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Three Essays on Public Policy Enforcement in China

By

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Professor Gerard Roland
Professor Marco Gonzalez-Navarro

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Three Essays on Public Policy Enforcement in China

Chapter 2 of this dissertation was previously published as “Career incentives of local leaders and crisis response: A case study of COVID-19 lockdowns in China” co-authored by Qianmiao Chen, Chang Liu and Peng Wang in European Journal of Political Economy, Volume 75, December 2022, 102180 © 2022 Elsevier. Reprinted with permission.

Chapter 3 of this dissertation was previously published as “Farewell to the God of Plague: Estimating the effects of China's Universal Salt Iodization on educational outcomes” co-authored by Chang Liu and Li-An Zhou in Journal of Comparative Economics, Volume 48, Issue 1, March 2020, Pages 20-36 © 2020 Elsevier. Reprinted with permission.

Abstract

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by

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Doctor of Philosophy in Agricultural and Resource Economics

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The enforcement of public policies is a critical aspect of governance that is significantly shaped by political and institutional forces. This dissertation studies the public policy enforcement in China, which features both strong state capacity and weak rule of law. Therefore, the relationship between the central and local government is particularly important in understanding public policy enforcement in this setting. Inspired by theoretical insights and the frontier literature in political economy and development economics, I employ reduced-form empirical methods to evaluate key features in China's public policy enforcement. The three chapters of my dissertation discuss three different modes of public policy enforcements in China, each featuring varying roles of the central and local governments. The findings in this dissertation shed light on the complex dynamics of policy enforcement in China's unique political and institutional context and have important implications for policymakers seeking to improve the effectiveness of governance.

Although the chapters are ordered to highlight the most important work from my doctoral study as Chapter 1, I would like to introduce the contents of each chapter in reverse order, beginning with Chapter 3 and ending with Chapter 1. The introduction starts with a discussion of the simplest mode of public policy enforcement, in which the central government dictates and vertically implements the policy, in Chapter 3. Moving to Chapter 2, the discussion explores the issue of incentives for local government officials, adding complexity to the enforcement model. Finally, in Chapter 1, the conflict between overlapping government hierarchies is analyzed. Overall, this dissertation presents a comprehensive examination of the challenges involved in public policy enforcement, with a focus on the role of government officials at different levels of the hierarchy.

Chapter 3 of this dissertation, which was co-authored with Chang Liu and Li-An Zhou and published in the Journal of Comparative Economics in 2020, evaluates the Universal Salt Iodization (USI) policy implemented in China in 1994. As the largest nutrition intervention policy in human history, the USI policy aimed to eliminate iodine deficiency diseases that could cause severe consequences on the cognitive abilities of future generations. Due to the central

government's monopoly on salt production, distribution, and retail, the policy was effectively enforced vertically. To evaluate the policy's impact on children's later-life educational outcomes, we employed a difference-in-differences strategy to compare the educational outcomes of cohorts born before and after USI across counties with different iodine deficiency disorder levels based on population census data combined with county-level information. Our results demonstrate that the USI policy increased primary school enrollment by 0.6 percentage points and was more beneficial for girls and children born in rural areas. These findings further highlight the efficacy of public policies when enforced vertically by the central government.

Chapter 2 is coauthored with Qianmiao Chen, Chang Liu, and Peng Wang, and published in the European Journal of Political in 2022. This chapter examines a more prevalent model of policy enforcement in China, where the central government sets general policy targets, and local governments have considerable discretion in their implementation. We use Chinese governments' crisis response to the COVID-19 pandemic as an example of such a model and investigate the role of local government leader' career incentives in determining city-wide lockdown measures. At the onset of the pandemic, most local leaders hesitated to impose lockdowns as their promotions depended on achieving strong numbers for economic growth in their regions, which could be suppressed by such measures. However, when the nation's top leader warned that local leaders who failed to control the disease would be removed from office, many rapidly implemented resolute measures. Nonetheless, our analysis reveals that local leaders with stronger promotion incentives were still more likely to downplay the virus by avoiding or minimizing lockdowns. The findings underscore how local politicians may be incentivized to act slowly during crises, undermining the central government's objectives in critical public policies.

Chapter 1 examines a complex scenario in which policy enforcement is limited by the conflict between overlapping government hierarchies, constraining the local governments' ability in effective policy enforcement, even if they have strong incentives. To illustrate this point, I document a real-world example that Chinese central and provincial state-owned enterprises (SOEs) were exempted from local environmental regulations due to an institutional barrier. This exemption pushed local regulators to impose more stringent regulations on private firms. Using rich firm-level panel data and exploiting the decentralization of Chinese central and provincial SOEs, I investigate the direct and spillover effects of removing this regulatory constraint. The results show that polluting SOEs invest more in pollution abatement inputs that do not contribute directly to production, pollutes less, and have lower productivity when decentralized to the prefectural level. Furthermore, private firms in the same prefecture pollute more while increasing output and TFP, especially those with more binding financial constraints. At the aggregate level, decentralizing polluting SOEs in a prefecture reduces total emissions without significantly affecting total industrial output or aggregate productivity. When hypothetically reallocating 10% of emissions from central and provincial SOEs to private firms, I calculate total industrial output gains of 0.74–3.31%. This chapter highlights the significance of institutional interactions, particularly between central and local governments' policy targets, in shaping policy outcomes.

Table of Contents

List of Figures

List of Tables

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1 Decentralization and environmental regulation under overlapping hierarchies: Evidence from China’s SOE reform

1.1 Introduction

1.1.1 Relevance to the dissertation research

The three chapters of this dissertation form a comprehensive study of policy enforcement in China, which is a critical aspect of governance shaped by political and institutional forces. Despite China’s strong state capacity, local governments have considerable flexibility in implementing policies within their jurisdictions. Chapter 1, which is the most important work in my doctoral study and my job market paper, delves into the complexities of policy enforcement in China, exploring the interplay between commands and incentive systems from the central government, the flexible yet constrained enforcement choices of local governments, and the state-owned economy as a third overlapping part in the hierarchical system. The latter can be regulated by a local government but is politically administered by an upper layer in the hierarchy, leading to *ex-post* exemptions from local regulations. This chapter highlights the complexity of China’s political structure and its impacts on policy enforcement in the context of environmental regulation, while also evaluating the efficiency losses resulting from political economy impacts.

My findings in Chapter 1 depict a complex scenario in which policy enforcement is limited by the conflict between overlapping government hierarchies, constraining the local governments’ ability in effective policy enforcement, even if they have strong incentives. To illustrate this point, I document a real-world example that Chinese central and provincial state-owned enterprises (SOEs) were exempted from local environmental regulations due to an institutional barrier. This exemption pushed local regulators to impose more stringent regulations on private firms. Using rich firm-level panel data and exploiting the decentralization of Chinese central and provincial SOEs, I investigate the direct and spillover effects of removing this regulatory constraint. The results show that polluting SOEs invest more in pollution abatement inputs that do not contribute directly to production, pollutes less, and have lower productivity when decentralized to the prefectural level. Furthermore, private firms in the same prefecture pollute more while increasing output and TFP, especially those with more binding financial constraints. At the aggregate level, decentralizing polluting SOEs in a prefecture reduces total emissions without significantly affecting total industrial output or aggregate productivity. When hypothetically reallocating 10% of emissions from central and provincial SOEs to private firms, I calculate total industrial output gains of 0.74–3.31%. This chapter highlights the significance of institutional interactions, particularly between central and local governments' policy targets, in shaping policy outcomes.

1.1.2 Context and literature review

The role of government regulations has long been a key topic in economics. Classical public interest theory justifies regulation by emphasizing the government's role in correcting market failures with Pigovian arrangements. This traditional view is then challenged by a growing emphasis on the political economies of regulators. Regulators are reported to have objective discretion or be captured by special interest groups, through various channels including political incentives, constraints on regulation, and information asymmetry (Stigler, 1971; Peltzman, 1976; Laffont and Tirole, 1993; Leaver, 2009; Duflo et al., 2018). However, little is known about the economic consequences of regulatory capture and discretion. Existing empirical studies suggest large distributive consequences that transfer income between consumers and firms, large firms and small firms, incumbents and new entrants, and so on (De Figueiredo and Edwards, 2007; Acemoglu et al., 2016; Schneider et al., 2020), but provide limited evidence on the net wealth or welfare effects which could potentially be very significant (Dal Bó, 2006; Dal Bó and Rossi, 2007).

This study sheds light on this question in the context of environmental regulations in developing countries, which highlights two features. On the one hand, environmental regulations rely heavily on local governments, especially in developing countries, leaving abundant space for localized and selective implementation serving regulators' political incentives (Burgess et al., 2012; Lipscomb and Mobarak, 2016; Cai et al., 2016; He et al., 2020; Wang and Wang, 2021). On the other hand, factor misallocations and credit constraints are especially prevalent in developing countries (Hsieh and Klenow, 2009; Restuccia and Rogerson, 2017), leading to highly dispersed environmental regulation costs across firms. To gain intuition, the environmental regulation costs depend on the firms' abilities to adjust their input bundles when facing additional factor demands for pollution abatement. Higher factor wedges or more binding credit constraints could therefore increase the cost of environmental regulations. Taken together, one could suppose a captured or discretionary regulation scenario that assigns more stringent regulations to firms with more limited access to factors, making them not only confront a higher cost of pollution reduction but also more stringent regulations, implying a higher aggregate economic cost for pollution reduction.

We explore the consequences of this scenario by documenting a real-world example caused by an institutional barrier in China's environmental regulation system. This barrier limits the enforcement of environmental regulations on a subset of Chinese state-owned enterprises (SOEs), which are widely documented to have more abundant factor access but lower economic efficiency (Brandt et al., 2013; Hsieh and Song, 2015).

This institutional barrier is shaped by China's overlapping organizational structure with centralized administration of SOEs and decentralized enforcement of environmental regulations. On the one hand, China's SOEs are overseen by different levels of government, among which a large share is overseen by central or provincial governments – the high-level governments in China's political system.[1] On the other hand, China's environmental protection law (EPL) imposes a constraint on environmental regulators when they target central and provincial SOEs. Local environmental regulators, usually at the prefectural level, are authorized to implement regulations by inspecting firms and applying punishments when environmental standards are violated. However, the regulators are not authorized to punish central or provincial SOEs without approval from central or provincial governments. This constraint made central and provincial SOEs into a *de-facto* "privileged class" exempt from environmental regulations.

With this exemption, central and provincial SOEs contributed significantly to China's total emissions of various pollutants and largely undermined the aggregate pollution reduction effort. In Panel A of Figure 1, we plot the logarithmic total value-added from all firms, all SOEs, and central and provincial SOEs in China's polluting industries. We observe an increasing trend in total industrial value-added with a relatively constant contribution from central and provincial SOEs. In Panels B, C, and D, we plot the emissions of waste gas, SO2, and sewage per 1,000 RMB of value-added by the three categories of firms. Though there is an aggregate decrease in emission intensities among all firms, the central and provincial SOEs are significantly more pollutive than others, and their pollution intensities were not decreasing over time despite an overall increase in regulatory stringency.

We leverage a rich firm-level panel database on emission outcomes and key financial indicators of Chinese manufacturing firms to empirically test the environmental and economic consequences of lifting the regulation constraint on central and provincial SOEs. We exploit the quasi-exogenous implementation of China's SOE reform to facilitate causal identification. During our sample period, over 600 central and provincial SOEs were decentralized to the prefectural level, mainly driven by concerns over low administrative efficiency owing to a lack of local information largely irrelevant to pollution concerns (Huang et al., 2017), thereby serving as plausible exogenous shocks to the organizational structure between the environmental regulator and SOEs. We further compare the decentralizations of polluting versus non-polluting SOEs to disentangle the effects of differential environmental regulations.

We present a rich set of empirical findings on the effects of decentralization of polluting SOEs on the decentralized SOEs and private firms in the same jurisdiction and discuss the implications for aggregate allocative efficiency. First, we report the direct effect that a polluting central or provincial SOE decreases its emissions of waste gas, SO2, and sewage when decentralized to the prefectural level. Compared to decentralization of a non-polluting SOE, it purchases more capital stock and labor, and has a lower total factor productivity (TFP). These findings indicate that the decentralized state-owned enterprises face increased intensities of environmental regulation and

[1] In the remainder of the paper, we use "central and provincial SOEs" to represent SOEs overseen by the central government or provincial governments. We use "lower level SOEs" to represent SOEs overseen by prefecture- and county-level governments, which are lower-level governments compared to the central government and provincial governments.

increase emission abatement inputs that do not contribute directly to production.

Second, the decentralization of polluting SOEs yields spillovers to private firms in the same prefecture; private firms emit more and increase in value-added, capital stock, and TFP. To understand the underlying mechanism, we demonstrate a simple conceptual framework where the local government is facing a total emission target regardless of the emission sources. If there is a *de-facto* "privileged class" exempt from regulation, the local regulator faces a tighter cap on allowable emissions from the remaining firms, forcing more stringent regulations on the private firms. When decentralization eliminates the "privileged class", the local regulators reallocate emission quotas by decreasing the regulation intensities on the private firms, leading to the spillover effects we report.

Third, we report evidence that dispersed financial access of private firms drives the spillover effects. Following the financial constraint literature, we construct three proxies for a firm's external financial constraints: liquidity, leverage, and the industry-average external finance dependence. The decentralization of polluting SOEs has greater positive spillovers on firms' value-added and TFP if they face more binding financial constraints, and less positive spillover effects on capital stock. These findings confirm the predictions from an extension of our conceptual framework that more financially constrained firms face greater costs from environmental regulations.

Finally, we illuminate the aggregate effects of removing this regulation constraint on central and provincial SOEs. Aggregating the prefectural level production and emission outcomes, we capture an empirical pattern in which decentralization of polluting SOEs significantly decreases total emissions at the prefecture level with no significant effects on total industrial output and aggregate TFP. A back-of-the-envelope welfare calculation suggests a total industrial output gain of 0.74–3.31% if we hypothetically reallocate 10% of total emissions from central and provincial SOEs to private firms.

This paper speaks to several strands of literature. First and foremost, we are among the first to report empirical evidence that regulatory capture and discretion lead to an aggregate inefficiency when intertwined with pre-existing factor misallocations. There exists a literature on distortions and efficiency losses caused by regulations (Fisman and Sarria-Allende, 2010; Pizer and Kopp, 2005; Tombe and Winter, 2015; Song, 2022); however, the existing works mostly assume uniform regulation intensities and shed limited light on regulatory capture or discretion. Among the scarce literature on the effects of regulatory capture and discretion, Duflo et al. (2018) report that discretion improves the effectiveness of environmental regulations by allowing regulators to better target extreme polluters. Our findings complement their work by discussing an alternative discretion scenario that does not necessarily facilitate targeting of heavy polluters and suggest that discretion shaped by exemptions of a subset of polluters could instead increase the aggregate economic costs of regulation.[2]

Second, this paper adds to a burgeoning literature on political decentralization and environmental regulation. Sigman (2005) shows that the decentralized water quality regulation in the United States leads to a free riding problem at the state border. Lipscomb and Mobarak (2016) exploit county splits in Brazil to identify the cross-border patterns of water pollution along rivers, showing that local authorities allowed more settlements to develop close to rivers in the downstream than the upstream portions of counties. Wang and Wang (2021) present that China's township mergers internalized regional environmental spillovers and led to lower emissions at an expense of lower output. Compared to the main takeaway from this literature, that political decentralizations are associated with greater border spillovers that could dampen the effectiveness of regulation, our paper highlights the role of an overlapping political hierarchy where the disparity between the centralized control over SOEs versus the decentralized environmental regulations creates a *de-facto* "privileged class" exempt from regulations, and decentralizations could therefore improve environmental outcomes by eliminating this "privileged class".

Third, our study enriches the understanding of the political determinants of environmental policies in developing countries in two ways. First, our study echoes findings that under many different political institutions, the enforcement of environmental regulations is highly localized and selective. Burgess et al. (2012) report that illegal logging and deforestation are linked to local elections in Indonesia; Lipscomb and Mobarak (2016) explore the border effect on environmental regulations in Brazil. He et al. (2020) and Wang and Wang (2021) indicate that regulatory enforcement by local governments is highly selective, although there is a universal nominal environmental standard in China. Our findings are consistent with this literature, but we further discuss the local regulators' strategic response to differential difficulties in regulating different firms. Second, we focus on strategic interactions across different levels of authoritarian governments in environmental regulation. Kahn et al. (2015) find that China's central government improved local leaders' incentives to reduce pollution by reforming the local political promotion criteria. Cai et al. (2016) speak to competition between local governments that leads to negative border spillovers in water pollution. Greenstone et al. (2022) find that automatic air pollution monitoring reduced systematic air pollution under-reporting in China's local governments. We contribute to this discussion by evaluating the political interactions between the SOEs and local environmental regulators as well as how the political concerns of local regulators react to the existence of polluters exempt from their regulations.

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2 In our context, the central and provincial SOEs are exempt from environmental regulations because they benefit from a superior status in the political hierarchies to local environmental regulators, which is rooted in China's overlapping organizational structure over local governments and SOEs. In this sense, we document discretion in a higher level of political hierarchy than local enforcement and hinders effective targeting of heavy polluters, which resonate the message from Duflo et al. (2018) that discretion does not improve the effectiveness of environmental regulation without effective targeting.

Finally, our findings contribute to understanding the role of the state-owned sector in transition economies. Existing works document a significant productivity gap between SOEs and private enterprises (Hsieh and Klenow, 2009; Brandt et al., 2012; Yang, 2015) and identify various sources, including the administrative and financial inefficiency of SOEs (Lin et al., 1998; Berglof and Roland, 1998; Qian and Roland, 1998; Megginson and Netter, 2001; Lin and Chang, 2019) and factor misallocations (Hsieh and Klenow, 2009; Hsieh and Song, 2015). We contribute a new perspective to this discussion: environmental regulators impose more stringent regulations on private firms facing more binding financial constraints and greater environmental regulation costs, while SOEs are more likely to be exempt from regulations.[3]

The remainder of the paper proceeds as follows. Section 1.2 describes the institutional context of China's SOE oversight system, SOE reform and decentralization, and the local enforcement of environmental regulations. Section 1.3 introduces a conceptual framework that motivates the empirical research. Section 1.4 introduces the data and sample. Section 1.5 presents our empirical strategies to identify the environmental and economic effects of removing the regulation constraint on central and provincial SOEs. Section 1.6 reports baseline empirical results, discusses the validity of the baseline estimates, and reports a set of extensions and robustness checks for the baseline findings. Section 1.7 discusses the implications for aggregate allocative efficiency. Section 1.8 concludes the paper.

1.2 Institutional Context

1.2.1 China's SOE Oversight System and Decentralization of SOEs

As a legacy of the command economy era, China's economy features the important role of SOEs, administered by an overlapping hierarchy of central and local governments. SOEs are said to be "overseen" by different government levels, which means their state-owned stock is held by those "oversight governments"; more precisely, by the local State-owned Assets Supervision and Administration Commission (SASAC), an administrative branch of the oversight government (Huang et al., 2017). By holding the state-owned shares, the oversight government has the power to influence a wide range of SOE operations, including finance, employment, social welfare, and personnel. The sharing of corporate tax revenue and economic residuals also falls under the oversight government. The leaders of the SOEs usually have stronger personal connections to the officials in the oversight government, which is especially important for central and provincial SOEs, which means the SOE's leadership has stronger connections to high-level political officials.

3 This study shares a similar research topic as the concurrent work of Fan et al. (2022), which also report substantial differences of environmental regulation effects by ownership structure in China, explained by differential financial market access. However, we explore a bigger picture of China's hierarchy of SOEs that overlaps the local government system. The overlapping organizational structure creates a fundamental difference in regulation enforcement across ownership types: the exemption of central and provincial SOEs.

To the oversight government, overseeing an SOE means more revenue sharing but may also cause a fiscal burden if the SOE is unprofitable. Overseeing an SOE also means additional ways to influence the local economy and more space for rent-seeking by influencing SOE personnel control.

It is noteworthy that there is a rich variety in the political levels of governments overseeing SOEs, from the central government to local governments at provincial, prefectural, and county levels. Waves of historical movements in China's pre-reform era shaped this variety. In the early 1950s, China established its planned economy by copying the Soviet Union's system, where all important plants were subject to the central government through eight ministries of industries, which are branches of the State Council (the formal name of China's central government). During the "Great Leap Forward" 1958–1960, Mao Zedong encouraged local governments to establish their own enterprises to incentivize local efforts to promote economic growth, leading to a growing number of SOEs overseen by local governments. The third wave started in 1964, when China's central government was worried about potential military strikes from the US and the Soviet Union; it, therefore, relocated many strategically important SOEs to the hinterland, and most of the relocated SOEs were controlled by the central government (the "Great Third Front Construction" movement). This was followed by the fourth wave started in 1971, when Mao further required each province to be self-sustaining during potential invasions and asked provinces, prefectures, and counties to establish their own enterprises in defense-related industries, creating another wave of SOEs overseen by local governments (Naughton, 2006; Brandt and Rawski, 2008).

By the 1980s, the SOEs established in all four waves suffered from low economic efficiency and profitability, placing severe fiscal burdens on all Chinese government levels, which motivated the SOE reform starting in the 1990s, as summarized in Huang et al. (2017). The main objective of the SOE reform was to improve SOE efficiency and performance using three primary measures: merger, decentralization, and privatization. First, China merged large, advanced, or strategically important SOEs into large industrial conglomerates under the direct control of central government. Second, for smaller and less productive enterprises, their oversight governments considered decentralizing them to lower-level governments to allow local governments closer to the SOEs to make more flexible production, investment, and personnel decisions. Third, for the smallest and least productive SOEs, the governments allowed private owners or investors to buy off the enterprises – "privatization" or "restructuring" (Qian et al., 2006; Brandt and Rawski, 2008; Naughton, 2015).

We leverage decentralization among the three measures as the key identifying variation for two reasons. First, decentralizations were the major shocks to the organizational structure of Chinese SOEs during the reform. Second, the variations in decentralization are better-understood thanks to the existing literature. As documented by Huang et al. (2017), decentralization decisions were made by the original oversight governments (the higher-level governments) alone and could not be rejected or reversed by the receiving governments at lower levels. Furthermore, Huang et al. (2017) carefully discuss the determinants of decentralizations and conclude that they are mostly explained by a Hayek conjecture that a lack of local information led to low administrative efficiency and that an SOE was more likely to be decentralized the greater the distance from its original oversight government. Taken together, decentralizations serve as quasi-exogenous shocks to the organizational structure between polluting SOEs and local environmental regulators in the sense that (1) the local regulators (local governments) could not decide whether an SOE in their jurisdictions was decentralized, and (2) the decentralization decisions were largely irrelevant to pollution concerns.

1.2.2 China's Environmental Regulation System

In this section, we introduce China's environmental regulation system focusing on its local implementation. China's immense economic growth since 1979 is impressive not only for its economic success but also for the environmental consequences it generates: by 1997, there were hundreds of thousands of premature deaths and incidents of serious respiratory illness caused by exposure to industrial air pollution; many of China's waterways became largely unfit for human use owing to sewage emissions (Dasgupta et al., 1997). The severe environmental problems also resulted in growing social discontent. Since 1996, the number of environmental protests has increased by an average of 29% every year.[4] In response to this challenge, China's central government has called for increasingly stringent environmental regulations since the 1990s. China's first environmental protection law (EPL) was issued in 1989 and came into effect in January 1990. In 1998, the State Environmental Protection Administration, the top government institution for environmental regulation, was upgraded to ministerial status in the central government and further renamed the Ministry of Environmental Protection (MEP) in 2008.

As in many developing countries, the central government's commitment to more stringent environmental regulations is never credible without enforcement by the local governments, who bear the specific responsibility to implement inspections, fines, and other punishments for violations and enjoy significant room for selective implementation serving their political interests. The division of responsibilities across different government levels is reported by a State Council document in 2020 that reviews China's historical experience of environmental regulation:

"The central government coordinates the major environmental protection policies, puts forward overall goals, and plans for major strategic measures; provincial governments bear overall responsibility for environmental governance in the region, organize the implementation of central government objectives, tasks, policies and measures, and provide financial support; prefectural and county governments bear specific responsibilities, implement supervision and law enforcement, market regulation, funding arrangements, publicity, and education…"

[4] Please see https://chinadialogue.net/en/cities/5561-china-s-new-middle-class-environmental-protests/ for an example of such discussion.

The main takeaway from this document is that higher-level governments at the central and provincial levels are primarily responsible for setting major strategic measures and policy objectives, while lower governments, especially at the prefecture level, are the regulators that implement the regulations in their jurisdictions. Thus, China's bureaucratic system enforces environmental regulations in a top-down fashion, similar to other political tasks in China, including the one-child policy, worker safety, disease control, and education promotion (Fisman and Wang, 2017; Zhang, 2017; Chen et al., 2018; Fang et al., 2020; Chen et al., 2022).

As the actual executors of environmental regulations, the prefecture- and county-level governments were authorized by the EPL to perform on-site inspections and charge emission fees within their jurisdictions. In case of violations of emission standards, local regulators may punish violators with fines, orders to overhaul the violations in limited time, production suspensions, or even more severely, ask a polluter to temporarily shut down. However, punishment measures are highly restricted when the polluter is an enterprise or institution subject to central or provincial government oversight. As noted in the Environmental Protection Law in 1989:

"Emission fees and fines can only be charged by the State Council if the targeted entity (enterprises and other institutions) is administered directly by the central government, and charged by the provincial government if administered by the provincial government… Overhaul, production suspension, or shutdown orders should be submitted to the State Council for approval if the targeted entity is administered directly by the central government, and to the provincial government if administered by the provincial government."

These constraints made prefecture- and county-level regulators unable to impose any effective punishments should central or provincial SOEs violate the EPL; this made them a *de-facto* "privileged class" exempt from environmental regulations.[5] The restrictions were removed later in the 2014 EPL amendment that took effect in January 2015, but unfortunately, the firm-level panel data used in this study does not include observations post-2015, and we were unable to capture the effects from the universal removal of these constraints.

The prefecture- and county-level governments are held responsible for pollution reduction tasks and incentivized by top-down imposed total emission targets that impact the likelihood of local government officials' promotions.[6] In every Five-Year Plan (FYP) since 2001's 10th FYP, the central government makes promises of pollution reduction at the national level and splits this into provincial government targets. The provincial governments then allocate emission targets across their prefectures. This process is largely imposed in a top-down manner, as the lower levels have

5 I report more anecdotal evidence on the local regulators' inability to regulate central and provincial SOEs in their jurisdictions in Appendix A.1.F.

6 As noted in the Environmental Protection Law (1989), "local governments at various levels shall be responsible for the environmental quality of their jurisdictions and take measures to improve the environmental quality."

very little bargaining power against targets imposed by higher authorities (Zhang, 2021).[7] Notably, the emission target is imposed on the total amounts of specific types of emissions produced within the jurisdictions regardless of the emission sources.

As a typical example of target-setting in China's regionally decentralized authoritarian (RDA) system (Xu, 2011), emission reduction serves as a typical "one-item-veto" target in the sense that failing to achieve the target makes local government leaders ineligible for career promotions even if they perform well in other targets such as facilitating economic growth. Therefore, failing to achieve emission targets has severe career consequences and pressures local officials to implement environmental regulations with increased stringency. However, a disparity arises when there are central- or provincial-administered polluters in their jurisdiction: the emissions from these polluters are exempt from regulations, but not exempt from the regulators' total emission target. Therefore, while the central and provincial SOEs contribute significantly to total emissions, local regulators confront more limited allowable emissions from the rest of the firms they regulate, pushing them to regulate the remaining polluters with increased stringency. This scenario will be further formalized under the conceptual framework discussed in section 1.3.

1.3 Conceptual Framework

In this section, we motivate the empirical study using a conceptual framework to conceptualize the economic and environmental effects of firms' environmental regulation as well as the local government's environmental regulation decisions, where they optimize regulation stringency across different firms while confronting career promotion incentives. For simplicity, assume two firms in a local unit – private ($i = 1$) and state-owned ($i = 2$) to allow space for the local government's selective regulatory enforcement over ownership types. For simplicity, assume the two firms are identical in all characteristics except ownership, and both firms are discussed under partial equilibrium.

1.3.1 Economic Costs of Firm-level Environmental Regulation

We borrow the firm settings in He et al. (2020) and Wang and Wang (2021) that discuss the effects of regulation intensity on firm outcomes under partial equilibrium. Assume each firm has a neoclassical production function over capital and labor $f(K, L)$, and the firm's emissions are determined by its production plus emission abatement capital (K_E) described by the function $E(f(K, L), K_E)$.[8] Firms maximize profit subject to the exogenous environmental regulation

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[7] An anecdotal example of this top-down manner is China's 11th FYP. During this process, the central government monitored the local objectives established by provinces and found they failed to achieve the aggregate national target and forced the provincial and prefectural governments to remake plans "under close instruction" from the central government.

[8] We further assume $f_1 > 0$, $f_2 > 0$, $f_{11} < 0$, $f_{22} < 0$, and $E_1 > 0$, $E_2 < 0$, $E_{11} > 0$, $E_{22} > 0$

intensity (r) conceptualized as tax-like fines on pollutive emissions:

$$\max \pi(K, L, K_E) = pf(K, L) - p_K(K + K_E) - p_L L - rE(f(K, L), K_E)$$

The same propositions as in He et al. (2020) can be derived to predict the effects of environmental regulations on firm observable attributes, which allows us to empirically infer changes in environmental regulation stringency from changes in firm characteristics when the stringency is not directly observed:

Proposition 1. Output, profit, and TFP decrease if a firm faces stricter environmental regulation ($\frac{\partial f_i}{\partial r_i} < 0, \frac{\partial \pi_i}{\partial r_i} < 0, \frac{\partial TFP_i}{\partial r_i} < 0$). [9]

Proposition 2. Investment in emission abatement capital will increase and total emissions will decrease if a firm faces stricter environmental regulation ($\frac{\partial K_{Ei}}{\partial r_i} > 0, \frac{\partial E_i}{\partial r_i} < 0$).

Furthermore, we extend the model by assigning Cobb-Douglas production and emission functions ($Y = AK^{\alpha_K} L^{\alpha_L},\ E = Y^{\delta} \left(\frac{K}{K+K_E}\right)^{\beta}$) and transform the production function in the spirit of Tombe and Winter (2015): [10]

$$Y = \tilde{A}(K + K_E)^{\tilde{\alpha}_K} L^{\tilde{\alpha}_L} E^{\tilde{\alpha}_E}$$

In this transformation, emissions can therefore be taken as an input in production function combined with total inputs in capital (productive plus abatement) and labor. Therefore, one can easily imply a positive but decreasing "marginal product of emission" ($\frac{\partial Y}{\partial E} > 0$ and $\frac{\partial^2 Y}{\partial E^2} < 0$). This transformation is especially meaningful for the regulators in the sense that the marginal economic benefits from allowing more emissions from a single firm are diminishing.

1.3.2 Regulatory Decisions of Local Governments

We model the local government objective function based on the meritocracy conjecture in China's political economy literature that local government officials balance across multiple key performance indicators (KPIs henceforth), and achieving each of the KPIs contributes to a higher

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[9] Here TFP is defined as the ratio of revenue relative to all inputs including K_E: $TFP = \frac{pf(K,L)}{p_K(K+K_E)+p_L L}$.

[10] $\tilde{A} = A^{\frac{\beta}{\beta+\delta\alpha_K}}$, $\tilde{\alpha}_K = \frac{\alpha_K \beta}{\delta\alpha_K+\beta}$, $\tilde{\alpha}_L = \frac{\alpha_L \beta}{\delta\alpha_K+\beta}$, $\tilde{\alpha}_E = \frac{\alpha_K}{\delta\alpha_K+\beta}$. $\tilde{\alpha}_K, \tilde{\alpha}_L, \tilde{\alpha}_E \in (0,1)$ if we assume $\alpha_K, \alpha_L \in (0,1)$ and $\delta > 1$.

likelihood of promotion.[11] There are two KPIs most relevant in the context of environmental regulations: first, local governments are incentivized to maximize regional GDP using a tournament system where prefectural leaders achieving higher economic growth are more likely to receive promotions (Li and Zhou, 2005; Xu, 2011; Wang et al., 2020); second, local governments are required to achieve local emissions targets as a constraint on total emissions. As introduced in section 2.2, such emission targets are set by higher-level governments and reported in provincial FYPs.

Therefore, we model the local government's objective function as maximizing the net contribution of the two targets to its promotion likelihood. For simplicity, we assume total output ($\sum_i Y_i$) and total emissions relative to emission target ($\bar{E} - \sum_i E_i$) contribute to promotion likelihood in a Cobb-Douglas form:

$$\max_{r_1, r_2} \gamma_1 \log\left(\sum_i Y_i(r_i)\right) + \gamma_2 \log\left(\bar{E} - \sum_i E_i(r_i)\right)$$

To further illuminate the government's regulatory decisions across different firms, we apply the Tombe and Winter (2015) style transformation by writing the production function as a Cobb-Douglas combination of total capital, labor, and emission. Noting that emission is a monotone function of regulation intensity r_i, the optimization problem can be transformed by taking E_i as the choice variable, in the sense that the regulator indirectly allocates emission quotas across firms by selecting regulation intensities:

$$\max_{E_1, E_2} \gamma_1 \log\left(\sum_i Y_i(E_i)\right) + \gamma_2 \log\left(\bar{E} - \sum_i E_i\right)$$

1.3.3 Regulation Constraints on Central and Provincial SOEs

As discussed in section 2.2, a regulatory constraint exists if the SOE is administered by central or provincial governments. To conceptualize this constraint, we assume a cap over SOE regulation intensities (i.e., $r_1 \leq \bar{r}$), which is binding for central and provincial polluting SOEs, or equivalently a lower limit on the emissions quota allocated to the SOE (i.e., $E_1 \geq E_1(\bar{r})$). With the transformed production function, we derive the comparative statics by taking E_i as the choice variable and exploiting properties of "marginal product of emission" ($\frac{\partial Y}{\partial E} > 0$ and $\frac{\partial^2 Y}{\partial E^2} < 0$):

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[11] Please see Xu (2011) for an excellent review on this literature.

$$\max_{E_1 \geq E_1(\bar{r}), E_2} \gamma_1 \log\left(\sum_i Y_i(E_i)\right) + \gamma_2 \log\left(\bar{E} - \sum_i E_i\right)$$

The comparative statics indicate that a stronger enforcement power represented by a larger $\bar{r}$ (and therefore smaller $E_1(\bar{r})$) will increase the stringency of SOE environmental regulation ($\frac{\partial r_1}{\partial \bar{r}} > 0, \frac{\partial E_1}{\partial \bar{r}} < 0$) and decrease the stringency of private firm environmental regulation ($\frac{\partial r_2}{\partial \bar{r}} < 0, \frac{\partial E_2}{\partial \bar{r}} > 0$), further predicting the effects of removing the distortion by decentralizing the central and provincial SOEs. As the restriction on local regulators is removed, prefectural regulators can more stringently regulate central and provincial SOEs, indicating an increase in $\bar{r}$, making it no longer a binding constraint, leading to a higher stringency on SOEs and lower stringency on private firms. Combined with previous propositions, the output, profit, productivity, and pollution amounts would decrease in the decentralized SOE but increase in remaining private firms. Therefore, the decentralization of central and provincial SOEs not only yields direct effects, in that the decentralized SOEs are more stringently regulated, decreasing productivity, but it also creates spillover effects by reallocating regulation intensities, in that private firms are less stringently regulated, increasing productivity.

1.4 Data

We combine several rich datasets that provide comprehensive observations on key financial indicators, ownership type, oversight status, and emission outcomes of manufacturing firms in China, together with prefecture-level socio-economic characteristics and political leaders' resume information.

1.4.1 The Annual Survey of Industrial Firms (ASIF)

Our firm-level production and ownership information is based on the Annual Survey of Industrial Firms (ASIF) database from 1998 to 2014.[12] The ASIF database is managed by the National Bureau of Statistics and contains a rich set of key financial indicators including capital and labor inputs, outputs, sales, taxes, expenses, and profits from firms' annual accounting reports. The dataset contains all SOEs in China and private firms above a specific scale threshold.[13]

As widely applied in empirical works on China's economy, we follow the standard procedure in the literature to clean the ASIF database following Brandt et al. (2012), Yang (2015), and Yu (2015).

[12] The year 2010 is dropped from the sample because all key financial indicators were missing.

[13] Private firms are included in the ASIF (the so-called "above-scale" firms) if the annual revenue is over 5 million RMB before 2011; the revenue threshold increased to 20 million RMB since 2011.

First, we use institutional codes, firm names, names of legal representatives, geographic codes, industry codes, founding years, addresses, and phone numbers to identify identical firms across different years of observation and construct a panel identifier. Second, we follow the standard procedure to construct real capital stock and deflate key financial variables at 2-digit industry by year level. Third, we drop observations with missing key financial indicators or those that apparently violate accounting principles.[14] Fourth, we drop observations with values of key variables below the 0.5^{th} percentile or above the 99.5^{th} percentile. Finally, we produce the semi-parametric TFP estimates following Olley and Pakes (1996) as the baseline TFP measure and take several alternative TFP estimates for robustness checks. The final sample includes 3,104,933 observations and 724,851 unique firms.

The key identifying variation in our research design is the ownership type and the level of oversight government if the firm is state-owned, also drawn from the ASIF dataset. We follow the criteria in Huang et al. (2017) that a firm is defined as an SOE if its state capital share exceeds 30%. The SOE is identified as decentralized in year t if its oversight status changes to a lower level than year $t-1$. SOEs whose oversight status changed repeatedly are not counted because this case is more likely to be caused by coding inconsistency across years instead of actual decentralizations. We also use GIS data on China's administrative divisions to generate the distance of SOEs from their oversight governments following Huang et al. (2017). We use a categorization of polluting industries at 2-digit and 3-digit industry levels published by China's MEP as introduced in Appendix Table A.1.3 to identify polluting firms in the ASIF dataset.

1.4.2 The Environmental Survey and Reporting (ESR) Dataset

The Environmental Survey and Reporting (ESR) dataset is managed by MEP and contains firm by year level observations on emission amounts by different pollutant categories including waste gas, SO_2, sewage, and other important pollutant types, as well as several proxies for emission abatement inputs.[15] The database contains 1,652,355 observations from 1998 to 2014. The ESR database includes important pollution sources including not only manufacturing firms but also hospitals, research institutes, and hazardous waste and sewage processors.

We use firm names and institutional codes to match the firms in the ESR dataset with the basic firm information in the ASIF dataset to (1) identify the manufacturing firms in the sample and (2) use the basic firm information in ASIF to identify SOEs and their decentralization records.[16] In

[14] These cases include: negative values for output, intermediate input, value-added, employment, capital stock; liquid assets or fixed assets greater than total assets; current depreciation greater than cumulative depreciation.
[15] The emissions are self-reported by the firms and double-checked by the MEP's auditors. The ESR data is not observable by local level regulators and are therefore not used as the basis of environmental regulations, which limited the firms' incentives to misreport their emission amounts. Please see He et al. (2020) and Wang and Wang (2021) for a more comprehensive discussion on the reporting process of the ESR database.
[16] As the ASIF contains the universe of all Chinese SOEs, the important polluting SOEs and their records of

the ESR dataset, 745,847 observations are matched with firms in the ASIF dataset. The main reason for the missing matches is that only 884,673 ESR observations satisfy the scale thresholds in the ASIF dataset. The matching rate among the "above scale" firms is then 84.3%. However, as the matched database only contains a small fraction of the ASIF dataset, we do not interpret the matched sample as a representative sample of the ASIF but cautiously interpret the matched sample as "above-scale manufacturing firms in the ESR dataset" and only use the basic firm information from the ASIF dataset. Therefore, we analyze the two datasets separately in the empirical analysis.

1.4.3 Prefecture-level Datasets

We use data from statistical yearbooks for 314 Chinese prefectural-level cities from 1999 to 2019 to construct a prefecture-level database with economic and pollution outcomes. Among them, 284 prefecture-level cities have non-missing observations on the total amount of industrial SO_2 and sewage emissions from 2003 to 2019. The summary statistics of ASIF, ESR, and prefectural-level panel data are reported in Appendix Table A.1.2.

1.5 Empirical Strategies

As discussed in section 1.3, our conceptual framework predicts that when a polluting central or provincial SOE is decentralized to the prefecture level, it will reduce its pollutant emissions, increase in abatement capital, and have lower productivity. The private firms in the same prefecture will increase their pollutant emissions and also have higher outputs and productivities. In this section, we discuss the empirical strategies to identify these effects.

1.5.1 Direct Effects on the Decentralized SOEs

Following the predictions in section 1.3, decentralization removes the regulation constraint on the SOEs previously overseen by the central or provincial governments, leading to an increase in regulation intensity and decrease in emission and production outcomes. We first focus on identifying the effects of decentralization on pollution outcomes. With the ESR dataset and firms' basic information matched from the ASIF, we identify the SOEs' among polluting firms in the ESR and their decentralization records. Decentralization acts as an external shock on the political relationship between the SOE and the regulating government, and we estimate the following difference-in-differences (DD) model specified in equation 1.1 to identify the effects of decentralization on pollution outcomes:

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decentralization are then identified in the ESR dataset. We use this relationship to construct decentralization relevant variables when analyzing the ESR dataset.

$$P_{ispt} = \alpha_i + \rho_{st} + \delta_{pt} + \beta Dece_{it} + \varepsilon_{ispt} \quad (1.1)$$

where P_{ispt} represents the pollution outcomes for firm i in 2-digit industry s, prefecture p, and year t. The outcome variables are logarithmic pollutant emissions in three types reported in the ESR dataset: waste gas, SO2, and sewage.[17] $Dece_{it}$ is a dummy event variable equaling 1 if the firm is an SOE and has been decentralized from the central or provincial level to the prefectural level. The coefficient β captures the effect of decentralization on firms' pollution outcomes. We control for firm fixed effects, 2-digit industry by year fixed effects, and prefecture by year fixed effects to account for unobservable firm characteristics, prefecture-specific time trends, and industry-specific time trends. The standard errors are clustered at the firm level as suggested by Cameron and Miller (2015).

As is typical for DD identification strategies, the key identifying assumption for model (1) is that decentralization should be uncorrelated with potential trends in a firm's pollution outcomes. The assumption is plausibly true given the institutional context that the prefectural level regulator does not have control over the decentralization decision, and the higher-level government does not consider pollution outcomes when they decide which SOE to decentralize. Therefore, the unobserved potential pollution intensity and decentralization are likely uncorrelated. However, being decentralized may also affect a firm's production, corporate governance, access to loans, and status in competition, which may lead to an indirect effect on pollution outcomes.

To address this concern, we use the same methodology as He et al. (2020) by splitting the firms into two groups: firms in a non-polluting industry versus firms in a polluting industry.[18] We perform a triple-differences (TD) estimation by comparing the two DD estimators on the decentralization effects between firms in polluting versus non-polluting industries and attribute this third difference to the effect of environmental regulation. This methodology is not directly applicable to the ESR dataset as the database only includes polluting firms, so we focus on the ASIF sample and firms' production outcomes when applying the TD strategy. The empirical model for TD is specified in equation 1.2:

$$Y_{ispt} = \alpha_i + \rho_{st} + \delta_{pt} + \beta Dece_{it} + \tau Dece_{it} \times Pollute\_ind_s + \varepsilon_{ipt} \quad (1.2)$$

where Y_{ispt} is the logarithm of the firm's key financial indicators (value-added, capital stock, employment) and TFP measurements. $Pollute\_ind_s$ is a dummy variable equaling 1 if the firm is in a polluting industry categorized by MEP. The coefficient of interest is τ for the interaction

[17] Waste gas and SO2 are indicators of air pollution while sewage is an indicator of water pollution. We pick the three variables because the ESR reports the most comprehensive observations on them, and they are highly representative of pollutant emissions in Chinese industrial firms.

[18] This categorization is made by the Ministry of Environmental Protection. Please refer to Table A1 for the detailed classification over industries.

between decentralization and the polluting industry indicator.

As suggested by Olden and Møen (2022), the validity of the TD strategy does not require the parallel trend assumptions to hold for the two DD estimators. The TD estimator, as the difference between two biased DD estimators is still unbiased if the biases in DD estimators are the same, therefore they can cancel out. In this research, the property of TD weakens the key identifying assumption in the sense that it allows for selection in decentralization based on potential outcomes in production outcomes, as long as the selection is irrelevant to whether the firm is in a polluting industry. The TD also allows non-zero decentralization effects in the absence of changes in regulation intensity. Instead, it requires that, except for environmental regulation, the decentralization effects should be the same for SOEs in polluting versus non-polluting industries.

1.5.2 Spillover Effects on the Private Firms

In addition to the direct effects, the conceptual framework in section 1.3 predicts a spillover effect from decentralization of polluting SOEs on private firms regulated by the same prefecture-level local regulator in that the private firms will benefit from more lenient environmental regulation and therefore increase emission and production outcomes. We estimate equations 1.3 and 1.4 among the private firms in the ESR and the ASIF to identify the spillover effects:

$$P_{ispt} = \alpha_i + \rho_{st} + X_p \times \delta_t + \beta N(Dece, ESR)_{pt} + \varepsilon_{ispt} \qquad (1.3)$$

$$Y_{ispt} = \alpha_i + \rho_{st} + X_p \times \delta_t + \beta_1 N(Dece, ASIF, all)_{pt} + \beta_2 N(Dece, ASIF, pol)_{pt} + \varepsilon_{ispt} \quad (1.4)$$

where P_{ispt} and Y_{ispt} are the outcomes of interest as discussed in equations 1.1 and 1.2. We construct the $N(\cdot)$ variables to identify the spillover effects by counting the cumulative number of SOE decentralizations in prefecture p by year t. Therefore, $N(Dece, ESR)_{pt}$ represents the cumulative number of records where polluting SOEs are decentralized to prefecture p by year t in the ESR dataset; $N(Dece, ASIF, all)_{pt}$ represents the cumulative number of records where all types of SOEs are decentralized to prefecture p by year t in the ASIF dataset; and $N(Dece, ASIF, pol)_{pt}$ represents the cumulative number of records where SOEs in polluting industries are decentralized to prefecture p by year t in the ASIF dataset. In equation 1.3, the coefficient β of $N(Dece, ESR)_{pt}$ captures the mean spillover effects of an additional polluting SOE being decentralized on the private firms' pollution outcomes; in equation 1.4, $N(Dece, ASIF, all)_{pt}$ and $N(Dece, ASIF, pol)_{pt}$ are taken as aggregations of the TD variables $Dece_{it}$ and $Dece_{it} \times Pollute\_ind_s$ at the prefecture by year level, in the sense that we allow a spillover effect from the decentralization of all SOEs on firm's financial indicators in the absence of environmental regulation, which is captured by β_1; we then attribute the additional spillover effects from SOE decentralizations in polluting industries to the change in environmental regulations captured by β_2. Therefore, β in equation 1.3 and β_2 in equation 1.4 are the main parameters of interest.

We control for a similar group of fixed effects in equations 1.3 and 1.4 to those in equations 1.1 and 1.2, with the exception that the prefecture-by-year fixed effects are not applicable as the key identifying variations at the prefecture-by-year level. As a substitute, we control for the interaction of a full set of times fixed effects δ_t with X_p, a vector of prefecture-level characteristics in 1998. The prefecture-level controls include the logarithm of population, non-agriculture population, GDP, and total industrial output in 1998, plus several indicators of industrial structural: the share of first and second industries in GDP, and the share of industrial output from polluting industries in 1998. The standard errors are clustered at prefecture level following Cameron and Miller (2015).

1.6 Baseline Results

In this section, we report the baseline estimates from equations 1.1 through 1.4 on the direct and spillover effects of decentralization on the pollution and production outcomes of the decentralized SOEs and the private firms under the same prefectural level environmental regulators. We then discuss potential threats to the validity of our baseline analysis, a set of extensions to the baseline estimates, and a series of additional robustness checks.

1.6.1 Direct Effects on the Decentralized SOEs

We first investigate the decentralization effects on pollution outcomes following the DD strategy as specified in equation 1.1. As shown in Table 1.1, we use the ESR sample for estimation where the outcomes of interest are the logarithmic emission amounts of three main types of pollutants: total waste gas (in standard cubic meters), SO2 (in kilograms), and sewage (in tons) in columns 1–3, respectively. The estimates indicate that, compared to all other firms in the same prefecture, year, and 2-digit industry, a polluting SOE decreases 47.5% of its waste gas emissions, 63.0% of its SO2 emissions, and 45.2% of its sewage emissions when decentralized from central or provincial governments to the prefectural government. These effects are statistically significant and also have considerable economic meaning, in the sense that the central and provincial SOEs are around 50% more pollutive than an average firm before decentralization as shown in Figure 1.

In Table 1.2, we investigate the effects of decentralization on firms' production outcomes following the TD specification in equation 1.2. In columns 1–4, we report the decentralization effects on logarithmic value-added, capital stock, employment, and TFP of firms. The first row reports the DD estimates of the decentralization effects among non-polluting firms, whereas the second row reports the TD estimates as the differential decentralization effects among polluting firms compared to non-polluting firms, which are the estimates of interest. As suggested by the first-row estimates, when a non-polluting SOE is decentralized to the prefecture level, it experiences a decrease in value-added, capital stock, and employment in a similar magnitude, indicating a shrinking scale without any significant effect on its TFP. The first-row estimates may also suggest a selection that shrinking SOEs may be more likely to be decentralized, but the biased DD effects do not directly threaten the validity of TD estimates as long as the biases caused by selections in decentralizations are comparable between polluting and non-polluting firms.

In contrast, the TD estimates reported in the second row suggest that when a polluting SOE is decentralized to the prefectural level, it does not experience an additional change in value-added when compared to the decentralization of a non-polluting SOE but increased 39.7% of its capital stock and 14.4% of its employment relative to the decentralization effects among non-polluting SOEs. These results indicate that SOEs in polluting industries spent more on factors not contributing to production, which is confirmed by column 4: after being decentralized to the prefecture level, polluting SOEs decreased 21.5% of their TFP.

These results are consistent with the predictions in the conceptual framework discussed in section 1.3 that the SOEs pollute less and become less economically efficient when decentralized from central or provincial governments to the prefectural level. As the polluting SOEs purchased more capital and labor without contributing to their outputs, we further infer that the increased inputs are possibly used for emission abatement confronting more stringent environmental regulations following decentralization.

1.6.2 Spillover Effects on the Private Firms

Our conceptual framework suggests that for the prefecture-level regulators, when there are central and provincial polluting SOEs exempt from environmental regulations in their jurisdictions, their strategic response would be to regulate other firms more stringently because of political targets on total emissions. Therefore, we predict that eliminating this “privileged class” through decentralizations of polluting SOEs leads to spillover effects on the private firms in the same jurisdiction. We confront the spillover effects by estimating equations 1.3 and 1.4 using the private firm sample from ESR and ASIF, respectively.

Table 1.3 shows that if an additional polluting SOE is decentralized to a prefecture, other firms gain the ability to pollute more. On average, one additional decentralization of a polluting SOE in the same prefecture leads to a 4.34% increase in a private firm’s waste gas emission, a 4.74% increase in SO_2 emission, and a 5.51% increase in sewage emission. These findings are consistent with the conceptual framework that when an additional polluting SOE is decentralized, the prefecture-level regulators will reduce the regulation intensities on the private firms and allow more emissions from them.

In Table 1.4, we investigate the spillover effects on production outcomes. Similar to Table 1.2, the first-row coefficients indicate the spillover effects from the decentralization of non-polluting SOEs, while the second-row coefficients indicate the relative spillover effects if the decentralized SOEs are in polluting industries, which is the parameter of interest. The first-row estimates show that an additional decentralization of a non-polluting SOE in a prefecture results in a slight decrease in value-added, capital stock, employment, and TFP, but the magnitudes are small, ranging from 0.2-1.1%. In sharp contrast, if the decentralized SOE is in a polluting industry, it leads to significant spillovers to private firms. On average, an additional decentralization of a polluting SOE leads to a 3.89% increase in value-added, a 2.55% increase in capital stock, a 1.47% increase in employment, and a 2.50% increase in TFP among private firms in the same prefecture. These findings confirm that private firms receive economic gains when they face less stringent environmental regulations following the decentralization of polluting SOEs.

1.6.3 Threats to the Baseline Estimates, Extensions, and Robustness Checks

In this section, we discuss the potential threats to the validity of our baseline research design and conduct several empirical tests to address them. We also discuss several extensions to the baseline estimates on the extensive margin effects and the political-economic mechanisms of local government leaders. We then report a series of checks for robustness.

A. Pre-decentralization Trends

To summarize the key strategies of causal identification employed in the baseline analysis, we rely on (1) the DD strategy for direct effects on emission outcomes, where the key identifying assumption is that decentralizations should be uncorrelated with potential trends in firm's pollution outcomes; (2) the TD strategy for direct effects on production outcomes, where the key identifying assumption weakens because violations of parallel trends can be allowed, but biases in the two DD estimators in polluting and non-polluting firms should be the same (Olden and Møen, 2022); and (3) the fixed effects model for spillover effects, with the identifying assumption that SOEs' decentralizations are irrelevant to potential outcomes among private firms in the same prefecture. Though the third strategy does not exploit specific identification designs, the assumption is most likely to hold in the sense that decentralizations of SOEs are very unlikely to yield any effects on private firms' potential emissions and production outcomes. In this section, we focus on the validity of the DD and TD assumptions by testing the pre-trends prior to decentralizations.

We first report pre-trend tests for the DD estimates of the direct effects on pollution outcomes. We employ an event study specification by including a full set of indicators denoting relative years to the last year before decentralization, omitting the last year before decentralization as the reference group. As shown in Figure 2, panels (a) and (b), there exist decreasing pre-decentralization trends in waste gas and SO2 emissions prior to decentralization, which suggests we should be cautious while interpreting the direct effects on these emissions. There is no significant pre-decentralization trend for sewage emissions as shown in panel (c) of Figure 2.

Given that the DD estimates are likely biased, we report the pre-decentralization trends for both the polluting firms and non-polluting firms on the production outcomes in the ASIF dataset. As shown in Figure 3, the pre-decentralization trends significantly exist for value-added and employment (columns a and b for Panels A and C) while less significantly for capital stock and TFP (columns a and b for Panels B and D). However, when comparing the pre-decentralization trends between polluting and non-polluting firms, they are very similar for all outcome variables, and the gaps between the pre-trend coefficients in the two groups are statistically insignificant and centered at zero (columns c for Panels A-D). These reassure us of the validity of TD estimates: there are likely biases in the DD estimates but the biases are largely comparable across the third difference: whether the firm is in polluting industries or not. Therefore, the TD estimates as the difference between the two DD estimates remain valid as the biases automatically cancel out.

We only report the pre-decentralization trends for direct effects, but not for spillover effects, for two reasons: first, the specification for spillover effects is different from standard DD or TD specifications (see equations 1.3 and 1.4, where the cumulative number of decentralization events can take values greater than 1), and therefore a regular event study analysis is not applicable; second, the spillover effects are less sensitive to the selection issue because a private firm's potential outcomes are less likely to correlate with a decentralized SOE in the same prefecture, compared to the potential outcomes of the decentralized SOE.

B. Selections in SOE Decentralizations

The validity of our empirical analysis also relies on the assumption that pollution is not a concern in the decision-making process of SOE decentralizations. If the higher level oversight governments are more likely to decentralize SOEs with decreasing emissions, our DD estimates for the direct effects on pollution outcomes will be downward biased; meanwhile, if pollution concern serves as a determinant of SOE decentralization, there would potentially be differential treatment effects for decentralized SOEs in polluting industries even without the change in the organizational structure between local regulators and SOEs. We suggest in section 1.2 that the determinants of SOE decentralization in China are driven by a concern that the SOEs operated at low efficiency and are unlikely to be driven by environmental concerns. We formally check for this concern by replicating the baseline specification in Huang et al. (2017) on the determinants of decentralizations and add pollution variables to the specification to test if pollution is among the main determinants of SOE decentralization.

We report the estimates in Table 1.5. Columns 1 and 3 report our replications to the baseline specifications in Huang et al. (2017), a Probit model estimating how various factors jointly contribute to the determination of decentralization, conditional on province-year level controls (GDP per capita, unemployment rate, SOE output share) and fixed effects at oversight government and year levels. Column 3 further includes 2-digit industry fixed effects. Consistent with Huang et al. (2017), our replication shows that the likelihood of decentralization significantly increases with the distance to the original oversight government, indicating a Hayek conjecture that the lack of local information is the primary concern of the oversight government. Asset stock, return-on-sales ratio, being fully state-owned, and firm importance are negatively correlated with decentralization.[19]

We then include four pollution indicators in the replications: the indicator of polluting industries used as the "third difference" for our TD strategy and firms' logarithmic emissions of waste gas, SO2, and sewage. The estimates with pollution indicators are reported in columns 2 and 4–6 of Table 1.5. The coefficients on the polluting industry indicator, logarithmic waste gas emission, and logarithmic SO2 emission are small in magnitude and not statistically significant. Though the coefficient on logarithmic sewage emission is significantly correlated with decentralization, our estimates only present a small magnitude, indicating that a 100% increase in sewage emission is only associated with a 0.04 percentage point increase in the likelihood of decentralization. Our replications of Huang et al. (2017) suggest that SOE decentralization is largely orthogonal to environmental regulation concerns and therefore plausibly exogenous to the environmental regulators.

C. Extensive Margin Effects

Our conceptual framework provides a simplified discussion to capture the firms' economic costs of environmental regulations assuming the firms are producing the same products and are not exiting. However, the implications may change if firms exit more when faced with increased regulation intensities. Our interpretation of the effects on production outcomes as changes in regulation intensities are also susceptible if the firms produce different products when decentralized. To address this concern, we construct two extensive margin variables as alternative outcomes of interest: first, we construct an exit indicator that equals 1 if the firm exits from the sample in the following year;[20] second, we construct an indicator equaling 1 if the firm switches to a different 4-digit industry in the following year. As 4-digit industry codes are the most disaggregated coding of niche industries under China's classification system, switching across the 4-digit industries serves as a plausible proxy for firms switching to a different product.

We report in Appendix Table A.1.4 that the decentralization of polluting SOEs does not yield any significant effects on decentralized firm exits, nor does it lead to a higher likelihood of producing a different product. As for the spillover effects, an additional polluting SOE being decentralized is associated with a slightly higher likelihood of exiting (0.72 percentage points) among private firms, while there are no significant effects on the likelihood of switching industries for these firms. These

[19] According to Huang et al. (2017), importance is defined as the ratio of the SOE's value-added to the total value-added of the SOEs under the same oversight government in the same 2-digit industry.

[20] One should be cautious when interpreting the exit variable in the sense that the ASIF only includes private firms above a scale threshold. Therefore, exiting from the ASIF sample may mean firm scale falls below the threshold without actually exiting the market. However, this is the best firm exit measurement we can construct from the ASIF.

estimates suggest that the extensive margin outcomes are not likely affected by SOE decentralizations.

D. Effects on Abatement Efforts

As we discuss in the conceptual framework, the economic costs from environmental regulations are determined by the inputs in unproductive factors for abatement. In the baseline results, we interpret the increase in capital and labor without increase in value-added when a polluting SOE is decentralized as an increase in unproductive inputs for pollution abatement. Though this interpretation is not directly testable because the investment or expenses on abatement inputs are not reported in the ESR dataset, we shed light on how decentralization affects firms' abatement efforts by checking the direct and spillover effects on a series of proxies on abatement efforts reported in ESR, similar to He et al. (2020).

We show the estimates in Appendix Table A.1.5. Columns 1–4 report several proxies of firms' adjustments in the production process: total hours of operation per year, logarithmic inputs in water, coal, and petroleum. Additional hours of operation and water and fossil fuels inputs are associated with greater production and discharge of pollutants. Columns 5 and 6 report two proxies for the "end-of-the-pipe" abatement: the abatement-to-emission ratios of SO2 and sewage. The abatement-to-emission ratio is defined as the ratio of the amount of pollutant absorbed by abatement facilities to the amount of pollutant emissions. This ratio quantifies the proportion of total pollutants generated in production that are processed by the abatement facilities, reconciling the issue that abatement facility capacities may not be fully utilized and therefore the abatement capacities may overstate firms' efforts to reduce emissions.

Although most of the estimates are not statistically significant (possibly because of notably smaller sample size and likely more measurement errors in these non-policy-target variables), the signs of all estimates are consistent with the predictions of the conceptual framework. For the decentralized polluting central and provincial SOEs, decentralization leads to a decrease in operating hours, water use, and fossil fuel use, combined with an increase in the proportion of SO2 and sewage processed by the abatement facilities; for the private firms in the same prefecture, the decentralization of polluting SOEs is associated with greater operating hours and pollutive inputs, combined with a decrease in the proportion of SO2 and sewage processed by the abatement facilities. These findings indicate that decentralization of polluting SOEs leads to an increase in abatement efforts of the decentralized SOEs and a decrease in abatement efforts of the private firms in the same prefecture.

E. Political Economies of Local Government Officials

In our discussion of the conceptual framework, there is a key implicit assumption that the objective functions of local regulators are reliant on the political career incentives of prefecture-level government officials, who supervise economic growth and emission control, which jointly contribute to their promotion likelihoods. To better understand how the political promotion incentives motivated local government enforcement of environmental regulations, we exploit variations in the political promotion incentives across different local government leaders.

Following a branch of literature on the meritocracy and career incentives of Chinese local government leaders (Guo, 2009; Wang, 2016; Wang et al., 2020), the career incentives are largely determined by an age-based retirement rule, where prefectural level leaders aged over 56 are ineligible for promotion because they will be unable to finish the next term of five years in the new position before they reach the retirement age of 60. We exploit this discontinuous change in promotion incentives to test whether the pollution and production effects from decentralizations of polluting SOEs are dependent on the political incentives of local government leaders. We digitize the resumes of Chinese prefectural party secretaries (the chief leaders of prefectural governments in China's institutional context) and define the party secretaries as having "strong promotion incentives" if their ages do not surpass 56 in a given year and having "weak promotion incentives" otherwise. As reported in Appendix Tables A.1.6 and A.1.7, the baseline findings are mainly driven by prefectural leaders having "strong promotion incentives," while the direct and indirect effects are negligible and not statistically significant when the prefectural leaders are "un-incentivized" as their ages surpass 56 in a given year. This provides compelling evidence that our baseline findings are driven by the political promotion incentives of the local government leaders.[21]

F. Robustness Checks

We present an additional series of robustness checks to further address concerns over our empirical findings. First, we drop the four provincial-level municipalities and 15 vice-provincial-level prefectures from the sample, whose local governments have significantly greater power than regular prefectures and may face fewer constraints on environmental regulations.[22] The results reported in Appendix Tables A.1.8 and A.1.9 are consistent with Tables 1–4. Second, we report two-way clustered standard errors on both firm (or prefecture, for spillover effects) and industry by year level. The estimates remain statistically significant as reported in Appendix Tables A.1.10 and A.1.11. Third, we drop mining industries and only keep pure manufacturing industries whose 2-digit industry codes are between 13 and 42 to address the concern that mining is usually more

[21] Notably, the size of the low promotion incentive subsample is significantly lower than the high promotion incentive subsample, and the number of decentralization events is also much lower in this sample. Therefore, the sub-sample results for low promotion incentive leaders should be interpreted with caution.

[22] The four provincial level municipalities are Beijing, Shanghai, Tianjin and Chongqing, where the local government leaders are also members of the central committee of CCP. The 15 vice-provincial level prefectures are Shenyang, Dalian, Changchun, Ha'erbin, Nanjing, Hangzhou, Jinan, Qingdao, Ningbo, Xiamen, Wuhan, Guangzhou, Shenzhen, Chengdu and Xi'an. These cities are regional economic centers where the local government leaders are also members of the provincial committee of CCP.

pollutive and less comparable to other industries. The results reported in Appendix Tables A.1.12 and A.1.13 are largely comparable to the baseline result. Fourth, we address the concern that the findings are driven by data quality issues by dropping the sample later than 2009 because (1) it does not include the year 2010 because all financial indicators in the ASIF are missing, and (2) the sample entrance criterion changed in 2011, when the "above scale" threshold rose from 5 million RMB to 20 million RMB. The estimates are unchanged and presented in Appendix Tables A.1.14 and A.1.15. Fifth, we estimate using a subsample excluding firms that exit from the market to address the concern that the estimated productivity effects are driven by the exits of less productive firms. The estimates are reported in Appendix Tables A.1.16 and A.1.17 and are consistent with our baseline estimates, although the sample size is significantly smaller. Finally, we report in Appendix Table A.1.18 a set of robustness checks using alternative estimates of TFP, including the Levinsohn and Petrin (2003) method and residuals from the estimated production functions reported in Yang (2015), which is the first empirical estimate of the industry-specific production functions using the ASIF database of China.

1.7 Implications for Allocative Efficiency

In section 1.6, we report baseline results on the direct effects of the decentralization of central and provincial SOEs. The decentralized SOE decreased its emissions, purchased more capital stock, and decreased in TPF, producing spillover effects where private firms in the same prefecture emitted more and experienced an increase in value-added, capital stock, and total factor productivity. Although these findings report a strong and robust distributional effect that economically injures the decentralized SOEs and benefits the private firms, they also shed limited light on aggregate allocative efficiency. However, the distributional effects intuitively imply an improved efficiency in that emission amounts are reallocated from less efficient SOEs to more efficient private firms.

To formalize this discussion, we first extend the conceptual framework in section 1.3 by assigning a binding credit constraint on private firms to reconcile the limited factor access. We find the economic costs from environmental regulations are greater for firms facing binding credit constraints. We further report a set of empirical results that: (1) spillover effects on private firms are more positive in production and TFP but less positive in capital stock among more financially constrained firms; and (2) the decentralization of polluting central and provincial SOEs is associated with significant decreases in total emissions without significantly decreasing total output or aggregate TFP at the prefecture level. We finally perform a back-of-the-envelope welfare calculation to interpret the magnitudes and welfare implications of our findings.

1.7.1 Conceptual Framework with Credit Constraints

It is well documented that private firms are facing a higher capital wedge than SOEs in China

(Chow and Fung, 1998; Hsieh and Klenow, 2009; Brandt et al., 2013). A stylistic difference occurs in that compared to private firms, SOEs can more easily finance their investment by borrowing from the financial system, a sector highly dominated by state-owned banks. As presented in Hsieh and Klenow (2009), the marginal revenue product of capital (MRPK) and TFP are higher among private firms in China, indicating they are below their optimal level of scale and capital stock if there is no factor misallocation. To reconcile this feature, we extend the conceptual framework by introducing a credit constraint on the private firm: the firm's total purchase on capital, $K + K_E$, should not exceed a constant level $\bar{K}$. With credit constraint, the private firm's optimization question changes to:

$$\max \pi(K, L, K_E) = pf(K, L) - p_K(K + K_E) - p_L L - rE(f(K, L), K_E)\ s.t. K + K_E \leq \bar{K}$$

The relationship between regulation intensity and firm outcomes maintains the same direction as described in propositions 1 and 2 in section 1.3.1. Meanwhile, the binding credit constraint indicates a perfect substitution between productive and abatement capital ($dK + dK_E = 0$). In contrast, if the credit constraint is not binding, the increase in K_E alleviates the drop in the marginal return of K caused by an increase in r and therefore the substitution between K and K_E is imperfect ($\frac{\partial K + K_E}{\partial r} > 0$). This indicates that the economic cost of environmental regulation is greater when the firm faces a binding credit constraint.

Without imposing any functional forms, output, profit, and TFP would be lower when the credit constraint is binding under any given level of regulation intensity r; furthermore, when the Cobb-Douglas production and emission functions are assigned, the firm's productive capital input decreases more rapidly in regulation intensity while the credit constraint is binding, leading to the following proposition that indicates greater economic costs from environmental regulation for firms facing binding credit constraints.

Proposition 3. Output, profit, and TFP decrease more rapidly in regulation intensity if the credit constraint is binding ($\left.\frac{\partial Y}{\partial r}\right|_{\bar{K}} < \left.\frac{\partial Y}{\partial r}\right|_{K^*, L^*, K_E^*} < 0$, $\left.\frac{\partial \pi}{\partial r}\right|_{\bar{K}} < \left.\frac{\partial \pi}{\partial r}\right|_{K^*, L^*, K_E^*}$, $\left.\frac{\partial TFP}{\partial r}\right|_{\bar{K}} < \left.\frac{\partial TFP}{\partial r}\right|_{K^*, L^*, K_E^*}$).[23]

With the Tombe and Winter (2015) style production function transformation, one could further imply that the "marginal product of emission" is smaller but diminishes more slowly with the binding credit constraint ($\left.\frac{\partial Y}{\partial E}\right|_{\bar{K}} < \left.\frac{\partial Y}{\partial E}\right|_{K^*, L^*, K_E^*}$; $\left.\frac{\partial^2 Y}{\partial E^2}\right|_{\bar{K}} > \left.\frac{\partial^2 Y}{\partial E^2}\right|_{K^*, L^*, K_E^*}$). Therefore, compared to a hypothetical scenario where both firms face no credit constraint, the government will allow more emissions from the private firm if it faces a binding credit constraint. Intuitively, this means the

[23] K^*, L^*, K_E^* represent the optimized level of capital, labor and abatement capital when credit constraint is not binding.

government should optimally impose less stringent regulation on firms with higher economic costs from regulation.

1.7.2 Heterogeneous Spillover Effects by Financial Constraint

We report in section 1.6.2 that one additional central or provincial SOE being decentralized yields spillovers on private firms in the same prefecture, as the local regulators could regulate the decentralized SOE more stringently and re-allocate more emission amounts to the private firms. As predicted in section 1.7.1, the decreased regulation intensity should yield greater economic gains for firms facing more binding credit constraints.

We empirically test this hypothesis by interacting the spillover effects indicators with three constructed measurements of firms' financial constraints following Krozner et al. (2007) and Manova and Yu (2016): the opposite of firms' lagged liquidity, firms' lagged leverage, and the average external finance dependence at 2-digit industry level.[24] We interact the three measurements with the cumulative numbers of decentralizations for all SOEs and polluting SOEs in the spillover specification, and the results are reported in Table 1.6 for the opposite of lagged liquidity and Appendix Tables A.1.19 and A.1.20 for the other two measurements.

The findings from the three tables are highly consistent: more financially constrained firms received greater positive spillovers on value-added and TFP from the decentralization of polluting SOEs but less positive spillovers on capital stock. These findings are highly consistent with the predictions from the conceptual framework that if the credit constraint is more binding, a firm will be less able to increase total capital stock when environmental regulations become less stringent, but the replacement between productive and abatement capital stocks has a higher economic return compared to firms with non-binding financial constraints.[25]

1.7.3 Aggregate Effects of Decentralization

The conceptual framework discussed here is highly simplified to explain intuition regarding firms' differential economic costs of environmental regulation and the local regulators' emission quota allocations across firms in response to the regulatory constraints on central and provincial SOEs. Because of this simplicity, the model is insufficient for predicting the effects of decentralization on aggregate outcomes. However, this is of apparent importance when interpreting what decentralization of polluting SOEs implies for allocative efficiency. We are especially interested in the question of whether there will be a decrease in total emissions at the prefecture level following the decentralizations of polluting SOEs, and furthermore, whether the positive

[24] Liquidity = (current assets – current liabilities)/total assets; Leverage = current liabilities/current assets.
[25] These findings are consistent to Fan et al. (2022), who also report differential financial market access as a key mechanism explaining differential environmental regulation effects among Chinese SOEs and private firms.

productivity spillovers on private firms can offset the negative direct productivity effects. To further illuminate the aggregate welfare effects of removing this regulation constraint, we aggregate the ASIF and ESR datasets at prefecture by year level, and estimate the following specifications:

$$P_{pt} = \alpha_p + \gamma_t + X_p \times \delta_t + \beta N(Dece, ESR)_{pt} + \varepsilon_{pt} \quad (1.5)$$

$$Y_{pt} = \alpha_p + \gamma_t + X_p \times \delta_t + \beta_1 N(Dece, ASIF, all)_{pt} + \beta_2 N(Dece, ASIF, pol)_{pt} + \varepsilon_{pt} \quad (1.6)$$

In equation 1.5, P_{pt} represents prefecture-by-year level total emission amount of waste gas, sewage, and SO2; in equation 1.6, Y_{pt} represents prefectural level aggregate industrial output and aggregate TFP. The treatment variables are the same as in equations 1.3 and 1.4, which are aggregated at prefecture by year level. We control for prefecture fixed effects, year fixed effects, and year fixed effects interacted by prefectural level characteristics in 1998, as described in section 1.5.2; we cluster the standard errors at the prefecture level.

The estimates from equations 1.5 and 1.6 are reported in Table 1.7. Columns 1–3 report estimates from the aggregate sample of ESR, while columns 4 and 5 report estimates from the aggregate sample of ASIF. At the prefecture by year level, an additional polluting SOE decentralized from the central or provincial levels decreases 7.20% of the prefecture's total waste gas emission, 10.71% of total SO2 emission, and 8.58% of total sewage emissions. The magnitudes are smaller than the direct effect, indicating that part of the direct effect is offset by the positive spillover. The magnitudes of emission reductions are comparable to a separate study on the effects of the new EPL evaluated at the prefecture level and reported in appendix A.1.a, as the new EPL removed the regulation constraint on central and provincial SOEs without affecting other political and economic ties between the local governments and central and provincial SOEs, which is less susceptible to the selection of decentralizations and confounding financial effects of decentralizations.

Columns 4 and 5 report estimates of aggregate productivity. The estimates are not statistically significant, and the small magnitudes of the coefficients indicate that the decentralization of polluting SOEs does not harm production efficiency. Even when ignoring the lack of statistical significance, the aggregate effects of an additional decentralized polluting SOE are -1.65% for total industrial output and -3.55% for aggregate TFP, which are notably smaller in magnitude than the direct effects on productivities or the aggregate effects on pollution outcomes.

1.7.4 Back-of-the-Envelope Welfare Computation

Our baseline estimates suggest that when a central and provincial SOE is decentralized to the prefectural level, its TFP decreases by 21.51%, whereas its emissions of waste gas, SO2, and sewage decrease by 47.45%, 63.00%, and 45.16%, respectively. This change is combined with a spillover effect where the private firms in the same prefecture increased their TFP by 2.50%, and emissions of waste gas, SO2, and sewage by 4.34%, 4.74%, and 5.51%, respectively.

Leveraging the estimated coefficients and a simple re-weighting technique developed by He et al. (2020) to adjust for sample criteria differences in ASIF and ESR, we evaluate the average economic cost for pollution reduction as the marginal rate of substitution (MRS) between TFP and emissions. As reported in Table 1.8, among central and provincial polluting SOEs, a 10% reduction in sewage emission is associated with a 5.51% reduction in TFP, indicating the economic losses from pollution reduction. Similarly, the economic cost of a 10% reduction in SO2 is 4.45% of TFP, and the economic cost of a 10% reduction in sewage is 5.72% of TFP. Consistent with section 1.7.1, the economic costs of emission reductions are higher among private firms: the economic costs of a 10% decrease in waste gas, SO2, and sewage are 11.68%, 10.68%, and 9.15% of TFP, respectively.

In Table 1.9, we further report a back-of-the-envelope welfare computation to help understand the magnitudes of our findings and the implications on aggregate economic costs of pollution control. Consider a hypothetical experiment where the central and provincial SOEs reduce emissions by 10%, and the emission quotas saved from central and provincial SOEs are reallocated to private firms. The reallocation of emissions reduces the outputs of central and provincial SOEs but increases private firms' outputs following the MRS estimates reported in Table 1.8. On aggregate, reallocating emissions from central and provincial SOEs to private firms leads to an increase in total output of 0.74–3.31%.

Taken together, our findings suggest decentralizing polluting SOEs would decrease the aggregate economic costs of pollution reduction by reallocating emissions across the state-owned and private sectors. This implication for aggregate efficiency is meaningful not only to the overall policy implications of SOE decentralizations but also elucidates the aggregate efficiency effects of removing a regulatory capture that intertwines with pre-existing misallocation. The aggregate implications further indicate that the regulatory capture that central and provincial SOEs were exempt from environmental regulations induces a distortion in the allocation of regulation efforts across firms. Removing such distortion not only leads to an improvement in environmental outcomes but also involves a lower economic cost. At the firm level, pollution reduction is costly, reflected by decreased TFP of decentralized SOEs; but at the aggregate level, pollution reduction can be costless by removing the aforementioned distortion, in that the positive productivity spillovers on private firms offset the negative direct effects.

1.8 Conclusion

1.8.1 Findings and policy implications

This study documents a real-world regulatory capture scenario where China's central or provincial SOEs were exempt from environmental regulations enforced by local governments. This exemption further forces the local regulators to more stringently exercise environmental regulations on other firms to achieve total pollution targets. We exploit the decentralizations of central and provincial SOEs as plausibly exogenous shocks in the regulatory relationship between local governments and polluting SOEs to identify the direct and spillover effects when this exemption is removed.

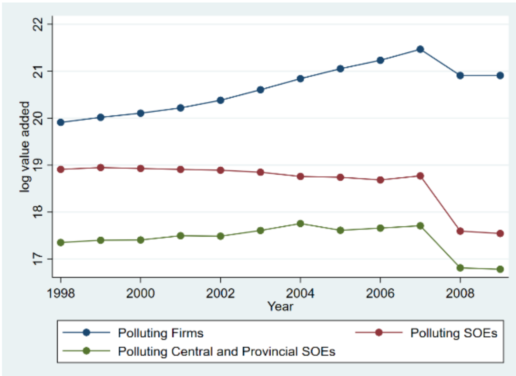
Following decentralization, a polluting SOE experiences a decrease in pollution amount and productivity, combined with an increase in unproductive capital stock aimed for abatement. Meanwhile, decentralization of polluting SOEs creates positive spillovers on private firms in the same prefecture, allowing them to emit more pollutants and gain higher value-added and productivity. We further document that among the private firms, the ones with greater financial constraints increased more in value-added and TPF but increased less in capital stock, which indicates that the economic costs of emission reduction are higher if the firms are facing a more binding credit constraint. Aggregated to the prefecture level, the decentralization of a polluting central or provincial SOE leads to a decrease in total emissions without a significant decrease in aggregate industrial output or productivity. When hypothetically reallocating 10% of emissions from central and provincial SOEs to private firms, we calculate total industrial output gains of 0.74–3.31%.

The message of this paper can be extended to a more general context. Although the importance of state economy and SOE decentralization is unique to China, regulatory captures and selective regulatory enforcement are not uncommon globally. Our findings highlight an efficiency loss from regulatory capture when it intertwines with pre-existing factor misallocation: in reaction to differential difficulties to regulate different firms, regulators may impose more stringent regulations on firms easier to control, instead of the firms bearing lower economic costs of regulation, a clear violation from the social optimum. This paper also reminds policymakers who widely favor firms or industries for political concerns, exempting them from regulations, that there may be unintended consequences that these discretions make regulations more costly in the aggregate sense.

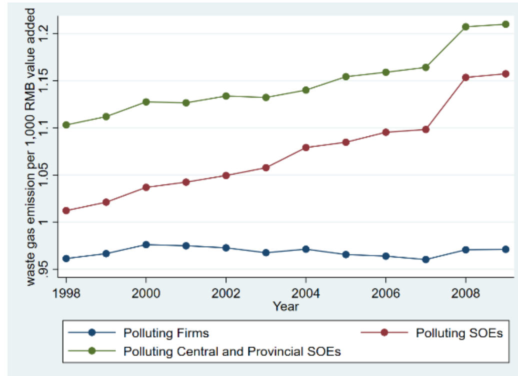
1.8.2 Relevance to other chapters

To conclude, Chapter 1 provides an in-depth analysis of the complex nature of policy enforcement in China, where multiple levels of government and state-owned enterprises can overlap and create intricate challenges for local policy enforcement. While this chapter represents the most complex case study in this dissertation, it is important to note that simpler cases are equally significant for two reasons. First, they provide a useful benchmark for comparison, enabling us to understand the effectiveness and efficiency of different policy enforcement modes. Second, many public policies in China are implemented in simpler modes, where the local governments face less constraints from the overlapping nature of the system, or the central government has more direct control over the implementation process, which will be explored in Chapters 2 and 3. Overall, this chapter highlights the importance of considering the unique political and institutional context of China when analyzing policy enforcement, which is a theme that continues to be explored in the subsequent chapters of this dissertation.

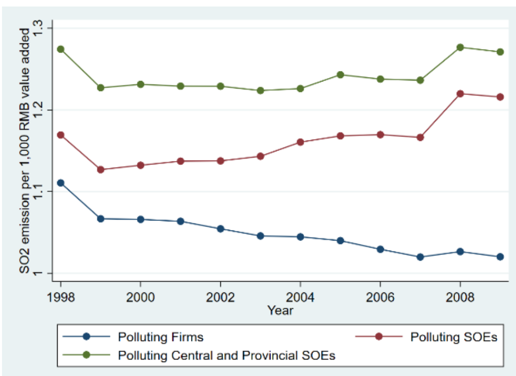
Figure 1.1 Value-added and Emission Intensities of China's Polluting Firms



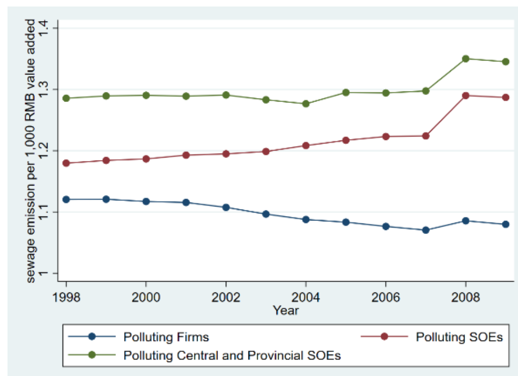
Panel A. Value-added



Panel B. Waste gas per 1,000 RMB value-added



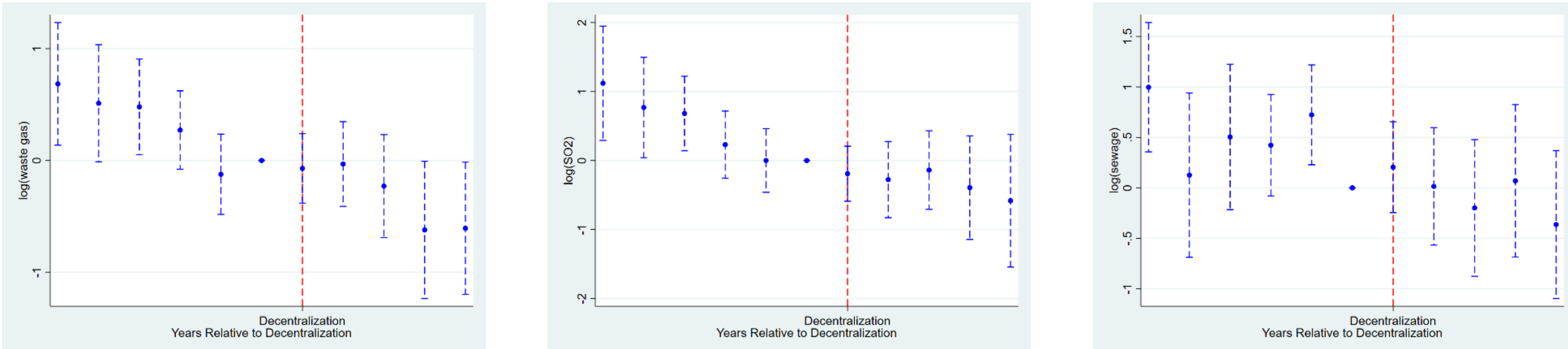
Panel C. SO2 per 1,000 RMB value-added



Panel D. Sewage per 1,000 RMB value-added

Notes: The figures report the dynamics in industrial output and pollution intensities in China's polluting firms. Panel A shows the trend of total value-added of China's polluting firms by all firms, SOEs and central and provincial SOEs. Panel B, C and D show the trend of emissions per 1,000 RMB value-added in waste gas, SO2 and sewage.

Figure 1.2 Event Studies: Direct Effects on Pollution Outcomes



(a) log(waste gas)

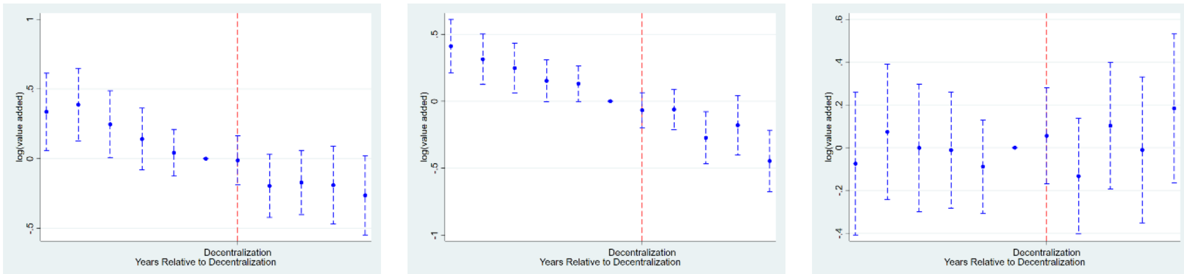
(b) log(SO2)

(c) log(sewage)

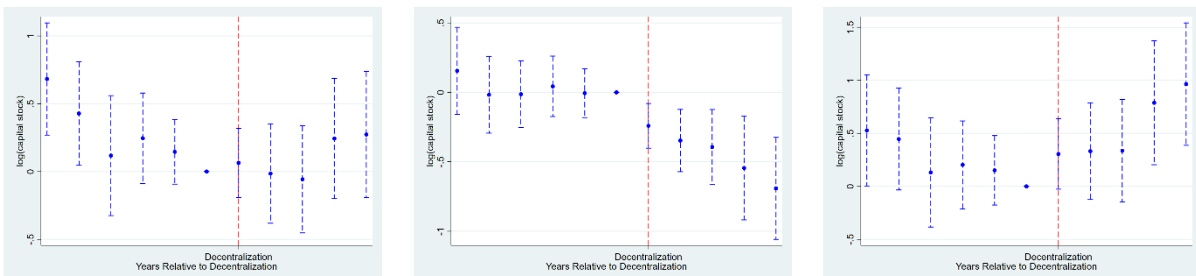
Notes: The figures visualize the DD pre-trends for the direct decentralization effects on pollution outcomes. We illustrate the estimated coefficients with the 95% confidence intervals of a full set of time dummies indicating relative years to decentralization. The last year before decentralization is omitted as the reference group.

Figure 1.3 Event Studies: Direct Effects on Pollution Outcomes, by Polluting Indicator

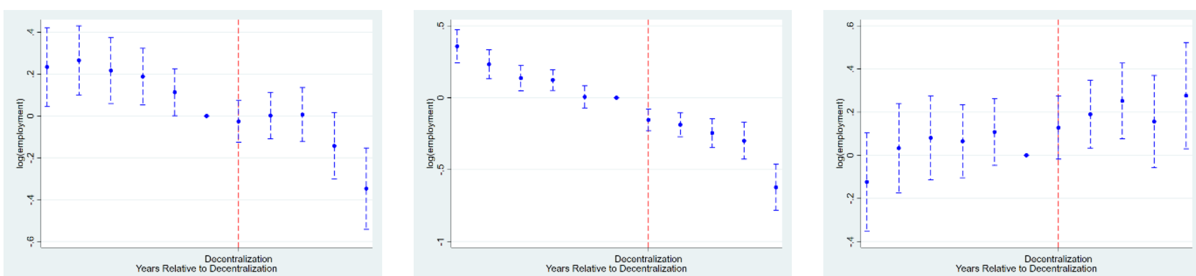
(a)Polluting DD (b) Non-polluting DD (c) TD



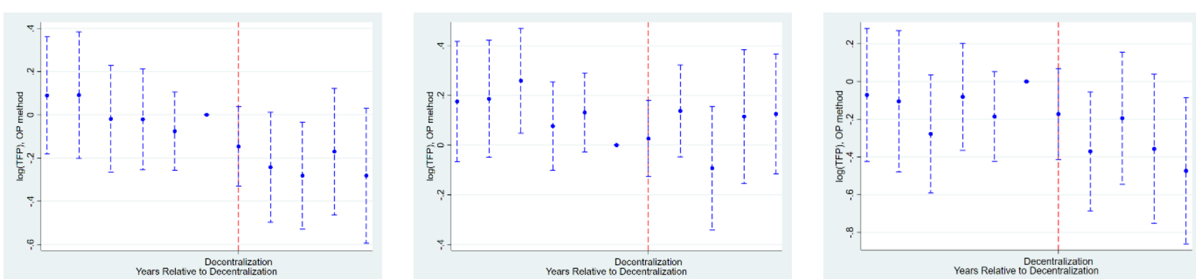
Panel A. Value-added



Panel B. Capital stock



Panel C. Employment



Panel D. TFP

Notes: The figures visualize the DD pre-trends for the direct decentralization effects on production outcomes by polluting and non-polluting firms, and the gaps between the two pre-trends (the TD pre-trends). We illustrate the estimated coefficients with the 95% confidence intervals of a full set of time dummies indicating relative years to decentralization. The last year before decentralization is omitted as the reference group.

Table 1.1 The Direct Effects of Decentralization on Pollution Outcomes

	(1)	(2)	(3)
Sample		ESR	
Dependent Variable	log(waste gas)	log(SO_2)	log(sewage)
Decentralized to Prefecture, Polluting Firms in ESR	-0.4745***	-0.6300**	-0.4516**
	(0.155)	(0.267)	(0.205)
Firm FE	YES	YES	YES
Prefecture-Year FE	YES	YES	YES
Industry-Year FE	YES	YES	YES
Observations	531,665	531,665	531,665

Notes: This table reports the direct effects of decentralization on firms' pollution outcomes. Robust standard errors are clustered at firm level and reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 1.2 The Direct Effects of Decentralization on Production Outcomes

	(1)	(2)	(3)	(4)
Sample	ASIF			
Dependent Variable	log(value-added)	log(capital stock)	log(employment)	log(TFP, OP)
Decentralized to Prefecture, ASIF	-0.3199***	-0.4438***	-0.3645***	-0.0238
	(0.062)	(0.093)	(0.040)	(0.072)
× Polluting Industry	0.0451	0.3965**	0.1444**	-0.2151**
	(0.101)	(0.184)	(0.061)	(0.106)
Firm FE	YES	YES	YES	YES
Prefecture-Year FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
Observations	2,945,013	2,529,237	2,945,013	2,529,237

Notes: This table reports the direct effects of decentralization on firms' production outcomes. Robust standard errors are clustered at firm level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.3 The Spillover Effects of Decentralization on Pollution Outcomes

	(1)	(2)	(3)
Sample		ESR, private firms	
Dependent Variable	log(waste gas)	log(SO2)	log(sewage)
N(Dece, ESR)	0.0434***	0.0474***	0.0551***
	(0.010)	(0.013)	(0.012)
Firm FE	YES	YES	YES
Prefecture Characteristics × Year FE	YES	YES	YES
Industry-Year FE	YES	YES	YES
Observations	379,231	379,231	379,231

Notes: This table reports the spillover effects of decentralization on firms' pollution outcomes. N(Dece, ESR) is constructed as the cumulative number of events that a polluting SOE in the ESR database is decentralized to prefecture p by year t. Prefecture controls include logarithm of population, non-agriculture population, GDP and total industrial output in 1998, plus several indicators of industrial structural – the share of first and second industries in GDP, and the share of industrial output from pollutive industries in 1998. Robust standard errors are clustered at prefecture level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.4 The Spillover Effects of Decentralization on Production Outcomes

	(1)	(2)	(3)	(4)
Sample		ASIF, private firms		
Dependent Variable	log(value-added)	log(capital stock)	log(employment)	log(TFP, OP)
N(Dece, ASIF, all)	-0.0113***	-0.0099***	-0.0020***	-0.0081***
	(0.001)	(0.001)	(0.001)	(0.001)
N(Dece, ASIF, pol)	0.0389***	0.0255***	0.0147***	0.0250***
	(0.003)	(0.004)	(0.002)	(0.003)
Firm FE	YES	YES	YES	YES
Prefecture Characteristics × Year FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
Observations	2,377,652	2,044,561	2,377,652	2,044,561

Notes: This table reports the spillover effects of decentralization on firms' pollution outcomes. N(Dece, ASIF, all) is constructed as the cumulative number of events that all types of SOEs in the ASIF database being decentralized to prefecture *p* by year *t*; N(Dece, ASIF, pol) is constructed as the cumulative number of events that an SOE in polluting industry being decentralized to prefecture *p* by year *t*. Prefecture controls include logarithm of population, non-agriculture population, GDP and total industrial output in 1998, plus several indicators of industrial structural – the share of first and second industries in GDP, and the share of industrial output from pollutive industries in 1998. Robust standard errors are clustered at prefecture level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.5 The Determinants of Decentralization

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Decentralized in the following year					
Pollution Indicator		Polluting Industry		log(waste gas)	log(SO2)	log(sewage)
log (distance to oversight government)	0.0031***	0.0031***	0.0029***	0.0026***	0.0026***	0.0026***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
log (asset stock)	-0.0010***	-0.0010***	-0.0011***	-0.0003	-0.0002	-0.0001
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
ROS	-0.0034	-0.0034	-0.0039*	-0.0060**	-0.0058**	-0.0056**
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
Importance	-0.0011	-0.0011	-0.0021	-0.0016	-0.0014	-0.0010
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
Fully state-owned	-0.0023**	-0.0023**	-0.0018*	-0.0030**	-0.0030**	-0.0032**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Polluting Indicator		0.0002		0.0001	-0.0001	-0.0004***
		(0.002)		(0.000)	(0.000)	(0.000)
Province controls	YES	YES	YES	YES	YES	YES
Government FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Industry FE			YES	YES	YES	YES
Observations	80,334	80,334	80,334	28,483	28,483	28,483

Notes: This table replicates the baseline specification from Huang et al. (2017). The coefficients report marginal effects from probit regressions. Province controls include province-year level GDP per capita, unemployment rate and share of SOE in industrial output. Standard errors clustered at oversight government level are reported in the parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.6 The Heterogeneous Spillover Effects by Financial Constraints

	(1)	(2)	(3)	(5)
Sample	ASIF, private firms			
Dependent Variable	log(value-added)	log(capital stock)	log(employment)	log(TFP, OP)
N(Dece, ASIF, all)	-0.0091***	-0.0086***	-0.0018***	-0.0056***
	(0.001)	(0.001)	(0.001)	(0.001)
× –(Liquidity$_{t-1}$)	-0.0008**	0.0060***	-0.0001	-0.0020***
	(0.000)	(0.001)	(0.000)	(0.000)
N(Dece, ASIF, pol)	0.0321***	0.0214***	0.0143***	0.0173***
	(0.003)	(0.005)	(0.002)	(0.003)
× –(Liquidity$_{t-1}$)	0.0043***	-0.0113***	0.0001	0.0059***
	(0.001)	(0.002)	(0.001)	(0.001)
Liquidity$_{t-1}$	0.0036**	-0.0196***	-0.0008	0.0073***
	(0.001)	(0.002)	(0.001)	(0.001)
Firm FE	YES	YES	YES	YES
Prefecture Characteristics × Year FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
Observations	1,848,305	1,568,704	1,848,305	1,568,704

Notes: This table reports the heterogeneous spillover effects of decentralization on firms' pollution outcomes by lagged liquidity standardized within prefecture-year grids so that their means equal zero and their standard deviations equal one. N(Dece, ASIF, all) is constructed as the cumulative number of events that all types of SOEs in the ASIF database being decentralized to prefecture *p* by year *t*; N(Dece, ASIF, pol) is constructed as the cumulative number of events that an SOE in polluting industry being decentralized to prefecture *p* by year *t*. Prefecture controls include logarithm of population, non-agriculture population, GDP and total industrial output in 1998, plus several indicators of industrial structural – the share of first and second industries in GDP, and the share of industrial output from pollutive industries in 1998. Robust standard errors are clustered at prefecture level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.7 The Aggregate Effects of Decentralization at Prefecture-Year Level

	(1)	(2)	(3)	(4)	(5)
Sample	Prefecture-Year Aggregate from ESR			Prefecture-Year Aggregate from ASIF	
Dependent Variable	log(total waste gas)	log(total SO2)	log(total sewage)	log(total output)	log(aggregate TFP)
N(Dece, ESR)	-0.0720*	-0.1071***	-0.0858***		
	(0.039)	(0.030)	(0.033)		
N(Dece, ASIF, all)				0.0021	0.0200
				(0.007)	(0.015)
N(Dece, ASIF, pol)				-0.0165	-0.0355
				(0.024)	(0.051)
Firm FE	YES	YES	YES	YES	YES
Prefecture Characteristics × Year FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Observations	2,705	3,554	3,553	3,396	3,367

Notes: This table reports the aggregate effects of decentralization on prefecture-year level pollution and productivity outcomes. N(Dece, ESR) is constructed as the cumulative number of events that a polluting SOE in the ESR database is decentralized to prefecture p by year t. N(Dece, ASIF, all) is constructed as the cumulative number of events that all types of SOEs in the ASIF database being decentralized to prefecture p by year t. N(Dece, ASIF, pol) is constructed as the cumulative number of events that an SOE in polluting industry being decentralized to prefecture p by year t. Prefecture controls include logarithm of population, non-agriculture population, GDP and total industrial output in 1998, plus several indicators of industrial structural – the share of first and second industries in GDP, and the share of industrial output from pollutive industries in 1998. Robust standard errors are clustered at prefecture level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.8 The Marginal Rates of Substitution between TFP and Emissions

MRS(TFP, emission)	Central and Provincial SOEs	Private firms
Emission = waste gas	0.551	1.168
Emission = SO2	0.445	1.068
Emission = sewage	0.572	0.915

Notes: This table reports estimates of the marginal rates of substitution between TFP and three types of emissions constructed from the baseline estimates on the direct and spillover effects of decentralizations. The computation follows a technique developed by He et al. (2020) to reconcile the differential sampling criteria in ASIF and ESR.

Table 1.9 The Economic Effects of Reallocating Emissions

	Percentage change in value-added		
Emission type	Central and provincial SOEs	Private firms	Aggregate
Waste gas	-5.51%	4.73%	3.31%
SO2	-4.45%	3.76%	2.62%
Sewage	-5.72%	1.69%	0.74%

Notes: This table reports computations on the effects of decreasing emission amounts by central and provincial SOEs by 10% and reallocating the same emission amounts to private firms based on the MRS reported in Table 1.1.8.

2 Career incentives of local leaders and crisis response: A case study of COVID-19 lockdowns in China[26]

2.1 Introduction

2.1.1 Relevance to the dissertation research

As outlined in the abstract of this dissertation, the three chapters comprise a body of literature that examines different modes of policy enforcement in China, a crucial aspect of governance that is significantly influenced by political and institutional forces. While China features strong state capacity but weak rule of law, the local governments enjoy considerable flexibility in implementing public policies in their jurisdictions. In Chapter 1, I depict how the enforcement of policies by local governments may be influenced by the overlapping branches belonging to the higher levels of the hierarchy. However, one should never underestimate the influence of local governments in policy enforcement in China. In this chapter, I examine how local government leaders' career incentives affect their enforcement of China's COVID-19 disease control policies. We highlight how promotion incentives for local leaders can lead to the erosion of the central government's call for resolute measures against the transmission of COVID-19. Chapter 2 builds upon these insights to investigate a more common model of policy enforcement in China, where the central government initiates a general policy target, but local governments have significant flexibility in enforcement.

In this sense, the chapter examines a real-world example of a very prevalent model of policy enforcement in China, where the central government sets general policy targets, and local governments have considerable discretion in their implementation. In this example, the central and local governments revealed different incentives in controlling the spread of COVID-19. At the onset of the pandemic, most local leaders hesitated to impose lockdowns as their promotions depended on achieving strong numbers for economic growth in their regions, which could be suppressed by such measures. However, when the nation's top leader warned that local leaders who failed to control the disease would be removed from office, many rapidly implemented resolute measures. Nonetheless, our analysis reveals that local leaders with stronger promotion incentives were still more likely to downplay the virus by avoiding or minimizing lockdowns. The findings underscore how local politicians may be incentivized to act slowly during crises, undermining the central government's objectives in critical public policies. The three chapters of my dissertation discuss three different modes of public policy enforcements in China, each

[26] This chapter is based on my previously published work acknowledged as Chen et al. (2022) in the dissertation.

featuring varying roles of the central and local governments. These findings shed light on the complex dynamics of policy enforcement in China's unique political and institutional context and have important implications for policymakers seeking to improve the effectiveness of governance.

2.2.2 Context and literature review

Local leaders in China play a central role in providing public goods, managing socio-economic affairs, and therefore in dealing with major crises. However, in many large-scale crises, such as the recent COVID-19 pandemic, even though resolute actions by local leaders, such as lockdowns, would create positive externalities for other jurisdictions and would benefit the nation as a whole, local bureaucrats in many countries were accused of being slow to act because of political career concerns.[27] Despite the torrent of studies on the consequences of COVID-19 and the widely implemented lockdown policies (e.g., Fang et al., 2020), research on the political economy of government interventions during the pandemic is still scant.

This paper aims to advance our understanding of this issue by studying the role of career incentives in the response of local Chinese leaders to the COVID-19 pandemic. During the first stage of the outbreak of the coronavirus, Chinese prefectural party secretaries (henceforth referred to as prefectural leaders) responded to the exponentially growing number of cases with only sluggish interventions, since economic performance within their jurisdictions is a key performance indicator (KPI) in upper-level officials' evaluation of their promotion. Thus, local leaders had intrinsic career incentives to minimize anti-epidemic measures for fear of harming the local economy. We first use cross-sectional regressions to reveal the impacts of local leaders' promotion incentives on their lockdown decisions. Our measurement of prefectural party secretaries' promotion incentives is borrowed from Wang et al. (2020), which is the ex-ante probability of promotion predicted by using their inauguration age and political hierarchy level. We find that Chinese prefectural leaders with larger career incentives were more reluctant to implement lockdowns during the COVID-19 pandemic.

However, most prefectural leaders swiftly changed their stance after the nation's top leader, Xi Jinping, sternly warned that failure to control the disease could hamper their career advancement and potentially lead to their removal from office, making pandemic response a *de-facto* "one-item-veto" task for their promotion.[28] Most prefectural leaders quickly responded to Xi's warning by

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[27] See https://www.voanews.com/europe/britain-was-too-slow-act-covid-19-opposition-labor-leader-says, https://www.bbc.com/news/world-asia-india-56771766, and https://www.medicalnewstoday.com/articles/eu-slow-inefficient-and-hampered-by-bureaucracy-in-early-covid-19-response for examples of such discussion.

[28] The "one-item-veto" task means that once you fail to meet the minimum requirements for this item, you will not be eligible for promotion, even if you perform well at other tasks like facilitating economic growth. Under this mechanism, over-fulfilling the specified "one-item-veto" task will not help with local leaders' career advancement, but under-fulfillment will have severe career consequences.

implementing resolute lockdown measures. Despite this general pattern, we document an interesting finding that prefectural leaders with larger career incentives were still more willing to put public health at risk by minimizing lockdowns. We construct prefecture-week-level panel data to unearth the interaction of Xi's warning and local Chinese leaders' career incentives on their anti-pandemic policy measures. On the extensive margin, we find that the pattern depicted in the cross-sectional regression remains: a one-standard-deviation increase in a prefectural leader's career incentive reduced the probability of locking down the prefecture after Xi's speech by over 11 percentage points during our sample period. On the intensive margin, using within-prefecture human mobility (offered by the Baidu migration index) to proxy for lockdown stringency as in Fang et al. (2020), we find that a prefecture's lockdown measure was significantly laxer if its party secretary had a larger career incentive measured by the ex-ante probability of promotion predicted by using their inauguration age and political hierarchy level.

One rationalized explanation of Chinese prefectural leaders' behavior is that they might have formed the belief from their experience with SARS that the importance of pandemic control to promotion, as emphasized by the central government, was only temporary, and that after the black swan event, economy-related factors would resume a decisive role in their career advancement. To investigate this, we first show that the tradeoff facing local leaders, between pandemic control and developing the local economy, also existed during the SARS (severe acute respiratory syndrome) epidemic in 2003, where leaders with larger promotion incentives also tended not to order lockdowns. In addition, we find that prefectural governments were more likely to order looser COVID-19 lockdowns if their leaders had experienced lockdowns during SARS, which in general were substantially laxer than those imposed during COVID-19 due to the difference in infection rate and spreading speed of the two viruses. In addition, our data show that no prefectural party secretary incumbent during SARS was actually punished for imposing loose lockdown measures. The evidence documented above supports our interpretation that leaders who had experienced SARS lockdowns might have formed the belief that they could downplay this new virus, just as former prefectural leaders had done during SARS.

Our findings highlight the difference in crisis responses implemented in China and other countries. In democracies, local leaders might hesitate to take crisis-response actions if concerned that such actions would be unpopular to voters; in authoritarian states like China, where their evaluation by upper-level officials is based on KPIs, such as their jurisdictions' economic growth (Maskin et al., 2000; Li and Zhou, 2005; Xiong, 2018; Wang et al., 2020), local leaders can be reluctant to act if they fear that such policy changes would negatively affect their career advancement.

There has been debate over the uneven effectiveness of the provision of public goods and services in centralized vs. decentralized organizations (e.g., Seabright, 1996, Bailey and Rom, 2004, Dahlberg and Edmark, 2008). The phenomenon exposed in our paper, where in order to compete with their peers for promotion, local Chinese leaders tended to trade public health for better economic growth to be promoted, sheds light on this by emphasizing the potential pitfalls of dealing with large-scale crises that have huge cross-regional externalities in a fully decentralized manner.

This paper joins the literature on the theory of self-interested bureaucrats (Tullock, 1965; Niskanen, 1971; Egeberg, 1995; Finan et al., 2017). Like most other people, bureaucrats pursue private interests, which can conflict with their role as public decision-makers to do a public favor. This situation would become even worse in the absence of local accountability and monitoring within hierarchies. We provide a novel empirical case to reveal local leaders' self-interest motivations in China. The paper most related to ours is Pulejo and Querubín (2021), which uncovers that, in democratic countries, incumbents eligible for re-election implemented less-stringent anti-pandemic measures when elections were imminent.[29] Compared to their study, ours focuses on the behavior of local government officials, who in many countries have more authority over public health than the federal government, including deciding whether to issue lockdown orders.[30] We also relate to the scholarship on how career incentives determine bureaucrats' performance (Alesina and Tabellini, 2007; Che et al., 2017; Hillman, 2019; Khan et al., 2019; Bertrand et al., 2020).

2.2 Background

2.2.1 Local leaders' incentives in China's bureaucratic system

A striking feature of China's unique bureaucratic system is that local leaders are evaluated and promoted by the level of government above them based on their performance during their tenure and several other considerations (Maskin et al., 2000; Li and Zhou, 2005; Xu, 2011; Shih et al., 2012). Therefore, the Chinese regime features complicated interactions between the central and subnational governments. On the one hand, local leaders have great power in managing socio-economic affairs in their jurisdictions. For example, prefectural governments have great flexibility when implementing policies such as locking down the cities to combat COVID-19 without direct interventions from the central or the provincial leadership. On the other hand, lower-level officials are effectively incentivized to implement policies suitable for the objectives set by upper-level

[29] In other related literature, Fisman et al. (2021) find that Chinese local governments may respond to citizen concerns in order to minimize dissent when they decide on reopening.

[30] For example, among the Group of Twenty countries, Australia, Brazil, Canada, Germany, Indonesia, and United States allowed regional governments rather than the federal government to order lockdowns, causing significant variation within the same country. Please see https://www.cfr.org/in-brief/comparing-coronavirus-lockdowns-federal-local-divide for details.

officials, e.g., reforms and economic development (Xu, 2011).

A famous example of this feature is the "tournament" for economic growth, where local leaders compete for better economic growth to be promoted. In this sense, local leaders' career advancement appears to be determined by this merit promotion system where economic growth is a key factor, and they are accountable to the upper-level officials. Another example is the implementation of lockdowns during the COVID-19 pandemic. As shown later in this paper, prefectural officials' attitudes towards the virus's outbreak drastically changed after the nation's top leader stipulated them to fight against it. This contrasts with the situations in other countries such as the United States, where state governors can and frequently would reject the president's orders on pandemic controls[31] because they come to their positions through elections rather than being appointed by upper-level officials like the president. As a result of this political personnel control mechanism, China's local leaders tend to strategically ignore or downplay some tasks not closely related to their promotion (Xu, 2011).

Besides the key indicators set by the upper-level officials such as economic growth, local leaders' career advancement also relies on other mechanisms such as nepotism and corruption, the importance of which has been stressed by several studies on China's political selection system. Shih et al. (2012) imply that patronage (i.e., the connections to higher-level leaders) determines the likelihood of promotion. Jia et al. (2015) suggest that connections and performance are complements in China's political selection system.[32] Kahana and Liu (2010) base their analysis on payments for promotion to superiors in the bureaucratic hierarchy. We do not consider whether nepotism is important in China's political promotion mechanism. Instead, we follow the procedure common in the literature of viewing a jurisdiction's economic performance, an observed signal of the local leader's ability, as among the most important promotion assessment criteria.[33] At the same time, we further control through a set of proxies for patronage and corruption in the empirical section to eliminate the potential confounding effects of nepotism.

It is noteworthy that there are two core leaders at the prefecture level, namely the prefectural party secretary and the mayor, both of which are appointed by their supervisor (the provincial government) through the Chinese Communist Party (CCP) system. In each prefecture, the party secretary heads the prefecture's party oversight bureaucracy, oversees the jurisdiction's

[31] Please see https://www.politico.com/news/2020/11/13/republican-governors-reject-biden-mask-orders-436385 for an example.

[32] As an anecdotal support to this view, a local officer from Gansu Province confessed that both job performance and connection contribute almost equally to the promotion. Please see http://news.sohu.com/20130516/n376141310.shtml for the discussion.

[33] The nepotism literature does not necessarily falsify the findings on meritocracy. Instead, most papers on nepotism acknowledge the role of performance and abilities. For example, Kahana and Liu (2010) models promotion prospects jointly depending on bribes and personal ability.

government, and has the final say on imposing major policies. Enlisted as the vice-secretary of the prefecture's CCP committee, the mayor heads the prefectural government and thus plays a subordinate role in making important decisions. Therefore, we use the prefecture's party secretary rather than its mayor as the primary leader in the empirical setting, but we also consider the career incentive of the mayor in case the two leaders jointly decide on local governance.

2.2.2 The spread of COVID-19 and the political concerns underlying Chinese prefectural leaders' implementation of lockdown policies

According to China's official statistics, the first COVID-19 case was diagnosed in the city of Wuhan in Hubei Province on January 11, 2020.[34] While the virus spread quickly to other prefectures, Chinese local governments initially responded with only sluggish interventions. Indeed, by February 2, 2020, only 13 prefectures had imposed lockdowns, while around 250 were already hit by the virus.

Lockdown decisions in China were made almost exclusively by local governments (in most cases prefectural governments). As specified in the Law of the People's Republic of China on the Prevention and Treatment of Infectious Diseases, "In the event of an outbreak, or the prevalence of an infectious disease, the local government shall immediately get people organized to control it and cut off the route of transmission".[35]

There are at least two reasons for the delayed response of prefectural governments in the early stage of the pandemic. First, since COVID-19 was a new disease, there was little medical knowledge that could be just taken off the shelf. Second, and more importantly, the promotion mechanism for prefectural leaders lies at the root of their initial reluctance to respond. Unlike most democracies, where candidates need to account for voters' interests such as public health to be elected, Chinese local political leaders are evaluated and promoted by upper-level officials based on their performance during their tenure and several other considerations. In any province, prefectural leaders compete for better economic growth to be promoted in the pyramid hierarchical political system. Since lockdown measures would hamper the economy and thus cast a shadow on their career advancement, prefectural leaders had little to no interest in imposing strict lockdown measures.

The stance of prefectural officials changed swiftly after February 3, 2020, when Xi Jinping, the

[34] As the origin and the transmission process of COVID-19 is still under scientific investigation and no consensus has been reached, one should not necessarily take this date for granted as the exact date of the COVID-19 outbreak.
[35] Please see http://english.mofcom.gov.cn/article/lawsdata/chineselaw/200211/20021100050619.shtml for the law. Consistent with the law, our data show that there was no province in which all prefectures within it implemented lockdowns on the same day.

general secretary of the Chinese Communist Party (CCP) and head of China, presided over an urgent meeting on COVID-19. Emphasizing the serious threat that COVID-19 posed to the nation, Xi urged local leaders to combat the virus with resolute measures: "Party committees and governments at all levels should firmly follow the unified command, coordination and arrangement of the CPC Central Committee … all job tasks must serve the goal of winning the blockade war against the pandemic … officials in every department in every region … should be warned and corrected if they don't follow the central committee's order well, and, if the mistake is considered to be serious, not only will the person involved get punished, their main leader should take responsibility too." Xi's stern warning marked a major watershed in the responses of prefectural governments, after which many prefectures launched campaigns against COVID-19. As in Figure 2.1, the number of prefectures imposing lockdown policies jumped dramatically from 13 to 123 in one week and further to 163 in two weeks. Figure 2.2 shows the geographic display of prefectures under lockdown and prefectures hit by the virus about ten days after Xi's speech.

While this pattern indicates that the views of local leaders regarding pandemic control were swiftly and remarkably changed after Xi delivered his message, this does not mean that their focus on economic growth was completely removed. The stringency of the counter-COVID-19 measures varied substantially among prefectures that announced lockdowns. Some prefectures imposed so-called "wartime emergency" lockdowns, including checking passengers in each vehicle, shutting down almost all businesses except grocery stores, and enforcing strictly monitored quarantine at assigned places for everyone coming from outside. At the same time, the counter-COVID-19 measures in other prefectures that announced lockdowns were not this strict. In some prefectures, commercial vehicles still operated freely, and people coming from outside were only required to self-quarantine.

A prefecture's lockdown stringency was highly influenced by its leader's career incentive, since the leader oversaw policy implementation and had the final say on specific measures. Xi's warning confronted each prefectural leader with a bitter dilemma: on the one hand, playing down the pandemic might lead to a COVID-19 explosion, a clear sign of incompetence that could lead to his/her removal; on the other hand, the promotion mechanism still incentivized him/her to place major emphasis on regional economic outcomes. Facing this dilemma, those who had great enough career incentives might still choose GDP growth over public health by imposing mainly non-coercive restrictions.

2.3 Data and variables

2.3.1 Sample and data sources

Our study focuses mainly on the COVID-19 pandemic but also connects to the SARS epidemic. We assemble two weekly balanced panels of 324 and 323 Chinese prefectures for the two periods, December 12, 2019 to March 14, 2020 and March 3 to July 28, 2003, respectively. Our samples exclude epidemic center cities; namely, Wuhan in COVID-19 and Guangzhou in SARS, since their lockdowns might be directly ordered by upper-level governments.[36] All prefectures of Tibet are also excluded from our analysis because their information is often missing.[37] We describe our main variables and the corresponding data sources below and report the descriptive statistics in Table 2.1.

Data on prefectures' lockdown policies. As in He et al. (2020), we designate a prefecture as being locked down when at least two of the following three preventive measures were enforced: 1) closure of non-essential businesses, such as bars, theatres, and libraries; 2) cancellation of large gatherings; 3) limitation or prohibition of private and public transportation.

Information on each prefecture's lockdown policy during SARS is hand collected from news reports, government documents, as well as the yearbooks for prefectures and provinces. Using these sources, we can tell whether a prefecture had ever imposed a lockdown as per our definition, and if so, when the lockdown started and ended. For COVID-19, we borrow heavily from Fang et al. (2020), and we complement their list with information collected from news reports and government documents. Our definition of lockdown is consistent across both parts of our study. The detailed list of lockdown dates for each prefecture is attached in Appendix Tables A.2.3 and A.2.4 for COVID-19 and in Appendix Table A.2.5 for SARS.

Data on the SARS epidemic and the COVID-19 pandemic. For SARS, the data we use are housed at the China Academy of Military Medical Sciences. It contains information on each of 5,327 Chinese SARS patients, including location, occupation, and importantly, dates of diagnosis and discharge or death. The data were collected retrospectively by researchers at the Academy through interviewing each hospital that had admitted SARS patients in 2002 and 2003. As a result, we can

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[36] According to information released later, on January 22nd, 2020, President Xi Jinping personally directed the provincial government of Hubei to implement strict restrictions on human mobility. Following his command, Wuhan was locked down on January 23rd. Source: http://www.gov.cn/xinwen/2020-02/15/content_5479271.htm. Nevertheless, including the epidemic center cities does not affect our empirical findings. The results are available upon request.

[37] Note that four directly administered municipalities, Beijing, Tianjin, Shanghai, and Chongqing, have provincial-level administrative ranking, and their secretaries are members of the Political Bureau of the CPC Central Committee; that is, China's national leaders. Thus, they are not included in our sample.

avoid the risks of under-reporting and manipulation as happened in the official data announced by China's Ministry of Health, which was the reason for the removal of the then Minister of Health Zhang Wenkang and the then mayor of Beijing Meng Xuenong. For our analysis, we aggregate the data to the prefecture level. Details on the data are described in Feng et al. (2009). The COVID-19 case count data were collected by a Chinese newspaper company called The Paper, and can be obtained from their website at thepaper.cn. Unlike our SARS data, our COVID-19 data contain only prefecture-by-date cumulative diagnoses and discharges instead of individual-level information.

Prefectural party secretaries' biographic information. We hand collect the resumés of all Chinese prefectural party secretaries in office from 1994 to 2020 from various sources, including Baidu Baike and numerous government websites.[38] In particular, the data include each local leader's start and end time in office, age and political hierarchical level at term start, and power status at term end, including promotions, lateral moves, and retirement. As a complement, we also collect the information for mayors to help distinguish the effects of party secretaries from that of mayors.

Human mobility data. We employ the within-prefecture mobility index from the travel map Baidu Migration offered by Baidu, the largest search engine in China. The mobility index is calculated from real-time location records for each user of Baidu's smart-phone map app, and is consistent across prefectures. We also use the data Baidu Migration created for 2019's Spring Festival season to eliminate the seasonality driven by the Spring Festival. Specifically, the outcome variable we construct, $\triangle$*Human Mobility*, denotes the difference in the mobility index between 2020 and 2019 on the same lunar calendar date of each prefecture. The sample we use is from January 1, 2020 to March 1, 2020, after which Baidu Migration data are not available for the comparable lunar calendar dates in 2019.

Weather data. The data are obtained from the National Climatic Data Center (NCDC) of the United States. For each prefecture, we use weekly temperature and precipitation. Missing values are interpolated in the software ArcGIS using the information of their neighborhood prefectures.

Additional prefecture-level information. We supplement the data above with information on GDP growth for each prefecture from the *China Statistical Yearbooks for Regional Economy*, which are created by the Statistics Bureau of China. We obtain the prefecture-level misused public funds detected by auditing institutions from the *China Audit Yearbook* in various years. This measure has been widely used as a proxy for corruption in the Chinese context (Liu and Ma, 2019; Bo et al., 2020; Jia et al., 2021). The prefecture-level geographic boundaries and centroids used in this paper are collected from China's National Geographic Information System (CNGIS). The travel

[38] Baidu Baike (https://baike.baidu.com) is the Chinese version of Wikipedia.

hours to Wuhan by road are retrieved from Google Map. [39]

2.3.2 Measuring the career incentives of Chinese prefectural leaders

To introduce our measure of Chinese prefectural leaders' career incentives, we note that in China's unique bureaucratic system, each prefectural government has two core leaders—the party secretary and the mayor. As discussed in section 2.2.1, we consider a Chinese prefecture's party secretary rather than its mayor as the primary leader. Nevertheless, we consider the career incentive of the mayor and show that it has limited explanatory power in the patterns we have documented. As noted in Wang et al. (2020), under China's promotion mechanism for government officials, the glass ceiling of a prefectural leader's career is largely determined by two factors, which are therefore the decisive factors in the leader's career advancement incentive.

The first determinant is a prefectural leader's hierarchical ranking, which can fall into one of two categories, including, in descending order, *deputy-provincial*, and *prefectural*. Since officials' promotion is determined by their supervisors who are "one-level-up," prefectural leaders ranked *prefectural* are evaluated and appointed by provincial governments, while prefectural leaders ranked *deputy-provincial* or higher are evaluated and appointed by the central government. As a result, leaders who are currently ranked higher should have a lower glass ceiling than others, which might affect their incentives for career advancement.

The second determinant of the career glass ceiling is the prefectural leader's age. To understand the role of age, one needs to differentiate between inauguration age and calendar age, and note that it is inauguration age that determines an official's career incentive.[40] Specifically, officials with a younger inauguration age, even if they have the same calendar age as their competitors at the same political level, would have larger career advancement incentives.

To gain intuition, consider the scenario where two candidates at the same political level, X and Y, compete for promotion. Suppose both people are the same calendar age, but person X took office at a younger age. He would thus have been working a longer time at the current political level compared to his competitor. In principal, under China's personnel control system, promotion to the next political level requires the candidate to work at least three years at the current level,[41] so person X would be closer in time to his "promotion window," and thus all else equal, his promotion is more justified. Besides, China's age-based retirement rule requires prefectural leaders to retire

[39] The travel hours to Guangzhou by road are calculated using the digitized map of China's road network created in 2000.
[40] Throughout this paper, the inauguration age is defined as the age at which a party secretary took their current position.
[41] In our data, the average tenure for prefectural party secretaries staying in office from 1994 to 2019 is 3.63 years.

from their positions at age 60, so once promoted, person X would be able to earn the gains associated with the higher ranked position for a longer time. As a result of both the work experience requirement and the age-based retirement rule, person X should have a higher expected payoff from working hard towards promotion, and thus he is believed to have larger career incentives.

Therefore, as pointed out in Wang et al. (2020), the glass ceiling of the career trajectory of a prefectural leader is largely fixed, given his/her age and hierarchy level. As a result, we construct our first measure of prefectural leaders' career incentives by replicating the work in Wang et al. (2020) using our sample, which includes all prefectural party secretaries who were incumbent from 1994 to 2019 and whose promotion outcomes can be observed. In Table 2.2, we use a probit model to estimate the effects of inauguration age and political hierarchy level on the promotion dummy,[42] and then we use the estimated coefficients to predict the *ex-ante* promotion probability, which is our first measure of career incentive. At the same time, as shown in Panel A of Figure 2.3, a plot of promotion outcome against inauguration age reveals an obvious negative correlation. Therefore, for transparency, we use inauguration age as an alternative measure of career incentive. In fact, a plot of lockdowns against inauguration age helps to motivate our main results: as depicted in Panel B of Figure 2.3, Chinese prefectural party secretaries inaugurated at a younger age were more reluctant to implement lockdowns during the COVID-19 pandemic. This suggests the negative impact of local leaders' career incentives on their implementation of lockdowns.

To further support our findings, we borrow our third measure of an official's career incentive, *Term Year*, from Guo (2009). This is defined as the years he/she has spent in the current position. The underlying logic of this measure is that local leaders would try to accelerate economic growth at certain points during their tenure, typically not the first or the second year, to maximize their chances of promotion. Recall that the average tenure for prefectural party secretaries in office from 1994 to 2019 is 3.63 years in our data. Prefectural leaders were expected to have larger career incentives after staying in the same position for a longer period.

In our subsequent analyses, we mainly use the promotion-incentive measure calculated by applying the method of Wang et al. (2020), and we use prefectural party secretaries' inauguration age and *Term Year* to conduct robustness checks.

[42] The promotion dummy is coded as one if the leader was promoted to a higher-level position at his/her term end. Examples of these positions include minister and deputy minister in the central government, provincial party-secretary, provincial deputy party secretary, provincial governor, and provincial deputy governor, and chairman or deputy chairman of provincial PC or CPPCC. For officials starting from the prefectural level, higher-level positions also include party secretary or mayor at deputy-province-level cities and member of the standing committee of the provincial CPC committee.

2.4 Effects of local leaders' career incentives on COVID-19 lockdown decisions

2.4.1 Validating the identification assumption

Our main message is that Chinese prefectural party secretaries with larger career incentives were more likely to downplay the virus by avoiding or minimizing lockdowns. The validity of our empirical strategy rests on the assumption that a prefectural leader's promotion incentive is not correlated with any factors that affect his/her lockdown decision conditional on some basic controls. To examine whether this assumption holds, we conduct a balance check, shown in Table 2.3, by regressing promotion incentive on a full set of potentially confounding factors that might alternatively give the patterns observed in our data.

On the one hand, local leaders' promotion incentives may coincide with some location-specific unobservable factors that may affect their career expectation. For example, there are anecdotes that province-level governments tend to place more favored officials in specific "promising prefectures" whose party secretaries were more likely to be promoted. Our results on how career incentives affect lockdown decisions can be biased if such prefectures happen to be major economic centers of systemic importance to the country and if officials assigned to such regions have different career incentives. Specifically, in such a region the cost of lockdown is higher due to its economic importance, and the probability of implementing lockdowns is thus lower. Therefore, in Table 2.3 we control for the historical promotion likelihood of each prefecture during 1994-2019. We further incorporate the number of prefectures within each province as a proxy for the difficulty of leaders' promotion, since prefectural leaders in the same province usually compete for promotion, and the number of prefectures within a province can also reflect the difficulties of coordinating anti-epidemic efforts for the provincial governments. Corruption can also impede effective government actions against COVID-19 and can erode the meritocratic system by weakening the linkages between promotion opportunities and the incentive to maintain economic growth, since corrupt officials would look out for themselves rather than seeking social benefit for the population (Kahana and Liu, 2010; Aidt et al., 2020). Inspired by this line of thought, we also incorporate the cumulative misused public funds detected by auditing institutions as a proxy for the corruption endemic in local governments, following existing studies (Liu and Ma, 2019; Bo et al., 2020; Jia et al., 2021). In addition, in the unlikely scenario where prefectural leaders with larger promotion incentives happen to be assigned to regions closer to Wuhan, our estimate can exaggerate the true effects of the career incentive. Therefore, we include as a potential confounding factor the prefecture's distance to Wuhan (measured by the hours travelled by road in log).

On the other hand, we consider several crucial personal attributes of prefectural leaders. To conduct the balance check, we control for leaders' calendar age, since older leaders might have different health concerns and thus different preferences for public health. First, we note that the ability of local leaders may correlate with both their lockdown decisions and their career incentives. Since a leader's ability cannot be measured in the data available, we resort to education as an alternative by controlling for a dummy indicator of college education or above. It has long been recognized by observers of Chinese politics that political stars tend to receive training in China's party schools, so we also take into consideration whether a leader has a party school degree. In addition, we acknowledge the possibility that a leader's ability can be reflected not only in his/her education but also in how well he/she has been supporting the local economy (the KPI in the promotion mechanism), and thus we include prefecture-level logarithmic GDP growth for the two years before the pandemic (2017-19) as another proxy for ability.

Second, informal patronage networks play a critical role in affecting the policy choices of local leaders (Jia et al., 2015; Jiang, 2018). One might worry about the validity of our results if correlation between patronage networks and leaders' incentives is suspected. In such cases, our results on how leaders' career incentives affect their lockdown decisions could be biased, since leaders better connected to upper-level officials might have different attitudes towards lockdowns after the nation's top leader called for a national pandemic control campaign. To address this concern, we use two dummies indicating a leader's past work experience at the central or provincial government as proxy for patronage networks and include them in the regression displayed in Table 2.3.

As shown in Table 2.3, the wide range of potentially confounding factors described above are largely comparable among local leaders with varying promotion incentives. Consequently, the systematic differences in the responses of prefectural governments to the supreme leader's call to combat the pandemic should be attributed to the difference in local leaders' promotion incentives rather than to other predetermined attributes. Nevertheless, our results are remarkably robust to including the wide range of controls described above, suggesting that these factors cannot confound our findings.

2.4.2 Cross-sectional evidence

We start by conducting a set of cross-section regressions of lockdown measures against the promotion incentives of prefectural leaders and other control variables as evidence to support our main specification, and the results are reported in Table 2.4.

In Columns 1 and 2 of Table 2.4, we consider a probit model and an ordered probit model, respectively, and both can be represented as equation 2.1 below:

$$Y_c = F\left(\alpha_i + \beta Incentive_c + \boldsymbol{X}_{\boldsymbol{c}}^{'}\boldsymbol{\theta} + \varepsilon_c\right) \qquad (2.1)$$

where F represents the standard normal cumulative distribution function (CDF). In the simple probit model, $Y_c = Pr(Lockdown_c|\boldsymbol{X_c})$, where $Lockdown_c$ is a dummy variable equal to one if the prefecture had ever implemented lockdown over our sample period, and $\boldsymbol{X_c}$ is the list of prefectural and leader characteristics specified in section 2.4.1. We then consider an ordered probit model where $Y_c = Pr(Lockdown_c \leq i|\boldsymbol{X_c})$. The subscript i is an integer and $Lockdown_c$ represents the number of days from Xi's speech to the date when the prefecture began lockdown, which proxies how responsive prefecture leaders were to the top leader's call. Note that this measure allows for negative values since a small proportion of prefectures had already implemented lockdowns before Xi's speech.[43]

In Column 3, we consider a Cox proportional hazards model as in equation 2.2 to characterize the dynamic decision-making process that highlights lockdown as a distinct one-time decision. This model shares the same spirit as Ru et al. (2021) who use a Cox proportional hazards model to estimate the effect of historical SARS experience on countries' COVID-19 lockdown decisions. In the model, the hazard function *h(t)* represents the likelihood of a prefecture being locked down at day *t*, conditional on the fact that the prefecture has not been locked down before day *t*. The hazard function is affected by time-varying factors, including the dynamics of the pandemic reflected in a nonparametric baseline hazard function $h_0(t)$, as well as prefecture-specific characteristics:

$$h(t) = h_0(t)exp\left(\alpha + \beta Incentive_c + \boldsymbol{X}_{\boldsymbol{c}}^{'}\boldsymbol{\theta} + \varepsilon_c\right) \qquad (2.2)$$

Each prefecture enters the hazard regression at the date of Xi's speech, and exits when lockdown policy takes effect. Prefectures that implemented lockdowns before Xi's speech are thus excluded from the hazard regression.

As shown in Table 2.4, we first report unconditional correlations between our main regressor, promotion incentive, and lockdown decisions in Columns 1, 3, and 5, and further include the full set of control variables used in Columns 2, 4, and 6. One might also be concerned that lockdown patterns were driven by the career incentives of mayors, rather than those of party secretaries. To address this concern, we further include the mayor's promotion incentive, which is also calculated using the method of Wang et al. (2020).[44] While the mayor nominally heads the prefectural

[43] To avoid sample attrition, we arbitrarily assign the days from Xi's speech to lockdown as 99 for those prefectures in our sample that never locked down.

[44] We construct the mayor's promotion incentive in a similar way to that of the prefectural party secretary. Please refer to appendix Table A.2.1 for more details.

government and is responsible for implementing policies, the party secretary is more powerful and directs the government's operation. Consistent with our expectation, all columns show that prefectures whose incumbent party secretaries had larger promotion incentives were less willing to implement lockdowns, and even if they finally did, they tended to do so later than others. For example, the estimate in Column 6 suggests that a one-standard-deviation increase in a prefectural leader's promotion incentive yields a hazard ratio of 0.735, indicating that the rate of lockdown decreases by 26.5%.[45] In addition, the set of control variables, except road travel distance to Wuhan, exhibit no clear signals of having strong impacts on lockdown decisions, which is consistent with our findings in Table 2.3.

2.4.3 Evidence from panel regression

One drawback of the cross-sectional study in Table 2.4 is that we are unable to consider the impact of the dynamics of disease transmission. To overcome this obstacle, we use the differences-in-differences strategy in equation 2.3 to identify the effects of local leaders' career incentives on whether lockdown policies were imposed:

$$Lockdown_{ct} = \beta Incentive_c \times Window_t + \alpha_c + \delta_t + (\boldsymbol{X_c} \times \delta_t)'\boldsymbol{\theta} + Z_{ct}^{'}\boldsymbol{\phi} + \varepsilon_{ct}, \qquad (2.3)$$

where subscripts c and t index prefecture and week, respectively. $Lockdown_{ct}$ denotes whether prefecture c is under lockdown in week t.[46] $Incentive_c$ denotes the promotion incentive of the incumbent party secretary of prefecture c and is constant over time. For convenience of interpretation, we standardize this variable so that its mean equals zero and its standard deviation equals one. $Window_t$ denotes the policy window when the attention of prefectural leaders was temporarily shifted from economic growth to disease control as a result of political pressure from the central government. In particular, $Window_t$ equals one if it's after President Xi's speech on February 3, 2020. β is our parameter of interest, α_c and δ_t denote prefecture and week fixed effects, and $\boldsymbol{X_c}$ is the same list of prefectural and leader characteristics specified above. Since lockdown decisions might be affected by the severity of COVID-19 in each prefecture, we also control for the number of active cases in each prefecture lagged by a week (denoted by Z_{ct}).[47]

Results from equation 2.3 are presented in Table 2.5. Column 1 includes only week fixed effects, prefecture fixed effects, and lagged active cases. The point estimate suggests that a one-standard-

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[45] 1-*exp*(-0.3075)=1-0.735=0.265.

[46] We define a prefecture to be under lockdown during a week if it's locked down for at least 4 days of that week. Alternative definitions of lockdown, such as denoting it to be one only if it's locked down for 7 days of the week, do not change our findings.

[47] Active cases equals total cases minus deaths and recoveries and represents the number of people currently affected. In our week-level observations, a case is defined as "active" in a week if it is in active status for over 3 days of that week.

deviation increase in a prefectural leader's promotion incentive decreases the likelihood of imposing a lockdown by 11.3 percentage points in COVID-19 within the policy window. Considering the fact that half of all Chinese prefectures went through lockdowns during the COVID-19 pandemic, the average effect is of both statistical and economic significance. In Column 2, we also include the interactions of week dummies and the list of prefectural characteristics described in Table 2.3 to rule out alternative hypotheses. Our estimate of interest changes only marginally, and together with Table 2.3, it reassures that we can safely rule out the possibilities of confounding factors. As in Table 2.4, in addition to the confounding factors in Table 2.3, in Table 2.5 we also include the mayors' promotion incentives to rule out the possibility that mayors played an important role in the decision-making process of implementing lockdowns.

To consider the dynamic effects, we conduct an event study estimation by replacing $Window_t$ in equation 2.3 with a full set of week dummies. Despite the strong political pressure from the central government, prefectural leaders with larger promotion incentives were more reluctant to impose lockdowns within the policy window, as shown in Figure 2.4.

2.4.4 The intensive margin effects of local leaders' promotion incentives on lockdown stringency

Tables 2.4 and 2.5 can uncover only the extensive margin effects of career incentives on whether to implement a lockdown or not. To study the intensive margin effects of local leaders' promotion incentives on the stringency of lockdown enforcement, we conduct a panel regression as in equation 2.4.

$$Y_{ct} = \alpha_c + \delta_t + \tau Lockdown_{ct} + \gamma Incentive_c \times Lockdown_{ct} + (\boldsymbol{X_c} \times \delta_t)'\boldsymbol{\theta} + \boldsymbol{Z}'_{\boldsymbol{ct}}\boldsymbol{\phi} + \epsilon_{it}, \quad (2.4)$$

where Y_{ct} denotes the proxy for lockdown stringency, for which we use the within-prefecture human mobility index developed by Baidu Map, following Fang et al. (2020). The mobility index is calculated from real-time location records for each user of Baidu's smart-phone map app, and is consistent across prefectures. We take the difference between the human mobility index in 2020 and the index on the same lunar calendar date in 2019 to eliminate the seasonality driven by the Spring Festival. Due to data availability constraints, our data on the human mobility index cover only the period January 1, 2020 to March 1, 2020. One may be concerned that the index measures not only the stringency of policy but also people's compliance, as discussed in Allcott et al. (2020). However, unless we have reason to believe that prefecture leaders' promotion incentive is somehow related to residents' compliance, we can still use the measure to proxy lockdown stringency. $\boldsymbol{Z_{ct}}$ is a vector of weather controls, including weekly average daytime (6 a.m.-6 p.m.)

precipitation, temperature and its square, which are crucial determinants of human activity, as well as the number of active cases lagged by a week in each prefecture. Other symbols are defined as in equation 2.3.

In Table 2.6, we report the results for equation 2.4. Column 1 presents the results where no controls were included except prefecture fixed effects, week fixed effects, weather controls and lagged active cases. In Column 2, we rule out the competing hypotheses as we did in Column 2 of Table 2.5. The estimates of $Lockdown_{ct}$ confirm lockdown's effectiveness in reducing human mobility, and more importantly, our estimates of the interaction term, $Incentive_c \times Lockdown_{ct}$, are significantly negative, indicating that prefectural leaders with greater career incentives were more inclined to implement loose lockdowns, arguably for consideration of the economy.

We conduct a similar event study estimation for equation 2.4, where $Lockdown_{ct}$ was replaced by the dummies of relative weeks to the first week of lockdown. Figure 2.5 plots the estimates and shows that a prefecture's lockdown tended to be less stringent if its leader had a larger career aspiration.

2.4.5 Robustness checks and placebo tests

We report our robustness checks in Table 2.7 to deal with the following concerns (Panel A for Table 2.5 and Panel B for Table 2.6). First, one might worry that our prefecture-by-week-level data conceal crucial information regarding prefectural leaders' lockdown decisions. We rerun our regressions using prefecture-by-date-level data, and the findings in Column 1 are highly consistent with our main results in Tables 2.5 and 2.6. Second, prefectural leaders might have systematic differences in imposing lockdowns if they were at the deputy-provincial political level or were newly inaugurated. In Columns 2 and 3 of Table 2.7, we drop the sample of party secretaries with these characteristics, and the findings stay unchanged. Third, one might suspect that our findings are driven by our proxy for the career incentives of prefectural party secretaries. Therefore, we borrow another measure, *Term Year*, from Guo (2009), which is defined as the years one official has spent in the current position. The underlying logic of this measure is that the longer a local leader has held the current position, the more intense is his/her desire to be promoted. For comparison, we standardize this measure as well, and the results reported in Column 4 are consistent with those calculated using the original incentive measure. Finally, we include the results using the standardized inauguration age of a prefectural leader as an alternative measure of promotion incentive and the findings are largely unchanged, as reported in Column 5. Essentially, using inauguration age as the proxy for each prefectural party secretary's career incentive shares similar sources of variation as the measure in Wang et al. (2020) but is more transparent. The high internal consistency strengthens our confidence in the results.

In addition, we conduct a set of placebo tests by using arbitrary policy windows, which are two-to-four weeks ahead of the actual periods. As reported in Table 2.8, all estimates of the interaction terms constructed using the false treatment variable are indistinguishable from zero, suggesting that our findings are not driven by other unobservables.[48]

2.5 Evidence from SARS and its long-lasting impact on COVID-19 lockdowns

The results above show that during the COVID-19 pandemic Chinese prefectural leaders with greater promotion incentives were more likely to delay and minimize lockdown measures for fear of damaging the economy. COVID-19 was not the first acute respiratory disease caused by a coronavirus, but SARS was. Chinese people and policymakers took SARS as an important reference when considering policy measures to cope with the COVID-19 pandemic.[49] In this section, we first study whether the tradeoff local leaders faced between combating an epidemic and developing the local economy that we observed with COVID-19 also existed during the SARS epidemic in 2003. Then we examine whether the SARS experience had long-lasting effects on the reactions of local Chinese governments towards COVID-19.

The first SARS case was retrospectively discovered during late November 2002 in China's Guangdong Province. As in the case of COVID-19, prefectural leaders were initially reluctant to impose lockdowns: by April 17, 2003, only 8 prefectures had imposed lockdowns among more than 40 hit by SARS, as shown in Figure 2.6.

The number of prefectures hit by SARS kept growing exponentially, and just as Xi did in the COVID-19 pandemic, on April 17, 2003, Hu Jintao, then general secretary of CCP and head of China, gave a speech during an urgent meeting on SARS. Hu emphasized the importance of controlling SARS and warned prefectural leaders that they "would be held accountable for the overall situation in their jurisdictions." The pressure coming from the nation's top leader had a substantial impact on prefectural leaders' behavior. As Figure 2.6 illustrates, 29 prefectural governments began to lock down their jurisdictions, and the national total rose to 77 after another week. Compared to the COVID-19 pandemic, prefectures' response to SARS appeared to be slower, partly because the transmission of COVID-19 is faster than SARS. Figure 2.7 shows the geographic displays of prefectures under lockdown and prefectures hit by the virus during the SARS outbreak about one month after Hu's speech.

Column 1 of Table 2.9 reports the results of equation 2.3 using data on SARS. Of particular note

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48 The point estimate in Column 3 is significant at the 90% level, but its sign is opposite to our baseline result.

49 Please see https://www.scmp.com/news/china/article/3047319/wuhan-coronavirus-full-blown-community-epidemic-chinese-health for an example of such a discussion during the early stage of COVID-19, when experts explained the severity of COVID-19 by referring to SARS.

is that $Window_t$ equals one if the week is between President Hu's speech on April 17, 2003 and Beijing's reopening on June 1, 2003. Consistent with our findings using the COVID-19 data, the estimate in Column 1 confirms that leaders with greater career incentives tended not to announce lockdowns.[50] As for COVID-19, we also conducted an event study estimation to illustrate the dynamic effects. As plotted in Figure 2.8, the estimates display a similar pattern.

We proceed to explore the connection between the two outbreaks by studying the repercussions of SARS: What would be the consequence in combating COVID-19 if a prefecture had experienced lockdown during SARS, and what would be the consequence if its leader had done so? Chinese prefectural leaders are rotated frequently and regularly across prefectures (Yao and Zhang, 2015; Jia and Xu, 2018), creating substantial variation in the SARS experience of prefectural leaders for us to study.

Specifically, we construct two dummy variables, one for whether a prefecture experienced SARS lockdown, and one for whether a prefecture party secretary experienced it at the prefecture he/she worked in during SARS. We include their interactions with our main regressors and report the results of equations 2.3 and 2.4 in Columns 2 and 3 of Table 2.9, respectively. Unsurprisingly, the effects of COVID-19 lockdowns on human mobility were stronger in prefectures that experienced SARS lockdowns, even though we do not observe the pattern that such regions were more likely to implement lockdowns than others. This finding is consistent with the association documented in Ru et al. (2021), which shows that governments in countries that did not experience SARS were significantly slower in their response to COVID-19.

Of particular note are the patterns of prefectural leaders' SARS lockdown experience. Evidence from news reports shows that containment measures were generally more strictly implemented during COVID-19 than during SARS, since COVID-19 was transmitted faster than SARS and caused more infections in China. The estimated parameter of the interaction terms with the party secretary's SARS lockdown experience suggests that prefectural leaders' lockdown decisions were independent of the prefecture's or their own experience of SARS lockdown, as shown in Column 2. However, if the leaders had experienced SARS lockdown, they were more likely to downplay the virus by implementing loose lockdown measures, as revealed in Column 3.

To better understand the career consequences of local leaders' performance in SARS, we use both a linear probability model (LPM) (appendix Table A.2.2 Column 1) and a probit model (appendix Table A.2.2 Column 2), and report the correlation between the promotion outcomes of prefectural

[50] As before, we exclude the epidemic center Guangzhou from our sample. Correspondingly, we replace the log road travel hours to Wuhan with the log road travel hours to Guangzhou. We are unable to control for the historical promotion likelihood from 1994 to 2019 in Column 1 because, for the shorter period from 1994 to 2002, there are many missing values in historical promotion outcomes.

leaders incumbent during SARS and a set of variables reflecting their performance in facilitating economic growth and responding to the epidemic. These include the logarithmic average GDP growth over their term, whether they implemented lockdowns, and the cumulative SARS cases per 10,000 people. We further include leaders' ex-ante promotion likelihood as a control, and to be consistent with our main specification in Table 2.3, we also control for several covariates, but their inclusion has a limited impact on the messages we want to deliver. In both columns, we find that prefectural leaders would have higher odds of being promoted if they achieved higher GDP growth during their term. Also, a leader's promotion incentive has more statistical power in explaining his/her promotion outcomes. This pattern suggests that even after controlling for prefectural leaders' *ex-ante* promotion likelihood, maintaining a spectacular record for economic growth is still a dominant strategy in the promotion game. At the same time, according to our dataset, not even one prefectural party secretary was dismissed for reluctance in responding to SARS. Combined with this anecdotal evidence, appendix Table A.2.2, which shows that neither SARS infection rates nor whether lockdown was implemented affected the promotion of prefectural party secretaries, gives rise to our preferred interpretation: prefectural party secretaries who had experienced SARS lockdowns would have formed the belief that, to be promoted in the future, facilitating economic growth would still be non-negligible during the pandemic, while in terms of public health responsibilities, they could muddle through COVID-19 just as before.

The heterogeneous impacts of prefectures' and prefectural leaders' SARS lockdown experience on COVID-19 lockdown measures enrich our understanding of the long-lasting consequences of anti-epidemic measures. Our empirical finding also cautions that failing to punish government officials' sluggishness in controlling disease outbreaks may come back to bite a nation.

2.6 Conclusion

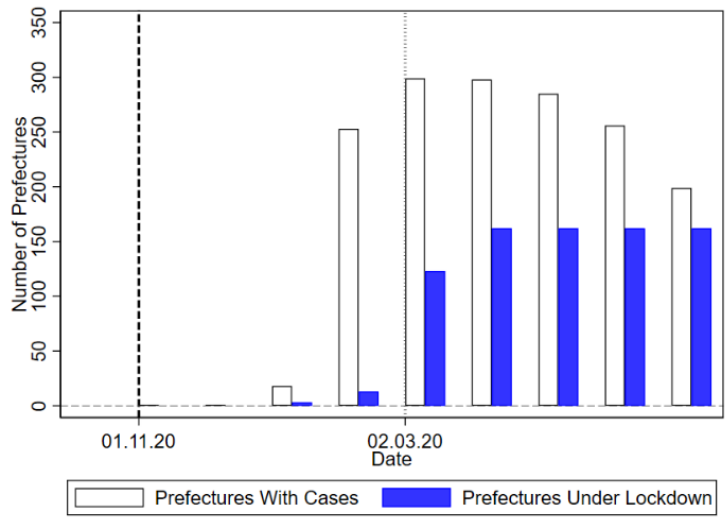
2.6.1 Findings and policy implications

This study draws lessons from China's campaign against COVID-19 to shed light on whether local leaders' career incentives contributed to their reluctance in adopting effective measures, such as implementing lockdowns, to combat the virus. We find evidence of reluctance to impose lockdowns at the beginning of the pandemic because of promotion concerns associated with economic growth. While warnings from the nation's top leader obstructed local leaders' desires to trade public health for economic growth, we find that local leaders with larger promotion incentives were still more likely to avoid or minimize lockdowns. As revealed by the data, local leaders might have formed the belief from their experience of SARS that the emphasis on pandemic control by the central government was temporary. Our focus on China suggests similarities with democracies. Politicians in authoritarian regimes like China, because of promotion incentives, may place a higher weight on economic growth than disease control, while politicians in democratic countries are incentivized to win elections, and thus may hesitate to take resolute policy actions for fear that voters care more about employment and incomes than disease control. No matter in which regime, politicians can have incentives to act slowly to combat a pandemic.

2.6.2 Relevance to other chapters

As discussed throughout the dissertation, especially in Chapters 1 and 2, the political economy impacts of the hierarchy of government systems in China matter a lot for understanding policy enforcement in China. However, one could still wonder whether a policy could be implemented effectively without the interactions across different layers in the hierarchy. To complement this discussion, Chapter 3 takes a different approach and examines a more centralized enforcement mode where the central government directly implements a public policy. By contrasting this mode with the more flexible local enforcement modes explored in the previous chapters, Chapter 3 provides a benchmark for comparison and highlights the role of institutional interactions in shaping policy outcomes.

Figure 2.1 The Spread of COVID-19 and the Implementation of Lockdowns



Notes: February 3, 2020: Xi's speech.

Figure 2.2 Geographic Display of Prefectures under Lockdown and Prefectures Hit by COVID-19 on February 13, 2020

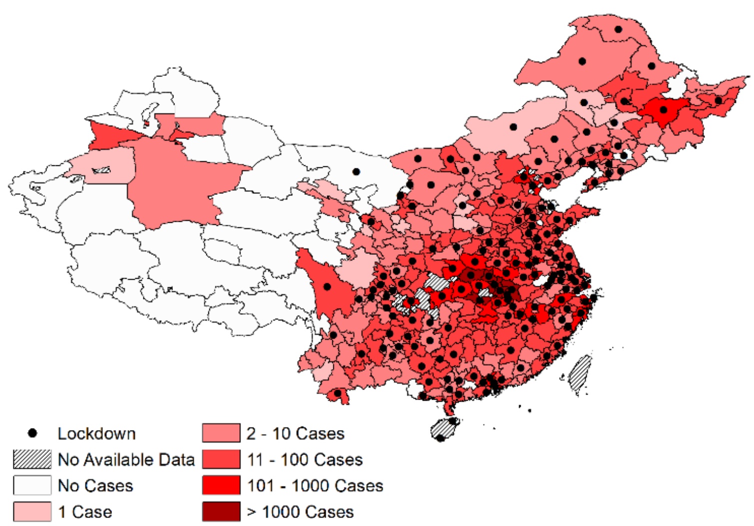
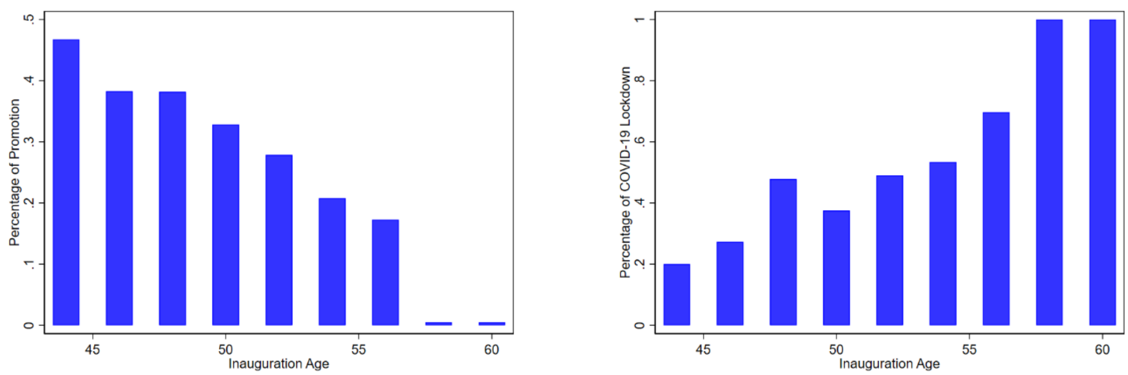


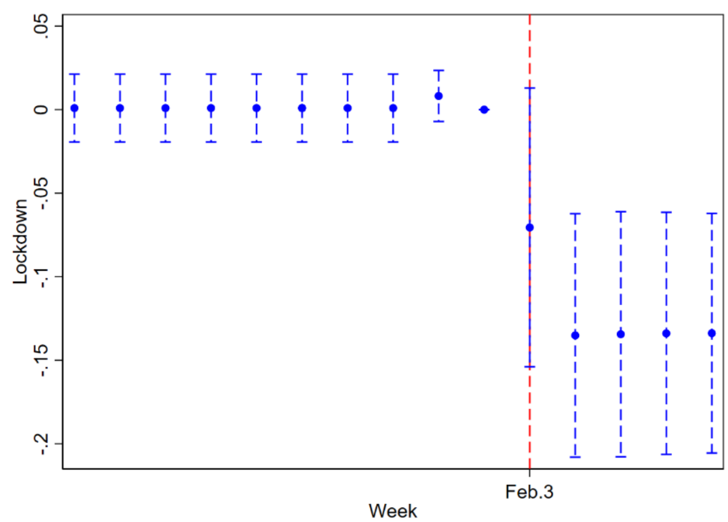
Figure 2.3 Party Secretaries' Inauguration Age, Promotion Likelihood and Lockdown Decisions

A: Promotion Likelihood B: Lockdown Decisions



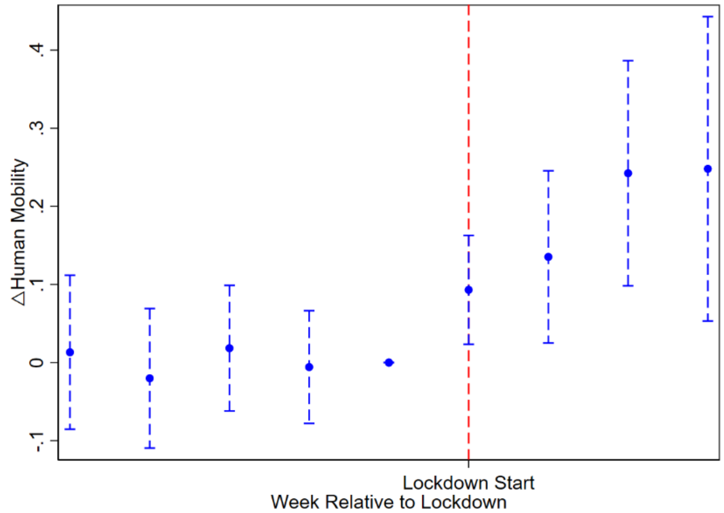
Notes: This figure presents the distribution of promotion likelihood for prefectural party secretaries who were in office from 1994 to 2019 and the percentage of prefectures implementing lockdown during the COVID-19 pandemic by inauguration age of prefectural party secretaries.

Figure 2.4 Event Study of Promotion Incentives on Lockdown Decisions during the COVID-19 Pandemic



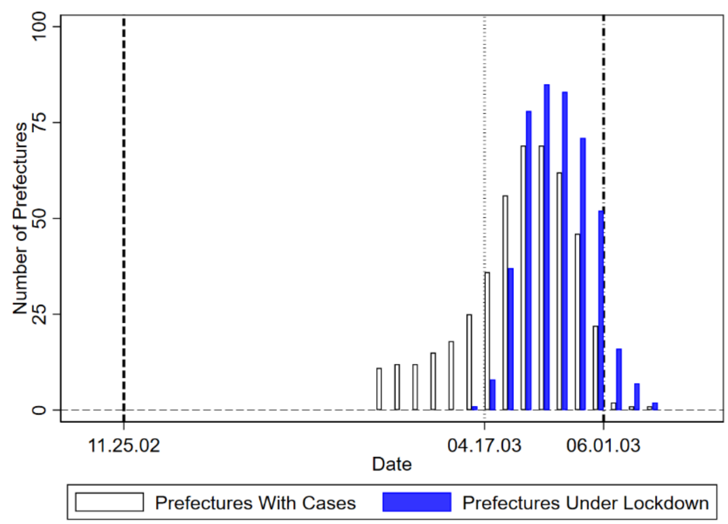
Notes: This figure visualizes the dynamic effects of prefectural party secretaries' promotion incentives on lockdown decisions during the COVID-19 pandemic using the specification in Column 2 of Table 2.5. We illustrate the estimated coefficients with the 95% confidence intervals of the interaction terms between promotion incentive and a full set of week dummies. February 3, 2020 denotes Xi's speech. The week before Xi's speech is omitted as the reference group. The vertical axis depicts the indicator for having lockdown implemented.

Figure 2.5 Event Study of Promotion Incentives on Lockdown Intensity during the COVID-19 Pandemic



Notes: This figure visualizes the dynamic effects of prefectural party secretaries' promotion incentives on lockdown stringency during the COVID-19 pandemic using the specification in Column 2 of Table 2.6. We illustrate the estimated coefficients with the 95% confidence intervals of the interactions between prefectural party secretaries' promotion incentives and a full set of week dummies denoting relative time to the lockdown. The week before the top leader's speech is omitted as the reference group. The vertical axis depicts the difference in mobility index between 2020 and 2019 on the same lunar calendar date.

Figure 2.6 The Spread of SARS and the Implementation of Lockdowns



Notes: April 17, 2003: Hu's speech; June 1, 2003: Beijing's reopening.

Figure 2.7 Geographic Display of Prefectures under Lockdown and Prefectures Hit by SARS on May 15, 2003

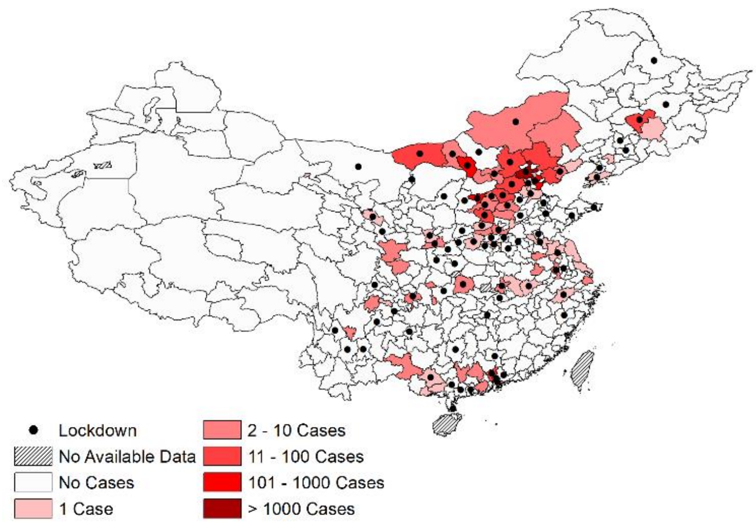
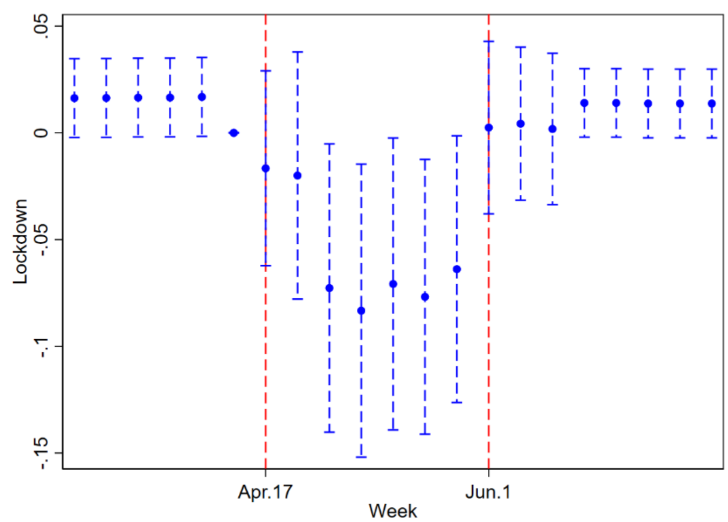


Figure 2.8 Event Study of Promotion Incentives on Lockdown Decisions during SARS



Notes: This figure visualizes the dynamic effects of prefectural party secretaries' promotion incentives on lockdown decisions during SARS using the specification in Column 1 of Table 2.9. We illustrate the estimated coefficients with the 95% confidence intervals of the interaction terms between promotion incentive and a full set of week dummies. April 17, 2003 denotes Hu's speech and June 1, 2003 denotes Beijing's reopening. The week before Hu's speech is omitted as the reference group.

Table 2.1 Summary Statistics

Variables	Obs.	Mean	Std. Dev.	Min	Max
Panel A. Prefecture/Leader Characteristics During COVID-19					
Promotion Incentive	324	0.349	0.060	0.125	0.523
Inauguration Age	324	52.59	2.699	44	61
Calendar Age	324	55.03	2.723	44	64
Deputy-Province-Level	324	0.043	0.204	0	1
Prefecture-Level	324	0.944	0.229	0	1
Historical Promotion Likelihood	324	0.363	0.254	0	1
Number of Prefectures Within Province	324	13.80	4.135	1	21
Log Misused Public Funds, 1999-2015	324	14.13	2.126	0	20.69
Log Road Travel Hours to Wuhan	324	2.394	0.628	0.182	3.871
College or Above Degree	324	0.966	0.181	0	1
Party School Degree	324	0.228	0.420	0	1
Log GDP Growth, 2017-19	324	0.323	0.792	-0.963	3.426
Central Experience	324	0.108	0.311	0	1
Provincial Experience	324	0.682	0.466	0	1
Mayor’s Promotion Incentive	324	0.460	0.075	0.125	0.638
Prefecture Experienced SARS Lockdown	324	0.269	0.444	0	1
Party Secretary Experienced SARS Lockdown	324	0.568	0.496	0	1
Term Year	324	1.922	1.592	0	7
Panel B. Prefecture by Week Level Variables, COVID-19 Sample					
Lockdown Indicator	4,860	0.160	0.367	0	1
Mean Daytime Rainfall	3,240	0.585	0.885	0.008	5.759
Mean Daytime Temperature	3,240	4.571	8.803	-23.48	22.20
Lagged Active Cases	4,860	13.014	80.091	0	2,138
△Human Mobility	2,889	-0.820	1.371	-5.818	2.456

Table 2.2 Summary Statistics (Cont'd)

Variables	Obs.	Mean	Std. Dev.	Min	Max
Panel C. Prefecture/Leaders During SARS					
Promotion Indicator	323	0.393	0.489	0	1
Promotion Incentive	322	0.413	0.112	0.082	0.621
Inauguration Age	322	48.25	3.768	39	57
Calendar Age	322	49.86	3.939	39	61
Deputy-Province-Level	323	0.0960	0.295	0	1
Number of Prefectures Within Province	323	13.98	3.841	2	21
Log Misused Public Funds, 1999-2003	323	10.94	1.687	0	15.71
Log Road Travel Hours to Guangzhou	323	3.205	0.685	0.499	4.473
College or Above Degree	323	0.889	0.315	0	1
Party School Degree	323	0.248	0.432	0	1
Log Average GDP Growth over Term	323	0.111	0.048	-0.108	0.290
Log GDP Growth, 2000-02	321	0.193	0.974	-0.157	0.820
Central Experience	323	0.040	0.197	0	1
Provincial Experience	323	0.607	0.489	0	1
Mayor's Promotion Incentive	319	0.513	0.070	0.135	0.695
Panel D. Prefecture by Week Level Variables, SARS Sample					
Lockdown Indicator	6,734	0.062	0.242	0	1

Table 2.3 Calculating Prefectural Party Secretaries' Career Incentives

	(1)
Dependent Variable	Promotion
Inauguration Age	-0.0505***
	(0.008)
Deputy-Province-Level	-5.7551***
	(1.554)
Inauguration Age×Deputy-Province-Level	0.0990***
	(0.031)
Observations	1,821
Dep. Mean	0.384

Notes: This table replicates Column 3 of Table 2 in Wang et al. (2020) using our sample of all prefectural party secretaries who were incumbent during 1994 to 2019. Robust standard errors are reported in parentheses, clustered at the prefecture level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.4 Balance Check on Promotion Incentives of Party Secretaries in COVID-19

	(1)
Dependent Variable	Promotion Incentive
Historical Promotion Likelihood	0.0085
	(0.012)
Number of Prefectures Within Province	0.0002
	(0.001)
Log Road Travel Hours to Wuhan	-0.0035
	(0.004)
Log Misused Public Funds, 1999-2015	-0.0043***
	(0.002)
Calendar Age	-0.0388***
	(0.003)
College or Above Education	-0.0156
	(0.013)
Party School Degree	0.0033
	(0.006)
Log GDP Growth, 2017-19	-0.0063
	(0.004)
Central Experience	-0.0053
	(0.012)
Provincial Experience	-0.0047
	(0.005)
Dep. Mean	0.349
Observations	324

Notes: In this table, we use cross-section data of 324 prefectures. Calendar age is standardized so that its mean equals zero and standard deviation equals one. Robust standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.5 Cross-Section Results for the Effects of Promotion Incentives on Lockdown Decisions

	(1)	(2)	(3)	(4)	(5)	(6)
Method	Probit		Ordered Probit		Cox Proportional Hazards	
Dependent Variable	**1**(Ever Locked Down During COVID-19)		Days from Xi's Speech to Lockdown		Lockdown	
Promotion Incentive	-0.3407***	-0.4306***	0.1847***	0.2326**	-0.3927***	-0.3075***
	(0.075)	(0.127)	(0.057)	(0.095)	(0.066)	(0.108)
Historical Promotion Likelihood		0.1782		-0.3109		0.2351
		(0.311)		(0.265)		(0.319)
Number of Prefectures Within Province		-0.0092		0.0331*		0.0119
		(0.021)		(0.018)		(0.023)
Log Misused Public Funds, 1999-2015		0.0292		0.0173		0.0585
		(0.039)		(0.035)		(0.078)
Log Road Travel Hours to Wuhan		-0.4352***		0.7102***		-0.3903**
		(0.119)		(0.122)		(0.159)
Calendar Age		-0.1583		0.1050		-0.0098
		(0.111)		(0.081)		(0.099)
College or Above Education		0.6884		-0.7233*		0.9308
		(0.453)		(0.435)		(0.671)
Party School Degree		0.1412		-0.0523		-0.0238
		(0.184)		(0.160)		(0.194)
Log GDP Growth, 2017-19		-0.0258		0.0501		-0.1192
		(0.107)		(0.086)		(0.101)
Central Experience		0.3008		-0.0892		0.0631
		(0.264)		(0.214)		(0.266)
Provincial Experience		-0.1982		0.2019		-0.2180
		(0.158)		(0.136)		(0.169)
Mayor's Promotion Incentive		-0.1067		0.0757		-0.1334
		(0.095)		(0.074)		(0.088)
Observations	324	324	324	324	300	300

Notes: This table reports results for the effects of promotion incentive on lockdown decisions using the cross-sectional sample of prefectures during COVID-19. Columns 1-2 use probit models, Columns 3-4 use ordered probit models, and Columns 5-6 use Cox proportional hazards models. Promotion incentive, calendar age, and mayor's promotion incentive are standardized so that their means equal zero and their standard deviations equal one. Robust standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.6 Panel Regression Results for the Effects of Promotion Incentives on Lockdown Decisions

	(1)	(2)
Dependent Variable	Lockdown in COVID-19	
Window×Promotion Incentive	-0.1130***	-0.1231***
	(0.022)	(0.035)
Lagged Active Cases	0.0006***	0.0003***
	(0.000)	(0.000)
Dep. Mean	0.160	0.160
Week FE	YES	YES
Prefecture FE	YES	YES
Controls×Week FE		YES
Observations	4,860	4,860
Num. of Clusters	324	324

Notes: Controls include historical promotion likelihood, the number of prefectures within each province, log misused public funds detected by auditing institutions in 1999-2015, log road travel hours to Wuhan, leader's calendar age, whether a leader has a college degree or above, whether a leader has a party school degree, the prefecture-level logarithmic GDP growth in 2017-19, two dummies indicating whether a leader has work experience at the central or provincial government, and mayor's promotion incentive. Promotion incentive, calendar age, and mayor's promotion incentive are standardized so that their means equal zero and their standard deviations equal one. Robust standard errors are reported in parentheses, clustered at the prefecture level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2.7 Panel Regression Results for the Effects of Promotion Incentives on Lockdown Stringency

	(1)	(2)
Dependent Variable	△Human Mobility	
Lockdown	-0.4811***	-0.3583***
	(0.073)	(0.069)
Lockdown×Promotion Incentive	0.2613***	0.1651***
	(0.043)	(0.056)
Lagged Active Cases	-0.0010**	-0.0011**
	(0.000)	(0.000)
Dep. Mean	-0.820	-0.820
Week FE	YES	YES
Prefecture FE	YES	YES
Weather Conditions	YES	YES
Controls×Week FE		YES
Observations	2,889	2,889
Num. of Clusters	321	321

Notes: *△Human Mobility* denotes the difference in the mobility index between 2020 and 2019 on the same lunar calendar date in each prefecture. Weather conditions include average daytime rainfall and average daytime temperature and its square. Controls include historical promotion likelihood, the number of prefectures within each province, log misused public funds detected by auditing institutions in 1999-2015, and log road travel hours to Wuhan, leader's calendar age, whether a leader has a college degree or above, whether a leader has a party school degree, the prefecture-level logarithmic GDP growth in 2017-19, two dummies indicating whether a leader has work experience at the central or provincial government, and mayor's promotion incentive. Promotion incentive, calendar age, and mayor's promotion incentive are standardized so that their means equal zero and their standard deviations equal one. Robust standard errors are reported in parentheses, clustered at the prefecture level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2.8 Robustness Checks for the Effects of Promotion Incentives on Lockdown Decisions

	(1)	(2)	(3)	(4)	(5)
Specification	Date-Level	Dropping Deputy-Provincial Party Secretaries	Excluding Prefectures with Turnovers of Local Leaders	Using Term Year to Measure Career Incentives	Using Inauguration Age to Measure Career Incentives
Panel A:			Lockdown in COVID-19		
Window×Promotion Incentive	-0.1156***	-0.1673***	-0.1288***		
	(0.034)	(0.050)	(0.035)		
Window×Alternative Career Incentives				-0.0761***	0.1392***
				(0.028)	(0.042)
Lagged Active Cases	0.0002*	0.0004***	0.0003***	0.0004***	0.0004***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Dep. Mean	0.215	0.156	0.160	0.160	0.160
Observations	19,581	4,650	4,785	4,860	4,860
Num. of Clusters	321	310	319	324	324
Panel B:			△Human Mobility		
Lockdown	-0.3390***	-0.3670***	-0.3389***	-0.3513***	-0.3579***
	(0.071)	(0.070)	(0.069)	(0.069)	(0.070)
Lockdown×Promotion Incentive	0.1612***	0.1817**	0.1692***		
	(0.056)	(0.072)	(0.056)		
Lockdown×Alternative Career Incentives				0.1542***	-0.1462**
				(0.056)	(0.058)
Lagged Active Cases	-0.0010**	-0.0012**	-0.0011**	-0.0012**	-0.0012**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Dep. Mean	-0.864	-0.816	-0.822	-0.820	-0.820
Observations	19,581	2,772	2,853	2,889	2,889
Num. of Clusters	321	308	317	321	321
Prefecture FE	YES	YES	YES	YES	YES
Date FE	YES				
Week FE		YES	YES	YES	YES
Controls×Week FE	YES	YES	YES	YES	YES

Notes: Column 1 uses prefecture-by-date-level data to estimate our model in Equation 1. We restrict our sample by dropping deputy-provincial party secretaries in Column 2 and prefectures with turnovers of local leaders in Column 3. Column 4 uses prefectural party secretaries' term year to measure career incentives as in Guo (2009). Column 5 uses the standardized inauguration age to measure career incentives. Controls include historical promotion likelihood, the number of prefectures within each province, log misused public funds detected by auditing institutions in 1999-

2015, and log road travel hours to Wuhan, leader's calendar age, whether a leader has a college degree or above, whether a leader has a party school degree, the prefecture-level logarithmic GDP growth in 2017-19, two dummies indicating whether a leader has work experience at the central or provincial government, and mayor's promotion incentive. Promotion incentive, term year, inauguration age, calendar age, and mayor's promotion incentive are standardized so that their means equal zero and their standard deviations equal one. The dependent variable in Panel B, *△Human Mobility*, denotes the difference in the mobility index between 2020 and 2019 on the same lunar calendar date in each prefecture. For all regressions in Panel B, we further control for weather conditions, including average daytime rainfall and average daytime temperature and its square. Robust standard errors are reported in parentheses, clustered at the prefecture level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2.9 Placebo Tests for the Effects of Promotion Incentives on Lockdown Decisions

	(1)	(2)	(3)
Specification	Placebo: 2 Weeks Earlier	Placebo: 3 Weeks Earlier	Placebo: 4 Weeks Earlier
Dependent Variable		Lockdown in COVID-19	
Placebo Window ×Promotion Incentive	0.0034	0.0023	0.0017
	(0.008)	(0.005)	(0.004)
Lagged Active Cases	0.0069**	0.0069**	0.0069**
	(0.003)	(0.003)	(0.003)
Dep. Mean	0.00432	0.00432	0.00432
Observations	3,240	3,240	3,240
Num. of Clusters	324	324	324
Dependent Variable		△Human Mobility	
Placebo Lockdown	-0.0421	-0.0090	0.0353
	(0.037)	(0.029)	(0.028)
Placebo Lockdown ×Promotion Incentive	-0.0338	-0.0059	-0.0360*
	(0.033)	(0.024)	(0.021)
Lagged Active Cases	-0.0068***	-0.0067***	-0.0068***
	(0.002)	(0.002)	(0.002)
Dep. Mean	-0.502	-0.502	-0.502
Observations	2,274	2,274	2,274
Num. of Clusters	321	321	321
Week FE	YES	YES	YES
Prefecture FE	YES	YES	YES
Controls×Week FE	YES	YES	YES

Notes: Controls include historical promotion likelihood, the number of prefectures within each province, log misused public funds detected by auditing institutions in 1999-2015, and log road travel hours to Wuhan, leader's calendar age, whether a leader has a college degree or above, whether a leader has a party school degree, the prefecture-level logarithmic GDP growth in 2017-19, two dummies indicating whether a leader has work experience at the central or provincial government, and mayor's promotion incentive. Promotion incentive, calendar age, and mayor's promotion incentive are standardized so that their means equal zero and their standard deviations equal one. The dependent variable in Panel B, *△Human Mobility*, denotes the difference in the mobility index between 2020 and 2019 on the same lunar calendar date in each prefecture. For regressions using △Human Mobility as outcomes, we further control for weather conditions, including average daytime rainfall and average daytime temperature and its square. To eliminate the real treatment effects, we exclude the sample after Xi's speech in the first panel, and the sample after lockdowns in the second panel. Robust standard errors are reported in parentheses, clustered at the prefecture level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.10 Evidence from SARS and the Long-Lasting Impact of SARS Experience on the Implementation of Lockdowns in COVID-19

	(1)	(2)	(3)
Dependent Variable	Lockdown in SARS	Lockdown in COVID-19	△Human Mobility
Window×Promotion Incentive	-0.0710***	-0.1188***	
	(0.023)	(0.036)	
Window×Prefecture's SARS Lockdown Experience		0.0981	
		(0.061)	
Window×Party Secretary's SARS Lockdown Experience		0.0537	
		(0.053)	
Lockdown			-0.3964***
			(0.099)
Lockdown×Promotion Incentive			0.1481***
			(0.055)
Lockdown×Prefecture's SARS Lockdown Experience			-0.2057**
			(0.098)
Lockdown×Party Secretary's SARS Lockdown Experience			0.1718*
			(0.097)
Lagged Active Cases	0.0045***	0.0004***	-0.0011**
	(0.001)	(0.000)	(0.000)
Dep. Mean	0.0629	0.160	-0.820
Week FE	YES	YES	YES
Prefecture FE	YES	YES	YES
Controls×Week FE	YES	YES	YES
Observations	6,666	4,860	2,889
Num. of Clusters	319	324	321

Notes: Controls include historical promotion likelihood (in Columns 2-3), the number of prefectures within each province, log misused public funds detected by auditing institutions (1999-2003 in Column 1 and 1999-2015 in Columns 2-3), and log road travel hours to epidemic centers (Guangzhou in Column 1, Wuhan in Columns 2-3), leader's calendar age, whether a leader has a college degree or above , whether a leader has a party school degree, the prefecture-level logarithmic GDP growth for the two years before events (2000-02 in Column 1 and 2017-19 in Columns 2-3), two dummies indicating whether a leader has work experience at the central or provincial government, and mayor's promotion incentive. Promotion incentive, calendar age, and mayor's promotion incentive are standardized so that their means equal zero and their standard deviations equal one. In Column 3, *△Human Mobility* denotes the difference in the mobility index between 2020 and 2019 on the same lunar calendar date in each prefecture. Robust standard errors are reported in parentheses, clustered at the prefecture level. *** p<0.01, ** p<0.05, * p<0.1.

3 Farewell to the God of Plague: Estimating the effects of China's Universal Salt Iodization on educational outcomes[51]

3.1 Introduction

3.1.1 Relevance to the dissertation research

Chapter 3 marks the beginning of my doctoral study and its findings serve as a benchmark for deeper analysis of the political structure in China elaborated in Chapters 1 and 2. In Chapter 3, the focus shifts towards a more centralized enforcement mode of public policies in China, which is believed to be more effective due to the absence of institutional interactions across different layers in the hierarchy. This chapter presents a case study of China's large-scale nutrition implementation policy implemented directly by the central government and analyzes the policy outcomes. By comparing and contrasting this mode of enforcement with the more flexible local enforcement modes explored in the previous chapters, Chapter 3 sheds light on the trade-offs between centralization and decentralization in policy enforcement and provides insights into the design of institutional arrangements that promote effective governance in China.

Chapter 3 focuses China's Universal Salt Iodization (USI) policy implemented in China in 1994. As the largest nutrition intervention policy in human history, the USI policy aimed to eliminate iodine deficiency diseases that could cause severe consequences on the cognitive abilities of future generations. Due to the central government's monopoly on salt production, distribution, and retail, the policy was effectively enforced vertically. To evaluate the policy's impact on children's later-life educational outcomes, we employed a difference-in-differences strategy to compare the educational outcomes of cohorts born before and after USI across counties with different iodine deficiency disorder levels based on population census data combined with county-level information. Our results demonstrate that the USI policy increased primary school enrollment by 0.6 percentage points and was more beneficial for girls and children born in rural areas. These findings further highlight the efficacy of public policies when enforced vertically by the central government.

3.1.2 Context and literature review

Scholars have long recognized a deficiency in the consumption of essential micronutrients as a

[51] This chapter is based on my previously published work acknowledged as Huang et al. (2020) in the dissertation.

primary impediment of health and human capital formation. Among various types of micronutrient deficiencies, iodine deficiency disorder (henceforth, IDD) has been the leading cause of preventable mental retardation (Ahmed, 2008).[52] Iodine deficiency *in utero* has irreversible detrimental impacts on the development of the infant nervous system, which ultimately limits the development of cognitive ability and hinders human capital formation.[53] There is substantial scientific evidence that the critical determinant of IDD prevalence is the iodine content in food and drinking water from which iodine intake is almost entirely derived (Murray et al., 2008). Iodine content in soil and water differs widely across localities as a result of the geological transformation between the sea and continental areas in ancient geological times. Therefore, IDD is a typical endemic disease.[54] According to a World Health Organization (WHO) report, nearly two billion people throughout the world live in iodine-deficient areas, a third of which are of school age (World Health Organization, 2007). WHO recommends USI as the cheapest and most efficient way to prevent IDD, especially for developing countries with large populations living in iodine-deficient areas.

This paper estimates the effect of China's USI policy – the largest nutrition intervention policy in human history – on children's later-life educational outcomes. China had over 700 million people living in areas suffering from IDD in the early 1990s. To eliminate IDD by 2000, the Chinese government initiated a USI policy on October 1, 1994, which mandated the iodization of edible salt throughout the country. Using China's 2005 population mini-census data combined with county-level information, we apply a difference-in-differences (DID) strategy which compares the outcomes of children born before and after the USI policy in 1994 across counties with different IDD prevalence levels. We find that the USI policy significantly increases primary school enrollment for the policy-affected cohorts in high goiter counties by 0.6 percentage points.[55] The costs of USI almost evenly fell on the iodized salt consumers through an in-price tax levied by China's central government. Therefore, our findings yield clear redistribution implications.

China's USI policy serves as an ideal natural experiment to examine the causal effects of salt iodization for two reasons. First, a state monopoly on salt in China ensured strict nationwide enforcement of USI and ruled out the potential endogeneity of producing or consuming iodized

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[52] Iodine is a key component of thyroid hormone, which is essential for metabolism. An adult requires about 60 μg of iodine per day to maintain the synthesis of thyroid hormone (Zimmermann, Jooste, and Pandavand, 2008). When iodine intake is insufficient, the secretion of TSH (thyroid-stimulating hormone) increases to produce thyroxin at higher efficiency, leading to the enlargement of the thyroid (i.e. goiter), which is a traditional signal of IDD.

[53] A large body of biological and medical literature has demonstrated that fetuses in the middle and late periods of utero are most vulnerable to iodine deficiency (Cao et al., 1994). Even mild or moderate iodine deficiency *in utero* will lead to lifelong cognitive impairment at varying degrees.

[54] According to Dicker et al. (2006), "Endemic refers to the constant presence and/or usual prevalence of a disease or infectious agent in a population within a geographic area."

[55] According to official policy criterion, if a county's goiter prevalence rate surpassed 3%, it is defined as a high goiter county.

salt, which may threaten a causal analysis of the policy effect. China instituted state monopolization of salt production, distribution, and sales beginning in 1990. Specifically, China's central government authorized China Salt Industry Corporation, a central state-owned enterprise, other local state-owned enterprises in the salt industry, and local branches of these enterprises throughout the country to monopolize the production, distribution, and sales of edible salt. Second, before the enactment of USI, China had the largest population in the world exposed to IDD and exhibited rich regional heterogeneity in IDD levels. The Chinese government conducted an iodine deficiency census in the 1980s, which furnishes us with a comprehensive dataset with rich county-level information, including IDD prevalence and water iodine content.

Our DID analysis is built on a solid analytical foundation. We find that there are no differential pre-trends of primary school enrollment across counties with different goiter prevalence rates. Our results are highly robust to a full battery of robustness checks and falsification tests. We use county-level water iodine content as an instrumental variable to deal with potential measurement error and nonrandomness in the spatial distribution of IDD prevalence. Although we have no direct measure of children's cognitive ability to certify the mechanism, we provide evidence that the USI policy does not work through improving children's physical health.

The effect of the USI policy on primary school enrollment in China is heterogeneous across several important socio-economic dimensions. We find that the USI policy effect almost exclusively shows up in rural areas rather than in urban areas with much better access to alternative ways of overcoming IDD (e.g., through seafood consumption) and girls benefit more from the USI policy. These findings forcefully suggest that the USI policy is desirable not only on efficiency terms but also on social justice grounds.

To the best of our knowledge, we are among the first empirical studies to investigate the causal effect of China's nationwide USI intervention on educational outcomes. In terms of research theme, this paper contributes to a growing body of scholarship evaluating the effects of various types of early-life micronutrient supplements on later-life outcomes, such as iron (Bobonis, Miguel and Puri-Sharma, 2006; Chong et al., 2016; Banerjee, Barnhardt and Duflo, 2018) and iodine (Field, Robles and Torero, 2009; Politi, 2014; Feyrer, Politi and Weil, 2017; Adhvaryu et al., 2020; Bengtsson, Sävje, and Peterson, 2020). This paper is not the first research to examine the effects of the eradication of iodine deficiency disorders on educational outcomes. In a pioneering work, Field, Robles, and Torero (2009) gauge a magnitude of 0.35-0.56 years of additional years of schooling for children treated *in utero* with iodine oil through an iodine supplementation policy in Tanzania. However, a replication work by Bengtsson, Sävje, and Peterson (2020) fail to establish a significant positive effect on educational attainment even when they use a larger sample and improve the precision of the treatment variable. Taking advantage of Switzerland's iodized salt introduction campaign, Politi (2014) documents a one percentage point increase in the secondary school graduation rate and a 0.7 percentage point increase in the tertiary school graduation rate. Motivated by another historical natural experiment, the fast salt iodization campaign in the United States in 1924, Feyrer, Politi and Weil (2017) find that this campaign had a significant effect on intelligence quotient when delving deeply into a unique dataset compiled from draft physicals for American army enlistees during World War I and World War II. Adhvaryu et al. (2020)'s analysis builds on the same historical natural experiment and provides evidence of considerable effects on labor force participation and income. However, the effect they find is smaller and insignificant in the subsample of males. In a word, reliable causal evidence, especially evidence from developing countries, is still inadequate to forcefully argue that large-scale salt iodization intervention causally improves educational outcomes. A formal empirical investigation into China's USI policy is itself of vital importance since it has a long-lasting impact on over 20% of the world's population. Our finding also serves as a counterweight to recent clamor and advocacy in China for abolishing USI.[56] Since most people might be unconscious of or underrate potential gains from micronutrient supplementation, policymakers should be cautious in handling this issue of important policy relevance.

Our study is closely related to a large and expanding literature on *Fetal Origins Hypothesis* (FOH), which examines the short- and long-term effects of specific factors *in utero* on later-life outcomes.[57] The recent FOH literature exhibits an increasing interest in examining the effects of positive policy-driven *in utero* interventions on later-life outcomes (Bharadwaj, Løken and Neilson, 2013; Almond, Currie and Duque, 2018; Nilsson, 2017). Researchers and policy-makers are especially eager to know whether some intervention policy tools derived from well-established causal evidence in scientific laboratory experiments or randomized controlled trials deliver their anticipated results when scaled up and implemented through government policies.[58] We contribute to this literature by investigating the causal effects of a nationwide health policy intervention in China on the early cognitive development of children (indirectly measured by educational outcomes).

Finally, our research speaks to a hotly debated issue about the role of geographic factors in shaping regional income disparities (Diamond, 1997; Sachs, 2003; Nunn and Puga, 2012; Henderson et al., 2017). Endemic diseases play a crucial role in translating geographic factors into human capital accumulation and regional development. Our findings advance existing studies by highlighting how imperceptible geographic-specific disparities can perpetuate unequal human capital endowments from the very beginning of human life. We also show that well-designed and strictly implemented government policy inventions can help to overcome geographical disadvantages.

[56] See http://epaper.bjnews.com.cn/html/2014-10/20/content_541739.htm?div=-1 for a case in the news.
[57] See Almond and Currie (2011) and Almond, Currie, and Duque (2018) for excellent literature reviews.
[58] See Banerjee et al. (2017) for a detailed discussion.

The rest of this paper proceeds as follows: Section 3.2 introduces China's IDD prevalence, China's state monopoly of salt starting in 1990, and national implementation of the USI policy in 1994. Section 3.3 describes our data. Section 3.4 formulates our identification strategy. Section 3.5 presents the empirical results. Section 3.6 concludes.

3.2 Background

3.2.1 Iodine Deficiency Disorders in China

Historically, China was among the countries most seriously affected by IDD. A nationwide census conducted in the 1980s found that IDD was pervasive in most areas in China, threatening a population of 425 million that accounted for roughly 40% of the total population living in IDD-affected areas throughout the world.[59] Figure 3.1 maps the spatial distribution of China's county-level goiter prevalence in 1980-1984. Almost every province (except for Shanghai) suffered from the incidence of goiter in the early 1980s to varying extents.

Although a limited number of counties in China gained access to iodized salt in the early 1960s, large-scale salt iodization campaign against IDD did not begin until the late 1970s. In 1979, China's Ministry of Health issued its first official salt iodization policy, which aimed to eradicate IDD in seriously affected areas essentially. Up to the end of 1982, there were 627 counties in China which had ever supplied iodized salt.[60] Although the first wave of salt iodization in the early 1980s had made some progress, IDD continued to be a public health challenge facing the Chinese government. By 1993, there were still six million babies born every year in the iodine-deficient areas. The average IQ of children born in iodine-deficient areas was 10-15 percentage points lower than those in iodine adequate areas.[61] These early efforts failed to eradicate IDD for two reasons. First, the central government only made seriously-affected areas a policy priority and paid little attention to those counties with a goiter prevalence rate under 3%, the official policy criterion for defining whether a county was "affected" by IDD.[62] Second, the salt iodization campaign in the 1980s was loosely enforced due to a lack of coercive action. Non-iodized salt was still available in iodine-deficient counties because the state did not monopolize salt production, distribution, or

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[59] Data source: *Plan for eliminating iodine deficiency disorders in China in 2000* (*Zhongguo 2000 nian xiaochu dian quefa bing guihua gangyao*), Ministry of Health, 1993.

[60] Appendix Figure A.3.1 displays the rollout of salt iodization in China in several specific years. Our subsequent analyses will fully account for the potential confounding effect brought about by those counties which already had access to iodized salt by the end of 1982.

[61] Data Source: *Outlines for eliminating iodine deficiency disorders in China in 2000* (*Zhongguo 2000 nian xiaochu dian quefa bing guihua gangyao*), Ministry of Health, 1993.

[62] *The Interim Rules for Prevention and Treatment of Endemic Goiter by Salt Iodization* (*Shiyan jiadian fangzhi dian quefa bing zanxing banfa*), was enacted by the Ministry of Health on December 21, 1979.

sales until 1990.

3.2.2 China's State Monopoly on Salt after 1990

China's central government enacted *Regulations on the Salt Industry* on March 2, 1990. At the heart of this administrative regulation is the introduction of a state monopoly on salt production, distribution, and sales. The government has strictly prohibited private production, distribution, or sales of salt since 1990. Any offenders are subject to being charged with criminal and civil liabilities. Furthermore, local branches of China's state-owned salt industry corporations (including the China National Salt Industry Corporation owned by the central government and other salt industry corporations owned by subnational governments) could only manage salt sales within their administrative regions.[63] The state also directly regulated prices for edible salt in the market. China's state monopoly of edible salt laid a solid foundation for the subsequent USI policy.

3.2.3 China's USI policy in 1994

WHO recommends universal salt iodization (USI) as the cheapest and most efficient way to prevent IDD, especially for developing countries with large populations living in iodine-deficient areas. According to WHO (2007)'s definition: "USI involves the iodization of all human and livestock salt, including salt used in the food industry. Adequate iodization of all salt will deliver iodine in the required quantities to the population on a continuous and self-sustaining basis." Thanks to the steady and inelastic demand for salt in daily diets, a small amount of salt fortification can provide adequate iodine to meet the needs of the human body. By 2008, over 120 countries had implemented some degrees of salt iodization, at least 97 of them had issued laws, regulations, or standards about salt iodization, and 34 countries had achieved USI, covering 70% of households throughout the world (UNICEF, 2008).

China's campaign against IDD gained renewed momentum in September 1990, when the World Summit for Children issued the *World Declaration on the Survival, Protection and Development of Children* and drafted the *Plan of Action for Implementing the Declaration*. China's then-Premier Li Peng signed these two documents on behalf of China in March 1991, solemnly declaring to the world that China would generally eradicate IDD by the end of the 20th century. The fact that a top leader of the Chinese government committed to the international community implied that China intended to accomplish this goal by all means. In August 1994, the State Council issued an official mandate that China would launch nationwide USI on October 1, 1994.[64] According to this USI

[63] China's state salt monopoly ended on January 1, 2017.
[64] The title of the State Council mandate is *Stipulations on the Enforcement of Salt Iodization to Eliminate Iodine Deficiency Disorders (Shiyan jiadian xiaochu dian quefa weihai guanli tiaoli)*, issued on August 23, 1994.

policy, as recommended by WHO, all counties throughout China should supply iodized salt (except for 25 officially approved counties with high water iodine levels and Tibet).[65] As a result of this mandate, the number of uncured goiter patients dropped dramatically from 16.1 million in 1995 to 8.7 million in 2001,[66] and cretinism in newborns was generally eliminated.[67]

Compared to salt iodization policies in other countries, several distinctive features characterized China's USI. First, China instituted a state monopoly on salt production, distribution, and sales. Private production, distribution, or sales of salt have been strictly prohibited since 1990. Local branches of China's salt industry corporation were only authorized to manage salt sales within their administrative regions. This regionally-based state monopoly means that households would have no access to non-iodized salt once their county was covered by USI. Second, China's USI since 1994 was universally imposed throughout the country in a short time, leaving little room for local strategic reactions to the policy and thus contributing a fruitful natural experiment setup. Third, drawing on the lessons of weak enforcement from the first wave of salt iodization in the 1980s, China's central government organized a sophisticated national surveillance program on salt iodization, effective since 1995, to ensure the strict enforcement of USI.

3.3 Data

3.3.1 County-level Data

This paper's analysis employs a comprehensive dataset of county-level information on the geographic distribution of IDD prevalence, water iodine content, and iodized salt supply drawn from China's Iodine Deficiency Census in the 1980s, which is compiled from *The Atlas of Endemic Diseases and their Environments in the People's Republic of China*. This data source, however, has one main drawback: it only reports the ranges of goiter and water iodine content (categorized by several groups) instead of their continuous values for each county.[68] Considering this restriction, we define a dummy variable (labeled *Highgoi*) indicating whether a county's goiter prevalence rate surpasses 3% in 1980-1984 to measure county-level IDD prevalence before the enforcement of USI. As Section 3.2.1 explained, the Chinese government explicitly introduced this 3% cutoff to define high goiter counties. In our final sample, 481 counties (26% of the full

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[65] Tibet was excluded in the 1994 wave of USI due to some technical difficulties since Tibet was one of China's most underdeveloped areas.
[66] Data source: *China's Health Statistical Yearbook*, various years.
[67] According to Chen and Hetzel (2010), "Endemic cretinism includes two syndromes: a more common neurological disorder with brain damage, deaf mutism, squint and spastic paresis of the legs and a less common syndrome of severe hypothyroidism, growth retardation and less severe mental defect. Both conditions are due to dietary iodine deficiency and can be prevented by correction of iodine deficiency before pregnancy."
[68] For instance, the intervals of goiter prevalence rate include 0, (0, 0.03), [0.03, 0.1), [0.1, 0.2), [0.2, 0.3) and [0.3, 1).

sample) are high goiter counties. A rich epidemiology literature shows that the IDD prevalence rate in China rises sharply in areas where water iodine content is below 5μg/L (Yu et al., 2004; Wang et al., 2011). Therefore, we define a dummy variable indicating whether a county's average water iodine content is less than 5μg/L, which we use as an instrumental variable for IDD prevalence in later analyses.

We consider several crucial time-invariant county characters, including whether a county had supplies of iodized salt before 1982, a county's distance to China's nearest coastline to capture its residents' access to seafood (alternative ways of iodine intake), and whether a county is located in a pastoral area to account for the potential influence of dietary habits.[69] County-level geographic information used in this paper comes from China's National Geographic Information System (CNGIS).

Finally, we collect data on the spatial distribution of counties affected by three other endemic diseases, namely Keshan Disease, Kaschin-Beck Disease, and Schistosomiasis, to conduct a placebo test.[70] We compile the list of Keshan Disease and Kaschin-Beck Disease affected counties in 1970-1982 from *The Atlas of Endemic Diseases and their Environments in the People's Republic of China,* and that of Schistosomiasis affected counties in 1981 comes from *The Atlas of Schistosomiasis Infection in the People's Republic of China*.

All of the county-level data are adjusted to administrative boundaries in 2005. We exclude city districts due to their special status in China.[71] All of the counties in Tibet are also excluded because of data availability. Our final sample includes 1,883 counties, covering 89% of China's entire population.

3.3.2 Individual-level Data

Our data on individuals' characteristics (such as age, gender, educational attainment, and health status) are drawn from China's population mini-census in 2005 (covering a 0.2% random sample of China's total population in 2005).[72] It is the best data available for us at present. We focus on

[69] People living in pastoral areas generally eat more meat. Considering the concentration of iodine throughout the food chain, they might have a better situation with regard to IDD.

[70] Keshan disease is a congestive cardiomyopathy caused by a combination of dietary deficiency of selenium and the presence of a mutated strain of Coxsackievirus. Kashin-Beck disease is a chronic, endemic type of osteochondropathy that is mainly distributed from northeastern to southwestern China, involving 15 provinces. Schistosomiasis is caused by digenetic blood trematodes. The spatial distribution of the three major endemic diseases are plotted in Appendix Figure A.3.2.

[71] The results, which we will present in Section 5.3, are highly robust to the inclusion of city districts.

[72] The Chinese government conducts a comprehensive population census every ten years and a 1% randomly sampled mini-census every five years in-between full population censuses. Specifically, the recent population censuses were conducted in 1990, 2000 and 2010, and mini-population censuses were conducted in 1995, 2005 and

individuals born in 1987-1997 for two reasons. On the one hand, the National People's Congress of China passed *Compulsory Schooling Law* in April 1986 requiring every Chinese citizen to receive at least nine years of compulsory education, which came into effect on July 1, 1986. Therefore, children born after 1986 are presumed to be immune from the disturbing impacts of the *Compulsory Schooling Law*. On the other hand, rural children were allowed to attend primary school when they were eight years old in some provinces (e.g., Inner Mongolia); thus, we restrict our sample to those born before 1998 to rule out the potential confounding effects of different school entrance ages. The USI was enacted in October 1994, and the birth cohorts affected by USI would be those who were born in 1995-1997 and aged 8-11 on November 1, 2005 (the reference time of the 2005 population mini-census). Given that these individuals had not reached the normal age for entering middle school, the most meaningful outcome of interest will be whether they attended primary school.

Our main dependent variable is a dummy indicating whether a child had ever enrolled in primary school.[73] In the 2005 population mini-census, each household head was asked to self-assess current health status for each of his (or her) family members with four default choices: healthy, capable of having normal work and life, unable to take care of oneself, or hard to tell. While the answer to this question may encompass several dimensions of health status, it mainly reflects physical health. Previous literature demonstrates that self-reported health status is a good predictor of health (Idler and Benyamini, 1997; Hoynes, Schanzenbach and Almond, 2016). Therefore, we construct a dummy variable—*Healthy*—indicating whether a child's health status was evaluated as healthy as an outcome to help rule out a competing channel that the USI policy mainly takes effect through improving physical health.

We use an individual's registered address in China's strict household registration system (well-known as the *hukou* system) instead of his (or her) current living address in 2005 when the population mini-census was undertaken.[74] Every Chinese citizen is required to be registered in the *hukou* system after birth. Under the *hukou* system, moving one's *hukou* across counties was difficult (especially for rural citizens) in the 1990s. Strong disincentives existed: citizens who did not hold the local *hukou* of a particular place could not access public services (such as public schools, medical insurance, and unemployment benefits) reserved for the *hukou*-holders. Moreover, even if parents worked in another county, their children—the so-called "left-behind children" in China—typically remained in the hometown. In this way, we can mitigate concerns about endogenous migration to a great extent. As a final way of confirmation, we construct a dummy variable *Migration* indicating whether an individual's living place was different from his or her

2015. However, individual-level data for the 2010 and 2015 population censuses are still unavailable. Our analyses' data set is a 20% random sample of the original 2005 mini-census, which is provided by China's National Bereau of Statistics.

[73] Specifically, the value of those school-age children who had dropped out of school was assigned as one.

[74] For more details about China's *hukou* System, please refer to the introduction in Chan (2015).

registration place in the *hukou* system to address this issue.

Table 3.1 presents the summary statistics for the main variables used in our subsequent analyses.

3.4 Empirical Strategy

We formulate a difference-in-differences specification to identify the causal effect of USI on primary school enrollment as follows:[75]

$$y_{ict} = \beta Highgoi_c \times Post_t + \mu_c + \gamma_t + \delta_{pt} + \theta \mathbf{X_c} \times \gamma_t + \varepsilon_{ict} \quad (3.1)$$

where subscripts i, c, p, and t index individual, county, province, and birth cohort year. y_{ict} denotes the outcome variables. β is the parameter of our interest identified from variation within counties across birth cohorts. $Highgoi_c$ is an iodine deficiency disorder measurement (i.e., whether a county was defined as a high goiter county according to the levels of goiter prevalence rate in 1980-1984) in county c. Since the USI policy was enacted in October 1994 and IDD mainly affects nervous system development *in utero*, the first affected cohort was born in 1995. It is noteworthy that even though the central government announced the USI policy would start on October 1, 1994, there might be some delays in the enforcement of the policy given that transporting and distributing iodized salt takes time. Therefore, it is difficult to assign a precise starting time of the USI policy for each county. In practice, we arbitrarily let $Post_t$ equals 1 for every individual born from 1995 and 0 if otherwise. This approach would lead us to underestimate the real policy effect since we regard some children born between October 1994 and December 1994 who might receive partial treatment as non-treated cohorts. μ_c denotes county fixed effects, and γ_t denotes birth cohort fixed effects. We also include province-cohort fixed effects δ_{pt} to absorb province-by-cohort invariant confounders. The standard errors of ε_{ict} are clustered at the county level.

As is standard in difference-in-differences estimations, interpreting our estimated coefficient as a causal effect relies on one necessary condition: a parallel trend before the policy intervention between treatment and control groups. We follow Duflo (2001) to control for the interaction of a variety of county characteristics $\boldsymbol{X_c}$ with birth cohort fixed effects γ_t to allow their impact on primary school enrollment to vary by birth cohorts. These county characteristics include whether a county had already gained access to iodized salt before 1982, whether a county locates in a pastoral area and a county's distance to China's nearest coastline. We also report event study

[75] Throughout this paper, we use linear probability models, which allow a straightforward interpretation of the estimated coefficients. More importantly, since our econometric specification controls for a large number of fixed effects and we use population census data with a large sample size, estimating nonlinear discrete choice models would involve daunting computing difficulties.

estimates and conduct a full set of other robustness checks to verify the parallel trend assumption.

Another potential threat to our estimation is the confounding effects stemming from endogenous migration. As we have documented above in section 3.2, the use of an individual's registered address in China's *hukou* system instead of his (or her) current living address in 2005 can mitigate this concern to a great extent. Furthermore, endogenous migration of households in response to variation in IDD prevalence across regions, which would threaten our identification strategy, was unlikely to have occurred because county-level IDD prevalence rate data was not made public. Therefore, people's awareness of IDD endangerment would not drive migration across counties. To further eliminate this worry, we also investigate USI's impact on *Migration* (a dummy variable indicating whether an individual's living place was different from her registration place in the *hukou* system) to provide additional quantitative confirmation.

3.5 Results

3.5.1 Event Study Estimates

Before proceeding to our main empirical results, we first formally test the parallel trend of primary school enrollment between treatment and control groups. Appendix Table A.3.1 presents the descriptive statistics of primary school enrollment by birth cohorts. Before the implementation of USI in 1994, there was a marked difference in the average primary school enrollment rate between high goiter and low goiter counties. However, the pattern saw a striking break that the differences vanished from the cohort born in 1995, suggestive of the gift of USI.

To formally investigate the dynamic patterns of primary school enrollment rate across different birth cohorts, we conduct an event study by interacting $Highgoi_c$ with a full set of birth cohort dummies as in equation 3.2:

$$y_{ict} = \sum_{k=1987}^{1997} \beta_k Highgoi_c \times \gamma_k + \mu_c + \gamma_t + \delta_{pt} + \theta \mathbf{X_c} \times \gamma_t + \varepsilon_{ict} \quad (3.2)$$

The cohort 1994 is omitted as the reference group. Other variables share the same meaning as those in equation 3.1. Figure 3.2 displays the estimated coefficients along with 95% confidence intervals for β_k in our event study specification in equation 3.2. The estimates in the pre-USI period are statistically indistinguishable from zero in stark contrast with a statistically significant and drastic change beginning in 1995. Overall, Figure 3.2 visually depicts a time pattern of primary school enrollment for high and low goiter counties generally consistent with the parallel trend assumption of our DID strategy.

3.5.2 USI's Effects on Primary School Enrollment

Table 3.2 reports our baseline difference-in-differences results. Column (1) only includes the basic controls of county, birth cohort, and province-by-cohort fixed effects, and column (2) further controls for flexible cohort trends varying by a set of pre-determined county characteristics $\boldsymbol{X_c}$.[76] Both columns report a significantly positive effect of the USI policy on primary school enrollment. Taking column (2) as our preferred specification for interpretation, we find that high goiter counties achieve a 0.6 percentage point increase in primary school enrollment compared to low goiter counties as a result of USI.

Admittedly, the magnitude of our estimated USI's policy effects might initially appear to be a little bit small as since the primary school education attendance there has already reached a high level (99% on average in our sample) in our sample period. Next, we try to situate our estimate in the existing literature by translating it into an effect on average years of schooling, which can be compared with previous studies' estimates (e.g., Field et al., 2009, Politi, 2014, and Adhvaryu et al., 2020). According to the aggregate data of China's population mini-census in 2015, the average years of schooling of people who have a primary school education and born in 1995-1997 is 12.69 years. To be cautious, we arbitrarily assume that children affected by the USI policy only received an additional six years of schooling. Multiplying our estimated policy effects by six suggests that China's USI leads to an increase in the average years of schooling of the affected group by 0.036 years (equivalent to 13.14 days). This effect on average years of schooling is comparable to previous estimate based on a historical natural experiment of the United States (about two weeks for females and even smaller and statistically insignificant for males) in Adhvaryu et al. (2020) and the most optimistic estimate (0.065 years) by Politi (2014) for Switzerland.[77] A more optimistic analysis provided by Field et al. (2009) finds an average policy effect of 0.35-0.56 additional years of schooling as a result of receiving iodization treatment. We should be cautious about this estimate since the replication work by Bengtsson, Sävje, and Peterson (2020) only finds an effect on average years of schooling ranging from 0.026 to 0.155 and all of these estimates are statistically insignificant. Nevertheless, this calculation indicates that our estimated USI policy effect is at least comparable to the estimates using other contexts on the order of the magnitude. Given China's huge population scale, the USI's policy relevance should not be overlooked.

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[76] The results throughout this paper are highly robust to the inclusion of several time-varying controls such as county-level fiscal expenditures and educational expenditures, which are available upon request.

[77] Politi (2014) does not offer a direct estimate on average years of schooling but find a 1.05 percentage points increase of secondary education graduation rate and a 0.474 percentage points increase of tertiary education graduation rate resulting from the introduction of iodized salt in Switzerland. For simplicity, we optimistically assume that the additional average years of schooling for secondary school and tertiary school graduates are 3 and 7 years. The upper bound for the policy effects of Switzerland's salt iodization should be 0.0647 (3*0.0105+7*0.00474) years.

3.5.3 Threats to Identification and Robustness Checks

We will next present additional results to deal with potential threats to our identification strategy and to check whether our baseline results are sensitive to alternative econometric specifications and considering a variety of alternative conjectures.

Falsification Test. There is a possibility that our results simply capture changes in pre-existing county trends that began before the USI policy. We use all possible years (1988-1994) as a false USI starting year and define $Post_t$ respectively for the seven years. The results, which are reported in Figure 3.3, show that none of the false treatments produces significant effects on primary school enrollment and the magnitude of each estimated coefficients is small compared with our baseline estimate (0.006). This falsification test helps us to affirm that our main results in Table 3.2 are not likely to be driven by any pre-existing county trends that have not been absorbed by our baseline controls.

Placebo tests using other endemic diseases. To reinforce our DID results, we use three other major endemic diseases, Keshan disease, Kaschin-Beck disease, and Schistosomiasis, to conduct placebo tests. Just like IDD, these three diseases are also related to specific geographical factors, but they have nothing to do with IDD. If the USI policy only works in a manner associated with the spatial distribution of IDD, we should not see any significant effects using regressors constructed for Keshan Disease, Kaschin-Beck disease, and Schistosomiasis. The results presented in Table 3.3 accord with this expectation. The estimates in columns (1)-(3) are negative and statistically insignificant, in stark contrast with our baseline results using IDD. This practice helps to alleviate the concern that our results are driven by contemporaneous improvements in health status induced by the eradication of other endemic diseases.

Robustness to Alternative Geographic Samples and Model Specifications. First, one may worry that our findings are sensitive to specific geographic sample choices since our baseline regressions drop city districts from the sample. To increase transparency, we bring back all of the sample in city districts and re-run our baseline regression in column (1) of Table 3.4. The DID estimate still reveals a significant policy effect although the magnitude decreases to some extent since our main results are exclusively driven by children born in rural areas as we document later in Section 5.5. Second, we exclude from the sample 25 officially approved counties with high water iodine levels where USI was not implemented and repeat our baseline regression in column (2) and the result is broadly similar. Third, we exclude from the sample iodine adequate counties where the average water iodine content is larger than 5μg/L. The result is reported in column (3), where the point estimate increases from 0.006 in our baseline specification to 0.0073. This robustness check boosts our confidence that our finding derives primarily from USI's blocking effect on IDD. Fourth, one may have concerns that counties with salt iodized before USI would experience a non-parallel trend of primary school enrollment against their counterfactuals, which might drive our findings. 635 counties (33.7% of our full sample) had already gained access to iodized salt until 1982. To formally address this concern, we exclude these counties. The point estimate in column (4) of Table 3.4 from this subsample decreases slightly from 0.006 to 0.005 but is still highly significant. Fifth, we allow each county to have its linear time trend in column (5) and our result still holds. Sixth, we apply the most demanding specification in column (6) by controlling for prefecture-by-cohort fixed effects. In this way, we are comparing counties within the same prefecture, which are geographically close to each other. The magnitude of our estimated coefficient drops slightly but still highly significant, implying that the spatial clustering of the IDD prevalence might not contaminate our findings.

Ruling out the Confounding Effects of Delayed Primary School Enrollment. There exists a possibility that children in poor regions may postpone but still attend primary school. If this is the common case, our main finding might only reflect the effects of delayed primary school entry instead of the permanent lifetime gains (i.e., from never enrolling in to ever enrolling in primary school), which might put us at risk of exaggerating our estimated USI policy effects. To formally address the concerns hinging on the role of children's schooling age in generating our results, we collect detailed historical administrative laws and regulations on legal schooling age promulgated by each province government.[78] Nine provinces in our sample, Inner Mongolia, Jilin, Jiangxi, Guangxi, Guizhou, Yunnan, Qinghai, Ningxia, and Xinjiang, allowed children to enroll in primary school after seven years old in certain circumstances. To isolate potential confounding effects of the differences in legal schooling age which might happen to be correlated with the spatial distribution of high goiter counties, column (7) of Table 3.4 trims counties in these provinces and the result is broadly similar.

Ruling out Alternative Conjectures. As mentioned in the introduction, IDD is an endemic disease which inflicts permanent damage to children's cognitive abilities and thus affects their schooling enrollment. A large literature has documented abundant evidence on the role that IDD plays in destroying cognitive ability (Bleichrodt and Born, 1994; Zimmermann et al., 2008; Feyrer, Politi and Weil, 2017). Our population census dataset does not have information on children's cognitive abilities, so we cannot directly test the linkage between the USI policy and improvement in cognitive ability. As a related attempt, we want to examine whether the USI policy affects the physical health of birth cohorts since 1995. In principle, the USI policy should not work directly on the physical health of the affected children, so if the USI policy works, we should not find any evidence of its effect on physical health. We construct a dummy variable from the 2005 population mini-census, namely *Healthy*, according to interviewees' assessment as proxies to children's physical health. We re-run our baseline DID estimation using this measure as the dependent

[78] The original documents of these laws and regulations are available upon request.

variable and present the result in column (1) of Table 3.5. The estimate is small in magnitude and statistically insignificant, revealing no evidence that the USI policy improves the self-reported health status. This finding lends support to our claim that USI does not affect primary school enrollment through the physical health channel. In column (2), we investigate USI's effects on *Migration* (a dummy variable indicating whether an individual's living place was different from his or her registration place in the *hukou* system). The point estimate is not only statistically insignificant but also very small in magnitude, which helps rule out the possibility that migration drives our findings. Finally, we add both *Healthy* and *Migration* as covariates in column (3). It is not surprising to see a strong positive correlation between health status and primary school enrollment. Moreover, most migrants in China moved from the countryside to urban areas, where their children would have comparatively better if still less than full access to educational opportunities. The point estimate of our DID regressor of interest remains stable, thus indicating little evidence that other competing hypotheses—such as physical health and endogenous migration patterns associated with USI—account for our findings.

3.5.4 Instrumental Variable Results

The basic DID strategy underpinning equation 3.1 might be threatened by endogeneity arising from nonrandomness of IDD for two reasons. First, our IDD proxy suffers from measurement error since we do not have IDD prevalence rate data right before 1995 and so use pre-determined values in 1980-1984 as a substitute proxy. Second, potential pitfalls arise when some unobservables correlated with IDD exert heterogeneous effects on school enrollment before and after 1994. On the one hand, county governments with better economic conditions may be more likely to employ propaganda to strengthen people's awareness of IDD's potential dangers. Or those residents with higher social-economic status may be more likely to change their consumption behavior in response to the government's propaganda. On the other hand, counties with better economic conditions were closer to universal primary school enrollment before USI and thus may have little room for further improvement. Either way, USI may be more effective in counties endowed with better economic conditions. In these cases, potential confounding omitted variables (OV) (e.g., economic development level) are correlated with IDD prevalence rates and have heterogeneous effects on school enrollment in the pre- and post-treatment era, leading our DID estimate to be biased. Therefore, we need to apply an instrumental variable strategy to address the endogeneity issue caused by measurement error and potential omitted variables.

Water iodine content is the first-order determinant of a county's IDD prevalence. The epidemiology literature shows that there is a strong correlation between water iodine content and goiter, and the IDD prevalence rate in China rises sharply in areas where water iodine content is below 5μg/L (Yu et al., 2004; Wang et al., 2011). These well-documented scientific findings provide us with a natural instrumental variable for IDD prevalence, which is a dummy variable indicating whether a county's water iodine content is less than 5μg/L (labeled *Low_Iodine*). We map the spatial distribution of China's iodine deficient counties in Figure 3.4. It works as a valid IV for two reasons. First, a county's water iodine content directly and significantly affects its level of IDD. Second, since iodine and iodine compounds are not important raw industrial materials, water iodine content is exclusively determined by exogenous local natural conditions shaped in the geological age and therefore should be orthogonal to any unobservables we are aware of that affect IDD prevalence rates. Admittedly, water iodine content still suffers from measurement error to some extent. However, as long as the measurement error of water iodine content is generally orthogonal to the measurement error of IDD prevalence, this instrumental variable is still valid. The first-stage and second-stage regressions are specified in equations 3.3 and 3.4 as follows:

$$Highgoi_c \times Post_t = \alpha Low\_Iodine_c \times Post_t + \mu_c + \gamma_t + \delta_{pt} + \theta \mathbf{X_c} \times \gamma_t + v_{ct} \qquad (3.3)$$

$$y_{ict} = \beta \widehat{Highgoi_c \times} Post_t + \mu_c + \gamma_t + \delta_{pt} + \delta \mathbf{X_c} \times \gamma_t + \varepsilon_{ict} \qquad (3.4)$$

where *Low_Iodine*$_c$ is a dummy variable as defined above. $\widehat{Highgoi_c \times} Post_t$ indicates the predicted value of $IDD_c \times Post_t$ in the first-stage regression. Other symbols are defined similarly to equation 3.1.

Columns (1)-(3) of Table 3.6 present the first-stage, reduced-form and second-stage results respectively. The Kleibergen-Paap *F*-statistic is about 84, implying a quite strong first-stage result. The first-stage estimate in column (1) indicates that iodine deficient counties are more likely to become high goiter counties, echoing well-established scientific evidence that iodine content in the ground layers of the earth is the first-order determinant of IDD. Given the exogeneity of our IV, we can safely argue the intent-to-treat (ITT) estimate in column (2) as a causal effect where the exclusion restriction is not needed. Our reduced-form estimate directly links iodine deficiency to the lower primary school enrollment. This finding makes a solid step forward in highlighting how imperceptible geographic-specific disparities perpetuate the inequality of human capital endowments from the very beginning of human life. We also show that the well-designed and strictly-implemented USI policy contributes to overpower geographical disadvantages (in our context, iodine deficiency in the soil and water). The IV estimate in column (3) shows that the USI policy brings about a 0.93 percentage point increase in primary school enrollment for high goiter counties compared with low goiter counties, which is approximately 1.55 times the corresponding DID estimate in column (2) of Table 3.2. The difference suggests that the DID estimate understates the real USI policy effect due to some combination of omitted variables bias and measurement error.

To sum up, applying an instrumental variable approach further confirms our findings in the baseline DID specification.

3.5.5 A Simple Cost-benefit Analysis

In this subsection, we conduct a rough cost-benefit analysis to better appreciate the economic magnitude of our estimated USI's policy effect.

According to the official data provided by the Chinese government, salt iodization cost approximately 25 RMB per ton of salt in 1995 constant prices. The Chinese central government imposed a new in-price excise tax called the "Salt Iodization Fund" to cover this expenditure; we suppose that this cost was to be paid by the whole population. According to data from the *Chinese Population Nutrition Survey* (CPNS) in 1992, a typical individual in China consumed 13.9 grams of salt per day on average. In other words, each consumer paid only 0.127 RMB every year for salt iodization, which took up only 0.003% of the per capita disposable income of urban citizens (4,283 RMB) or 0.008% of rural citizens' income per capita (1,577.7 RMB) in 1995.[79] The economic cost of USI was negligible and should not impair household welfare. The aggregate cost of USI was about 153.8 million RMB in 1995.

There were about 41.1 million children aged 7-9 years old in our sample according to the 2005 population mini-census. Applying the DID estimate (0.006) as a lower-bound and the IV estimate (0.0093) as an upper-bound, implementing USI was associated with an additional 82,200 to 127,410 children attending primary school every year.[80] A simple back-of-the-envelope calculation implies an average cost of about 121 to 187 RMB to save an out-of-primary-school child, which is neglected comparing to its impressive economic and social benefits. Considering that the cost of USI almost evenly fell on the iodine salt consumers of the entire nation via the "Salt Iodization Fund" levied by China's central government, our estimate yields clear redistribution implications.

3.5.6 Heterogeneous Effects of USI

This subsection explores heterogeneity regarding USI's effects on primary school enrollment, based on two important socio-economic dimensions, including rural-urban difference and gender.

We first look at the rural-urban difference in the USI policy effect by applying DID to urban and rural subsamples separately. Urban citizens generally had easier access to seafood and relevant knowledge about how to prevent IDD, and so we expect that rural people were more sensitive to

[79] Data comes from *China's Statistic Yearbook* and *China's Education Statistic Yearbook*.
[80] This calculation is conservative since China's school year age children were still fast-growing in the first decade after the implementation of USI.

the effects of the mandatory USI policy. As shown in columns (1)-(2), Table 3.7, we find that the effects observed in our baseline results are predominantly driven by the policy effect on rural citizens, who are most vulnerable to IDD.

Next, we examine how the USI policy works differently by gender in columns (3)-(4) of Table 3.7. We can see a larger effect on females, which echoes the findings in existing studies using data from Tanzania and the United States that females are more prone to be affected by IDD (Field, Robles, and Torero, 2009; Adhvaryu et al., 2020). Our finding also helps to explain the relatively large initial gender education gap (2.1 percentage points in primary school attendance for people born in 1987-1994 according to the aggregate data provided by *China's Educational Statistical Yearbook, 2002*) and its rapid disappearance after USI in China.

To sum up, children born in rural families and girls benefit more from the USI policy. When generalized to a broader context, these findings speak to a dearth of studies focusing on how public policies help to enhance social justice and mobility (Almond and Currie, 2011; Aizer and Currie, 2014; Chetty et al., 2014). IDD-affected areas are not the lands of opportunity, and children born in these areas are more likely to have limited cognitive ability and lose chances to go to school. Our findings provide strong evidence in support of mandatory universal public health interventions like USI as an effective way to correct inequality at birth and promote social mobility at a low cost.

3.6 Conclusion

3.6.1 Findings and policy implications

Iodine deficiency disorder (IDD) is the leading cause of preventable mental retardation while universal salt iodization (USI) has been one of the most widely used weapons to fight back against IDD-related health problems. This paper studies the effects of China's USI policy in 1994 on children's later-life educational outcomes. Using population census data, our difference-in-differences strategy compares the school enrollment of cohorts born before and after USI across counties with varying IDD prevalence. Empirical findings suggest that the USI policy increased primary school enrollment by 0.6 percentage points for the policy-affected cohorts. Further investigation suggests that rural children and girls benefit more from USI. Since the costs of USI almost evenly fell on the iodized salt consumers through an in-price tax levied by China's central government, our findings yield clear redistribution implications.

We show that a well-designed and strictly-implemented government policy contributes to overpowering initial geographical disadvantages. Our finding also serves as a counterweight to recent clamor and advocacy in China for abolishing USI. Considering that most people might be unconscious of or underrate potential gains from micronutrient supplementation, policymakers should be cautious in handling this issue of tremendous policy relevance.

One main limitation of this paper is that our data only cover a relatively short time window and provides limited outcomes for us to look at the comprehensive effects of the USI policy. The quantification of USI's long-term effects on labor market performance and productivity still awaits future work. Since there has been an increasing interest in detecting human intelligence's impacts on a full battery of socioeconomic outcomes, our research design enabled by China's unique institutional context can easily be extended to future studies on these important issues.[81]

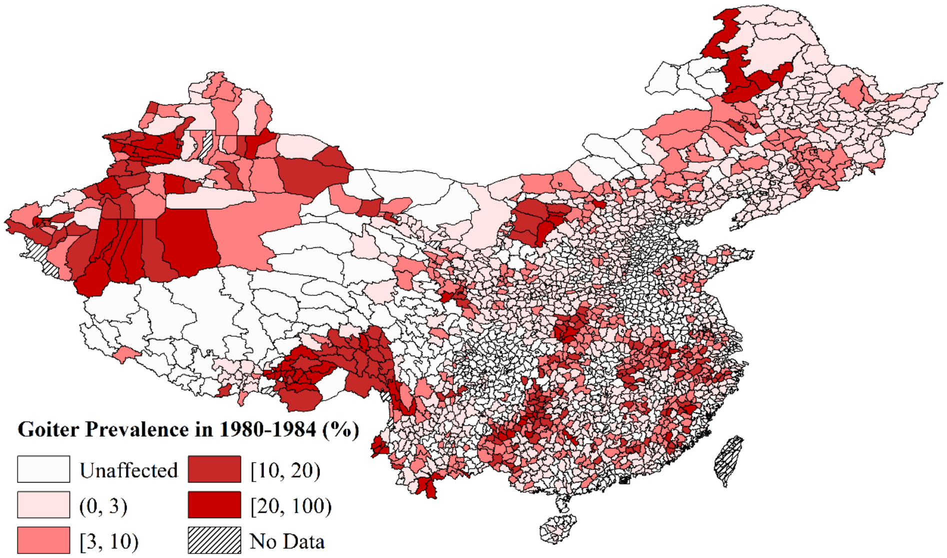
3.6.2 Relevance to other chapters

Throughout the dissertation, I have explored the intricacies of policy enforcement in China and the impact of political and institutional forces on policy outcomes. While Chapters 1 and 2 provided insights into the complexities of local enforcement modes and their impact on policy outcomes, Chapter 3, on the other hand, examined a more centralized enforcement mode and highlighted the efficacy of public policies when enforced vertically by the central government, and complements the entire discussion as an effective benchmark model for comparison.

Combining the findings from the 3 chapters, my dissertation yields rich implications for policymakers and scholars seeking to understand policy enforcement in China and other similar contexts. The research highlights the importance of considering the political and institutional context in designing and implementing public policies. Together, these three chapters contribute to the literature on policy enforcement in China, and shed light on the critical importance of political and institutional factors in shaping governance outcomes.

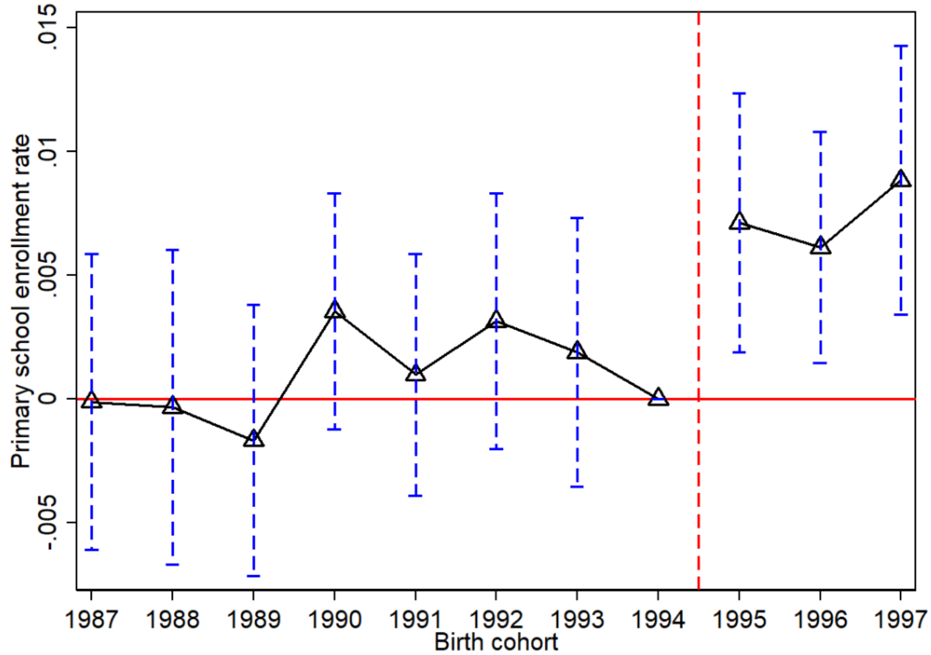
[81] For example, recent experimental research shows that higher intelligence groups cooperated far more than the lower IQ group in a repeated prisoner's dilemma game (Proto, Rustichini and Sofianos, 2019).

Figure 3.1 Goiter Prevalence in 1980-1984



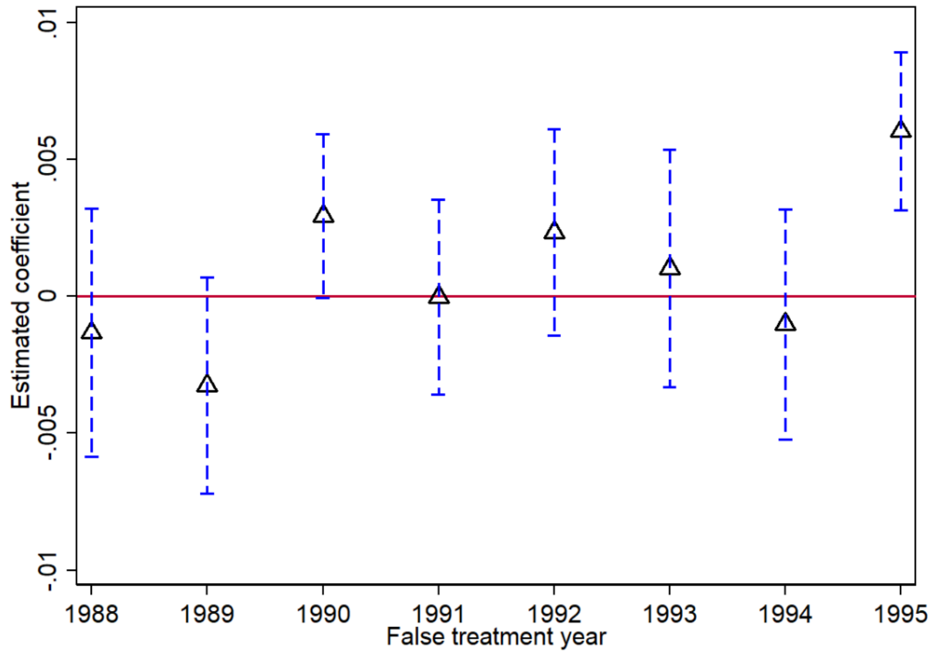
Data Source: The Atlas of Endemic Diseases and their Environments in the People's Republic of China.

Figure 3.2 Event Study



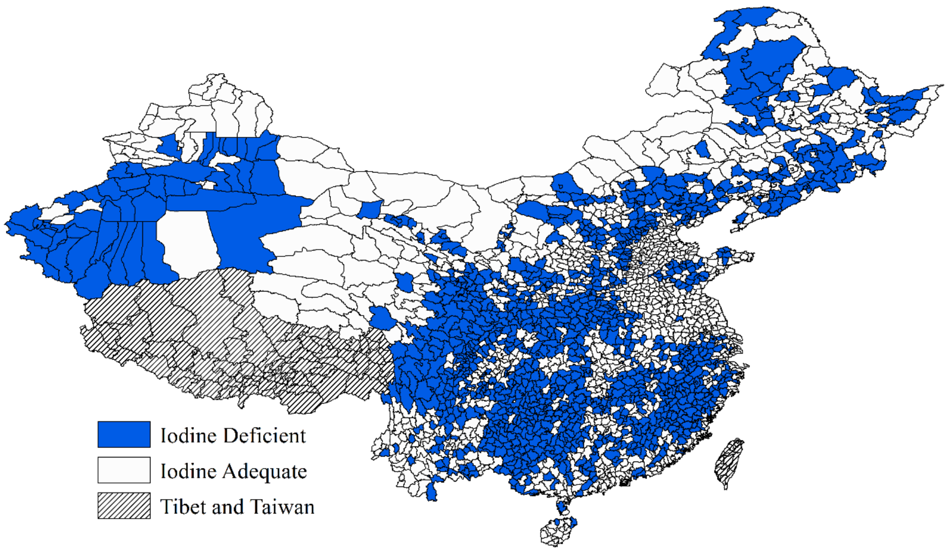
Notes: This figure reports the estimated coefficients along with 95% confidence intervals for a variety of birth cohort dummies in equation 3.2. The cohort 1994 is omitted as a reference group.

Figure 3.3 Falsification Test



Notes: This figure conducts a series of falsification tests by using all possible false treatment times (1988-1994) to construct our main regressor. We focus on the sample born in 1987-1994 to tease out the real treatment effect. We also include our baseline estimate (0.006) using the real treatment time (1995) as a reference. County-level clustered standard errors are in parentheses.

Figure 3.4 Spatial Distribution of Iodine Deficient Counties in China



Notes: Counties where the average water iodine content is lower than 5 μg/L are defined as iodine deficient counties, and iodine adequate counties if otherwise. Water iodine content data for counties in Tibet and Taiwan are unavailable.

Sources: *The Atlas of Endemic Diseases and their Environments in the People's Republic of China.*

Table 3.1 Descriptive Statistics

Variables	Full Sample Obs.	Mean	S.D.
Panel A. County-level Variables:			
High goiter	1,883	0.255	0.436
Salt iodized, 1982	1,883	0.337	0.473
Distance to coastline (measured by radian degree)	1,883	6.333	6.429
Pastoral area	1,883	0.115	0.319
Low iodine	1,883	0.510	0.500
Panel B. Individual-level Variables:			
Enrolling in primary school	346,674	0.990	0.101
Healthy	346,674	0.994	0.0755
Migration	346,674	0.0561	0.230

Table 3.2 The Effects of USI on Primary School Enrollment

	(1)	(2)
Dep. Var.	Enrolling in primary school	
High goiter×Post	0.0063***	0.0060***
	(0.001)	(0.001)
Dep. Mean	0.990	0.990
County FE	YES	YES
Cohort FE	YES	YES
Province×Cohort FE	YES	YES
Controls	NO	YES
Observations	346,674	346,674
Num. of Clusters	1,883	1,883

Notes: This table reports our baseline difference-in-differences estimations of the USI's effects on primary school enrollment. County-level clustered standard errors are in parentheses. *** denotes significance at 1%, ** at 5% and * at 10%.

Table 3.3 Placebo Test using Three Other Major Endemic Diseases

	(1)	(2)	(3)
Dep. Var.	Enrolling in primary school		
Keshan×Post	-0.0031		
	(0.0021)		
Kaschin-Beck×Post		-0.0016	
		(0.0024)	
Schistosomiasis×Post			-0.0007
			(0.0013)
Dep. Mean	0.990	0.990	0.990
County FE	YES	YES	YES
Cohort FE	YES	YES	YES
Province×Cohort FE	YES	YES	YES
Controls	YES	YES	YES
Observations	346,674	346,674	346,674
Num. of Clusters	1,883	1,883	1,883

Notes: This table employs three other major endemic diseases in China to conduct a placebo test. *Highgoi* in our baseline difference-in-differences specification is replaced by three dummy variables indicating whether a county was affected by Keshan disease, Kaschin-Beck disease or Schistosomiasis in each column. County-level clustered standard errors are in parentheses. *** denotes significance at 1%, ** at 5% and * at 10%.

Table 3.4 Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. Var.	Enrolling in primary school						
High goiter×Post	0.0044***	0.0053***	0.0073***	0.0050***	0.0045**	0.0040**	0.0046***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Dep. Mean	0.990	0.989	0.988	0.989	0.990	0.991	0.991
County FE	YES	YES	YES	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES	YES	YES	YES
Province×Cohort FE	YES	YES	YES	YES	YES		YES
Controls	YES	YES	YES	YES	YES	YES	YES
County-specific Linear Time Trend					YES		
Prefecture×Cohort FE						YES	
Observations	391,162	343,070	157,259	223,945	346,674	346,662	264,168
Num. of Clusters	2,066	1,861	961	1,248	1,883	1,883	1,355

Notes: This table conducts a battery of robustness checks for our baseline difference-in-differences results. In column (1), we include sample in city districts to address the concern that our results are driven by sample selection. Column (2) excludes sample in high water iodine counties where USI was not implemented. Column (3) excludes sample in iodine adequate counties where the average water iodine content is larger than 5 μg/L. Column (4) excludes sample from counties with iodized salt before 1982. Column (5) further controls for county-specific linear time trend. Column (6) controls for prefecture-by-cohort fixed effects instead of province-by-cohort fixed effects. Column (7) excludes sample in the provinces children were allowed to attend primary school after seven years old. County-level clustered standard errors are in parentheses. *** denotes significance at 1%, ** at 5% and * at 10%.

Table 3.5 Ruling out Alternative Hypotheses

	(1)	(2)	(3)
Dep. Var.	Healthy	Migration	Enrolling in primary school
High goiter×Post	0.0007	-0.0012	0.00571***
	(0.001)	(0.003)	(0.00145)
Healthy			0.349***
			(0.0119)
Migration			0.00159**
			(0.00069)
Dep. Mean	0.994	0.0560	0.990
County FE	YES	YES	YES
Cohort FE	YES	YES	YES
Province×Cohort FE	YES	YES	YES
Controls	YES	YES	YES
Observations	347,316	347,316	347,316
Num. of Clusters	1,883	1,883	1,883

Notes: This table reports additional results to rule out alternative hypotheses. *Healthy* is a dummy denotes whether the self-assessed health status for a child by the household head was "healthy". *Migration* is a dummy indicating whether an individual's living place was different from her registration place in the *hukou* system. County-level clustered standard errors are in parentheses. *** denotes significance at 1%, ** at 5% and * at 10%.

Table 3.6 IV Results

	(1)	(2)	(3)
	First-stage	Reduced-form	Second-stage
Dep. Var.	High goiter×Post	Enrolling in primary school	Enrolling in primary school
High goiter×Post			0.0093**
			(0.005)
Low Iodine×Post	0.2087***	0.0019**	
	(0.023)	(0.001)	
Dep. Mean	0.0504	0.990	0.990
Kleibergen-Paap F statistics	83.99		
County FE	YES	YES	YES
Cohort FE	YES	YES	YES
Province×Cohort FE	YES	YES	YES
Controls	YES	YES	YES
Observations	346,674	346,674	346,674
Num. of Clusters	1,883	1,883	1,883

Notes: Columns (1)-(3) report first-stage, reduced-form and second-stage results for the instrumental variable estimation respectively. County-level clustered standard errors are in parentheses. *** denotes significance at 1%, ** at 5% and * at 10%.

Table 3.7 Heterogeneous Effects

	(1)	(2)	(3)	(4)
Dep. Var.	Enrolling in primary school			
Sample	Urban citizens	Rural citizens	Male	Female
High goiter×Post	0.0011	0.0064***	0.0039**	0.0078***
	(0.002)	(0.002)	(0.002)	(0.002)
Dep. Mean	0.996	0.989	0.991	0.988
County FE	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES
Province×Cohort FE	YES	YES	YES	YES
Controls	YES	YES	YES	YES
Observations	39,054	306,559	177,008	169,666
Num. of Clusters	1,663	1,876	1,883	1,883

Notes: County-level clustered standard errors are in parentheses. *** denotes significance at 1%, ** at 5% and * at 10%.

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Appendix

A.1. Decentralization and environmental regulation under overlapping hierarchies: Evidence from China's SOE reform

A.1.a. The Effects of New EPL on Aggregate Pollution Outcomes

A main drawback of our baseline analysis is that though unlikely given the institutional context of decentralization, there might by unobserved selection over which SOE get decentralized, and which prefecture has more decentralized SOE. The effects of decentralization may also be confounded by the change in financial relationships between the prefectural level government and the SOE, as decentralization not only means the prefectural government its capable to enforce more stringent environmental regulations, but also means the prefectural government receives the tax payment and financial surplus from the SOE. To address this concern, we introduce a second set of analysis exploiting the enforcement of the new EPL in 2015. As described in section II, the new EPL removes the constraint that prefecture level governments have to ask for approval before punishing a polluting SOE under the oversight of provincial or central government. Therefore, the new EPL removes this regulation constraint without affecting the financial and personnel ties between the regulator and SOE. If the effects from the two separate shocks are comparable, one would have more confidence in the validity of our findings.

Unfortunately, the new EPL was issued in October 2014 and became effective on January 1st, 2015. Neither ASIF nor ESR is able to support the analysis of the new EPL effects because the year coverage of both samples are limited to 1998-2014, missing the post event periods. Therefore, we use prefecture level panel data described in section 4.3 in this analysis, while using the ESR and ASIF to construct treatment group variables by exploiting the pre-treatment pollution intensity from SOEs oversight by central or provincial government. The empirical model is specified in equation A.1.1.

$$P_{pt} = \alpha_p + \gamma_t + X_p \times \delta_t + \beta Treat_p \times Post_t + \varepsilon_{pt} \quad \text{(A. 1.1)}$$

where P_{pt} represents the total emissions of SO2 or sewage from industrial sources. $Post_t$ defines the post treatment period, equaling to 1 if the year is 2015 or later. $Treat_p$ is a continuous proxy on control-treatment groups, constructed as the share of SO2 or sewage emission from central or provincial SOEs in 2014 by summarizing the ESR database. As in equations 1.5 and 1.6, we control for prefecture fixed effects, year fixed effects and year fixed effects interacted with prefecture characteristics.[1] As the outcome variable is prefectural level aggregate pollution amounts, we compare the estimates from this model to that from equation 1.5 described in 1.5.3.

In Table A1, we report a parallel analysis of the implementation of new EPL. As explained in 5.4, the ESR and ASIF samples are unable to analyze this event because of limitations in year coverage. Instead, we use a prefecture-year level sample from prefectural annual yearbooks which covers prefectural aggregate pollution amounts on SO2 and sewage from 2003 to 2019. In the difference-in-differences analysis, we define the post treatment period as years 2015-2019, and construct control-treatment proxies by using the ESR database before the new EPL.

Columns 1-2 in Table A1 report DD estimates when treatment proxies are constructed as the pollution share of central and provincial SOEs in 2014, the last year of ESR coverage and the last period before new EPL. The shares are standardized so that the magnitudes are meaningful. If the SO2 pollution share from central and provincial SOEs are 1-sd higher, the prefecture's total SO2 emission decreases by 5.24% after the implementation of the new EPL; if the sewage pollution share from central and provincial SOEs are 1-sd higher, the prefecture's total sewage emission decreases by 5.71% after the implementation of the new EPL. Columns 3-4 construct discrete treatment proxies by splitting the shares in columns 1-2 into higher half and lower half. When the SO2 pollution share from central and provincial SOEs are in the higher half, the prefectures total SO2 emission decrease by an additional 16.59% than the lower half. The magnitude on sewage is small and statistically insignificant.

The main takeaway from the new EPL analysis is that it provides a similar but independent shock to the environmental regulation abilities of prefectural level regulators on SOEs oversight by higher levels of government. In the decentralization case, there's selection on the decentralized SOEs, combined with a change in financial relationships between the prefectural government and the SOE; in the new EPL case, there's no selection on which firms are affected, and there's no change in financial relationships, creating a cleaner natural experiment. As the effects and magnitudes in Tables 7 and 8 are comparable, it cross-validates our findings in the decentralization context, where we have richer data to investigate more detailed structure and mechanisms.

[1] In equation 1.6, prefecture characteristics take values in year 2003, as the prefectural level pollution records in yearbooks start from year 2003.

Table A.1.1 The Aggregate Effects of New Environmental Protection Law

	(1)	(2)	(3)	(4)
Sample	Prefecture Annual Statistical Yearbooks, 2003-2019			
Dependent Variable	log(total SO2)	log(total sewage)	log(total SO2)	log(total sewage)
Treat Construction	SO2 emission share from central and provincial SOEs	Sewage emission share from central and provincial SOEs	Higher half of SO2 emission share from central and provincial SOEs	Higher half of sewage emission share from central and provincial SOEs
New EPL × Treat	-0.0524*	-0.0571*	-0.1659**	-0.0156
	(0.031)	(0.032)	(0.078)	(0.059)
Firm FE	YES	YES	YES	YES
Prefecture Characteristics × Year FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Observations	4,205	4,169	4,205	4,169

Notes: This table reports the aggregate effects of the new EPL on prefecture-year level pollution and productivity outcomes. New EPL equals 1 if year equals 2015 or above. Prefecture controls include logarithm of population, non-agriculture population, GDP and total industrial output in 2003, plus several indicators of industrial structural – the share of first and second industries in GDP, and the share of industrial output from pollutive industries in 2003. Robust standard errors are clustered at prefecture level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

A.1.b. Details on Sample and Data

Table A.1.2 Summary Statistics

Variables	Obs.	Mean	Std. Dev.	Min	Max
Panel A. ASIF variables					
Value-added (log)	2,529,237	8.807	1.332	4.125	13.29
# Employees (log)	2,529,237	4.919	1.051	2.303	8.303
Capital stock (log)	2,529,237	7.989	1.712	2.328	13.15
TFP (log)	2,529,237	4.674	1.086	-2.494	10.32
Liquidity	2,521,705	0.0546	0.324	-104.0	9.046
SOE	2,529,237	0.0799	0.271	0	1
Post decentralization	2,529,237	0.000458	0.0214	0	1
Polluting industries	2,529,237	0.356	0.479	0	1
Panel B. ESR variables					
Waste gas emission (log)	409,037	6.116	3.692	0	17.18
SO2 emission (log)	531,664	6.958	4.702	0	21.50
Sewage emission (log)	531,664	8.482	4.524	0	18.27
Panel C. Prefecture-by-year-level variables					
N(Dece, ESR)	3,553	0.491	1.224	0	14
N(Dece, ASIF, all)	3,396	1.749	4.826	0	67
N(Dece, ASIF, pol)	3,396	0.514	1.348	0	15
Total industrial output (log)	3,396	17.64	1.503	12.91	21.92
Aggregate TFP (log)	3,367	6.453	1.178	1.564	16.00
Total waste gas emission (log)	2,701	14.50	1.468	6.280	18.81
Total SO2 emission (log)	3,549	16.10	1.208	7.720	21.50
Total sewage emission (log)	3,553	16.97	1.285	8.882	20.63
Panel D. Prefecture-level variables					
Population, 1998 (log)	213	5.768	0.757	2.660	8.039
Non-agricultural population, 1998 (log)	213	4.500	0.677	2.438	6.860
GDP, 1998 (log)	213	14.62	0.905	11.99	17.42
GDP growth rate, 1998	213	9.134	12.39	-48.80	146.3
Total industrial output, 1998 (log)	213	16.42	1.156	12.91	20.03
First industry GDP share, 1998	213	21.16	11.22	1	51.20
Second industry GDP share, 1998	213	44.70	10.91	18.80	83.80
Share of polluting industry in industrial output, 1998	213	0.487	0.205	0.0290	0.982

Table A.1.3 Polluting Industries in the MEP Classification

Polluting Industries	Non-Polluting Industries	
Mining and Washing of Coal [6]	Forestry [2]	Non-Ferrous Metal Processing [33]
Mining and Processing of Ferrous Metal Ores [8]	Extraction of Petroleum and Natural Gas [7]	Fabricated Metal Products Manufacturing [34]
Mining and Processing of Non-metallic Mineral [10]	Mining and Processing of Non-ferrous Metal Ores [9]	General Purpose Machinery Manufacturing [35]
Fermentation [14(6)]	Agricultural and Sideline Food Processing [13]	Special purpose Machinery Manufacturing [36]
Beverage Manufacturing [15]	Food Manufacturing [14]	Transport Equipment Manufacturing [37]
Textiles Mills [17]	Tobacco Manufacturing [16]	Electrical Equipment Manufacturing [39]
Leather, Fur and Related Products Manufacturing [19]	Wearing Apparel and Clothing Accessories Manufacturing [18]	Computers and Electronic Products Manufacturing [40]
Pulp and Paper Manufacturing [22(1,2)]	Wood and Bamboo Products Manufacturing [20]	General Instruments and Other Equipment Manufacturing [41]
Petrochemicals Manufacturing [25]	Furniture Manufacturing [21]	Craftworks Manufacturing [42]
Chemical Products Manufacturing [26]	Paper Products Manufacturing [22]	Renewable Materials Recovery [43]
Medicine Manufacturing [27(1,2,4)]	Printing and Reproduction of Recorded Media [23]	Electricity and Heat Supply [44]
Chemical Fibers Manufacturing [28]	Education and Entertainment Articles Manufacturing [24]	Gas Production and Supply [45]
Non-Metallic Mineral Products Manufacturing [31]	Medical Goods Manufacturing [27]	Water Production and Supply [46]
Iron and Steel Smelting [32(1,2)]	Rubber Products Manufacturing [29]	
Non-Ferrous Metal Smelting [33(1)]	Plastic Products Manufacturing [30]	
Fossil-Fuel Power Station [44(1)]	Basic Metal Processing [32]	

Notes: 2-digit (3-digit if there exists a third digit in parenthesis) industry codes are reported in brackets. The division of polluting versus non-polluting industries are sourced from MEP and reported in He et al. (2020).

A.1.c. Extensions of the Baseline Results

Table A.1.4 Effects on Exits and Switching Industries

	(1)	(2)
Dependent Variable	Exit in the next year	Switch to a different 4-digit industry in the next year
	Direct Effects, ASIF	
Decentralized to Prefecture, ASIF × Polluting Industry	-0.0109	0.0110
	(0.029)	(0.020)
Observations	3,265,686	3,265,686
Firm FE	YES	YES
Prefecture-Year FE	YES	YES
Industry-Year FE	YES	YES
	Spillover effects, ASIF, private firms	
N(Dece, ASIF, pol)	0.0072**	0.0002
	(0.001)	(0.001)
Observations	2,865,094	2,865,094
Firm FE	YES	YES
Prefecture Characteristics × Year FE	YES	YES
Industry-Year FE	YES	YES

Notes: This table reports the direct and spillover effects of decentralization on firms' exits and switching across industries. We only report the parameters of interest in this table, which are the interaction term of decentralization with polluting industry dummy in the direct effects specification, and the cumulative number of events that an SOE in polluting industry being decentralized in the spillover effects specification. Robust standard errors are clustered at firm level for direct effects and prefecture level for spillover effects and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.1.5 Effects on Abatement Efforts

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Operating Hours/yr	log(water input)	log(coal input)	log(petroleum input)	Abatement-to-emission ratio, SO2	Abatement-to-emission ratio, Sewage
			Direct Effects, ESR			
Decentralized to Prefecture, ESR	-186.2722	-0.4554***	-0.0695	-0.1548	4.3061	8,878.55
	(578.605)	(0.128)	(0.114)	(0.114)	(11.531)	(18,054.35)
Observations	277,583	510,889	266,491	248,909	212,377	232,869
Firm FE	YES	YES	YES	YES	YES	YES
Prefecture-Year FE	YES	YES	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES	YES	YES
			Spillover effects, ESR, private firms			
N(Dece, ESR)	6.7892	0.0120	0.0264	0.0592***	-13.2574	-5,157.75
	(50.778)	(0.011)	(0.021)	(0.017)	(9.146)	(3,793.92)
Observations	218,458	363,213	189,118	157,181	133,668	168,763
Firm FE	YES	YES	YES	YES	YES	YES
Prefecture Characteristics × Year FE	YES	YES	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES	YES	YES

Notes: This table reports the direct and spillover effects of decentralization on firms' abatement efforts. The abatement-to-emission ratio is defined as the ratio of the amount of pollutant absorbed by abatement facilities to the amount of pollutant emissions. Robust standard errors are clustered at firm level for direct effects and prefecture level for spillover effects and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.1.6 The Political Mechanism of Decentralization, Pollution Outcomes

	(1)	(2)	(3)
Dependent Variable	log(waste gas)	log(SO2)	log(sewage)
	Direct Effects, ESR		
Decentralized to Prefecture	-0.4113**	-0.5255*	-0.4114*
Leader age $\leq$ *56*	(0.169)	(0.272)	(0.221)
Observations	416,933	416,933	416,933
Decentralized to Prefecture	0.0539	-1.2495	-0.0843
Leader age $>$ *56*	(0.399)	(1.186)	(0.962)
Observations	46,677	46,677	46,677
Firm FE	YES	YES	YES
Prefecture-Year FE	YES	YES	YES
Industry-Year FE	YES	YES	YES
	Spillover Effects, ESR, private firms		
N(Dece, ESR)	0.0368**	0.0616***	0.0862***
Leader age $\leq$ *56*	(0.016)	(0.021)	(0.023)
Observations	293,313	293,313	293,313
N(Dece, ESR)	-0.0639	0.1534**	0.2150***
Leader age $>$ *56*	(0.045)	(0.061)	(0.066)
Observations	35,150	35,150	35,150
Firm FE	YES	YES	YES
Prefecture Characteristics × Year FE	YES	YES	YES
Industry-Year FE	YES	YES	YES

Notes: This table reports the direct and spillover effects of decentralization on firms' pollution outcomes in two subsamples. The strong promotion incentives subsample is defined that the incumbent party secretary is 56 years old or younger. The weak promotion incentives subsample is defined that the incumbent party secretary is over 56 years old. Robust standard errors are clustered at firm level for direct effects and prefecture level for spillover effects and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.1.7 The Political Mechanism of Decentralization, Production Outcomes

	(1)	(2)	(3)	(4)
Dependent Variable	log(value-added)	log(capital stock)	log(employment)	log(TFP, OP)
	Direct Effects, ASIF			
Decentralized to Prefecture, ASIF × Polluting Industry	-0.0019	0.4850**	0.1204*	-0.3045**
Leader age ≤ 56	(0.125)	(0.209)	(0.065)	(0.124)
Observations	2,243,249	1,951,621	2,243,249	1,951,621
Decentralized to Prefecture, ASIF × Polluting Industry	0.1829	0.3992	0.0261	0.0212
Leader age > 56	(0.220)	(0.487)	(0.149)	(0.285)
Observations	287,059	236,529	287,059	236,529
Firm FE	YES	YES	YES	YES
Prefecture-Year FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
	Spillover effects, ASIF, private firms			
N(Dece, ASIF, pol)	0.0356***	0.0081	0.0016	0.0335***
Leader age ≤ 56	(0.005)	(0.007)	(0.003)	(0.005)
Observations	1,778,247	1,550,876	1,778,247	1,550,876
N(Dece, ASIF, pol)	-0.0164	-0.0295	-0.0174*	0.0039
Leader age > 56	(0.016)	(0.022)	(0.010)	(0.016)
Observations	245,452	201,799	245,452	201,799
Firm FE	YES	YES	YES	YES
Prefecture Characteristics × Year FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES

Notes: This table reports the direct and spillover effects of decentralization on firms' production outcomes in two subsamples. We only report the parameters of interest in this table. The strong promotion incentives subsample is defined that the incumbent party secretary is 56 years old or younger. The weak promotion incentives subsample is defined that the incumbent party secretary is over 56 years old. Robust standard errors are clustered at firm level for direct effects and prefecture level for spillover effects and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

A.1.d.Robustness Checks

Table A.1.8 Robustness Check 1: Drop Provincial and Vice-Provincial Level Cities, Pollution Outcomes

	(1)	(2)	(3)
Dependent Variable	log(waste gas)	log(SO2)	log(sewage)
Panel A. Direct effects			
Decentralized to Prefecture, Polluting Firms in ESR	-0.3922**	-0.4235	-0.3541*
	(0.168)	(0.258)	(0.211)
Observations	417,948	417,948	417,948
Firm FE	YES	YES	YES
Prefecture-Year FE	YES	YES	YES
Industry-Year FE	YES	YES	YES
Panel B. Spillover effects			
N(Dece, ESR)	0.0187	0.0904***	0.1348***
	(0.018)	(0.024)	(0.028)
Observations	285,065	285,065	285,065
Firm FE	YES	YES	YES
Prefecture Characteristics × Year FE	YES	YES	YES
Industry-Year FE	YES	YES	YES

Notes: This table reports the robustness check for the direct and spillover effects of decentralization on firms' pollution outcomes by dropping the 4 provincial level cities and 15 vice-provincial level cities from the sample. Robust standard errors are clustered at firm level for direct effects and prefecture level for spillover effects and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.1.9 Robustness Check 1: Drop Provincial Level Cities, Production Outcomes

	(1)	(2)	(3)	(4)
Dependent Variable	log(value-added)	log(capital stock)	log(employment)	log(TFP, OP)
Panel A. Direct effects				
Decentralized to Prefecture, ASIF	-0.2852***	-0.5969***	-0.3695***	0.0819
	(0.072)	(0.110)	(0.039)	(0.087)
Decentralized to Prefecture, ASIF × Polluting Industry	-0.0426	0.5807***	0.1691**	-0.3689***
	(0.123)	(0.213)	(0.067)	(0.126)
Observations	2,142,190	1,878,868	2,142,190	1,878,868
Firm FE	YES	YES	YES	YES
Prefecture-Year FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
Panel B. Spillover effects				
N(Dece, ASIF, all)	-0.0200***	0.0188***	-0.0060***	-0.0187***
	(0.003)	(0.004)	(0.002)	(0.002)
N(Dece, ASIF, pol)	0.0397***	-0.0086	-0.0023	0.0345***
	(0.006)	(0.009)	(0.004)	(0.006)
Observations	1,645,880	1,447,243	1,645,880	1,447,243
Firm FE	YES	YES	YES	YES
Prefecture Characteristics × Year FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES

Notes: This table reports the robustness check for the direct and spillover effects of decentralization on firms' production outcomes by dropping the 4 provincial level cities and 15 vice-provincial level cities from the sample. Robust standard errors are clustered at firm level for direct effects and prefecture level for spillover effects and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.1.10 Robustness Check 2: Two-way Clustered Standard Errors, Pollution Outcomes

	(1)	(2)	(3)
Dependent Variable	log(waste gas)	log(SO2)	log(sewage)
Panel A. Direct effects			
Decentralized to Prefecture	-0.4745***	-0.6300**	-0.4516**
	(0.156)	(0.263)	(0.214)
Observations	531,665	531,665	531,665
Firm FE	YES	YES	YES
Prefecture-Year FE	YES	YES	YES
Industry-Year FE	YES	YES	YES
Panel B. Spillover effects			
N(Dece, ESR)	0.0434***	0.0474***	0.0551***
	(0.012)	(0.015)	(0.014)
Observations	379,231	379,231	379,231
Firm FE	YES	YES	YES
Prefecture Characteristics × Year FE	YES	YES	YES
Industry-Year FE	YES	YES	YES

Notes: This table reports the robustness check for the direct and spillover effects of decentralization on firms' pollution outcomes by reporting robust standard errors two-way clustered at firm (or prefecture, for spillover effects) and industry-by-year level, which are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.1.11 Robustness Check 2: Two-way Clustered Standard Errors, Production Outcomes

	(1)	(2)	(3)	(4)
Dependent Variable	log(value-added)	log(capital stock)	log(employment)	log(TFP, OP)
Panel A. Direct effects				
Decentralized to Prefecture, ASIF	-0.3199***	-0.4438***	-0.3645***	-0.0238
	(0.066)	(0.092)	(0.042)	(0.074)
Decentralized to Prefecture, ASIF × Polluting Industry	0.0451	0.3965**	0.1444**	-0.2151**
	(0.104)	(0.182)	(0.064)	(0.107)
Observations	2,945,013	2,529,237	2,945,013	2,529,237
Firm FE	YES	YES	YES	YES
Prefecture-Year FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
Panel B. Spillover effects				
N(Dece, ASIF, all)	-0.0113***	-0.0099***	-0.0020***	-0.0081***
	(0.001)	(0.001)	(0.001)	(0.001)
N(Dece, ASIF, pol)	0.0389***	0.0255***	0.0147***	0.0250***
	(0.005)	(0.005)	(0.002)	(0.004)
Observations	2,377,652	2,044,561	2,377,652	2,044,561
Firm FE	YES	YES	YES	YES
Prefecture Characteristics × Year FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES

Notes: This table reports the robustness check for the direct and spillover effects of decentralization on firms' production outcomes by reporting robust standard errors two-way clustered at firm (or prefecture, for spillover effects) and industry-by-year level, which are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.1.12 Robustness Check 3: Drop Mining Industries, Pollution Outcomes

	(1)	(2)	(3)
Dependent Variable	log(waste gas)	log(SO2)	log(sewage)
Panel A. Direct effects			
Decentralized to Prefecture	-0.4633***	-0.6671**	-0.4213**
	(0.163)	(0.280)	(0.206)
Observations	500,879	500,879	500,879
Firm FE	YES	YES	YES
Prefecture-Year FE	YES	YES	YES
Industry-Year FE	YES	YES	YES
Panel B. Spillover effects			
N(Dece, ESR)	0.0417***	0.0473***	0.0531***
	(0.010)	(0.013)	(0.012)
Observations	364,427	364,427	364,427
Firm FE	YES	YES	YES
Prefecture Characteristics × Year FE	YES	YES	YES
Industry-Year FE	YES	YES	YES

Notes: This table reports the robustness check for the direct and spillover effects of decentralization on firms' pollution outcomes by dropping mining industries (2-digit industry codes from 6 to 10) from the sample. Robust standard errors are clustered at firm level for direct effects and prefecture level for spillover effects and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.1.13 Robustness Check 3: Drop Mining Industries, Production Outcomes

	(1)	(2)	(3)	(4)
Dependent Variable	log(value-added)	log(capital stock)	log(employment)	log(TFP, OP)
Panel A. Direct effects				
Decentralized to Prefecture, ASIF	-0.3035***	-0.4261***	-0.3637***	-0.0042
	(0.063)	(0.094)	(0.040)	(0.074)
Decentralized to Prefecture, ASIF × Polluting Industry	0.0462	0.4345**	0.1404**	-0.2058*
	(0.103)	(0.190)	(0.063)	(0.109)
Observations	2,826,247	2,422,237	2,826,247	2,422,237
Firm FE	YES	YES	YES	YES
Prefecture-Year FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
Panel B. Spillover effects				
N(Dece, ASIF, all)	-0.0113***	-0.0099***	-0.0023***	-0.0081***
	(0.001)	(0.001)	(0.001)	(0.001)
N(Dece, ASIF, pol)	0.0388***	0.0259***	0.0153***	0.0248***
	(0.003)	(0.004)	(0.002)	(0.003)
Observations	2,303,085	2,303,085	2,303,085	2,303,085
Firm FE	YES	YES	YES	YES
Prefecture Characteristics × Year FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES

Notes: This table reports the robustness check for the direct and spillover effects of decentralization on firms' production outcomes by dropping mining industries (2-digit industry codes from 6 to 10) from the sample. Robust standard errors are clustered at firm level for direct effects and prefecture level for spillover effects and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.1.14 Robustness Check 4: Drop Observations Post 2009, Pollution Outcomes

	(1)	(2)	(3)
Dependent Variable	log(waste gas)	log(SO2)	log(sewage)
Panel A. Direct effects			
Decentralized to Prefecture	-0.2775*	-0.3079	-0.4856**
	(0.164)	(0.202)	(0.225)
Observations	360,141	360,141	360,141
Firm FE	YES	YES	YES
Prefecture-Year FE	YES	YES	YES
Industry-Year FE	YES	YES	YES
Panel B. Spillover effects			
N(Dece, ESR)	0.0659***	0.0550***	0.0385***
	(0.010)	(0.012)	(0.012)
Observations	243,722	243,722	243,722
Firm FE	YES	YES	YES
Prefecture Characteristics × Year FE	YES	YES	YES
Industry-Year FE	YES	YES	YES

Notes: This table reports the robustness check for the direct and spillover effects of decentralization on firms' pollution outcomes by dropping observations post 2009 from the sample. Robust standard errors are clustered at firm level for direct effects and prefecture level for spillover effects and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.1.15 Robustness Check 4: Drop Observations Post 2009, Production Outcomes

	(1)	(2)	(3)	(4)
Dependent Variable	log(value-added)	log(capital stock)	log(employment)	log(TFP, OP)
Panel A. Direct effects				
Decentralized to Prefecture, ASIF	-0.3077***	-0.4655***	-0.3244***	-0.0267
	(0.051)	(0.096)	(0.040)	(0.075)
Decentralized to Prefecture, ASIF × Polluting Industry	0.0857	0.4076**	0.1320**	-0.2322**
	(0.080)	(0.195)	(0.064)	(0.110)
Observations	2,303,540	2,027,816	2,303,540	2,027,816
Firm FE	YES	YES	YES	YES
Prefecture-Year FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
Panel B. Spillover effects				
N(Dece, ASIF, all)	-0.0098***	-0.0037***	-0.0024***	-0.0088***
	(0.001)	(0.001)	(0.000)	(0.001)
N(Dece, ASIF, pol)	0.0310***	-0.0006	0.0132***	0.0259***
	(0.003)	(0.004)	(0.002)	(0.003)
Observations	1,830,681	1,617,929	1,830,681	1,617,929
Firm FE	YES	YES	YES	YES
Prefecture Characteristics × Year FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES

Notes: This table reports the robustness check for the direct and spillover effects of decentralization on firms' production outcomes by dropping observations post 2009 from the sample. Robust standard errors are clustered at firm level for direct effects and prefecture level for spillover effects and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.1.16 Robustness Check 5: Drop Exiting Firms, Pollution Outcomes

	(1)	(2)	(3)
Dependent Variable	log(waste gas)	log(SO2)	log(sewage)
Panel A. Direct effects			
Decentralized to Prefecture	-0.6345***	-0.5371	-0.7340**
	(0.236)	(0.565)	(0.378)
Observations	274,101	274,101	274,101
Firm FE	YES	YES	YES
Prefecture-Year FE	YES	YES	YES
Industry-Year FE	YES	YES	YES
Panel B. Spillover effects			
N(Dece, ESR)	0.0398***	0.0355***	0.0453***
	(0.014)	(0.019)	(0.017)
Observations	207,960	207,960	207,960
Firm FE	YES	YES	YES
Prefecture Characteristics × Year FE	YES	YES	YES
Industry-Year FE	YES	YES	YES

Notes: This table reports the robustness check for the direct and spillover effects of decentralization on firms' pollution outcomes by dropping exiting firms from the sample. Robust standard errors are clustered at firm level for direct effects and prefecture level for spillover effects and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.1.17 Robustness Check 5: Drop Exiting Firms, Production Outcomes

	(1)	(2)	(3)	(4)
Dependent Variable	log(value-added)	log(capital stock)	log(employment)	log(TFP, OP)
Panel A. Direct effects				
Decentralized to Prefecture, ASIF	-0.4478***	-0.5498***	-0.5315***	-0.0672
	(0.117)	(0.174)	(0.091)	(0.120)
Decentralized to Prefecture, ASIF × Polluting Industry	0.1318	0.6325**	0.1739	-0.0868
	(0.187)	(0.289)	(0.114)	(0.160)
Observations	1,226,934	1,035,041	1,226,934	1,035,041
Firm FE	YES	YES	YES	YES
Prefecture-Year FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
Panel B. Spillover effects				
N(Dece, ASIF, all)	-0.0094***	-0.0086***	-0.0007	-0.0059***
	(0.001)	(0.002)	(0.001)	(0.001)
N(Dece, ASIF, pol)	0.0376***	0.0254***	0.0120***	0.0230***
	(0.005)	(0.006)	(0.003)	(0.004)
Observations	1,030,701	871,045	1,030,701	871,045
Firm FE	YES	YES	YES	YES
Prefecture Characteristics × Year FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES

Notes: This table reports the robustness check for the direct and spillover effects of decentralization on firms' production outcomes by dropping exiting firms from the sample. Robust standard errors are clustered at firm level for direct effects and prefecture level for spillover effects and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.1.18 Robustness Check 6: Alternative TFP Estimates

	(1)	(2)	(3)
Dependent Variable	log TFP, Oley and Pakes (1996)	log TFP, Levinsohn and Petrin (2003)	log TFP, Yang (2015)
Decentralized to Prefecture, ASIF	-0.0238	-0.0347	0.0841
	(0.072)	(0.071)	(0.077)
Decentralized to Prefecture, ASIF × Polluting Industry	-0.2151**	-0.1962*	-0.2925**
	(0.106)	(0.105)	(0.122)
Observations	2,529,237	2,529,237	2,422,237
Firm FE	YES	YES	YES
Prefecture-Year FE	YES	YES	YES
Industry-Year FE	YES	YES	YES
N(Dece, ASIF, all)	-0.0081***	-0.0082***	-0.0067***
	(0.001)	(0.001)	(0.001)
N(Dece, ASIF, pol)	0.0250***	0.0252***	0.0206***
	(0.003)	(0.003)	(0.003)
Observations	2,044,561	2,044,561	1,976,595
Firm FE	YES	YES	YES
Prefecture Characteristics × Year FE	YES	YES	YES
Industry-Year FE	YES	YES	YES

Notes: This table reports the robustness check for the direct and spillover effects of decentralization on firms' TFP by using alternative TFP estimates. Column 1 reports the baseline TFP estimates following Olley and Pakes (1996). Column 2 reports TFP estimates following Levinsohn and Petrin (2003). Column 3 reports TFP estimates from Yang (2015), which is the first empirical estimate on industry specific production functions using the ASIF database of China. Robust standard errors are clustered at firm level for direct effects and prefecture level for spillover effects and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

A.1.e. Alternative measurements of financial constraints

Table A.1.19 The Heterogeneous Spillover Effects by Financial Constraints – External Finance Dependence

	(1)	(2)	(3)	(5)
Sample	Private firms in ASIF			
Dependent Variable	log(value-added)	log(capital stock)	log(employment)	log(TFP, OP)
N(Dece, ASIF, all)	-0.0107***	-0.0112***	-0.0017***	-0.0074***
	(0.001)	(0.001)	(0.001)	(0.001)
× Ext. Fin. Dep.	-0.0015	0.0034*	-0.0015*	-0.0018
	(0.001)	(0.002)	(0.001)	(0.001)
N(Dece, ASIF, pol)	0.0343***	0.0305***	0.0115***	0.0208***
	(0.003)	(0.005)	(0.002)	(0.003)
× Ext. Fin. Dep.	0.0115**	-0.0122	0.0098***	0.0103*
	(0.005)	(0.008)	(0.003)	(0.005)
Firm FE	YES	YES	YES	YES
Prefecture Characteristics × Year FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
Observations	2,303,085	1,976,595	2,303,085	1,976,595

Notes: This table reports the heterogeneous spillover effects of decentralization on firms' pollution outcomes by external finance dependence. N(Dece, ASIF, all) is constructed as the cumulative number of events that all types of SOEs in the ASIF database being decentralized to prefecture p by year t; N(Dece, ASIF, pol) is constructed as the cumulative number of events that an SOE in polluting industry being decentralized to prefecture p by year t. Ext. Fin. Dep. is the average external finance dependence at 2-digit industry level constructed following Kroszner et al., (2007) and Manova and Yu (2016). Prefecture controls include logarithm of population, non-agriculture population, GDP and total industrial output in 1998, plus several indicators of industrial structural – the share of first and second industries in GDP, and the share of industrial output from pollutive industries in 1998. Robust standard errors are clustered at prefecture level and reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.1.20 The Heterogeneous Spillover Effects by Financial Constraints – Leverage

	(1)	(2)	(3)	(5)
Sample	Private firms in ASIF			
Dependent Variable	log(value-added)	log(capital stock)	log(employment)	log(TFP, OP)
N(Dece, ASIF, all)	-0.0090***	-0.0090***	-0.0018***	-0.0054***
	(0.001)	(0.001)	(0.001)	(0.001)
× $Leverage_{t-1}$	-0.0009***	0.0031***	-0.0002	-0.0014***
	(0.000)	(0.000)	(0.000)	(0.000)
N(Dece, ASIF, pol)	0.0319***	0.0227***	0.0144***	0.0167***
	(0.003)	(0.005)	(0.002)	(0.003)
×$Leverage_{t-1}$	0.0043***	-0.0113***	0.0001	0.0059***
	(0.001)	(0.002)	(0.001)	(0.001)
$Leverage_{t-1}$	-0.0133***	0.0717***	-0.0030***	-0.0260***
	(0.001)	(0.002)	(0.001)	(0.001)
Firm FE	YES	YES	YES	YES
Prefecture Characteristics × Year FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
Observations	1,847,533	1,567,999	1,847,533	1,567,999

Notes: This table reports the heterogeneous spillover effects of decentralization on firms' pollution outcomes by lagged leverage standardized within prefecture-year grids so that their means equal zero and their standard deviations equal one. N(Dece, ASIF, all) is constructed as the cumulative number of events that all types of SOEs in the ASIF database being decentralized to prefecture *p* by year *t*; N(Dece, ASIF, pol) is constructed as the cumulative number of events that an SOE in polluting industry being decentralized to prefecture *p* by year *t*. Prefecture controls include logarithm of population, non-agriculture population, GDP and total industrial output in 1998, plus several indicators of industrial structural – the share of first and second industries in GDP, and the share of industrial output from pollutive industries in 1998. Robust standard errors are clustered at prefecture level and reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

A.1.f. Anecdotal Evidence on Environmental Regulations over Central and Provincial SOEs

In this section, we list several examples of anecdotal evidence on the local governments' inability to regulate central and provincial SOEs stringently when they violate the EPL or emission standards:

1. A stated-owned coal mine in Henan province had been punished 33 times by the local government since 2015 but continued to heavily pollute the water till 2018.[83]
2. A branch of Sinopec (world's second largest enterprise and largest SOE) in Jiangsu Province was detected violation repeated in 2013, 2015 and 2017, severely warned by the local government several times, but not actually punished until 2019.[84]
3. The central government's environmental auditing system only audits local governments; began to audit SOEs only after 2019.[85]
4. Lardy (2014) documents qualitative discussions on the unbalanced competition between SOEs and private firms in China, where environmental regulations are among the important features that differ by firm type (others factors include financing, subsidy, land market, etc.).
5. Eaton and Kostka (2017) recorded 2,370 violations from big SOEs from 2004 to 2016.
6. Maurel and Pernet-Coudrier (2020) report that the SOE dominated cities in China are less effective in emission reduction under a national program to reduce SO2 emissions.

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[83] Source: https://news.sina.cn/2018-09-12/detail-ihiycyfx4827875.d.html

[84] Sources: https://www.nbd.com.cn/articles/2013-06-24/751905.html; https://www.chinanews.com.cn/ny/2015/01-09/6952470.shtml; http://news.huishoushang.com/84171.html; http://finance.sina.com.cn/money/future/fmnews/2020-01-19/doc-iihnzhha3399323.shtml

[85] Source: https://www.jiemian.com/article/2166930.html

A.2. Career incentives of local leaders and crisis response: A case study of COVID-19 lockdowns in China

A.2.a. Additional Empirical Results

Table A.2.1 Calculating Prefectural Mayors' Career Incentives Using the Method of Wang et al. (2020)

	(1)
Dependent Variable	Promotion
Inauguration Age	-0.0393***
	(0.007)
Deputy-Province-Level	5.0379**
	(2.469)
Inauguration Age×Deputy-Province-Level	-0.1066**
	(0.048)
Observations	2,035
Dep. Mean	0.499

Notes: This table reports the same specification as Table 2 using our sample of all prefectural mayors who were incumbent during 1994 to 2019. The promotion dummies for mayors are defined in the same way as for party secretaries except that we define the dummy as one if a prefecture's mayor becomes a party secretary afterwards. Robust standard errors are reported in parentheses, clustered at the prefecture level. *** p<0.01, ** p<0.05, * p<0.1.

Table A.2.2 Determinants of Promotion of Prefectural Party Secretaries Incumbent in SARS

	(1)	(2)
Method	LPM	Probit
Dependent Variable	Promotion	
Promotion Incentive	0.1948***	0.7276***
	(0.025)	(0.151)
Log Average GDP Growth over Term	2.0256***	6.8749***
	(0.576)	(1.993)
Ever Locked Down in SARS	0.0308	0.0886
	(0.064)	(0.192)
Cumulative SARS Cases per 10,000 People	-0.0386	-0.0990
	(0.153)	(0.754)
Number of Prefectures Within Province	-0.0172**	-0.0496**
	(0.007)	(0.022)
Log misused public funds, 1999-2003	0.0698***	0.2532***
	(0.016)	(0.067)
Log Road Travel Hours to Guangzhou	-0.1283***	-0.4024***
	(0.040)	(0.129)
Calendar Age	0.0084	0.1141
	(0.027)	(0.121)
College or Above Education	-0.0107	-0.0115
	(0.086)	(0.284)
Party School Degree	0.0321	0.0876
	(0.062)	(0.181)
Central Experience	0.3067**	1.0490**
	(0.125)	(0.423)
Provincial Experience	0.1148**	0.3729**
	(0.054)	(0.164)
Mayor’s Promotion Incentive	-0.0137	-0.0539
	(0.025)	(0.084)
Dep. Mean	0.394	0.394
Observations	322	322

Notes: This table explores the determinants of the promotion outcomes of prefectural party secretaries who were incumbent during the SARS outbreak. Promotion incentive, calendar age, and mayor’s promotion incentive are standardized so that their means equal zero and their standard deviations equal one. Column 1 applies a linear probability model (LPM) and Column 2 uses a probit model. Robust standard errors are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

A.2.b. Data Appendix

Table A.2.3 Prefecture Lockdown Records during COVID-19

Prefecture	Lockdown Date	Prefecture	Lockdown Date	Prefecture	Lockdown Date	Prefecture	Lockdown Date	Prefecture	Lockdown Date
Wuhan	2020/1/23	Binzhou	2020/2/3	Fushun	2020/2/5	Huaihua	2020/2/5	Lanzhou	2020/2/7
Ezhou	2020/1/23	Tongren	2020/2/3	Dandong	2020/2/5	Zhuhai	2020/2/5	Qinhuangdao	2020/2/8
Huanggang	2020/1/23	Qiannan	2020/2/3	Jinzhou	2020/2/5	Maoming	2020/2/5	Foshan	2020/2/8
Huangshi	2020/1/24	Songyuan	2020/2/4	Fuxin	2020/2/5	Zhaoqing	2020/2/5	Chongqing	2020/2/8
Shiyan	2020/1/24	Ha'erbin	2020/2/4	Liaoyang	2020/2/5	Nanning	2020/2/5	Ziyang	2020/2/8
Yichang	2020/1/24	Shuangyashan	2020/2/4	Panjin	2020/2/5	Guilin	2020/2/5	Dalian	2020/2/9
Jingmen	2020/1/24	Nanjing	2020/2/4	Tieling	2020/2/5	Wuzhou	2020/2/5	Wuxi	2020/2/9
Xiaogan	2020/1/24	Xuzhou	2020/2/4	Chaoyang	2020/2/5	Haikou	2020/2/5	Huainan	2020/2/9
Jingzhou	2020/1/24	Nantong	2020/2/4	Daqing	2020/2/5	Sanya	2020/2/5	Huaibei	2020/2/9
Xianning	2020/1/24	Zhenjiang	2020/2/4	Heihe	2020/2/5	Luzhou	2020/2/5	Huizhou	2020/2/9
Suizhou	2020/1/24	Hangzhou	2020/2/4	Daxinganling	2020/2/5	Nanchong	2020/2/5	Meizhou	2020/2/9
Enshi	2020/1/24	Ningbo	2020/2/4	Changzhou	2020/2/5	Meishan	2020/2/5	Dongguan	2020/2/9
Xiangyang	2020/1/28	Jiaxing	2020/2/4	Lianyungang	2020/2/5	Ganzi	2020/2/5	Deyang	2020/2/9
LvLiang	2020/1/29	Wuhu	2020/2/4	Yancheng	2020/2/5	Kunming	2020/2/5	Mianyang	2020/2/9
Sanmenxia	2020/1/31	Bengbu	2020/2/4	Yangzhou	2020/2/5	Lijiang	2020/2/5	Hanzhong	2020/2/9
Yinchuan	2020/1/31	Liuan	2020/2/4	Taizhou	2020/2/5	Tangshan	2020/2/6	Beijing	2020/2/10
Wuzhong	2020/1/31	Fuzhou	2020/2/4	Suqian	2020/2/5	Suzhou	2020/2/6	Shanghai	2020/2/10

Table A.2.4 Prefecture Lockdown Records during COVID-19 (Cont'd)

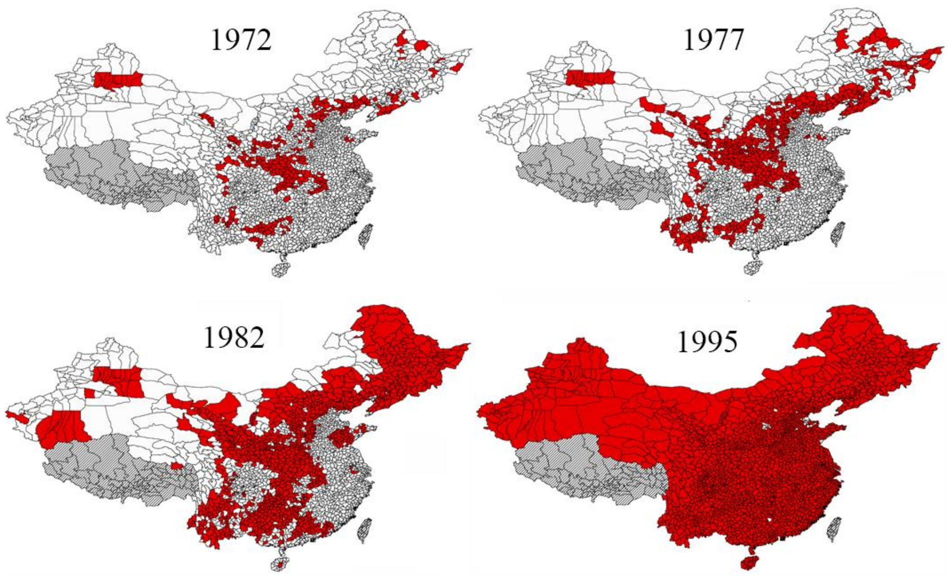
Prefecture	Lockdown Date	Prefecture	Lockdown Date	Prefecture	Lockdown Date	Prefecture	Lockdown Date	Prefecture	Lockdown Date
Lishui	2020/2/1	Jingdezhen	2020/2/4	Huzhou	2020/2/5	Jinhua	2020/2/6	Dongying	2020/2/10
Liupanshui	2020/2/1	Yingtan	2020/2/4	Quzhou	2020/2/5	Ma'anshan	2020/2/6	Huhehaote	2020/2/12
Xinzhou	2020/2/2	Linyi	2020/2/4	Hefei	2020/2/5	Fuzhou	2020/2/6	Baotou	2020/2/12
Wenzhou	2020/2/2	Dezhou	2020/2/4	Fuyang	2020/2/5	Liaocheng	2020/2/6	Wuhai	2020/2/12
Guiyang	2020/2/2	Zhengzhou	2020/2/4	Quanzhou	2020/2/5	Xinyang	2020/2/6	Chifeng	2020/2/12
Zunyi	2020/2/2	Nanyang	2020/2/4	Nanchang	2020/2/5	Fangchenggang	2020/2/6	Tongliao	2020/2/12
Anshun	2020/2/2	Zhumadian	2020/2/4	Jiujiang	2020/2/5	Yulin	2020/2/6	Ereduosi	2020/2/12
Qianxinan	2020/2/2	Liuzhou	2020/2/4	Ganzhou	2020/2/5	Ya'an	2020/2/6	Hulunbeier	2020/2/12
Bijie	2020/2/2	Zigong	2020/2/4	Yichun	2020/2/5	Tianjin	2020/2/7	Bayannaoer	2020/2/12
Jincheng	2020/2/3	Qiandongnan	2020/2/4	Jinan	2020/2/5	Guangzhou	2020/2/7	Wulanchabu	2020/2/12
Anshan	2020/2/3	Xishuangbanna	2020/2/4	Qingdao	2020/2/5	Shenzhen	2020/2/7	Xing'an	2020/2/12
Huaian	2020/2/3	Xi'an	2020/2/4	Taian	2020/2/5	Guigang	2020/2/7	Xilinguole	2020/2/12
Zhoushan	2020/2/3	Shijiazhuang	2020/2/5	Rizhao	2020/2/5	Hechi	2020/2/7	Alashan	2020/2/12
Taizhou	2020/2/3	Chengde	2020/2/5	Kaifeng	2020/2/5	Chengdu	2020/2/7	Zaozhuang	2020/2/12
Weifang	2020/2/3	Hengshui	2020/2/5	Pingdingshan	2020/2/5	Guangyuan	2020/2/7		
Jining	2020/2/3	Shenyang	2020/2/5	Zhoukou	2020/2/5	Suining	2020/2/7		

Table A.2.5 Prefecture Lockdown Records During SARS

Prefecture	Lockdown Start	Lockdown End	Prefecture	Lockdown Start	Lockdown End	Prefecture	Lockdown Start	Lockdown End	Prefecture	Lockdown Start	Lockdown End
Beijing	2003/4/24	2003/6/5	Xilinguole	2003/4/14	2003/6/1	Liaocheng	2003/4/25	2003/6/3	Huizhou	2003/4/26	2003/5/29
Tianjin	2003/4/24	2003/6/18	Alashan	2003/4/11	2003/5/16	Binzhou	2003/4/29	2003/6/3	Yangjiang	2003/4/30	2003/5/15
Shijiazhuang	2003/4/24	2003/6/1	Shenyang	2003/4/27	2003/5/14	Zhengzhou	2003/4/29	2003/6/1	Dongguan	2003/4/24	2003/5/31
Qinhuangdao	2003/4/26	2003/5/19	Dalian	2003/4/29	2003/5/25	Kaifeng	2003/4/28	2003/6/1	Nanning	2003/4/22	2003/6/1
Xingtai	2003/4/21	2003/6/4	Fushun	2003/4/26	2003/6/10	Luoyang	2003/5/13	2003/6/13	Guilin	2003/5/3	2003/5/18
Baoding	2003/4/20	2003/5/29	Benxi	2003/4/28	2003/5/14	Pingdingshan	2003/4/25	2003/6/1	Yulin	2003/4/25	2003/5/15
Zhangjiakou	2003/4/21	2003/6/1	Yingkou	2003/5/1	2003/5/25	Xinxiang	2003/4/24	2003/5/29	Chongqing	2003/4/24	2003/6/5
Cangzhou	2003/4/24	2003/6/10	Tieling	2003/4/20	2003/5/26	Xuchang	2003/5/1	2003/6/1	Chengdu	2003/4/26	2003/5/24
Langfang	2003/4/20	2003/6/8	Changchun	2003/5/2	2003/6/1	Sanmenxia	2003/5/1	2003/6/1	Luzhou	2003/4/28	2003/6/16
Hengshui	2003/4/24	2003/5/30	Harbin	2003/5/7	2003/6/1	Shangqiu	2003/5/1	2003/6/1	Guiyang	2003/4/28	2003/6/25
Taiyuan	2003/4/18	2003/6/10	Heihe	2003/5/4	2003/5/26	Zhoukou	2003/4/29	2003/6/1	Kunming	2003/5/7	2003/5/20
Datong	2003/4/23	2003/6/9	Nanjing	2003/4/23	2003/6/3	Wuhan	2003/4/28	2003/5/24	Zhaotong	2003/4/25	2003/5/20
Yangquan	2003/4/20	2003/5/23	Xuzhou	2003/4/28	2003/6/8	Shiyan	2003/5/5	2003/5/26	Lijiang	2003/4/13	2003/5/20
Changzhi	2003/4/18	2003/5/29	Huai'an	2003/4/24	2003/5/28	Yichang	2003/4/26	2003/5/26	Chuxiong	2003/4/22	2003/5/20
Jincheng	2003/4/24	2003/5/22	Zhenjiang	2003/4/25	2003/6/3	Xianning	2003/4/23	2003/5/26	Xi’an	2003/4/28	2003/6/1
Jinzhong	2003/4/20	2003/6/10	Hangzhou	2003/4/21	2003/5/27	Enshi	2003/4/20	2003/6/5	Xianyang	2003/5/8	2003/5/22
Yuncheng	2003/4/26	2003/5/22	Lishui	2003/4/29	2003/5/27	Changsha	2003/4/29	2003/6/20	Yulin	2003/4/28	2003/5/22
LvLiang	2003/5/12	2003/5/31	Anqing	2003/5/9	2003/5/19	Guangzhou	2003/4/26	2003/5/29	Ankang	2003/4/22	2003/5/22
Huhehaote	2003/4/15	2003/6/4	Fuzhou	2003/4/24	2003/5/10	Shaoguan	2003/4/28	2003/5/29	Shangluo	2003/4/26	2003/5/22
Baotou	2003/4/17	2003/5/30	Qingdao	2003/4/24	2003/6/3	Shenzhen	2003/4/25	2003/6/1	Lanzhou	2003/4/30	2003/6/6
Wuhai	2003/4/24	2003/6/16	Zibo	2003/4/29	2003/6/3	Zhuhai	2003/4/30	2003/5/13	Dingxi	2003/4/23	2003/6/1
Bayannao'er	2003/4/10	2003/6/16	Zaozhuang	2003/4/25	2003/6/3	Zhanjiang	2003/4/27	2003/5/15	Yinchuan	2003/4/17	2003/5/13
Wulanchabu	2003/4/14	2003/6/15	Weihai	2003/4/22	2003/6/3	Maoming	2003/5/1	2003/5/15			

A.3. Farewell to the God of Plague: Estimating the effects of China's Universal Salt Iodization on educational outcomes

Figure A.3.1 Salt Iodized Counties in Several Years

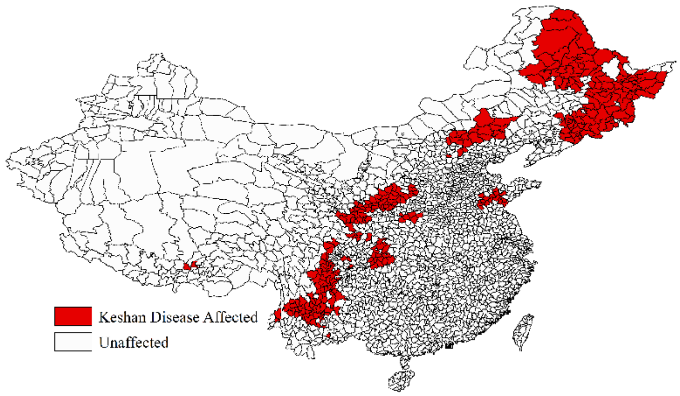


Notes: Salt-iodized counties are shaded in red for several specific years which we have information. Counties in Tibet and Taiwan are painted in shadow because they are excluded from our analyses. In 1995, high iodine counties not supplying iodized salt are left blank.

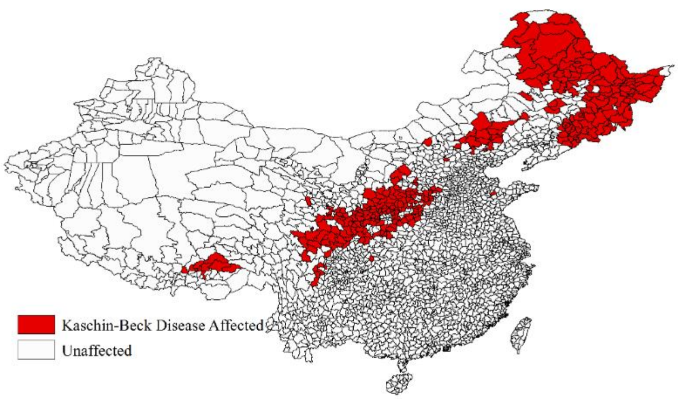
Data Source: *The Atlas of Endemic Diseases and their Environments in the People's Republic of China.*

Figure A.3.2 Falsification Tests from Three Major Endemic Diseases

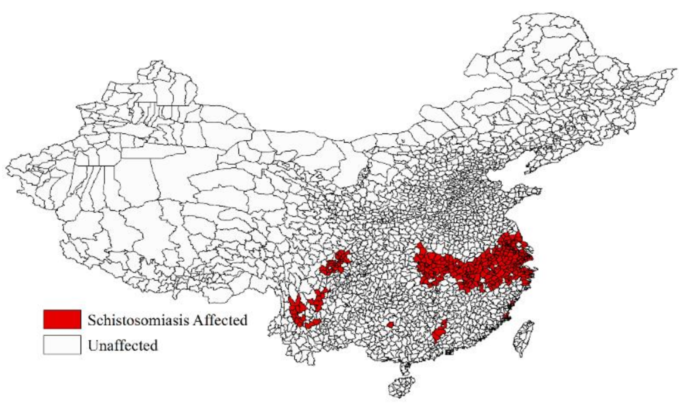
Panel A: Keshan Disease



Panel B: Kaschin-Beck Disease



Panel C: Schistosomiasis Infection



Notes: Panels A-B show the spatial distribution of Keshan Disease and Kaschin-Beck Disease affected counties in 1970-1982 and Panel C shows that of Schistosomiasis Affected counties in 1985.

Date Source: *The Atlas of Endemic Diseases and their Environments in the People's Republic of China* and *The Atlas of China's Schistosomiasis Infection.*

Table A.3.1 Descriptive Statistics of Primary School Enrollment by Birth Cohort

	(1)	(2)	(3)
Birth Cohort	Low Goiter	High Goiter	Diff.
1987	0.989	0.981	-0.008***
	[0.1031]	[0.1350]	(0.0016)
1988	0.991	0.982	-0.008***
	[0.0968]	[0.1311]	(0.0015)
1989	0.991	0.983	-0.009***
	[0.0925]	[0.1301]	(0.0013)
1990	0.991	0.988	-0.003**
	[0.0944]	[0.1077]	(0.0012)
1991	0.991	0.987	-0.004***
	[0.0929]	[0.1139]	(0.0013)
1992	0.991	0.988	-0.003**
	[0.0940]	[0.1096]	(0.0013)
1993	0.991	0.988	-0.003**
	[0.0937]	[0.1068]	(0.0013)
1994	0.992	0.987	-0.005***
	[0.0869]	[0.1116]	(0.0013)
1995	0.991	0.990	-0.001
	[0.0954]	[0.1008]	(0.0014)
1996	0.990	0.990	0.001
	[0.1009]	[0.0977]	(0.0015)
1997	0.988	0.989	0.001
	[0.1103]	[0.1059]	(0.0017)

Notes: This table report the summary statistics of primary school enrollments by birth cohort in our control group (low goiter counties) and treatment group (high goiter counties). Columns (1) and (2) show means as well as standard deviations in square brackets. Column (3) shows the differences between the high goiter counties and low goiter counties. The standard errors of the differences are reported in parentheses. *** denotes the differences between the two groups is significant at 1%, ** at 5% and * at 10%.

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