
Observations	13,253	13,253	13,253
R-squared	0.690	0.690	0.720
Panel B. Inter-city Hometown Favoritism			
Leader # × Inspection	-0.039***	-0.036***	-0.029**
	(0.011)	(0.011)	(0.011)
Observations	13,253	13,253	13,253
R-squared	0.690	0.690	0.719
Firm FEs	✓	✓	✓
Year FEs	✓	✓	
Pre-firm Controls × Inspection FEs	✓	✓	✓
Regional Controls		✓	✓
2-Digit Industry × Year FEs			✓

Notes: This table illustrates the effect of anti-corruption inspections on subsidies for firms located in the hometown of politicians, using subsidy allocation levels collapsed to the firm level. The sample includes all publicly listed Chinese firms from 2009 to 2018. Standard errors are clustered at the province-year level, indicated in parentheses, due to the anti-corruption inspections. Across all columns, the main dependent variable is $Log(Subsidy)$, denoting the logarithm value of the amount of subsidy allocation to firm i located in city c in year t. This table reports the coefficients of the interaction term between Hometown Leader (Leader #) × Inspection. Hometown Leader is a dummy denoting prefecture city c that had local-born city party secretaries or mayors from 2009 to 2013. Leader # is a continuous variable indicating the average number of Chinese leaders (city mayor and city party secretary) that prefecture c produces from 2009 to 2013. Inspection denotes the staggered anti-corruption inspections, which takes 1 in affected province p for both the investigation year t and the following years, and 0 for all other years. The analysis includes firm fixed effects, year fixed effects, pre-firm controls interacting with the anti-corruption inspection, regional controls, and 2-digit industry-year fixed effects. Significance level: * p $<$ 0.10, ** p $<$ 0.05, *** p $<$ 0.01.

Table 5: Subsidy Allocation Heterogeneity: Specific Subsidy vs. Unspecific Subsidy

	(1)	(2)	(3)	(4)
	Length of Subsidy Description		Whether Subsidy Description is Specific	
VARIABLES	Subsidy $\leq$ Mean Length	Subsidy $>$ Mean Length	Opaque Subsidy	Subsidy for Programs
Panel A. Intra-city Hometown Favoritism				
Hometown Leader $\times$ Inspection	-0.292***	-0.001	-0.259***	-0.114
	(0.060)	(0.070)	(0.096)	(0.077)
Observations	95,387	46,950	96,235	46,120
R-squared	0.312	0.285	0.306	0.291
Panel B. Inter-city Hometown Favoritism				
Leader # $\times$ Inspection	-0.029***	-0.013	-0.027***	-0.017
	(0.009)	(0.012)	(0.009)	(0.010)
Observations	95,387	46,950	96,235	46,120
R-squared	0.312	0.238	0.305	0.290
Firm FEs	✓	✓	✓	✓
Regional Controls	✓	✓	✓	✓
Pre-firm Controls $\times$ Inspection FEs	✓	✓	✓	✓
2-Digit Industry $\times$ Year FEs	✓	✓	✓	✓

Notes: This table illustrates the effect of anti-corruption inspections on subsidies for firms located in the hometown of politicians, but restricting the sample to different types of subsidies: (1) Subsidy $\leq$ Mean Length of Description: subsidies with descriptions of no more than 15 Chinese characters (mean value); (2) Subsidy $>$ Mean Length of Description: subsidies with descriptions exceeding 15 Chinese characters; (3) Opaque Subsidy: subsidies with no specific program information; (4) Subsidy for Specific Programs: subsidies with specific program information. The sample includes all publicly listed Chinese firms from 2009 to 2018. Standard errors are clustered at the province-year level, indicated in parentheses, due to the anti-corruption inspections. Across all columns, the main dependent variable is $Log(Subsidy)$, denoting the logarithm value of the amount of subsidy allocation to firm i located in city c in year t. This table reports the coefficients of the interaction term between Hometown Leader (Leader #) $\times$ Inspection. Hometown Leader is a dummy denoting prefecture city c that had local-born city party secretaries or mayors from 2009 to 2013. Leader # is a continuous variable indicating the average number of Chinese leaders (city mayor and city party secretary) that prefecture c produces from 2009 to 2013. Inspection denotes the staggered anti-corruption inspections, which takes 1 in affected province p for both the investigation year t and the following years, and 0 for all other years. The analysis includes firm fixed effects, year fixed effects, pre-firm controls interacting with the anti-corruption inspection, and 2-digit industry-year fixed effects. Significance level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: The Effect of Firm Rent-seeking Costs on Subsidies: Before and After Anti-Corruption Inspections

	(1)	(2)	(3)	(4)	(5)
	Log(Subsidy)				
VARIABLES	All Subsidies	$\leq$ Mean Length	$>$ Mean Length	Opaque	Specific
Panel A: Intra-city Hometown Favoritism					
Hometown Leader $\times$ Inspection	-0.214**	-0.289***	0.064	-0.215*	-0.173**
	(0.095)	(0.101)	(0.106)	(0.118)	(0.065)
Hometown Leader $\times$ Firm Entertainment & Travel Costs	-0.125	-0.222	-0.038	-0.075	-0.272
	(0.103)	(0.195)	(0.108)	(0.133)	(0.162)
Hometown Leader $\times$ Inspection $\times$ Firm Entertainment & Travel Costs	0.047	0.118	-0.037	-0.038	0.309
	(0.118)	(0.200)	(0.196)	(0.173)	(0.188)
Observations	113,303	75,512	37,675	75,961	37,245
R-squared	0.292	0.336	0.241	0.324	0.311
Panel B: Inter-city Hometown Favoritism					
Leader # $\times$ Inspection	-0.006	-0.006	-0.001	-0.007	-0.010
	(0.013)	(0.014)	(0.022)	(0.015)	(0.014)
Leader # $\times$ Firm Entertainment & Travel Costs	0.035***	0.049***	0.016	0.049**	0.019*
	(0.012)	(0.011)	(0.017)	(0.018)	(0.011)
Leader # $\times$ Inspection $\times$ Firm Entertainment & Travel Costs	-0.035**	-0.048***	-0.008	-0.056**	0.002
	(0.015)	(0.015)	(0.022)	(0.020)	(0.014)
Observations	113,303	75,512	37,675	75,961	37,245
R-squared	0.292	0.336	0.241	0.324	0.311
Firm FEs	✓	✓	✓	✓	✓
Pre-firm Controls $\times$ Inspection FEs	✓	✓	✓	✓	✓

Continued on next page

Table 6 – *Continued from previous page*

	Log(Subsidy)				
	(1) All Subsidies	(2) $\leq$ Mean Length	(3) $>$ Mean Length	(4) Opaque	(5) Specific
Regional Controls	✓	✓	✓	✓	✓
2-Digit Industry $\times$ Year FEs	✓	✓	✓	✓	✓

Notes: This table illustrates the effect of the anti-corruption inspection on the correlation between firm entertainment and travel costs (a corruption-related expense) and allocated subsidies, but restricting the sample to different types of subsidies: (1) Subsidy $\leq$ Mean Length of Description: subsidies with descriptions of no more than 15 Chinese characters (mean value); (2) Subsidy $>$ Mean Length of Description: subsidies with descriptions exceeding 15 Chinese characters; (3) Opaque Subsidy: subsidies with no specific program information; (4) Subsidy for Specific Programs: subsidies with specific program information. The sample includes all publicly listed Chinese firms from 2009 to 2018. Standard errors are clustered at the province-year level, indicated in parentheses, due to the anti-corruption inspections. Across all columns, the main dependent variable is $Log(Subsidy)$, denoting the logarithm value of the amount of subsidy allocation to firm i located in city c in year t. This table reports the coefficients of the interaction term between Hometown Leader (Leader #) $\times$ Inspection. Hometown Leader is a dummy denoting prefecture city c that had local-born city party secretaries or mayors from 2009 to 2013. Leader # is a continuous variable indicating the average number of Chinese leaders (city mayor and city party secretary) that prefecture c produces from 2009 to 2013. Firm Entertainment & Travel Costs denotes the firm's entertainment and travel costs, scaled by profit in the year. Inspection denotes the staggered anti-corruption inspections, which takes 1 in affected province p for both the investigation year t and the following years, and 0 for all other years. The analysis includes firm fixed effects, year fixed effects, pre-firm controls interacting with the anti-corruption inspection, and 2-digit industry-year fixed effects. Significance level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Decomposition of Treatment Effects: Intensive vs. Extensive Margin

	(1)	(2)	(3)	(4)
	Log(Subsidy)			
VARIABLES	Baseline	Decomposed	Baseline	Decomposed
Panel A. Intra-city Hometown Favoritism				
Hometown Leader × Inspection	−0.226***			
	(0.047)			
Hometown Leader (Stayer) × Inspection		−0.266**		
		(0.120)		
Hometown Leader (Leaver) × Inspection		−0.209**		
		(0.097)		
Panel B. Inter-city Hometown Favoritism				
Leader # × Inspection			−0.023***	
			(0.008)	
Leader # (Stayer) × Inspection				−0.067***
				(0.013)
Leader # (Leaver) × Inspection				0.015
				(0.010)
Sample Period	2009–2018	2011–2018	2009–2018	2011–2018
Observations	142,442	125,214	142,442	125,214
R-squared	0.274	0.285	0.274	0.285
Firm FEs	✓	✓	✓	✓
Regional Controls	✓	✓	✓	✓
Pre-firm Controls × Inspection FEs	✓	✓	✓	✓
2-Digit Industry × Year FEs	✓	✓	✓	✓
# Stayer Cities		12		169
# Leaver Cities		25		84

Notes: This table decomposes treatment effects into Stayers (intensive margin) and Leavers (extensive margin) to distinguish behavioral change from mechanical turnover. Stayers are politicians in office during 2011–2012 who remained through 2014; Leavers are politicians in office during 2011–2012 who departed by 2014. Columns (1) and (3) present baseline results using the full sample period (2009–2018) and standard treatment definitions. Columns (2) and (4) present decomposed results restricting the sample to 2011–2018 to focus on the period most relevant for identifying politicians who stayed versus left during the inspection. Standard errors are clustered at the province-year level, indicated in parentheses. The dependent variable is Log(Subsidy), denoting the logarithm value of subsidy allocation to firm i in city c in year t. Hometown Leader is a dummy equal to 1 if prefecture city c had local-born city party secretaries or mayors from 2009 to 2013. Leader # is a continuous variable indicating the average number of leaders (city mayor and city party secretary) that prefecture c produces from 2009 to 2013. Inspection equals 1 in affected province p for both the investigation year t and following years, and 0 otherwise. The analysis includes firm fixed effects, regional controls, pre-firm controls interacting with inspection, and 2-digit industry-year fixed effects. Significance level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Heterogeneity in Inter-City Favoritism by Network Ties and Political Competition

	(1)	(2)	(3)	(4)
			Log(Subsidy)	
VARIABLES	Same Province	Workplace Tie	Shanghai Gang	Communist Youth League of China
Panel A: Network Ties and Inter-City Favoritism				
Leader # (Same Province) × Inspection	−0.225**			
	(0.094)			
Leader # (Different Province) × Inspection	0.058			
	(0.103)			
Leader # (Workplace Tie) × Inspection		−0.725**		
		(0.306)		
Leader # (No Workplace Tie) × Inspection		0.043		
		(0.081)		
Leader # (Shanghai Gang) × Inspection			−0.221	
			(1.334)	
Leader # (Non-Member) × Inspection			−0.045	
			(0.200)	
Leader # (CYLC) × Inspection				−0.129
				(0.235)
Leader # (Non-CYLC) × Inspection				−0.046
				(0.082)
Observations	142,442	142,442	142,442	142,442
R-squared	0.274	0.274	0.274	0.274
VARIABLES	Predicted Promotion	Same Cohort	Same Tenure	
Panel B: Political Competition and Inter-City Favoritism				
Leader # (Different Promotion Prob.) × Inspection	−0.162**			
	(0.070)			
Leader # (Similar Promotion Prob.) × Inspection	0.112			
	(0.109)			
Leader # (Different Cohort) × Inspection		−0.294**		
		(0.138)		
Leader # (Same Cohort) × Inspection		0.181		
		(0.183)		
Leader # (Different Tenure) × Inspection			−1.187***	
			(0.297)	
Leader # (Similar Tenure) × Inspection			−0.059	
			(0.043)	
Observations	142,442	142,442	142,442	
R-squared	0.274	0.274	0.274	
Firm FEs	✓	✓	✓	✓
Regional Controls	✓	✓	✓	✓
Pre-Firm Controls × Inspection	✓	✓	✓	✓
Industry × Year FEs	✓	✓	✓	✓

Notes: This table examines heterogeneity in inter-city hometown favoritism by network characteristics and political competition intensity. The sample includes all publicly listed Chinese firms from 2009 to 2018. Standard errors are clustered at the province-year level, indicated in parentheses. The main independent variable is the number of inter-city leaders (*Leader #*) produced by a firm's hometown city, disaggregated by network tie strength in Panel A and by competition intensity in Panel B. Panel A tests whether strong network ties facilitate reciprocity. Column (1) distinguishes leaders serving within the same province from those serving in different provinces. Column (2) distinguishes leaders who previously worked together from those without workplace ties. Column (3) distinguishes Shanghai Gang members from non-members. Column (4) distinguishes Communist Youth League of China (CYLC) members from non-members. Panel B tests whether direct competition prevents cooperation. Column (1) distinguishes leaders with different predicted promotion probabilities from those with similar probabilities. Column (2) distinguishes leaders from different age cohorts (more than 5 years apart) from same-cohort leaders. Column (3) distinguishes leaders with different tenure from those with similar tenure. Significance level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Anti-Corruption Effects Across the Distribution of Political Networks

VARIABLES	(1)	(2)	(3)
	Log(Subsidy)		
Mid Network (33–66%) × Inspection	0.119*	0.111*	0.119*
	(0.064)	(0.061)	(0.061)
High Network (66–99%) × Inspection	−0.082	−0.100*	−0.102*
	(0.055)	(0.055)	(0.055)
Elite Network (Top 1%) × Inspection	−0.467***	−0.462***	−0.418***
	(0.093)	(0.094)	(0.094)
Observations	142,442	142,442	142,442
R-squared	0.264	0.274	0.275
Firm FEs	✓	✓	✓
Year FEs	✓		
Regional Controls			✓
Pre-Firm Controls × Inspection	✓	✓	✓
2-Digit Industry × Year FEs		✓	✓

Notes: This table examines non-linear effects of political network strength on subsidy reductions after the anti-corruption inspection. Political networks are measured by the number of leaders produced by the firm's city serving elsewhere. Cities are divided into four groups: Bottom 33% (reference, omitted), Mid Network (33–66%), High Network (66–99%), and Elite (top 1%). Coefficients represent the differential effect of the inspection relative to the bottom 33%. The sample includes all publicly listed Chinese firms from 2009 to 2018. Standard errors are clustered at the province-year level, indicated in parentheses, due to the anti-corruption inspections. Across all columns, the main dependent variable is Log(Subsidy), denoting the logarithm value of the amount of subsidy allocation to firm i located in city c in year t. Inspection denotes the staggered anti-corruption inspections, which takes 1 in affected province p for both the investigation year t and the following years, and 0 for all other years. The analysis includes firm fixed effects, year fixed effects, regional controls, 2-digit industry-year fixed effects, and pre-treatment firm characteristics interacted with the inspection indicator. Significance level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Average Firm Level Outcomes in Political Leaders' Hometown after Anti-corruption Inspections

	(1)	(2)	(3)
	Log(City Outcomes)		
VARIABLES	Firm Revenue	Firm Employment	Firm Labor Productivity
Panel A. Intra-city Hometown Favoritism			
Hometown Leader × Inspection	0.153**	0.052	0.098*
	(0.070)	(0.064)	(0.052)
Observations	2,388	2,388	2,388
R-squared	0.691	0.656	0.639
Panel B. Inter-city Hometown Favoritism			
Leader # × Inspection	0.013	0.001	0.019**
	(0.013)	(0.012)	(0.010)
Observations	2,388	2,388	2,388
R-squared	0.690	0.656	0.639
Year FEs	✓	✓	✓
City FEs	✓	✓	✓
Regional Controls	✓	✓	✓

Notes: This table illustrates the effect of anti-corruption inspection on the prefecture city c's average firm level outcomes. Log(Firm Labor Productivity) is proxied by Log(Revenue/Employee #). The sample includes all Chinese listed firms in the period 2009-2018. Standard errors are clustered at the province-year level, indicated in parentheses, due to anti-corruption inspection. This table reports the coefficients of the interaction term between Hometown Leader/Leader # and Inspection. Hometown Leader is a dummy denoting prefecture city c that had local-born city party secretaries or mayors from 2009 to 2013. Leader # is a continuous variable indicating the average number of Chinese leaders (city mayor and city party secretary) that prefecture c produces from 2009 to 2013. Inspection denotes the staggered anti-corruption inspections, which takes 1 in affected province p for both the investigation year t and the following years, and 0 for all other years. I include regional controls, prefecture city fixed effects, year fixed effects. Significance level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Firm Heterogeneity in Subsidy Allocation following Anti-Corruption Inspections

	(1)	(2)	(3)	(4)	(5)	(6)
	Log(Subsidy)					
	Firm Scale		SOE Status		Pre-Subsidy Recipients	
VARIABLES	Intra-city	Inter-city	Intra-city	Inter-city	Intra-city	Inter-city
Hometown Leader × Inspection	1.988*		-0.202***		-0.166***	
	(1.066)		(0.069)		(0.063)	
Hometown Leader × Inspection × Firm Scale	-0.098**					
	(0.048)					
Leader # × Inspection		0.495***		-0.007		-0.009
		(0.145)		(0.012)		(0.012)
Leader # × Inspection × Firm Scale		-0.023***				
		(0.006)				
Hometown Leader × Inspection × SOE			-0.055			
			(0.099)			
Leader # × Inspection × SOE				-0.030*		
				(0.016)		
Hometown Leader × Inspection × High Ex-ante Subsidy					-0.208**	
					(0.101)	
Leader # × Inspection × High Ex-ante Subsidy						-0.033**
						(0.015)
Observations	142,442	142,442	142,442	142,442	142,442	142,442
R-squared	0.278	0.278	0.275	0.274	0.277	0.276
Firm FEs	✓	✓	✓	✓	✓	✓
Regional Controls	✓	✓	✓	✓	✓	✓
Pre-firm Controls × Inspection	✓	✓	✓	✓	✓	✓
2-Digit Industry × Year FEs	✓	✓	✓	✓	✓	✓

Notes: This table examines heterogeneity in subsidy allocation following anti-corruption inspections across firm size, ownership type, and pre-inspection subsidy receipt. The sample includes all publicly listed Chinese firms from 2009 to 2018. Standard errors are clustered at the province-year level, indicated in parentheses. The dependent variable is Log(Subsidy). Hometown Leader is a dummy denoting prefecture city c that had local-born city party secretaries or mayors from 2009 to 2013. Leader # is a continuous variable indicating the average number of leaders that prefecture c produces from 2009 to 2013. Firm Scale is measured by log total assets. SOE is an indicator for state-owned enterprises. High Ex-ante Subsidy is an indicator for firms with above-median subsidy receipts in the pre-inspection period. Inspection denotes the staggered anti-corruption inspections, which takes 1 in affected province p for both the investigation year t and the following years, and 0 for all other years. Significance level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: Market Competition and Firm Productivity: Small Firms Benefit More from Anti-Corruption

	(1)	(2)	(3)	(4)	(5)
	Total Factor Productivity				
VARIABLES	OLS	LP	ACF	WRDG	IND
Panel A. Intra-city Hometown Favoritism					
Hometown Leader × Inspection	1.364**	1.364**	1.308**	1.368**	1.414**
	(0.627)	(0.627)	(0.626)	(0.628)	(0.622)
Hometown Leader × Inspection	−0.059**	−0.059**	−0.056**	−0.059**	−0.062**
× Firm Scale	(0.028)	(0.028)	(0.028)	(0.029)	(0.028)
Observations	8,376	8,376	8,376	8,376	8,376
R-squared	0.777	0.780	0.790	0.779	0.989
Panel B. Inter-city Hometown Favoritism					
Leader # × Inspection	0.235**	0.237**	0.239**	0.237**	0.293***
	(0.101)	(0.101)	(0.102)	(0.101)	(0.096)
Leader # × Inspection	−0.011**	−0.011**	−0.011**	−0.011**	−0.013***
× Firm Scale	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)
Observations	8,376	8,376	8,376	8,376	8,376
R-squared	0.777	0.779	0.790	0.779	0.989
Firm FEs	✓	✓	✓	✓	✓
Regional Controls	✓	✓	✓	✓	✓
Pre-firm Controls × Inspection FEs	✓	✓	✓	✓	✓
Industry × Year FEs	✓	✓	✓	✓	✓

Notes: This table examines whether small firms benefit more from the restoration of market competition following anti-corruption inspections. The sample includes all publicly listed Chinese firms from 2009 to 2018. Standard errors are clustered at the province-year level, indicated in parentheses. The dependent variable is firm-level total factor productivity (TFP), estimated using five methods: ordinary least squares (OLS), Levinsohn and Petrin (2003) (LP), Ackerberg et al. (2015) (ACF), Wooldridge (2009) (WRDG), and industry-specific production functions (IND). Firm Scale is measured by log total assets; higher values indicate larger firms. The positive coefficients on the two-way interactions indicate that anti-corruption inspections increased firm productivity in hometown-favored regions, while the negative coefficients on the triple interactions indicate that smaller firms experienced larger productivity gains, consistent with the competition restoration hypothesis. Hometown Leader is a dummy denoting prefecture city c that had local-born city party secretaries or mayors from 2009 to 2013. Leader # is a continuous variable indicating the average number of Chinese leaders (city mayor and city party secretary) that prefecture c produces from 2009 to 2013. Inspection denotes the staggered anti-corruption inspections, which takes 1 in affected province p for both the investigation year t and the following years, and 0 for all other years. The analysis includes firm fixed effects and industry × year fixed effects. Significance level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Online Appendix

Table of Contents

A Conceptual Framework

In this section, I propose a simple conceptual framework where a local politician allocates two types of subsidies to hometown firms: legitimate subsidies (s_l) that are transparent and carry no political risk, and opaque or potentially corrupt subsidies (s_c) that offer flexibility but entail a risk of detection and punishment by a central authority. The politician derives benefits from allocating both types of subsidies to hometown firms but faces a trade-off (where allocating corrupt subsidies has a higher payoff): opaque subsidies are subject to monitoring, and the expected cost of allocating them increases with the intensity of anti-corruption efforts. I model anti-corruption inspections as an increase in the probability of detecting the misuse of opaque funds.

A.1 Setup

I consider a static model with three types of agents: a local Politician, Firms (located either in the politician's hometown, H, or elsewhere, O), and a Central Authority (CA).

Politician: The politician is responsible for allocating a budget of government subsidies. Their objective is to maximize their own utility, which depends on the amount and type of subsidies allocated to hometown firms. I focus on the allocation decision regarding hometown firms. The politician chooses the level of legitimate subsidies, $s_l \geq 0$, and opaque/corrupt subsidies, $s_c \geq 0$, directed towards hometown firms.

Firms: Firms receive subsidies and use them in production. I focus on hometown firms (H) which benefit from the politician's allocations. Non-hometown firms (O) receive the remainder of the subsidy budget, but their allocation is not explicitly modeled as the politician's choice variable in this simplified setup.

Central Authority (CA): The CA monitors the politician's allocation of opaque subsidies (s_c). It can conduct anti-corruption inspections, which affects the monitoring intensity.

Politician's Preferences: The politician derives utility from the total amount of subsidies allocated to hometown firms, $s_l + s_c$. This captures various potential benefits, such as political support, personal satisfaction from helping the hometown, or potential kickbacks associated implicitly with hometown favoritism. I assume the benefits exhibit diminishing returns. Specifically, the gross benefit is given by $b(s_l, s_c) = \beta(s_l + s_c)$, where $\beta > 0$ represents the marginal benefit of allocating subsidies to hometown firms. I incorporate diminishing returns through a quadratic cost term related to the *effort* or *political capital*

needed to secure and direct each type of subsidy, represented by $\frac{1}{2}\delta(s_l^2 + s_c^2)$, where $\delta > 0$. This formulation implies that increasing either type of subsidy becomes progressively costly.

Corruption, Monitoring, and Punishment: A key distinction is made between legitimate (s_l) and opaque (s_c) subsidies regarding corruption risk.

Legitimate Subsidies (s_l): These are assumed to be transparent, justifiable (e.g., linked to specific investment projects, performance metrics), and conform to established rules. Allocating s_l carries no risk of corruption detection, $p(s_l) = 0$.

Opaque Subsidies (s_c): These represent funds over which the politician has greater discretion. They might be allocated through less transparent channels, potentially used for patronage, personal gain, or inefficient pet projects, making them susceptible to being classified as corrupt if scrutinized. Allocating s_c entails a risk of being detected by the CA. The probability of detection is assumed to be increasing in the amount of opaque subsidies allocated and the CA's monitoring intensity, γ. I model this probability simply as $p(s_c) = \gamma s_c$, assuming s_c is scaled such that $p(s_c) \in [0, 1]$. The parameter $\gamma \geq 0$ reflects the baseline monitoring intensity or institutional capacity for oversight.

Anti-Corruption Inspections: An anti-corruption inspection is modeled as an exogenous increase in the monitoring intensity from γ to $\gamma' > \gamma$. This reflects heightened scrutiny and a greater likelihood of uncovering misuse of discretionary funds.

Punishment: If the allocation s_c is deemed corrupt and detected, the politician incurs a cost $C > 0$. This cost represents penalties such as fines, reputational damage, loss of office, or legal sanctions. The expected cost of corruption is thus $p(s_c)C = \gamma s_c C$.

Firm Productivity: Hometown firms' output or productivity depends on a baseline level and the subsidies received. Based on empirical evidence, I model the productivity differently before and after anti-corruption inspections:

Before anti-corruption inspection:

$$\pi_{before} = \pi_0 + \alpha s_l + \eta s_c$$

After anti-corruption inspection:

$$\pi_{after} = \pi_0 + \alpha s_l + (\eta + \theta) s_c$$

where π_0 is the baseline productivity level of hometown firms, α is the marginal productivity of legitimate subsidies, η is the initial marginal productivity of opaque subsidies before inspection, and $\theta > 0$ is the additional productivity gain from opaque subsidies after inspection.

I assume $\alpha > \eta \geq 0$, meaning legitimate subsidies are initially more productive than opaque ones. This reflects the idea that s_l likely funds productive investments, while s_c may be partially diverted or allocated to less efficient uses prior to inspection. The key insight from our empirical findings is that $\theta > 0$, indicating that after anti-corruption inspections, the remaining opaque subsidies become more productive. This could occur through reduced diversion or leakage in how opaque subsidies are utilized; improved governance mechanisms at the firm level; reallocation to more productive activities after increased scrutiny; or reduced rent-seeking behavior by firms or local officials.

A.2 Politician's Optimization Problem

The politician chooses the levels of legitimate subsidies (s_l) and opaque subsidies (s_c) to maximize their expected utility, which is the gross benefit minus the allocation costs and the expected cost of corruption:

$$\max_{s_l \geq 0, s_c \geq 0} U(s_l, s_c) = \beta(s_l + s_c) - \frac{1}{2}\delta(s_l^2 + s_c^2) - \gamma s_c C$$

The first-order conditions (FOCs) for an interior solution ($s_l > 0, s_c > 0$) are:

$$\frac{\partial U}{\partial s_l} = \beta - \delta s_l = 0$$

$$\frac{\partial U}{\partial s_c} = \beta - \delta s_c - \gamma C = 0$$

Solving these yields the potential interior solutions:

$$s_l^* = \frac{\beta}{\delta}$$

$$s_c^* = \frac{\beta - \gamma C}{\delta}$$

However, I must respect the non-negativity constraints. Since $\beta > 0$ and $\delta > 0$, the optimal legitimate subsidy is always positive: $s_l^* = \frac{\beta}{\delta} > 0$. The optimal opaque subsidy depends on the comparison between the marginal benefit β and the marginal expected cost of the first unit, γC. If $\beta > \gamma C$, the marginal benefit of the first unit of s_c exceeds its expected cost, and the optimal allocation is interior: $s_c^* = \frac{\beta - \gamma C}{\delta} > 0$. If $\beta \leq \gamma C$, the marginal benefit of even the first unit of s_c does not cover its expected cost, leading to a

corner solution: $s_c^* = 0$.

Therefore, the optimal allocations are:

$$s_l^* = \frac{\beta}{\delta}$$

$$s_c^* = \max\left\{\frac{\beta - \gamma C}{\delta}, 0\right\}$$

The total subsidy allocated to hometown firms is $s^* = s_l^* + s_c^*$.

A.3 Comparative Statics

The optimal allocations respond to changes in the model parameters as follows:

Monitoring Intensity (γ): An increase in γ raises the expected cost of allocating opaque subsidies. It does not affect s_l^*, but it reduces s_c^*: $\frac{\partial s_c^*}{\partial \gamma} = -\frac{C}{\delta} < 0$ (for the interior case). If γ increases sufficiently such that $\gamma C \geq \beta$, s_c^* drops to zero.

Cost of Punishment (C): An increase in C also raises the expected cost of corruption. It does not affect s_l^*, but it reduces s_c^*: $\frac{\partial s_c^*}{\partial C} = -\frac{\gamma}{\delta} \leq 0$ (strictly negative if $\gamma > 0$ and $s_c^* > 0$).

Marginal Benefit (β): An increase in β raises the marginal benefit of both types of subsidies. It increases s_l^* ($\frac{\partial s_l^*}{\partial \beta} = \frac{1}{\delta} > 0$) and increases s_c^* ($\frac{\partial s_c^*}{\partial \beta} = \frac{1}{\delta} > 0$, for the interior case).

Allocation Cost Parameter (δ): An increase in δ makes allocating both types of subsidies more costly (steeper diminishing returns). It reduces both s_l^* ($\frac{\partial s_l^*}{\partial \delta} = -\frac{\beta}{\delta^2} < 0$) and s_c^* ($\frac{\partial s_c^*}{\partial \delta} = -\frac{\beta - \gamma C}{\delta^2} < 0$, for the interior case).

Productivity Parameter Effects: Additionally, the productivity parameters (α, η, and θ) affect the welfare implications but not the politician's optimal choices in our model. An increase in α or η raises firm productivity but does not affect the politician's allocation decision. An increase in θ improves the post-inspection productivity without affecting pre-inspection allocations.

These comparative statics align with intuition: factors increasing the net benefit of subsidies (β) increase allocations, while factors increasing the costs (δ, γ, C) decrease them, with monitoring (γ) and punishment (C) costs specifically affecting the risky component (s_c).

A.4 Propositions

Based on the politician's optimal behavior, I derive predictions regarding subsidy allocation, the impact of anti-corruption measures, and firm productivity, linking them explicitly to the empirical stylized facts.

Proposition 1 (Optimal Subsidy Allocation and Hometown Favoritism) *Given the model parameters β, δ, γ, C, the politician optimally allocates legitimate subsidies $s_l^* = \frac{\beta}{\delta}$ and opaque subsidies $s_c^* = \max\left\{\frac{\beta-\gamma C}{\delta}, 0\right\}$ to hometown firms. Hometown favoritism, measured by the total allocation $s^* = s_l^* + s_c^*$, is positive as long as $\beta > 0$. Opaque subsidies are used ($s_c^* > 0$) if and only if the marginal benefit of subsidies exceeds the marginal expected cost of corruption at zero allocation ($\beta > \gamma C$).*

This proposition establishes the baseline allocation. The positive allocation s_l^* reflects the inherent benefits the politician derives from supporting hometown firms through transparent channels. The allocation s_c^* represents an additional layer of favoritism channeled through opaque means, undertaken only when the perceived benefits outweigh the risks associated with potential detection and punishment. This provides a rationale for the observed hometown bias in subsidy allocation.

Proposition 2 (Anti-Corruption Inspections and Subsidy Composition) *An inspection, modeled as an increase in monitoring intensity from γ to $\gamma' > \gamma$, reduces the allocation of opaque subsidies from s_c^* to $s_c^{*'} = \max\left\{\frac{\beta-\gamma' C}{\delta}, 0\right\} \leq s_c^*$. The allocation of legitimate subsidies $s_l^* = \frac{\beta}{\delta}$ remains unchanged. The total reduction in hometown subsidies is $\Delta s = s_c^* - s_c^{*'} \geq 0$.*

This proposition directly addresses the heterogeneous impact of anti-corruption efforts. The model predicts that heightened monitoring specifically targets the risky component of subsidies (s_c) because only these carry an expected cost that depends on γ. Legitimate subsidies (s_l), being risk-free in the model, are unaffected by changes in monitoring intensity. This aligns precisely with the empirical findings that anti-corruption audits reduce overall hometown subsidies but that this reduction is driven entirely by declines in opaque or discretionary funds, while transparent funds remain stable. The predicted reduction $\Delta s = s_c^* - s_c^{*'} = \frac{(\gamma'-\gamma)C}{\delta}$ (assuming an interior solution before and after) can be calibrated to match the empirically observed magnitude (e.g., 22.6%). This mechanism, where increased monitoring deters risky behavior, is consistent with findings from studies on the effectiveness of audits in reducing corruption and leakage.

Proposition 3 (Firm Productivity Effects) *Following an anti-corruption inspection that reduces opaque subsidies from s_c^* to $s_c^{*'}$, the change in hometown firm productivity is given by:*

$$\Delta\pi = \pi_{after} - \pi_{before} = (\eta + \theta)s_c^{*'} - \eta s_c^* = \eta(s_c^{*'} - s_c^*) + \theta s_c^{*'}$$

For productivity to increase despite reduced subsidies ($\Delta\pi > 0$ while $s_c^{*'} < s_c^*$), the efficiency gain parameter θ must satisfy:

$$\theta > \eta \frac{s_c^* - s_c^{*'}}{s_c^{*'}}$$

This proposition explains our empirical finding that hometown firm productivity increases following anti-corruption inspections despite reductions in total subsidies. The model suggests two competing effects: (1) a negative effect from reduced opaque subsidies: $\eta(s_c^{*'} - s_c^*) < 0$, and (2) a positive effect from increased efficiency of the remaining opaque subsidies: $\theta s_c^{*'} > 0$. When the efficiency gain (θ) is sufficiently large, the second effect dominates, leading to a net increase in productivity. This occurs when opaque subsidies were initially inefficiently allocated or partially diverted, and the anti-corruption inspection improves governance and oversight. The condition for productivity increase is more likely to be satisfied when the initial productivity of opaque subsidies (η) is low, the reduction in these subsidies is modest, or the efficiency improvement (θ) is substantial. These findings align with literature suggesting that corruption often involves resource misallocation, and that improved governance can enhance economic performance even as it reduces total resource flows.

B Supplementary Analyses

In this section, I present additional analyses and robustness checks.

B.1 Career Incentives and Political Clientelism

I have demonstrated that the anti-corruption inspection reduced hometown favoritism in subsidy allocation. But why did local politicians engage in such favoritism initially, and what mechanism drove the behavioral change? I argue that hometown favoritism operated as a form of political clientelism, where politicians strategically channeled resources to cultivate political capital and advance their careers (Kitschelt and Wilkinson, 2007; Stokes et al., 2013).

In China's competitive promotion tournament, local politicians face strong incentives to build relationships with powerful political networks (Jiang and Zhang, 2020; Shi et al., 2021). Politicians from politically important cities may leverage their hometown connections to signal loyalty to senior leaders or engage in reciprocal exchanges with other officials (Shi et al., 2021). If hometown favoritism indeed served as a career advancement strategy, we should observe that politicians who channeled more resources to their hometowns enjoyed higher promotion probabilities before the anti-corruption inspections. Conversely, if the inspection successfully disrupted this clientelistic system, the career returns to hometown favoritism should have diminished or reversed.

To test this hypothesis, I construct a politician-level panel dataset covering all city mayors and party secretaries from 2008 to 2016, covering more than 330 prefecture-level cities. For each politician-year observation, I calculate the total subsidies allocated to listed firms in the politician's birthplace city. For hometown leaders serving in their birthplace, this equals the subsidies allocated in their current jurisdiction. For exported leaders serving elsewhere, this captures the subsidies flowing to their birthplace city, providing a measure of their engagement in cross-jurisdictional resource allocation.

I estimate the following specification separately for the pre-inspection period (2008–2012) and post-inspection period (2013–2016):

$$\text{Promoted in Next Term}_{lt} = \alpha \ln(\text{Hometown Subsidy})_{lt} + \gamma \mathbf{X}_{lt} + \alpha_l + \delta_t + \theta_c + \phi_b + \varepsilon_{lt} \tag{5}$$

where $\text{Promoted in Next Term}_{lt}$ is an indicator for whether politician l (leader) was promoted in the next term following year t. I control for politician fixed effects (α_l), year fixed effects (δ_t), city fixed effects (θ_c), and birth cohort fixed effects (ϕ_b).[38] Control variables ($\mathbf{X}_{lt}$)

[38] I report results without politician fixed effects as a robustness check; the findings are consistent across specifications, indicating that the results hold for both within-politician and cross-politician variation.

include politician characteristics such as starting age interacted with deputy provincial rank, education level, and gender. Standard errors are clustered at the province-year level.

Table D.7 presents the results. The most saturated specifications in Columns 3 and 6 include the full set of fixed effects. Column 3 shows that before the anti-corruption inspection, hometown subsidies were positively and significantly associated with promotion probability. This provides direct evidence that hometown favoritism was a rational career strategy: politicians who channeled more resources to their hometown networks enjoyed tangible career benefits. The economic magnitude is meaningful—a 10% increase in hometown subsidies during a politician's tenure is associated with a 0.2 percentage point increase in the probability of promotion in the next term.

Column 6 reveals a striking reversal in the post-inspection period. The coefficient becomes negative and statistically significant, indicating that hometown favoritism transformed from a career asset into a career liability. Politicians who channeled more resources to their hometowns during and after the inspection faced lower promotion probabilities, suggesting that such behavior now attracted scrutiny and hindered career advancement rather than facilitating it.

This reversal reveals the mechanism underlying the main findings. The anti-corruption campaign's effectiveness stemmed not merely from increased detection risk, but from fundamentally altering politicians' career incentives. By eliminating the career returns to hometown favoritism, the inspection removed the primary motivation for clientelistic resource allocation while simultaneously raising its costs. This explains why continuing politicians reduced favoritism even when they remained in office, as documented in the intensive margin analysis (Table 7): the expected benefits had disappeared while potential costs had increased. These findings demonstrate that anti-corruption inspection achieves behavioral change by disrupting the political economy equilibrium that sustains corrupt practices (Olken, 2007), consistent with evidence that breaking clientelistic systems requires altering expected payoffs rather than merely increasing punishment probability (Ferraz and Finan, 2011).

B.2 Firm-Level Heterogeneity: Registration vs. Headquarter Location

The baseline analysis defines hometown connections using firms' headquarter addresses. However, firms may maintain political connections through multiple geographic channels. A firm headquartered in city A but formally registered in city B may access political networks through either location. This distinction provides an opportunity to examine whether political favoritism operates through headquarter-based operational ties or registration-based administrative ties, and whether the relative political influence of these locations moderates the anti-corruption effect.

To exploit this heterogeneity, I distinguish between two location measures for each firm: (1) the headquarter address, which reflects operational presence and is used in the baseline analysis; and (2) the registration address, which determines formal administrative jurisdiction. For each location, I construct the corresponding hometown favoritism variables: Hometown Leader (HQ) and Leader # (HQ) based on the headquarter city, and Hometown Leader (Reg) and Leader # (Reg) based on the registration city. I further construct continuous measures of political network power for each location. Power (HQ) measures the number of leaders produced by the firm's headquarter city during 2009–2013. Power (Reg) measures the number of leaders produced by the registration city over the same period. Power Diff captures the difference in political influence between registration and headquarter cities (Power Reg − Power HQ). For firms where the registration and headquarter cities are identical, these relative power measures are set to zero.

A key limitation of this analysis is the limited variation between headquarter and registration locations in the sample. Among publicly listed Chinese firms, only 6.3% have different registration and headquarter cities. This low rate of geographic separation substantially restricts statistical power for detecting heterogeneous effects based on relative political influence across locations.

Table D.11 presents the results. Columns (1)–(2) replicate the baseline specification using registration address to define hometown connections. Reassuringly, the coefficients are nearly identical to the main results using headquarter addresses. Column (1) shows that firms registered in cities with a hometown leader experienced a 19.7% reduction in subsidies following anti-corruption inspections. Column (2) shows that each additional leader produced by the registration city is associated with a 2.3% subsidy reduction. This consistency confirms that the baseline findings are robust to alternative definitions of firm location.

Columns (3)–(6) examine whether the relative political power of headquarter versus registration cities moderates the anti-corruption effect. Column (3) interacts headquarter-based connections with Power Diff; a positive coefficient would indicate that the anti-corruption effect is attenuated when the registration city is more powerful than the headquarter city. Column (4) directly interacts headquarter connections with registration city power. Columns (5)–(6) conduct analogous tests using registration-based connections interacted with Power Diff and headquarter power, respectively.

The triple interaction terms are statistically insignificant across all specifications. In columns (3)–(4), the coefficients on the triple interactions involving headquarter-based connections are 0.079 and −0.027, respectively. Columns (5)–(6) show similarly insignificant coefficients of 0.048 and 0.002 for registration-based connections. The baseline two-way interactions remain stable at approximately −0.023 across all columns.

Given the limited variation in headquarter-registration separation, these null results should be interpreted with caution. The analysis may lack sufficient statistical power to detect meaningful heterogeneity, even if such heterogeneity exists in the underlying population. Nevertheless, the stability of the baseline coefficients across specifications provides reassurance that the main findings are not driven by a particular definition of firm location. Future research with data featuring greater geographic separation between firm registration and operational locations may be better positioned to investigate this margin of heterogeneity.

B.3 Reversing the Crowding-Out Effect: Public Expenditure Recovery After Anti-Corruption Inspections

Prefecture cities, where leaders originate from within (Hometown Leader), may have experienced lower public expenditures compared to their counterparts before anti-corruption inspections. This phenomenon could be attributed to local governments favoring the corrupt allocation of subsidies to local firms, thus prioritizing them over other public expenditures and potentially crowding out essential services. Consequently, these cities might witness a surge in public expenditures following anti-corruption inspections, as these inspections curtail unnecessary subsidies to firms.

Conversely, in cases of inter-city hometown favoritism (Leader #), anti-corruption inspections might not significantly boost local public expenditures. This is because subsidies stem from multiple jurisdictions or cities, thereby reducing their impact on politicians' hometown public expenditures. To investigate this possibility, I constructed a new panel dataset spanning prefecture cities from 2009 to 2018. The regression equation is structured as follows:

$$\begin{aligned} Log(1 + Public\,Expenditure_{c,t}) =& \alpha_1 Hometown_{c,2009-2013} + \alpha_2 Inspection_{p,t} \\ &+ \alpha_3 Hometown_{c,2009-2013} \times Inspection_{p,t} \\ &+ X'\beta + \gamma_c + \tau_t + \epsilon_{c,t} \end{aligned} \tag{6}$$

where the subscripts are the same as defined above. α_3 is the coefficient of interest, capturing the effect of anti-corruption inspections on the local public expenditures.

In columns (1) - (2) of Panel A in Table D.12, focusing on intra-city hometown favoritism, the statistically significant coefficient estimate for the interaction term between hometown leader and inspection suggests a positive effect on local public expenditures. This implies that anti-corruption inspections may catalyze increased fiscal allocation in cities with leaders rooted within, possibly due to the reduction of corrupt subsidy practices—a

pattern congruent with preceding discussions.

Columns (1) - (2) of Panel B, which analyze inter-city hometown favoritisms, indicate that anti-corruption inspections may not significantly influence local fiscal spending in politicians' hometown cities, as previous corrupt subsidies originated from various jurisdictions/governments.

B.4 Are Hometown-Connected Regions Intensely Inspected?

One primary concern regarding the main specification is whether regions with hometown leaders or those generating more leaders experienced intensive anti-corruption inspections, potentially introducing endogeneity issues and biasing the estimates.

To address this concern, I construct a new panel dataset where each observation represents a province-year pair. To investigate whether hometown-connected regions are subject to intense anti-corruption inspections, I employ four dependent variables to represent anti-corruption intensity at the province-level: (1) Log(Media Coverage 1); (2) Log(Media Coverage 2); **1**(Provincial Leader Expelled); (4) Log(1+Corruption Cases) as the main dependent variable.[39] [40] For the degree of hometown favoritism by politicians at the province-level, I construct two variables: (1) Hometown Leader # Province, denoting the average number of hometown leaders within each province prior to the inspections; (2) Leader # Province, denoting the average number of leaders that each province produced before the inspection. I interact these variables with anti-corruption inspections to examine whether regions produced more hometown leaders and city-level leaders experienced intensive inspections.

As shown in Table D.15, I find no significant differences in the above four outcomes between provinces producing more hometown leaders or leaders serving in other regions, and provinces generating fewer hometown leaders or leaders serving elsewhere. These results suggest that regions connected to more leaders are not subjected to intense inspections compared to other provinces, indicating that all regions undergo similar levels of inspection intensity. This finding implies that the issue of endogeneity may not be relevant in this specification.

[39] For Log(Media Coverage 1) and Log(Media Coverage 2), I include two types of anti-corruption-related terms from all newspaper titles in each province: (1) *fanfubai* in Chinese meaning "anti-corruption"; (2) *fubai* in Chinese meaning "corruption".

[40] Incorporating media coverage of anti-corruption-related terms as a proxy for the intensity of anti-corruption is based on the assumption that regions undergoing intensive inspections may attract more attention from local media.

B.5 Additional Fixed Effects and Subsample Analyses

In this section, I assess the robustness of the baseline results by incorporating additional fixed effects and analyzing different sub-samples. Table D.14 presents these findings. Columns (1)-(2) demonstrate that the results remain statistically significant even after including province-by-year fixed effects and province-specific linear time trends. While the estimated coefficient for Hometown Leader decreases to -0.131 with the inclusion of province-by-year fixed effects, the coefficient for Leader # remains unchanged. In column (3), following Wang et al. (2020), I exclude firms located in four provincial-level cities: Beijing, Shanghai, Chongqing, and Tianjin from the sample.[41] The estimated coefficients for both Hometown Leader and Leader # remain stable, suggesting that the baseline results are generalizable to any type of city. In column (4), I exclude firms from government-dependent sectors as defined by Colonnelli and Prem (2022).[42] This exclusion controls for the potential confounding effect of these sectors' higher vulnerability to corruption. The similarity between these estimates and the baseline results suggests that the impact of anti-corruption inspections is not confined to government-dependent industries but is more pervasive across the economy. Overall, these additional fixed effects and subsample analyses show the robustness of the baseli

B.6 Placebo Anti-corruption Inspections

One potential concern with identifying the impact of the anti-corruption inspection is the possibility of concurrent policies that may have influenced subsidies in China during the same period, potentially biasing the results. Furthermore, the conventional event-study approach, which includes both pre-period and post-period estimated coefficients, may suffer from contaminated pre-period estimates, suggesting a violation of the parallel trend assumption, even if the event-study figure does not detect such a violation Borusyak et al. (2024).

To mitigate these concerns, I conduct a placebo analysis using a placebo anti-corruption inspection in either 2011 or 2012, limiting the data to observations before 2013.[43] The results are presented in Table D.16. In columns (1)-(4), the coefficients on the interactions between Hometown Leader (Leader #) and Post2011(2012) are not statistically significant. These findings suggest that the prefecture cities exposed to the anti-corruption campaigns did

[41] These four cities' social and economic characteristics are likely to be heavily influenced by the central government's directives (Wang et al., 2020). To rule out the possibility that the effect of anti-corruption inspection is primarily driven by these centrally-controlled cities, I exclude them from the sample.

[42] In the Chinese context, government-dependent sectors typically include agriculture, electricity, natural resources, media, and mining.

[43] Assuming Xi's anti-corruption campaign started in 2011 or 2012 for the placebo analysis.

not exhibit differential trends in subsidies compared to other prefecture cities, consistent with the event-study illustrated in Figure 3.

B.7 Alternative Estimators Robust to Staggered DiD

One concern regarding staggered difference-in-differences analysis is that the conventional two-way fixed effect model might not produce unbiased estimates due to heterogeneous treatment effects (see De Chaisemartin and d'Haultfoeuille (2020); Callaway and Sant'Anna (2021); Goodman-Bacon (2021); Sun and Abraham (2021); Borusyak et al. (2024)). In this paper, Chinese provinces underwent the anti-corruption inspections in 2013 (11 provinces) and 2014 (20 provinces). To address this concern stemming from the staggered nature of anti-corruption inspections, I employ alternative estimators proposed by Callaway and Sant'Anna (2021) and Borusyak et al. (2024) to replicate the baseline analyses.[44] [45]

As illustrated in Figure 5, the estimated coefficients of the approaches suggested by Callaway and Sant'Anna (2021) and Borusyak et al. (2024) exhibit a similar pattern to those of the conventional OLS estimates. Overall, this suggests that the conventional OLS estimates are not subject to severe issues arising from heterogeneous treatment effects or negative weighting problems associated with the staggered DiD design.

B.8 Sensitivity Analyses for Parallel Trends

One concern regarding the difference-in-differences approach is researchers' uncertainty about the validity of the parallel trend assumption and the robustness of estimates to potential violations of parallel trends (Roth (2022); Rambachan and Roth (2023)). Additionally, a key concern is that there may be unobserved city-specific shocks that could have affected firm subsidies differently, even without anti-corruption inspections. To mitigate this concern and evaluate the robustness of estimates in light of potential violations of parallel trends, I utilize the sensitivity analyses proposed by Rambachan and Roth (2023).[46]

Figure C.4 presents the results of sensitivity analyses. In Panel (a), robust confidence interval sets for all treatment effects in post-periods are displayed for $\Delta_{SD}(\bar{M})$, utilizing

[44] While the approach proposed by Callaway and Sant'Anna (2021) may not be directly applicable to the subsidy allocation level data, I replicate all regressions using alternative estimators at the firm level instead of the subsidy allocation level.

[45] Both Callaway and Sant'Anna (2021) and Borusyak et al. (2024) may not deal with continuous treatment variables, e.g., Leader # in this paper. Therefore, I use a dummy variable taking the value of one when Leader # is above the 95 percentile, which represents 15% of the sample.

[46] The sensitivity analyses are also conducted at the firm level, using the event-study coefficients from Callaway and Sant'Anna (2021).

various values of $\bar{M}$ for the results of Hometown Leader.[47] Panel (a) of Figure C.4 shows that by imposing $\Delta_{SD}(\bar{M}) = 0.06$, the confidence interval would include zero. In Panel (b), the figure reports the various values of $\bar{M}$ for the results of Leader #. By imposing $\Delta_{SD}(\bar{M}) = 0.14$, the confidence interval set can thus include zero.[48]

Panels (c) and (d) illustrate the sensitivity analyses using $\Delta_{RM}(\bar{M})$, where post-period violations are confined by exploiting the relative magnitude in the violation of parallel trends.[49] In Panel (c), a robust confidence set of [-0.99, 0.30] is attained for the causal effect on firm subsidies across all years, which includes zero. However, as noted by Rambachan and Roth (2023), wider confidence sets are expected for parameters pertaining to later periods, as the identified set tends to be larger for later periods given that the treatment and control groups have had more time to diverge. The "breakdown value" for a null effect of inspection on firms located in cities with hometown leaders is approximately $\bar{M} = 0.6$. Thus, the determination of a significant effect on firm subsidies hinges on whether post-treatment violations of parallel trends are constrained to be no more than 0.6 times as large as the maximal pre-treatment violation.

Panel (d) illustrates the sensitivity analysis for estimates of Leader #. With $\bar{M} = 1$, the robust confidence set now ranges from [-1.21, 0.02], including zero. Although wider than the original OLS confidence interval, which is solely valid under exact adherence to the parallel trends assumption, it nonetheless excludes a null effect of anti-corruption inspection on firm subsidies in post-periods. Consequently, the overall estimates remain robust to potential unobserved violations of the parallel trends assumption. Given the absence of significant economic shocks around the time of anti-corruption inspections in China, a "breakdown value" of $\bar{M} = 1$ suggests the robustness of the estimates.

Overall, the results are robust to the sensitivity analyses suggested by Rambachan and Roth (2023), especially when imposing the linear violation to the parallel trends assumption.

[47] The analysis is based on bounds on the second derivative class $\Delta_{SD}(\bar{M})$, where post-period violations are confined by exploiting the linear violation of parallel trends. For example, such bounds on how far δ, the bias from a difference in trends, can deviate from linearity.

[48] Essentially, $\Delta_{SD}(\bar{M}) = 0.06$ would correspond to allowing the slope of the differential trend to change by 0.06 percentage points in subsidies for the Hometown Leader results. For $\Delta_{SD}(\bar{M}) = 0.14$, it would correspond to allowing the slope of the differential trend to change by 0.14 percentage points in subsidies for the Leader # results. Overall, any confounding changes related to 0.06 percentage points in subsidies can be allowed in the results for the Hometown Leader group, and 0.14 percentage points in subsidies can be allowed in the results for the Leader group. This is a relatively significant violation of the parallel trends assumption (see Section 6.3 of Rambachan and Roth (2023)), yet robust estimates can still be obtained by imposing this level of violation, demonstrating the robustness of the current results.

[49] By imposing $\bar{M} = 1$, indicating a restriction on post-treatment violations of parallel trends to not exceed the maximal pre-treatment violation.

B.9 Randomization Inference

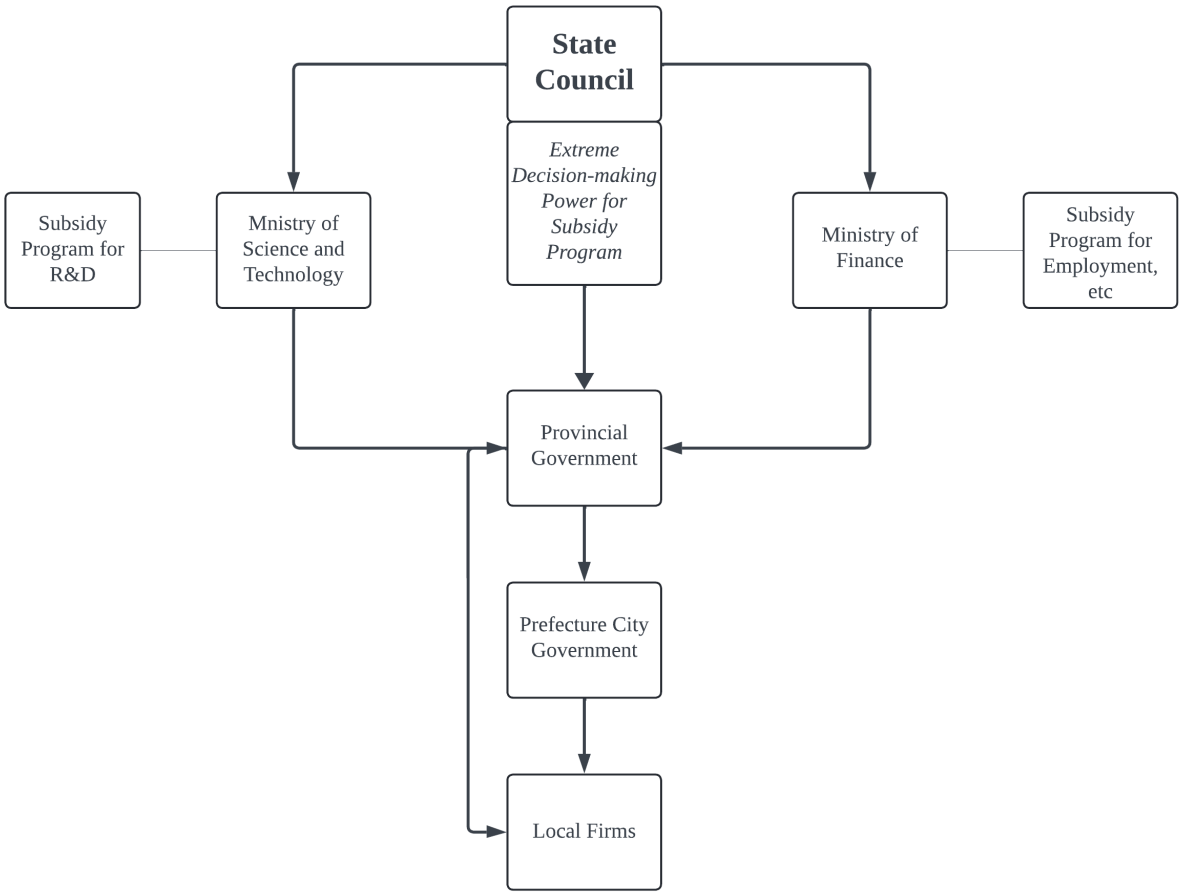
Another concern regarding the baseline results is that other groups may also be affected by the anti-corruption inspection regarding subsidies, as the anti-corruption campaign is a nationwide event that could affect any firm. This spatial correlation would lead to spurious estimates of the impact of the anti-corruption inspection. I thus follow the randomization inference procedure to compute the placebo estimated coefficients and *p*-value by comparing the baseline coefficients to the distribution of placebo hometown favoritism effects (see, for example, Cavallo et al. 2013).

To do the test, I randomly generate 500 fake treatment groups interacting with the anti-corruption inspection and run the baseline regression for each group.[50] I plot the distribution of the estimated coefficients and p-values of the interaction term in Figure C.5. Most estimated coefficients on the interactions between fake treatment and the anti-corruption inspection and p-values are concentrated between 0 and 1, respectively. Thus, the aforementioned concern is unlikely to explain my main findings.

[50] For the continuous variable of Leader #, I generate the placebo continuous treatment following the range and distribution of the actual Leader #.

C Figures

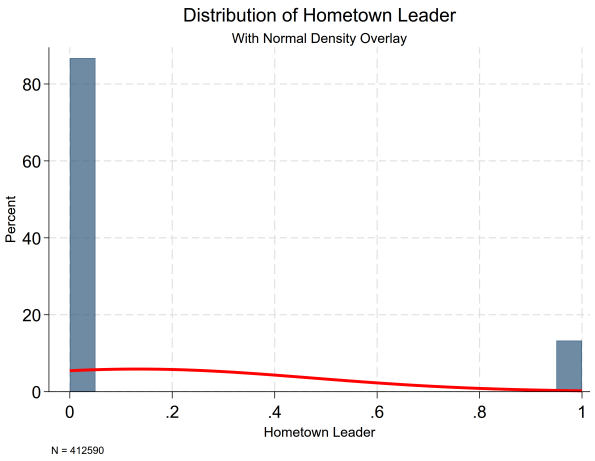
Figure C.1: The Organizational Chart of Subsidy Allocation Process in China



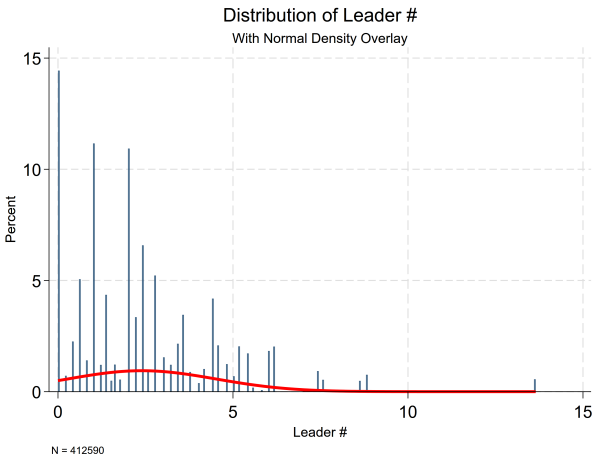
Notes: This figures illustrates the subsidy allocation process in China.

Figure C.2: Distribution of Hometown Leader and Leader #

(a) Hometown Leader

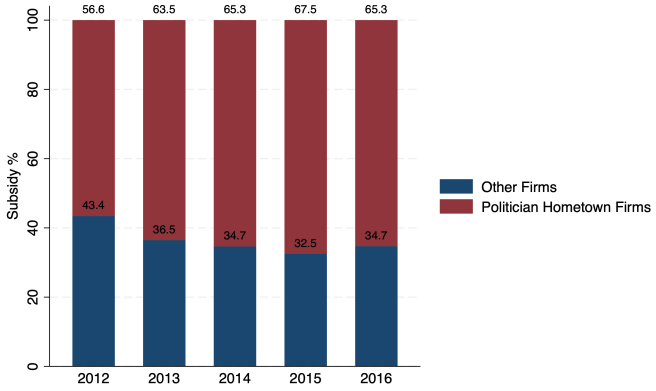


(b) Leader #

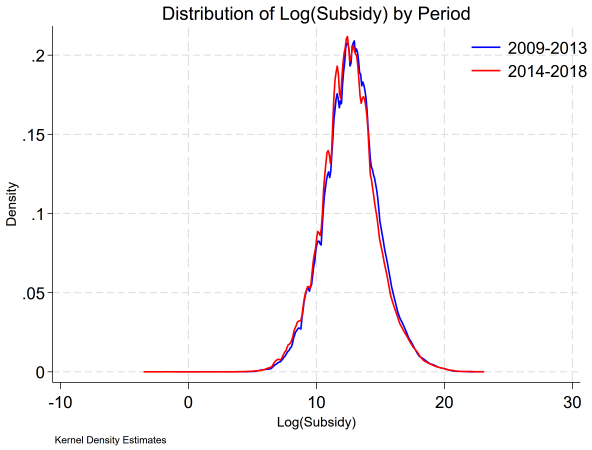


Notes: This figure illustrates the distribution of two main independent variables: (1) Hometown Leader; (2) Leader #.

Figure C.3: Subsidy Patterns



(a) Share of Subsidies to Politicians' Hometown

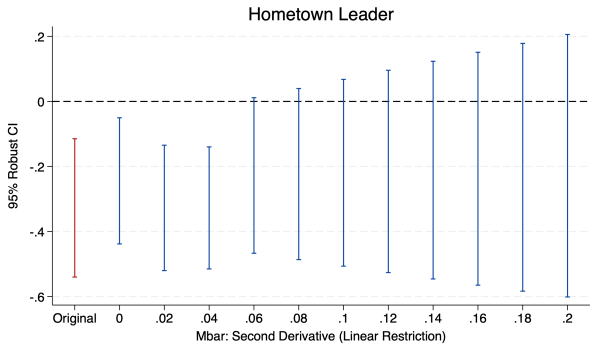


(b) Distribution of Log(Subsidy) by Two Periods

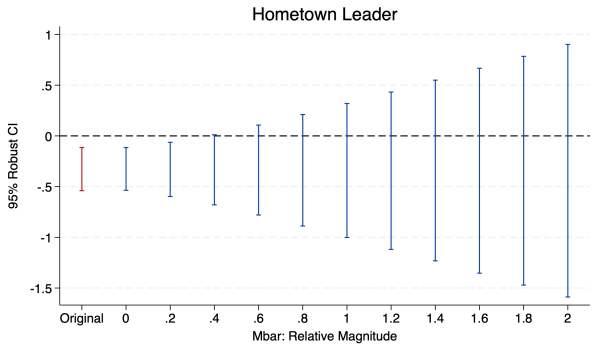
Notes: Panel (a) illustrates the share of subsidies allocated to politicians' hometowns over the year. Panel (b) illustrates the distribution of Log(Subsidy) by two periods: (1) 2009–2013; (2) 2014–2018.

Figure C.4: Sensitivity Analyses for Parallel Trends Assumption using Rambachan and Roth (2023) for Event-study Coefficients of Callaway and Sant'Anna (2021)

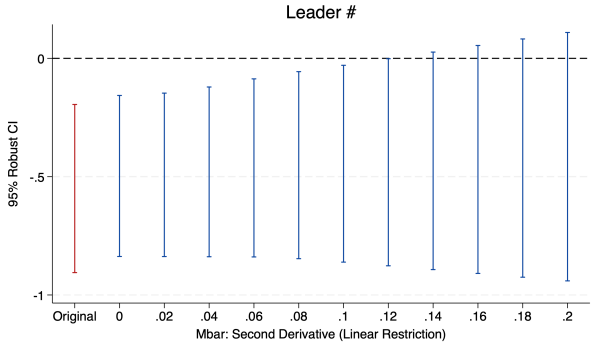
(a) Hometown Leader



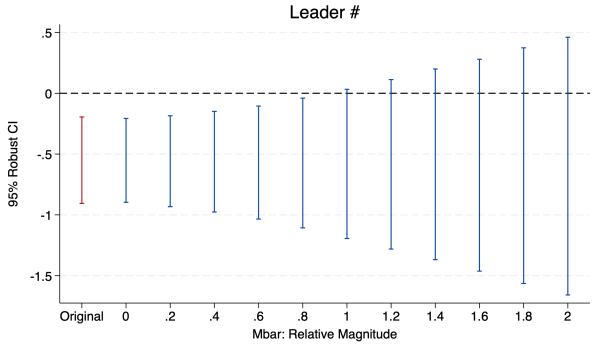
(c) Hometown Leader



(b) Leader #



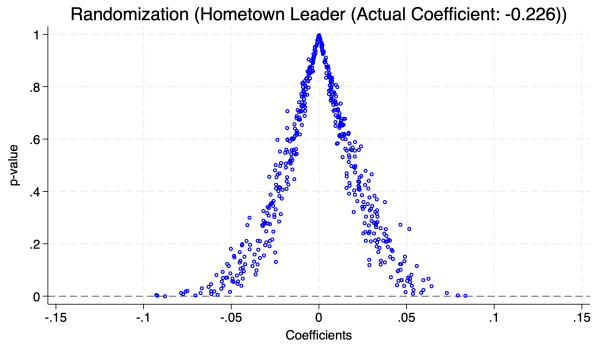
(d) Leader #



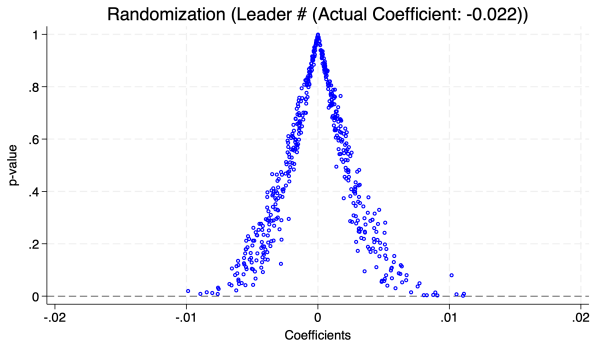
Notes: The figure illustrates the results of sensitivity analyses for the parallel trends assumption, based on the study by Rambachan and Roth (2023). It presents two measures: Mbar: Second Derivative, which indicates how far δ, the bias from a difference in trends, can deviate from linearity; and Mbar: Relative Magnitude, which denotes how many times the confidence interval of post-treatment coefficients can exceed the maximal value of the confidence interval of pre-treatment coefficients.

Figure C.5: Randomization Test and Inference

(a) Hometown Leader



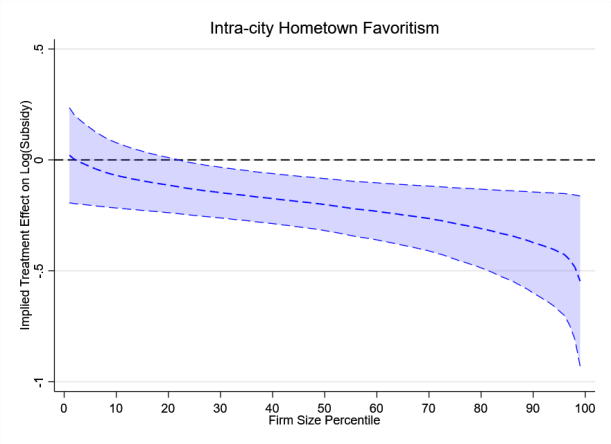
(b) Leader #



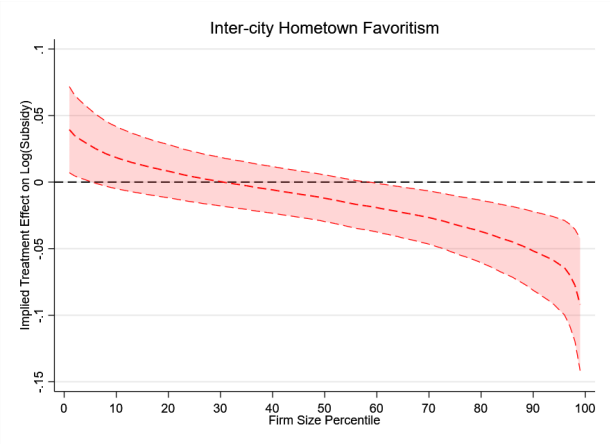
Notes: I repeat the baseline regressions 500 times but using different placebo treatment groups. I estimate the following equation: $Log(Subsidy_{a,i,c,t}) = \alpha_1 Hometown_{c,2009-2013} + \alpha_2 Inspection_{p,t} + \alpha_3 Hometown_{c,2009-2013} \times Inspection_{p,t} + X'_{i,p,t}\beta + \delta_i + \tau_t + \epsilon_{i,t}$

Figure C.6: Distribution of Implied Treatment Effects by Firm Size

(a) Intra-city Hometown Favoritism

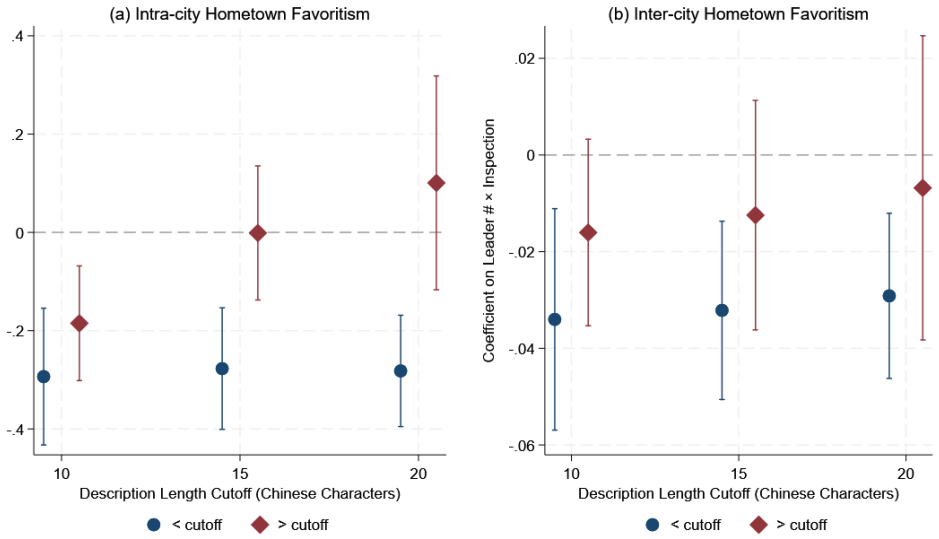


(b) Inter-city Hometown Favoritism



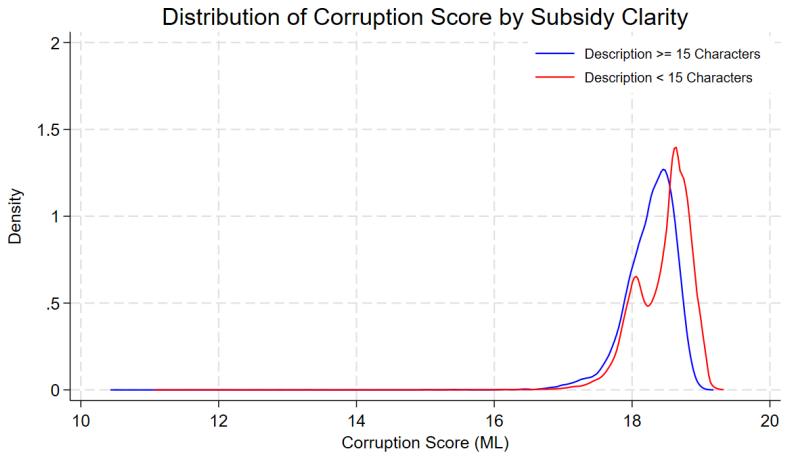
Notes: This figure plots the implied treatment effect of the anti-corruption inspection on log subsidy allocation across the firm size distribution, for intra-city (Panel A) and inter-city (Panel B) hometown favoritism. The implied effect at each percentile is computed as $\hat{\beta}_1 + \hat{\beta}_2 \times \text{Scale}_p$, where $\hat{\beta}_1$ is the coefficient on the two-way interaction (Hometown $\times$ Inspection or Leader # $\times$ Inspection) and $\hat{\beta}_2$ is the coefficient on the triple interaction with firm scale. The shaded area represents 95% confidence intervals constructed using the delta method. The dashed horizontal line denotes zero. Firm scale is measured by log total assets. The sample includes all publicly listed Chinese firms from 2009 to 2018.

Figure C.7: Alternative Cutoffs: Subsidy Heterogeneity by Description Length

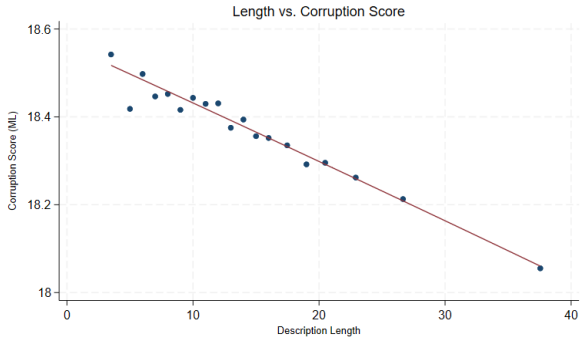


Notes: This figure examines the robustness of results to alternative description length cutoffs. Each point represents the coefficient on the interaction term (Hometown × Inspection) from a separate regression, restricting the sample to subsidies with descriptions shorter than (< cutoff, navy circles) or longer than (> cutoff, maroon diamonds) the indicated threshold. Panel (a) presents results for intra-city hometown favoritism using the Hometown Leader indicator; Panel (b) presents results for inter-city hometown favoritism using the Leader # variable. The pattern is consistent across all cutoffs: anti-corruption effects are concentrated among subsidies with shorter descriptions (coefficients negative and significant), while subsidies with longer descriptions show no significant effects (coefficients close to zero). This demonstrates that the 15-character threshold used in the main analysis is not arbitrary—the differential effects hold regardless of the exact cutoff chosen. 95% confidence intervals shown; standard errors clustered at the province-year level.

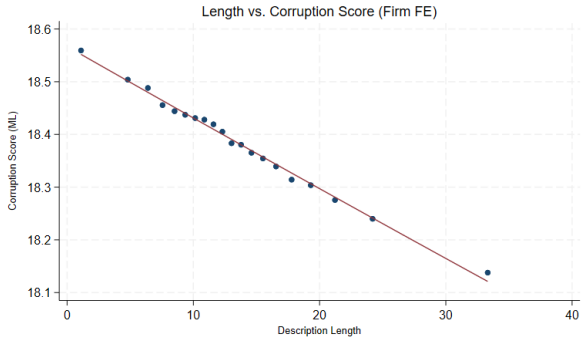
Figure C.8: Validation: Subsidy Description Length vs. ML-Based Corruption Score



(a) Distribution of ML Corruption Scores by Description Length



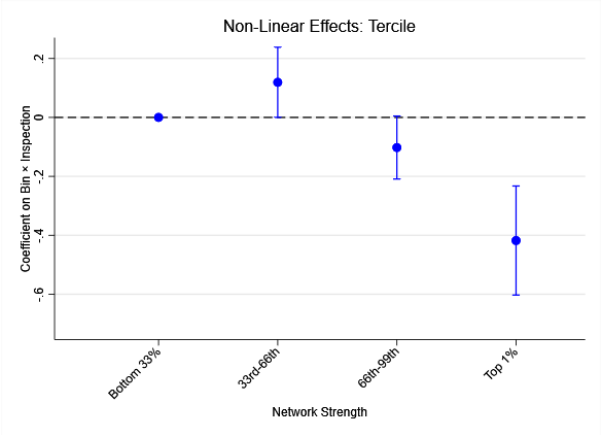
(b) Correlation



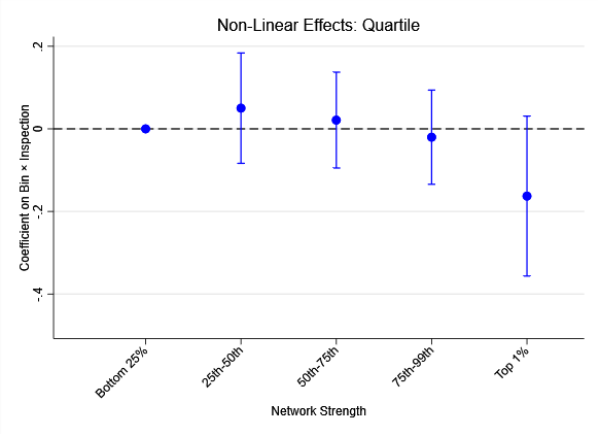
(c) Controlling for Firm Fixed Effects

Notes: This figure validates the description length classification using the machine learning-based corruption score from Section 5.5.3, where higher scores indicate greater ambiguity and corruption risk. Panel A shows kernel density estimates of corruption scores separately for subsidies with descriptions below 15 characters (red) and at or above 15 characters (blue). The rightward shift of the red distribution indicates that shorter descriptions are systematically associated with higher corruption risk. Panel B presents a binned scatter plot showing the negative correlation between description length and corruption scores without controls. Panel C replicates Panel B after partialling out firm fixed effects, demonstrating that the relationship holds within firms. Together, these panels validate that the 15-character threshold captures meaningful variation in subsidy opacity.

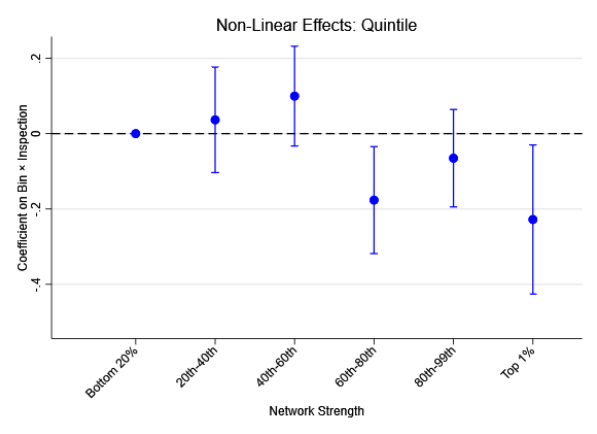
Figure C.9: Robustness: Non-Linear Effects of Political Network Strength



(a) Tercile (Bottom 33%, 33rd–66th, 66th–99th, Top 1%)



(b) Quartile



(c) Quintile

Notes: This figure presents robustness checks on the non-linear effects of political network strength on inter-city hometown favoritism following anti-corruption inspections. Each panel plots the estimated coefficients on the interaction between network strength bins and the inspection indicator, with the bottom group serving as the reference category (coefficient normalized to zero). The top 1% of cities by network strength is always separated as an elite group. Panel A uses the baseline tercile classification (Bottom 33%, 33rd–66th percentile, 66th–99th percentile, Top 1%). Panels B and C present alternative binning approaches using quartiles and quintiles respectively, demonstrating that the non-linear pattern is not sensitive to the specific cutoffs chosen. Vertical bars represent 95% confidence intervals. Standard errors are clustered at the province-year level.

D Tables

Table D.1: Theoretical Predictions and Empirical Facts

Empirical Fact	**Model Prediction (Proposition)**	**Mechanism**
Anti-corruption inspections reduce *total* hometown subsidies	s^* decreases because s_c^* decreases (Proposition 2)	Increased audit risk γ' raises expected cost $\gamma' s_c C$, reducing optimal s_c^*.
Reduction concentrated in opaque/-corrupt subsidies	Only s_c^* decreases (Proposition 2)	Only s_c carries detection risk dependent on γ.
Legitimate/transparent subsidies remain stable	s_l^* unchanged (Proposition 2)	Allocation $s_l^* = \beta/\delta$ is independent of monitoring γ.
Hometown firm productivity remains stable and even increases	$\Delta\pi \geq 0$ (Proposition 3)	Opaque subsidies s_c have low productivity ($\eta \geq 0$) compared to legitimate s_l (α).

Notes: This table illustrates the model predictions (propositions) and the actual empirical results.

Table D.2: Example: Subsidy Details for A Firm in 2015

Firm ID	Year	Subsidy Amount	Description (Chinese)	Description (English)	Length of Description (Number of Chinese Words)
8	2015	30000	企业知识产权保护体系建设	Construction of Enterprise Intellectual Property Protection System	12
8	2015	1000000	2015北京高新技术成果转化项目	2015 Beijing High-Tech Project	16
8	2015	6300	北京中关村企业信用促进会补贴款	Subsidy from the Beijing Zhongguancun Enterprise Credit Promotion Association	15
8	2015	450	国家知识产权局专利局专利资助金	Patent Subsidy from the State Intellectual Property Office Patent Bureau	15
8	2015	5000	安全生产奖励费	Safety Production Reward	7
8	2015	139803.2	社保就业补贴	Social Security Employment Subsidy	6
8	2015	16670.41	工大科技园2013年度扶持资金	2013 Annual Support Fund for the Technology Zone	15
8	2015	198300	2015科技创业家企业贷款贴息款	2015 Loan Interest Subsidy for Technology Entrepreneurs	16
8	2015	200000	2014年度科技创新券兑换	2014 Technology Innovation Voucher Exchange	13
8	2015	8500	九州认证补贴款	Jiuzhou Subsidy	7
8	2015	500000	苏科技[2014]318号2014年度第三十六批科技发展计划资金	Su Technology [2014] No. 318: 2014 36th Batch of Science and Technology Development Plan Fund	30
8	2015	35000	专利补助	Subsidy for patenting	4

Notes: This table illustrates an example of subsidy allocation for a listed Chinese firm.

Table D.3: Correlation between Politicians' Hometown and Allocated Government Subsidies before 2013

VARIABLES	(1) Log(Subsidy)	(2) Log(Subsidy)
Pre-2013 Sample (2009–2012)		
Hometown Leader	0.155***	
	(0.040)	
Leader #		0.009**
		(0.004)
Population (log)	−0.033	−0.026
	(0.041)	(0.041)
GDP (log)	−0.213**	−0.235***
	(0.089)	(0.090)
Firm Scale	0.547***	0.549***
	(0.009)	(0.009)
Fiscal Income (log)	0.251***	0.265***
	(0.067)	(0.067)
ROA	0.111	0.117
	(0.139)	(0.139)
Leverage	0.198***	0.196***
	(0.040)	(0.040)
SOE	0.001	−0.002
	(0.023)	(0.023)
Observations	40,027	40,027
R-squared	0.103	0.104

Notes: This table examines the correlation between hometown connections and subsidy allocation before the anti-corruption inspection. The sample includes all publicly listed Chinese firms from 2009 to 2012 (pre-inspection period). The dependent variable is Log(Subsidy), denoting the logarithm value of the amount of subsidy allocation to firm i located in city c in year t. Hometown Leader is a dummy equal to one if prefecture city c had local-born city party secretaries or mayors. Leader # is a continuous variable indicating the average number of Chinese leaders (city mayor and city party secretary) that prefecture city c produces. The analysis controls for regional characteristics (population, GDP per capita, fiscal income) and firm characteristics (firm scale measured by log total assets, ROA, leverage, SOE status). All control variables are measured in the pre-treatment period. Standard errors are clustered at the firm level, indicated in parentheses. Significance level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.4: The Timeline of Anti-corruption Inspection

Province Name	Start Year	Duration	Batch
Panel A. Inspected Provinces in 2013			
Jiangxi	2013	83 days	1
Guizhou	2013	61 days	1
Chongqing	2013	61 days	1
Hubei	2013	51 days	1
Neimenggu	2013	64 days	1
Jilin	2013	57 days	2
Guangdong	2013	59 days	2
Yunnan	2013	59 days	2
Shanxi	2013	60 days	2
Anhui	2013	57 days	2
Hunan	2013	59 days	2
Panel B. Inspected Provinces in 2014			
Hainan	2014	64 days	3
Fujian	2014	60 days	3
Gansu	2014	61 days	3
Henan	2014	60 days	3
Tianjin	2014	61 days	3
Shandong	2014	60 days	3
Xinjiang	2014	55 days	3
Liaoning	2014	56 days	3
Beijing	2014	60 days	3
Ningxia	2014	61 days	3
Xizang	2014	61 days	4
Qinghai	2014	65 days	4
Guangxi	2014	61 days	4
Jiangsu	2014	61 days	4
Heilongjiang	2014	61 days	4
Sichuan	2014	62 days	4
Hebei	2014	58 days	4
Zhejiang	2014	61 days	4
Shanghai	2014	61 days	4
Shaanxi	2014	60 days	4

Notes: This table shows the timeline of the staggered anti-corruption inspection and the duration of each inspection.

Table D.5: Balance Test for Hometown Status

	(1)	(2)	(3)	(4)	(5)	(6)
	1(Hometown Leader)			Leader #		
			Before Inspection			Before Inspection
Log(Fiscal Income)	0.028	0.011	-0.023	0.014	0.058	-0.335
	(0.037)	(0.025)	(0.036)	(0.031)	(0.263)	(0.304)
Log(GDP per capita)	-0.029	0.006	0.020	-0.015	-0.124	0.146
	(0.022)	(0.025)	(0.013)	(0.026)	(0.187)	(0.119)
Log(Population)	-0.046	-0.430	-1.042	-0.568*	-0.085	-1.100
	(0.028)	(0.476)	(0.654)	(0.309)	(1.055)	(1.221)
Log(Firm #)	0.019	0.062	0.063	0.044	0.165	0.173
	(0.017)	(0.049)	(0.102)	(0.045)	(0.191)	(0.378)
Population Growth Rate	-0.002	-0.001	-0.002	-0.001	-0.010	-0.011
	(0.001)	(0.001)	(0.002)	(0.001)	(0.007)	(0.009)
Log(Wage)	0.016	0.009	0.189	-0.125	0.281	-0.494
	(0.115)	(0.177)	(0.257)	(0.203)	(0.586)	(0.940)
Log(Employment)	-0.002	-0.072	0.132*	-0.081	-0.135	-0.069
	(0.038)	(0.081)	(0.074)	(0.065)	(0.169)	(0.250)
Observations	2,782	2,782	1,297	2,782	2,782	1,297
R-squared	0.015	0.558	0.717	0.557	0.794	0.869
Year FEs	✓	✓	✓	✓	✓	✓
City FEs	✓	✓	✓	✓	✓	✓
Regional Controls		✓	✓		✓	✓

Notes: This table illustrates the effect of prefecture city attributes on whether a local-born party secretary or mayor is present, as well as the number of leaders produced by a prefecture city. Standard errors are clustered at the province-year level, indicated in parentheses. Hometown Leader is a dummy denoting whether prefecture city c has a local-born city party secretary or mayor in year t. Leader # is a continuous variable indicating the number of the current Chinese leader (city mayor and party secretary) that prefecture city c produces in year t. I include prefecture fixed effects and year fixed effects. Significance level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.6: Balance Test for Provinces Experienced Early Anti-corruption Inspection

VARIABLES in 2012	(1)	(2)	(3)
	1(Anti-corruption Inspection in 2013)		
Log(GDP per capita)	-0.556	-0.930	-0.633
	(0.381)	(0.819)	(0.992)
Log(Population)	0.219*	-0.076	0.371
	(0.114)	(0.617)	(0.987)
Log(Wage)	0.565	0.468	0.370
	(0.726)	(0.809)	(1.172)
Log(Firm #)		0.063	0.008
		(0.182)	(0.353)
Log(Fiscal Income)		0.238	0.323
		(0.508)	(0.645)
Log(Agricultural Employment)			-0.052
			(0.120)
Log(Manufacturing Employment)			-0.288
			(0.502)
Log(Energy Industry Employment)			0.293
			(0.472)
Log(Construction Industry Employment)			0.058
			(0.351)
Log(Real-estate Industry Employment)			-0.032
			(0.277)
Log(Public Service Employment)			-0.455
			(0.531)
Mean of Dependent Variable	0.354	0.354	0.354
Observations	31	31	31
R-squared	0.143	0.152	0.182

Notes: This table illustrates the effect of provincial attributes in 2012 on anti-corruption inspection in 2013. Standard errors are clustered at the province-year level, indicated in parentheses. Significance level: * $p< 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.7: Career Returns to Hometown Favoritism

	(1)	(2)	(3)	(4)	(5)	(6)
	1(Promotion in the Next Year)					
	Pre-Inspection (2008–2012)			Post-Inspection (2013–2016)		
VARIABLES						
Log(Subsidy Allocated to Hometown)	0.015***	0.020**	0.020**	0.006	−0.032***	−0.032***
	(0.005)	(0.008)	(0.008)	(0.014)	(0.010)	(0.010)
Observations	3,194	3,021	3,021	2,503	2,346	2,346
R-squared	0.386	0.883	0.884	0.348	0.906	0.907
Politician Controls	✓	✓	✓	✓	✓	✓
City FEs	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓
Politician FEs		✓	✓		✓	✓
Cohort FEs			✓			✓

Notes: This table examines whether hometown favoritism generates career benefits for politicians. The sample includes all city-level mayors and party secretaries from 2008–2016. The dependent variable is an indicator equal to 1 if the politician is promoted in the next term. Log(Subsidy Allocated to Hometown) is the log of total subsidies allocated to firms in the politician's birthplace city . Columns (1)–(3) report results for the pre-inspection period (2008–2012), and Columns (4)–(6) report results for the post-inspection period (2013–2016). Standard errors are clustered at the province-year level, indicated in parentheses. Politician controls include start age interacted with deputy provincial status, education, and gender. Significance level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.8: Anti-corruption Inspections and the Mitigating Effect of Length of Subsidy Description

VARIABLES	(1)	(2)	(3)
	Log(Subsidy)		
Panel A. Intra-city Hometown Favoritism			
Hometown Leader × Inspection	-0.331***	-0.341***	-0.360***
	(0.088)	(0.087)	(0.070)
Hometown Leader × Inspection × Subsidy Description Length	0.009**	0.009**	0.009***
	(0.004)	(0.004)	(0.003)
Observations	142,442	142,442	142,442
R-squared	0.266	0.266	0.276
Panel B. Inter-city Hometown Favoritism			
Leader # × Inspection	-0.044***	-0.043**	-0.039**
	(0.016)	(0.016)	(0.015)
Leader # × Inspection × Subsidy Description Length	0.002*	0.002*	0.002*
	(0.001)	(0.001)	(0.001)
Observations	142,442	142,442	142,442
R-squared	0.265	0.266	0.276
Firm FEs	✓	✓	✓
Year FEs	✓	✓	
Pre-firm Controls × Inspection FEs	✓	✓	✓
Regional Controls		✓	✓
2-Digit Industry × Year FEs			✓

Notes: This table illustrates the effect of anti-corruption inspections on subsidies for firms located in the hometown of politicians. The sample includes all publicly listed Chinese firms from 2009 to 2018. Standard errors are clustered at the province-year level, indicated in parentheses, due to the anti-corruption inspections. Across all columns, the main dependent variable is $Log(Subsidy)$, denoting the logarithm value of the amount of subsidy allocation to firm i located in city c in year t. This table reports the coefficients of the interaction term between Hometown Leader (Leader #) × Inspection × Subsidy Description. Subsidy Description Length denotes the length of the subsidy description (Chinese character). Hometown Leader is a dummy denoting prefecture city c that had local-born city party secretaries or mayors from 2009 to 2013. Leader # is a continuous variable indicating the average number of Chinese leaders (city mayor and city party secretary) that prefecture c produces from 2009 to 2013. Inspection denotes the staggered anti-corruption inspections, which takes 1 in affected province p for both the investigation year t and the following years, and 0 for all other years. The analysis includes firm fixed effects, year fixed effects, pre-firm controls interacting with the anti-corruption inspection, and 2-digit industry-year fixed effects. Significance level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.9: The Effect of Anti-corruption on Corrupt Subsidy Allocation

	(1)	(2)	(3)
	Log(Firm Corrupt Subsidy Scores)		
Panel A. Intra-city Hometown Favoritism			
Hometown Leader × Inspection	-0.053***	-0.057***	-0.060***
	(0.019)	(0.018)	(0.018)
Observations	13,166	13,166	13,166
R-squared	0.433	0.433	0.433
Panel B. Inter-city Hometown Favoritism			
Leader # × Inspection	0.001	0.000	-0.000
	(0.004)	(0.004)	(0.004)
Observations	13,166	13,166	13,166
R-squared	0.434	0.435	0.435
Firm FEs	✓	✓	✓
Regional Controls	✓	✓	✓
Pre-firm Controls × Inspection FEs	✓	✓	✓
2-Digit Industry × Year FEs	✓	✓	✓

Notes: This table illustrates the effect of anti-corruption inspections on corrupt subsidy allocation across firms. The sample includes observations from publicly listed Chinese firms from 2009 to 2018. Standard errors are clustered at the province-year level, indicated in parentheses, due to the anti-corruption inspections. Across all columns, the main dependent variable is Log(Corrupt Subsidy Scores), which measures the average textual ambiguity of subsidy descriptions at the firm level. Higher scores indicate greater ambiguity and potential corruption risk, with vague descriptions lacking project details, funding amounts, named recipients, or oversight mechanisms receiving higher scores. This score is calculated using BERT (Bidirectional Encoder Representations from Transformers) to analyze features such as description length, presence of specific keywords, and semantic clarity. Panel A reports the coefficients of the interaction term between Hometown Leader × Inspection. Hometown Leader is a dummy denoting prefecture city c that had local-born city party secretaries or mayors from 2009 to 2013. Panel B reports the interaction between Leader # × Inspection. Leader # is a continuous variable indicating the average number of Chinese leaders (city mayor and city party secretary) that prefecture c produces from 2009 to 2013. Inspection denotes the staggered anti-corruption inspections, which takes 1 in affected province p for both the investigation year t and the following years, and 0 for all other years. The analysis includes firm fixed effects, year fixed effects, pre-firm controls interacting with year fixed effects, provincial controls, and 2-digit industry-year fixed effects. Significance level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.10: Did Entertainment and Travel Costs Decrease After Anti-Corruption Inspection?

VARIABLES	(1) ETC/Profit	(2) ETC/Revenue	(3) ETC/Profit	(4) ETC/Revenue
Hometown Leader × Inspection	0.010	−0.000		
	(0.022)	(0.001)		
Leader # × Inspection			−0.007**	−0.001*
			(0.003)	(0.000)
Observations	10,509	10,509	10,509	10,509
R-squared	0.411	0.783	0.411	0.783
Firm FEs	✓	✓	✓	✓
Regional Controls	✓	✓	✓	✓
Pre-firm Controls × Inspection FEs	✓	✓	✓	✓
Industry × Year FEs	✓	✓	✓	✓

Notes: This table examines whether firm entertainment and travel costs (ETC)—a proxy for rent-seeking expenditures—decreased after anti-corruption inspections. The sample includes all publicly listed Chinese firms from 2009 to 2018. Standard errors are clustered at the province-year level, indicated in parentheses. In Columns (1) and (3), the dependent variable is ETC scaled by profit; in Columns (2) and (4), ETC is scaled by revenue. Hometown Leader is a dummy denoting prefecture city c that had local-born city party secretaries or mayors from 2009 to 2013. Leader # is a continuous variable indicating the average number of Chinese leaders (city mayor and city party secretary) that prefecture c produces from 2009 to 2013. Inspection denotes the staggered anti-corruption inspections, which takes 1 in affected province p for both the investigation year t and the following years, and 0 for all other years. The analysis includes firm fixed effects, industry × year fixed effects, pre-firm controls interacting with inspection, and provincial controls. Significance level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.11: Registration Address vs. Headquarter Location

VARIABLES	(1) Registration Address	(2) Registration Address	(3) Headquarter vs. Registration Location	(4) Headquarter vs. Registration Location	(5) Headquarter vs. Registration Location	(6) Headquarter vs. Registration Location
			HQ City, Power Diff	HQ City, Reg Power	Reg City, Power Diff	Reg City, HQ Power
Panel A. Intra-city Hometown Favoritism						
Hometown Leader (Reg) × Inspection	−0.219***					
	(0.046)					
Panel B. Inter-city Hometown Favoritism						
Leader # (HQ) × Inspection			−0.023***	−0.023***		
			(0.008)	(0.008)		
Leader # (HQ) × Inspection × Power Diff			0.079			
			(0.081)			
Leader # (HQ) × Inspection × Reg Power				−0.027		
				(0.057)		
Leader # (Reg) × Inspection		−0.023***			−0.023***	−0.025***
		(0.008)			(0.008)	(0.009)
Leader # (Reg) × Inspection × Power Diff					0.048	
					(0.466)	
Leader # (Reg) × Inspection × HQ Power						0.002
						(0.004)
Observations	142,442	142,442	142,442	142,442	142,442	142,442
R-squared	0.274	0.274	0.274	0.274	0.274	0.274
Firm FEs	✓	✓	✓	✓	✓	✓
Regional Controls	✓	✓	✓	✓	✓	✓
Pre-firm Controls × Inspection FEs	✓	✓	✓	✓	✓	✓
2-Digit Industry × Year FEs	✓	✓	✓	✓	✓	✓

Notes: This table examines heterogeneity in anti-corruption effects based on firm location. Columns (1)–(2) use the firm's registration address to define hometown connections. Columns (3)–(6) distinguish between headquarter (HQ) and registration (Reg) locations to test whether political connections operate through different geographic channels. Leader # (HQ) measures the number of leaders produced by the firm's headquarter city; Leader # (Reg) measures leaders produced by the registration city. Power Diff captures the difference in political influence between HQ and registration cities. Reg Power and HQ Power measure political influence of the registration and headquarter cities, respectively. The sample includes all publicly listed Chinese firms from 2009 to 2018. Standard errors clustered at the province-year level are reported in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.12: Hometown Favoritism by Politicians and Crowding Out Effect on Local Public Expenditure

	(1)	(2)
	Log(1+ City Public Expenditure)	
Panel A. Intra-city Hometown Favoritism		
Hometown Leader × Inspection	0.029**	0.027**
	(0.012)	(0.012)
Observations	2,850	2,850
R-squared	0.987	0.987
Panel B. Inter-city Hometown Favoritism		
Leader # × Inspection	0.002	0.002
	(0.003)	(0.003)
Observations	2,850	2,850
R-squared	0.987	0.987
City FEs	✓	✓
Year FEs	✓	✓
Regional Controls	✓	✓
Pre-provincial Controls × Inspection FEs		✓

Notes: This table illustrates the effect of anti-corruption inspection on prefecture city c's public expenditures. Standard errors are clustered at the province-year level, indicated in parentheses, due to the anti-corruption inspection. This table reports the coefficients of the interaction term between Hometown Leader/Leader # and Inspection. Log(1+ City Public Expenditure) denotes the logarithm value of the prefecture city c's public expenditure. Hometown Leader is a dummy denoting prefecture city c that had local-born city party secretaries or mayors from 2009 to 2013. Leader # is a continuous variable indicating the average number of Chinese leaders (city mayor and city party secretary) that prefecture c produces from 2009 to 2013. Inspection denotes the staggered anti-corruption inspections, which takes 1 in affected province p for both the investigation year t and the following years, and 0 for all other years. I include provincial controls, prefecture city fixed effects, year fixed effects, and pre-provincial controls interacting with the inspection. Significance level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.13: Firm Heterogeneity in TFP Gains: SOE Status

	(1)	(2)	(3)	(4)	(5)
	Total Factor Productivity				
VARIABLES	OLS	LP	ACF	WRDG	IND
Panel A. Intra-city Hometown Favoritism					
Hometown Leader × Inspection	0.101**	0.102**	0.100**	0.101**	0.094**
	(0.045)	(0.045)	(0.046)	(0.045)	(0.045)
Hometown Leader × Inspection	−0.003	0.002	−0.011	0.002	−0.026
× SOE	(0.052)	(0.052)	(0.053)	(0.052)	(0.052)
Observations	8,376	8,376	8,376	8,376	8,376
R-squared	0.777	0.779	0.790	0.779	0.989
Panel B. Inter-city Hometown Favoritism					
Leader # × Inspection	0.005	0.005	0.005	0.005	0.002
	(0.008)	(0.008)	(0.007)	(0.008)	(0.007)
Leader # × Inspection	0.002	0.002	−0.000	0.002	0.003
× SOE	(0.009)	(0.009)	(0.008)	(0.009)	(0.008)
Observations	8,376	8,376	8,376	8,376	8,376
R-squared	0.776	0.779	0.790	0.778	0.989
Firm FEs	✓	✓	✓	✓	✓
Regional Controls	✓	✓	✓	✓	✓
Pre-firm Controls × Inspection	✓	✓	✓	✓	✓
Industry × Year FEs	✓	✓	✓	✓	✓

Notes: This table examines heterogeneity in TFP gains by SOE status following anti-corruption inspections. The sample includes all publicly listed Chinese firms from 2009 to 2018. Standard errors are clustered at the province-year level, indicated in parentheses. The dependent variable is firm-level total factor productivity (TFP), estimated using five methods: ordinary least squares (OLS), Levinsohn and Petrin (2003) (LP), Ackerberg et al. (2015) (ACF), Wooldridge (2009) (WRDG), and industry-specific production functions (IND). SOE is an indicator for state-owned enterprises. Hometown Leader is a dummy denoting prefecture city c that had local-born city party secretaries or mayors from 2009 to 2013. Leader # is a continuous variable indicating the average number of Chinese leaders that prefecture c produces from 2009 to 2013. Inspection denotes the staggered anti-corruption inspections, which takes 1 in affected province p for both the investigation year t and the following years, and 0 for all other years. Significance level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.14: Robustness Test: More Fixed Effects and Subsample Analyses

	(1)	(2)	(3)	(4)
			Log(Subsidy)	
VARIABLES	Province-Year FEs	Province Specific Trend	No Provincial-level Cities	No Government-dependent Sectors
Panel A. Intra-city Hometown Favoritism				
Hometown Leader × Inspection	-0.131**	-0.245***	-0.165**	-0.243***
	(0.058)	(0.069)	(0.063)	(0.068)
Observations	142,442	142,442	116,354	115,346
R-squared	0.281	0.276	0.238	0.269
Panel B. Inter-city Hometown Favoritism				
Leader # × Inspection	-0.026**	-0.018*	-0.020*	-0.024**
	(0.011)	(0.010)	(0.011)	(0.011)
Observations	142,442	142,442	116,354	115,346
R-squared	0.281	0.276	0.238	0.269
Firm FEs	✓	✓	✓	✓
Year FEs		✓		
Regional Controls		✓	✓	✓
Pre-firm Controls × Inspection	✓	✓	✓	✓
2-Digit Industry × Year FEs	✓	✓	✓	✓
Province × Year FEs	✓			
Province Specific Linear Time Trend		✓		

Notes: This table illustrates the robustness of the baseline results to additional fixed effects and subsample analyses. The sample includes all publicly listed Chinese firms from 2009 to 2018, except in columns (3) and (4) which use subsamples as indicated. Standard errors are clustered at the province-year level, indicated in parentheses, due to the anti-corruption inspections. Across all columns, the main dependent variable is Log(Subsidy), denoting the logarithm value of the amount of subsidy allocation to firm i located in city c in year t. This table reports the coefficients of the interaction term between Hometown Leader (Leader #) × Inspection. Hometown Leader is a dummy denoting prefecture city c that had local-born city party secretaries or mayors from 2009 to 2013. Leader # is a continuous variable indicating the average number of Chinese leaders (city mayor and city party secretary) that prefecture c produces from 2009 to 2013. Inspection denotes the staggered anti-corruption inspections, which takes 1 in affected province p for both the investigation year t and the following years, and 0 for all other years. The analysis includes firm fixed effects, year fixed effects, pre-firm controls interacting with the anti-corruption inspection, and other controls as indicated. Significance level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.15: Robustness Test: Do Provinces With More Leaders Face Intensive Inspections?

VARIABLES	(1) Media Anti-corruption 1	(2) Media Anti-corruption 2	(3) Leader Expelled	(4) Corrupt Cases
Panel A. Intra-city Hometown Favoritism				
Hometown Leader # × Inspection	0.004	0.020	0.041	0.031
	(0.028)	(0.029)	(0.053)	(0.052)
Observations	300	300	300	300
R-squared	0.913	0.944	0.471	0.956
Panel B. Inter-city Hometown Favoritism				
Leader # × Inspection	−0.003	−0.001	0.006	0.001
	(0.003)	(0.003)	(0.004)	(0.007)
Observations	300	300	300	300
R-squared	0.914	0.944	0.478	0.956
Province FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
Regional Controls	✓	✓	✓	✓

Notes: This table illustrates the effect of anti-corruption inspections on media coverage of anti-corruption-related terms at the province level. The sample includes all Chinese provinces from 2009–2018. Standard errors clustered at the province-year level are in parentheses. Dependent variables: (1) Log of newspaper titles including *fanfubai* (anti-corruption); (2) Log of titles including *fubai* (corruption); (3) Indicator for provincial governor/party secretary expulsion; (4) Log(1 + corruption cases). In Panel A, Hometown Leader # is the average number of local-born city party secretaries in province p during 2009–2013. In Panel B, Leader # is the average number of city mayors and party secretaries from province p during 2009–2013. Inspection equals 1 for affected provinces in the investigation year and subsequent years. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.16: Robustness Test: Placebo Anti-corruption Inspections (No Contaminated Post-period Coefficients)

VARIABLES	(1)	(2)	(3)	(4)
	Log(Subsidy)			
Panel A. Intra-city Hometown Favoritism				
Hometown Leader × Placebo Inspection in 2012	-0.000			
	(0.084)			
Hometown Leader × Placebo Inspection in 2011		0.044		
		(0.066)		
Panel B. Inter-city Hometown Favoritism				
Leader # × Placebo Inspection in 2012			-0.018	
			(0.012)	
Leader # × Placebo Inspection in 2011				0.013
				(0.009)
Observations	39,990	39,990	39,990	39,990
R-squared	0.350	0.349	0.350	0.350
Firm FEs	✓	✓	✓	✓
Pre-firm Controls × Post 2012 (2011)	✓	✓	✓	✓
Regional Controls	✓	✓	✓	✓
2-Digit Industry × Year FEs	✓	✓	✓	✓

Notes: This table illustrates the effect of placebo anti-corruption efforts on allocated subsidies for firms located in the prefecture city that had local-born city party secretaries/produced more Chinese leaders. The sample includes all Chinese listed firms in the period 2009-2012 (omitted observations after the actual anti-corruption inspection). Standard errors are clustered at the province-year level, indicated in parentheses, due to the anti-corruption inspection. Across all columns, the main dependent variable is $Log(Subsidy)$, denoting the logarithm value of the amount of subsidy allocation to firm i located in city c in year t. This table reports the coefficients of the interaction term between Hometown Leader/Leader # and placebo anti-corruption efforts. Hometown Leader is a dummy denoting prefecture city c that had local-born city party secretaries or mayors from 2009 to 2012. Leader # is a continuous variable indicating the average number of Chinese leaders (city mayor and city party secretary) that prefecture c produced between 2009 and 2012. Post 2011 is an indicator variable that takes 1 after 2011 (the placebo anti-corruption inspection); Post 2012 is an indicator variable that takes 1 after 2012 (the placebo anti-corruption inspection). I limit the sample after 2012, which means that the observations after 2012 across all columns are dropped. The analysis includes firm fixed effects, year fixed effects, pre-firm controls interacting with the placebo anti-corruption inspection, regional controls, and 2-digit industry-year fixed effects. Significance level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.17: Robustness: Leaver Results Excluding Cities with Successor Hometown Leaders

	(1)	(2)
	Log(Subsidy)	
VARIABLES	Intra-city Favoritism	Inter-city Favoritism
Hometown Leader (Stayer) × Inspection	−0.265**	
	(0.120)	
Hometown Leader (Leaver) × Inspection	−0.221**	
	(0.110)	
Leader # (Stayer) × Inspection		−0.064***
		(0.013)
Leader # (Leaver) × Inspection		0.013
		(0.011)
Sample Period	2011–2018	2011–2018
Observations	125,214	125,214
R-squared	0.285	0.285
Firm FEs	✓	✓
Regional Controls	✓	✓
Pre-firm Controls × Inspection FEs	✓	✓
2-Digit Industry × Year FEs	✓	✓

Notes: This table replicates the Leaver results from Table 7 using a restricted sample that addresses the concern that a departing hometown leader may have been replaced by *another* hometown leader. Specifically, the Leaver group is restricted to cities where no hometown leader held office in any year after the 2013 inspection. If the Leaver results were driven by a successor hometown leader continuing — and then curtailing — favoritism, we would expect the coefficients to shrink or lose significance in this restricted sample. The stability of the Leaver coefficients relative to Table 7 confirms that the results reflect the exit of the original incumbent rather than the behavior of any successor. Stayers are politicians in office during 2011–2012 who remained through 2014; Leavers are politicians in office during 2011–2012 who departed by 2014. Standard errors are clustered at the province-year level, indicated in parentheses. Significance level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.