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Three Essays on Applied Microeconomics and Political Economy

A Dissertation submitted in partial satisfaction
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by

Andong Yan

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To my family for all the support.

ABSTRACT OF THE DISSERTATION

Three Essays on Applied Microeconomics and Political Economy

by

Andong Yan

Doctor of Philosophy, Graduate Program in Economics
University of California, Riverside, June 2024
Dr. Urmee Khan, Chairperson

This dissertation consists of three independent essays in applied microeconomics and political economy.

Chapter 1 is an introduction, which summarizes the research questions, data, research methods, and main findings for the following chapters.

Chapter 2 presents an investigation of the economic consequences of the zero-COVID policy implemented by the Chinese government as a pilot experiment in using big data for country management from 2020 to 2022. Our study includes an original county-daily panel data set on the COVID-19 *Risk Level* issued by the State Council of the People's Republic of China (PRC). To measure economic activities, we used satellite data on night lights and PM2.5, and geographical data on population mobility. Our findings indicate that the zero-COVID policy did not result in significant economic loss in 2021. However, in 2022, when the Omicron variant emerged, a stricter zero-COVID policy led to a 30% decline in mobility, a 1.17% decrease in PM2.5 and a 7.7% reduction in night lights. Based on our

calculations, China experienced a 3.9% loss in GDP as a consequence of the implementation of the zero-COVID policy in 2022.

Chapter 3 investigates the compliance of local Chinese officials with the zero-Covid policy throughout the COVID-19 pandemic. By examining biographical data from political elites and using a prefecture-day data set on risk levels – an indicator reflecting the status of zero-Covid policy - we discover a significant impact of prefecture leaders' promotion incentives on their response to COVID-19 outbreaks. Our empirical analysis reveals that leaders with stronger promotion incentives tend to exhibit increased reactions to emerging cases. Evidence shows that such a phenomenon is driven by the different choices of the prefecture leaders facing relatively larger-scale COVID-19 outbreaks. Furthermore, local governors whose jurisdictions are more economically developed tend to enforce more stringent mobility restrictions. However, for prefecture leaders who oversee more developed regions and possess strong promotion incentives, the combined effects of these two factors tend to balance each other out in terms of pandemic response. These results suggest a natural tension between demands for crisis management during the pandemic and routine performance in economic development within the political framework of China.

Chapter 4 is a work in progress, which focuses on the principal-agent problem when both principal and agent are constrained only by limited commitment. We parameterize the commitment environment by two factors, the probability of potential contract breach and the cost of contract default. In contrast to the conventional wisdom that lack of commitment (high chance and low cost for contract default) would harm parties' benefits in the contracting, we find that the principal could obtain positive marginal benefits with a higher probability of contract breach, particularly when the costs associated with violating the contract are relatively low. The driving forces behind this unexpected result are that the potential threat of contract breach could behave as a screening tool to separate the agent in their reporting strategy, which leads to a more efficient payment scheme for the principal in the equilibrium.

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Chapter 1

Introduction

This dissertation consists of three independent essays in applied microeconomics and political economy.

Chapter 2 is based on a published paper *Economic Impacts of China's Lockdown and Zero-COVID Policies during COVID-19.* This chapter presents an investigation of the economic consequences of the zero-COVID policy implemented by the Chinese government as a pilot experiment in using big data for country management from 2020 to 2022. Our study includes an original county-daily panel data set on the COVID-19 *Risk Level* issued by the State Council of the People's Republic of China (PRC). To measure economic activities, we used satellite data on night lights and PM2.5, and geographical data on population mobility. Our findings indicate that the zero-COVID policy did not result in significant economic loss in 2021. However, in 2022, when the Omicron variant emerged, a stricter zero-COVID policy led to a 30% decline in mobility, a 1.17% decrease in PM2.5 and a 7.7%

reduction in night lights. Based on our calculations, China experienced a 3.9% loss in GDP as a consequence of the implementation of the zero-COVID policy in 2022.

Chapter 3 is a working paper, *Crisis Control in Top-down Bureaucracy: Evidence from China's Zero-Covid Policy*. This chapter investigates the compliance of local Chinese officials with the zero-Covid policy throughout the COVID-19 pandemic. By examining biographical data from political elites and using a prefecture-day data set on risk levels – an indicator reflecting the status of zero-Covid policy - we discover a significant impact of prefecture leaders' promotion incentives on their response to COVID-19 outbreaks. Our empirical analysis reveals that leaders with stronger promotion incentives tend to exhibit increased reactions to emerging cases. Evidence shows that such a phenomenon is driven by the different choices of the prefecture leaders facing relatively larger-scale COVID-19 outbreaks. Furthermore, local governors whose jurisdictions are more economically developed tend to enforce more stringent mobility restrictions. However, for prefecture leaders who oversee more developed regions and possess strong promotion incentives, the combined effects of these two factors tend to balance each other out in terms of pandemic response. These results suggest a natural tension between demands for crisis management during the pandemic and routine performance in economic development within the political framework of China.

Chapter 4 is a work in progress, *Do Autocrats Break Their Promises? A Principal-Agent Problem with Limited Commitment*. This chapter focuses on the principal-agent problem when both principal and agent are constrained only by limited commitment. We parameterize the commitment environment by two factors, the probability of potential contract breach and the cost of contract default. In contrast to the conventional wisdom that lack of commitment (high chance and low cost for contract default) would harm parties' benefits in the contracting, we find that the principal could obtain positive marginal benefits with a higher probability of contract breach, particularly when the costs associated with violating the contract are relatively low. The driving forces behind this unexpected result are that the potential threat of contract breach could behave as a screening tool to separate the agent in their reporting strategy, which leads to a more efficient payment scheme for the principal in the equilibrium.

Chapter 2

Economic Impacts of China's Lockdown and Zero-COVID Policies during COVID-19

2.1 Introduction

The COVID-19 pandemic severely disrupted general economic activity as human mobility was restricted, social gatherings were banned, and businesses were halted. However, research that examines the effects of the pandemic on the economy has focused primarily on specific areas, such as unemployment, consumer spending, labor demand, and pollution. There is a demand for a comprehensive assessment of the economic consequences of the pandemic and the corresponding anti-contagion policies. Additionally, most of the research has focused only on the year 2020 and has not considered the subsequent periods 2021 and 2022. Our paper aims to fill this gap.

In this paper, we compile a unique dataset of China's COVID-19 risk level on prefecture/county level, which is constructed based on big data provided by the State Council of the People's Republic of China (PRC). We examine the impact associated with China's COVID-19 policies on several salient economic indicators from 2020 to 2022. Specifically, we analyze the effects on mobility, air pollution measured by the concentration of fine particulate matter (PM2.5) and night lights. We rely on a difference-in-differences framework for identification, with the assumption that, conditional on daily confirmed COVID-19 cases and other prefecture-day level controls, the difference in economic indicators between regions with and without COVID-19 containment policies would remain stable over time.

From February 17, 2020, after one month of the pandemic outbreak and a series of strict lockdown measures, China has utilized big data and established a nationwide risk-level system, which aimed to contain the spread of the virus within communities while keeping the economic costs to a minimum, also referred to as "zero-COVID" policy. To be specific, China implemented a nationwide risk response system that mandated local officials to classify communities into low-, medium-, and high-risk levels based on recent confirmed COVID-19 cases and other factors. Areas rated as medium- and high-risk imposed more stringent containment measures compared to low-risk areas, such as stay-at-home order, mass testing, contact tracing and mobility restrictions. Therefore, the classification of an area as *Risk* or non-risk is closely linked to the stringency of the zero-COVID policies enforced by local authorities.

It is important to evaluate the economic consequences of zero-COVID policy in the context of both economics and politics. Zero-COVID policies are considered as the Chinese government's pilot experiment in using big data for national management and crisis response.[1] In 2021, China's media outlets portrayed the low mortality rate from COVID-19 as the success of this risk-level system. Moreover, China's GDP growth rate reached 8.1% in 2021. The Chinese government has been promoting their zero-COVID policies as a model for the rest of the world to follow, claiming that it has been effective in both preserving lives while maintaining economic growth. However, in 2022, the emergence of the Omicron variant resulted in shutdowns of financial, manufacturing, and exporting centers, including Shanghai, Shenzhen, Guangzhou, and Changchun, leading to the failure of China's zero-COVID policy to safeguard people's lives and economic vitality (Mark and Schuman, 2022).

Using an original daily panel data at the prefecture/county-level on COVID-19 risk levels collected from the website of the State Council, our study firstly shows that on average the zero-COVID policy took 21 days to eliminate local COVID-19 cases in 2021, but it took approximately 50 days in 2022. Our second finding reveals a 30% reduction in inter-prefecture traffic flow after a prefecture has been classified as a *Risk* region in either 2021 or 2022. Furthermore, our study revealed that the probability of being classified as a *Risk* region was positively and significantly associated with changes in PM2.5 and night lights in 2021, while the effects of the zero-COVID policy are negligible. However, in 2022, the zero-COVID policy led to a decrease in PM2.5 concentration by 1.17% and a reduction

[1] Check out the coverage provided by state-controlled media: `https://www.tsinghua.edu.cn/info/1182/51343.htm`

in night lights by 7.7%. The differences in policy effects observed between 2021 and 2022 can be primarily attributed to differences in the stringency of the zero-COVID policy. In 2022, with the emergence of the Omicron variant and stricter zero-COVID policies, the negative policy effects on economic activities became significantly larger. Our back-of-the-envelope calculations indicate that the zero-COVID policy caused China to experience a reduction of around 3.9% in GDP in 2022.

The previous studies on COVID-19 pandemic in China have two limitations. First, the majority of studies draw their conclusions focusing on lockdown policies in the early stage of 2020 rather than zero-COVID policies in 2021 and 2022.[2] To date, only one paper has estimated the economic impacts using truck flows in 2020 and 2021 (Chen et al., 2022b). However, it is worth noting that the policy object under study in this paper is prefecture-level city lockdown, rather than zero-COVID policy, therefore it could not account for less stringent policies such as restrictions on human mobility, the establishment of body temperature checkpoints, neighborhood sanitization, monitoring of suspected COVID-19 cases, and other anti-contagious measures at the local community level. Second, they primarily focused on the economic consequences of COVID policies from a single aspect. Dang et al. (2023); Gong et al. (2022a); Zhang (2021) focus on the COVID-19 policies' adverse effects on labor market outcomes such as unemployment, wage, and labor market participation. Using high-frequency transaction data, Chen et al. (2021) provided evidence that the pandemic has caused a sharp decline in consumption immediately after the COVID outbreak. Fang et al. (2020c) documented that the human mobility restrictions imposed by Chinese

[2] For example, see Fang et al. (2020a); He et al. (2020); Fang et al. (2020c); Liu et al. (2020). For a systemic review, see Huang et al. (2023)

government in the early phase of the pandemic effectively controlled the spread of the virus. Despite the seemingly high economic and social costs, researchers have also shown that the COVID-19 pandemic significantly improved air quality and reduced environmental pollution (He et al., 2020; Brodeur et al., 2021).

This paper makes three primary contributions. First of all, to be best of our knowledge, our paper is the first empirical study that examines the economic impact of the zero-COVID policy spanning from 2020 to 2022. We offer evidence of the heterogeneous outcomes linked to the implementation of the zero-COVID policy during the three-year pandemic. This research provides insight into the efficacy of the zero-COVID strategy in contributing to China's rapid economic recovery in 2021, and also highlights the disruptions caused by the escalating pandemic and the frequent re-imposition of the zero-COVID policy in 2022. Second, we compiled a unique dataset that reflects the stringency of China's zero-COVID policy. Our dataset provides daily risk level indices at the county level in China from April 2021 to December 2022, including 2853 counties and 368 prefecture-level cities. Local governments have implemented various anti-contagion policies based on risk ratings. The granularity of our dataset could provide new insights and serve as a valuable tool for future research in general to better understand the economic consequences of the pandemic and the zero-COVID policies in China. Lastly, our paper contributes to the existing literature with an in-depth analysis of the economic impact of the COVID-19 policies along three dimensions: human mobility, air pollution, and night lights. The three outcomes in our research offer varying insights into economic performance, such as transportation, manufacturing, and service sectors. Furthermore, the inter-prefecture traffic mobility index and PM2.5 can be used as proxies for short-term economic activities, particularly human mobility and factory productions. On the other hand, night lights can be used as proxies for medium-term economic activities.

The remainder of this paper is structured as follows. Section 2 details the policy background and data. Section 3 delineates the identification strategy. Section 4 presents the main results and performs robustness checks. Section 5 concludes.

2.2 Policies and Data

In this section, we cover basic facts and data source. Initially, we outline China's COVID-19 policies, encompassing lockdown and the zero-COVID. Then, we describe the sources of data for mobility, pollution, and night lights. Finally, we describe the control variables, which include daily confirmed cases and weather.

2.2.1 China's COVID-19 Policy — Lockdown (Jan 23 — Feb 16, 2020)

With the initial COVID-19 outbreak in Wuhan in 2020, the Chinese government implemented unprecedented prefecture lockdown to contain the virus. Stringent measures were put in place in the locked-down prefectures, including the prohibition of traffic leaving, the imposition of stay-at-home orders, and the enforcement of quarantine measures. It's worth mentioning that anti-contagion policies were also enforced in prefectures without lockdowns, albeit with less strict measures compared to the locked-down ones. According to Qiu et al. (2020), by February 16, 2020, more than 250 prefectures had implemented such measures.[3] Starting from February 17, 2020, the Chinese government implemented a policy package to precisely contain COVID-19 transmission at the community level. As a result, the central government no longer recommended prefecture-level lockdowns, as they were considered too detrimental to the economy.

The "Lockdown" in this study is defined as China's major COVID-19 policy from January 23 to February 16, 2020. Our data on lockdowns come from He et al. (2020), who originally collected from Wikipedia, various sources of news media and government announcements.

2.2.2 China's COVID-19 Policy — zero-COVID (Feb 17, 2020 — Dec 25, 2022)

Following the one-month-long enforcement of strict lockdowns and nationwide public health interventions, the central government sought to revive the economy and loosen the lockdown measures (Gong et al., 2022a). On February 17, Prevention Guidance for Novel Coronavirus Pneumonia (version 5) was issued by the State Council and National Health Commission of China.[4] This guidance mandated local governments to classify COVID-19 risk at the community level. Any community that reported COVID-19 cases would be categorized as either a medium- or high-risk zone, and corresponding containment measures and closures would be enforced. However, in principle, low-risk communities should only impose quarantines on individuals traveling from medium- or high-risk areas and should not limit the traveling of residents or economic activities. The objective of this policy is to

[3] "In all Chinese cities, the Spring Festival holiday was extended, and people were advised to stay at home when possible, enforce social distancing and maintain good hygiene." (He et al., 2020)

[4] Prevention Guidance for Novel Coronavirus Pneumonia (version 5): `http://www.nhc.gov.cn/jkj/s3577/202002/a5d6f7b8c48c451c87dba14889b30147.shtml`

eradicate COVID-19 transmission at the local level by assigning each community a risk level and implementing corresponding measures. This is commonly known as the zero-COVID policy.

In order to comply with the guidance, starting from March 2020, the State Council of China began to release a national COVID-19 risk level system on a regular basis through its website. This system categorizes communities within the 2853 counties into high-, medium-, or low-risk groups and updates on a daily basis. All zero-COVID policies, including quarantine, closures of public places, travel restrictions, Travel QR Codes, etc., were implemented based on this system.[5] The COVID-19 risk level system is viewed as a pilot experiment in utilizing big data for national management and crisis response.[6] In particular, the risk level is reported by local governments and compiled by National Health Commission of China.[7] The criteria used to designate a community as either a *Risk* or non-risk area are based on the presence of confirmed cases of COVID-19 reported within recent days. It is important to note that local officials have some flexibility to adjust the coverage range of medium- or high-risk areas. In cases of overreaction, neighboring communities without any cases may still be classified as medium- or high- risk.

Our data on risk level information are drawn from *China's COVID-19 Risk Level Dataset*, a newly constructed dataset containing COVID-19 risk level information for communities within the 2853 counties on a daily basis from April 02, 2021 to December 15,

[5]Check out the news from State Council's website: `http://www.gov.cn/fuwu/2020-03/25/content_5495289.htm`

[6]Check out the coverage provided by state-controlled media: `https://www.tsinghua.edu.cn/info/1182/51343.htm`

[7]The term "risk" used in this context is distinct from its traditional usage in economic research, which involves prediction and expectation. Here, "risk" refers to the assessment of COVID-related risk based on the current presence of COVID-19 cases.

2022, which marks the end of the zero-COVID policies. This information was collected from the State Council's website (see Appendix A for more details). To the best of our knowledge, this is the first dataset to document China's county-level daily implementation of the zero-COVID policy during 2021 and 2022.[8] We define a county as *Risk* region on a given day if it contains at least one community categorized as medium- or high- risk according to the aforementioned criteria. We define a prefecture as *Risk* region on a given day if at least one community within it is categorized as *Risk* area.

Table 2.1 shows that on average, from April 02, 2021 to December 15, 2022, 74 counties were classified as *Risk* regions on a daily basis. Averagely, each county was classified as *Risk* region for a duration of 16 days by December 15, 2022 (the end of zero-COVID).

Figure 2.1 shows that the aggregate nationwide daily confirmed cases correlates positively with number of counties with *Risk* areas.[9] Furthermore, we have noticed a steep rise in the number of counties categorized as *Risk* regions beginning in July 2022, while the number of confirmed cases experienced a sharp surge starting only after October 2022. These trends suggest that, comparing to 2021, local officials may be more inclined to enforce stricter zero-COVID policies or potentially overreact with their policies in response to the more transmissible Omicron variant in 2022. This finding is further supported by Figure 2.2, which illustrates a comparison between the green bar and blue bars. The results show that in 2022, there were much more counties classified as *Risk* regions for longer duration compared to 2021. Additionally, Figure Figure A.2 indicates that only a small fraction of counties

[8]The previous research mainly focus on 2020 or lockdowns, rather than 2021 and 2022 or zero-COVID.

[9]Shanghai is excluded from the sample due to a skyrocketed increase in COVID-19 cases during April 2022.

were classified as *Risk* regions in 2021, whereas by the end of 2022, 1700 out of 2853 counties were classified as *Risk* regions.[10]

There are three things worth noting. Firstly, our binary variable of a county classified as *Risk* or non-risk region does not differentiate the level of intensity of treatment. For instance, a county with only one community designated as *Risk* area and another county with 100 communities designated as *Risk* areas are likely to receive varying impacts from zero-COVID policies. Although there will be differences in the treatment, we are unable to distinguish between them. Secondly, our risk level data does not provide information on the specific zero-COVID policies implemented in each county. For example, if two counties with the same number of communities are classified as *Risk* areas, County A may require all residents to stay home, while County B may only quarantine individuals who have tested positive for COVID-19. The bottom line is that as long as a county/prefecture is categorized as *Risk* region, corresponding zero-COVID policies will be implemented in this region. Finally, a prefecture-wide lockdown remains as an option within the zero-COVID policy framework for the years 2021 and 2022,[11] despite variations in official terminology like "citywide static management", "silence period" and so on. Our research does not aim to differentiate between lockdown and other aspects of the zero-COVID policy during 2021 and 2022. Instead, we regard our estimates as capturing the average impact of a range of interventions, including both stringent measures like lockdowns and milder restrictions.

[10] See Panel B of Table 2.1

[11] Prominent cities such as Xi'an and Shanghai implemented lockdown measures, with Xi'an being in lockdown for approximately a month starting from the end of 2021, and Shanghai undergoing a lockdown for about four months during the first half of 2022.

2.2.3 Mobility

We use the data from the Baidu Qianxi (Migration) website, which is publicly shared by Hu et al. (2020b), to construct our measures of human mobility. Baidu is the largest search engine in mainland China. Their migration data are based on real-time location records for every smart phone that uses the company's mapping app, and thus can accurately reflect population mobility between cities.

The Baidu Qianxi data set covers 120,142 pairs of prefecture-level cities per day for 364 such cities. For each prefecture-level city, Baidu Migration provides the following two sets of information: (1) the top 100 origination cities for the population moving to the target city and the corresponding percentages of the inflow population that originated from each of the top 100 origination cities; (2) the top 100 destination cities for the population moving out of the city and the corresponding percentages of the outflow population that go into each of the top destination cities (Fang et al., 2020c). The mobility data used in this research cover the periods from January 1, 2020, to March 27, 2021 and from September 2, 2021, to April 21, 2022.

To achieve our research objectives, we converted the raw mobility data into two daily indices at the prefecture level: inflow mobility and outflow mobility. To compute the inflow mobility index for a given prefecture-level city, e.g. City A, we averaged the outflow values from all other cities directed toward City A, based on Baidu Qianxi data for a specific date.[12] Specifically, this average is derived from the percentages of outflow population originating from cities that include City A in their list of top 100 mobility

[12] In this context, the outflow mobility from other cities to City A is essentially considered as inflow mobility for City A.

destinations. Similarly, for the outflow mobility index, we followed the same procedure but substituted inflow values for outflow values in the Baidu Qianxi data. When City A implements the zero-COVID policy and assuming inter-city traffic among other cities remains constant, the share of population mobility associated with City A relative to the total population mobility of other cities is likely to decline due to imposed restrictions. This anticipated decrease would be reflected in the mobility indices we have devised.

2.2.4 PM2.5

The county-level weekly data on PM2.5 is derived from the Aerosol Optical Depth (AOD) data, which are from NASA's Global Modeling and Assimilation Office (GMAO) released Modern-Era Retrospective analysis for Research and Applications, Version 2 (MERRA-2). Comparing to station-level PM 2.5 data, satellite data cover all the counties in China and are widely used in economic research.[13] The data is reported with a nested resolution of 50km × 60km at a hourly base. Firstly, the grid-level PM2.5 concentration is computed using the formula provided by Buchard et al. (2016). Next, to achieve a higher resolution, we split each grid into smaller grids of 5km x 6 km using an upsampling method.[14] Lastly, we adopt the *Raptor Join* method described in Singla et al. (2021) to aggregate the data from the smaller grids into county-level for each hour and compute the weekly sum for each county.[15]

[13] see Fu et al. (2021); Chen et al. (2022d); Sager and Singer (2022).

[14] If we do not upsample, there will be missing values for some counties that are smaller than 50km × 60km in size.

[15] To account for the daily air pollution's high volatility, we follow He et al. (2020) and aggregate the PM 2.5 at the weekly level.

2.2.5 Night Lights

China's government has not released any county or prefecture-level GDP data for the years 2020 to 2022. Even if such data were available, there are concerns about the possibility of manipulation and over-reporting (Martinez, 2022; Angrist et al., 2021). To obtain a consistent measure of local economic activity across China, we utilize visible lights emitted from the Earth's surface at night as a proxy — night lights (nighttime light) data have already been recognized to be capable of accurately capturing changes in local economic activity (Hodler and Raschky, 2014).[16]

We obtain the night lights data from the Visible Infrared Imaging Radiometer Suite (VIIRS) on a monthly basis,[17] covering the period from 2019 to September 2022. To filter out noise from sources such as aurora, fires, and other temporary lights, we employ a threshold of 0 and $1.5(\mu + 3\sigma)$, following Li et al. (2020); Gibson (2021).[18] The spatial resolution of VIIRS image data is 413m, the absolute radiation values in the unit of $Watts/cm^2/sr$ (Chen et al., 2022c). We use the same *Raptor Join* method describe in the PM 2.5 section to aggregate the grids at county level by month.

2.2.6 Weather Data

We obtain the weather data including precipitation and temperature from Global Historical Climatology Network form the National Oceanic and Atmospheric Ad-

[16] Also see Harari (2020); Storeygard (2016); Henderson et al. (2018) and Donaldson and Storeygard (2016) for a comprehensive review of economic literature using night lights as proxy for economic actives.

[17] See Elvidge et al. (2017). The raw data from VIIRS is at monthly basis.

[18] See Figure A.3, an example of filtered data of Night Lights in March 2022 obtained from VIIRS, combine with the shapefile of China's county boundary.

minictration (NOAA).[19] We use the inverse distance weights to calculate the daily prefecture-level weather data.

2.2.7 Daily Confirmed COVID-19 Cases

We gather the daily confirmed COVID-19 cases provided by the *Dingxiangyuan* website, which compiles official daily COVID-19 cases at the prefecture level.

2.3 Identification

Our empirical analysis relies on two sets of difference-in-differences (DiD) models to identify the impact of the zero-COVID policy on the pandemic's dynamics during local outbreaks and its subsequent influence on various measures of economic activity, including traffic mobility, air pollution, and night lights. We employ a DiD specification as our baseline regression to estimate the relative change in the outcome variable between the treated and control groups. The model is specified as follows:

$$Y_{it} = \beta D_{it} + \mathbf{X}_{it} \times \alpha + \mu_i + \theta_t + \varepsilon_{it}$$

where Y_{it} represents the outcome variable of interest in region (prefecture or county) i during period (day, week or month) t. D_{it} is a dummy variable indicating the treatment status in region i at time t, where it equals 1 if any community within this region is classified as a *Risk* area and 0 otherwise. Regions with *Risk* areas would be subject to the enforcement of zero-COVID policies. $\mathbf{X}_{it}$ are the control variables. μ_i represents prefecture (county) fixed effects, which control for time-invariant prefecture (county)-level factors, and θ_t represents

[19]See Menne et al. (2012)

time fixed effects, which control for shocks that are common to all regions during a given time period.

The underlying assumption for the DiD estimator is that the zero-COVID policy implementation is not driven by unobserved factors that could also systematically influence the differences in outcome variable between regions with *Risk* areas and regions without *Risk* areas. This assumption is unverifiable as it requires knowledge of the counterfactual scenario, but we can investigate whether the parallel trends assumption is satisfied before the date when any areas within these regions were classified as Risk areas. To do so, we performed an event study approach to estimate the dynamic effect of the treatment. Moreover, we can understand how long the treatment effect persists. Our model is as follows:

$$Y_{it} = \sum_{k \neq -1} \beta_k D_{it}^k + \mathbf{X}_{it} \times \alpha + \mu_i + \theta_t + \varepsilon_{it}$$

where D_{it}^k represents the indicator for i's treatment status at k periods relative to period t. It takes a value of 1 if region i has any areas classified as *Risk* was k periods relative to period t and 0 otherwise. We exclude $k = -1$ so that the dynamic effect is compared to the period immediately before initial treatment. The parameter of interest β_k estimates the effect of zero-COVID policy k periods after/before the implementation. We expect the pre-trends to be parallel, as β_k would not be significantly different from zero for $k \leq -2$. Intuitively, economic activities were restricted by the zero-COVID policy in the enforced regions and slowly recovered after the implementation was over, thus we expect β_k to be negative for $k \geq 0$ and converge to zero as k increases.

To investigate the heterogeneity of the effect of the Lockdown and zero-COVID policy over time, we perform separate DiD regressions and event studies for the years 2020, 2021, and 2022. As in some regions the zero-COVID policies were triggered multiple times across 2021 and 2022, we exclude the regions that have already been classified as *Risk* during 2021 from our subsample used in the analysis for year 2022.[20] As the risk level data is unavailable for 2020, we use the lockdown data from He et al. (2020) to generate the treatment status for year 2020. In the following sections, we present our empirical results for different outcome variables and provide more details on the regression specifications used for our analysis.

2.4 Results

2.4.1 COVID-19 Cases

Before we examine the economic consequences of zero-COVID policies, we apply an event study approach to examine the dynamic effects of the risk level on COVID-19 cases in China, with the goal of examining the trends in COVID-19 cases before and after the implementation of the zero-COVID policy and estimating the average time it took from the launch of zero-COVID policy to when the outbreak was under control. To achieve this,

[20] We did not exclude regions that have experienced lockdown in 2020 in any of these regressions, because, in fact, almost all prefectures in China implemented some level of restriction in mobility during the initial outbreak of the pandemic. On the other hand, the share of regions that were at *Risk* during 2021 is relatively small so the subsample after excluding these regions could still be representative.

we estimate the following model:

$$Case_{it} = \sum_{k=-30}^{-2} \beta_k D_{it}^k + \sum_{k=0}^{50} \beta_k D_{it}^k + \mu_i + \theta_t + \varepsilon_{it}$$

where $Case_{it}$ represents confirmed COVID-19 cases in prefecture i at date t. D_{it}^k represents the indicator for prefecture i's treatment status at k periods relative to date t. Given the potential reverse causality between COVID-19 cases and risk level status and potential anticipation[21], we are not estimating a causal impact, but examining the correlation. The coefficient of interest β_k estimates the correlation between the status of *Risk* or non-risk k periods after/before the risk level classification and the daily confirmed COVID-19 cases. The dynamic effect results are displayed in Figure 2.3.

We begin by presenting the dynamic effect of the 2020 lockdown implementation in Figure 2.3a. Prior to the lockdown, the dynamic effect is negative. Subsequently, the effect remains positive for approximately 50 days after the initial lockdown, and reaches its peak at 40 around 21 days later, before starting to decline towards 0. It is unsurprising to observe a surge in daily confirmed cases following a lockdown, as extensive COVID-19 testing is likely to start after the lockdown is imposed when the virus has already spread for some time. As a result, the daily confirmed cases during the weeks following the lockdown tend to be higher on average than before it. Additionally, the extensive variation in the estimated dynamic effect and the predominantly insignificant estimators suggest that some prefectures prefectures may have implemented precautionary policies before potential increases in cases.

In Figure 2.3b and 2.3c, we present the results of our event study analysis for the years 2021 and 2022, respectively. Our findings suggest that the dynamic effect of

[21] See Goodman-Bacon and Marcus (2020) for a review of challenges of causality identification in COVID-19 research.

zero-COVID policy on COVID-19 cases differs over the two years. Specifically, in 2021, the dynamic effect increases from day 0 to day 7 and then gradually declines, becoming negligible after day 21. In contrast, in 2022, the dynamic effect remains high for a more extended period, it takes around 25 days to control the size of the pandemic to about 5 cases, and around 50 days to decrease the magnitude close to 0. The peak of the curve is also much higher than in 2021, with an average of more than 10. Additionally, the variation of the dynamic effect in 2022 is much larger than in 2021. These findings suggest that while some prefectures were able to reduce COVID-19 cases quickly by implementing stringent measures immediately after they were classified as *Risk* regions, others found it more challenging to contain the spread of the virus effectively in 2022.

Overall, these findings suggest that the risk level policy in China has been effective in controlling the spread of COVID-19 in 2021, with the number of cases peaking shortly after the initial intervention and declining afterwards. However, in 2022, the emergence of new virus variants, such as the Omicron, poses challenges to the effectiveness of the policy, as it took much longer to control the pandemic in 2022 compared to 2021.

Additionally, these results highlight the considerable variation in the implementation of the zero-COVID policy across different regions in China, with some prefectures experiencing a rapid decline in cases immediately after being classified as *Risk* regions, while others had a slower decline or even an increase in cases before a decline.

2.4.2 Traffic Mobility

Next, we investigate the effect of the zero-COVID policy on mobility. Our models are as follow:

$$Mobility_{it} = \beta D_{it} + \mathbf{X}_{it} \times \alpha + \mu_i + \theta_t + \varepsilon_{it}$$

$$Mobility_{it} = \sum_{k=-30}^{-2} \beta_k D_{it}^k + \sum_{k=0}^{50} \beta_k D_{it}^k + \mathbf{X}_{it} \times \alpha + \mu_i + \theta_t + \varepsilon_{it}$$

where the dependent variable $Mobility_{it}$ has two measures: inflow and outflow traffic mobility index at prefecture i on date t, taking the natural log. For the sample period of 2020, D_{it} is an indicator variable for lockdown or not.[22] For the sample period of 2021 or 2022, D_{it} is a binary variable equal to 1 if any community within this prefecture i at date t is classified as a *Risk* area and 0 otherwise. We control prefecture fixed effects by μ_i and date fixed effects by θ_t. It should be noted that the timing of the risk level classification and the adoption of corresponding zero-COVID policies may be correlated with the severity of COVID-19. We therefore include daily confirmed COVID-19 cases in the matrix of prefecture-day level controls $\mathbf{X}_{it}$. We also include weather factors in $\mathbf{X}_{it}$. The standard errors are clustered at the prefecture level. We estimate the effect of the zero-COVID policy on mobility separately for year 2020, 2021, and 2022.

The DiD regression results in Table 2.2 show that the impacts of the zero-COVID policy on inflow and outflow mobility in 2021 and 2022 are significantly negative. However, the impact of lockdown on mobility in 2020 is negligible. In columns (3) and (4), the coefficients for both inflow and outflow traffic mobility during 2021 and 2022 are approximately -0.3, indicating a 30% decrease in traffic flow between a prefecture and other prefectures

[22] For the sample period of 2020, we use similar setting with He et al. (2020)

after it is listed as *Risk* region. This result is significant at the 1% level. In columns (1) and (2), the magnitude of the coefficient is only around -0.02, suggesting only a 2% change in traffic mobility, which is not significant. The R-squared for all regression specifications indicate that the models explain a considerable proportion of the variance, lending credibility to our estimation.

We present the dynamic effects of the lockdown and zero-COVID policy implementation on inflow and outflow traffic mobility in Figure 2.4 and 2.5, respectively. The patterns are similar for the two sets of figures within the same year. Figures 2.4a and 2.5a display the dynamic effect of lockdown on inflow and outflow mobility in 2020. There is no significant trend in the pre-treatment periods, indicating that the treatment does not affect mobility before the launch of the lockdown. Both mobility measures experienced a significantly negative effect immediately after the lockdown and stopped the decreasing trend within one week. There are sharp increases in mobility that happened during the third week after the lockdown, which may be due to the fact that the lockdown duration in 2020 was clustered around 20 days, and the mobility increase reflected the lifting of restrictions. This pattern help us to explain the insignificant lockdown effect in Table 2.2, On average, a significant positive rebound in traffic flow during the third week offsets the negative effects observed in the first two weeks.

In Figure 2.4b and 2.5b, we present the effect of zero-COVID on inflow and outflow mobility in 2021. The figures show a significantly negative effect that occurs immediately after the prefectures were classified as a *Risk* region, remains at a large effect size for around 15 days, and gradually returns to null around 30 days after the initial treatment. Regarding the impact of zero-COVID policy on mobility in 2022, as displayed in Figure 2.4c and 2.5c, we observe almost an identical pattern as in 2021, while the magnitude of the dynamic effects in 2022 was larger than in 2021 at its peak.

There are two possible reasons to explain this phenomenon. Firstly, it could be due to the more stringent implementation of the zero-COVID policy, which led to greater restrictions on mobility. Secondly, the release of the Travel Codes Tracker system could have also contributed to this effect by limiting travel and mobility across regions. In early 2020, despite the virus being more lethal, only individuals traveling from Wuhan were required to undergo quarantine [23]. However, in 2021 and 2022, anyone with a travel history to medium- or high-risk areas within 14 to 21 days were required to undergo mandatory quarantine at their own expense. Individuals would be tracked by the combination of Travel Code and the risk level system [24]. With the higher expected cost for traveling, it is reasonable to observe larger negative effect on the inter-prefecture traffic flow in 2021 and 2022, as compared to 2020.

In all event studies in 2021 and 2022, we observe that the pre-trend has a dip around 3 days before the enforcement of the zero-COVID policy. This suggests that people observed the COVID-19 cases and voluntarily avoided entering and leaving the region. Nevertheless, we believe that this will not harm the credibility of our DiD estimation as the scale of the pre-treatment change due to anticipation is relatively small compared to the post-treatment changes in inter-prefecture traffic mobility.

[23] See Prevention Guidance for Novel Coronavirus Pneumonia (version 4): http://www.nhc.gov.cn/xcs/zhengcwj/202002/573340613ab243b3a7f61df260551dd4/files/c791e5a7ea5149f680fdcb34dac0f54e.pdf

[24] See the reports on China's truck drivers stuck in the quarantine rules and QR trackers:https://www.reuters.com/world/china/china-truckers-use-fake-travel-records-clean-drivers-dodge-covid-rules-2022-03-30/

It is important to note that the impact of zero-COVID policy on traffic mobility may vary across regions, depending on the severity of the pandemic and the specific measures taken to restrict mobility. Nonetheless, our results suggest that the zero-COVID policy has been effective in restricting inter-prefecture mobility, which could contribute to controlling the spread of the virus, while also negatively impacting the transportation industry and other related sectors. It should be emphasized that the measured effect is a combination of the traffic restriction effect and the "voluntary" precaution effect of the Travel Code tracker system. Furthermore, since the outcome variables are inter-prefecture traffic flows, the effect could not be attributed to within-prefecture traffic.

2.4.3 Pollution

We proceed by examining the influence associated with the zero-COVID policy on PM2.5 concentration levels in China from 2020 to 2022. Specifically, we fitted the following equations:

$$Pollution_{it} = \beta D_{it} + \mathbf{X}_{it} \times \alpha + \mu_i + \theta_t + \pi_{it,jm} + \varepsilon_{it}$$

$$Pollution_{it} = \sum_{k=-5}^{-2} \beta_k D_{it}^k + \sum_{k=0}^{5} \beta_k D_{it}^k + \mathbf{X}_{it} \times \alpha + \mu_i + \theta_t + \pi_{it,jm} + \varepsilon_{it}$$

where $Pollution_{it}$ represents the average PM2.5 concentration level at county i during week t, taking natural log. Here, we aggregate the hourly PM2.5 data into week level to average out the high volatility of the daily air pollution, following He et al. (2020). For the sample period of 2020, D_{it} is a indicator for lockdown launched in county i during week t or not. For the sample period of 2021 or 2022, D_{it} is a binary variable equals 1 if any community within county i during week t is classified as a *Risk* area and 0 otherwise. We control county fixed effects μ_i and week fixed effects θ_t. $\mathbf{X}_{it}$ include daily confirmed COVID-19 cases and weather factors such as temperature and precipitation. $\pi_{it,jm}$ denotes prefecture by month fixed effects, taking value of 1 for any county i within prefecture j during month m including week t and 0 otherwise. We control prefecture by month fixed effects to account for time-variant regional conditions shared by counties within the same prefecture in a given month. The standard errors are clustered at the county level.

Table 2.3 reports our DiD regression results. In column (1), we replicate the estimations used in He et al. (2020) and estimate the impact of lockdown on PM2.5 pollution levels in 2020. Our result is similar to theirs. In columns (2) - (5), we estimate the influence of implementing the zero-COVID policy on PM2.5 pollution levels in 2021 and 2022 and our results show an ambiguous policy effect.

Different from the lockdown effects found in column (1) of Table 2.3 in 2020, our findings suggest that the zero-COVID policy may not significantly reduce pollution levels in 2021. Column (2) shows a significantly positive correlation between the implementation of zero-COVID policy and PM2.5 concentration in the baseline regression. We further control for prefecture by month fixed effects in column (3), and the coefficient remains significantly positive but with a smaller magnitude. This suggests that potential time-variant prefecture-level factors that are positively correlated with the risk level status may contribute to the positive change in PM2.5 pollution level. Moreover, some time varying county-level factors might be correlated with both the probability of being classified as *Risk* region and pollution concentrations. For example, Urban counties may have a higher chance of being classified as *Risk* regions and may also experience faster increases in PM2.5 pollution levels than their rural counterparts due to their larger number of manufacturers and motor vehicles that elevate PM2.5 pollution. Overall, in 2021, county-specific growth trend of pollution appears to outweigh the influence of the zero-COVID policy.

In columns (4) and (5), we find that the policy effects become significantly negative in 2022. The zero-COVID policy reduces the PM2.5 concentration by 1.2% to 3.5%. This is expected because the zero-COVID policy imposes more stringent restrictions on economic activities in 2022. As a result, similar to the scenario in 2020, counties with *Risk* areas experienced a significant reduction in PM2.5.

To further explore the influence of zero-COVID policy on pollution levels, we present our event study analysis in Figure 2.6. We first replicate the model of He et al. (2020) in Figure 2.6a for the dynamic effect of lockdown policy on pollution levels in 2020. Then we perform event studies for 2021 and 2022. Figure 2.6b illustrates the dynamic effect of zero-COVID on PM2.5 concentration in 2021, showing a slightly decreasing trend after the counties were classified as *Risk* areas, but with an increasing trend starting from week 3, and a positive and significant effect in weeks 4 and 5. In contrast, Figure 2.6c shows that in 2022, the treatment effects is negative in the first three weeks after the counties are categorized as *Risk* region. In both figures, the pre-trends are consistent with the parallel trends assumption as the coefficients prior to the treatment are all close to zero and statistically insignificant. Combining the results from our baseline DiD regression, we find that the zero-COVID policy in 2021, unlike the strict lockdown implementation in 2020, does not bring substantial improvement to air pollution levels as the restriction imposed by zero-COVID policy is limited within a county rather than the entire prefecture. However, the change in PM2.5 concentration becomes larger and more significant when counties are categorized as *Risk* regions with more stringent zero-COVID policy, as seen in 2022.

As discussed in Sun and Abraham (2021), the event study approach requires relatively strong assumptions on the homogeneity of treatment effect, especially over time and across individuals, to deliver consistent estimates. These assumptions are likely to be violated in our context of zero-COVID policy, as the treatments are implemented across multiple time periods and local government could endogenously choose the stringency of their policy implementation and result in heterogeneous treatment effects. In order to overcome this potential identification issue and allow for heterogeneity in treatment effects, we apply the method proposed by Sun and Abraham (2021) and present the robust estimators in our figures. In Figure 2.6, it can be observed that the robust estimators follow a similar pattern to the regular dynamic effect estimators and our results are robust to the potential heterogeneous treatment effects.

In conclusion, our findings reveal ambiguous effects of the zero-COVID policy on PM2.5 concentration level in 2021 and 2022. In 2021, when the zero-COVID policy was less stringent, the county-specific growth trend of pollution appears to outweigh the influence of the zero-COVID policy. In 2022, with the more stringent implementation of the zero-COVID policy, it took an average of three weeks for PM2.5 concentration to return to its original level. This suggests a corresponding three-week decrease in industrial production and traffic flow within the county. It is worth noting that during 2021 and 2022, COVID-19 containment was prioritized over environmental protection. As a result, when counties were categorized as *Risk* regions, local governments may have relaxed environmental restrictions, leading to increased pollution. This could potentially lead us to overestimate the change in pollution level associated with the implementation of the zero-COVID policy.

2.4.4 Night Lights

Finally, we present empirical evidences related to night lights (nighttime light). We use the following models:

$$NightLight_{it} = \beta D_{it} + \mathbf{X}_{it} \times \alpha + \mu_i + \theta_t + \pi_{it,j} + \varepsilon_{it}$$

$$NightLight_{it} = \sum_{k=-5}^{-2} \beta_k D_{it}^k + \sum_{k=0}^{5} \beta_k D_{it}^k + \mathbf{X}_{it} \times \alpha + \mu_i + \theta_t + \pi_{it,j} + \varepsilon_{it}$$

where $NightLight_{it}$ represents the night lights level at county i during month t, taking natural log. For the sample period of 2020, D_{it} is an indicator for lockdown launched in county i during month t or not. For the sample period of 2021 or 2022, D_{it} is a binary variable equal to 1 if any community within county i during month t is classified as a *Risk* area and 0 otherwise. We control county fixed effects μ_i and month fixed effects θ_t. $\mathbf{X}_{it}$ include daily confirmed COVID-19 cases and weather factors. We also include prefecture by month fixed effects for robustness, where $\pi_{it,j}$ denotes prefecture by month fixed effects, taking a value of 1 for any county i within prefecture j during month t and 0 otherwise.

Similar to the effects on PM2.5, we found divergent effects of the zero-COVID policy on night lights over the sample periods, which are presented in Table 2.4. In column (1), we find the lockdown implementation has a significantly negative coefficient at -0.0391, which implies counties that underwent lockdowns in 2020 had a 4% decrease in night lights compared to counties that did not implement lockdowns. In columns (2) and (4), we report the estimations for the zero-COVID policy in 2021 and 2022. In 2021, we observed a positive correlation between the implementation of the zero-COVID policy and the changes in night lights, while in 2022, the zero-COVID policy reduced 14% economic activities proxied by night lights. In 2021, compared to the county-specific economic growth trend, the change in night lights associated with the less stringent zero-COVID policy in 2021 was negligible, as it only imposed restrictions in a limited number of communities within the county. However, in 2022, with the emergence of the highly contagious Omicron variant, the zero-COVID policy became stricter and seriously disrupted economic activities. We show the robustness of our results in columns (3) and (5) by controlling for prefecture by month fixed effects, and find that the coefficients remain statistically significant at the 1% level.

The dynamic effect results in Figure 2.7 provide further support for our argument. To allow for heterogeneity in the treatment effect over time and across treated units, we include the robust estimators of Sun and Abraham (2021) in the figures. In 2020, lockdowns occurred mostly during the early phase of the pandemic, severely affecting the economic environment and consumer confidence. As shown in Figure 2.7a, the negative impact of lockdown on the night lights was significant and persistent, lasting more than five months after the event date, with no signs of recovery. In 2021, shown in Figure 2.7b, a slight increasing trend of night lights is associated with the probability that a county is categorized as a *Risk* region. A possible explanation is that counties that had more active economic performance were more likely to be classified as *Risk* regions while also experiencing faster economic recovery. However, in 2022, as shown in Figure 2.7c, the decreasing trend was evident, with all treatment effects negative and significant after the implementation of the zero-COVID policy. The magnitude of the negative impact continued to expand until four months after the county was categorized as a *Risk* region, with no complete recovery observed. This implies that the zero-COVID policy in 2022 brought persistent negative impacts to local economies, which may have contributed to the end of the era of zero-COVID policy and the reopening in December 2022.

It should be noted that, in Figure 2.7b, we observe positive pre-trend and post-trend that are significantly away from zero. This indicates that, compared to the difference in night lights between the treated and control groups in the baseline period $t = -1$, these differences in night lights are larger between two groups in periods at least two months before or after the implementation of the zero-COVID policy. As the zero-COVID policy is unlikely to bring more economic opportunities to the region due to its nature of suppressing human activities, this result could be explained by the positive correlation between the likelihood that a county will be classified as a *Risk* region and its county-specific economic growth trend in 2021. As shown in Figure A.2a, only a small proportion of regions in China experienced the zero-COVID policy in 2021. It is plausible that a county in a more economically developed prefecture could enjoy a stronger recovery from the pandemic shock in 2020 and display a higher economic growth rate in 2021. Meanwhile, such prefectures were more likely to experience a pandemic outbreak in 2021. As shown in previous Section 4.1 as well as in Figure 2.2, the COVID-19 outbreaks in 2021 were usually on small scales and the zero-COVID policy in 2021 lasted for relatively short periods. Therefore, the persistent impact of the zero-COVID policy could be very limited in 2021. As a result, these counties could pick up the economic growth trend from the disruption of the zero-COVID policy quickly and continue with strong economic performance even after the zero-COVID policy.

This potentially explains the positive estimated influence of the zero-COVID policy on night lights in 2021, as shown in columns (4)-(5) of Table 2.4, as well as the upward trend of dynamic effects after the treatment of zero-COVID in Figure 2.7b.

We provide back-of-the-envelope calculations of the GDP loss caused by the zero-COVID policy in 2022. We replicate the original dataset used in Martinez (2022) and calculate the elasticity between GDP and night light. The calculation shows that a 1% change in night lights corresponds to a 0.858% change in China's GDP. Then, as shown in Panel B of Table 2.1, by the end of December 2022, zero-COVID policies had been implemented in 1700 out of 2853 counties in China. Finally, based on our calculation, the economic loss can be estimated as $0.077 * 0.858 * 1700/2853 = 0.039$,[25] suggesting that the zero-COVID policy resulted in a reduction in China's GDP of approximately 3.9%. Interpreting the results from this back-of-the-envelope calculation should be approached with caution due to two major limitations: (1) the estimated policy effects derived from the DiD setting may be subject to bias due to spillover effects; (2) the elasticity estimated from the data of Martinez (2022) does not consider the regional heterogeneity within China. In the presence of heterogeneity between counties affected by the zero-COVID policy and counties not affected, this calculation could be inaccurate.

2.4.5 Spillover Effect Results

When two adjacent regions exhibit a close economic linkage, the implementation of human activity restrictions, such as a zero-COVID policy, in one region could exert an impact on activities in the other. This spatial correlation poses a potential bias in our

[25] We choose policy effect as .077, from Column 5 of Table 2.4

difference-in-difference estimation. To isolate the spillover effects of zero-COVID policy in neighboring regions, we introduce a control variable for adjacent treated areas, following the spirit of literature on spillover effects in difference-in-difference settings (Clarke, 2017), as well as on peer effects (Goldsmith-Pinkham and Imbens, 2013). Specifically, we define a control variable termed "Neighbors Risk" as follows:

$$Neighbors\ Risk_{it} = \frac{\sum_{j \in I \setminus i} D_{jt} R_{ij}}{\sum_{j \in I \setminus i} R_{ij}}$$

where for any two regions $i, j \in I$ at any period t, D_{jt} is a dummy variable for whether j is under the zero-COVID policy at t, R_{ij} is a dummy variable for whether a pair of prefectures i, j is neighboring. Consequently, $Neighbors\ Risk_{it}$ calculates the proportion of neighboring regions implementing a zero-COVID policy relative to all adjacent regions for a given region i at time t.

We incorporate this constructed "Neighbors Risk" variable, along with its lagged terms, into the primary regression models presented in prior sections. Note that to fully capture the potential spatial correlation, a spatial econometric model, such as Spatial Durbin model (SDM) is desired. Our approach only accounts for the proximate (lagged) spillover effects from zero-COVID policies in neighboring regions during 2021 and 2022. The resulting regression results for mobility, pollution, and night lights are presented in Table 2.5, Table 2.6, and Table 2.7, respectively.

In columns (1), (3), (5), and (7) of Table 2.5, we present the baseline estimates initially showcased in Table 2.2. Correspondingly, columns (2), (4), (6), and (8) offer estimates of local policy effects on traffic mobility that are robust to spillover influences. Across all these specifications, the local estimates exhibit only negligible variations when compared to the original findings. There is no statistically significant negative spillover effect from adjacent zero-COVID policies in 2021. However, a notable negative impact emerges in 2022, consistent with our earlier results that the stringent zero-COVID measures in 2022 exerted a more pronounced adverse effect on economic activities than those in 2021.

In columns (1) and (3) of Table 2.6, we offer the baseline estimates for pollution outcomes from Table 2.3, while columns (2) and (4) include regression results accounting for spillover effects. No substantive changes are observed compared to the original estimations, and negative, statistically significant spillover effects are found for both 2021 and 2022. Given that PM2.5 concentration data are extracted from satellite image and aggregated at the county level, it is plausible that the implementation of a zero-COVID policy in a neighboring county could reduce pollution levels in adjacent areas due to restricted traffic mobility and manufacturing.

In columns (1) and (3) of Table 2.7, we present the baseline estimates for night light data from Table 2.4 and include spillover-adjusted regression results in columns (2) and (4). Again, the estimates remain substantively unchanged compared to the original findings, and no statistically significant spillover effects on night lights are observed for either 2021 or 2022. This suggests that long-term economic activities, as reflected by night light data, are unlikely to be influenced by zero-COVID policies in nearby regions.

2.4.6 Synthetic Diff-in-Diff Results

As mentioned in Section 4.4, potential selection bias may exist within the treated sample. Specifically, regions with greater economic development could be more susceptible to experiencing COVID-19 outbreaks, thereby making it more likely for them to implement the zero-COVID policy and consequently be included in the treatment group. In estimating the impact of zero-COVID policy implementation on economic outcomes like pollution and night lights, uncontrolled county-level time-varying trends could raise concerns regarding the validity of our estimated results.

To enhance the comparability between the treatment and control groups in our empirical examination of pollution and night lights, we conduct several auxiliary regressions employing the Synthetic Difference-in-Differences (SDID) method, a fusion of the Difference-in-Differences and Synthetic Control methodologies (Arkhangelsky et al., 2021). The SDID approach assigns weights to individual fixed effects and time fixed effects to approximate the pretrends between the treatment and control groups, thereby mitigating the reliance on the parallel trends assumption and generating more stable and robust estimates. It is noteworthy that, to comply with the SDID framework, we keep a balanced sample, resulting in a reduced sample size. We also disclose the outcomes of our primary regressions utilizing the balanced sample in Table A.1 and Table A.2 for reference.

We present our SDID estimations for pollution and night lights in Table 2.8 and Table 2.9, respectively. In column (1) of Table 2.8, the impact of lockdowns on PM2.5 concentration remains significantly negative. In column (2) of Table 2.8, the estimated changes in PM2.5 following the initiation of zero-COVID policy retain the same sign as the original estimate. In column (3), the estimated coefficient shifts from negative to positive, though with a relatively small magnitude. These outcomes align with our prior findings presented in the dynamic effect results of Figure 2.6. Specifically, local pollution showed a marked decline post-lockdown in 2020; its trend began to rise a few weeks following the implementation of the zero-COVID policy in 2021; and in 2022, the pollution exhibited a short-lived dip but did not sustain it.

In Table 2.9, we observe similar results for changes in night lights correlated with zero-COVID policy, compared with the original estimates in Table 2.4. The estimated coefficient for 2020 remains negative, though its statistical significance diminishes, while the results for 2021 and 2022 retain their original signs and are statistically significant. In summary, despite potential confounding factors involving the relationship between the implementation of zero-COVID policy and pre-treatment economic trends, our SDID estimations reaffirm the robustness of our primary regression outcomes and are consistent with our other findings.

2.5 Conclusions

In this paper, we provide evidence on the economic impacts of the zero-COVID policy implemented by the Chinese government as a pilot experiment in using big data for country management from 2020 to 2022. We employ a difference-in-differences specification with a novel dataset of China's COVID-19 risk level system. First, we find that the zero-COVID policy in China effectively contained COVID-19 transmission within a 21-day window in 2021. However, controlling virus transmission took twice as long with the emergence of the Omicron variant in 2022. Second, the zero-COVID policy led to a 30% reduction in inflow and outflow mobility, indicating a potential negative impact on the transportation industry and related sectors. Third, our study indicates that the zero-COVID policy had a negligible effect on pollution levels in 2021. Nevertheless, it led to a decrease in PM2.5

concentration in the estimated range of 1.17% to 3.47% in 2022. Lastly, the evidence reveals that the zero-COVID policy had trivial impacts on night lights in 2021, which was overshadowed by the strong economic performance due to the recovery effect. However, we discover a significant reduction in economic activities proxied by night lights, ranging from 7.7% to 14%, as a result of the implementation of the zero-COVID policy in 2022. We calculate that the zero-COVID policy resulted in a reduction of approximately 3.9% in GDP.

Several other countries pursued an elimination strategy like China, with strict border controls and lockdowns to keep the virus at bay, for example, New Zealand, Australia, Singapore, Vietnam, and Thailand. Studies generally show that COVID-19 has had a negative impact on these economies, especially for the countries that rely heavily on tourism and international trade (Dang et al., 2023; Bui et al., 2022; Fouda et al., 2020; O'Sullivan et al., 2020). Most countries experienced an economic contraction during the initial stage of the pandemic, but were able to have a quick rebound since their proactive response to the pandemic had effectively minimized cases infected. An exact comparison of economic impacts between China and these countries, however, is not feasible because the strictness of containment policies enforced by different countries varies, and some countries shift their strategies in response to changing circumstances at different times.

Overall, our findings offer important insights into the effectiveness and limitations of the zero-COVID policy in controlling the spread of COVID-19, as well as its impact on various aspects of the economy and society. These insights can inform the design and implementation of big data-driven public health policies that aim to reduce the impact of public health crises and minimize economic costs in China.

2.6 Figures and Tables

2.6.1 Figures

Figure 2.1: Daily Confirmed Cases v.s. Number of Counties with Risk (excluding Shanghai)

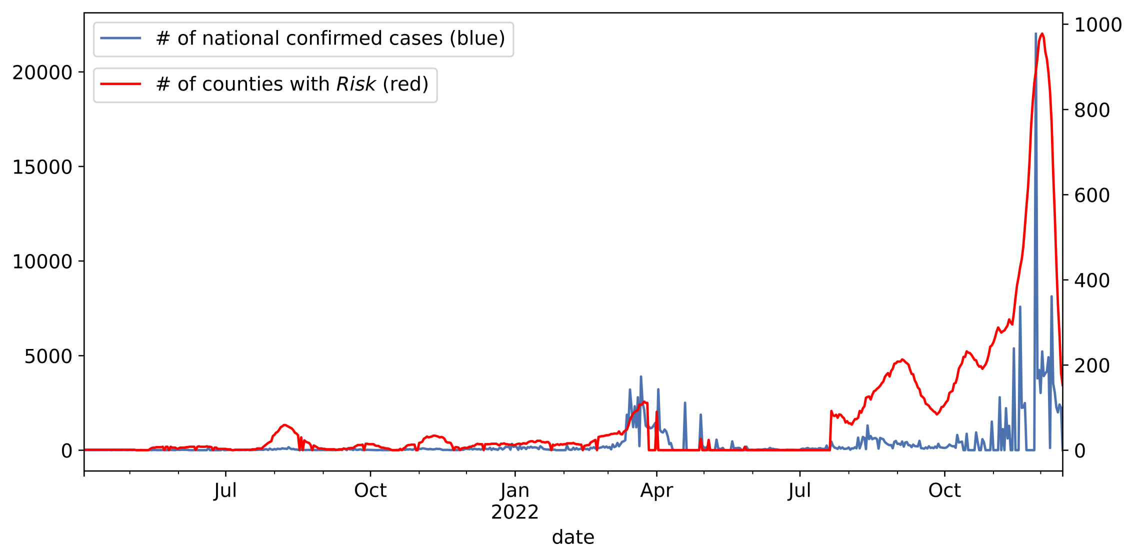


Figure 2.2: Distribution of *Risk* Duration per County

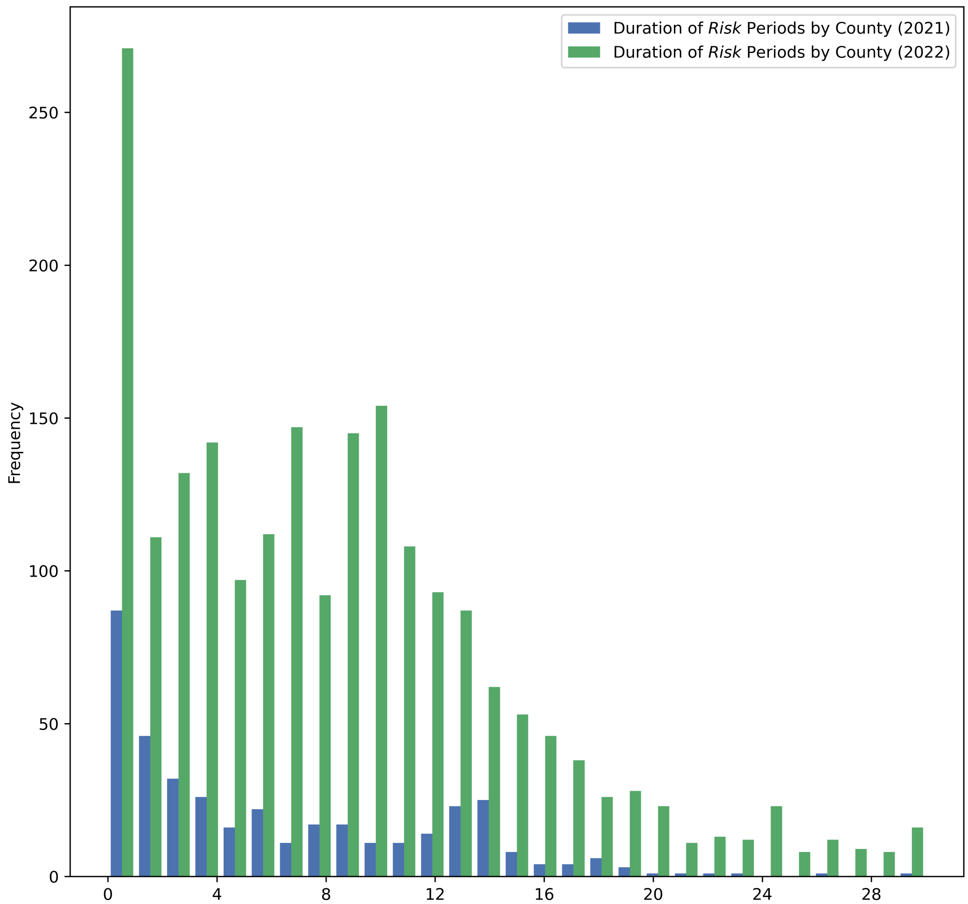
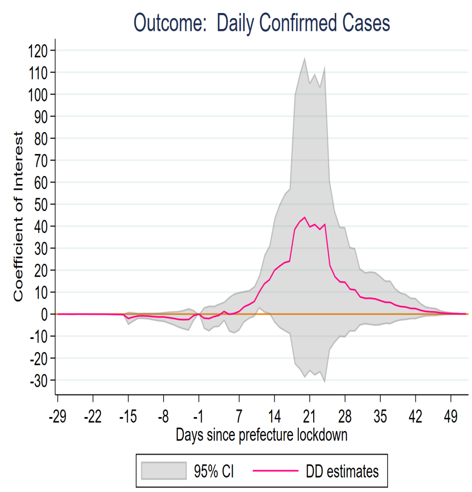
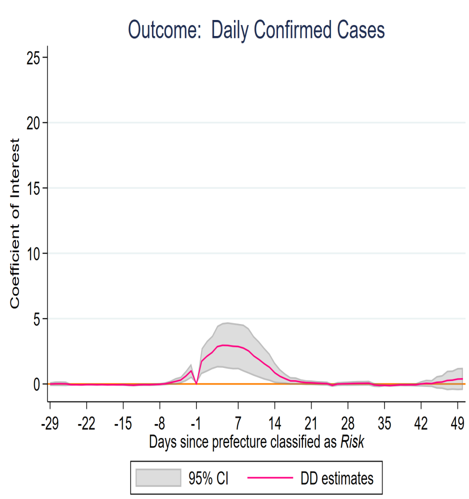


Figure 2.3: Event Study: Daily Confirmed Cases

(a) Lockdown 2020



(b) *Risk* 2021



(c) *Risk* 2022

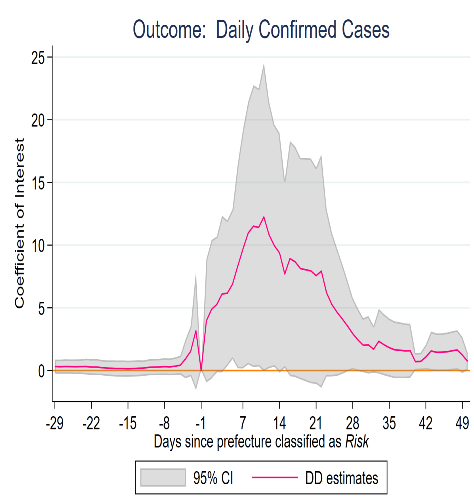
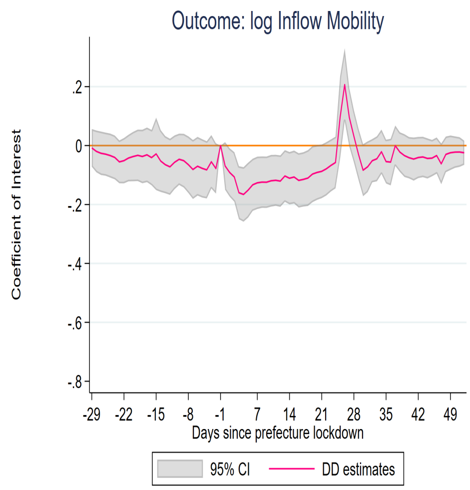
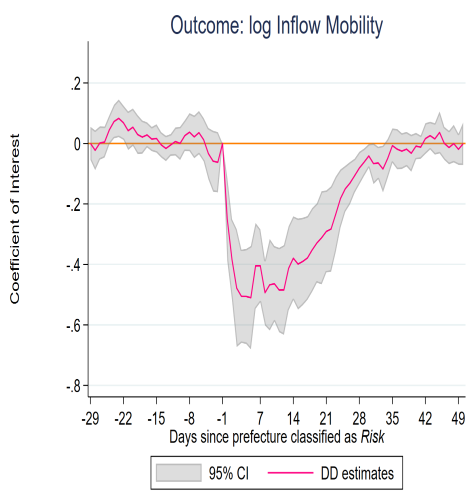


Figure 2.4: Event Study: Inflow Mobility

(a) Lockdown 2020



(b) *Risk* 2021



(c) *Risk* 2022

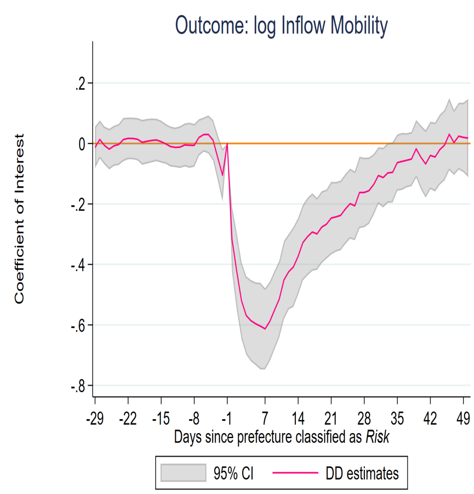
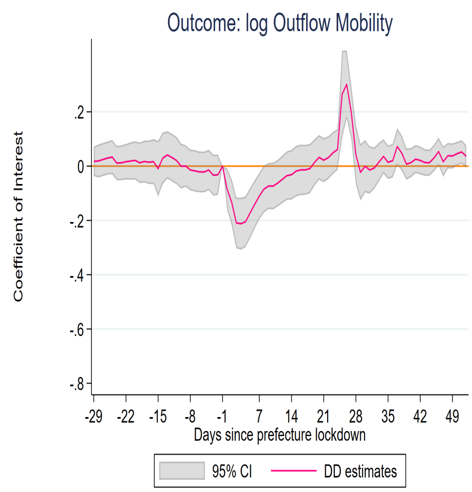
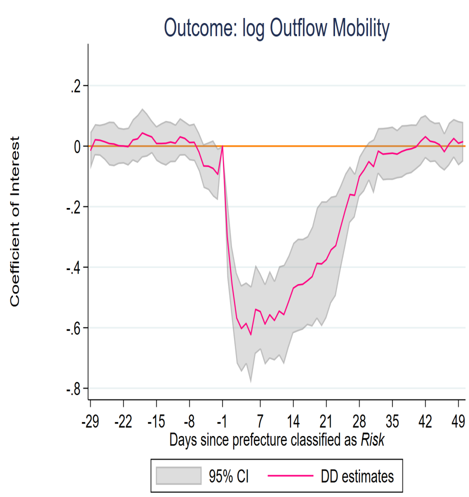


Figure 2.5: Event Study: Outflow Mobility

(a) Lockdown 2020



(b) *Risk* 2021



(c) *Risk* 2022

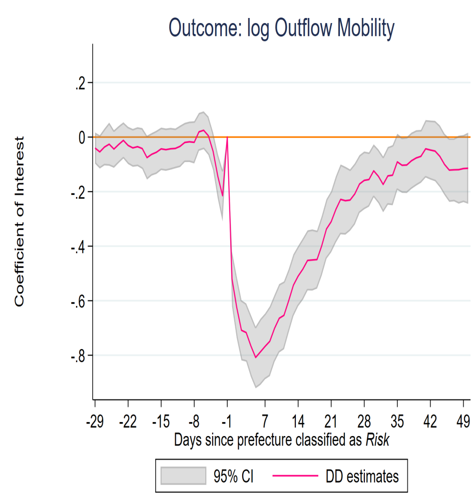
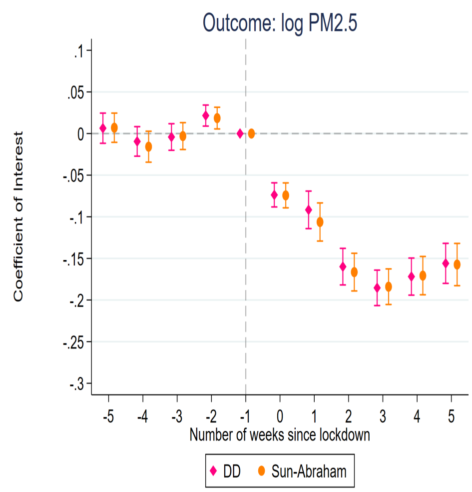
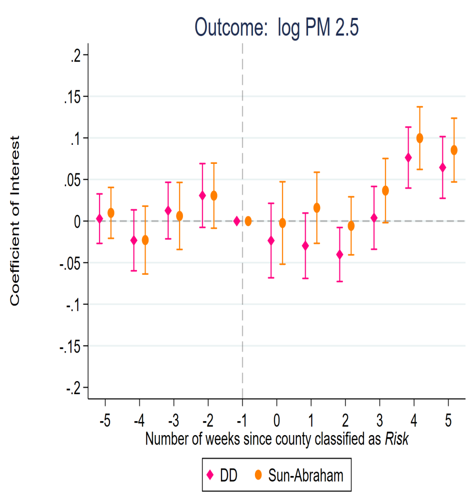


Figure 2.6: Event Study: PM2.5

(a) Lockdown 2020



(b) *Risk* 2021



(c) *Risk* 2022

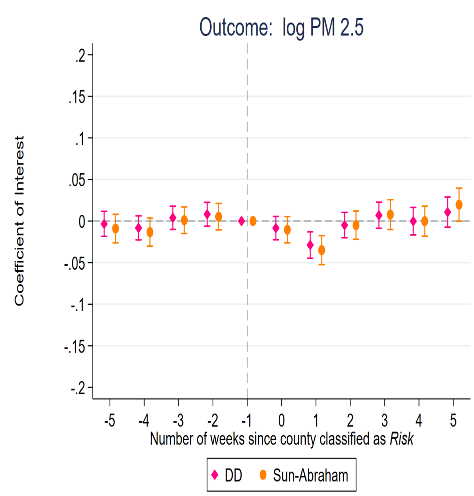
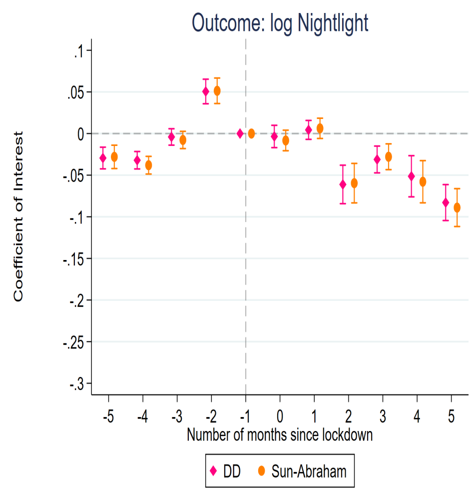
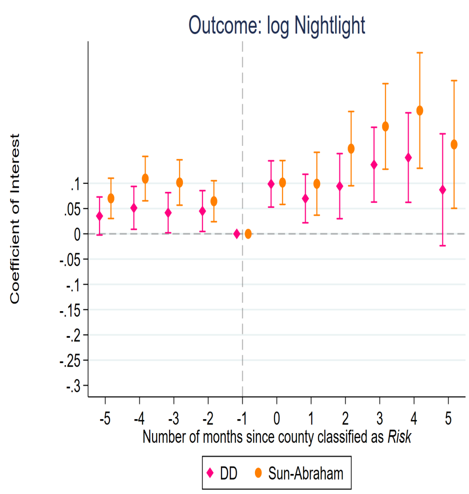


Figure 2.7: Event Study: Night Light

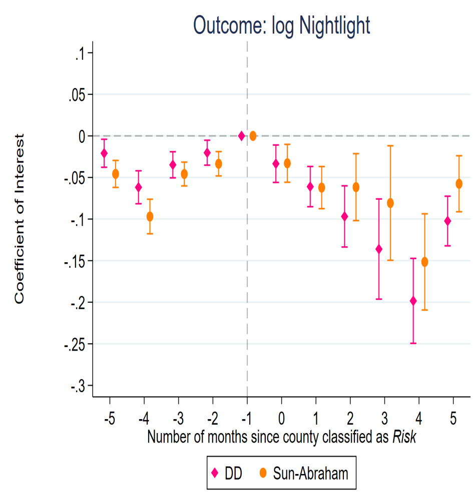
(a) Lockdown 2020



(b) *Risk* 2021



(c) *Risk* 2022



2.6.2 Tables

Table 2.1: Statistical Summary

	Obs	Mean	Std.Dev	Min	Max
Panel A: County Panel	ref.				
Classified as *Risk* (County)	1777419	0.026	0.159	0.0	1
Night Lights (monthly) ($Watts/cm^2/sr$)	45350	2.420	4.457	0.1	53
PM2.5 (weekly) (μ/m^3)	253352	26.665	15.070	0.4	394
Panel B: County by Dec15,2022	ref.				
Cumulative Days Classified as *Risk* (County)	2853	16.095	23.231	0.0	243
Cumulative Days Classified as *Risk* (Exclude Never Treated)	1700	27.011	24.716	1.0	243
Panel C: Prefecture Panel	ref.				
Classified as *Risk* (Pref)	229264	0.074	0.262	0.0	1
Share of counties Classified as risk (Pref)	229264	0.025	0.117	0.0	1
Num of Counties Classified as *Risk* (Pref)	229264	0.200	1.025	0.0	35
Daily Confirmed COVID Cases	657218	0.545	47.862	0.0	23718
Inflow Mobility	179041	0.281	0.313	0.0	4.039
Outflow Mobility	175695	0.281	0.321	0.0	4.671
Panel D: Prefecture by Dec15,2022	ref.				
Cumulative Days Classified as *Risk* (pref)	368	46.220	41.815	0.0	250
Cumulative Confirmed COVID Cases	356	1001.298	5239.218	1.0	64978

Table 2.2: Mobility Regression Results

	2020		2021		2022	
	(1)	(2)	(3)	(4)	(5)	(6)
	log Mobility Inflow	log Mobility Outflow	log Mobility Inflow	log Mobility Outflow	log Mobility Inflow	log Mobility Outflow
Lockdown	-0.0137	-0.0283				
	(0.0442)	(0.0366)				
Risk			-0.287***	-0.350***	-0.292***	-0.323***
			(0.0344)	(0.0431)	(0.0385)	(0.0383)
R-squared	0.835	0.849	0.968	0.963	0.888	0.883
Observations	23231	23234	30060	30060	38352	38352
Mean of Mobility	0.277	0.277	0.284	0.284	0.234	0.235
Controls	✓	✓	✓	✓	✓	✓
Prefecture FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.3: Pollution Regression Results

	(1) log PM2.5 2020	(2) log PM2.5 2021	(3) log PM2.5 2021	(4) log PM2.5 2022	(5) log PM2.5 2022
Lockdown	-0.162***				
	(0.00956)				
Risk		0.0842***	0.0159***	-0.0347***	-0.0117***
		(0.0153)	(0.00576)	(0.0100)	(0.00320)
R-squared	0.870	0.773	0.873	0.749	0.882
Observations	42750	99750	99050	145944	144864
Mean of PM2.5 (Weekly Average)	31.52	25.83	25.83	26.94	26.94
Controls	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓
Week FE	✓	✓	✓	✓	✓
Prefecture × Month FE			✓		✓

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.4: Night Lights Regression Results

	(1) log Night Lights 2020	(2) log Night Lights 2021	(3) log Night Lights 2021	(4) log Night Lights 2022	(5) log Night Lights 2022
Lockdown	-0.0391***				
	(0.00643)				
Risk		0.0812***	0.219***	-0.139***	-0.0771***
		(0.0229)	(0.0241)	(0.0135)	(0.0129)
R-squared	0.969	0.980	0.965	0.962	0.980
Observations	40103	19598	19991	23838	23341
Mean of Nightlight (Monthly)	2.614	2.268	2.268	2.354	2.354
Controls	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓
Month FE	✓	✓		✓	
Prefecture × Month FE			✓		✓

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.5: Mobility Spillover Results

	2021				2022			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	log Mobility Inflow	log Mobility Inflow	log Mobility Outflow	log Mobility Outflow	log Mobility Inflow	log Mobility Inflow	log Mobility Outflow	log Mobility Outflow
Risk	-0.287***	-0.286***	-0.350***	-0.349***	-0.292***	-0.295***	-0.322***	-0.321***
	(0.0344)	(0.0354)	(0.0431)	(0.0446)	(0.0385)	(0.0394)	(0.0384)	(0.0398)
Neighbors Risk		-0.0309		-0.0620		-0.0849*		-0.195***
		(0.0345)		(0.0500)		(0.0495)		(0.0429)
R-squared	0.968	0.969	0.963	0.963	0.888	0.885	0.883	0.880
Observations	30060	29970	30060	29970	38352	36585	38352	36585
Mean of Mobility	0.284	0.284	0.284	0.284	0.234	0.234	0.235	0.235
Neighbors Risk Lag 7 days		✓		✓		✓		✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Prefecture FE	✓	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓	✓

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.6: Pollution Spillover Results

	(1)	(2)	(3)	(4)
	log PM2.5 2021	log PM2.5 2021	log PM2.5 2022	log PM2.5 2022
Risk	0.0159***	0.0161***	-0.0117***	-0.00917***
	(0.00576)	(0.00624)	(0.00320)	(0.00326)
Neighbors Risk		-0.0982***		-0.0464***
		(0.0306)		(0.00868)
R-squared	0.873	0.878	0.882	0.880
Observations	99050	93060	144864	139072
Mean of PM2.5 (Weekly Average)	25.83	25.59	26.94	26.72
Neighbors Risk Lag 2 weeks		✓		✓
Controls	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Week FE	✓	✓	✓	✓
Prefecture × Month FE	✓	✓	✓	✓

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.7: Night Lights Spillover Results

	(1) log NightLight 2021	(2) log NightLight 2021	(3) log NightLight 2022	(4) log NightLight 2022
Risk	0.0812***	0.0997***	-0.0771***	-0.0703***
	(0.0229)	(0.0247)	(0.0129)	(0.0129)
Neighbors Risk		-0.0386		0.00277
		(0.0393)		(0.0178)
R-squared	0.980	0.979	0.980	0.979
Observations	19598	17196	23341	20626
Mean of Nightlight (Monthly)	2.268	2.317	2.354	2.254
Neighbors Risk Lag 1 month		✓		✓
Controls	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Prefecture × Month FE	✓	✓	✓	✓

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.8: Pollution SDID Results

	(1) log PM2.5 2020	(2) log PM2.5 2021	(3) log PM2.5 2022
Lockdown	-0.115***		
	(0.00839)		
Risk		0.0314	0.00537
		(0.0312)	(0.00541)
Observations	42750	99750	145584
Mean of PM2.5 (Weekly Average)	31.52	25.83	26.93
Controls	✓	✓	✓
County FE	✓	✓	✓
Week FE	✓	✓	✓

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.9: Night Lights SDID Results

	(1) log NightLight 2020	(2) log NightLight 2021	(3) log NightLight 2022
Lockdown	-0.00401		
	(0.00465)		
Risk		0.108***	-0.124***
		(0.0226)	(0.0245)
Observations	23790	12976	11000
Mean of Nightlight (Monthly)	2.385	2.083	1.844
Controls	✓	✓	✓
County FE	✓	✓	✓
Month FE	✓	✓	✓

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Chapter 3

Crisis Control in Top-down Bureaucracy: Evidence from China's Zero-Covid Policy

3.1 Introduction

The COVID-19 pandemic has had a profound impact on general economic activity, with restrictions on human mobility, ban on social gatherings, and closing of businesses. Recent literature has explored the economic consequences of the pandemic, such as unemployment, consumer spending, labor demand, and pollution. However, our understanding of how governments determine policies to combat the pandemic remains limited. This question is particularly complex in the context of China, which has a centralized, top-down hierarchical government structure. Although the central government prioritizes economic growth and evaluates the performance of local governments based on their GDP growth, the COVID-19 pandemic requires a slowing of economic development to curb the spread of the virus. This situation creates a tension between routine tasks and crisis control within the bureaucratic system. Our aim is to address this gap in the literature.

This paper examines the compliance of local Chinese officials with the zero-Covid policy during the COVID-19 pandemic. In China's political system, a conventional rule stipulates that governors at the prefecture level who are 58 years old or older become ineligible for further promotion. Consequently, officials whose ages are approaching this promotion eligibility threshold exhibit particularly strong incentives for advancement. Through an analysis of biographical data from prefecture party secretaries and a database at the prefecture-day level detailing the implementation of the zero-Covid policy, we find a significant influence of promotion incentives on the response of these leaders to COVID-19 outbreaks. Our empirical findings indicate that leaders with higher promotion prospects tend to exhibit an exaggerated response to emerging cases and maintain zero-Covid measures for extended periods. Compared to prefectures governed by leaders with fewer promotion incentives, those led by individuals with strong promotion incentives had a 0.727% higher chance of implementing the zero-Covid policy for every 7-day average daily case.

Interestingly, we observe a diminishing of these promotion incentives in regions that are more economically developed, signifying that these prefecture leaders have to pick a balance point between the mandate of pandemic control and the potential hazard to economic prosperity. Further analysis unveils that following the initiation of the zero-Covid policy, party secretaries with strong promotion incentives tend to enforce even stricter restrictions on traffic mobility when their jurisdictions are more economically developed, underscoring their desire to expedite pandemic containment to minimize its impact on the economy.

Our research contributes to three strands of literature. First, our research speaks to the studies on governance within a top-down bureaucracy, particularly when the system involves multitasking agency problems. Theoretically, within the multitasking framework, an agent will prioritize tasks that are strongly incentivized by clearly observed outcomes over poorly measurable, weakly incentivized ones (Holmstrom and Milgrom, 1991; Baker, 1992; Hart et al., 1997; Dewatripont et al., 1999). Whether governments could monitor their officials' multitasking efforts could significantly alter the execution of public policies and, subsequently, the overall social welfare (Dixit et al., 1997; Dixit, 2002).

However, for local officials facing multitasking in a pandemic scenario, the prediction is ambiguous, especially when efforts to contain the virus's spread could profoundly hurt economic development. Although rigorous anti-contagion measures may curb the pandemic's spread, the economic slowdown is evident, contrasting with the more intangible and less measurable efforts devoted to pandemic containment. Nevertheless, without prompt and effective interventions, the exponentially growing cases become another unwelcome outcome that local leaders aim to avoid. Our research sheds light on this complex multitasking circumstance, highlighting the deliberate balance that local officials strive to achieve between these competing priorities.

Second, our study contributes to the extensive body of literature examining the incentive role of personnel control in China's governance. Past studies indicate that local officials' drive for promotion in China has enhanced economic administration efficiency (Maskin et al., 2000; Blanchard and Shleifer, 2001; Li and Zhou, 2005). Empirical evidence also supports that local GDP performance stands as a pivotal benchmark for officials with marked promotion aspirations (Li and Zhou, 2005; Chen et al., 2005; Yao and Zhang, 2015). Given the outbreak of an unparalleled pandemic, one might wonder the efficacy of China's personnel-driven political system in crisis mitigation. Our study provides an affirmative answer to this question.

Moreover, zero-Covid policy is economically costly (Chen et al., 2022a; Ke and Hsiao, 2022; Gong et al., 2022b, 2023). This raises concerns about whether personal political incentives might spur local officials into outrageous actions, risking potential backfires. Existing literature indicates that political incentives can sometimes transform into policy radicalism detrimental to society at large (Kung and Chen, 2011) or lead officials to manipulate data, provide biased information, and distort official statistics for promotional gains (Zhou and Zeng, 2018; Suárez Serrato et al., 2019). However, our findings do not support the notion that promotion incentives encourage reckless actions among local officials concerning pandemic policy.

Third, our research contributes to the literature that investigates the role of age restriction and tenure in political system. Countries like China (Xi et al., 2018; Zhou and Zeng, 2018; Shi and Xi, 2018; Huang et al., 2020; Shi et al., 2021), India (Bertrand et al., 2020) and etc. all have similar age restriction upon governor's age when deciding their career path.

Lastly, our research delves into the political and economic determinants behind COVID-19 policy decisions, a field with limited literature to date. Since the onset of the pandemic, global policymakers have taken varying approaches to strike a balance between health concerns and economic implications. The interplay of political pressures, interest group dynamics, and population needs largely shapes these policy decisions. McCann and Wood (2022) underscore how the political-economic environment of states in the U.S. influences their COVID-19 policy choices. Grossman et al. (2020) examine the extent to which partisanship affects adherence to physical distancing in the U.S., while Bosancianu et al. (2020) offer insights into the political and social determinants that may account for variations in COVID-19 mortality rates.

Feng et al. (2023) is most related to our work. They analyze the role of local governors' patronage connection during China's nationwide stringent anti-contagion measures in the early stage of the pandemic. Their findings suggest that when a prefecture-level city leader maintained personal ties with the provincial supervisor, there was an increase in the stringency of the measures implemented. While their analysis focuses on the early 2020 phase of the pandemic in China, our study shifts the lens to the 2021-2022 period. During this time, local governors possessed greater flexibility over zero-Covid policy decisions. Our exploration sheds light on the nuance within China's COVID-19 policies, which were delegated to local officials who burdened the dual objectives of pandemic containment and economic vitality.

The remainder of the paper is structured as follows. Section 2 details the policy and institutional background. Section 3 describes the data. Section 4 outlines the empirical strategy. Section 5 presents the empirical results and robustness checks. Section 6 concludes the paper.

3.2 Policy and Institutional Background

3.2.1 China's zero-Covid Policy

In this section, we briefly introduce the background of China's zero-Covid policy.[1] In response to the initial COVID-19 outbreak in Wuhan, in early 2020, the Chinese government implemented unprecedented prefecture lockdowns to shut down the spread of the virus. Stringent measures were implemented in 58 out of 337 prefectures, including restrictions on outbound traffic, the imposition of stay-at-home orders, and the enforcement of quarantine measures (Fang et al., 2020b; Qiu et al., 2020). Additionally, other anti-contagion policies, known as Community Stringent Measures (CSMs), were enforced in most prefectures nationwide. Unlike lockdowns, CSMs are less stringent measures that involve restrictions on human mobility, the establishment of body temperature checkpoints, neighborhood sanitization, monitoring of suspected COVID-19 cases, and other anti-contagious measures at the local community level. By February 20, 2020, 303 prefecture-level cities in China had implemented CSMs, covering 89.9% of all such cities (Qiu et al., 2020; Feng et al., 2023). The Chinese government introduced a policy package on February 18, 2020, aimed at precise containment of COVID-19 transmission at the community level.[2] As a result, the central government ceased to recommend prefecture-level lockdowns due to their harm-

[1] Gong et al. (2023) provides a detailed documentation about the background and the description of the zero-Covid policy.

[2] *Guidelines on Scientific Prevention and Control, Precise Measures, Zone-Based and Tiered Approach for the COVID-19 Epidemic Prevention and Control*: `https://www.gov.cn/xinwen/2020-02/18/content_5480514.htm`

ful impacts on the economy. This research scope excludes the consideration of lockdown or CSM decisions made by local governments during this period since these were national policies directly announced and enforced by the central government, and not endogenous decisions made by local prefecture leaders.

After a one-month period of strict lockdowns and nationwide public health interventions, the central government sought to stimulate economic recovery and ease lockdown measures. The State Council and National Health Commission of China issued *Prevention Guidance for Novel Coronavirus Pneumonia (version 5)* on February 21.[3] This guidance mandated local governments to assess COVID-19 risk at the community level. Communities reporting COVID-19 cases would be designated as either medium or high-risk zones, triggering the implementation of appropriate containment measures and closures. This is commonly known as the *zero-Covid policy*. In Figure B.1, we present a time-series graph illustrating the count of prefectures with an ongoing pandemic and the number of counties implementing the zero-Covid policy. In principle, low-risk communities should primarily impose quarantines on individuals traveling from high or medium-risk areas and refrain from restricting the movements of residents or economic activities. The policy's objective is to eliminate COVID-19 transmission at the local level by assigning risk levels to each community and implementing corresponding measures.

To supplement the zero-Covid policy, in March 2020, the State Council of China published a national COVID-19 risk level system on the official website. This system classifies communities within the 2853 counties into high, medium, or low-risk areas and updates

[3] *Prevention Guidance for Novel Coronavirus Pneumonia (version 5)*: `http://www.nhc.gov.cn/jkj/s3577/202002/a5d6f7b8c48c451c87dba14889b30147.shtml`

it daily.[4] All zero-Covid policy measures, including quarantine, public place closures, travel restrictions, and travel QR codes, were implemented based on this system. Specifically, local governments determine the risk level index (or non-risk) of a community based on recently reported confirmed and suspected cases of COVID-19, which is then reported to the National Health Commission of China. Local officials have some flexibility in adjusting the threshold for the risk level index and deciding whether to implement the zero-Covid policy. In certain situations, neighboring communities with no cases may still be classified as medium or high-risk areas; at the same time, areas that experienced outbreaks with dozens of cases could still be categorized as non-risk areas. Our research aims to investigate the endogeneity in this decision and understand how promotion incentives influence the choice of the local prefecture leaders regarding the zero-Covid policy when new COVID-19 cases emerge in their jurisdictions.

3.2.2 Promotion and Multitasking

The Chinese political system is both centralized and decentralized (Xu, 2011). On one hand, political appointments are typically determined by higher-level governments in China, with local leaders' career progression contingent on performance evaluations conducted by their superiors (Landry, 2008). For instance, provincial-level organizations oversee and assess the performance of prefecture-level officials. Therefore, this top-down hierarchical government structural manages to align the incentives of local officials with that of the party through centralized personnel control. This centralized personnel control allows

[4] State Council introduced risk level system on its official website: `http://www.gov.cn/fuwu/2020-03/25/content_5495289.htm`

the central government to align local officials' incentives with those of the party, a critical institutional foundation that has facilitated economic reforms in China since the 1980s (Blanchard and Shleifer, 2001; Enikolopov and Zhuravskaya, 2007).

On the other hand, economic decision-making and daily governance are highly decentralized in China's contemporary political-economic landscape. Local governments enjoy significant policy autonomy, driven by strong career-concern incentives for government officials. Economic and spending policies are predominantly decentralized, and local leaders hold substantial influence over local economic development (Jin et al., 2005). Party secretaries and mayors have a wide span of controls over policies that help boost the short term economic growth. The revenue-sharing arrangements within a decentralized fiscal system also motivates local leaders to promote economic growth (Qian and Weingast, 1997). In addition, the performance evaluation encompasses a broad range of tasks including social stability and public safety (Nie et al., 2013; Xi et al., 2018). In the 2014 version of the Central Committee of the Communist Party of China (CPC)'s guidelines, the significance of GDP as a performance metric was reduced. Instead, greater emphasis was placed on factors such as environmental protection, political loyalty, and government debt.

This duality of centralization and decentralization creates a multitasking challenge for local officials. When a potential crisis arises, such as the COVID-19 pandemic, the central government's crisis control objectives may clash with local leaders' personal incentives. While crisis control tasks demands containment of the virus and stability, local governors might still anticipate that their performance evaluations will cover various policy domains, including economic development. This unique multitasking dilemma, particularly faced by prefecture-level leaders, creates a complex situation. During the pandemic, they must strategically allocate their efforts across multiple policy areas, balancing central government directives with potential economic performance trade-offs.

3.2.3 Age Restrictions in Promotion

Age restrictions in China's cadre system have been frequently used to measure promotion incentive in recent literature (Xi et al., 2018; Zhou and Zeng, 2018; Shi and Xi, 2018; Huang et al., 2020; Shi et al., 2021). The CPC imposed restriction on the promotion of aging officials since the 1980s[5] and introduced age limits for officials in the 2000s (Kou and Tsai, 2014). Government policy explicitly states that "party and government cadres should resign from the position......upon reaching the age limit for assuming a position or the retirement age limit" (*Regulations on the Work of Selecting and Appointing Leading Party and Government Cadres*).[6] In his speech about the general election of the 17th CPC Conference in 2007, President Hu Jintao asserted that mayor-level officials aged 58 or more are ineligible for promotion and are subject to a mandatory retirement age of 60.[7] Moreover, it is a norm for officials in prefecture-level leadership roles to serve for a certain duration, typically three years or more, before being considered for promotion.[8] All the evidence mentioned above suggests that prefecture-level leaders aged 58 and older have little chance of promotion and are more likely to be reassigned to less critical ceremonial roles. Zhou and

[5] *The Decision of the Central Committee of the Communist Party of China on Establishing a Retirement System for Senior Party Cadres*, 1982

[6] `https://www.gov.cn/jrzg/2014-01/15/content_2567800.htm`

[7] `https://news.ifeng.com/mainland/200702/0210_17_75079_1.shtml`

[8] In latest revision of *Regulations on the Work of Selecting and Appointing Leading Party and Government Cadres* (2014), "If a county-level or higher leadership position is to be appointed by a deputy-level official to a higher-level position, they should have worked in the deputy-level position for at least two years. If being appointed from a lower-level leadership position to a higher-level deputy position, they should have worked in the lower-level leadership position for at least three years."

Zeng (2018) provide empirical findings indicating a significant decline in mayors' promotion probability once they surpass the age of 57, as shown in their original figure included in Figure B.2.

In this study, we categorize party secretaries whose ages are close to the promotion eligibility cutoff age as having a strong promotion incentive. Since the appointments to vice-provincial level positions are typically announced during the provincial National People's Congress conference, which usually takes place before February every year, we consider a prefecture party secretary eligible for promotion if his or her age is 57 years or less by the time of the next provincial National People's Congress conference. Consequently, we define the age of the official as the age they attain by the time of the next provincial National People's Congress conference. In our primary specification, we generate a *Promotion* dummy variable and assign a value of 1 to party secretaries aged between 54 and 57, and a value of 0 to all others. This range is the last time window for prefecture leaders to be promoted (Shi and Xi, 2018), and we argue that it creates unique promotion incentives for party secretaries within this age range. In Section 4.4.3, we will further discuss details regarding the robustness of the age range we employed for the *Promotion* dummy variable and other potential concerns.

3.3 Data

3.3.1 COVID-19 Pandemic Data

We collect daily confirmed COVID-19 case data from the *Dingxiangyuan* website, which aggregates official reports of daily COVID-19 cases at the prefecture level. We define that an *outbreak* event of COVID-19 pandemic occurs in a prefecture when a new confirmed COVID-19 case is reported after a 14-day period with no reported cases in the same prefecture. The outbreak is considered to have ended when the prefecture maintains a clean record of confirmed cases for a consecutive 14-day period. By constructing event windows for these outbreaks, we can pinpoint the initial date of each outbreak in the prefectures. It is worth noting that many prefectures experience multiple such outbreaks.

3.3.2 Zero-Covid Policy Data

Our data regarding zero-Covid policy status (risk level index) are sourced from the *China's COVID Risk Level Database* (Gong et al., 2023). This database provides information on COVID-19 risk levels for communities within the 2853 counties on a daily basis, spanning from April 02, 2021, to December 15, 2022, which corresponds to the conclusion of the zero-Covid policies. To determine whether a prefecture has a zero-Covid policy in place, we look at whether at least one community within that prefecture is reported as a medium or high-risk level area. Additionally, we create three other variables related to zero-Covid policy: the percentage of zero-Covid policy coverage within each prefecture, the highest risk level index value within each prefecture, and the count of counties implementing the zero-Covid policy within each prefecture. The utilization of these three variables is discussed in further detail in Section 4.4.2. We present a heatmap plot illustrating the cumulative number of days under the zero-Covid policy at the county level, as of the end of both 2021 and 2022 in Figure 3.2.

3.3.3 Characteristics of Prefecture Leader

We obtained information about prefecture party secretaries' age, education, gender, and ethnicity from government websites, Baidu Baike, and Wikipedia. Their tenure start and end dates were manually collected to accurately assign the prefecture leader to each prefecture at every date in our panel data.[9] As mentioned in previous section, we categorize officials as having strong promotion incentives and assign a "Promotion" dummy variable equal to 1 if their age falls within the range of 54 to 57; otherwise, it is set to 0. We plot the distribution of prefecture leaders' age in Figure 3.1 and provide a map indicating which prefectures were governed by *Promotion* or non-promotion leaders by the end of 2021 and 2022 in Figure 3.3.

Before delving into a formal regression analysis, we employ a non-parametric approach to explore the relationship between the zero-Covid policy and the pandemic's scale, as well as its variations across groups of prefecture leaders with or without promotional incentives. Specifically, we conduct a local kernel regression on the status of the zero-Covid policy and its coverage using the natural logarithm of the 7-day average case count, segmented by groups defined by promotional incentives. The results are presented in Figure 3.4. We observe that officials with strong promotional incentives exhibit similar zero-Covid policy implementation patterns as their counterparts when the 7-day average case count is relatively low—below 50 daily cases. However, the divergence becomes notably significant once the scale of the pandemic surpasses this threshold. While this result does not represent

[9] There are few prefectures which do not have party secretary in position for few months. We classified these prefectures as non-promotion.

a comprehensive analysis, it illuminates potential behavioral differences and lends credence to our forthcoming detailed analysis.

3.3.4 Mobility Data

Our traffic mobility data is originally from the Baidu Qianxi (Migration) website data and collected by Hu et al. (2020a). This data is obtained by monitoring the characteristics of HTTP requests to the data server. It provides information on traffic flow between prefectures. Specifically, it includes two indices: inflow mobility, representing traffic flow towards the destination prefecture, and outflow mobility, indicating traffic flow away from the departure prefecture. To ensure comparability across time and prefectures, we standardized the inflow and outflow mobility indices within each prefecture. The data covers the period from September 23, 2021, to April 21, 2022.

3.3.5 Prefecture Characteristics and Sample Data

We gathered data on prefecture-level GDP, population, the share of the service sector in GDP, and the urbanization rate, all evaluated in 2019, from provincial and city yearbooks. To obtain our final sample data for empirical analysis, we excluded the four municipalities directly administered by the central government (Beijing, Shanghai, Tianjin, and Chongqing). Additionally, we calculated the total number of days each prefecture experienced pandemic outbreaks (defined in previous section 3.1) and excluded those with more than 500 days of outbreak. These outlier prefectures were either port cities for international flights during the pandemic or located close to such entry points. We removed them from our sample as they remained under outbreak conditions for most of our data period from April 02, 2021 to December 15, 2022 (622 days). We provide a scatter plot of each outbreak's duration and cumulative confirmed cases in Figure B.3. Our focus is on the cluster of data points in the lower left corner of the graph[10] and most outlier points are dropped from our sample data. A statistical summary of the final sample data is provided in Table 3.1.

3.4 Empirical Strategies

3.4.1 Promotion Incentive

This research provides evidence that promotion incentives could influence the decisions made by prefecture leaders regarding the implementation of the zero-Covid policy. Our empirical analysis estimates the following regression:

$$ZeroCovid_{it} = \beta_1 Cases_{it} + \beta_2 Promotion_{it} + \beta_3 Cases_{it} \times Promotion_{it} + \gamma Cases_{it} \times X_{it} + \mu_i + \theta_t + \varepsilon_{it} \tag{1}$$

where $ZeroCovid_{it}$ is a dummy indicating the zero-Covid policy status in the prefecture i at date t; $Cases_{it}$ is the 7-day running average number of Covid-19 cases in the prefecture i at date t; $Promotion_{it}$ is a dummy indicating the party secretary's promotion incentive of the prefecture i at date t, which assigns value of 1 if the secretary's age falls between 54 and 57 and 0 otherwise; X_{it} is a set of control variables, including a dummy of year 2022, prefecture leader's tenure in position, education, gender and ethnicity; μ_i is the prefecture fixed effect and θ_t is the time fixed effect. To further isolate the potential influence of

[10]A zoomed-in figure is in Figure B.4

provincial leaders' preferences for different governance objectives, we control for province-by-month fixed effects in all regression specifications.

Among the estimated coefficients, β_1 represents the effect of emerging cases of COVID-19 on the zero-Covid policy decision in the absence of strong promotion incentives. As the prefecture leader observes more cases, it will be a greater chance for the prefecture leader to announce the zero-Covid policy, thus β_1 is expected to be positive and significant. β_2 represents the probability difference in the declaration of a zero-Covid policy between prefecture leaders with and without promotion incentives regardless of the COVID-19 cases. The coefficient of interest for this research is β_3, which represents the impact of the promotion incentives of the prefecture leader on the marginal probability increase of a zero-Covid policy caused by the emerging cases of COVID-19. For prefecture leaders with high incentives, if promotion pressure leads to greater compliance with the zero-Covid policy facing the same scale of the pandemic, we should observe a positive and significant β_3.

3.4.2 Event Study

Another important identification assumption underlying our empirical strategy is that there are no other factors that generate a differential trend in zero-Covid policy decisions rather than the emerging COVID-19 cases. In other words, for the estimated effect to have a causal interpretation, the model requires a parallel trend assumption: the differential in policy decision between prefectures with or without new cases of COVID-19 is constant in the absence of a pandemic outbreak. In the context of China's zero-Covid policy, the NHS guideline explicitly describes the condition for a region to be classified as high or medium risk, which requires the detection of COVID-19 cases. This policy background relieves our concern that other unobservable factors determine the zero-Covid policy.

Additionally, although the parallel trend assumption is hard to verify empirically as we were unable to observe the counterfactual policy outcomes, we could employ an event study to display that there is no pre-trend in the zero-Covid policy outcome difference between prefectures which face new outbreaks of the pandemic or have no ongoing pandemic. Specifically, we estimate the following model for prefectures governed by party secretaries in high- and low-incentive groups separately:

$$ZeroCovid_{it} = \sum_{k=-7}^{-1} \beta_k D_{it}^k + \sum_{k=0}^{21} \beta_k D_{it}^k + \mu_i + \theta_t + \varepsilon_{it} \tag{3}$$

where D_{it}^k represents the indicator for the treatment status as k periods relative to initial outbreak, which takes value of 1 if date t is k days relative to the first day of an outbreak in prefecture i and 0 otherwise, and other notation remains identical to that of equation (1).

The event study could provide us two pieces of supportive evidence regarding the role of promotion incentive in the decision-making process related to the zero-Covid policy. Firstly, it could lend us confidence that the rising COVID-19 cases casually drive the adoption of the zero-Covid policy. Consequently, our estimation in equation (1) could identify differences in the choice of zero-Covid policy between party secretaries with and without strong promotion incentive facing a similar scale of the pandemic outbreak. Secondly, through an examination of the dynamic effect of a new COVID-19 outbreak on the implementation of the zero-Covid policy, we can determine the likelihood of the zero-Covid policy being implemented shortly after the first case emerged and the timeframe within which the local government responds. Conducting this event study separately for both groups of prefecture leaders enables us to explore any systematic variations in their behavior following the outbreak.

3.4.3 Multitasking

We are also interested in investigating whether the impact of promotion incentives varies across regions with distinct socioeconomic foundations, particularly when the zero-Covid campaign may impose a higher economic cost, leading to conflicting demands with regard to economic development. We estimate the following regression to explore this potential heterogeneity:

$$\begin{aligned} ZeroCovid_{it} = {} & \beta_1 Cases_{it} + \beta_2 Promotion_{it} + \beta_3 Cases_{it} \times Promotion_{it} \\ & + \beta_4 Cases_{it} \times Promotion_{it} \times Z_i + \gamma Cases_{it} \times X_{it} + \mu_i + \theta_t + \varepsilon_{it} \end{aligned} \tag{4}$$

where Z_i is the socioeconomic characteristic of prefecture i, and other notation remains the same as previous specifications. We select three factors that are likely to be correlated to the economic cost of the zero-Covid policy: GDP per capita, the share of the service sector in GDP, and the urbanization rate. These factors were evaluated in the year 2019, prior to the pandemic.

When initiating zero-Covid policy, prefectures with higher economic development would incur a higher loss in the slowdown of growth of GDP, service sectors are more vulnerable to stay-at-home orders or city lockdowns and urban areas would experience more disturbance than rural areas due to the containment measures and restrictions. When implementing the zero-Covid policy, prefectures with higher levels of economic development are likely to face more substantial setbacks in terms of the slow down in GDP growth.

The service sector, being highly susceptible to stay-at-home orders and city lockdowns, is expected to be more vulnerable. Additionally, urban areas are likely to experience greater disruptions compared to rural areas due to the containment measures and restrictions. It is an empirical question that whether local governors, even when facing strong promotion pressures, will compromise on the implementation of the zero-Covid policy due to the economic burden it imposes. We aim to answer this question by estimating the coefficient β_4 in equation (4). This coefficient reflects the disparity in the response of emerging COVID-19 cases among prefectures with varying socioeconomic foundations, conditional on that the prefecture leaders are subject to strong promotion incentives.

3.4.4 Threats to Identification

Endogenous COVID-19 Cases

Our identification in equation (1) is based on a quasi-experimental design. While the emergence of a new wave of the pandemic is not entirely exogenous to prefecture-level factors, the number of confirmed COVID-19 cases shortly after the detection of the first case is semi-exogenous. It is plausible that, when the first case is detected, the virus has not yet spread widely among the population, resulting in a limited number of subsequent confirmed cases in the following weeks. Conversely, another scenario entails the virus quietly spreading for a period before the detection of the first case, leading to an exponential increase in confirmed cases until effective control measures are implemented. Given the uncertainty faced by party secretaries, who could not predict the specific condition they would encounter, we leverage this inherent randomness in the number of COVID-19 cases to identify variations in zero-Covid policy compliance as a response to the outbreak. This variation occurs among prefecture leaders with and without strong promotion incentives and we would like to identify the systematic difference between these two groups.

In the context of China's zero-Covid campaign, the primary objective is the dynamic elimination of the ongoing pandemic within each prefecture, ultimately reducing the number of confirmed cases to zero. Nevertheless, the implementation of the zero-Covid policy can exert a substantial influence on the trajectory of confirmed COVID-19 cases. Starting the zero-Covid policy earlier can potentially "flatten the curve" and expedite the termination of the outbreak. This implies that among the group of prefectures that were hesitant to implement the zero-Covid policy , they may eventually encounter with a larger number of confirmed COVID-19 cases for an extended period. This endogeneity between policy implementation and confirmed cases has the potential to introduce significant bias into our identification. For prefecture leaders with fewer promotion incentives, they might delay initiating the zero-Covid policy. Consequently, this delay could endogenously generate more observations with a large number of confirmed cases and no zero-Covid policy in place in the sample, resulting in an overestimate of the impact of promotion incentives in our estimation.

While we acknowledge that we cannot entirely eliminate the endogeneity issue from our estimation, we can mitigate it to some extent by constructing a subsample that consists of data from a short window after the first COVID-19 case is detected. By doing so, we exclude the period when the zero-Covid policy has already been implemented, which tends to suppress the number of confirmed cases. We also exclude the period when confirmed cases may have surged due to the exponential growth of the population affected by the virus. Before the implementation of the zero-Covid policy or the widespread transmission of the virus, the number of confirmed cases in this subsample closely approximates a randomized treatment in a natural experiment, which remains exogenous to other prefecture-level factors. During this selected period, prefecture leaders' decisions can better reflect their willingness to comply with the zero-Covid policy, as they have limited information about the actual outbreak situation. The regression results using this subsample could provide confidence in our identification of the impact of promotion incentives on the zero-Covid implementation.

Measurement of Zero-Covid Policy

There might be potential concerns regarding the measurement of the zero-Covid policy's implementation in our previous empirical analysis. We employed a binary variable to indicate whether a prefecture is subject to the zero-Covid policy, but this approach does not capture the depth and scope of the policy's application within the jurisdiction. It is plausible that a local governor might only impose restrictions in a limited area where recently diagnosed COVID-19 patients had visited, while the majority of the city and rural counties within the same prefecture remain unaffected by the zero-Covid policy.[11] Prefecture leaders driven by strong promotion incentives may decide to implement the zero-Covid policy in a limited area within their jurisdiction much more quickly compared to

[11] This scenario was indeed prevalent in many cities during the pandemic. For instance, in Shanghai, prior to the extensive lockdown initiated on April 22, 2022, there were no city-wide restrictions or containment measures in place since the first confirmed COVID-19 case of this outbreak on March 1. During this 52-day period, except for areas classified as medium or high-risk, the remainder of the city was not subject to any restrictions.

their counterparts with lower promotion incentives at the beginning of outbreaks. However, as the outbreak escalates and extends its reach, both groups may exhibit similar behavior in terms of expanding the coverage of the zero-Covid policy. Our binary measurement of the zero-Covid policy would lose track of this complexity of the policy decisions made by prefecture leaders, potentially exacerbating the influence of promotion incentives in their decision-making processes.

The granularity in our data provides us the possibility to examine the coverage and stringency of the zero-Covid policy implementation. To address potential biases caused by the measurement errors of the binary zero-Covid policy status, we propose an alternative approach. Instead of relying on the binary indicator for the zero-Covid policy status at the prefecture level, we could employ the county-level zero-Covid policy status and aggregating these data at the prefecture level.

Specifically, in equation (1), rather than utilizing a dummy indicator for the presence of the zero-Covid policy within the prefecture, we replace it by the percentage of county-level areas within the prefecture that have implemented the zero-Covid policy as the dependent variable of interest. This adjustment in equation (1) will enable us to analyze the influence of promotion incentives on the relationship between emerging COVID-19 cases and the extent of zero-Covid policy coverage within a prefecture. Then the estimated coefficient β_3 will represent the impact of the promotion incentives of the prefecture leader on the marginal percentage increase of a zero-Covid policy coverage at county level caused by the emerging cases of COVID-19. Moreover, to gain insight into the stringency of the implemented zero-Covid policy, we could also use the highest level of the zero-Covid policy risk level index within a prefecture to be the outcome variable in the regression of equation (1). This risk level index is categorized as "High", "Medium", and "Low" with corresponding assigned values of 2, 1, and 0, respectively, to serve as our dependent variable in this specification. In this context, β_3 represents the impact of the promotion incentives of the prefecture leader on the value of the highest risk level index associated with the zero-Covid policy, in response to emerging COVID-19 cases.

Measurement of Promotion Incentives

The primary assumption of our identification is that prefecture leaders experience a marked decline in promotion incentives as they surpass the age of 58, due to significantly diminished promotion prospects thereafter. While we could potentially exploit the discontinuity in promotion incentives linked with their age using a regression discontinuity design (RDD) to identify the causal impact of these incentives on policy results, we instead opt for measuring promotion incentives by age range because of two considerations.

First, the RDD approach relies on the assumption that the running variable—officials' ages—is not manipulated around the cut-off value. In the context of Chinese politics, this is questionable, as more competent and capable governors might advance in their ranks before nearing the age threshold. Figure 3.2 demonstrates that the distribution of officials around the threshold undergoes significant changes, which implies a potential breach of the no-manipulation assumption.

Second, in instances where the literature takes advantage of the age threshold's promotion incentive discontinuity, it is common to account for official fixed effects which controls official's talents, backgrounds, and other time-invariant personal traits. This allows for the identification of behavioral shifts for the same officials as they cross the age threshold. This approach is constrained in our case: our data cover only two years, as opposed to spanning multiple decades. In our dataset, only a select number of officials crossed the age threshold of 58 during the pandemic period.

Consequently, we choose to focus on officials nearing the promotion eligibility threshold, classifying them as our treatment group. We aim to estimate the average treatment effect of promotion incentives on the treatment group concerning zero-Covid policy compliance, drawing comparisons with a control group of officials either in the early stages of their career or past their final promotion opportunity. Though questions persist about the validity of the RDD in this context, we have included RDD approach regression findings in the Appendix.

There might be additional concerns regarding our chosen measure of promotion incentives in the baseline model. One potential concern suggests that younger officials may have different political motivations when making policy decisions (Alesina et al., 2019). To cope with the potential bias brought by other promotion incentives that are unobserved in our data, we propose several robustness checks. Firstly, we narrow our sample to officials aged 54 and above, drawing comparisons between officials at roughly equivalent career stages. Alternatively, we refine our treatment criteria to include all officials aged under 58. This strategy changes the scope to officials still holding promotion prospects against those with nearly no opportunities left. According to our assumption, the expected treatment effect on the likelihood of implementing zero-Covid policies should be positive under both these specifications.

3.5 Results

3.5.1 Main Results

We present the baseline estimates of the impact of promotion incentives on compliance with the zero-Covid policy in Table 3.2. In all specifications, we control for an interaction term of COVID-19 cases and the dummy of year 2022 and cluster the standard errors at the province-month level. In column (1), we estimate a two-way fixed-effect regression similar to that of equation (1), except that we did not include the interaction term between the COVID-19 case term and the promotion incentive dummy. The coefficient of $Promotion_{it}$ is not statistically significant. This suggests that prefecture leaders, whether with or without strong promotion incentives, do not exhibit a significant difference in their average probability of initiating the zero-Covid policy when controlling for the 7-day average confirmed COVID-19 cases. However, our primary interest lies in understanding how promotion incentives shapes the prefectures' response to the emerging outbreak of the pandemic as observed COVID-19 cases increase. Therefore, we aim to estimate the coefficient of $Cases_{it} \times Promotion_{it}$ in equation (1). Column (2) reports the estimates for the regression results of equation (1). The coefficient of $Cases_{it} \times Promotion_{it}$ is 0.00727 and statistically significant at the 0.01 level. This finding indicates that, for prefectures in the strong promotion incentive group, one average daily case in the past 7 days is associated with a 0.727% increase in the probability of implementing the zero-Covid policy compared to their counterparts in the low promotion incentive group. As we calculate a 7-day average daily case for $Cases_{it}$, we can construct a hypothetical scenario in which there have been 70 confirmed COVID-19 cases over the past 7 days. In this scenario, it would result in the strong promotion incentive group having a 7.27% higher probability of implementing the zero-Covid policy.

3.5.2 Dynamic Effects

As discussed in Section 4.3, our aim is to confirm whether the emergence of COVID-19 cases indeed drives the implementation of the zero-Covid policy, as opposed to other influencing factors. We employ an event study approach and estimate equation (3). Figure 3.2a presents the dynamic effects of the pandemic outbreak on the status of the zero-Covid policy, separately for prefectures in the strong and low promotion incentive groups. We establish the baseline as the date before the first confirmed case is found ($t = -1$). The coefficients $t = -7, ..., 21$ represent β_k in equation (3). From Figure 3.5a, it is clear that for both groups of prefectures, there is no pre-trend in the status of the zero-Covid policy leading up to $t = -1$. A noticeable coefficient jump occurs at $t = -1$, indicating an increased chance of implementing the zero-Covid policy just before the confirmation of the first COVID-19 case. This is reasonable given that test results could take time to conclude a positive COVID-19 case,[12] while local governments may initiate restrictions as soon as they receive reports of suspected COVID-19 cases. It is also possible that there may be delays in updating the number of confirmed cases.

At the same time, after the first COVID-19 case was confirmed, there is a rapid increase in the likelihood of zero-Covid policy in the subsequent days. Both groups of prefectures exhibit nearly identical patterns up to seven days after the outbreak. The coefficients for the low promotion group exhibit a declining trend starting from $t = 7$,

[12] The commonly used COVID-19 test in China, nucleic acid test, takes about 6 hours to get the result.

whereas the coefficients for the strong promotion group remain relatively high for a few more days before declining. Within the observation window, the differences in coefficients between the two groups are not statistically significant.

Building upon the insights derived from the Figure 3.4, which indicated that prefecture leaders with stronger promotion incentives tend to exhibit a greater inclination to actively comply with the zero-Covid policy during larger-scale outbreaks, we conduct an event study analysis using a subsample of outbreaks with a total of more than 50 cases. We present the dynamic effects of this specific analysis in Figure 3.5b. The overall trends in the dynamic patterns closely resemble those observed in Figure 3.5a. Consistent with the findings in Section 5.3, it is evident that when confronting larger pandemic outbreaks, the implementation of the zero-Covid policy in response to new outbreaks by prefectures in the strong promotion group displays a larger divergence from those in the low promotion group. The likelihood of the zero-Covid policy being implemented reaches its peak at almost 50%, significantly higher than the results obtained using the full samples, where it hovers around 25% at its peak. Furthermore, the strong promotion group extends the duration of the zero-Covid policy.

The dynamic patterns lend support to the argument that the implementation of the zero-Covid policy is driven by confirmed COVID-19 cases. On average, prefecture leaders with stronger promotion incentives tend to maintain the zero-Covid policy for a longer duration after the outbreak compared to their counterparts with less promotion incentives, while the difference between these two groups is not statistically significant.

3.5.3 Multitasking

In Table 3.3, we present the results of estimating the multitasking effect as described in Section 4.4. In columns (1), (3), and (5), we estimate the heterogeneous impacts of emerging COVID-19 cases on the zero-Covid policy decisions across prefectures, taking into account various economic factors, such as log of GDP per capita, the service sector's share of GDP, and the urbanization rate.

The coefficients of the interaction terms between the 7-day average cases and the economic factors are all statistically insignificant, suggesting that, on average, economic factors did not influence compliance with the zero-Covid policy. In columns (2), (4) and (6), we estimate the equation (4) by incorporating three-way interaction terms of confirmed cases, the dummy variable of promotion incentive, and the economic factors. We observe that the three coefficients hold values of -0.00918, -0.0543, and -0.0483, and are statistical significant at the 0.1, 0.05, and 0.01 levels, respectively. These findings suggest that, in a prefecture where the GDP per capita, the share of the service sector in GDP, or the urbanization rate is 1% higher than the average representative prefecture, and given that the prefecture's party secretary has a strong promotion incentive, the likelihood of implementing the zero-Covid policy per 7-day average confirmed cases will decrease by 0.918%, 5.43%, and 4.83%, respectively.

While it's important to only interpret these coefficients at the average value of these economic factors across all prefectures, the negative sign of all three coefficients suggests that prefecture leaders with strong promotion incentives also take into account the potential economic challenges raised by the zero-Covid policy. This result highlights the inherent tension that local governors face between pandemic restrictions and the target of economic prosperity. Although prefecture leaders may be incentivized by the prospect of promotion to closely follow the crisis control policies announced by the central government, they cannot completely neglect their daily tasks, including economic development, as economic performance may still be a part of their promotion evaluation.

3.5.4 Robustness Checks

Endogeniety Concern

In this section, we present robustness checks addressing the identification concerns discussed in the previous Section 4.2. We construct a subsample of data from a 7-day period before and a 28-day period after a COVID-19 outbreak. We then replicate the regressions from columns (1) - (3) in Table 3.2. In Table 3.4, we present the regression results for this robustness check.

As shown in column (2) of Table 3.4, the coefficient of $Cases_{it} \times Promotion_{it}$ remains positive and significant at the 0.01 level, with a value of 0.0125. This value is nearly twice the original coefficient value in column (2) of Table 3.2. This suggests that during the constructed window following the initial outbreak, prefecture leaders with strong promotion incentives exhibit a more proactive response to emerging COVID-19 cases, as compared to our original estimation in Table 3.2 covering the entire period. This alleviates our concern that endogeneity between the pandemic scale and the implementation of the zero-Covid policy could lead to an overestimation of the impact of promotion incentives.

Furthermore, we categorize the outbreaks based on their total confirmed COVID-19 cases, defining them as large outbreaks if the total cases exceed 50 and as small outbreaks otherwise. Through this approach, our goal is to verify whether party secretaries exhibit similar behavior when facing small outbreaks, and whether secretaries with strong promotion incentives tend to demonstrate more proactive compliance with the zero-Covid policy specifically in the case of large outbreaks. Columns (4) - (5) focus on the subsample of large outbreaks, and columns (6) - (7) specifically analyze the subsample of small outbreaks.

In column (5) of Table 3.4, when analyzing the subsample of large outbreaks, we note that the coefficient of interest is slightly larger than that in column (2) and remains statistically significant. However, in column (7), the coefficient is notably smaller and not significant. Consequently, we conclude that promotion incentives do indeed play a differentiating role in the zero-Covid policy decisions of prefecture leaders, but primarily during large outbreaks.

Alternative Measurement of Zero-Covid Policy

To address the potential bias introduced by measurement errors in the zero-Covid policy status, we replicate the baseline regression presented in Table 3.2 using three distinct dependent variables: the percentage of zero-Covid policy coverage within the prefecture, the highest value of the risk level index within the prefecture, and the count of counties implementing the zero-Covid policy within the prefecture. The results are presented in columns (1) - (2) of Table 3.5, Table 3.6, and Table 3.7, respectively.[13] The estimations do not reveal significant discrepancies from the original findings.

[13]Figure 3.6 visually presents the comparison of the primary coefficients of β_3 in Equation 1 across various specifications.

We further replicate the multitasking regression outcomes as presented in Table 3.4, utilizing alternative measurements for the zero-Covid policy. These replicated results are displayed in columns (3) - (8) of Table 3.5, Table 3.6, and Table 3.7. Most of these results align closely with our main findings.[14]

Alternative Encoding and Subsample for Promotion Incentives

We present the robustness checks for potential measurement error in promotion incentives in Table 3.8. In columns (1) - (3), we display the results from Table 3.2 for reference purposes. In columns (4) - (6), we replicate the baseline regressions using the subsample of officials aged 54 or above; in columns (7) - (9), we use the full sample while relaxing the criteria of having high promotion incentives to all officials aged less than 58.

The findings remain consistent with our primary conclusions. For columns (4) - (6), the estimated coefficients increase in magnitude compared to their counterparts in the original results while remaining statistically significant. This suggests that younger officials—those excluded from this subset—might be less proactive in the zero-Covid policy, possibly due to a lack of pressing promotional aspirations. In contrast, columns (7) - (9) see the coefficients diminish substantially in magnitude, though they remain positive and significant. This reduction underscores that, in comparison to their promotion-eligible peers, older officials with minimal promotional prospects are the least motivated to align with the zero-Covid policy. The robustness check results emphasize that the most promotion incentive likely resides among officials aged between 54 and 57, further affirming the reliability of our baseline results.

[14] Figure 3.7 visually presents the comparison of the primary coefficients of β_4 in Equation 4 for different economic factors across various specifications.

3.5.5 Stringency of Zero-Covid Policy

To capture the heterogeneous effects of the zero-Covid policy on traffic mobility across prefectures within the strong and low promotion incentive groups, as well as those with different socioeconomic foundations, we estimate the following model:

$$\begin{aligned} Mobility_{it} = {} & \alpha_1 ZeroCovid_{it} + \alpha_2 Promotion_{it} + \alpha_3 ZeroCovid_{it} \times Promotion_{it} \\ & + \gamma ZeroCovid_{it} \times X_{it} + \mu_i + \theta_t + \varepsilon_{it} \end{aligned} \tag{5}$$

where $Mobility_{it}$ is the measurement of traffic mobility for prefecture i at date t and other notation remains the same as previous specifications. Gong et al. (2023) have already estimated the impact of zero-Covid policy on traffic mobility. However, our focus lies on the coefficients α_3 and γ, which represent the differentials in the degree of mobility disruption following the implementation of the zero-Covid policy, influenced by promotion incentives and economic factors. We report the estimates of equation (5) in Table 3.9, where the dependent variable is the mobility inflow index (standardized traffic flow directed to the prefecture), and in Table 3.10, where the dependent variable is the the mobility outflow index (standardized traffic flow originating from the prefecture).

In column (1) of Table 3.9 and Table 3.10, both coefficients of $ZeroCovid_{it} \times Promotion_{it}$ in these two specifications are not statistically significant. This suggests that promotion incentives do not systematically influence the mobility restriction effect of the zero-Covid policy. In columns (2), (4), and (6), we observe that in more economically developed prefectures with higher GDP per capita, a greater share of the service sector in GDP, and a higher urbanization rate, the zero-Covid policy leads to a more significant decrease in traffic mobility. This is logical because economically developed, service-oriented, and urbanized prefectures tend to have stronger traffic connections with other regions, making them more susceptible to mobility reductions due to zero-Covid restrictions.

In columns (3), (5), and (7), we include the three-way interaction terms involving the zero-Covid policy status, promotion incentive dummies, and economic factors. The results consistently show negative coefficients, although none of them reaches statistical significance. This appears to be contradictory to the findings in Section 5.5, where prefecture leaders in more economically developed areas with strong promotion incentives tended to be less enthusiastic about implementing the zero-Covid policy. Meanwhile, in these same prefectures, we observe a larger decrease in traffic mobility following the initiation of the zero-Covid policy.

These seemingly conflicting results are inherent in the rationale of local governors with strong promotion incentives. The objective of these prefecture leaders is to earn favor in promotion evaluations by demonstrating their ability to control pandemics while also safeguarding the economy from substantial slowdowns, if not stimulating its growth. Therefore, when they assess it necessary to initiate the zero-Covid policy due to the emergence of confirmed cases, their aim is to swiftly control the pandemic, allowing for the reopening of the city as early as possible and ensuring economic growth. Our empirical findings can indeed offer evidence to support this narrative.

3.6 Conclusions

In conclusion, this study provides valuable insights into the complex interplay between crisis control and economic development in the context of China's zero-Covid policy.

Our findings suggest that promotion incentives can significantly influence the response of local officials to emerging COVID-19 outbreaks, leading to a natural tension between crisis management and routine performance in economic development.

Specifically, we find that prefecture leaders who are closer to the promotion eligibility age threshold exhibit a higher propensity to adhere to the zero-Covid policy. Yet, they may be cautious if their jurisdictions are more vulnerable to the side effects of anti-contagion measures, given the potential for greater economic setbacks. Additionally, we observe that in economically advanced regions, the mobility constraints during the zero-Covid policy are more rigorous. This suggests that officials aim to swiftly curtail the virus's spread by limiting human activity, thereby mitigating the policy's economic impact.

Overall, this study contributes to the ongoing debate on the optimal balance between public health and economic growth in the face of pandemics, and underscores the need for effective policy and institutional frameworks to address these challenges. Future research could explore the generalizability of our findings to other countries and contexts, and investigate the role of other factors, such as political ideology and public opinion, in shaping pandemic response.

3.7 Figures and Tables

3.7.1 Figures

Figure 3.1: Prefecture Leader Age Categorized by Promotion Incentives

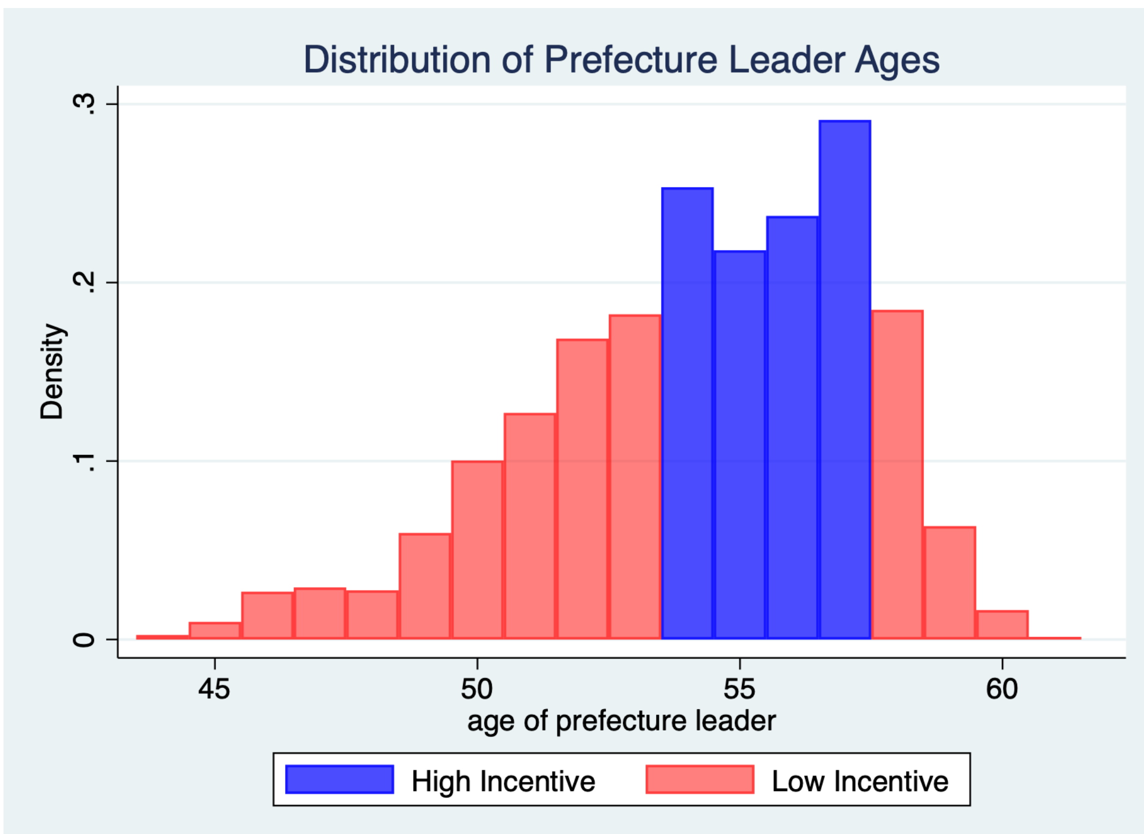
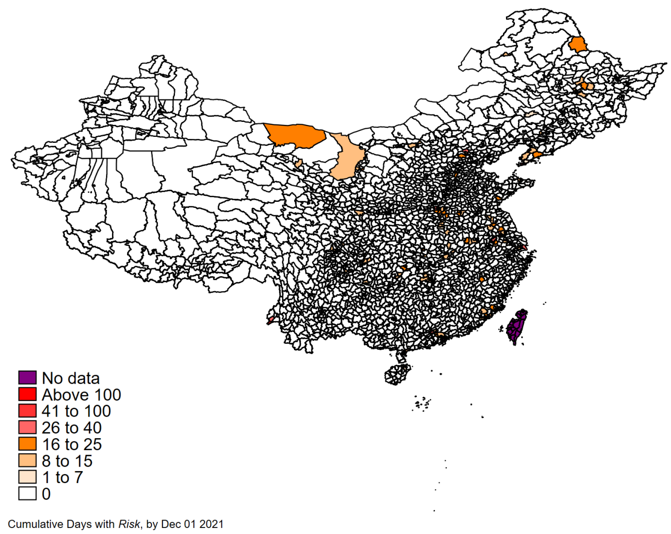


Figure 3.2: Cumulative Days Under Zero-Covid Policy at County Level

(a) Cumulative Days Under zero-Covid policy by end of 2021



(b) Cumulative Days Under zero-Covid policy by end of 2022

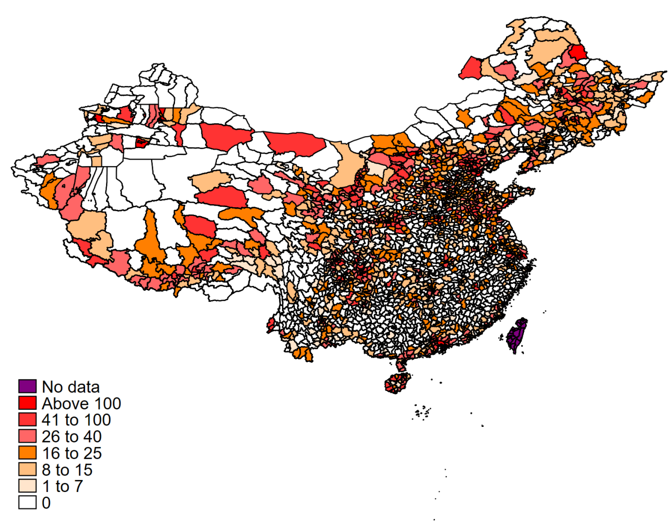
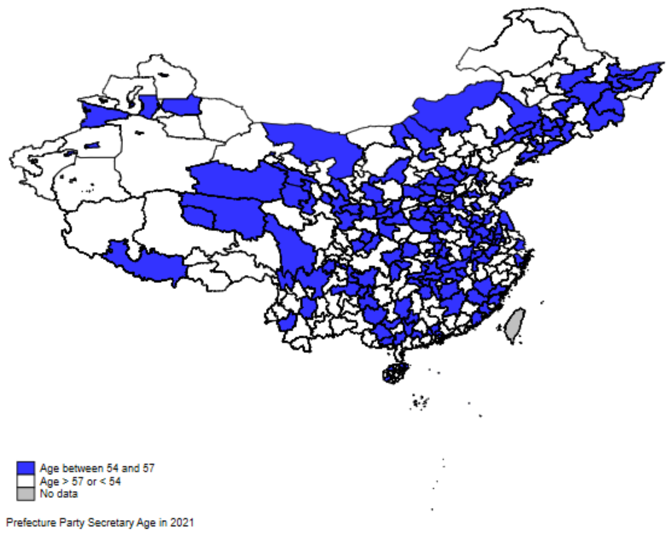


Figure 3.3: Prefecture Leader Age Categorized by Promotion Incentives

(a) Prefecture Leader Age in 2021



(b) Prefecture Leader Age in 2022

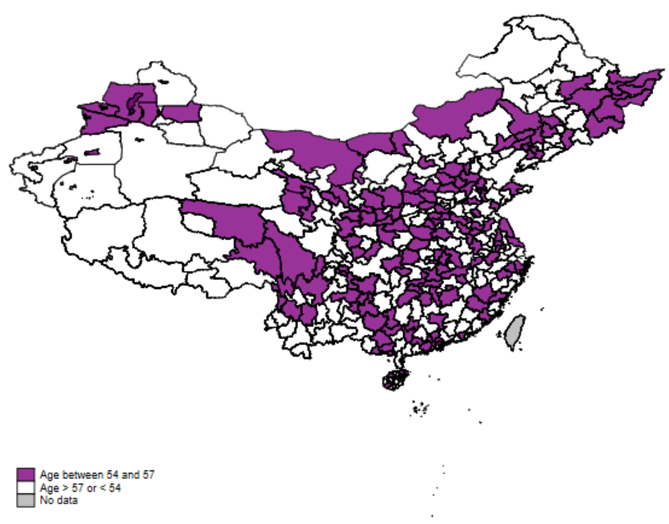
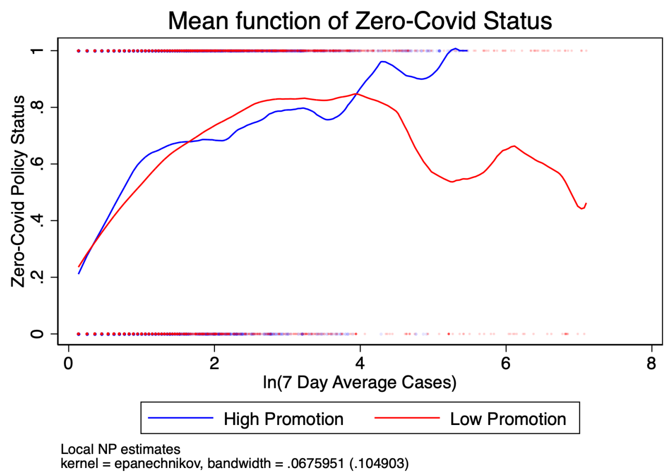


Figure 3.4: Non-parametric estimates of zero-Covid policy measurements on natural log of 7 day average cases

(a) Kernel mean of status of zero-Covid policy



(b) Kernel mean of portion of counties with zero-Covid policy

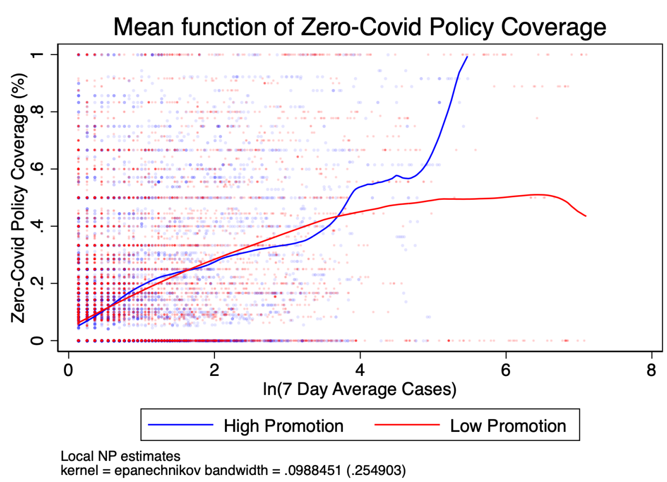
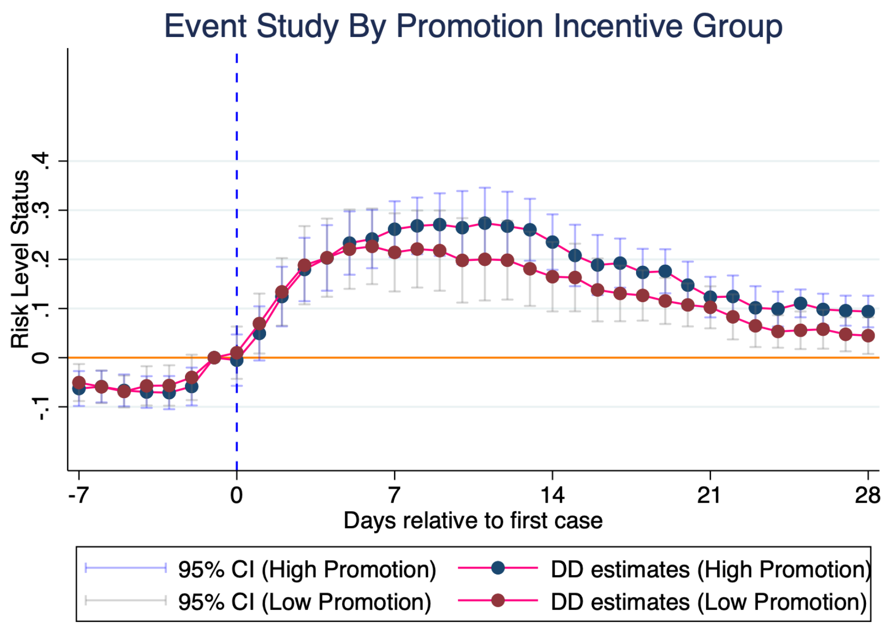


Figure 3.5: Plot of coefficients of the event study regression

(a) Full Sample



(b) Sample of outbreaks with 50 or more cases

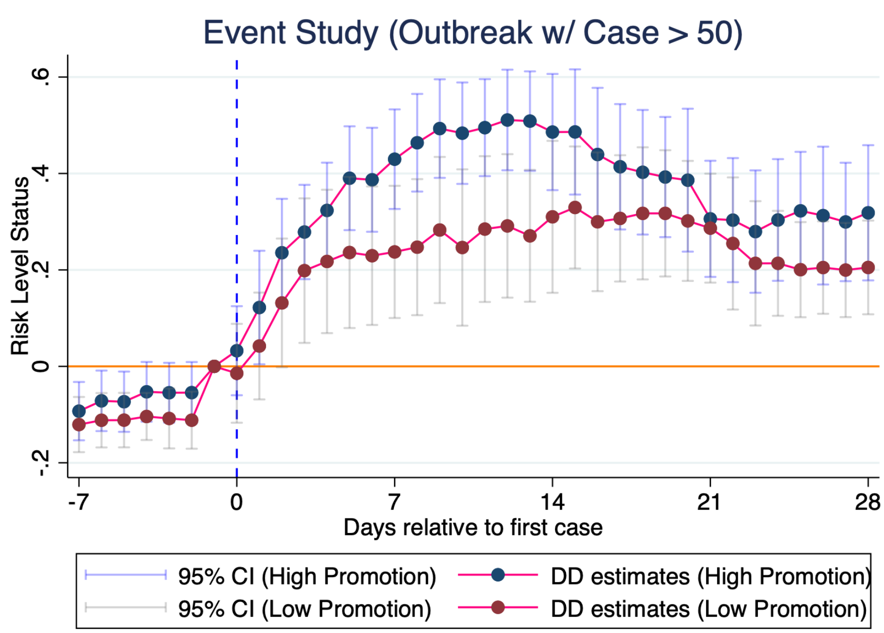


Figure 3.6: Main Regression Results for Alternative Dependent Variables

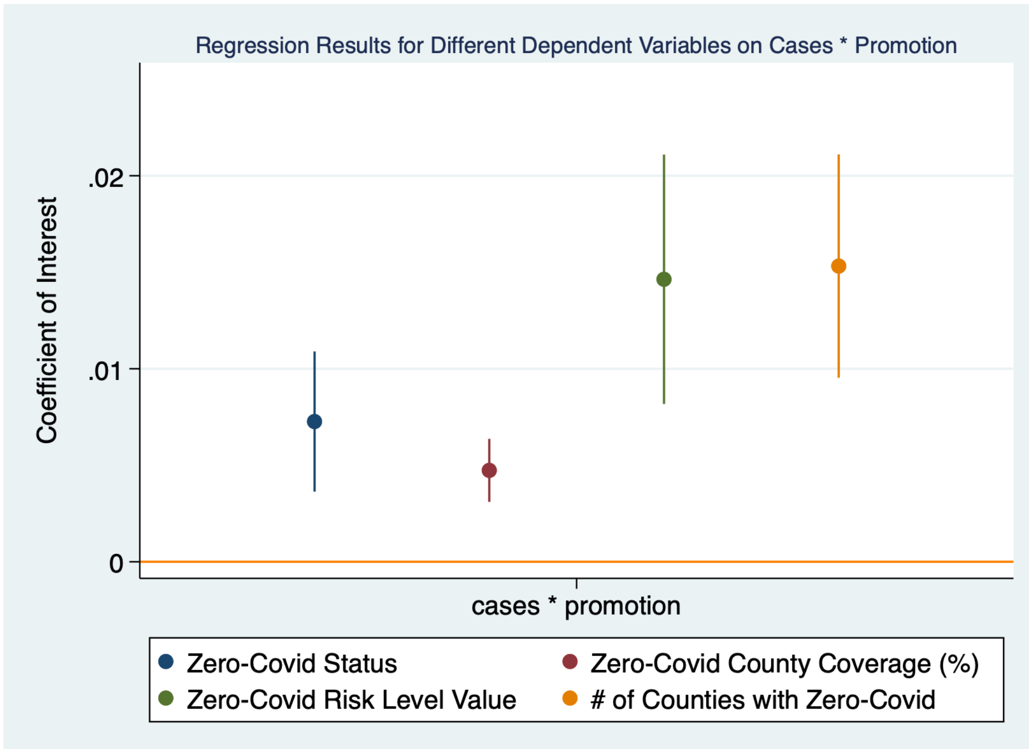
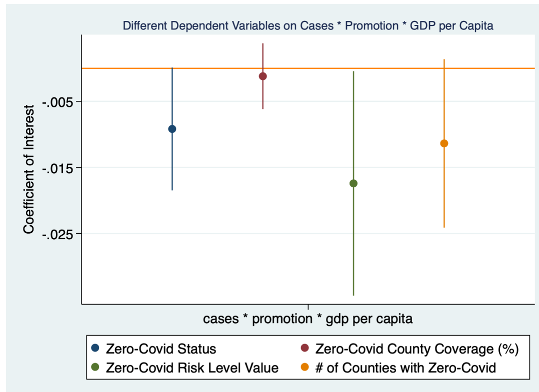
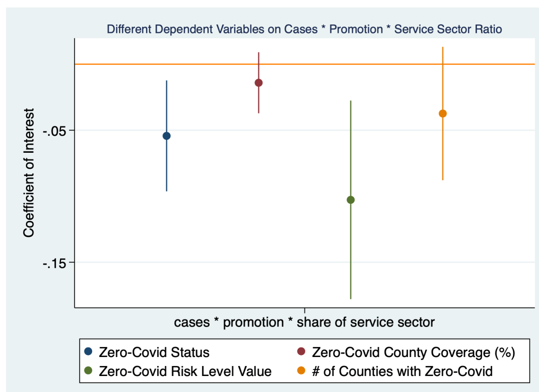


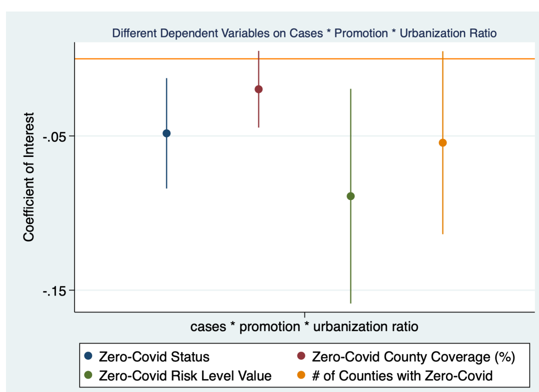
Figure 3.7: Multitasking Regression Results for Alternative Dependent Variables



(a) GDP per capita



(b) Share of service sector in GDP



(c) Urbanization ratio

3.7.2 Tables

Table 3.1: Statistical Summary

Statistical Summary

VARIABLES	(1) N	(2) mean	(3) sd	(4) min	(5) max
Panel A: Party Secretaries					
age of prefecture leader	451	54.58	3.278	45	61
promotion	451	0.437	0.497	0	1
female	451	0.0532	0.225	0	1
minor ethnicity	451	0.0732	0.261	0	1
Bachelor degree	451	0.208	0.407	0	1
Master degree	451	0.627	0.484	0	1
PhD degree	451	0.162	0.369	0	1
Panel B: Prefecture Characteristics					
population (Millions)	274	4.180	2.664	0.438	12.75
share of service sector	274	0.490	0.0726	0.285	0.805
urbanization ratio	274	0.605	0.121	0.351	0.954
GDP (Billion Yuan)	274	279.0	292.0	20.60	2,017
Panel C: COVID-19 Pandemic, Zero-Covid Policy and Mobility					
Zero-Covid Status	170,428	0.0717	0.258	0	1
Number of counties under zero-Covid	170,428	0.190	0.914	0	18
Percentage of counties under zero-Covid	170,428	0.0224	0.103	0	1
Highest Risk Level Value	170,428	0.125	0.465	0	2
Daily confirmed COVID-19 Cases	170,428	0.653	18.81	0	2,622
7-day Average Case	170,428	0.642	13.83	0	1,211
Outflow Mobility	63,294	0.0716	0.0674	0.00354	0.594
Inflow Mobility	63,294	0.0710	0.0658	0.00380	0.633

Table 3.2: Main Regression: Effect of Promotion Incentives on the choice of Zero-Covid Policy

Dependent Variable: Zero-Covid Policy Status

VARIABLES	(1)	(2)	(3)	(4)
promotion	-0.00256	-0.00601	-0.00713	-0.000283
	(0.00653)	(0.00646)	(0.00694)	(0.00751)
cases * promotion		0.00727***	0.00913***	0.00920***
		(0.00184)	(0.00138)	(0.00167)
7 day cases	0.0237***	0.0176**	0.0160**	0.0153*
	(0.00734)	(0.00803)	(0.00711)	(0.00778)
cases * Year_2022	-0.0225***	-0.0167**	-0.0160**	-0.0153**
	(0.00738)	(0.00803)	(0.00730)	(0.00765)
Observations	170,702	170,702	167,930	167,930
R^2	0.409	0.414	0.415	0.432
Prefecture FEs	YES	YES	YES	YES
Prov-Month FEs	YES	YES	YES	YES
Cases * Control	NO	NO	YES	YES
Secretary FEs	NO	NO	NO	YES
Clustered SE	Prefecture	Prefecture	Prefecture	Prov-Month
Age Range	All	All	All	All

Clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.3: Main Regression: Heterogeneous Effect of Promotion Incentives on Zero-Covid Policy by GDP per capita, Share of service sector, Urbanization Ratio

	Dependent Variable: Zero-Covid Policy Status					
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES						
7 day cases	0.0255*	0.0261**	0.0216***	0.0165**	0.0216*	0.0231**
	(0.0152)	(0.0110)	(0.00812)	(0.00721)	(0.0111)	(0.00981)
cases * Year_2022	-0.0226***	-0.0170**	-0.0224***	-0.0167**	-0.0223***	-0.0182**
	(0.00742)	(0.00814)	(0.00749)	(0.00702)	(0.00763)	(0.00755)
promotion		-0.00719		-0.00692		-0.00775
		(0.00643)		(0.00643)		(0.00644)
cases * promotion		0.111**		0.0385***		0.0438***
		(0.0539)		(0.0128)		(0.0134)
cases * gdp per capita	-0.000159	-0.000752				
	(0.00113)	(0.000676)				
cases * promotion * gdp per capita		-0.00918*				
		(0.00472)				
cases * share of service sector			0.00376	0.00206		
			(0.00511)	(0.00316)		
cases * promotion * share of service sector				-0.0543**		
				(0.0213)		
cases * urbanization ratio					0.00276	-0.00625
					(0.00940)	(0.00963)
cases * promotion * urbanization ratio						-0.0483***
						(0.0182)
Observations	170,702	170,702	170,702	170,702	170,702	170,702
R^2	0.409	0.415	0.409	0.416	0.409	0.417
Prefecture FEs	YES	YES	YES	YES	YES	YES
Prov-Month FEs	YES	YES	YES	YES	YES	YES
Clustered Standard Error	Prefecture	Prefecture	Prefecture	Prefecture	PPrefecture	Prefecture

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.4: Main Regression Using Sample Data in the window of 7 days before till 28 days after an Outbreak

	Dependent Variable: Zero-Covid Policy Status					
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES						
7 day cases	0.0129***	0.00229	0.0101***	-0.00353	0.215***	0.210***
	(0.00350)	(0.00305)	(0.00249)	(0.00577)	(0.0336)	(0.0349)
cases * Year_2022	-0.0110***	-0.000858	-0.00876***	0.00441	-0.0906**	-0.0899**
	(0.00363)	(0.00300)	(0.00263)	(0.00576)	(0.0390)	(0.0387)
promotion	-0.0433	-0.0623*	-0.127	-0.204*	-0.0495	-0.0528
	(0.0327)	(0.0326)	(0.108)	(0.115)	(0.0387)	(0.0386)
cases * promotion		0.0125***		0.0156***		0.0131
		(0.00386)		(0.00455)		(0.0317)
Observations	31,599	31,599	6,749	6,749	25,433	25,433
R^2	0.412	0.418	0.490	0.508	0.463	0.463
Prefecture FEs	YES	YES	YES	YES	YES	YES
Prov-Month FEs	YES	YES	YES	YES	YES	YES
Clustered SE	Prefecture	Prefecture	Prefecture	Prefecture	Prefecture	Prefecture
Subsample	Within 28 Days	Within 28 Days	Large Outbreak	Large Outbreak	Small Outbreak	Small Outbreak

Clustered standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 3.5: Dependent Variable: Portion of Counties with Zero-Covid

	Dependent Variable: Zero-Covid Policy Coverage							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES								
7 day cases	0.00952***	0.00551*	0.00832	0.0112*	0.00348	0.00137	-0.00141	8.62e-05
	(0.00250)	(0.00285)	(0.00979)	(0.00571)	(0.00389)	(0.00316)	(0.00526)	(0.00547)
cases * Year_2022	-0.00855***	-0.00475*	-0.00852***	-0.00481*	-0.00807***	-0.00477*	-0.00711**	-0.00499*
	(0.00253)	(0.00284)	(0.00256)	(0.00289)	(0.00280)	(0.00274)	(0.00279)	(0.00272)
promotion	0.000311	-0.00193		-0.00220		-0.00207		-0.00220
	(0.00258)	(0.00245)		(0.00245)		(0.00243)		(0.00244)
cases * promotion		0.00474***		0.0184		0.0124*		0.0187**
		(0.000829)		(0.0287)		(0.00697)		(0.00927)
cases * gdp per capita			0.000106	-0.000509				
			(0.000842)	(0.000463)				
cases * promotion * gdp per capita				-0.00119				
				(0.00253)				
cases * share of service sector					0.0107**	0.00803***		
					(0.00465)	(0.00301)		
cases * promotion * share of service sector						-0.0141		
						(0.0117)		
cases * urbanization ratio							0.0143**	0.00863
							(0.00576)	(0.00726)
cases * promotion * urbanization ratio								-0.0197
								(0.0126)
Observations	170,702	170,702	170,702	170,702	170,702	170,702	170,702	170,702
R^2	0.460	0.474	0.460	0.475	0.465	0.477	0.464	0.476
Prefecture FEs	YES	YES	YES	YES	YES	YES	YES	YES
Prov-Month FEs	YES	YES	YES	YES	YES	YES	YES	YES
Clustered SE	Prefecutre	Prefecutre	Prefecutre	Prefecutre	Prefecutre	Prefecutre	Prefecutre	Prefecutre

Clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.6: Dependent Variable: Highest Value of Zero-Covid Risk Level

	Dependent Variable: Zero-Covid Policy Level							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES								
7 day cases	0.0378***	0.0254**	0.0426	0.0444**	0.0309**	0.0212*	0.0304	0.0344**
	(0.0112)	(0.0122)	(0.0296)	(0.0199)	(0.0131)	(0.0108)	(0.0192)	(0.0174)
cases * Year_2022	-0.0358***	-0.0241**	-0.0360***	-0.0246**	-0.0353***	-0.0242**	-0.0349***	-0.0267**
	(0.0113)	(0.0122)	(0.0114)	(0.0124)	(0.0117)	(0.0102)	(0.0118)	(0.0112)
promotion	-0.000347	-0.00728		-0.00960		-0.00894		-0.0104
	(0.0117)	(0.0116)		(0.0115)		(0.0116)		(0.0116)
cases * promotion		0.0146***		0.212**		0.0734***		0.0818***
		(0.00328)		(0.0985)		(0.0228)		(0.0258)
cases * gdp per capita			-0.000421	-0.00167				
			(0.00237)	(0.00143)				
cases * promotion * gdp per capita				-0.0174**				
				(0.00863)				
cases * share of service sector					0.0122	0.00841		
					(0.0108)	(0.00689)		
cases * promotion * share of service sector						-0.103***		
						(0.0382)		
cases * urbanization ratio							0.00979	-0.00957
							(0.0191)	(0.0205)
cases * promotion * urbanization ratio								-0.0890**
								(0.0354)
Observations	170,702	170,702	170,702	170,702	170,702	170,702	170,702	170,702
R^2	0.446	0.453	0.446	0.454	0.446	0.455	0.446	0.455
Prefecture FEs	YES	YES	YES	YES	YES	YES	YES	YES
Prov-Month FEs	YES	YES	YES	YES	YES	YES	YES	YES
Clustered SE	Prefecutre	Prefecutre	Prefecutre	Prefecutre	Prefecutre	Prefecutre	Prefecutre	Prefecutre

Clustered standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 3.7: Dependent Variable: Number of Counties Under Zero-Covid

	Dependent Variable: Zero-Covid Policy Number of Counties							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES								
7 day cases	0.0341***	0.0211**	0.0304	0.0358**	0.0212*	0.0145	0.00599	0.0137
	(0.00804)	(0.00961)	(0.0279)	(0.0163)	(0.0125)	(0.0103)	(0.0160)	(0.0158)
cases * Year_2022	-0.0315***	-0.0192**	-0.0314***	-0.0196**	-0.0305***	-0.0192**	-0.0278***	-0.0202**
	(0.00813)	(0.00960)	(0.00822)	(0.00972)	(0.00878)	(0.00913)	(0.00894)	(0.00922)
promotion	-0.000903	-0.00816		-0.00973		-0.00863		-0.00933
	(0.00886)	(0.00850)		(0.00844)		(0.00846)		(0.00845)
cases * promotion		0.0153***		0.144**		0.0361**		0.0547**
		(0.00294)		(0.0730)		(0.0148)		(0.0211)
cases * gdp per capita			0.000325	-0.00129				
			(0.00236)	(0.00123)				
cases * promotion * gdp per capita				-0.0114*				
				(0.00647)				
cases * share of service sector					0.0227	0.0127		
					(0.0152)	(0.00918)		
cases * promotion * share of service sector						-0.0374		
						(0.0257)		
cases * urbanization ratio							0.0368**	0.0129
							(0.0164)	(0.0197)
cases * promotion * urbanization ratio								-0.0544*
								(0.0301)
Observations	170,702	170,702	170,702	170,702	170,702	170,702	170,702	170,702
R^2	0.476	0.491	0.476	0.493	0.478	0.492	0.479	0.493
Prefecture FEs	YES	YES	YES	YES	YES	YES	YES	YES
Prov-Month FEs	YES	YES	YES	YES	YES	YES	YES	YES
Clustered SE	Prefecutre	Prefecutre	Prefecutre	Prefecutre	Prefecutre	Prefecutre	Prefecutre	Prefecutre

Clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.8: Alternative Encoding and Subsample for Promotion Incentives

	Dependent Variable: Zero-Covid Policy Status								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES									
7 day cases	0.0176**	0.0160**	0.0153*	0.0134*	0.0192**	0.0181**	0.0218***	0.0140*	0.0133
	(0.00803)	(0.00711)	(0.00778)	(0.00749)	(0.00767)	(0.00833)	(0.00746)	(0.00833)	(0.00858)
cases * Year_2022	-0.0167**	-0.0160**	-0.0153**	-0.0129*	-0.00898*	-0.00874	-0.0213***	-0.0201***	-0.0195**
	(0.00803)	(0.00730)	(0.00765)	(0.00749)	(0.00501)	(0.00595)	(0.00746)	(0.00762)	(0.00778)
promotion	-0.00601	-0.00713	-0.000283	-0.0155*	-0.0185*	-0.0113	-0.00916	-0.0166*	-0.00885
	(0.00646)	(0.00694)	(0.00751)	(0.00796)	(0.0104)	(0.0111)	(0.00767)	(0.00974)	(0.0102)
cases * promotion	0.00727***	0.00913***	0.00920***	0.00778***	0.0113***	0.0113***	0.00193*	0.00600***	0.00595***
	(0.00184)	(0.00138)	(0.00167)	(0.00196)	(0.00232)	(0.00263)	(0.000998)	(0.00185)	(0.00223)
Observations	170,702	167,930	167,930	104,092	101,320	101,320	170,702	167,930	167,930
R^2	0.414	0.415	0.432	0.421	0.424	0.434	0.411	0.414	0.430
Prefecture FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES
Prov-Month FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES
Cases * Control	NO	YES	YES	NO	YES	YES	NO	YES	YES
Secretary FEs	NO	NO	YES	NO	NO	YES	NO	NO	YES
Clustered Standard Error	Prefecture	Prefecture	Prov-Month	Prefecture	Prefecture	Prov-Month	Prefecture	Prefecture	Prov-Month
Age Range	All	All	All	54 - 61	54 - 61	54 - 61	All	All	All
Promotion Criteria	54 - 57	54 - 57	54 - 57	54 - 57	54 - 57	54 - 57	< 57	< 57	< 57

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.9: Effect of Zero-Covid Policy on Mobility Inflow

	Dependent Variable: Mobility Inflow						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES							
Zero-Covid	-0.176***	1.188***	0.941**	0.471***	0.363***	0.280***	0.173**
	(0.0451)	(0.371)	(0.471)	(0.125)	(0.0867)	(0.0921)	(0.0824)
Zero-Covid * Year_2022	0.0344	0.0391	0.0327	0.0220	0.0238	0.0292	0.0282
	(0.0352)	(0.0306)	(0.0351)	(0.0213)	(0.0259)	(0.0268)	(0.0321)
promotion	-0.00645	-0.00866	-0.00690	-0.00843	-0.00572	-0.00783	-0.00581
	(0.00607)	(0.00595)	(0.00596)	(0.00571)	(0.00544)	(0.00571)	(0.00572)
Zero-Covid * promotion	-0.0366		0.468		0.257		0.208
	(0.0461)		(0.689)		(0.229)		(0.144)
Zero-Covid * gdp per capita		-0.125***	-0.101**				
		(0.0337)	(0.0411)				
Zero-Covid * promotion * gdp per capita			-0.0454				
			(0.0642)				
Zero-Covid * share of service sector				-1.266***	-1.013***		
				(0.265)	(0.197)		
Zero-Covid * promotion * share of service sector					-0.594		
					(0.488)		
Zero-Covid * urbanization ratio						-0.706***	-0.515***
						(0.154)	(0.121)
Zero-Covid * promotion * urbanization ratio							-0.372
							(0.254)
Observations	63,294	63,294	63,294	63,294	63,294	63,294	63,294
R^2	0.929	0.930	0.930	0.932	0.933	0.931	0.932
Prefecture FEs	YES	YES	YES	YES	YES	YES	YES
Prov-Month FEs	YES	YES	YES	YES	YES	YES	YES
Clustered Standard Error	Prefecture	Prefecture	Prefecture	Prefecture	Prefecture	Prefecture	Prefecture

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.10: Effect of Zero-Covid Policy on Mobility Outflow

	Dependent Variable: Mobility Outflow						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES							
Zero-Covid	-0.212***	1.433***	0.913*	0.667***	0.556***	0.383***	0.260***
	(0.0568)	(0.456)	(0.473)	(0.151)	(0.103)	(0.112)	(0.0818)
Zero-Covid * Year_2022	0.0460	0.0522	0.0429	0.0289	0.0292	0.0392	0.0372
	(0.0424)	(0.0358)	(0.0418)	(0.0221)	(0.0292)	(0.0308)	(0.0384)
promotion	-0.00690	-0.00979	-0.00749	-0.00953	-0.00591	-0.00873	-0.00605
	(0.00714)	(0.00712)	(0.00704)	(0.00677)	(0.00637)	(0.00674)	(0.00667)
Zero-Covid * promotion	-0.0484		0.988		0.276		0.244
	(0.0553)		(0.811)		(0.253)		(0.161)
Zero-Covid * gdp per capita		-0.151***	-0.101**				
		(0.0413)	(0.0401)				
Zero-Covid * promotion * gdp per capita			-0.0934				
			(0.0757)				
Zero-Covid * share of service sector				-1.719***	-1.441***		
				(0.318)	(0.235)		
Zero-Covid * promotion * share of service sector					-0.664		
					(0.540)		
Zero-Covid * urbanization ratio						-0.923***	-0.695***
						(0.189)	(0.122)
Zero-Covid * promotion * urbanization ratio							-0.445
							(0.288)
Observations	63,294	63,294	63,294	63,294	63,294	63,294	63,294
R^2	0.927	0.929	0.929	0.934	0.934	0.932	0.932
Prefecture FEs	YES	YES	YES	YES	YES	YES	YES
Prov-Month FEs	YES	YES	YES	YES	YES	YES	YES
Clustered Standard Error	Prefecture	Prefecture	Prefecture	Prefecture	Prefecture	Prefecture	Prefecture

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Chapter 4

Do Autocrats Break Their Promises? A Principal-Agent Problem with Limited Commitment

4.1 Introduction

The principal-agent problem under limited commitment, involving a powerful principal (such as an autocrat, the central government or employer) delegating tasks to an agent (such as a bureaucrat or employee) whose interests may not align with those of the principal. This misalignment often leads to inefficiencies and requires the design of institution to ensure that the agent acts in the principal's best interest. The complexities were exacerbated by limited commitment, where neither party can fully commit to future actions, leading to potential conflicts and suboptimal outcomes.

This research focuses on the principal-agent problem when both principal and agent are constrained by limited commitment. We parameterize the commitment environment by two factors: the probability of potential contract default and the cost of contract default. Contrary to the conventional wisdom that a lack of commitment (high chance and low cost for contract default) would harm the parties' benefits in contracting, we find that the principal could obtain positive marginal benefits with a higher probability of contract breach, particularly when the costs associated with violating the contract are relatively low. The driving force behind this unexpected result is that the potential threat of contract breach can act as a screening tool, influencing the agent's reporting strategy and leading to a more efficient payment scheme for the principal in equilibrium.

Our study makes contributions to three main branches of literature. First, our work is related to the limited commitment principal-agent problem, particularly in political and economic contexts. Previous studies (Greif, 1993; Myerson, 2015; Acemoglu and Robinson, 2000; Acemoglu, 2003) have highlighted how limited commitment shapes economic institutions and governance. Our research builds on this foundation by parameterizing the commitment environment using the probability of the agent breaking the promise and the cost associated with a costly audit. We find that the principal can obtain positive marginal benefits from a higher probability of breaking the promise given to the agent, particularly when the costs of a costly audit are relatively low. The potential threat of breaking the promise acts as a screening tool, influencing the agent's reporting strategy and leading to a more efficient payment scheme for the principal. The parametric setting also reveals the dynamics of the principal and the agent's benefits as the factors of commitment problem vary.

The political economy of autocratic regimes presents unique challenges in governance and administrative efficiency. Olson (1993), Tullock (1987), Egorov and Sonin (2011) and many other seminal works have discussed the dynamics of dictatorship and the difficulties autocrats face in maintaining power and extracting resources. Our research contributes to this literature by analyzing the principal-agent problem that mirrors the context of autocratic governance, where limited commitment and costly verification play crucial roles. We demonstrate that autocratic rulers can use the threat of contract breach and the strategic allocation of verification resources to create efficient incentive structures, even in environments where credible commitment is lacking. However, when the commitment problem is extremely serious, the autocrat has to play the strategy that does not involve any verification to prevent paying for the extra premium that is required to incentivize the agent. This result is similar to the work by Ma and Rubin (2019), who explore the paradox of power in Imperial China, where the lack of credible commitment leads to a low wage-low tax equilibrium. This approach provides new insights that under what environment and mechanism that autocrats can maintain control and ensure administrative efficiency.

The role of costly verification in contract design, delegation problem and mechanism design has been extensively studied. Literature (Townsend, 1979; Halac and Yared ,2020; Ben-Porath, Dekel, and Lipman, 2014) have explored how verification costs shape the structure of optimal contracts. In our model, we incorporate costly verification into a principal-agent with limited commitment framework, examining how the threat of breaking the promise and the associated costly audits influence the principal's strategy. Our findings suggest that the potential for breaking the promise, coupled with low verification costs, can improve the principal's ability to design efficient contracts by acting as a screening mechanism that incentivizes the agent to report accurately.

The rest of the chapter is organized as below. Section 2 introduces the model setup. Section 3 displays the strategy of the principal and the agent. Section 4 discusses the equilibrium results. Section 5 presents the numerical example. Section 6 concludes.

4.2 Model Setup

4.2.1 Preferences and Setup

We consider a one-period principal-agent problem with limited commitment. A principal owns a project which could potentially generates a income of τ conditional on its success, and nothing if it fails. Whether project is successful is solely determined by a binary status of effort: if there is an input of effort, the project will be a success, otherwise, it fails. The cost of effort to the principal is significantly higher than the income, while the principal could hire an agent who is skilled to the project to work on the project.

The cost of the project for the agent c is drawn from a distribution determined by the state. The agent may face one of a finitie number of possible states, $\theta \in \Theta = \{\theta_1, ..., \theta_n\}$, and we denote the distribution for the cost of the project derived from state θ by $F(\cdot, \theta)$ and its density form $f(\cdot, \theta)$. We assume $F(\cdot, \cdot)$ has common support over $[\underline{c}, \bar{c}]$ for all $\theta \in \Theta$, and $\bar{c} \leq \tau$ so that agent is always efficient in implementing the project. We further assume

$F(\cdot, \cdot)$ follows the Monotone Likelihood Ratio Property (MLRP) such that for any $k < j$, $x < y$, we have

$$\frac{f(x, \theta_j)}{f(x, \theta_k)} \leq \frac{f(y, \theta_j)}{f(y, \theta_k)}$$

so that a "higher" state θ_j represents a higher chance to draw a higher project cost than a "lower" state θ_k. The prior distribution for the states is a common knowledge, denote it as $\pi = (\pi_1, ..., \pi_n) \in \mathcal{P}(\Theta)$, $s.t. \sum_{i=1}^{n} \pi_i = 1$, where $\mathcal{P}(\Theta)$ represents the set of all probability distributions on Θ.

The principal hires the agent and promises a reward $r \in \mathbb{R}^+$ after the project turns out to be a success. Thus the principal will have a payoff of $\tau - r$ conditional on a success, and 0 if project fails. The agent will have a payoff of $r - c$ conditional on a success, and $-c$ if project fails. Thus, we are assuming both the principal and the agent are risk neutral. The agent is also endowed with an outside option that could generate a small but position payoff $\epsilon > 0$.

We assume that the agent could not commit to the effort input which is unobserved by the principal, so the effort input is not a contractible term. And the principal could not fully commit to the payment of the reward after the project outcome is realized either, and we assume that the principal could break the contract at his or her will by paying a "audit" cost κ^*. We consider this as an "auditing" attempt to extract any remaining surplus from the agent, that is $\tau - c$. Moreover, we have κ^* is drawn from a distribution with δ probability to be a finite value κ and $1 - \delta$ probability to be $+\infty$. In other words, there is an exogenous chance of δ that the principal finds breaking the contract is feasible. The audit decision is denoted by $q \in \{0, 1\}$ where 0 stands for "no auditing" and 1 represents "auditing".

4.2.2 Timing and Information

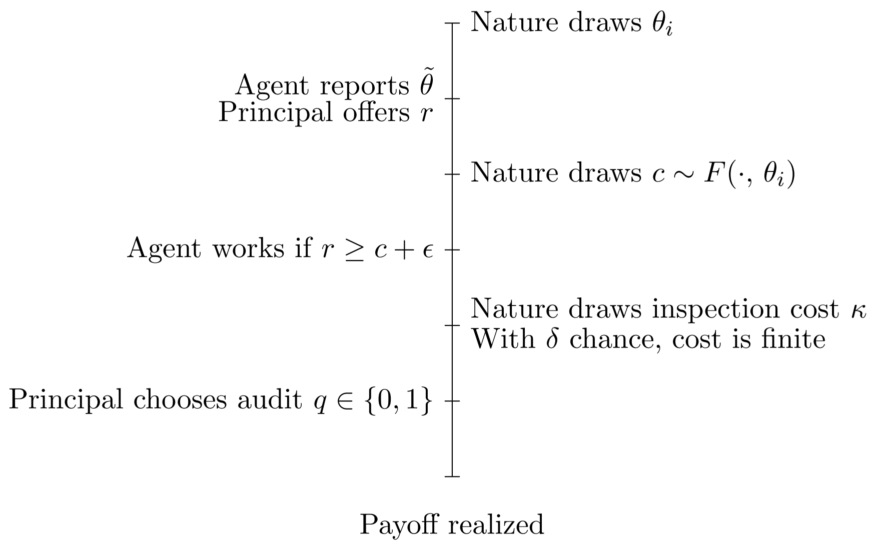
In this section, we consider the sequence of the game and the information set for the principal and the agent at every possible decision node.

First, there is a contract stage. The agent observes the state $\theta_i \in \Theta$ which is a private information. The agent makes a report $\tilde{\theta} \in \Theta$ to the principal, and then the principal promises a reward r to the agent, which will be realized only if the project delivers a success.

Next, in the action stage, the nature draws the cost of the project c from the distribution $F(c, \theta_i)$, and the cost c is directly revealed to the agent. The agent then decides whether to input an effort into the project. The project outcome is realized according to the agent's decision. It is obvious that the agent will only input an effort if $r \geq c + \epsilon$. It is noteworthy that there exists two levels of information asymmetry in the contract stage and the action stage across the principal and the agent. In the first stage, the agent is better informed by having a signal of θ_i to better infer the distribution of c, while in the second stage, c is directly revealed to the agent. We justify this setting by allowing the agent to learn about the project cost after the project was initiated. And then the agent makes the action decision by comparing the known cost and the potential reward.

Third, an audit stage. The project outcome is revealed to the principal. Conditional on the success of the project, with δ probability, the audit decision is nontrivial to

Figure 4.1: Timeline of the game.



the principal when the cost of auditing is finite. Then the principal could make a decision to audit or not, q, knowing that the project is successful.

Finally, the payoffs are realized for the principal and the agent. Given a successful project, the principal will have a payoff of $\tau - r$ if there is no auditing, and $\tau - c - \kappa$ if an audit happened; the agent will have a payoff of $r - c$ without being audited, and 0 if audited. If the project fails, both the principal and the agent will have 0 payoff.

Figure 4.1 summarizes the stages and presents the timeline of the game.

4.3 Strategy

In the game, the agent makes two decisions, the reporting scheme in the contract stage and the effort input in the action stage. We have stated that the agent will choose to invest efforts only if $r \geq c + \epsilon$, so the action decision is trivial. We only focus on the strategy of the agent's reporting. For a agent who observes the state θ_i, we denote his or her reporting strategy as $\mu_i : \Theta \to \mathcal{P}(\Theta)$. In other words, the agent could choose a mixed strategy by randomly reporting among possible states $\tilde{\theta}$. Denote the agent's strategy $\{\mu_i\}_{i=1,\ldots,n}$ by μ. We further assume that the agent chooses a symmetric strategy: for $i \neq j$, if $supp(\mu_i) = supp(\mu_j)$, then we have $\mu_i(\tilde{\theta}) = \mu_j(\tilde{\theta})$ for all $\tilde{\theta}$. That is, if the agent facing different states find a same set of states could yield equally optimal payoffs, the agent will choose the same mixing scheme over the set of states in the reporting strategy.

The principal makes two decisions, the promised reward in the contract stage and the audit decision in the audit stage. The principal could only rely on the report provided by the agent to make decisions. So the principal's strategy is represented by $\sigma = (r, q) : \Theta \to \mathbb{R}^+ \times \{0, 1\}$, and the principal updates the belief after observing the reports and a conjectured agent's strategy μ by the Bayes' Rule:

$$\phi_k(\tilde{\theta}) = \frac{\pi_k \, \mu_k(\tilde{\theta})}{\sum_{j=1}^{n} \pi_j \, \mu_j(\tilde{\theta})} \, .$$

Given the principal's strategy σ, the agent's problem after observing state θ_i could be represented as:

$$\begin{aligned}
\max_{\mu(\cdot)} U(\mu, r, q; \theta_i) &= \sum_{j=1}^{n} \mu(\tilde{\theta}_j)(1 - \delta q_j) \int_{r_j \geq c+\epsilon} (r_j - c) f(c, \theta_i) \, dc \\
&= \sum_{j \in q^{-1}(\{0\})} \mu(\tilde{\theta}_j) \int_{r_j \geq c+\epsilon} (r_j - c) f(c, \theta_i) \, dc \\
&+ \sum_{j \in q^{-1}(\{1\})} \mu(\tilde{\theta}_j)(1 - \delta) \int_{r_j \geq c+\epsilon} (r_j - c) f(c, \theta_i) \, dc
\end{aligned}$$

The agent takes the expectation over the net payoff on the range of the cost realization that will ensure a project success, and then weight it by the probability of each state that is mixing in the report scheme. We decompose the agent's value function to explicitly show that the agent will discount the expected payoff by $(1-\delta)$ for the range of reports that the principal will audit ($q^{-1}(\{1\})$). To simplify the notation, we define the integral term in the agent's value function as below:

$$I_k(r) = \int_{\underline{c}}^{r-\epsilon} (r-c) f(c,\, \theta_k)\, dc\,.$$

Given the agent's strategy, the principal maximizes expected utility with sequential rationality constraints

$$\max_{\{r_j, q_j\}_{j=1,..,n}} \sum_{i=1}^{n} \sum_{j=1}^{n} \pi_i \mu_i(\tilde{\theta}_j) V(\mu, r_j, q_j; \tilde{\theta}_j)$$

where

$$\begin{aligned} V(\mu, r_j, q_j; \tilde{\theta}_j) &= \sum_{k=1}^{n} \phi_k(\tilde{\theta}_j) \Big[(1-\delta q_j) \int_{r_j \geq c+\epsilon} (\tau - r_j) f(c, \theta_k)\, dc \\ &\quad + \delta q_j \int_{r_j \geq c+\epsilon} (\tau - \hat{c}_k(r_j) - \kappa) f(c, \theta_k)\, dc \Big] \\ \hat{c}_k(r) &= \int c\, f_{c \leq r}(c, \theta_k)\, dc \\ q_j &= \arg\max_{q} \{\tau - r_j, \tau - \hat{c}_k(r_j) - \kappa\}\,. \end{aligned}$$

The principal considers the posterior probability of true state being θ_k given the agent's report $\tilde{\theta}_j$ and forms the expected utility. r_j represents the promised reward receiving report $\tilde{\theta}_j$, q_j represents the proposed audit decision, $\hat{c}_k(r)$ represents the expected cost of the project conditional on a success given a reward of r under state θ_k, and the last equality constraint is the sequential rationality constraint for the principal, which requires the principal to have the correct incentive to make the audit decision consistent to the proposed strategy.

We define q is a cut-off audit strategy if there exists $0 \leq \bar{k} \leq n$ such that for $i \in \{1, ..., n\}$, we have

$$q_i = \begin{cases} 1\,, \text{for } i > \bar{k} \\ \\ 0\,, \text{for } i \leq \bar{k} \end{cases}$$

and define r is a type-monotone reward strategy if we have

$$r_1 \leq ... \leq r_i \leq ... \leq r_n\,.$$

For the solution concept, we consider the Perfect Bayesian Equilibrium (PBE) and a strategy profile (σ, μ) is an equilibrium if it solves the agent and the principal's problems stated above. We are particularly interested in the subset of the equilibria which has a type-monotone reward strategy. This is an natural restriction as a higher state represents a higher chance of a higher cost faced by the agent, thus requires the principal to provide stronger incentives. Note that this restriction is not necessary for the equilibrium to sustain, but only helps us to focus on the set of the equilibria that we are more interested in.

4.4 Equilibrium Results

Our first result states the form of the agent's optimal response μ to the principal's strategy σ. We first define the following mapping $\hat{r}_k(\cdot) : \mathbb{R}^+ \to \mathbb{R}^+$ such that

$$I_k(r) = (1-\delta)I_k(\hat{r}_k) \Rightarrow \hat{r}_k(r) = I_k^{-1}\left(\frac{I_k(r)}{1-\delta}\right)$$

Since $I_k(\cdot)$ is a continuous and increasing function, the mapping is well defined and we have $\hat{r}_k(r) > r$. Essentially, $\hat{r}_k(\cdot)$ pins down the equality of the two terms in the agent's value function, and maps from any reward promised by the principal with no potential audit, to the desired reward amount to yield same expected payoff under the scenario that an audit is proposed. Then we could have the following proposition regarding the agent's decision rule:

Proposition 1 *For agent type θ_i, facing a type-monotone reward strategy and a non-trivial audit strategy ($q_j = 1$ for at least one reported state $\tilde{\theta}_j$) by the principal, the agent will choose to report:*

$$\begin{cases} \mu\left(\left\{\tilde{\theta}_i \,|\, r_i \in \max\{r_j \,|\, j \in q^{-1}(\{0\})\}\right\}\right) = 1 & \textit{if } r_{k_1} < \hat{r}_k(r_{k_0}) \\ \mu\left(\left\{\tilde{\theta}_i \,|\, r_i \in \max\{r_j \,|\, j \in q^{-1}(\{1\})\}\right\}\right) = 1 & \textit{if } r_{k_1} > \hat{r}_k(r_{k_0}) \\ \mu\left(\left\{\tilde{\theta}_i \,|\, r_i \in \left\{\max\{r_j \,|\, j \in q^{-1}(\{0\})\} \cup \max\{r_j \,|\, j \in q^{-1}(\{1\})\}\right\}\right\}\right) = 1 & \textit{if } r_{k_1} = \hat{r}_k(r_{k_0}) \end{cases}$$

where $k_0 = \max\left\{i \in q^{-1}(\{0\})\right\}$ *and* $k_1 = \max\left\{i \in q^{-1}(\{1\})\right\}$.

Proof. From the agent's value function, we have $I_k(r)$ an increasing function in r, which implies the agent has a dominant strategy to report at the state that promises the highest reward, conditional on the audit strategy. The audit strategy creates two subsets in the state space, namely $q^{-1}(\{0\})$ and $q^{-1}(\{1\})$, and the candidate of states to report could be only from the highest state in each subset, due to the type-monotone reward strategy we are considering here. Finally, we consider the comparison between the expected payoff of the agent in these two scenarios to pin down the optimal reporting scheme. ■

Next, we have the second result to show that we could limit our focus to the equilibrium with a cut-off audit strategy:

Proposition 2 *There exists an equilibrium with a cut-off strategy that could result in the outcome derived from any equilibrium.*

Proof. The arbitrary audit strategy q generates $q^{-1}(\{0\})$ and $q^{-1}(\{1\})$. Following proposition 1, we could find k_0 and k_1. There are only two scenarios regarding the order of k_0 and k_1.

Suppose $k_0 < k_1$, then the same equilibrium outcome could be achieved by a cut-off audit strategy by setting $\bar{k} = k_0$, in this case $k_1 = n$. The equilibrium outcome is either the agent reports at $\tilde{\theta}_{k_0}$ and other states with same reward, the principal proposes a reward r_{k_0} and $q = 0$, or the agent reports at $\tilde{\theta}_n$ and other states with same reward, the principal proposes a reward r_n and $q = 1$, which depends on the comparison of r_{k_n} and $\hat{r}_k(r_{k_0})$.

Suppose $k_0 > k_1$, by proposition 1, agents will always choose to report at θ_{k_0} or its equivalent states as we have $\hat{r}_k(r_{k_0}) > r_{k_0} > k_1$. To have this equilibrium outcome, we could set the cut-off strategy with $\bar{k} = n$ so that the principal proposes a contract that rules out the possibility of auditing. This will result in the same equilibrium outcome as the original equilibrium. ■

Now combining proposition 1 and 2, we could rewrite the principal's strategy as $(r_0, r_1, \bar{k})$ such that

$$q(\tilde{\theta}_i) = \mathbb{I}(i > \bar{k});\ (\tilde{\theta}_i) = \begin{cases} r_0 \text{ for } i \leq \bar{k} \\ \\ r_1 \text{ for } i > \bar{k} \end{cases}$$

and also the principal's optimization problem could be represented as:

$$\max V_k \in \{V_0, V_1, ...V_n\}$$

where

$$V_k = \max_{r_0,r_1} \sum_{i=1}^{k} \pi_i \int_{r_0 \geq c+\epsilon} (\tau - r_0) f(c,\theta_i)\, dc +$$

$$\sum_{i=k+1}^{n} \pi_i \left[(1-\delta) \int_{r_1 \geq c+\epsilon} (\tau - r_1) f(c,\theta_i)\, dc + \delta \int_{r_1 \geq c+\epsilon} (\tau - \hat{c}_i(r_1) - \kappa) f(c,\theta_i)\, dc \right]$$

with following constraints

$$r_1 \in [\hat{r}_{\bar{k}+1}(r_0), \hat{r}_{\bar{k}}(r_0)] \tag{Agent IC}$$

$$\mathbb{E}_{\theta_i}\left[U(\hat{\mu}, r, q; \theta_i)\right] \geq \epsilon \tag{Agent IR}$$

$$r_0 \leq \kappa + \sum_{i=1}^{\bar{k}} \pi_i \hat{c}_i(r_0) \tag{Principal IC-No Audit}$$

$$r_1 \geq \kappa + \sum_{i=\bar{k}+1}^{n} \pi_i \hat{c}_i(r_1) \tag{Principal IC-Audit}$$

In the simplified principle's problem, we are searching over all potential thresholds for the cut-off audit strategy, while imposing constraints that ensure the incentive compatibility for both the principal and the agent, and then maximize the principal's expected payoff by controlling the two reward levels within the constrained ranges. We are imposing that the agent will follow the proposed contract by the principal, in the sense that if the agent's type is below the threshold, the agent to choose to take the reward r_0, otherwise the agent will take the reward r_1 even it is accompany with a potential audit.

The agent's incentive constraint concerns whether the agent will deviate to the opposite reward scheme, e.g. from $(r_0, q=0)$ to $(r_1, q=1)$, or vice versa. We could show that $\hat{r}_{i+1}(r_0) > \hat{r}_i(r_0)$ for every i, thus we only need to consider the two types across the threshold of the boundary, that is $\theta_{\bar{k}}$ and $\theta_{\bar{k}+1}$. We restrict $r_1 \leq \hat{r}_{\bar{k}}(r_0)$ which prevents the agent of type $\theta_{\bar{k}}$ from deviating, and $r_1 \geq \hat{r}_{\bar{k}+1}(r_0)$ ensures the incentive compatibility for the type $\theta_{\bar{k}+1}$.

The agent's individual rationality constraint ensures that the agent has an ex-ante expected payoff more than the outside option. And the last two principal's incentive constraints state that the principal should indeed follow the proposed audit decision in the ranges below and above the cut-off threshold.

There are several remarks:

(1) In the principal's value function V_k given the cutoff audit threshold at k, we are aggregating the prior distributions π_i of each state θ_i to formulate the expectation in the ranges below or above the threshold. This is feasible as the proposed reward is a flat value in these two ranges, so that by the agent's symmetric reporting strategy, any reported state within the range is uninformational and the principal could only rely on the prior distribution.

(2) It is possible that there is no feasible solution to the maximization problem of V_k as the constraints are too tight and resulting in no feasible ranges. However, there is a last resort for the equilibrium to exist at V_n, which represents that the principal's strategy involves no audit. This is always feasible by paying a low flat wage for all reported states.

(3) Agent's incentive constraint might be conflicting when $\hat{r}_{\bar{k}+1}(r_0) > \hat{r}_{\bar{k}}(r_0)$, which implies there are no feasible reward scheme to provide incentives to persuade the agent not to deviate to the other types' reward scheme. While we allow this situation to happen in general, we could show that under some further normality assumption on the cost distri-

bution, we could have $\hat{r}_i(r_0) > \hat{r}_j(r_0)$ for every $i > j$, thus the reward scheme to separate between any two types is feasible.

Now we have our main result:

Proposition 3 *The solution to the principal's simplified problem* $(r_0, r_1, \bar{k})$*, together with the induced agent's reporting scheme, such that the agent will choose to report randomly over* $\{\tilde{\theta}_1, ..., \tilde{\theta}_{\bar{k}}\}$ *if* $\theta_i \in \{\theta_1, ..., \theta_{\bar{k}}\}$*; and report randomly over* $\{\tilde{\theta}_{\bar{k}+1}, ..., \tilde{\theta}_n\}$ *if* $\theta_i \in \{\theta_{\bar{k}+1}, ..., \theta_n\}$*, is a PBE.*

Proof. It could be verified that the belief induced by the agent's strategy is consistent with the principal's belief in the optimization problem. The principal's strategy satisfies the sequential rationality which is imposed in the problem. And the principal's strategy and the agent's strategy are the optimal responses to each other. ■

4.4.1 Discussion

We could compare the result in last section with the benchmark when the principal has no commitment problem in the environment. In other words, the principal could commit to a reward scheme ad-hoc. In this scenario, as the agent has a dominant strategy to always choose to report at the state related to the highest reward regardless of the state, there will be a pooling equilibrium that any report is uninformational to the principal, and the principal has to make the decision based on the prior belief.

On the other hand, the result we have displayed in the limited commitment environment shows that the principal could utilize the potential audit to separate the agents into two groups. There is a tradeoff in this new feature of the equilibrium: on one side, the principal could be better off due to the improved efficiency from the information gain of the screening results; on the other side, the principal has to pay a premium of risk to the higher type group to ensure the incentive compatibility. Moreover, the principal is still limited by his or her own incentive compatibility constraints.

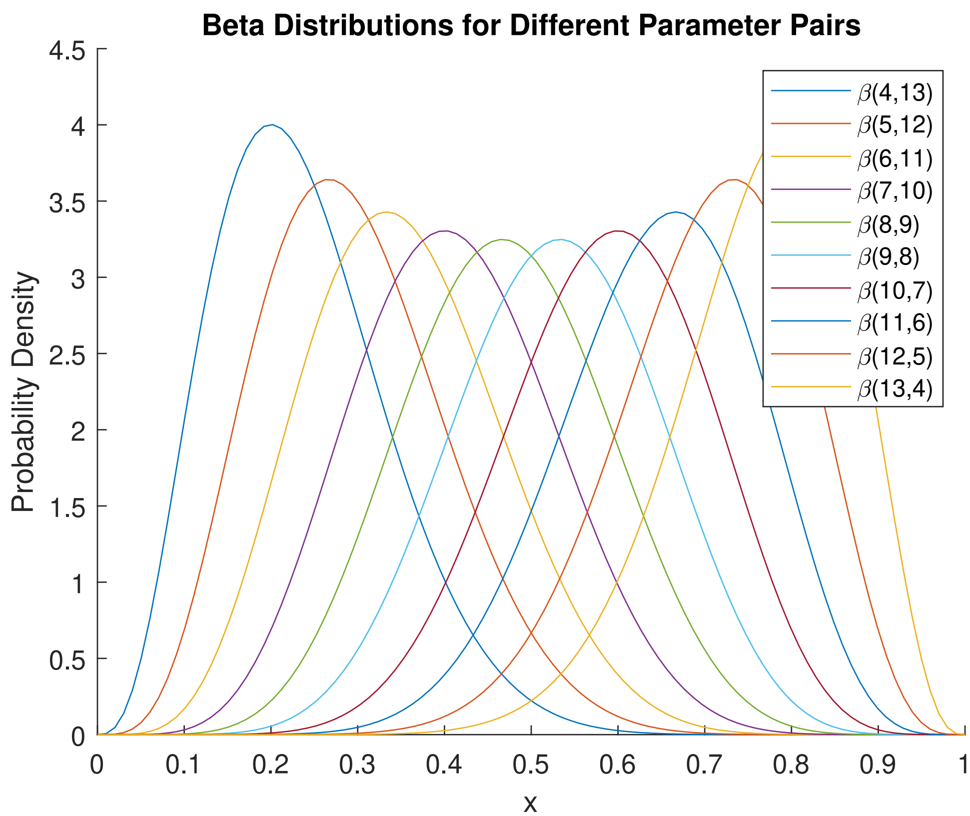
Overall, whether the principal (and the agent) could benefit from the exercise of the audit is ambiguous. Due to the complexity of the analysis of the non-linear optimization problem, we present a numerical analysis in the next section.

4.5 Numerical Analysis

In this section, we solve the principal's simplified problem numerically, and discuss the analysis of comparative statics. We consider the state θ_i to be drawn from $\Theta = \{\theta_1, ..., \theta_{10}\}$, with equally likely probability. The cost distribution follows a family of beta distributions, and as i increases, the density distribution assigns more weight from left to the right. Figure 4.2 displays the family of beta distributions that we are considering in this example.

We consider $\tau = 1$, $\epsilon = 0.01$, $[\underline{c}, \bar{c}] = [0, 1]$, $\kappa \in \{0.35, 0.4, 0.43\}$ and $\delta \in (0.01, 0.99)$. The results are presented in Figure 4.3. We would like to display the dynamics of the equilibrium in both dimensions of δ and κ. Thus, in each of the panel, we solve for the equilibrium (ex-ante) payoffs for the principal and the agent, as well as the cut-off threshold of the audit strategy, for the range of δ with a step of 0.01. In the three panels of Figure 4.3, we use value of κ to be 0.35, 0.4 and 0.43, respectively.

Figure 4.2: Family of Beta Distribution

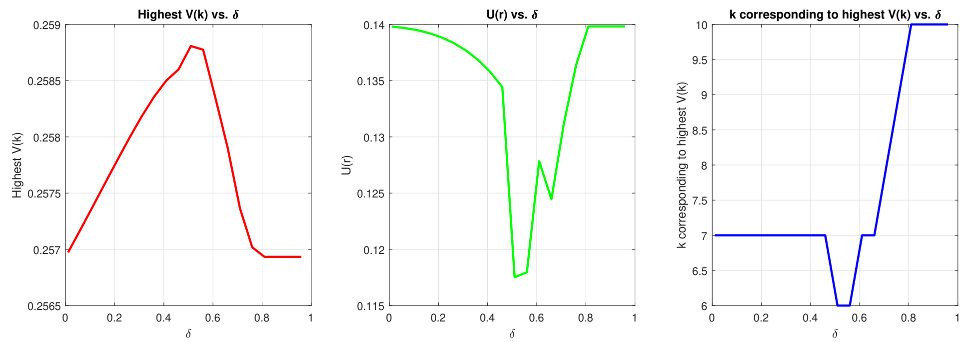


One interesting finding is that the principal's payoff exhibits an non-monotone relation as δ increases, when κ is relatively low. This implies when δ level is intermediate, the efficiency improvement effect from the screening of the agents dominates the increased cost of the reward payment. However, the marginal benefits decreases as δ keeps increasing and the principal has to promise more reward to satisfy the agent's incentive constraints.

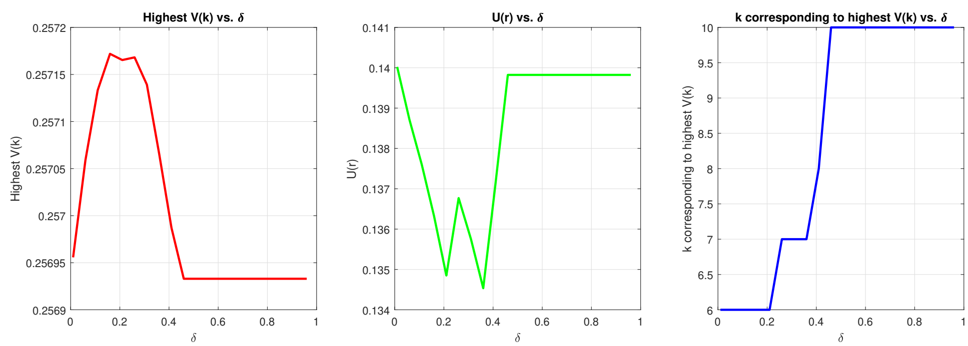
When δ is close to 1, that is when the commitment problem is very serious, the principal found it not beneficial to use its power anymore, and resulting in the non-audit strategy with threshold $\bar{k} = 10$, as shown in the figure. This implies the payoffs generated from strategies involving auditing is bringing negative marginal benefits so the principal resorts back to the equilibrium without auditing.

We could also find that the agent's utility generally decreases with δ, except at the point where the audit strategy changes. This is natural as when there exists some possibility of auditing, a higher chance of auditing will decrease the agent's payoffs. When the auditing threshold becomes less tight, that is when $\bar{k}$ increases, the agent will benefit from the less scenarios that will be audited. When strategies with non-audit are in place, the agent receives the most payoffs.

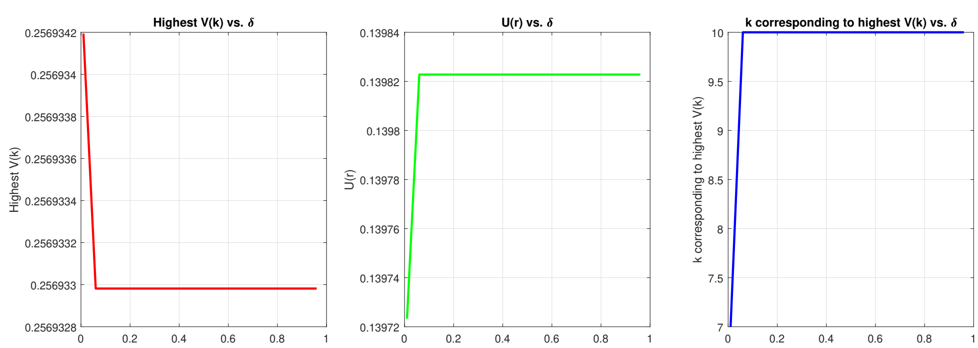
Figure 4.3: Optimized Values in the Equilibrium



(a) $\kappa = 0.35$, $\tau = 1$, $\epsilon = 0.01$, $[\underline{c}, \bar{c}] = [0, 1]$



(b) $\kappa = 0.4$, $\tau = 1$, $\epsilon = 0.01$, $[\underline{c}, \bar{c}] = [0, 1]$



(c) $\kappa = 0.43$, $\tau = 1$, $\epsilon = 0.01$, $[\underline{c}, \bar{c}] = [0, 1]$

4.6 Conclusion

This research examines the principal-agent problem under limited commitment, focusing on the interaction between a principal and an agent constrained by their inability to fully commit to future actions. By parameterizing the commitment environment through the probability of contract default and the associated costs, we provide an counterexample to the conventional wisdom that limited commitment generally harms contracting parties. Our findings reveal that a higher probability of contract breach, particularly when the costs of violation are low, can provide marginal benefits to the principal because the threat of contract breach acts as a screening tool, enhancing the efficiency of the payment scheme. This study extends to the political economy of autocratic regimes, demonstrating how autocratic rulers can use the threat of contract breach and strategic allocation of verification resources to create efficient incentive structures and maintain administrative efficiency, even in environments where credible commitment is lacking. These insights offer valuable implications for understanding the dynamics of governance and policy implementation, especially in autocratic settings, highlighting the nuanced strategies that can be employed to address the challenges posed by limited commitment.

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Appendix A

Appendix for Chapter 2

A.1 China's COVID Risk Level Dataset

In order to comply with the Prevention Guidance for Novel Coronavirus Pneumonia (version 5),[1] starting from March 2020, the State Council of China began to release a national COVID risk level system on a regular basis through their website. This system categorizes communities within the 2853 counties into high, medium, or low-risk groups on a daily basis. In specific, the risk level is reported by local governments and compiled by National Health Commission of China.

This website had two access interfaces. Interface A on the left column of Figure A.1 is a search engine that allows users to obtain communities' risk level results for a specific county by entering its name. Interface B, located in the right column, displays all counties

[1] Prevention Guidance for Novel Coronavirus Pneumonia (version 5): http://www.nhc.gov.cn/jkj/s3577/202002/a5d6f7b8c48c451c87dba14889b30147.shtml and a follow up guidance: http://www.gov.cn/zhengce/zhengceku/2020-04/16/content_5503261.htm

that have communities classified as *Risk* along with their corresponding community names. Counties that do not appear on this list are considered non-risk areas.[2]

We started risk level data collection through interface B since April 02, 2021 and ended by Dec 15, 2022.[3] [4] The China COVID Risk Level Dataset contains daily risk level information for 2853 counties from April 02, 2021 to December 15, 2022. This dataset is the most systematic compilation of China's risk level classification during 2021 and 2022.

Please visit this link to access the dataset.

[2]The web links for both pages have already expired. Interface A: `bmfw.www.gov.cn/yqfxdjcx/index.html` and Interface B: `bmfw.www.gov.cn/yqfxdjcx/risk.html`

[3]The weblink of interface B expired on Dec 15, 2022. But the weblink of interface A was still active until Dec 25, 2022, we collected the data between Dec 15 to Dec 25 through a third party website, `http://bj.bendibao.com/` but did not integrate the last 10 days data into our dataset yet.

[4]We thank open-source projects *BeautifulSoup* and *Selenium*.

A.2 Figures

Figure A.1: Demo of State Council's website for the Risk Level System.

(a) Interface A

(b) Interface B

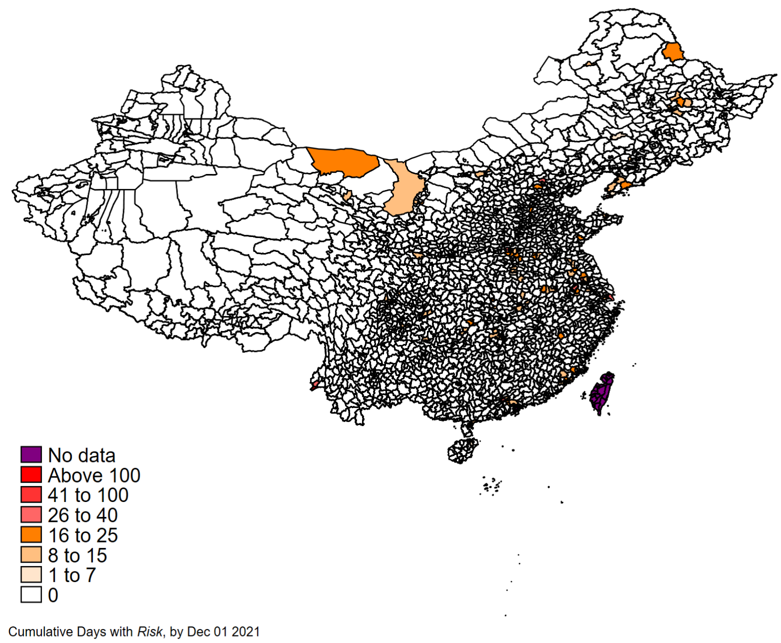
Notes: The web links for both pages have already expired.

Interface A: `bmfw.www.gov.cn/yqfxdjcx/index.html`

and Interface B: `bmfw.www.gov.cn/yqfxdjcx/risk.html`

Figure A.2: Geographical Distribution of counties with *Risk*

(a) *Risk* 2021



(b) *Risk* 2022

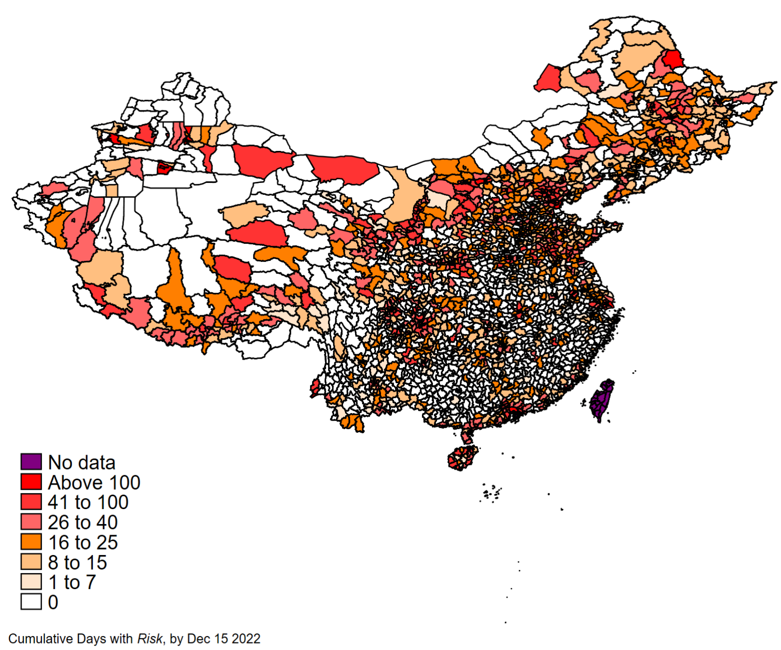


Figure A.3: Night Lights in March 2022

Note: This is the filtered data of Night Lights in March 2022 obtained from VIIRS, combine with the shapefile of China's county boundary.

A.3 Tables

Table A.1: Pollution Balanced Sample Regression Results

	(1) log PM2.5 2020	(2) log PM2.5 2021	(3) log PM2.5 2022
Lockdown	-0.162***		
	(0.00956)		
Risk		0.0842***	-0.0344***
		(0.0153)	(0.0101)
R-squared	0.870	0.773	0.749
Observations	42750	99750	145584
Mean of PM2.5 (Weekly Average)	31.52	25.83	26.93
Controls	✓	✓	✓
County FE	✓	✓	✓
Week FE	✓	✓	✓

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.2: Night Lights Balanced Sample Regressions Results

	(1) log NightLight 2020	(2) log NightLight 2021	(3) log NightLight 2022
Lockdown	-0.0669***		
	(0.00951)		
Risk		0.0777***	-0.137***
		(0.0249)	(0.0261)
R-squared	0.966	0.981	0.953
Observations	23790	12648	11000
Mean of Nightlight (Monthly)	2.385	2.083	1.844
Controls	✓	✓	✓
County FE	✓	✓	✓
Month FE	✓	✓	✓

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix B

Appendix for Chapter 3

B.1 Regression Discontinuity Approach

To identify a casual effect of promotion incentives on the implementation of the zero-Covid policy, we also employ the regression discontinuity design (Imbens and Lemieux, 2008; Lee and Card, 2008) to verify the discontinuity that lies in the ineligible age of promotion of the prefecture leaders (i.e., 58). The following local regression was estimated for the subsample of party secretaries aged close to 58 years:

$$\begin{aligned} ZeroCovid_{it} = {} & \beta_1 Cases_{it} + \beta_2 Cases_{it} \times I(Year \leq 57)_{it} + \beta_3 Cases_{it} \times DAGE_{it} \\ & + \beta_4 Cases_{it} \times DAGE_{it} \times I(Year \leq 57)_{it} + \gamma Cases_{it} \times X_{it} + \mu_i + \theta_t + \varepsilon_{it} \end{aligned} \tag{A1}$$

where $DAGE_{it}$ is a running variable for the age of the party secretary of prefecture i at date t, specifically calculated by $DAGE =$ age of secretary-58; $I(Year \leq 57)_{it}$ is a dummy of secretary's age less than or equal to 57 years, and other notation remains identical to that of equation (A1). β_2 identifies the impact of promotion incentives on the prefecture leader's zero-Covid policy decision at the promotion eligible age cutoff point. We expect β_2 to be positive and significant if the age restriction creates a discontinuity in the promotion incentive and thus affects prefecture leaders' compliance with the zero-Covid policy.

In Table A.1, we present the results of a regression discontinuity approach based on equation (A1). In columns (1) - (3), the estimation is conducted using a subsample of party secretaries aged between 55 and 61; and in columns (4) - (6), we used a subsample of officials aged between 56 and 60. In columns (1) and (4), the coefficients of $Cases_{it} \times I(Year \leq 57)_{it}$ are 0.0125 and 0.0215 and significant at the level 0.01 and 0.1, respectively. These results imply a discontinuity in the marginal increase in the probability of zero-Covid policy brought by the confirmed cases at the promotion eligible age cutoff point of 58 years. In prefectures with party secretaries aged 57 or younger, the probability of implementing the zero-Covid policy is approximately 2% higher for every 7-day average daily case compared to prefectures with older party secretaries. Using our previous hypothetical scenario in Section 5.1, this implies a 20% lower probability of implementing the zero-Covid policy when facing a pandemic outbreak with 70 cases over the past 7 days in prefectures with elder party secretaries.

In columns (2)(3)(5)(6), we add include interaction terms between 7-day average case and control variables and party secretary fixed effects. The regression results do not show substantial changes. Thus, we conclude that the estimation from our regression discontinuity approach is consistent with our baseline regression results in Section 5.1, giving credence to our overall findings.

B.2 Tables

Table B.1: Regression Discontinuity: Effect of Promotion Incentives on the Choice of Zero-Covid Policy

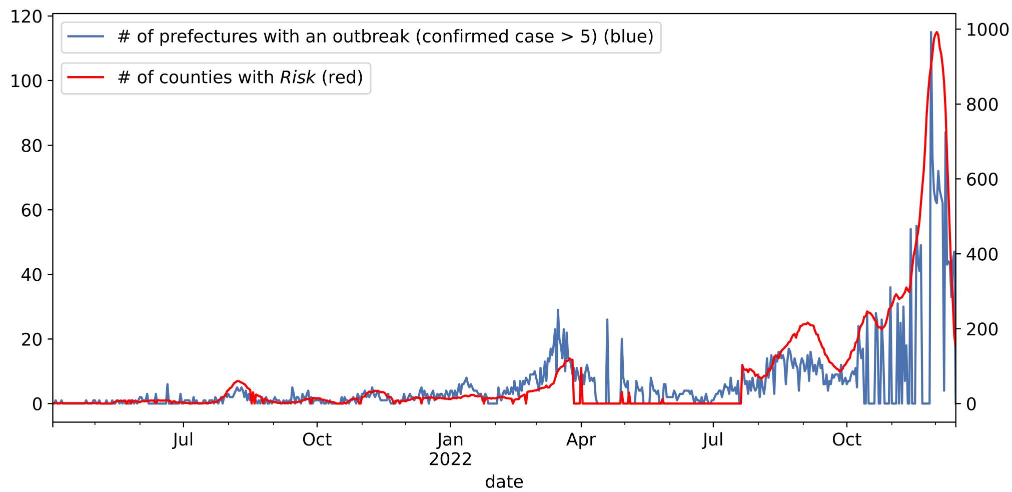
Dependent Variable: Zero-Covid Policy Status						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES						
7 day cases	0.0123*	0.0209**	0.0199**	0.0232***	0.0157	0.0153
	(0.00625)	(0.00904)	(0.00896)	(0.00769)	(0.0110)	(0.0110)
cases * Year_2022	-0.0118*	-0.00638*	-0.00626*	-0.0227***	-0.00458	-0.00511
	(0.00624)	(0.00335)	(0.00335)	(0.00769)	(0.0130)	(0.0129)
cases * I(age ¡= 57)	0.0125***	0.0185***	0.0189***	0.0215*	0.0244**	0.0243**
	(0.00422)	(0.00586)	(0.00604)	(0.0113)	(0.00941)	(0.00967)
cases * DAGE	0.135***	0.131***	0.133***	0.124***	0.131***	0.131***
	(0.0190)	(0.0183)	(0.0183)	(0.0188)	(0.0203)	(0.0197)
cases * DAGE * I(age ¡= 57)	-0.132***	-0.128***	-0.129***	-0.116***	-0.124***	-0.124***
	(0.0188)	(0.0183)	(0.0184)	(0.0175)	(0.0195)	(0.0192)
Observations	81,631	81,631	81,631	63,308	63,308	63,308
R^2	0.427	0.433	0.442	0.447	0.450	0.458
Prefecture FEs	YES	YES	YES	YES	YES	YES
Prov-Month FEs	YES	YES	YES	YES	YES	YES
Cases * Control	NO	YES	YES	NO	YES	YES
Secretary FEs	NO	NO	YES	NO	NO	YES
Clustered SE	Prefecture	Prefecture	Prefecture	Prefecture	Prefecture	Prefecture
Age Range	54-61	54-61	54-61	55-60	55-60	55-60

Clustered standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

B.3 Figures

Figure B.1: Timeline of Prefectures with Emerging COVID-19 Cases and Counties with Zero-Covid Policy



Note: The left y-axis represents the number of prefectures with daily confirmed COVID-19 cases exceeding five, while the right y-axis indicates the number of counties where the zero-COVID policy is in effect.

Figure B.2: Promotion Probability of Mayors (Zhou and Zeng, 2018)

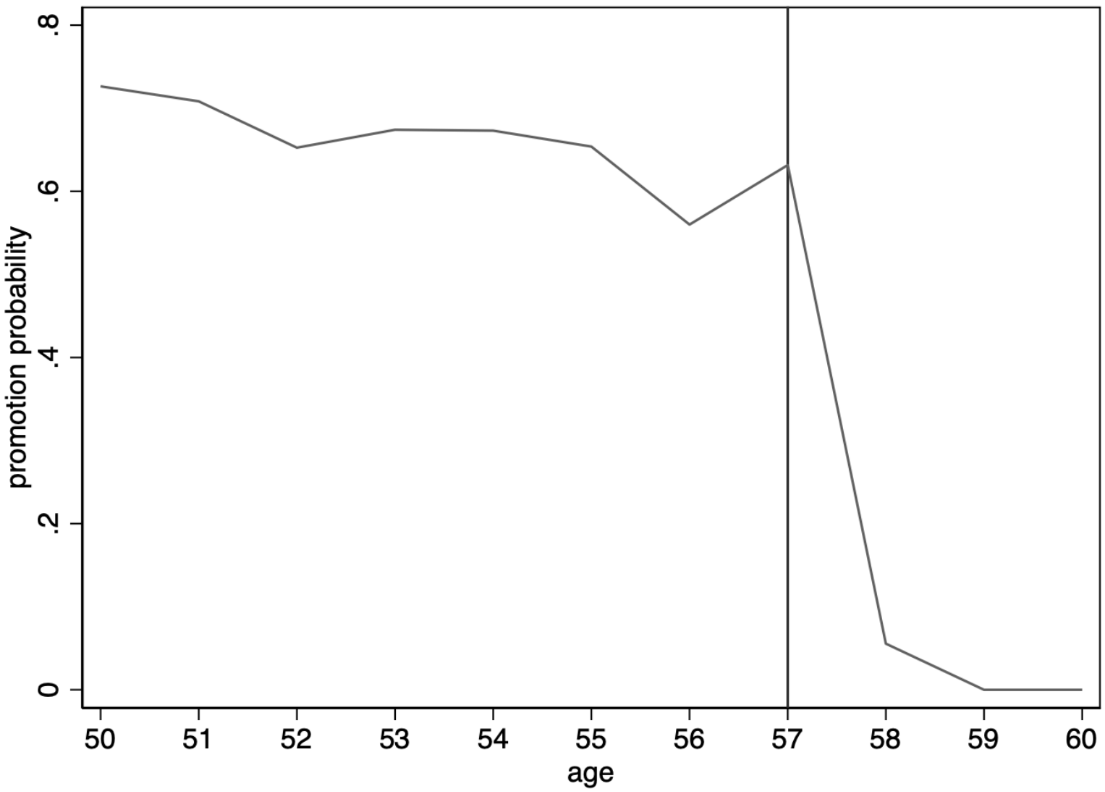


Figure B.3: Distribution of Outbreak Duration and Cumulative Case Number

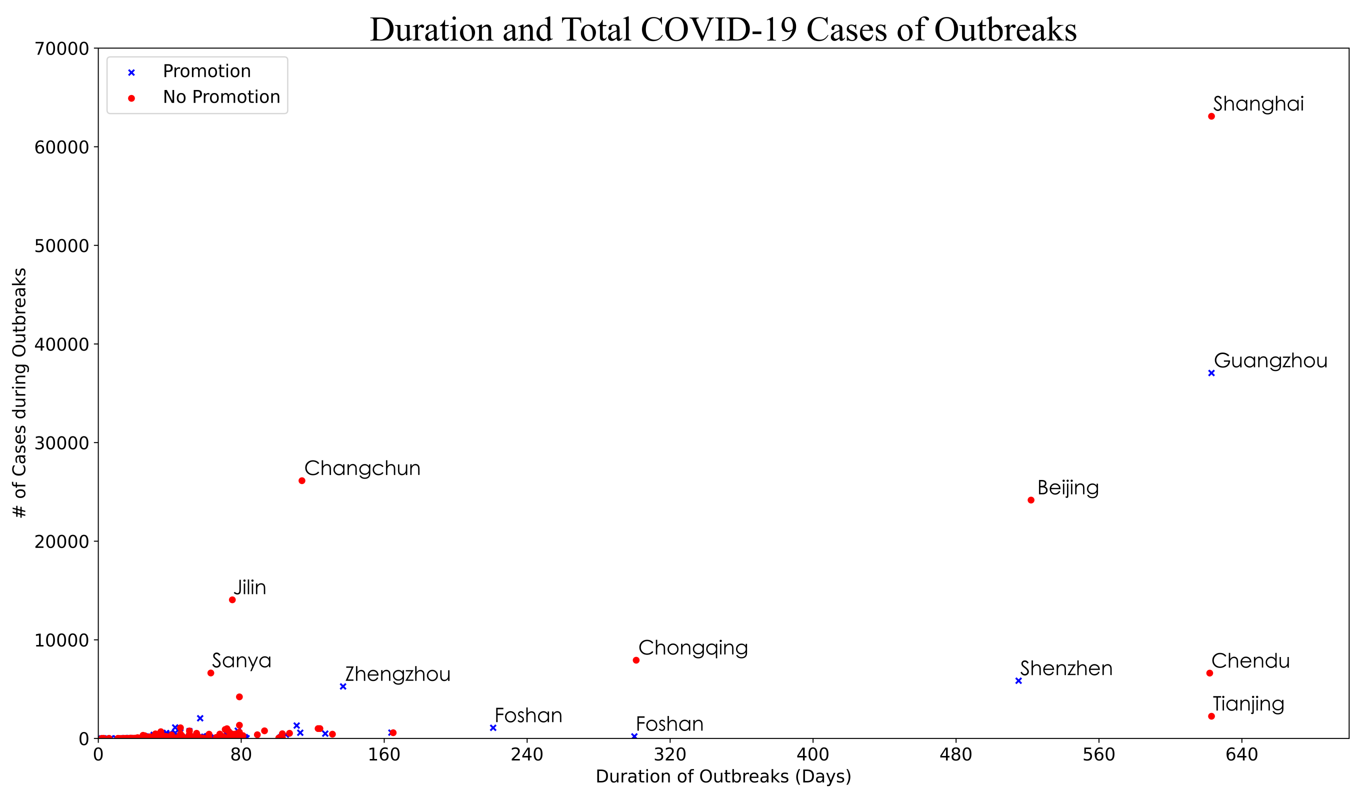


Figure B.4: Distribution of Outbreak Duration and Cumulative Case Number (Subsample of Outbreaks with duration less than 200 and total COVID-19 cases less than 5000