When the State Turns Down the Power: Administrative Power Rationing and Firm Production in China*

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Abstract

We study the real effects of China's 2021 administrative power rationing on manufacturing firms. Using an innovative plant-level measure of production based on satellite thermal infrared radiation (TIR) and the staggered adoption of city-level mandatory rationing, we implement a stacked difference-in-differences design and estimate that rationing reduces plant activity by 17.2%. The effects are robust to a wide range of tests and are stronger in plants operated by private firms. Firms offset the shock by reallocating production across facilities and front-loading output, yet exposed firms experience negative short-term stock price reactions and reduce long-run capital expenditures. Overall, our results highlight both the intended and unintended consequences of China's "dual control" energy policy framework, and show how these outcomes are shaped by the interaction between government intervention and market forces.

Keywords: Power rationing, thermal infrared radiation, production reallocation, spillover

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1 Introduction

China's hybrid economy has long been characterized by the coexistence of government intervention and market forces (Xu, 2011; Naughton, 2021; Xiong, 2018; Chang, Wang, and Xiong, 2025b). While state planning and macroeconomic management remain prominent in daily economic activities, market forces have expanded significantly in scope and influence—adapting to policy change, reshaping resource allocation, and at times feeding back into subsequent policy design. As market mechanisms deepen, market participants—firms, financial intermediaries, and investors— respond endogenously to policy shifts along multiple margins, so that realized outcomes reflect the interaction between administrative intervention and market-based responses (Song, Storesletten, and Zilibotti, 2011; He and Wei, 2023; Song and Xiong, 2018; Chang, Wang, and Xiong, 2025a; He, Nelson, Su, Zhang, and Zhang, 2025). This evolving dynamic complicates the traditional view that the central government exerts a decisive influence on economic activity through nationwide regulatory requirements and their local implementation. Instead, the realized effects of policy depend on the interaction between policy design and firm behavior, as market responses can reinforce, dampen, or even redirect the intended consequences of intervention. Quantifying these real consequences—and the endogenous responses through which they arise—is central to assessing the strengths and limits of China's hybrid economy.

The electricity sector provides a particularly sharp lens on this debate. While most sectors in China have become more market-oriented and competitive, the electricity market—accounting for roughly one-third of the global electricity demand and serving as a strategic input in the world's largest manufacturing economy—remains heavily shaped by administrative coordination, regulated pricing, and a state-owned grid for power delivery. When supply-demand imbalances arise, or when policy priorities shift in response to government objectives, policymakers often rely on quantity-based controls—such as consumption caps and rationing—rather than market-clearing price adjustments to implement micro-level intervention in firms' operations. Consequently, power rationing in China offers an ideal setting to assess how administrative quantity controls affect firm production activity and how endogenous firm responses shape both the intended and unintended consequences of government intervention in China's hybrid economy.

Against this backdrop, China's nationwide power rationing in 2021—closely linked to the "dual control" framework for total energy consumption and energy intensity—provides a particularly informative episode of administrative intervention in the allocation of a critical manufacturing input. Following heightened enforcement pressure on local governments to meet energy and carbon targets, many cities imposed abrupt and binding curtailment measures that disproportionately targeted industrial users and differed in timing. This staggered implementation, together with our ability to observe plant-level activity at high frequency, allows us to identify the direct production effects of mandatory rationing and to examine how firms respond through observable adjustment margins. We exploit these features of the 2021 episode in the empirical analyses that follow.

Using China's 2021 power-rationing campaign as a staggered policy shock, we quantify how an abrupt administrative constraint on electricity use translates into real activity at the plant level. Combining a novel production proxy based on satellite thermal infrared radiation (TIR) with a stacked difference-in-differences design around each city's mandatory-rationing rollout, we examine how rationing affects production and how firms' operational adjustments shape its real consequences. A key

advantage of our setting and measurement is that we observe operational adjustments at high frequency and fine spatial resolution, rather than inferring production from infrequent financial reports. We find that rationing leads to a sizable contraction in manufacturing activity at directly affected plants—about 17.2% on average over an eight-month window around the policy announcement. The estimated production impact is robust to a wide range of specifications and validation exercises. Despite the stated objective of disproportionately constraining energy-intensive and heavy-polluting production, our triple-difference estimates show no economically meaningful differential production impact between these industries and others. This pattern is consistent with a campaign-style, "one-size-fits-all" implementation and echoes Cao, Ho, Ma, and Teng (2021), who show that regulation in the electricity sector can redirect the incidence of environmental policy toward quantity adjustments. On the other hand, the effect is stronger for private firms, consistent with prior evidence that state-owned firms are more likely to have preferential access to critical resources in China (see, e.g., Allen, Qian, and Qian, 2005; Li, Meng, Wang, and Zhou, 2008; Lu, Zhu, and Zhang, 2012; Cong, Gao, Ponticelli, and Yang, 2019).

Facing government intervention, firms do not simply absorb the shock but proactively adjust production to mitigate disruption. Among multi-plant firms, production rises at unaffected facilities when another plant is rationed, consistent with within-firm reallocation that partially offsets the rationing-induced production decline—an increase of roughly 3.7% at unaffected plants across specifications. Beyond the firm boundary, we also document economically large spillovers: plants in untreated cities increase production by about 12.6% when rationing is introduced in a neighboring city in the previous month, suggesting geographically proximate adjustment and forward-looking operational responses to salient local interventions. The underlying mechanisms driving the spillover effect reflect both within-firm reallocation by multi-plant firms operating in neighboring cities (one rationed and the other not) and anticipatory responses by firms not yet rationed but facing the possibility of future restrictions.

Capital markets also respond to the onset of mandatory power rationing: treated firms experience significantly negative abnormal returns around rationing announcements (i.e., -0.5% over a [-1,1] window and -2% over a [-10,10] window), consistent with investors rapidly re-pricing firms affected by rationing-induced production disruptions. Complementing the event-study evidence, a stacked difference-in-differences design over a 252-trading-day window shows statistically significant post-event underperformance for treated firms (-2.7% for CAPM adjusted and -1.8% for Fama-French three-factor model adjusted), highlighting that rationing has persistent valuation effects beyond the immediate announcement.

Lastly, we investigate whether the 2021 power-rationing campaign has long-run effects on investment and operating outcomes for affected firms. In the years after 2021, firms with plants ever exposed to mandatory rationing reduce investment, with capital expenditures falling by about 9.7% relative to otherwise comparable control firms, enriching existing evidence that government interventions can generate unintended consequences. Power rationing also depresses other performance metrics, including sales, employment, R&D, total factor productivity, and leverage. Taken together, the results highlight both the direct real effects of administrative quantity controls and the endogenous responses they elicit—responses that shape the ultimate impact of the policy in China's hybrid economic environment.

Literature review. Our paper contributes to several strands of literature. A long-standing debate focuses on how the interplay between market forces and government intervention affects resource allocation in China's hybrid economy. A substantial literature examines how this interplay shapes productivity and growth. Early work by Brandt and Zhu (2000) argues that China's decentralized reforms produced cycles of rapid growth followed by inflation and retrenchment, as local governments alternated between expansionary investment and central restraint. Recent studies continue to evaluate the role of government involvement in economic activities and resource allocation. For instance, Song and Xiong (2023) propose a theoretical "mandarin" growth model in which the central government's promotion incentives for local officials can spur growth but may crowd out private investment when local debt constraints bind.[1] Building on the ambiguous effects of central macroeconomic management on performance, follow-up studies have empirically examined the role of specific planning tools—such as annual economic growth targets (Chang, Wang, and Xiong, 2025b) and infrastructure investment campaigns (Qian, Ru, and Xiong, 2025)—in shaping local economic outcomes.[2]

Our study complements this literature by investigating how the private sector responds to an influential macroeconomic policy shock—China's 2021 administrative power rationing—and how firms proactively adapt to local government intervention. Moreover, we find that manufacturers with plants in cities affected by the rationing subsequently cut back on investment, an outcome unintended by policymakers. This finding connects to research on the unintended consequences of government policy shocks, such as the rise of shadow banking (Bai, Hsieh, and Song, 2016; Chen, Ren, and Zha, 2018; Cong, Gao, Ponticelli, and Yang, 2019; Chen, He, and Liu, 2020; Acharya, Qian, Su, and Yang, 2025), asset-price distortions (Ding, Xiong, and Zhang, 2022; Chen, Chen, He, Liu, and Xie, 2023), shifts in corporate behavior (Cong and Howell, 2021; Hu, Liu, Yao, and Zong, 2025), and household education decisions (Liu, Meng, Pan, and Zhao, 2025). More broadly, power rationing represents a strategic lever of macroeconomic intervention to guide the sectoral allocation of resources. By studying firm reactions to this policy, our paper adds to the emerging literature on the political economy of industrial policy in China (see, e.g., Liu, Simon, Sun, and Cao, 2011; Chen and Naughton, 2016; Ge, Xue, and Zhang, 2024; Barwick, Kalouptsidi, and Zahur, 2025; Chen, Gong, and Li, 2025; Fang, Li, and Lu, 2025; Garcia-Macia, Kothari, Li, and Tao, 2025).

Secondly, our paper contributes to the growing literature that uses alternative data to measure economic activity and to study its economic implications. Satellite remote sensing of land surface temperature has become increasingly influential, aided by the development of open-source platforms such as Google Earth Engine (Ermida, Soares, Mantas, Göttsche, and Trigo, 2020). More recently, thermal infrared radiation (TIR) imagery has provided a direct, high-frequency, and high-spatial-resolution proxy for the intensity of plant-level production. Xue, Zhang, and Zhao (2025) validate plant-level

[1] China's governance is often characterized as a "tournament" among local officials (Li and Zhou, 2005), where promotion to higher rank depends on meeting certain economic performance criteria. This incentive structure has far-reaching implications for policy implementation, influencing various outcomes including pollution control (Jiang and Tang, 2023), resource allocation (Cull, Xu, Yang, Zhou, and Zhu, 2017; Liu, Xu, Yu, Rong, and Zhang, 2020; Wang, Zhang, and Zhou, 2020; He, Liao, and Wang, 2022; Lei and Zhou, 2022), local protectionism (Barwick, Cao, and Li, 2021; Fang, Li, and Wu, 2025a; Hu, Xue, and Zhao, 2025), and neighborhood development (Yu, Zhou, and Zhu, 2016; Shi and Xi, 2018). For reviews of the political economy of China's growth, see, e.g., Xu (2011), Zhu (2012), and Brandt, Ma, and Rawski (2014).

[2] Other work explores additional facets of the state–market interplay in China, including firms' geographic proximity to government (Duchin, Gao, and Shu, 2020), government-sponsored industrial parks (Tian and Xu, 2022), information frictions (Sockin and Xiong, 2024), bottom-up institutional changes (Chen, Li, and Zhu, 2024), strategic emissions (Zhao and Yuan, 2026), various forms of government-induced investment (Chen, Sun, Tang, and Wu, 2011; Hao and Lu, 2018; Liu, Pan, and Tian, 2018; Ru, 2018; Huang, Pagano, and Panizza, 2020), and real estate market dynamics under policy influence (Xiong, 2023; Chang, Wang, and Xiong, 2025a; Jin, Ru, Yang, and Zou, 2025; Chen, Du, and Ma, 2024).

TIR signals as a real-time measure of corporate activity and show that TIR-based indicators predict firms' operating performance and stock returns in the cross section. From a global perspective, Huang, Zhang, and Zhao (2023) demonstrate that TIR-derived measures contain macroeconomic information and track aggregate economic output both over time and across cities in various countries. Building on these insights, we use plant-level TIR to study a large, policy-induced production shock—China's 2021 power rationing. Relative to these studies, which emphasize measurement and validation of the predictive content of TIR for operating activity, our focus is causal identification and, more importantly, firms' proactive production adjustments under administrative constraints. Our empirical design exploits the granularity of the TIR dataset to examine how market forces respond immediately to local power-rationing-induced production interruptions.

In addition, satellite-based night-light intensity has been widely used as a proxy for economic output and growth (Chen and Nordhaus, 2011; Henderson, Storeygard, and Weil, 2012; Gibson, Olivia, Boe-Gibson, and Li, 2021; Bluhm and Krause, 2022; Hu and Yao, 2022; Pandey, Brelsford, and Seto, 2022; Chor and Li, 2024). Our study contributes to this broader remote-sensing literature by employing satellite-based TIR data to quantify manufacturing output at the plant level, providing additional evidence on the value of satellite-based measures for economic analysis.[3]

Finally, our research relates to studies on the economic impacts of power rationing. In China, electricity rationing has been examined both as an unintended consequence of power shortages and as a deliberate tool of macroeconomic management. Recent work shows that power shortages—often triggered by extreme weather (see, e.g., Liang, Qiu, Wang, Shen, and Liu, 2025)—can lead authorities to prioritize residential electricity use at the expense of industrial users, with adverse effects on manufacturing output and operational performance (Hao, Huang, and Zhang, 2025; Guo, Li, Liu, Shi, and Yu, 2023; Jia, Guo, and Liu, 2023; Bao, Fu, Yu, and Zhang, 2024). In contrast, the widespread 2021 power rationing represents an active, policy-driven intervention motivated by the "dual control of energy consumption" agenda and broader carbon-neutrality objectives. Using the 2021 power rationing as a natural experiment, Meng, Kang, Yang, and Yang (2024) find negative stock-market reactions for firms in high-energy-intensity industries following the policy implementation at the provincial level. Related studies on earlier regulatory interventions document differential effects across ownership types (Xue, Hong, and Chen, 2013), implications for electricity market efficiency (Cao, Ho, Ma, and Zhang, 2024), and declines in firm performance (Liu, Fang, and Xie, 2021). Our analysis adds to this literature by showing how affected firms reallocate production across regions in response to the 2021 power rationing, by documenting immediate and medium-term stock-market reactions, and by highlighting an unintended longer-run consequence: lower subsequent investment. In addition, we connect China's experience to the broader evidence on power outages and rationing in other countries (see, e.g., Allcott, Collard-Wexler, and O'Connell, 2016; Cole, Elliott, Occhiali, and Strobl, 2018; Meles, 2020; Gorman, 2022; Thomas and Fung, 2022; Borenstein, Bushnell, and Mansur, 2023; Mensah, 2024; Lin and Kassem,

[3]For reviews of studies using satellite-based night lights to measure economic performance, see Donaldson and Storeygard (2016) and Gibson, Olivia, Boe-Gibson, and Li (2021). Recently, a variety of other novel proxies have also been introduced. For instance, researchers use smartphone and GPS geolocation data to track human mobility and infer economic activity (Froot, Kang, Ozik, and Sadka, 2017; Jia, Lu, Yuan, Xu, Jia, and Christakis, 2020; Fang, Wang, and Yang, 2020; Alder, Song, and Zhu, 2023; Elenev, Quintero, Rebucci, and Simeonova, 2025; Kreindler and Miyauchi, 2023; Li, Ruan, and Wang, 2025). High-frequency indicators such as parking lot occupancy have been used as measures of consumer activity (Kang, Stice-Lawrence, and Wong, 2021; Gerken and Painter, 2023; Hao, Wang, Wang, and Wu, 2025); real-time consumer transactions have been used to study stock price informativeness (Zhu, 2019); and satellite-based land-use measures have been used to study urbanization and development trends (Burchfield, Overman, Puga, and Turner, 2006; Saiz, 2010).

2025). By studying China's 2021 episode, our paper provides new evidence on how large-scale power rationing in an emerging economy affects firm performance and reshapes firm behavior.

2 Institutional Background

In this section, we provide discussions on the history of power rationing policies in China, the background of "dual control of energy consumption" in the past decade, and the resulting 2021 power rationing campaign across the country.

2.1 History of power rationing in China

Power rationing ("la zha xian dian" in Chinese) has long served as a practical instrument of macroeconomic management in China's hybrid economy. After decades of reform and market opening, most sectors have been governed largely by market forces, but the electricity sector—an essential intermediate input for manufacturing economies like China—is treated as a strategic public utility rather than a standard commercial commodity and thus remains tightly regulated. While power plants are owned by profit-seeking firms, mostly SOEs with an increasing share from non-SOEs, they do not sell electricity directly to customers. Instead, electricity is delivered through the state-owned grid, a regulated natural monopoly. Under China's "orderly electricity use" framework, local governments coordinate with grid companies to implement curtailment plans and load-management lists during system stress, typically prioritizing residential and essential public services while rationing industrial users.[4] With retail tariffs adjusting sluggishly and dispatch remaining administrative, supply–demand imbalances are therefore often resolved through *quantity controls* rather than market-clearing price movements (see, e.g., Fisher-Vanden, Mansur, and Wang, 2015).[5]

In policy practice, rationing takes two main forms. Under orderly management, local authorities pre-announce off-peak schedules, rotate shutdowns across plants, and impose differentiated restrictions that prioritize on public and residential use over industry use. In contrast, campaign-style rationing involves abrupt and broad curtailments across almost all firms, sometimes even affecting public services or residents. A useful distinction views episodes as either *passive curtailment* driven by genuine shortages or *active regulation* deployed to meet administrative objectives (Guo, Li, Liu, Shi, and Yu, 2023). Our focus—the 2021 nationwide power ration—was triggered by passive rationing in some regions but quickly evolved to active regulation motivated by political incentives, which we will discuss in detail in Section 2.3.

[4] China's "orderly electricity use" began as a pragmatic load-rationing tool during the early-2000s power shortages, when many provinces implemented planned rotation and peak-shifting schedules to keep the system stable. By the mid-2000s, it evolved from ad-hoc outages into more structured demand-side management, with local governments and grid companies drafting orderly-use plans, specifying controllable loads and time windows, and pairing these with load-management systems and pricing incentives. It was formalized nationwide in 2011/5 when the National Development and Reform Commission (NDRC) issued the Measures for Orderly Consumption of Electricity, creating a legal mechanism to control part of electricity demand during supply shortfalls or emergencies through administrative, economic, and technical measures. In 2023, NDRC issued an updated edition of Measures for Electricty Load Management that provides guidance for local governments to maintain and revise orderly-use plans (typically ahead of summer/winter peaks), and for grid operators to monitor supply-demand conditions and issue warnings and daily execution arrangements, following principals of safety, stability, priority, and prevention.

[5] Other aspects of "quantity control"-type regulation in China include but not limited to bank credit allocation (Cong, Gao, Ponticelli, and Yang, 2019), monetary policy target on bank balance sheets (Chen, Ren, and Zha, 2018), real estate (Brueckner, Fu, Gu, and Zhang, 2017), and pollution control Shi and Xu (2018).

Since the early 2000s, large-scale rationing has arrived in three waves (Guo, Li, Liu, Shi, and Yu, 2023). The 2002–2004 episode was primarily (passive) capacity-driven. Post-WTO industrial expansion and unexpectedly high energy intensity pushed demand far beyond existing supply capacity. With strict caps on electricity prices, many cities managed shortages through planned factory blackouts, staggered production days, and restrictions on energy-intensive industries. The 2010–2011 wave, by contrast, was more (active) policy-driven. Binding energy-intensity targets in the final assessment year of the 11th Five-Year Plan strengthened enforcement incentives, leading some local officials to adopt campaign-style cuts (Xue, Hong, and Chen, 2013). The 2021 episode combined tight conditions in parts of the power system with stronger political incentives to comply with central government's conservation of energy and decarbonization objectives, making it more *active* than a purely shortage-driven event. In the next section, we describe China's "dual control of energy consumption" regime, which has functioned as the foundational regulatory paradigm shaping the allocation of electricity resources in the past decade and helps explain the nationwide power rationing in 2021.

2.2 Dual control of energy consumption

The Dual Control System of Total Energy Consumption and Energy Intensity serves as the foundational pillar of China's energy governance. Formally introduced during the Fifth Plenary Session of the 18th Central Committee of the Communist Party of China in 2015, the regime imposes binding targets on both absolute energy consumption and energy intensity, i.e., energy per unit of GDP. Under this framework, energy metrics function as high-stakes performance indicators for local officials, often carrying the weight of a "one-vote veto", i.e., failing to meet energy quotas can automatically disqualify a cadre from promotion, regardless of success in driving economic growth.

The pressure of the "one-vote veto" internalizes national ecological goals into local officials' objective functions and incentivizes direct state intervention in the power market. When provincial or prefectural energy consumption nears its regulatory limit, or when political mandates surrounding "dual control" intensify nationwide, local governments use an administrative tool for power rationing. To satisfy annual audits and avoid political sanctions, authorities may implement abrupt rationing, especially targeting high-energy-consuming and high-emission industries. These curtailments represent a political-economy equilibrium in which administrative compliance can dominate short-run economic goals (see, e.g., Chen, Li, and Lu, 2018), effectively using power supply as a valve to manage aggregate energy consumption and carbon emissions.

2.3 The 2021 power rationing

Following a multi-year policy transition from a development-first mandate to a balance between economic growth and energy- and carbon-efficient development, the 2021 power rationing can be viewed as the culmination of this strategic shift—local enforcement of a centrally designed decarbonization agenda under binding energy constraints. The sequence began on September 22, 2020, when President Xi Jinping announced at the United Nations General Assembly that China would aim to peak carbon dioxide emissions before 2030 and achieve carbon neutrality before 2060. Building on this pledge, the 14th Five-Year Plan (2021–2025)—initially outlined in October 2020 and officially released in March 2021—set binding targets to reduce energy consumption per unit of GDP by 13.5% and carbon

emissions per unit of GDP by 18% between 2021 and 2025. These targets were operationalized through the dual-control system, turning energy use into a hard constraint that is monitored and enforced administratively.

However, even as the central government reiterated the importance of energy and carbon targets, many regions struggled to meet them during the final period of the 13th Five-Year Plan (2016–2020). Entering 2021, this constraint tightened sharply. On August 12, 2021, the NDRC released the *Barometer of 2021 H1 target completion for dual control of energy consumption by region*, flagging many provinces as severely off track on their annual dual-control objectives. For "red-alert" regions, the NDRC required provincial governments to suspend energy-saving reviews for new "two-high" projects (high energy consumption and high pollution), effectively delaying project approvals and raising the political stakes of compliance. The central government soon reaffirmed energy-intensity reduction as a binding constraint. On September 11, 2021, the NDRC issued the *Plan for improving the dual-control system for total energy consumption and energy intensity*, which emphasized stricter supervision of "two-high" projects and strengthened accountability for officials who would fail to meet dual-control requirements.

Against this backdrop, rationing emerged as a practical enforcement mechanism. Local governments translated dual-control pressure into electricity curtailment by coordinating with grid operators through orderly-use plans and load-management lists. While parts of the system experienced genuine supply tightness,[6] the 2021 episode quickly evolved into a predominantly policy-driven intervention: a total of 152 cities adopted abrupt, mandatory, and campaign-style cuts—often concentrated on energy-intensive manufacturing firms—to deliver near-term reductions in measured energy use. This convergence of localized systemic stress and heightened political incentives helps explain both the suddenness and the administrative intensity of the 2021 rationing. It also provides an ideal empirical setting to study how firms adjust and reallocate production across plants in the short run and recalibrate their investment trajectories over longer horizons.

3 Data and Variable Construction

In this section, we describe our data sources and the construction of key variables. We also compare pre-policy city and firm characteristics between treated and control groups to assess baseline differences in exposure to mandatory power rationing.

3.1 Satellite-based Thermal Infrared Radiation Data

We measure firms' production activity using satellite-based thermal infrared radiation (TIR), which captures heat emissions generated during industrial operations. The underlying physical basis is *Planck's Law of Radiation*: any object with a temperature above absolute zero emits electromagnetic radiation, and radiance in the thermal infrared spectrum increases monotonically with surface tem-

[6] The cross-city correlation between the average planned (unplanned) power shortage index from (Guo, Li, Liu, Shi, and Yu, 2023) over 2007–2016 and the indicator for mandatory power rationing in 2021 is −0.11 (−0.08). Moreover, for the 30% cities with the least planned (unplanned) power shortage over 2007–2016, 53.97% (50.79%) issued mandatory power-rationing policies in 2021; by constrast, the corresponding shares are 46.03% (42.86%) among the 30% of cities with the highest historical shortage. Taken together, these patterns suggest that the 2021 rationing episode reflected not only passive supply shortfalls but also active administrative constraints on electricity use.

perature (Kogure and Leung, 2007). Manufacturing activity—transforming materials and energy into final goods—generates substantial waste heat through machinery operation, chemical reactions, and other energy-intensive processes, making TIR a useful proxy for real production activity in industrial settings (Xue, Zhang, and Zhao, 2025).

In terms of measurement, satellite sensors record long-wave infrared radiance from the Earth's surface and convert it into brightness temperature, measured in Kelvin. Relative to financial statement data or cross-region macro indicators commonly used in economic research, TIR offers several advantages in our setting: it is available at fine spatial resolution that can be matched to individual plants, updated at high frequency, difficult to manipulate, and directly tied to physical production processes. At the same time, TIR may also reflect non-production anthropogenic activity and meteorological conditions. We therefore construct our TIR-based production measure by netting out background heat and controlling for relevant environmental conditions.

The TIR data are obtained from the Alt Data Tech TIR Database, which is constructed from imagery collected by NASA's Landsat 8 and Landsat 9 satellites. These satellites provide thermal infrared bands at a spatial resolution of roughly 30 meters, allowing us to capture temperature variation at the plant level. Each satellite revisits a given location every 16 days; because their orbit paths are staggered, most locations are observed about once every eight days. Raw imagery is processed by the U.S. Geological Survey (USGS), which classifies observations into Tier 1 and Tier 2 products based on geometric and radiometric quality. The vast majority (97%) of images used in our analysis are Tier 1. To ensure consistent measurement across plants and avoid biases from irregular satellite coverage, we restrict the analysis to a balanced panel of plants with complete monthly observations.

Our construction of plant-level abnormal TIR follows Xue, Zhang, and Zhao (2025). For each plant owned by publicly listed manufacturing firms identified in the Controlled & Participated Company Database of Listed Companies (CPCD) on the Chinese Research Data Services (CNRDS) platform, we use cloud-free Landsat thermal infrared observations to construct a monthly measure of surface temperature. Specifically, observed plant TIR ($PR_{p,m}$) is defined as the average surface temperature of all thermal infrared pixels within plant p's geographic boundary in month m.

To account for local meteorological and background conditions, we also compute a bare-land benchmark using pixels in a 5-km buffer surrounding the plant, excluding the plant footprint itself.[7] Bare-land TIR ($BR_{p,m}$) is the average surface temperature of these surrounding pixels and proxies for the local ambient thermal baseline, which is largely orthogonal to plant-level production activity. Our main production-related measure, adjusted thermal infrared radiation ($ATIR_{p,m}$),[8] is defined as the difference between plant TIR and the surrounding bare-land benchmark:

$$ATIR_{p,m} = PR_{p,m} - BR_{p,m} \tag{1}$$

[7] In constructing the bare-land benchmark used to compute adjusted thermal infrared radiation, we treat roads as impervious surfaces rather than natural bare land and therefore exclude them from the surrounding buffer zone. Regarding the choice of buffer size, a smaller radius (e.g., 1 km or 3 km) may not adequately capture the relevant ambient thermal background and local microclimate, whereas a substantially larger buffer (e.g., 10 km) may introduce heterogeneous land cover and unrelated heat sources that add noise and dilute the production-related signal. The 5-km buffer therefore balances capturing local environmental conditions with limiting measurement noise.

[8] To investigate the determinants of production-related thermal infrared radiation, we run cross-city regressions of ATIR on a set of city characteristics—environmental variables and economic indicators—measured in 2020, prior to the policy shock. Meteorological factors, especially humidity and precipitation, are most strongly associated with ATIR (see Appendix Table A1). This evidence motivates controlling for local meteorological conditions in subsequent analyses.

To further mitigate the influence of seasonality and time-invariant regional conditions, following Xue, Zhang, and Zhao (2025), we construct a year-over-year change of production-related TIR, denoted as $\Delta ATIR_{p,m}$. Specifically, we define $\Delta ATIR_{p,m}$ as the change in the plant's adjusted TIR in month m relative to the same calendar month in the previous year, scaled by the absolute level of adjusted TIR in month $m-12$:

$$\Delta ATIR_{p,m} = \frac{ATIR_{p,m} - ATIR_{p,m-12}}{|ATIR_{p,m-12}|}. \tag{2}$$

Lastly, we apply several filters to the plant-level sample. Specifically, we retain plant observations that 1) belong to manufacturing firms, 2) are operated by Shanghai- or Shenzhen-listed firms, 3) are owned by firms that completed their IPOs before the sample period, 4) are owned by firms that are not delisted during the sample period, 5) are owned by firms that are never designated as ST, *ST, or PT during the sample period, 6) are not labeled as non-plant sites, 7) are not located in province-administered counties or in Taiwan Province. Our final plant-level sample covers 9939 plants operated by 1933 publicly listed Chinese manufacturing firms.[9]

3.2 Power Rationing Policy Data

We hand-collect city-level power rationing policies issued by local governments in China during 2021. The policy materials include proposal for energy-conservation initiatives, official notices, and implementation guidelines by municipal governments or local electricity authorities. Over 2021/4 to 2021/12, 253 out of 336 cities have implemented power rationing or released proposal for power saving, accounting for 75.3% of cities in China.[10]

Policy content and enforcement intensity vary substantially across cities. While some cities impose strict requirements on electricity usage, others merely encourage firms and households to conserve power voluntarily. Because administrative interventions under the "orderly electricity use" framework have long been a recurring feature in China, milder initiatives may not be viewed as binding by firms and therefore may have more limited real effects than stringent mandates. To classify policy strictness in a systematic manner, we use a large language model (LLM) to categorize each policy document into one of five tone classes: very lenient, lenient, neutral, strict, or very strict. We implement this procedure using GPT-4o, OpenAI's flagship multimodal model announced on May 13, 2024. In our implementation, GPT-4o supports a 128,000-token context window and up to 16,384 output tokens per request, which largely eliminates the need to truncate or otherwise trim policy texts before model input. For each policy document, we use the following prompt (originally in Chinese; translated into English here) to ask the model to (i) select exactly one of the five strictness levels and (ii) provide a brief justification for its classification.

Prompt: *"Assume you are a public policy expert. Based on the policy document or initiative*

[9] Manufacturing firms are defined using the China Securities Regulatory Commission (CSRC) twenty-nine industry classifications (2012 edition) where manufacturing corresponds to industry codes starting with "C". We exclude three plants located in Taiwan Province from three listed companies (002158.SZ, 300666.SZ, 601138.SH); we also exclude 67 plants located in 12 province-administered counties. The number of firms and plants remaining after each filter is reported in Appendix Table A2.

[10] The full text of the official announcement is not available for 73 out of 253 cities that issued a power-rationing policy in 2021. For these cases, we collect local news reports on power rationing and require that the article explicitly cites the title of the official document or reproduces its key implementation guidelines.

(title and body), classify its tone into one of five levels—very lenient, lenient, neutral, strict, or very strict. Select exactly one level and provide a one-sentence justification."

The model classifies 15 of the 253 cities in our policy sample as "very strict", 137 as "strict", 81 as "neutral", and 20 as "lenient"; none are labeled "very lenient". We then collapse the five categories into two groups, treating "strict/very strict" as *mandatory* power rationing and "neutral/lenient" as *voluntary* conservation advocacy.[11] In the empirical analysis that follows, we define treated plants as those located in cities with mandatory rationing (see Appendix Table A3 for the full list of the 152 treated cities), which represents a binding, administratively enforced constraint on electricity use. Plants located in cities that never issued a rationing policy or issued only voluntary conservation notices serve as the control group.

Figure 2 plots the cumulative number of Chinese cities that had issued power rationing policy by the end of each month from April through December 2021. Over this period, 253 of 336 cities released rationing policies, including 152 mandatory measures and 101 voluntary initiatives. Policy adoption accelerates in August and September 2021, coinciding with the NDRC's release of the *Barometer of 2021 H1 Target Completion for Dual Control of Energy Consumption by Region* and the *Plan for Improving the Dual-Control System for Total Energy Consumption and Energy Intensity*.

[Insert Figure 2 about here]

To assess whether cities implementing mandatory power rationing differ systematically from other cities prior to policy adoption, we compare a broad set of pre-policy city characteristics. These include environmental conditions (temperature, humidity, precipitation, sunshine duration), economic indicators (GDP growth, per capita GDP, industrial structure), energy consumption intensity, population size, urban land area, total factor productivity, and new COVID-19 cases. Table 1 reports average values measured in 2020. Overall, treated cities are broadly comparable to other cities along most dimensions. A small number of differences are statistically significant: treated cities appear modestly more economically developed and less energy intensive. In particular, treated cities have 10.5% higher per capita GDP and consume 800 fewer tons of coal per RMB 100 million of GDP, on average. Taken together, these patterns suggest limited systematic divergence between treated and control cities before the policy shock, alleviating concerns that mandatory rationing is concentrated in places with markedly different pre-existing economic or climatic conditions.

[Insert Table 1 about here]

In addition, we compare pre-policy firm characteristics across groups using variables measured as of 2020/12/31. Table 2 reports the results. Panel A contrasts manufacturing firms with non-manufacturing firms in the Chinese A-share market. The two groups are similar in size, but manufacturing firms are significantly less levered and have lower book-to-market ratios. They also exhibit stronger operating performance and higher investment intensity: average ROA is higher by 2.9% and CAPX by 1.5% relative to non-manufacturing firms. Manufacturing firms further display a higher

[11] Following the approach of Zhang, Shao, Chen, and Benitez (2024), we also manually classify all documents into the mandatory and voluntary categories. The resulting manual and LLM-based indicators are positively correlated ($\rho = 0.65$, $p < 0.01$), indicating broad concordance between the two approaches.

Tobin's Q, consistent with stronger growth opportunities and, potentially, higher market valuations in the manufacturing sector (Brandt, Van Biesebroeck, Wang, and Zhang, 2017; Megginson, Xia, et al., 2022).

Panel B restricts the sample to manufacturing firms and compares treated firms—those with any plant-level exposure to mandatory power rationing during our sample period—with control firms that have no such exposure or have only exposure to voluntary advocacy. Treated firms are larger on average and have modestly higher leverage and book-to-market ratios, suggesting that mandatory rationing is more likely to occur in economically important manufacturing hubs. In contrast, ROA and CAPX are similar across the two groups, while Tobin's Q is lower for treated firms. Overall, while treated firms are larger and differ modestly in leverage, book-to-market, and Tobin' s Q, our stacked DID design—which we will describe in detail in Section 4.1.1—absorbs these time-invariant baseline gaps through plant×cohort fixed effects; given the short sample window, this helps ensure identification comes from within-plant changes around the policy event rather than from persistent cross-firm differences in levels.

[Insert Table 2 about here]

3.3 Other Data

Other data of this study come from multiple sources. We obtain stock return data and firm-level accounting data from CSMAR, and market-wide measures on financial conditions and the number of COVID-19 cases are from WIND. Environmental variables—including temperature, relative humidity, precipitation, and sunshine duration—are from the China Meteorological Data Network. City-level economic indicators, energy consumption intensity, population, urban land area, and total factor productivity are from the China Urban Statistical Yearbook.

Table 3 reports summary statistics for the main variables used in our analysis. All variables are winsorized at the 1st and 99th percentiles. Panel A summarizes plant-month environmental variables, including our production measures based on thermal infrared radiation: The mean adjusted TIR, $ATIR_{p,m}$, is 1.1 Kelvin, and the mean year-over-year change, $\Delta ATIR_{p,m}$, is 27.5%. Panel B reports firm-day stock market variables and Panel C reports firm-year accounting variables.[12]

[Insert Table 3 about here]

4 Empirical Results

In this section, we present our empirical findings on the economic effects of mandatory power rationing on plant-level production. We begin with baseline stacked DID estimates and trace the dynamic treatment effects around the onset of rationing. We then examine heterogeneity across subsamples and conduct a comprehensive set of robustness checks. After documenting how government intervention affects firms' production, we study firms' proactive production reallocation across plants and whether

[12]The detailed variable definitions are provided in Appendix Table A4. We also report summary statistics for the stacked plant-–month panel and the stacked firm–day panel in Appendix Table A5.

rationing in one city spills over to production in neighboring cities. Finally, we turn to broader market implications, including stock-price responses, long-run investment, and operating performance.

4.1 The Impact of Power Rationing Policies: DID Estimator

We begin our empirical analysis with a stacked DID design. We first describe the identification strategy, present our main estimates, assess the plausibility of the parallel-trends assumption, examine heterogeneity across subsamples, and conduct a battery of robustness checks.

4.1.1 Stacked DID Estimations

To estimate the effects of staggered policy adoption across cities on manufacturing firms' production, we employ a stacked DID estimation strategy. The goal is to compare TIR-measured production for plants located in treated cities with that for plants in untreated cities, before and after each cohort of power rationing policy announcements. Because policies are introduced in a staggered fashion and treatment effects may be dynamic, a conventional two-way fixed effects DID can place negative weights on some comparisons (De Chaisemartin and d' Haultfoeuille, 2020; Callaway and Sant' Anna, 2021; Goodman-Bacon, 2021), or mix effects from different relative-time periods (Sun and Abraham, 2021). Moreover, in standard staggered DID regressions, already-treated plants may serve as controls for later-treated plants, which can further bias estimation (Baker, Larcker, and Wang, 2022). By contrast, the stacked DID design constructs cohort-specific comparison sets with "clean" controls and aggregates time-varying treatment effects in a way that is robust to staggered adoption and dynamic responses.[13]

Following recent work (see., e.g., Chang, Hsiao, Ljungqvist, and Tseng, 2022; Chen, Qian, Wang, and Wu, 2024; Hirshleifer and Ma, 2024), we implement stacked DID by partitioning the data into cohort-specific "sub-experiments" defined by the month in which a city first implements mandatory power rationing. For each cohort month c, we include 1) plants located in cities first treated in month c and 2) plants located in cities that remain untreated throughout the $[c-4, c+3]$-month event window.[14] We then stack these cohort-specific samples and estimate a single OLS regression that absorbs plant×cohort fixed effects and time×cohort fixed effects (equivalently, sub-experiment-by-time effects). This design ensures that identification comes from within-cohort comparisons between newly treated plants and clean controls within the event window. Over our sample period, this procedure yields nine cohorts. Our stacked difference-in-differences specification is:

$$\Delta ATIR_{c,p,m} = \beta_1 Treat_{c,p} \times Post_{c,m} + \gamma_{c,p} + \delta_{c,m} + \sigma_{k,m} + \eta_{l,m} + X'_{p,m} e + \epsilon_{c,p,m} \tag{3}$$

The dependent variable, $\Delta ATIR_{c,p,m}$, is the year-over-year change in production-related thermal infrared radiation for plant p of cohort c in calendar month m. $Treat_{c,p}$ equals one if plant p is located in a treated city in cohort c, and $Post_{c,m}$ equals one for months on and after the policy announcement for cohort c. $\gamma_{c,p}$ denotes Plant×Cohort fixed effects, which absorb time-invariant plant-level determinants

[13] Stacked DID has been widely used in recent quasi-experiment studies, such as information environment shocks (Chang, Ljungqvist, and Tseng, 2023), environmental risk (Cong, Qu, and Wang, 2025), local shocks and real responses(Jeffers, 2024), tax frictions (Dambra, Glover, Lee, and Quinn, 2025), and religion and political economy (Bentzen, Pizzigolotto, and Sperling, 2026).

[14] Our choice of four observations before and after treatment follows standard practice in the recent literature (see, e.g., Cengiz, Dube, Lindner, and Zipperer, 2019; Chang, Ljungqvist, and Tseng, 2023). In Section 4.1.4, we also report estimates using a shorter window of $[c-3, c+2]$-month and a longer window of $[c-6, c+5]$-month, with similar results.

(within cohort) of production. $\delta_{c,m}$ denotes Year-month×Cohort fixed effects, which capture cohort-specific aggregate shocks common to all plants in cohort c in month m. With these two sets of fixed effects, the main effects $Treat_{c,p}$ and $Post_{c,m}$ are absorbed, so the model is identified off within-cohort, within-month differences between treated and control plants around the event.

In addition, we include Year-month×Industry fixed effects, $\sigma_{k,m}$, to control for industry-specific shocks over time, and Year-month×Province fixed effects, $\eta_{l,m}$, to absorb time-varying shocks common to plants within the same province, which are most likely driven by province-level policy changes. Finally, we control for a set of plant-month environmental conditions, $X_{p,m}$, including temperature, relative humidity, precipitation, sunshine duration, and new COVID-19 cases.

[Insert Table 4 about here]

Table 4 reports our stacked DID estimates across alternative specifications. Columns (1) and (2) include Plant×Cohort and Year-month×Cohort fixed effects, excluding and including controls, respectively. Column (3) further adds Year-month×Industry fixed effects,[15] Column (4) instead adds Year-month×Province fixed effects, and Column (5) includes both sets of fixed effects. Coefficients are reported in percentage, and t-statistics based on standard errors clustered at the plant-cohort level are reported in parentheses.[16]

After the mandatory power rationing policy is announced for a plant's city, Column (1), which excludes additional controls, yields an estimate of -19.1%, statistically significant at the 1% level. Adding the full set of controls in Column (2) leaves the estimate qualitatively unchanged: the coefficient remains negative at -15.4% and statistically significant. Economically, this implies that, over the eight-month window surrounding the announcement, plants in treated cities experience, on average, a 15.4% decline in production-related thermal infrared radiation relative to plants in control cities. Because abnormal TIR is approximately linear in anthropogenic activity (Xue, Zhang, and Zhao, 2025), and assuming a similar linear mapping to plant production, this magnitude suggests that affected plants reduce production by roughly 15% relative to their counterparts in unaffected cities.

One potential endogeneity concern is that time-varying industry shocks may be correlated with roll-out of mandatory power rationing, because cities often specialize—due to historical and policy-driven factors—in different industries, leading to heterogeneous reliance on particular sectors (Giorcelli and Li, 2025). Two such shocks are particularly plausible in 2021. First, sharp swings in key manufacturing input prices—coal, natural gas, steel, non-ferrous metals, and chemical raw materials—could differentially shift marginal costs and operating rates across industries, with larger effects on heavy chemical, metallurgical, and building-material sectors than on light manufacturing.[17] Second, the nationwide "dual control" agenda is often enforced through targeted measures against "two-high" industries and may intensify in particular months, potentially placing disproportionate pressure on energy-intensive and high-emission sectors. We address these concerns by adding Year-month×Industry fixed effects,

[15] We use the CSRC twenty-nine industry classifications (2012 edition). Our results are similar if we use other industry classifications provided by WIND or CITIC Securities.

[16] We also cluster standard errors at the plant, firm, and industry levels; statistical significance is similar. The results are reported in Online Appendix Table A6.

[17] Several pieces of evidence indicate there could be sharp price volatility in key energy and industrial inputs around 2021, including coal (https://www.cpnn.com.cn/news/hy/202201/t20220119_1476573.html), natural gas (https://www.chinagas.org.cn/4628/202111/667.html), steel (https://www.shfe.com.cn/upload/20220316/1647409882438.pdf), non-ferrous metal, (https://www.cnmn.com.cn/ShowNews1.aspx?id=433845), and chemical feedstock (http://youjia.chemcp.com/news/202507/130895.asp).

which absorb industry-wide shocks that vary over time—whether stemming from input-cost movements or from policy pressure concentrated on specific industries. Column (3) shows that our estimates remain very similar: treated plants experience a statistically significant 15.6% decline in production-related thermal infrared radiation relative to untreated plants following the policy shock.

Another identification concern is that province-month shocks—such as other policy announcements or the severity of power shortages themselves—may be correlated with the rollout of mandatory power rationing for cities within a province. If so, our baseline estimates could partly capture province-level shocks rather than the causal effect of staggered city-level rationing. We address this concern by adding Year-month×Province fixed effects, which absorb unobserved time-varying shocks common to all plants within the same province. Column (4) reports the results. The estimated effect remains statistically significant and becomes slightly more negative at -16.7% (t-statistic = 5.39), suggesting that our baseline estimates are not driven by province-level concurrent shocks.

Column (5) reports estimates from the specification that includes both sets of interactive fixed effects discussed above. The estimated policy effect remains negative and statistically significant, with a magnitude of -17.2%. Overall, these results are consistent with the conjecture that government intervention reduces production activity, even we control for a rich set of potential confounding shocks.

4.1.2 Dynamic Treatment Effects of the Policy Shock

After documenting the impact of mandatory power rationing policies on plant-level production activities, we next estimate a dynamic treatment effects model to assess the plausibility of the parallel-trends assumption. Specifically,we examine whether treated and control plants exhibit similar pre-policy trajectories in production activities, which would support a parallel counterfactual path absent the policy. The model specification is:

$$\Delta ATIR_{c,p,m} = \sum_{\tau=-4,\tau\neq-1}^{3} \beta_\tau Treat_{c,p} \times \mathbb{1}_{c,\tau} + \gamma_{c,p} + \delta_{c,m} + \sigma_{k,m} + \eta_{l,m} + X_{p,m}^{'} e + \epsilon_{c,p,m} \tag{4}$$

We partition the event window into eight monthly subperiods around the event month. The indicator $\mathbb{1}_{c,\tau}$ equals one for subperiod τ in cohort c and zero otherwise. We include Plant×Cohort fixed effects $\gamma_{c,p}$, Year-month×Cohort fixed effects $\delta_{c,m}$, Year-month×Industry fixed effects $\sigma_{k,m}$, Year-month×Province fixed effects $\eta_{l,m}$, and a set of plant-month environmental conditions $X_{p,m}$.

Figure 3 plots the event-study coefficients β_τ with 95% confidence intervals; the coefficient for the month immediately before the event month is normalized to zero. Following the policy announcement, the estimated treatment effect turns sharply negative. The effect is economically large and statistically significant in the first three post-event months: $\Delta ATIR$ falls by roughly 20–28 percentage points, and the confidence intervals lie clearly below zero. In the subsequent month (+3), the estimate moves back toward zero while remaining negative.

By contrast, the pre-event point estimates fluctuate around zero, and their confidence intervals overlap with zero (except for the estimate at event month -4), indicating no statistically detectable differences in production trends between treated and control plants prior to the policy month. Overall, the event-study pattern provides support for the parallel-trends assumption in our setting.

4.1.3 Heterogeneity Analyses

As discussed in Section 2, the 2021 power rationing was implemented in the shadow of the "dual control of energy consumption" regime, which places particular regulatory pressure on "two-high" activities—high energy consumption and high pollution. Therefore, the contractionary effects of rationing may differ across firms in energy-intensive or heavily polluting industries. In addition, to the extent that enforcement is discretionary or politically mediated (Xue, Hong, and Chen, 2013), ownership may shape compliance: state-owned enterprises (SOEs) and private-owned enterprises (POEs) may face different rationing intensity and thus experience differential production impacts. In this section, we conduct stacked triple-DID tests to explore the heterogeneous treatment effects across industry and ownership types. Following Chen, Qian, Wang, and Wu (2024), our model specification is as follows:

$$\begin{aligned}\Delta ATIR_{c,p,m} = \beta_1 Treat_{c,p} \times Post_{c,m} \times Group_p + \beta_2 Treat_{c,p} \times Post_{c,m} \times (1 - Group_p) \\ + \gamma_{c,p} + \delta_{c,m} + \sigma_{k,m} + \eta_{l,m} + X'_{p,m} e + \epsilon_{c,p,m}\end{aligned} \tag{5}$$

The indicator $Group_p$ equals one if plant p is operated by a firm in a specific specific industry type (energy-intensive or heavy-polluting) or with a specific ownership type (SOE), and zero otherwise.[18] In addition to the benchmark specification similar to the one in Column (1) of Table 4, we include the full set of interacted fixed effects and the plant-month environmental controls $X_{p,m}$, mirroring the specification in Column (5) of Table 4.

In Table 5, Columns (1)–(3) report results based on the benchmark specification, while Columns (4)–(6) add the full set of interacted fixed effects and controls. Contrary to the stated policy objective, the estimated production decline is smaller for energy-intensive plants than for non–energy-intensive plants in the benchmark specification (15.2% v.s. -21.1%). However, this difference disappears once we include the interacted fixed effects and controls (Column 4). A Similar pattern EMERGES for heavy-polluting versus non-heavy-polluting industries (-16.6% v.s. -17.6%), indicating limited evidence of differential effects for the targeted industries. While these results differ from our initial prior, they are consistent with a campaign-style, "one-size-fits-all" implementation that is not uncommon in China's policy execution (Cao, Ho, Ma, and Teng, 2021).

Columns (3) and (6) report results of heterogeneous effects for SOEs versus POEs. Because SOEs often have stronger explicit or implicit ties to local governments, they may receive greater implicit protection or administrative support during power-rationing episodes than POEs (Xue, Hong, and Chen, 2013). Consistent with this view, the estimated production contraction is smaller for SOEs than for POEs under both the benchmark specification (-11.5% v.s. -22.1%) and the specification with the full set of fixed effects and controls (-11.9% v.s. -19.2%). A Wald chi-squared test yields mixed statistical evidence for $\beta_1 = \beta_2$ ($p = 0.05$ in Column 3 and $p = 0.19$ in Column 6); nevertheless, the point estimates consistently indicate a sizable and economically meaningful SOE–POE gap in rationing-related production losses. This pattern is consistent with SOEs being partly insulated from rationing-related disruptions—potentially through preferential access to electricity or stronger administrative

[18] We define energy-intensive industries based on the sectors covered by the NDRC catalogue *Benchmark and standard energy efficiency levels in key high energy-consuming industries (2021 Edition)*, including petroleum processing (25), chemicals (26), nonmetallic mineral products (30), ferrous metals (31), and nonferrous metals (32), which correspond to the CSRC Level-2 industry codes C25, C26, C30, C31, C32. Following Zhou, Qiu, and Wang (2021), we classify heavy polluting industries as those in the CSRC Level-2 industry codes C15, C17, C18, C19, C22, C25, C26, C27, C28, C29, C30, C31, C32. Data on firms' ownership type are from WIND.

support—while POEs bear a larger share of the policy-induced production contraction.

Overall, the heterogeneity results reinforce our baseline finding that mandatory power rationing reduces production activity and point to an uneven distribution of impacts: while the contraction is more pronounced among POEs, it remains relatively uniform across plants with varying energy intensities or pollution levels. These patterns suggest that the policy may face challenges in fully achieving its intended environmental and efficiency objectives.

[Insert Table 5 about here]

4.1.4 Robustness Tests

In this section, we conduct a series of robustness tests for our stacked DID estimates. We consider alternative event windows, use voluntary (rather than mandatory) rationing as the event, restrict the control group to plants in never-treated cities, exclude observations from the four municipalities, and re-estimate the design at the province level. Table 6 reports the results. All specifications include the full set of environmental controls and interacted fixed effects discussed above, except for the province-level analysis, in which Year-month×Province fixed effects are not applicable and therefore omitted.

Columns (1) and (2) report estimates using shorter and longer event windows around the policy announcement month. The shorter window is designed to capture firms' immediate responses, while the longer window allows treatment dynamics to evolve over a wider horizon (Wing, Yozwiak, Hollingsworth, Freedman, and Simon, 2024; Chen, Qian, Wang, and Wu, 2024). Using the shorter $[-3, 2]$ window, the estimated effect is -26.8%, significantly larger in magnitude than our benchmark estimate of -17.2%. Using the longer $[-6, 5]$ window, the estimate slightly attenuates to -15.2%. Both estimates are statistically significant at the 1% level. This pattern is consistent with the event-study results: the production effects are most pronounced shortly after policy issuance and fade over time, suggesting that the 2021 episode is unlikely to reflect a purely persistent, shortage-driven disruption and is more consistent with a policy-driven (and potentially campaign-style) intervention.

In addition to the 152 cities that issued *mandatory* rationing policies, 101 cities released *voluntary* electricity-saving notices in 2021. Although these notices are not backed by binding enforcement, they provide an informative contrast for how firms respond to softer forms of government signaling.[19] Column (3) reports results when we use the voluntary notice as the event month. Different from the mandatory-policy estimates, the coefficient is economically small and statistically indistinguishable from zero, with a magnitude of -1.7%. This pattern is in line with prior evidence that voluntary environmental programs tend to generate modest effects on environmental performance(see., e.g., Montero, 1999; Ito, Ida, and Tanaka, 2018).

In our baseline stacked design, each cohort-specific sample compares plants in treated cities to plants in control cities, where the control group includes plants in cities that are either never treated or not yet treated within the $[c-4, c+3]$ event window. A potential concern with using plants in to-be-treated cities as controls is that (i) the composition of the control group varies across cohorts

[19] In this exercise, we drop all plants that are located in cities ever issued mandatory policy for three reasons: 1) to avoid overlapping treatments, since cities that later adopt mandatory rationing effectively experience a second, stronger intervention; 2) to mitigate concerns about post-treatment selection that arise when observations are censored following subsequent policy adoption; 3) to preserve a clear interpretation of the estimated effects as responses to non-binding government signaling rather than to impending enforcement.

and (ii) plants in not-yet-treated cities may begin adjusting behavior as rationing becomes more likely, which could blur the treated–control contrast within the event window (Baker, Larcker, and Wang, 2022). To address this concern, following Fisman, Ghosh, Sarkar, and Zhang (2023) and Du, Masulis, Pham, and Tak (2025), we re-estimate the stacked DID using only plants located in never-treated cities as controls for every cohort. Column (4) reports the result. The estimated coefficient is -16.2%, close to our benchmark estimate, suggesting that our main findings are not sensitive to the choice of clean controls.

Column (5) further shows that excluding observations from the four municipalities—Beijing, Shanghai, Tianjin, and Chongqing—does not affect our inference. The estimated effect remains negative and economically similar, with a magnitude of -16.9%, alleviating concerns that our baseline results are disproportionately driven by these administratively special municipalities.

Lastly, in addition to city-level administrative measures, some provincial governments also issued regulatory documents mandating power rationing. Over the period of 2021/4 to 2021/12, 26 out of 31 provinces issued power rationing policies, including 21 mandatory measures and 5 voluntary initiatives. Column (6) therefore redefines the event using province-level announcements. The estimated effect remains negative but is smaller in magnitude (-5.8%) and statistically significant at the 10% level. This attenuation is consistent with substantial within-province heterogeneity in the design and enforcement of power-rationing measures, reflecting the fact that rationing can be deployed as a flexible administrative tool for macroeconomic management and implemented with considerable local discretion.[20]

[Insert Table 6 about here]

4.2 Firms' Response: Intra-firm Production Reallocation and Anticipation

We have shown that plant-level production, as proxied by TIR, declines after a city implements mandatory power rationing policy, highlighting the direct effects of administrative intervention on local manufacturing activity. In China's hybrid economy, however, government quantity controls coexist with market-driven forces, and firms may not remain passive recipients of policy shocks. In this section, we study these responses and quantify how production adjusts when mandatory power rationing disrupts operations.

4.2.1 Intra-firm Production Reallocation

Baseline estimates show that mandatory power rationing forces treated plants to curtail or suspend production, creating potentially costly disruptions to order fulfillment and delivery schedules. These disruptions give multi-plant firms incentives to reallocate production from rationed facilities to plants in unaffected cities to stabilize effective capacity and protect short-run revenues. However, such reallocation is not automatic: it is optimal only when the benefits of preserving output outweigh the transfer and adjustment costs (Zhu and Ruth, 2015), so whether firms actually reallocate production is

[20] For example, although the Fujian provincial government issued a mandatory power-rationing directive on 2021/9/28, cities within Fujian adopted different approaches: Quanzhou implemented a voluntary policy on 2021/9/29, whereas Nanping did not implement power rationing in 2021. This within-province variation is consistent with prior evidence that policy enforcement and implementation can differ markedly across Chinese cities (see., e.g., He, Wang, and Zhang, 2020; Xie and Yuan, 2023).

an empirical question.[21] To test this within-firm reallocation channel, we restrict the sample to firms that operate plants in at least two cities and estimate the following specification:

$$\begin{aligned}\Delta ATIR_{c,p,m} = \beta_1 Treat_{c,p} \times Post_{c,m} + \beta_2 Other_{c,p,m} \\ + \gamma_{c,p} + \delta_{c,m} + \sigma_{k,m} + \eta_{l,m} + X_{p,m}^{'} e + \epsilon_{c,p,m}\end{aligned} \tag{6}$$

$Other_{c,p,m}$ is an indicator equal to one if plant p is not subject to mandatory power rationing in month m, but its parent firm operates at least one other plant located in a city that began implementing mandatory power rationing in month $m-1$.[22] The coefficient of interest is β_2, which captures within-firm reallocation by measuring whether production rises at an untreated plant when another plant in the same firm becomes newly rationed.

Table 7 reports the estimated coefficients β_2 on $Other_{c,p,m}$ alongside the direct treatment effect β_1, using the same sequence of specifications as in our benchmark estimates. Several patterns stand out. First, the direct impacts of mandatory power rationing on production remain negative and statistically significant across specifications, with point estimates ranging from roughly -12% to -15%. Second, the estimated β_2 are consistently positive—around 4% across all model specifications—suggesting that multi-plant firms do increase production at unaffected plants following rationing elsewhere within the firm. Importantly, this reallocation effect remains similar once we control for time-varying industry shocks and province-level shocks (Column 5), indicating that the finding is not driven by correlated aggregate disturbances. Finally, the reallocation effect is smaller in magnitude than the direct production contraction at treated plants, implying that net production disruptions persist even after intra-firm adjustments. Overall, these results support the view that market-driven production reallocation partially mitigates policy-induced disruptions, consistent with the broader interplay between market forces and government intervention in China's hybrid economy.

[Insert Table 7 about here]

4.2.2 Spillover Effects

We have shown that directly affected plants reduce production measured by TIR, while multi-plant firms partially offset these losses by reallocating activity from affected plants to unaffected plants. In this section, we investigate another form of proactive adjustment: whether and how plants in an untreated city change production when a neighboring city implements mandatory power rationing.

[21] Our focus on within-firm production reallocation relates to a large body of evidence from developed economies on internal adjustment margins in multi-unit firms. Fontagné, Martin, and Orefice (2024) show that French manufacturing firms optimize energy use across plants—as part of a broad set of adjustment channels—to proactively adapt to energy shocks. In the U.S. context, for example, Acharya, Bhardwaj, and Tomunen (2023) show that multi-location firms respond to local heat shocks by reallocating employment toward unaffected locations, Seetharam (2018) quantifies the spatial impacts of hurricanes on manufacturing jobs through plant ties within firms, and Giroud and Mueller (2015) document that financially constrained firms reallocate capital and labor across plants in response to plant-specific shocks. Additional examples include production reallocation in response to stringent environmental regulation (Cui and Moschini, 2020), capital reallocation to facilitate investment adjustment (Gallo, Labro, and Omartian, 2025; Almeida, Ersahin, Fos, Irani, and Kronlund, 2026), and the redeployment of resources from unproductive establishments to productive ones (Brav, Jiang, and Kim, 2015; Ersahin, Irani, and Le, 2021).

[22] This "impulse" construction isolates firms' short-run operational response to a newly realized disruption at another plant, while reducing confounding from longer-horizon adaptation and subsequent policy adjustments. It also keeps interpretation transparent: if $Other_{c,p,m}$ remained equal to one in all subsequent months, it would effectively become a staggered post-exposure indicator. A single coefficient would then average reallocation effects across heterogeneous exposure durations, blurring interpretation and potentially reintroducing mixed-horizon comparisons.

Spillovers are plausible because the 2021 rationing shock is highly salient and manufacturing firms that depend on stable electricity supply may respond to policy actions in nearby jurisdictions (see, e.g., Dessaint and Matray, 2017; Huang, Li, Lin, and McBrayer, 2022). In addition, China's tournament-style promotion system can amplify cross-city policy interactions, as prefectural leaders often compete for promotion within the same province (Li and Zhou, 2005; Yu, Zhou, and Zhu, 2016; Zeng and Zhou, 2024; Fang, Li, and Wu, 2025b). Under these conditions, a rationing announcement in one city may influence production decisions in adjacent cities even if those cities have not (yet) implemented mandatory curtailment.

With that being said, the sign of anticipatory effects is ultimately an empirical question (Ladino, Saavedra, and Wiesner, 2021; Hendricks, Neilson, Shakespeare, and Williams, 2023; Bruno and Hagerty, 2025). On the one hand, rationing in a neighboring city may trigger precautionary adjustment or anticipatory compliance with broader energy-conservation initiatives, leading firms to scale back output voluntarily. On the other hand, firms may instead ramp up production preemptively—accelerating output and deliveries before local restrictions arrive. Moreover, for multi-plant firms with facilities already affected elsewhere, neighboring untreated plants may absorb displaced production. We therefore examine the net spillover effects of mandatory power rationing using the following specification:

$$\begin{aligned}\Delta ATIR_{c,p,m} = \beta_1 Treat_{c,p} \times Post_{c,m} + \beta_2 Neighbor_{c,p,m} \\ + \gamma_{p,c} + \delta_{c,m} + \sigma_{k,m} + \eta_{l,m} + X'_{c,p,m} e + \epsilon_{c,p,m}\end{aligned} \tag{7}$$

Sharing similar flavor of Dessaint and Matray (2017), we define $Neighbor_{c,p,m}$ as an indicator that equals one if, in month m, plant p (in cohort c) is located in a city that remains untreated but shares a border with a city that first implements mandatory power rationing in month $m-1$. The coefficient of interest, β_2, captures the spillover response of geographically proximate yet untreated plants to nearby rationing shocks. We exclude the four municipalities from this analysis because our neighboring-city definition is based on prefecture-level jurisdictions within provinces.

Table 8 reports the estimated coefficients, β_1, on $Neighbor_{c,p,m}$ together with the direct treatment effect, β_1, across the same sequence of specifications used in our benchmark analysis. Two observations worth emphasizing. First, the direct effect of mandatory power rationing remains negative and statistically significant: treated plants experience sizable contractions in production-related TIR, with magnitudes between roughly -10% and -13% depending on the set of interacted fixed effects. Second, we find strong evidence of positive spillovers to geographically proximate but untreated plants. Across all specifications, the estimated β_2 on $Neighbor_{c,p,m}$ is positive and highly significant, with magnitudes around 12–26%. The stability of this coefficient as we successively add Year-month×Industry and Year-month×Province fixed effects suggests that the spillover is not driven solely by contemporaneous industry shocks or province-level trends, but reflects a systematic response of nearby plants to rationing shocks in adjacent jurisdictions.[23] Taken together, the positive spillover estimates are consistent with the preemptive output-expansion channel discussed above: on net, plants in neighboring untreated

[23] The estimated spillover effect may exceed the direct disruption effect in absolute magnitude. This pattern is consistent with the idea that rationing imposes a binding quantity constraint on treated plants, which limits the scope for contemporaneous adjustment at those locations, whereas neighboring untreated plants face no such constraint and may increase output. In this sense, the spillover coefficient can be interpreted as capturing an output-expansion margin among unconstrained plants, while the treatment coefficient reflects contraction at constrained plants. Importantly, the direct effect remains negative and economically meaningful across specifications, suggesting that spillovers attenuate but do not overturn the net production disruption.

cities increase production when rationing is introduced nearby, rather than contracting in anticipation.

[Insert Table 8 about here]

4.2.3 Decomposition of Spillover Effects

The positive spillover in neighboring, untreated cities can arise through two distinct channels. First, for multi-plant firms that are already affected elsewhere, production may be reallocated from rationed plants to nearby untreated plants—an intra-firm reallocation channel. Second, plants that are located in untreated neighboring cities may adjust production in anticipation of potential future rationing in their own city—an anticipation channel. To separate these mechanisms, we decompose the neighbor indicator into two mutually exclusive components:

$$Neighbor_{c,p,m} = \underbrace{Neighbor_{c,p,m} \times Other_{c,p,m}}_{\text{Intra-firm reallocation}} + \underbrace{Neighbor_{c,p,m} \times (1 - Other_{c,p,m})}_{\text{Anticipation of future power rationing}} \tag{8}$$

By construction, $Neighbor_{c,p,m} \times Other_{c,p,m}$ equals one for an untreated plant p in month m whose city borders at least one city implementing mandatory rationing in month $m-1$, and whose parent firm operated at least one other plant in an affected city in month $m-1$. In contrast, $Neighbor_{c,p,m} \times \left(1 - Other_{c,p,m}\right)$ equals one for an untreated plant in a bordering city whose parent firm had no plant exposed to mandatory rationing in the previous month. The first interaction therefore captures spillovers operating through within-firm production reallocation, whereas the second captures spillovers consistent with anticipatory adjustments by firms that remain untreated but face the risk of future rationing. We then assess the relative importance of these two mechanisms by augmenting the baseline specification with the two interaction terms and estimating:

$$\begin{aligned} \Delta ATIR_{c,p,m} = {} & \beta_1 Treat_{c,p} \times Post_{c,m} + \beta_2 Neighbor_{c,p,m} \times Other_{c,p,m} \\ & + \beta_3 Neighbor_{c,p,m} \times (1 - Other_{c,p,m}) + \gamma_{p,c} + \delta_{c,m} + \sigma_{k,m} \\ & + \eta_{l,m} + X'_{c,p,m} e + \epsilon_{c,p,m} \end{aligned} \tag{9}$$

Table 9 shows that both components are positive, suggesting that the two candidate channels are simultaneously at work. The coefficient on $Neighbor \times Other$ is economically large (about 9–24%) and highly significant, consistent with production being reallocated toward nearby unrestricted plants when the firm faces binding curtailment elsewhere. The coefficient on $Neighbor \times (1 - Other)$ is of similar magnitude (about 13–27%) and also highly significant, indicating that spillovers are not driven solely by within-firm shifting, but also reflect a broader response among firms that are geographically close to cities implementing rationing while not otherwise exposed. At the same time, β_1 remains negative and significant, indicating that plants directly subject to mandatory rationing continue to experience a production contraction relative to the untreated benchmark plants in the same specification.[24] Overall,

[24] Two points help interpret the relatively large magnitudes in this decomposition analysis. First, the spillover and decomposition regressions focus on the subset of plants in geographically exposed areas (and exclude municipalities), so both the direct treatment coefficient and the spillover coefficients need not match the all-treated average in Table 4. Second, Appendix Tables A7 show that the spillover response is strongest at the first exposure to neighboring rationing and attenuates with repeated exposure, consistent with mechanisms based on salience/attention and experience-based updating (see, e.g., Bordalo, Gennaioli, and Shleifer, 2012; Malmendier and Nagel, 2016).

these decomposition results provide additional evidence on how market responses shape the magnitude of policy-induced production disruptions through multiple channels.

[Insert Table 9 about here]

4.3 Stock Market Reactions

In this section, we quantify stock-market reactions to the staggered implementation of mandatory power rationing in 2021. Prior work documents a positive unconditional relation between firms' production—proxied by abnormal TIR—and subsequent stock returns in the cross section (Xue, Zhang, and Zhao, 2025). Under the efficient market hypothesis (Fama, 1970), stock prices should incorporate the expected valuation consequences of rationing-induced production disruptions, which can depress expected cash flows and/or increase required returns through heightened uncertainty. In the very short run around policy announcements, discount-rate news may be particularly important, whereas over longer horizons investors may also update beliefs about real adjustments—such as within-firm reallocation and spillovers—that shape the net impact on firm value. To study these reactions, we first conduct a short-window event study around the policy date and then estimate a stacked DID specification using a six-month window before and after the announcement.

Because firms may operate multiple plants, we define a firm's policy date as the earliest date on which any of its plants becomes subject to mandatory power rationing. We drop firms whose first affected plant is located in a city without a recorded policy publication date,[25] since we cannot align event timing precisely. To ensure a six-month event window centered on the policy shock and a twelve-month pre-event window to estimate firms' factor betas, we further exclude firms listed after October 1, 2019. The final sample consists of 1,725 firms, including 1,380 ever-treated firms and 345 never-treated firms.

4.3.1 Short-window Event Study

We first conduct short-window event-study tests using an industry-benchmark return model around policy announcements (Neuhierl, Scherbina, and Schlusche, 2013; Krüger, 2015; Jia, Wang, and Xiong, 2017; Barinov, Park, and Yıldızhan, 2024). This approach helps isolate announcement effects by controlling for industry-wide shocks that may coincide with the policy news and contemporaneously affect stock prices (see, e.g., Chen, Chen, Chen, and Li, 2019). We define the industry-adjusted abnormal return as the firm's daily return net of an industry-specific benchmark:

$$AR^{Ind}_{i,k,t} = r_{i,k,t} - r_{\bar{i},k,t}, \tag{10}$$

where $r_{i,k,t}$ is the stock return of firm i in industry k on day t, and $r_{\bar{i},k,t}$ is the average return of other firms in the same CSRC Level-2 industry on day t. We then examine the distributional properties of cumulative industry-adjusted abnormal returns, CAR^{Ind}, constructed over several event windows, including $[-1, 1]$, $[-3, 3]$, $[-5, 5]$, $[-10, 10]$, and $[-21, 21]$.

[25] 16 out of 152 cities ever issued mandatory power rationing policies only have announcement month but do not have the exact announcement date as shown in Appendix Table A3.

Table 10 reports industry-adjusted CARs around the adoption of mandatory power rationing, along with t-statistics computed following Campbell, Lo, MacKinlay, and Whitelaw (1998), the minimum, median, and maximum CARs, the fraction of positive CARs, and the number of observations. Several patterns stand out. First, the average market reaction is negative and statistically significant across all event windows, indicating that investors view mandatory power rationing as adverse news for firm value. The magnitude increases monotonically as the window widens: average CAR^{Ind} declines from -0.5% over $[-1, 1]$ to -0.6% over $[-3, 3]$, -1.2% over $[-5, 5]$, -2.0% over $[-10, 10]$, and -4.8% over $[-21, 21]$, with the corresponding t-statistics significant at the 1% level. This monotonic pattern suggests that the valuation impact accumulates over time, consistent with persistent operational disruptions and/or gradual information revelation. Second, the distributional statistics point to broad-based losses: the median CAR^{Ind} is negative in every window, and only about one-third to two-fifths of observations have positive CARs, implying that the negative response is pervasive rather than driven by a small subset of firms. Finally, the estimated effects are economically meaningful: with an average market capitalization of RMB 20.8 billion as of 2020/12/31 for sample firms and a mean CAR^{Ind} of -4.8% over $[-21, 21]$, the implied decline in market capitalization is approximately RMB 1.0 billion for the average treated firm. Overall, the event-study evidence indicates a sizable and pervasive negative announcement-window market response to mandatory rationing.

[Insert Table 10 about here]

4.3.2 Stacked DID Regressions

To assess whether the stock-price response to rationing announcements is persistent beyond the immediate announcement window, we follow Chen, Hu, Xi, and Zhu (2025) and estimate a stacked DID specification using cumulative excess (or abnormal) returns measured over a [-126,125] trading-day window centered on each firm's first mandatory-rationing event date. Each cohort c is defined by a calendar day on which at least one mandatory rationing policy is issued, and a firm's event date is the earliest day on which any of its plants becomes subject to mandatory rationing. We then compare the evolution of cumulative returns for treated firms relative to control firms within the same cohort, before versus after the event date, while absorbing firm-by-cohort fixed effects and time-by-cohort fixed effects. The regression design is:

$$CER_{i,c,t} = \beta_1 Treat_{c,i} \times Post_{c,t} + \phi_{i,c} + \delta_{c,t} + X_{i,t}^{'} e + \epsilon_{c,i,t} \tag{11}$$

The dependent variable is the daily cumulative excess return, $CER_{i,c,t}$, or daily cumulative abnormal return, $CAR_{i,c,t}$, for stock i on day t in cohort c, where CAR is computed using either the CAPM or the Fama-French three-factor model. $Treat_{i,c}$ equals one if firm i operates at least one plant in a city that becomes subject to mandatory power rationing in cohort c, and $Post_{c,t}$ equals one for trading days on and after the cohort's event date. We include Firm×Cohort fixed effects, $\phi_{i,c}$, which absorb the main effect of $Treat_{i,c}$. We also include Calendar-week×Cohort fixed effects to absorb cohort-specific time-varying shocks common to all firms in a given cohort-week; as a result, the main effect $Post_{c,t}$ is subsumed by these cohort-specific time fixed effects.[26] We further control for a set

[26] Because cohort event dates fall on different weekdays, a single convention for calendar weeks (e.g., Monday–Sunday) would misalign

of daily macro-financial variables, $X_{i,t}$, including the TED spread, the implied volatility index from options on the SSE50 ETF (CVIX), and the CSI All A-share Index return (MKT). Finally, we restrict the control group to never-treated firms so that control observations are never exposed to mandatory rationing within the cohort's $[-126, 125]$ trading-day window. This restriction mitigates treatment contamination that could arise if firms treated in nearby months were used as controls while facing their own impending or contemporaneous shocks.

Table 11 reports the stacked DID estimates. Columns (1) and (2) use daily cumulative excess returns (CER) as the dependent variable and include Firm×Cohort and Year-week×Cohort fixed effects; Column (1) excludes additional controls, whereas Column (2) includes the full set of macro-financial controls. Columns (3) and (4) replace CER with cumulative abnormal returns CAR adjusted using the CAPM and Fama-French three-factor model, respectively, and otherwise adopt the same specification as Column (2). Coefficients are reported in percentage points, and standard errors clustered at the firm level are reported in parentheses.

Without adjusting for systematic risk exposures, Columns (1) and (2) show no detectable differential response in cumulative excess returns between treated and control firms. The estimated effect is economically small and statistically indistinguishable from zero, with a point estimate of -0.07%. In contrast, once we benchmark returns using standard factor models, we observe a sizable and statistically significant post-event underperformance among treated firms. Column (3) indicates that treated firms experience 2.7% (t-statistic = -3.9) lower CAPM-adjusted CAR than untreated firms over the [-126,125]-trading-day window around the policy shock. Column (4) yields a qualitatively similar but smaller estimate of -1.8% (t-statistic = 2.2) for the Fama-French three-factor-adjusted CAR, suggesting that part of the CAPM-based decline is reflects differential exposure to the size and value factors.

Taken together, relative to the short-window event-study results, the factor-adjusted stacked-DID evidence suggests modest underperformance over the twelve-month window around the event, consistent with investors partially pricing in longer-horizon consequences beyond the immediate announcement reaction.

[Insert Table 11 about here]

4.4 Real Effects of Power Rationing on Firm Investment and Operating Outcomes

In the previous sections, we show that mandatory power rationing materially disrupts plant-level production and affects firms' stock-market performance over relatively short horizons around the policy shock. We now turn to the longer-horizon real effects of this campaign-style administrative intervention on corporate investment and other operating outcomes. Even if firms can partially cushion the immediate production shock through intra-firm reallocation or preemptive output adjustments, rationing episodes may still alter longer-run plans by increasing perceived regulatory and political risk.

the cohort-specific post-period across cohorts by mechanically splitting the event date and its nearby trading days into different week bins. We therefore define the cohort-specific "calendar-week" index in seven-day blocks allowing the week to start on any weekday. Concretely, we construct seven alternative week partitions (Monday–Sunday, Tuesday–Monday, …, Sunday–Saturday) and, for each cohort, use the partition in which the cohort's event date falls into a uniquely defined seven-day interval. This ensures that the cohort-week fixed effects align with event timing while remaining purely calendar-time controls.

In particular, firms with operations in heavily rationed cities may become more hesitant to invest if the episode signals a greater likelihood of future local interventions and policy uncertainty (Julio and Yook, 2012; An, Chen, Luo, and Zhang, 2016; Gulen and Ion, 2016). We therefore examine whether experience with the 2021 mandatory rationing is followed by changes in capital expenditures and other related outcomes, including sales, employment, R&D spending, productivity, and financing decisions. The annual firm-level sample spans 2017–2024.

4.4.1 Impact on Corporate Investment

To examine whether affected firms reduce investment after the rationing episode, we estimate the following standard DID specification using annual firm-level data from 2017–2024:

$$CAPX_{i,a} = \beta_1 Treat_i \times Post_a + \gamma_i + \delta_a + \sigma_{k,a} + \eta_{l,a} + X'_{i,a-1}e + \epsilon_{i,a} \tag{12}$$

The dependent variable, $CAPX_{i,a}$, is firm i's annual capital expenditure in year a, scaled by lagged total assets and expressed in percentage points (He, Liao, and Wang, 2022). $Treat_i$ equals one if firm i operates at least one plant in a city that is subject to mandatory power rationing, and zero otherwise. $Post_a$ equals one for 2021 and subsequent years, and zero for pre-2021 years. We include firm fixed effects γ_i and year fixed effects δ_a, and further add Industry×Year fixed effects $\sigma_{k,a}$ and Province×Year fixed effects $\eta_{l,a}$ to absorb unobservable, time-varying shocks common to firms within an industry or province in a given year. Finally, we control for a standard set of firm characteristics $X_{i,a-1}$, including Tobin's Q, operating cash flow scaled by lagged total assets, log total assets, leverage, and return on assets. Following He, Liao, and Wang (2022), all controls are lagged by one year except cash flow, which is measured contemporaneously.

Table 12 reports the DID estimation results. Column (1), which includes only firm fixed effect and year fixed effects, yields a negative but statistically insignificant estimate of -0.3%. After adding the full set of controls in Column (2) to better separate the post-2021 shift from fundamental-driven investment variation, the estimate becomes statistically significant and increases in magnitude to -0.5%. The coefficient remains stable as we further absorb time-varying industry shocks and province-level shocks: -0.5% in Column (3) and -0.6% in Column (4)–(5). The implied magnitudes are economically meaningful: relative to the sample mean CAPX of 5.7%, firms with at least one plant exposed to mandatory rationing exhibit an average annual CAPX that is about 9.7% lower than that of never-exposed firms. Overall, the evidence suggests that firms reduce investment after experiencing campaign-style power rationing, highlighting an unintended longer-run consequence of the intervention.[27] Our finding also echoes the theoretical work of Fried and Lagakos (2023), who use a dynamic general-equilibrium model to show that power outages depress productivity not just contemporaneously, but persistently—by shrinking incumbent scale and discouraging entry—thereby amplifying the intervention's longer-run consequences.

[Insert Table 12 about here]

[27] In a robustness test, we exclude the policy year 2021; the estimated coefficient remains similar at -0.5% (Appendix Table A8). We also use alternative investment measures, including fixed asset growth as in Badertscher, Shroff, and White (2013), total asset growth as in Hou, Xue, and Zhang (2015), and fixed asset growth including depreciation as in Belo, Hao, Lin, Qiu, and Tong (2024). The estimated effects remain negative across these measures (Appendix Table A9).

4.4.2 Impact on Other Operating Outcomes

Lastly, we examine the effects of mandatory power rationing on a broader set of firm outcomes, including year-over-year growth in sales, employment, R&D expenditure, total factor productivity, and total liabilities scaled by lagged book equity. The sample period is 2017–2024. Table 13 reports DID estimates based on Eq.(12). Odd-numbered columns include firm and year fixed effects without additional controls, while even-numbered columns add the full set of controls and further absorb time-varying industry and province-level shocks using Year×Industry and Year×Province fixed effects. Columns (1) and (2) show a sizable and statistically significant decline in ΔSales, with estimates of -3.3% and -3.5%, respectively. Columns (3)–(8) reveal similarly negative and statistically significant effects on employment, R&D spending, and productivity, indicating that the production disruption is followed by broader and persistent deterioration in real operating performance. Finally, Columns (9) and (10) show that exposed firms reduce long-term liabilities, with coefficients of -3.2% and -4.1%, consistent with either tighter external financing capacity or a more conservative balance-sheet policy after the shock. Taken together, these results suggest that mandatory rationing not only disrupts production in the short run, but also has longer-horizon real effects on multiple dimensions of firm operations and financial policies.

[Insert Table 13 about here]

4.5 Policy follow-up

The widespread disruptions associated with the 2021 rationing wave appear to have prompted a shift in the central government's policy communication and implementation stance, emphasizing the need to avoid "one-size-fits-all" enforcement to better balance economic costs with environmental objectives. In a May 2022 *Qiushi* article, President Xi Jinping explicitly criticized campaign-style approaches to the "dual control" agenda and occurrence of blanket "power cut-offs." Consistent with this guidance, the intensity of rationing measures receded after the 2021 episode.

Importantly, this retreat did not generally take the form of explicit, city-level termination announcements. In assembling the policy timeline, we do not find public municipal documents stating that rationing ended on a specific date. At the province level, only two provinces—Fujian and Zhejiang—formally suspended "orderly electricity use," while most other localities just allowed the measures to fade out quietly.[28] This implementation pattern is informative for interpreting our results. Even when campaign-style interventions are not sustained as a stable policy regime, they can generate sizable short-run disruptions and leave longer-run effects through belief updating about policy risk and the perceived likelihood of future administrative constraints.

[28] On November 1st, 2021, the Fujian Provincial Department of Industry and Information Technology issued the Notice on the Suspension of Orderly Power Consumption. On November 7th, 2021, the Zhejiang Provincial Energy Bureau issued the Notice on the Suspension of Orderly Power-Rationing Measures.

5 Conclusion

In this paper, we study China's 2021 mandatory power-rationing campaign as a staggered policy shock and quantify its real economic consequences. Using plant-level production measured by abnormal thermal infrared radiation (ATIR)—a high-frequency, remotely sensed proxy that allows us to track operational activity for a large universe of plants and in near real time—we show that rationing leads to a sharp and economically meaningful contraction in production at directly affected plants. At the same time, firms do not remain passive: multi-plant firms shift activity toward their unaffected facilities, and production also rises at nearby untreated plants, consistent with geographically proximate adjustment and forward-looking responses around salient local interventions. Importantly, the episode also leaves a longer shadow: firms affected by the 2021 power rationing subsequently scale back investment, suggesting that campaign-style interventions can alter longer-run plans by elevating perceived policy uncertainty. Overall, our findings highlight how administrative shocks can impose large real costs while eliciting rapid market-based adaptation—an interaction that is central to understanding firm behavior in China's institutional environment.

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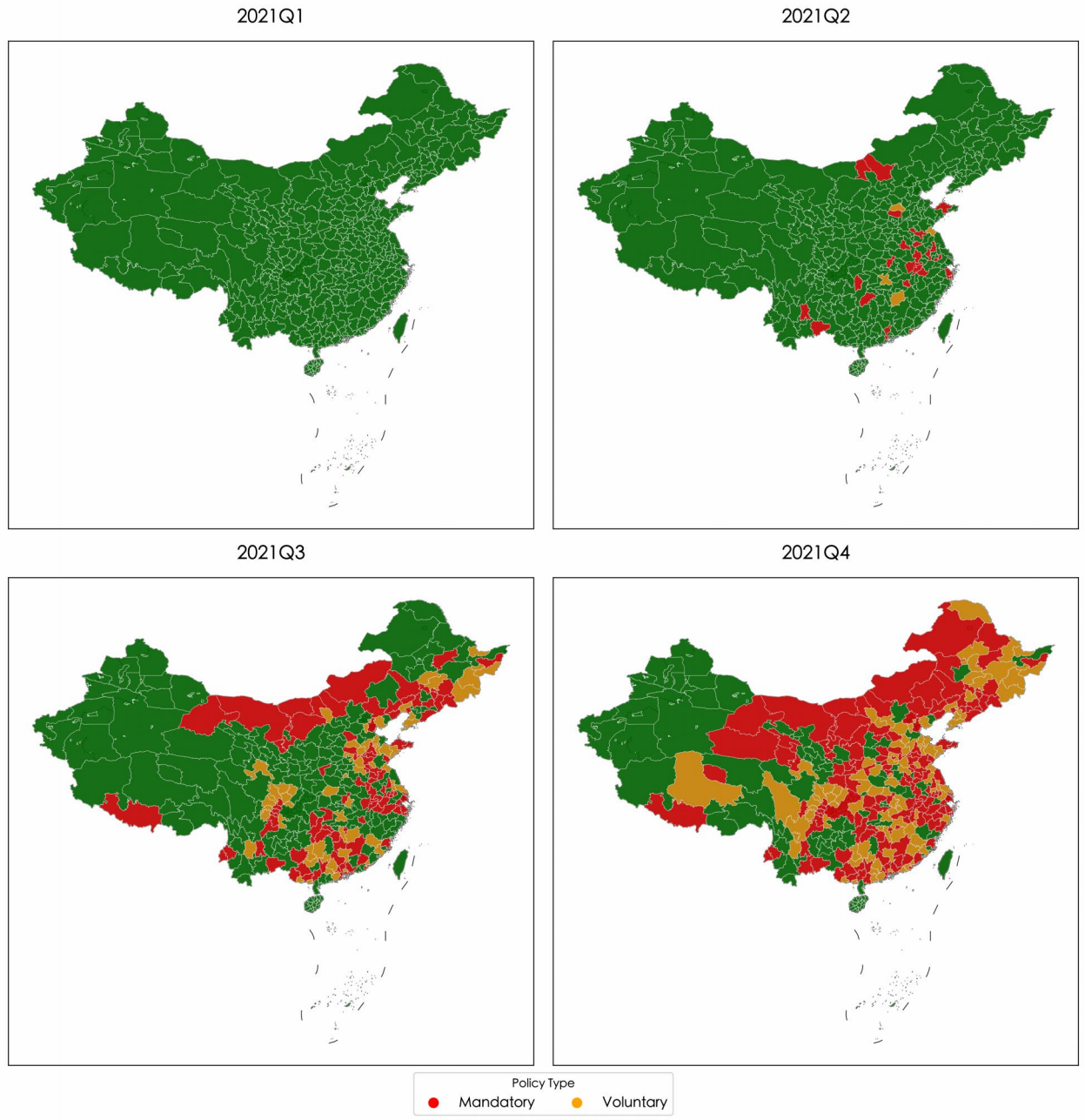


Figure 1: Staggered Adoption of City-level Power Rationing Policies in 2021

This figure visualizes the staggered adoption of city-level power rationing policies in China during 2021. Each panel maps the cumulative set of cities that had implemented a power rationing policy by the end of the corresponding quarter (2021Q1–2021Q4). Cities are classified by policy stringency: mandatory rationing (red) and voluntary rationing (orange); cities without a recorded rationing policy in that quarter are marked in green. Policy timing is defined by the quarter in which a city first issued a rationing directive. The policy dataset is hand-collected from publicly available government announcements and official releases.

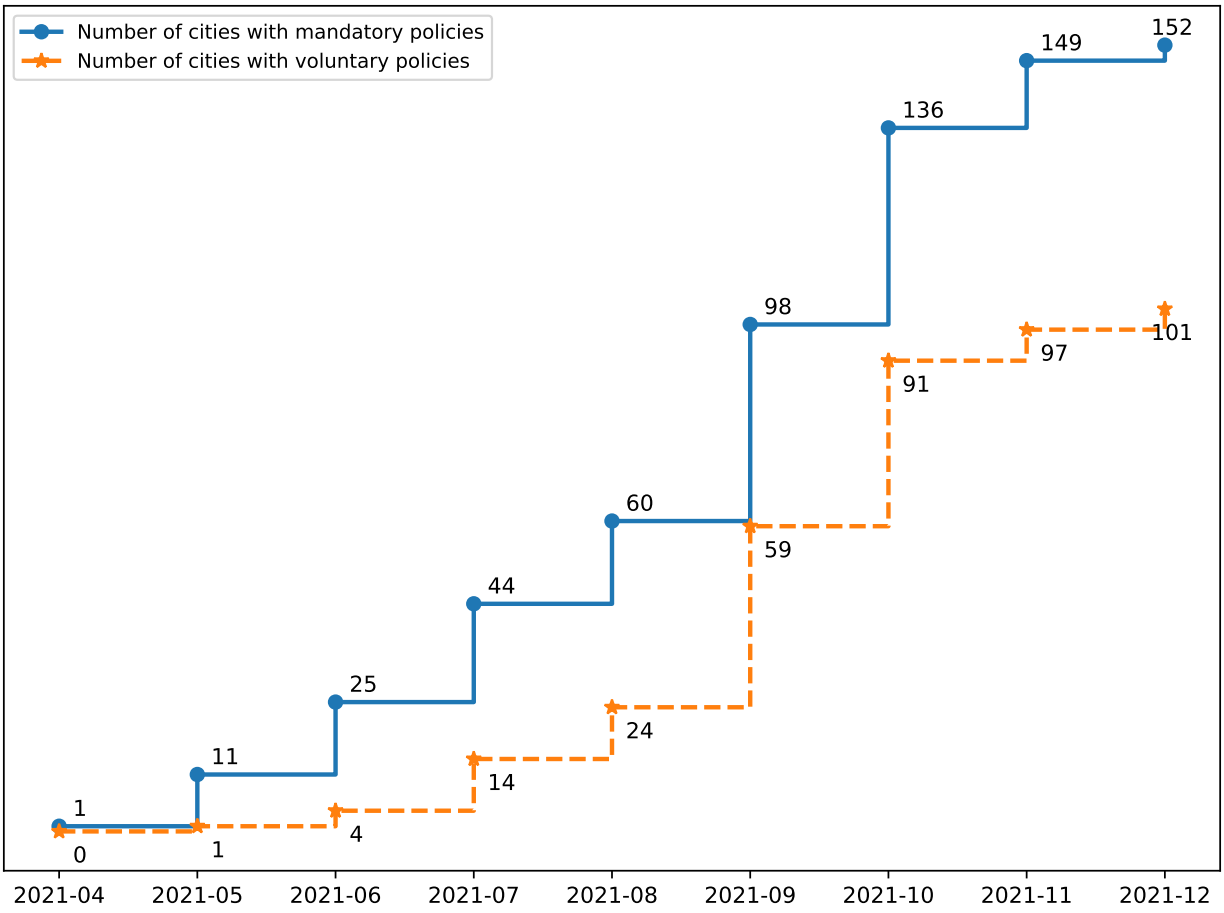


Figure 2: Cumulative Number of Cities Adopting Power Rationing Policies

This figure plots the cumulative number of Chinese cities that had implemented a power rationing policy by the end of each month from April to December 2021. The solid blue line with circles reports the cumulative count of cities adopting mandatory rationing policies, and the dashed orange line with stars reports the cumulative count adopting voluntary rationing policies. A city is counted from the month in which it first issued a rationing directive and remains counted thereafter. Policy timing and classification are based on a hand-collected dataset compiled from publicly available government announcements and official releases.

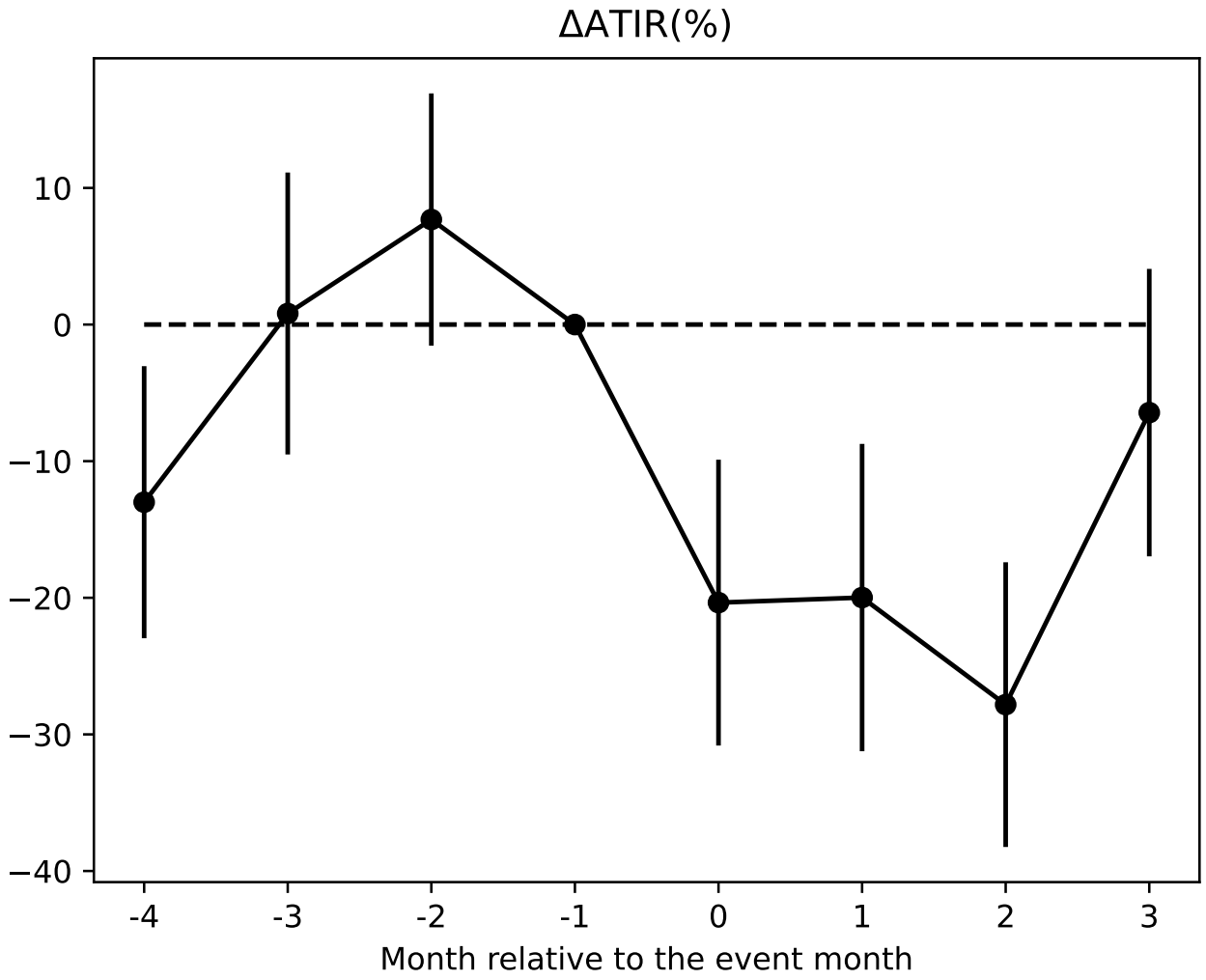


Figure 3: Dynamic Treatment Effects of the Policy Shock

This figure plots event-time estimates on $Treat_{c,p} \times \mathbb{1}_{c,\tau}$ and their confidence intervals from the stacked difference-in-differences specification in Eq. (4). The dependent variable is the year-over-year change in production-related TIR. $Treat_{c,p}$ indicates plants located in cities that adopt a mandatory power rationing policy for cohort c; control plants are located in cities not yet affected within the stacked sample. $\mathbb{1}_{c,\tau}$ equals one for the subperiod τ of cohort c and zero otherwise. The regression includes plant-by-cohort fixed effects, year-month-by-cohort fixed effects, and plant-time-varying controls (temperature, relative humidity, precipitation, sunshine duration, and new COVID-19 cases). The event window is [-4,3]-month with month 0 indicates the policy issuance month. The point estimate at the -1 month is normalized to zero. The vertical bars show 95% confidence intervals, with standard errors clustered at the plant–cohort level.

Table 1: Comparison of City Characteristics: Mandatory-policy Cities versus Other Cities

This table compares city characteristics between cities that ever implement mandatory power rationing policies and all other cities. The first two columns report group means for city-level characteristics measured in 2020, including average temperature, average relative humidity, precipitation, sunshine duration, GDP growth, log GDP per capita, the share of the secondary industry added value in GDP, energy usage (coal consumption scaled by GDP), log population scaled by total city area, the share of build-up land in total city area, total factor productivity, and new COVID-19 cases. The last column reports the difference in means between the two groups with standard errors in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Variables	Treat	Control	Difference
Temperature	14.93	14.60	0.34
			(0.54)
Humidity	68.90	68.52	0.39
			(0.31)
Precipitation	89.11	86.43	2.68
			(0.50)
Sunshine	174.33	177.18	-2.85
			(-0.43)
GDPGrowth	2.59	2.78	-0.19
			(-0.51)
PerCapitaGDP	11.00	10.90	0.10*
			(1.94)
SecondaryShare	0.38	0.38	0.00
			(0.38)
Energy	0.61	0.69	-0.08**
			(-2.15)
Population	5.77	5.63	0.14
			(1.14)
UrbanLands	2.06	1.54	0.51
			(1.53)
TFP	0.31	0.32	-0.01
			(-0.47)
Covid19	126.65	405.05	-278.40
			(-0.91)

Table 2: Comparison of Firm Characteristics

This table compares firm characteristics between manufacturing firms and other firms (Panel A), and between firms located in affected cities and firms located in cities without mandatory power rationing policies (Panel B). The first two columns report group means for firm characteristics measured as of 2020/12/31, including market capitalization, leverage, book-to-market ratio, return on assets, capital expenditure scaled by lagged total assets, and Tobin's Q. The last column reports the difference in means between the two groups with standard errors in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Comparison between manufacturing firms and rest

Variables	Manufacturing	Others	Difference
Size (RMB Billion)	14.03	13.51	0.52
			(0.57)
Leverage (%)	39.07	45.86	-6.79***
			(-9.27)
BM	0.43	0.59	-0.17***
			(-11.63)
ROA (%)	3.91	1.00	2.91***
			(8.56)
CAPX (%)	5.78	4.31	1.46***
			(7.08)
TobinQ	2.81	2.29	0.52***
			(7.31)

Panel B: Comparison between treated and control firms within manufacturing industries

Variables	Treat	Control	Difference
Size (RMB Billion)	16.99	10.63	6.36***
			(4.83)
Leverage (%)	40.40	37.97	2.43**
			(2.36)
BM	0.47	0.43	0.04**
			(2.12)
ROA (%)	4.03	4.10	-0.07
			(-0.18)
CAPX (%)	5.70	5.80	-0.10
			(-0.28)
TobinQ	2.53	2.77	-0.24*
			(-1.92)

Table 3: Summary Statistics

This table presents summary statistics for environmental variables (Panel A), stock market variables (Panel B), and firm characteristics (Panel C). The unit of observation is the plant–month in Panel A, the firm–day in Panel B, and the firm–year in Panel C. Variables in Panel A include production-related thermal infrared radiation (ATIR in Kelvin and its year-over-year change Δ ATIR, in %), temperature (*Temperature*, in °C), relative humidity (*Humidity*, in %RH), precipitation (*Precipitation*, in mm), sunshine duration (*Sunshine*, in hours), and the number of new COVID-19 infections (*Covid*19). Variables in Panel B include cumulative excess returns (*CER*, in %), cumulative abnormal returns relative to the CAPM (CAR^{CAPM}, in %) and the Fama–French three-factor model (CAR^{FF3}, in %), the CSI All A-Share Index return (*MKT*), the spread between the three-month Shanghai Interbank Offered Rate (SHIBOR) and the three-month treasury bill yield (*TED*), and the implied volatility index from options on the SSE50 ETF (*CVIX*). Variables in Panel C include capital expenditure scaled by lagged total assets (*CAPX*), Tobin's Q (*TobinQ*), cash flow from operating activities scaled by lagged total assets (*CashFlow*), log total assets (*LogAsset*), leverage (*Leverage*), return on assets (*ROA*), and year-over-year changes in sales (Δ*Sales*), employment (Δ*Employment*), R&D expenditure (Δ*R&D*), total factor productivity (Δ*TFP*), and annual change in total debt scaled by lagged book value (Δ*LongDebt*).

Variable	N	Mean	STD	P10	P25	Median	P75	P90
Panel A: Environmental variables								
ATIR	147721	1.10	1.44	-0.44	0.12	0.83	1.88	3.08
Δ*ATIR*	147721	27.46	247.95	-98.77	-34.18	2.88	50.92	155.90
Temperature	143197	12.98	9.58	1.10	5.80	12.50	21.50	26.60
Humidity	143197	68.41	11.81	51.00	61.00	71.00	77.00	82.00
Precipitation	143197	77.50	85.86	3.50	15.00	49.50	114.30	188.30
Sunshine	143197	155.32	51.53	85.10	122.70	155.90	190.20	214.90
Covid19	146058	28.33	111.81	0.00	0.00	0.00	3.00	52.00
Panel B: Stock market variables								
CER	458111	12.52	32.27	-20.88	-7.83	5.54	26.61	56.67
CAR^{CAPM}	458111	5.65	40.83	-42.41	-17.41	2.13	26.96	59.01
CAR^{FF3}	458111	-1.91	35.52	-44.48	-20.57	-1.99	15.51	41.31
MKT	458111	0.00	1.06	-1.34	-0.56	0.05	0.67	1.27
TED	458111	0.46	0.18	0.20	0.29	0.49	0.61	0.67
CVIX	458111	20.28	2.03	18.02	18.87	19.91	21.36	23.20
Panel C: Firm-level characteristics								
CAPX	14450	5.69	5.59	0.86	1.88	3.93	7.46	12.76
TobinQ	14250	2.38	1.57	1.08	1.37	1.88	2.82	4.29
CashFlow	14450	0.06	0.07	-0.02	0.02	0.05	0.10	0.15
LogAsset	14450	22.28	1.21	20.86	21.43	22.13	22.97	23.95
Leverage	14450	0.40	0.18	0.16	0.26	0.40	0.53	0.64
ROA	14450	4.04	6.09	-1.37	1.45	4.06	7.23	10.72
ΔSales	14449	12.83	29.07	-16.37	-3.41	8.91	23.70	44.09
ΔEmployment	14447	5.28	19.15	-11.39	-4.05	1.90	10.66	24.34
ΔR&D	14184	18.34	40.17	-16.17	-2.34	11.20	29.15	55.21
ΔTFP	14299	0.68	2.59	-2.12	-0.77	0.58	1.99	3.68
ΔLongDebt	14450	10.55	27.84	-12.43	-2.30	5.48	18.37	39.36

Table 4: Impacts of Mandatory Power Rationing on Firm Production

This table reports stacked difference-in-differences estimates of the effect of cities' adoption of mandatory power rationing policies on plant production. The stacked DID is estimated over an event window of [-4,3] months around the policy announcement month. The dependent variable, $\Delta ATIR$, is the year-over-year change in production-related thermal infrared radiation ($ATIR$). Cohorts are defined by the month of policy issuance. $Treat$ equals one for a plant located in a treated city for a given cohort, and $Post$ equals one for months on and after the policy announcement. Control variables include temperature, relative humidity, precipitation, sunshine duration, and new COVID-19 cases. All specifications include Plant×Cohort and Year-month×Cohort fixed effects. Column (3) adds Year-month×Industry fixed effects, Column (4) adds Year-month×Province fixed effects, and (5) include both sets of fixed effects. t-statistics based on standard errors clustered at the plant-cohort level are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent: $\Delta ATIR$	(1)	(2)	(3)	(4)	(5)
$Treat \times Post$	-19.09***	-15.42***	-15.60***	-16.74***	-17.16***
	(-7.45)	(-5.86)	(-5.88)	(-5.39)	(-5.49)
Temperature		3.17***	3.01***	7.79***	7.55***
		(12.27)	(11.46)	(9.11)	(8.72)
Humidity		1.87***	1.85***	2.49***	2.46***
		(18.60)	(18.21)	(12.29)	(12.09)
Precipitation		0.08***	0.07***	0.02**	0.02*
		(11.78)	(11.51)	(2.23)	(1.91)
Sunshine		0.16***	0.16***	0.03	0.03
		(8.85)	(8.57)	(1.11)	(1.21)
Covid19		0.00	0.01	-0.01	-0.01
		(0.30)	(0.71)	(-1.03)	(-0.45)
Plant×Cohort FE	YES	YES	YES	YES	YES
Year-month×Cohort FE	YES	YES	YES	YES	YES
Year-month×Industry FE	NO	NO	YES	NO	YES
Year-month×Province FE	NO	NO	NO	YES	YES
Observations	474564	454846	454845	454846	454845
R-square	0.19	0.19	0.20	0.23	0.24

Table 5: Heterogeneities in Impacts of Mandatory Power Rationing on Firm Production

This table reports stacked difference-in-differences estimates of the effect of cities' adoption of mandatory power rationing policies on plant production with group indicators. The stacked DID is estimated over an event window of [-4,3] months around the policy announcement month. The dependent variable, $\Delta ATIR$, is the year-over-year change in production-related thermal infrared radiation ($ATIR$). Cohorts are defined by the month of policy issuance. $Treat$ equals one for a plant located in a treated city for a given cohort, and $Post$ equals one for months on and after the policy announcement. EI (Non-EI) equals one if a plant operates in an energy-intensive (non-energy-intensive) industry, and zero otherwise. HP (Non-HP) is equals one if a plant operates in a heavy polluting (non-heavy polluting) industry, and zero otherwise. SOE (POE) equals one if a plant is a state-owned (private-owned) enterprise, and zero otherwise. Control variables include temperature, relative humidity, precipitation, sunshine duration, and new COVID-19 cases. Columns (1)–(3) report specifications with Plant×Cohort and Year-month×Cohort fixed effects but no controls. Columns (4)–(6) add the full set of controls and include Plant×Cohort, Year-month×Cohort, Year-month×Industry, and Year-month×Province fixed effects. t-statistics based on standard errors clustered at the plant-cohort level are reported in parentheses. We perform Wald Chi-squared test to compare the effects of mandatory power rationing policies across different groups, and report the p-value of the test in the last row. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent: $\Delta ATIR$	(1)	(2)	(3)	(4)	(5)	(6)
$Treat \times Post \times$ EI	-15.23***			-16.89***		
	(-2.81)			(-2.76)		
$Treat \times Post \times$ Non-EI	-20.12***			-17.24***		
	(-7.21)			(-5.15)		
$Treat \times Post \times$ HP		-14.22***			-16.58***	
		(-3.78)			(-3.83)	
$Treat \times Post \times$ Non-HP		-22.59***			-17.60***	
		(-7.09)			(-4.67)	
$Treat \times Post \times$ SOE			-11.52**			-11.88**
			(-2.35)			(-2.26)
$Treat \times Post \times$ POE			-22.12***			-19.17***
			(-7.81)			(-5.68)
Temperature				7.55***	7.55***	7.56***
				(8.72)	(8.72)	(8.73)
Humidity				2.46***	2.46***	2.47***
				(12.09)	(12.09)	(12.09)
Precipitation				0.02*	0.02*	0.02*
				(1.91)	(1.91)	(1.92)
Sunshine				0.03	0.03	0.03
				(1.21)	(1.20)	(1.20)
Covid19				-0.01	-0.01	-0.01
				(-0.45)	(-0.45)	(-0.46)
Plant×Cohort FE	YES	YES	YES	YES	YES	YES
Year-month×Cohort FE	YES	YES	YES	YES	YES	YES
Year-month×Industry FE	NO	NO	NO	YES	YES	YES
Year-month×Province FE	NO	NO	NO	YES	YES	YES
Observations	474564	474564	474564	454845	454845	454845
R-square	0.19	0.19	0.19	0.24	0.24	0.24
p-value (β_1=β_2)	0.00***	0.07*	0.05*	0.96	0.84	0.19

Table 6: Impacts of Mandatory Power Rationing on Firm Production: Additional Results

This table reports stacked difference-in-differences estimates of the effect of cities' adoption of mandatory power rationing policies on plant production under alternative specifications. Columns (1)–(2) report results using shorter and longer event windows ([-3,2] and [-6,5] months) around the policy announcement month, Column (3) uses voluntary power rationing policies as the event, Column (4) restricts the control group to never-treated plants, Column (5) excludes observations from the four municipalities, and Column (6) uses province-level mandatory power rationing policies. The dependent variable, $\Delta ATIR$, is the year-over-year change in production-related thermal infrared radiation ($ATIR$). Cohorts are defined by the month of policy issuance. $Treat$ equals one for a plant located in a treated city for a given cohort, and $Post$ equals one for months on and after the policy announcement. Control variables include temperature, relative humidity, precipitation, sunshine duration, and new COVID-19 cases. All specifications except the province-level analysis in Column (6) include Plant×Cohort, Year-month×Cohort, Year-month×Industry, and Year-month×Province fixed effects. t-statistics based on standard errors clustered at the plant-cohort level are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent: $\Delta ATIR$	Short	Long	Voluntary	Never-treated	W/o municipality	Province
	(1)	(2)	(3)	(4)	(5)	(6)
$Treat \times Post$	-26.83***	-15.20***	-1.67	-16.18***	-16.85***	-5.77*
	(-7.18)	(-5.68)	(-0.31)	(-4.94)	(-5.36)	(-1.94)
Temperature	8.64***	8.25***	9.90***	4.89***	7.64***	2.07***
	(7.89)	(12.43)	(6.86)	(5.08)	(8.80)	(6.64)
Humidity	2.00***	2.80***	3.52***	2.60***	2.47***	2.17***
	(7.90)	(17.64)	(10.62)	(11.37)	(12.11)	(16.34)
Precipitation	0.03***	0.02***	-0.06***	-0.03***	0.02**	0.10***
	(2.64)	(2.59)	(-3.65)	(-2.69)	(1.92)	(13.36)
Sunshine	0.04	-0.02	-0.10**	0.01	0.03	0.30***
	(1.15)	(-0.75)	(-2.21)	(0.18)	(1.16)	(13.48)
Covid19	-0.05**	0.01	-0.32***	0.02	-0.01	0.01
	(-2.24)	(0.64)	(-3.03)	(1.21)	(-0.41)	(0.80)
Plant×Cohort FE	YES	YES	YES	YES	YES	YES
Year-month×Cohort FE	YES	YES	YES	YES	YES	YES
Year-month×Industry FE	YES	YES	YES	YES	YES	YES
Year-month×Province FE	YES	YES	YES	YES	YES	NO
Observations	338242	689137	153162	292234	401588	323995
R-square	0.27	0.20	0.25	0.22	0.23	0.20

Table 7: Firms' Response: Intra-firm Production Reallocation

This table reports difference-in-differences estimates of the effect of cities' adoption of mandatory power rationing policies on affected firms' other plants located in unaffected cities. The stacked DID is estimated over an event window of [-4,3] months around the policy announcement month. The dependent variable, $\Delta ATIR$, is the year-over-year change in production-related thermal infrared radiation ($ATIR$). Cohorts are defined by the month of policy issuance. $Treat$ equals one for a plant located in a treated city for a given cohort, and $Post$ equals one for months on and after the policy announcement. $Other$ equals one for a plant that is not subject to the mandatory power rationing policy, but whose parent firm had at least one other plant located in an affected city in the previous month. Control variables include temperature, relative humidity, precipitation, sunshine duration, and new COVID-19 cases. All specifications include Plant×Cohort and Year-month×Cohort fixed effects. Column (3) adds Year-month×Industry fixed effects, Column (4) adds Year-month×Province fixed effects, and (5) include both sets of fixed effects. t-statistics based on standard errors clustered at the plant-cohort level are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent: $\Delta ATIR$	(1)	(2)	(3)	(4)	(5)
$Treat \times Post$	-15.17***	-11.81***	-12.02***	-14.11***	-14.48***
	(-5.28)	(-4.02)	(-4.05)	(-4.13)	(-4.21)
$Other$	4.18***	4.56***	5.37***	3.59**	3.69***
	(3.01)	(3.22)	(3.66)	(2.57)	(2.54)
Temperature		2.93***	2.79***	7.36***	7.09***
		(10.54)	(9.89)	(7.95)	(7.56)
Humidity		1.71***	1.73***	1.92***	1.90***
		(15.58)	(15.59)	(8.80)	(8.66)
Precipitation		0.07***	0.07***	0.02	0.01
		(10.27)	(10.35)	(1.51)	(1.10)
Sunshine		0.15***	0.16***	0.00	0.00
		(7.55)	(7.76)	(0.03)	(0.15)
Covid19		0.01	0.02	0.01	0.02
		(1.07)	(1.50)	(0.45)	(0.98)
Plant×Cohort FE	YES	YES	YES	YES	YES
Year-month×Cohort FE	YES	YES	YES	YES	YES
Year-month×Industry FE	NO	NO	YES	NO	YES
Year-month×Province FE	NO	NO	NO	YES	YES
Observations	399541	382286	382285	382286	382285
R-square	0.19	0.19	0.20	0.23	0.24

Table 8: Firms' Response: Spillover Effects

This table reports difference-in-differences estimates of the spillover effect of cities' adoption of mandatory power rationing policies on neighboring but unaffected plants. The stacked DID is estimated over an event window of [-4,3] months around the policy announcement month. The dependent variable, $\Delta ATIR$, is the year-over-year change in production-related thermal infrared radiation ($ATIR$). Cohorts are defined by the month of policy issuance. $Treat$ equals one for a plant located in a treated city for a given cohort, and $Post$ equals one for months on and after the policy announcement. $Neighbor$ equals one if the plant's city shares a border with a city that implemented the mandatory policy in the previous month, while the plant's own city remained untreated in that month. Control variables include temperature, relative humidity, precipitation, sunshine duration, and new COVID-19 cases. All specifications include Plant×Cohort and Year-month×Cohort fixed effects. Column (3) adds Year-month×Industry fixed effects, Column (4) adds Year-month×Province fixed effects, and (5) include both sets of fixed effects. t-statistics based on standard errors clustered at the plant-cohort level are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent: $\Delta ATIR$	(1)	(2)	(3)	(4)	(5)
$Treat \times Post$	-12.82***	-10.25***	-10.03***	-13.23***	-13.35***
	(-4.45)	(-3.46)	(-3.36)	(-4.18)	(-4.18)
$Neighbor$	22.78***	24.38***	25.77***	12.47***	12.57***
	(13.64)	(14.44)	(15.18)	(5.43)	(5.39)
Temperature		1.91***	1.68***	7.76***	7.60***
		(7.11)	(6.14)	(9.07)	(8.75)
Humidity		1.61***	1.62***	2.46***	2.44***
		(15.15)	(15.02)	(12.09)	(11.92)
Precipitation		0.05***	0.05***	0.03***	0.02**
		(7.18)	(6.92)	(2.72)	(2.39)
Sunshine		0.14***	0.14***	0.03	0.03
		(7.24)	(7.32)	(1.10)	(1.16)
Covid19		-0.00	0.00	-0.01	-0.01
		(-0.27)	(0.09)	(-0.98)	(-0.36)
Plant×Cohort FE	YES	YES	YES	YES	YES
Year-month×Cohort FE	YES	YES	YES	YES	YES
Year-month×Industry FE	NO	NO	YES	NO	YES
Year-month×Province FE	NO	NO	NO	YES	YES
Observations	421307	401589	401588	401589	401588
R-square	0.19	0.19	0.20	0.22	0.23

Table 9: Firms' Response: Decomposition of Spillover Effects

This table reports difference-in-differences estimates of the spillover effect of cities' adoption of mandatory power rationing policies on neighboring but unaffected plants, decomposing the spillovers into a reallocation-related component and an other-unaffected-firm component. The stacked DID is estimated over an event window of [-4,3] months around the policy announcement month. The dependent variable, $\Delta ATIR$, is the year-over-year change in production-related thermal infrared radiation ($ATIR$). Cohorts are defined by the month of policy issuance. $Treat$ equals one for a plant located in a treated city for a given cohort, and $Post$ equals one for months on and after the policy announcement. $Other$ equals one for a plant that is not subject to the mandatory power rationing policy, but whose parent firm had at least one other plant located in an affected city in the previous month. $Neighbor$ equals one if the plant's city shares a border with a city that implemented the mandatory policy in the previous month, while the plant's own city remained untreated in that month. Control variables include temperature, relative humidity, precipitation, sunshine duration, and new COVID-19 cases. All specifications include Plant×Cohort and Year-month×Cohort fixed effects. Column (3) adds Year-month×Industry fixed effects, Column (4) adds Year-month×Province fixed effects, and (5) include both sets of fixed effects. t-statistics based on standard errors clustered at the plant-cohort level are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent: $\Delta ATIR$	(1)	(2)	(3)	(4)	(5)
$Treat \times Post$	-12.82***	-10.25***	-10.02***	-13.23***	-13.35***
	(-4.44)	(-3.46)	(-3.36)	(-4.18)	(-4.18)
$Neighbor \times Other$	21.03***	24.35***	23.86***	10.67***	9.26***
	(7.92)	(8.95)	(8.82)	(3.49)	(3.00)
$Neighbor \times (1 - Other)$	23.77***	24.40***	26.85***	13.44***	14.35***
	(11.76)	(12.00)	(13.08)	(5.25)	(5.51)
Temperature		1.91***	1.68***	7.74***	7.58***
		(7.11)	(6.14)	(9.06)	(8.73)
Humidity		1.61***	1.62***	2.46***	2.44***
		(15.15)	(15.02)	(12.08)	(11.91)
Precipitation		0.05***	0.05***	0.03***	0.02**
		(7.18)	(6.91)	(2.74)	(2.42)
Sunshine		0.14***	0.14***	0.03	0.03
		(7.24)	(7.32)	(1.10)	(1.15)
Covid19		-0.00	0.00	-0.01	-0.01
		(-0.27)	(0.09)	(-0.98)	(-0.36)
		(4.034)	(4.110)	(1.747)	(2.357)
Plant×Cohort FE	YES	YES	YES	YES	YES
Year-month×Cohort FE	YES	YES	YES	YES	YES
Year-month×Industry FE	NO	NO	YES	NO	YES
Year-month×Province FE	NO	NO	NO	YES	YES
Observations	421307	401589	401588	401589	401588
R-square	0.19	0.19	0.20	0.22	0.23

Table 10: Stock Market Reactions: Event Study

This table reports event-study results around the adoption of mandatory power rationing policies. It presents the average industry-adjusted cumulative abnormal return (CAR^{Ind}) different event windows, along with the corresponding t-statistics, minimum, median, maximum, the fraction of positive event CARs, and the number of observations. For firm i in industry k on day t, the abnormal return is $AR^{Ind}_{i,k,t} = r_{i,k,t} - r_{\bar{i},k,t}$ and $CAR^{Ind}_{i,[-\tau,\tau]} = \sum_{t=-\tau}^{\tau} AR^{Ind}_{i,t}$, where $r_{\bar{i},k,t}$ is the average return of other firms in firm i's industry k. t-statistics are computed following Campbell, Lo, MacKinlay, and Whitelaw (1998). *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Window	Mean	t-stat	Min	Median	Max	Frac (CAR>0)	N
CAR^{Ind}[-1,1]	-0.45***	-3.43	-11.96	-0.86	16.13	39.74	1374
CAR^{Ind}[-3,3]	-0.64***	-3.12	-18.89	-1.21	26.74	41.30	1373
CAR^{Ind}[-5,5]	-1.17***	-4.72	-22.01	-1.91	30.61	38.37	1371
CAR^{Ind}[-10,10]	-1.98***	-5.67	-29.43	-3.15	41.82	37.57	1368
CAR^{Ind}[-21,21]	-4.78***	-9.00	-46.58	-6.76	65.42	32.99	1358

Table 11: Stock Market Reactions: DID Estimation

This table reports difference-in-differences estimates of stock-market reactions to mandatory power rationing policies. Each event date on which at least one power rationing policy is issued defines a cohort. A firm's event date is the earliest date on which any of its plants becomes subject to a mandatory power rationing policy. The dependent variable is cumulative excess return (CER) or cumulative abnormal returns (CAR), computed over the [-126,125]-trading-day window around each announcement day. Columns (1)–(2) report the results for CER, Column (3) reports CAPM-adjusted CAR, and Column (4) reports Fama-French three-factor-adjusted CAR. Factor model betas are estimated over a 252-trading-day window prior to the [-126,125] window, requiring at least 150 observations. $Treat$ equals one for a firm whose event date occurs in cohort c; $Post$ equals one for trading days on and after that event date. Control variables include the CSI All A-Share Index return (MKT), the spread between the three-month Shanghai Interbank Offered Rate (SHIBOR) and the three-month treasury bill yield (TED), and the implied volatility index from options on the SSE50 ETF ($CVIX$). All specifications include Firm×Cohort and Calendar-week×Cohort fixed effects, where calendar weeks are defined as cohort-specific seven-day bins aligned to the event weekday. t-statistics based on standard errors clustered at the firm-cohort level are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent:	CER		CAR^{CAPM}	CAR^{FF3}
	(1)	(2)	(3)	(4)
$Treat \times Post$	-0.07	-0.07	-2.72***	-1.76**
	(-0.09)	(-0.10)	(-2.95)	(-2.15)
Mkt		0.34***	-0.07***	-0.02***
		(184.53)	(-42.64)	(-13.17)
TED		-1.07***	-1.52***	-0.27***
		(-22.98)	(-32.85)	(-6.02)
CVIX		-0.40***	-0.00	0.01***
		(-121.46)	(-0.29)	(3.52)
Firm×Cohort FE	YES	YES	YES	YES
Calendar-week×Cohort FE	YES	YES	YES	YES
Observations	4963392	4963392	4963392	4963392
R-square	0.71	0.71	0.72	0.69

Table 12: Impacts of Mandatory Power Rationing on Firm Investment

This table reports difference-in-differences estimates of the effect of mandatory power-rationing policies on firms' investment. The dependent variable, $CAPX$, is annual capital expenditures scaled by lagged total assets. $Treat$ equals one for a firm with at least one plant located in a treated city, and $Post$ equals one for 2021 and subsequent years. Control variables include Tobin's Q, cash flow from operations scaled by lagged total assets, log total assets, leverage, and return on assets. Columns (1)–(2) include Firm and Year fixed effects, Column (3) adds Year×Industry fixed effects, Column (4) adds Year×Province fixed effects and Column (5) adds both Year×Industry and Year×Province fixed effects. *t*-statistics based on standard errors clustered by firm are reported in parentheses. The sample period is 2017–2024. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent: $CAPX$	(1)	(2)	(3)	(4)	(5)
$Treat \times Post$	-0.34	-0.52**	-0.50**	-0.55**	-0.55**
	(-1.40)	(-2.14)	(-2.08)	(-2.23)	(-2.27)
TobinQ		0.59***	0.57***	0.59***	0.57***
		(9.86)	(9.42)	(10.06)	(9.60)
CashFlow		1.58*	1.61*	1.75*	1.74*
		(1.72)	(1.73)	(1.92)	(1.88)
LogAsset		-1.31***	-1.34***	-1.30***	-1.33***
		(-5.53)	(-5.54)	(-5.70)	(-5.74)
Leverage		-2.09***	-2.17***	-2.07***	-2.17***
		(-2.91)	(-2.96)	(-2.96)	(-3.06)
ROA		0.11***	0.10***	0.11***	0.10***
		(10.74)	(10.11)	(10.90)	(10.25)
Firm FE	YES	YES	YES	YES	YES
Year FE	YES	YES	NO	NO	NO
Year×Industry FE	NO	NO	YES	NO	YES
Year×Province FE	NO	NO	NO	YES	YES
Observations	14450	14250	14250	14250	14250
R-square	0.54	0.58	0.59	0.58	0.60

Table 13: Impacts of Mandatory Power Rationing Policies on Other Firm Metrics

This table reports difference-in-differences estimates of the effect of mandatory power rationing policies on other firm metrics. The dependent variables are year-over-year changes in sales, employment, R&D expenditure, total factor productivity, and total liabilities scaled by lagged book equity. *Treat* equals one for a firm with at least one plant located in a treated city, and *Post* equals one for 2021 and subsequent years. Control variables include Tobin's Q, cash flow from operations scaled by lagged total assets, log total assets, leverage, and return on assets. Odd-numbered columns report specifications with Firm and Year fixed effects but no controls. Even-numbered columns add the full set of controls and include Firm, Year×Industry, and Year×Province fixed effects. *t*-statistics based on standard errors clustered by firm are reported in parentheses. The sample period is 2017–2024. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent:	ΔSales		ΔEmployment		ΔR&D		ΔTFP		ΔLongDebt	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treat×Post	-3.32**	-3.52***	-1.63**	-1.72**	-4.38**	-5.11***	-0.20*	-0.19	-3.22**	-4.14***
	(-2.58)	(-2.61)	(-2.01)	(-2.08)	(-2.23)	(-2.63)	(-1.78)	(-1.62)	(-2.53)	(-2.92)
TobinQ		2.31***		1.76***		2.11***		0.14***		1.84***
		(5.65)		(7.63)		(4.23)		(4.28)		(5.81)
CashFlow		52.44***		7.98*		38.53***		3.20***		-9.58
		(7.19)		(1.93)		(4.17)		(5.48)		(-1.36)
LogAsset		-14.51***		-9.60***		-14.62***		-1.53***		-15.53***
		(-10.43)		(-10.75)		(-7.27)		(-13.85)		(-9.36)
Leverage		-2.17		-16.45***		8.84		-0.25		-53.19***
		(-0.54)		(-5.64)		(1.40)		(-0.72)		(-11.97)
ROA		-0.62***		0.34***		0.61***		-0.10***		0.32***
		(-7.74)		(6.96)		(6.12)		(-12.00)		(4.59)
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO
Industry×Year FE	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Province×Year FE	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Observations	14449	14250	14447	14247	14180	13986	14299	14110	14450	14250
R-square	0.25	0.34	0.22	0.32	0.20	0.27	0.21	0.31	0.25	0.36

Appendix

Table A1: Determinants of Production-related Thermal Infrared Radiation

This table reports OLS estimates of the determinants of production-related thermal infrared radiation ($ATIR$) at the city level in 2020. The dependent variable is $ATIR$. Explanatory variables include average temperature, average relative humidity, precipitation, sunshine duration, GDP growth, log GDP per capita, the share of the secondary industry added value in GDP, energy usage (coal consumption scaled by GDP), log population scaled by total city area, the share of build-up land in total city area, total factor productivity, and new COVID-19 cases. t-statistics based on heteroscedasticity-consistent standard errors are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependents: $ATIR$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Temperature	-1.36												2.97**
	(-1.41)												(2.58)
Humidity		-2.38***											-2.16***
		(-3.99)											(-2.78)
Precipitation			-0.39***										-0.09
			(-3.74)										(-0.69)
Sunshine				0.13									-0.09
				(1.61)									(-0.71)
GDPGrowth					1.20								-3.56**
					(0.67)								(-2.17)
PerCapitaGDP						0.93							-3.90
						(0.11)							(-0.23)
SecondaryShare							46.97						-68.41
							(1.15)						(-1.22)
Energy								15.96					-30.70
								(0.65)					(-1.58)
Population									-2.35				4.25
									(-0.37)				(0.59)
UrbanLands										1.27			0.27
										(0.75)			(0.09)
TFP											-115.94**		46.77
											(-2.34)		(1.14)
Covid19^{Annual}												0.00	-0.02
												(0.26)	(-1.20)
Observations	312	312	312	312	297	295	297	297	291	271	327	309	255
R-square	0.01	0.11	0.05	0.01	0.00	0.00	0.00	0.00	0.00	0.00	0.03	0.00	0.11

Table A2: TIR Sample Data Filters

This table summarizes the step-by-step sample filters used to construct our final sample of manufacturing firms from the original Alt Data Tech thermal infrared radiation (TIR) dataset for publicly listed firms.

Filters	Remaining Firms	Remaining Plants
Firms in Alt Data Tech TIR Database	2586	20793
Less: Non-manufacturing firms	2562	20769
Less: Stocks of non Shanghai and Shenzhen A-shares	2527	20734
Less: Stocks listed during the sample period	2346	20140
Less: Stocks delisted during the sample period	2329	20014
Less: Stocks in ST, *ST, PT status during the sample period	2192	18758
Less: Observations labeled as non-plant sites	1933	10007
Less: Observations from Taiwan Province or counties	1933	9939
Final sample	1933	9939

Table A3: List of Cities that issued mandatory power rationing policies

This table lists all cities that issued a mandatory power rationing policy in 2021. For each city, the table reports the policy announcement date when available; "Unknown" indicates that only the issuance month is known and the exact date is unavailable.

April, 2021 (N=1)	
Hebei	Handan, Unknown
May, 2021 (N=10)	
Anhui	Chizhou, 2021-05-12
Anhui	Tongling, 2021-05-17
Anhui	Fuyang, 2021-05-27
Guangdong	Guangzhou, 2021-05-10
Guangdong	Zhongshan, 2021-05-27
Guangdong	Shantou, 2021-09-21
Hunan	Xiangxi, 2021-05-24
Jiangsu	Nanjing, 2021-05-26
Jiangsu	Xuzhou, 2021-05-31
Yunnan	Wenshan, 2021-05-25
June, 2021 (N=14)	
Anhui	Bengbu, 2021-06-07
Anhui	Hefei, 2021-06-10
Anhui	Huangshan, 2021-06-24
Anhui	Anqing, Unknown
Hubei	Xiaogan, 2021-06-11
Hunan	Shaoyang, 2021-06-30
Jiangsu	Wuxi, 2021-06-10
Jiangsu	Yangzhou, 2021-06-17
Jiangxi	Nanchang, 2021-06-04
Inner Mongolia	Ulanqab, 2021-06-15
Inner Mongolia	Baotou, Unknown
Shandong	Yantai, Unknown
Yunnan	Kunming, 2021-06-04
Zhejiang	Ningbo, 2021-06-18
July, 2021 (N=19)	
Anhui	Wuhu, 2021-07-15
Gansu	Jiuquan, 2021-07-21
Guangxi	Hechi, 2021-07-13
Guangxi	Nanning, 2021-07-28
Guangxi	Wuzhou, 2021-07-28
Guangxi	Yulin, 2021-07-29
Guangxi	Qinzhou, 2021-07-30
Hebei	Chengde, 2021-07-05
Hebei	Shijiazhuang, 2021-07-14
Hunan	Hengyang, 2021-07-15
Hunan	Loudi, 2021-07-15
Hunan	Chenzhou, 2021-07-17

July, 2021 (N=19)	
Jiangsu	Suqian, 2021-07-13
Jiangxi	Ganzhou, 2021-07-15
Jiangxi	Pingxiang, 2021-07-16
Inner Mongolia	Alxa, 2021-07-19
Shanghai	Shanghai, 2021-07-21
Sichuan	Yibin, 2021-07-13
Zhejiang	Hangzhou, 2021-07-07
August, 2021 (N=16)	
Guangxi	Chongzuo, 2021-08-09
Henan	Sanmenxia, 2021-08-09
Hunan	Huaihua, 2021-08-03
Inner Mongolia	Bayan Nur, 2021-08-06
Inner Mongolia	Ordos, 2021-08-08
Inner Mongolia	Wuhai, 2021-08-16
Ningxia	Shizuishan, 2021-08-22
Ningxia	Wuzhong, 2021-08-22
Ningxia	Zhongwei, 2021-08-22
Sichuan	Suining, 2021-08-03
Sichuan	Neijiang, 2021-08-04
Sichuan	Ziyang, 2021-08-04
Sichuan	Zigong, 2021-08-06
Yunnan	Dehong, 2021-08-04
Yunnan	Baoshan, 2021-08-26
Yunnan	Zhaotong, Unknown
September, 2021 (N=38)	
Anhui	Huainan, 2021-09-23
Anhui	Xuancheng, 2021-09-23
Anhui	Suzhou, 2021-09-30
Fujian	Fuzhou, 2021-09-28
Guangdong	Qingyuan, 2021-09-03
Guangdong	Meizhou, 2021-09-18
Guangdong	Huizhou, 2021-09-22
Guangdong	Zhuhai, 2021-09-22
Guangdong	Foshan, 2021-09-29
Guangdong	Shenzhen, 2021-09-29
Heilongjiang	Suihua, 2021-09-28
Heilongjiang	Shuangyashan, 2021-09-30
Henan	Puyang, 2021-09-27
Hunan	Changde, 2021-09-23
Jiangsu	Changzhou, 2021-09-23

September, 2021 (N=38)	
Jilin	Baishan, 2021-09-13
Jilin	Liaoyuan, 2021-09-24
Jilin	Jilin, 2021-09-26
Jilin	Siping, 2021-09-27
Jilin	Tonghua, 2021-09-28
Liaoning	Fushun, 2021-09-10
Liaoning	Huludao, 2021-09-25
Liaoning	Zhaoyang, 2021-09-27
Liaoning	Dandong, 2021-09-27
Liaoning	Fuxin, 2021-09-27
Liaoning	Shenyang, 2021-09-27
Liaoning	Panjin, 2021-09-30
Inner Mongolia	Tongliao, Unknown
Inner Mongolia	Xilingol, Unknown
Shandong	Weihai, 2021-09-13
Shandong	Rizhao, 2021-09-17
Shandong	Taian, 2021-09-20
Shandong	Jining, 2021-09-27
Shandong	Jinan, 2021-09-28
Tianjin	Tianjin, 2021-09-30
Xizang	Shigatse, 2021-09-08
Zhejiang	Shaoxing, 2021-09-24
Zhejiang	Jiaxing, 2021-09-26
October, 2021 (N=38)	
Beijing	Beijing, 2021-10-02
Chongqing	Chongqing, 2021-10-05
Fujian	Zhangzhou, 2021-10-12
Guangdong	Maoming, 2021-10-09
Guangdong	Heyuan, 2021-10-27
Guangxi	Guigang, 2021-10-13
Heilongjiang	Heihe, 2021-10-05
Henan	Hebi, 2021-10-13
Henan	Jiaozuo, 2021-10-13
Henan	Zhengzhou, 2021-10-14
Henan	Xuchang, Unknown
Hubei	Jingmen, 2021-10-08
Hubei	Jingzhou, 2021-10-22
Hubei	Yichang, 2021-10-25
Jiangsu	Yancheng, 2021-10-18
Jiangxi	Jiujiang, 2021-10-21

October, 2021 (N=38)	
Jiangxi	Xinyu, 2021-10-21
Jiangxi	Shangrao, 2021-10-27
Liaoning	Benxi, 2021-10-08
Liaoning	Liaoyang, 2021-10-08
Inner Mongolia	Hulunbuir, 2021-10-06
Inner Mongolia	Chifeng, 2021-10-27
Qinghai	Haibei, 2021-10-09
Qinghai	Haidong, 2021-10-09
Qinghai	Hainan, 2021-10-09
Qinghai	Haixi, 2021-10-09
Qinghai	Huangnan, 2021-10-09
Qinghai	Xining, 2021-10-09
Shandong	Linyi, 2021-10-18
Shanxi	Jincheng, 2021-10-15
Shanxi	Zhangzhi, Unknown
Shanxi	Taiyuan, Unknown
Shanxi	Xinzhou, Unknown
Shanxi	Yangquan, Unknown
Zhejiang	Wenzhou, 2021-10-10
Zhejiang	Zhoushan, 2021-10-15
Zhejiang	Quzhou, 2021-10-16
Zhejiang	Jinhua, 2021-10-27
November, 2021 (N=13)	
Anhui	Bozhou, 2021-11-12
Inner Mongolia	Hinggan, 2021-11-03
Ningxia	Guyuan, 2021-11-05
Ningxia	Yinchuan, 2021-11-19
Shaanxi	Weinan, 2021-11-17
Shaanxi	Baoji, 2021-11-22
Shaanxi	Hanzhong, 2021-11-25
Shaanxi	Yulin, 2021-11-26
Shaanxi	Tongchuan, 2021-11-30
Shaanxi	Xian, 2021-11-30
Shaanxi	Xianyang, 2021-11-30
Sichuan	Yaan, 2021-11-17
Zhejiang	Lishui, 2021-11-19
December, 2021 (N=3)	
Shaanxi	Yanan, 2021-12-02
Shaanxi	Ankang, 2021-12-07
Yunnan	Honghe, 2021-12-09

Table A4: Variable Definition

This table provides the definitions of variables used in this study.

Variables	Definition	Source	Unit
Panel A: Plant-level Data			
ATIR	Production-related TIR	Alt Data Technology	Kelvin
$\Delta ATIR$	Year-on-year changes in production-related TIR	Alt Data Technology	%
Temperature	Monthly average temperature	China Meteorological Data Network	°C
Humidity	Monthly average relative humidity	China Meteorological Data Network	%RM
Precipitation	Monthly precipitation	China Meteorological Data Network	mm
Sunshine	Monthly sunshine duration	China Meteorological Data Network	hour
Covid19	The number of new COVID-19 infections	WIND	decimal
Other	Equals one for untreated plants whose firms had another plant treated in the prior month; zero otherwise.	Alt Data Technology	dummy
Neighbor	One for plants in untreated cities bordering a city treated in the prior month; zero otherwise.	WIND	dummy
Panel A: City-level Data			
GDPGrowth	GDP growth	China Urban Statistical Yearbook	decimal
PercapitaGDP	GDP per capita	China Urban Statistical Yearbook	decimal
SecondaryShare	The secondary-industry value-added share	China Urban Statistical Yearbook	decimal
Energy	Coal consumption scaled by GDP	China Urban Statistical Yearbook	decimal
Population	The natural logarithm of total population scaled by the total area for city	China Urban Statistical Yearbook	decimal
UrbanLands	The area of build-up lands to the total area for city	China Urban Statistical Yearbook	%
TFP	Total factor productivity	China Urban Statistical Yearbook	decimal
Covid19^{Annual}	Annual number of COVID-19 infections	WIND	decimal
Panel C: Trading Data			
AR^{Ind}	Industry-adjusted abnormal return estimated using the stock return less the equal-weighted return of other firms in the same industry	CSMAR	%
CAR^{Ind}	Cumulative industry-adjusted abnormal return	CSMAR	%
CER	Cumulative excess returns	CSMAR	%
CAR^{CAPM}	Cumulative abnormal returns estimated with respect to CAPM using a 252-day window with a minimum observation requirement of 150 days	CSMAR	%
CAR^{FF3}	Cumulative abnormal returns estimated with respect to the Fama-French three-factor model using a 252-day window with a minimum observation requirement of 150 days	CSMAR	%
MKT	The CSI All-A-share Index return	WIND	%
TED	The spread between the three-month Shanghai Interbank Offered Rate and the three-month treasury bill yield	WIND	%
CVIX	The China implied volatility index constructed from Shanghai Stock Exchange 50 ETF option prices	WIND	decimal
Panel D: Accounting Data			
Invest	Capital expenditure scaled by lagged total asset	CSMAR	%
TobinQ	The sum of the market value of equity and book value of total liabilities, divided by the book value of total assets	CSMAR	decimal
CashFlow	Cash flow from operating activities scaled by lagged total assets	CSMAR	decimal
LogAsset	The natural logarithm of total assets	CSMAR	decimal
Leverage	Total liabilities divided by total assets	CSMAR	decimal
ROA	Net profit scaled by total assets	CSMAR	%
ΔSales	Year-over-year changes in sales	CSMAR	%
ΔEmployment	Year-over-year changes in employment	CSMAR	%
ΔR&D	Year-over-year changes in R&D expenditure	CSMAR	%
ΔTFP	Total factor productivity	CSMAR	%
ΔLongDebt	The change in total debt divided by book value	CSMAR	%
INV	The change in gross fixed assets	CSMAR	%
IA	The change in total assets	CSMAR	%
IK	The change in gross fixed assets plus the decrease in accumulated depreciation, scaled by the average of beginning and end net fixed assets	CSMAR	%

Table A5: Summary Statistics: Stacked Sample

This table reports summary statistics of variables used in the stacked regressions. Panel A provides summary statistics at the plant-month level of variables and Panel B provides summary statistics at the firm-day level of variables. Variables in Panel A include production-related thermal infrared radiation (ATIR in Kelvin and its year-over-year change Δ ATIR, in %), temperature (*Temperature*, in °C), relative humidity (*Humidity*, in %RH), precipitation (*Precipitation*, in mm), sunshine duration (*Sunshine*, in hours), and the number of new COVID-19 infections (*Covid*19). Variables in Panel B include cumulative excess returns (CER, in %), cumulative abnormal returns relative to the CAPM (CAR^{CAPM}, in %) and the Fama–French three-factor model (CAR^{FF3}, in %), the CSI All A-Share Index return (MKT), the spread between the three-month Shanghai Interbank Offered Rate (SHIBOR) and the three-month treasury bill yield (TED), and the implied volatility index from options on the SSE50 ETF ($CVIX$). Variables in Panel C include capital expenditure scaled by lagged total assets ($CAPX$), Tobin's Q ($TobinQ$), cash flow from operating activities scaled by lagged total assets ($CashFlow$), log total assets ($LogAsset$), leverage ($Leverage$), return on assets (ROA), and year-over-year changes in sales ($\Delta Sales$), employment ($\Delta Employment$), R&D expenditure ($\Delta R\&D$), total factor productivity (ΔTFP), and annual change in total debt scaled by lagged book value ($\Delta LongDebt$).

Variable	N	Mean	STD	P10	P25	Median	P75	P90
Panel A: Environmental variables								
$ATIR$	480065	1.44	1.58	-0.34	0.30	1.20	2.39	3.63
$\Delta ATIR$	480065	37.42	243.39	-81.50	-27.76	6.74	54.58	164.27
Temperature	466147	17.22	9.11	4.60	10.90	19.10	25.50	27.40
Humidity	466147	70.07	12.15	51.00	63.00	73.00	79.00	83.00
Precipitation	466147	100.21	97.09	7.30	27.30	67.50	149.00	236.00
Sunshine	466147	162.27	51.89	97.10	129.70	159.90	193.80	226.10
Covid19	473134	9.38	44.42	0.00	0.00	0.00	2.00	17.00
Panel B: Stock market variables								
CER	4963392	12.17	31.82	-20.30	-7.22	5.37	25.33	54.62
CAR^{CAPM}	4963392	8.52	38.85	-35.22	-12.74	4.51	27.86	58.98
CAR^{FF3}	4963392	-1.20	33.99	-41.48	-18.30	-1.62	14.34	39.46
MKT	4963392	-0.01	1.04	-1.33	-0.56	0.05	0.65	1.24
TED	4963392	0.46	0.18	0.19	0.29	0.50	0.61	0.66
CVIX	4963392	20.15	1.97	17.89	18.81	19.84	21.15	23.02

Table A6: Alternative Standard Error Clustering

This table reports stacked difference-in-differences estimates of the effect of cities' adoption of mandatory power rationing policies on plant production under alternative standard error clustering schemes. The stacked DID is estimated over an event window of [-4,3] months around the policy announcement month. The dependent variable, $\Delta ATIR$, is the year-over-year change in production-related thermal infrared radiation ($ATIR$). Cohorts are defined by the month of policy issuance. $Treat$ equals one for a plant located in a treated city for a given cohort, and $Post$ equals one for months on and after the policy announcement. Control variables include temperature, relative humidity, precipitation, sunshine duration, and new COVID-19 cases. All specifications include Plant×Cohort and Year-month×Cohort fixed effects. Column (3) adds Year-month×Industry fixed effects, Column (4) adds Year-month×Province fixed effects, and (5) include both sets of fixed effects. t-statistics based on standard errors clustered at the plant, firm, and industry levels are reported in parentheses, curly braces, and square brackets, respectively. The sample period is 2020/9–2022/9. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent: $\Delta ATIR$	(1)	(2)	(3)	(4)	(5)
$Treat \times Post$	-19.090	-15.420	-15.600	-16.740	-17.160
	$(-6.43)^{***}$	$(-5.09)^{***}$	$(-5.07)^{***}$	$(-4.52)^{***}$	$(-4.57)^{***}$
	$\{-4.65\}^{***}$	$\{-3.75\}^{***}$	$\{-3.77\}^{***}$	$\{-3.49\}^{***}$	$\{-3.53\}^{***}$
	$[-4.03]^{***}$	$[-3.11]^{***}$	$[-3.32]^{***}$	$[-3.30]^{***}$	$[-3.38]^{***}$
Control	NO	YES	YES	YES	YES
Plant×Cohort FE	YES	YES	YES	YES	YES
Year-month×Cohort FE	YES	YES	YES	YES	YES
Year-month×Industry FE	NO	NO	YES	NO	YES
Year-month×Province FE	NO	NO	NO	YES	YES
Observations	474564	454846	454845	454846	454845
R-square	0.19	0.19	0.20	0.23	0.24

Table A7: Firms' Response: Spillover Effects by Repetitive Explosure

This table reports difference-in-differences estimates of the spillover effect of cities' adoption of mandatory power rationing policies on neighboring but unaffected plants, allowing the spillover effect to vary with the frequency of exposure to neighboring cities' policy adoption. The stacked DID is estimated over an event window of [-4,3] months around the policy announcement month. The dependent variable, $\Delta ATIR$, is the year-over-year change in production-related thermal infrared radiation ($ATIR$). Cohorts are defined by the month of policy issuance. $Treat$ equals one for a plant located in a treated city for a given cohort, and $Post$ equals one for months on and after the policy announcement. $Neighbor$ equals one if the plant's city shares a border with a city that implemented the mandatory policy in the previous month, while the plant's own city remained untreated in that month. $First\ time$ and $Second\ time$ ($and\ more$) equal one if the plant's city shares a border with a city that implemented the mandatory policy in the previous month for the first and second time (and more), respectively, while the plant's own city remained untreated in that month. Control variables include temperature, relative humidity, precipitation, sunshine duration, and new COVID-19 cases. All specifications include Plant×Cohort, Year-month×Cohort, Year-month×Industry, and Year-month×Province fixed effects. t-statistics based on standard errors clustered at the plant-cohort level are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent: $\Delta ATIR$	(1)	(2)	(3)	(4)	(5)
$Treat \times Post$	-13.33***	-10.77***	-10.53***	-13.93***	-14.00***
	(-4.62)	(-3.64)	(-3.53)	(-4.39)	(-4.37)
$Neighbor \times First\ time$	27.89***	31.08***	32.23***	18.88***	18.66***
	(13.59)	(15.05)	(15.43)	(6.94)	(6.76)
$Neighbor \times Second\ time\ (and\ more)$	12.99***	11.17***	12.95***	1.45	1.97
	(5.21)	(4.35)	(4.99)	(0.46)	(0.61)
Temperature		2.02***	1.78***	7.94***	7.77***
		(7.47)	(6.48)	(9.29)	(8.95)
Humidity		1.64***	1.64***	2.50***	2.48***
		(15.36)	(15.23)	(12.30)	(12.12)
Precipitation		0.05***	0.05***	0.03***	0.02**
		(7.10)	(6.82)	(2.88)	(2.53)
Sunshine		0.14***	0.15***	0.03	0.03
		(7.33)	(7.41)	(0.96)	(1.03)
Covid19		-0.00	0.00	-0.01	-0.01
		(-0.21)	(0.16)	(-0.99)	(-0.36)
Plant×Cohort FE	YES	YES	YES	YES	YES
Year-month×Cohort FE	YES	YES	YES	YES	YES
Year-month×Industry FE	NO	NO	YES	NO	YES
Year-month×Province FE	NO	NO	NO	YES	YES
Observations	421307	401589	401588	401589	401588
R-square	0.19	0.19	0.20	0.22	0.23

Table A8: Impacts on Firm Investment excluding the Event Year (2021)

This table reports difference-in-differences estimates of the effect of mandatory power-rationing policies on firms' investment, excluding the event year 2021. The dependent variable, $CAPX$, is annual capital expenditures scaled by lagged total assets. $Treat$ equals one for a firm with at least one plant located in a treated city, and $Post$ equals one for years after 2021. Control variables include Tobin's Q, cash flow from operations scaled by lagged total assets, log total assets, leverage, and return on assets. Columns (1)–(2) include Firm and Year fixed effects, Column (3) adds Year×Industry fixed effects, Column (4) adds Year×Province fixed effects and Column (5) adds both Year×Industry and Year×Province fixed effects. t-statistics based on standard errors clustered by firm are reported in parentheses. The sample period is 2017–2024 excluding 2021. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent: CAPX	(1)	(2)	(3)	(4)	(5)
$Treat \times Post$	-0.32	-0.52**	-0.50**	-0.54**	-0.54**
	(-1.18)	(-2.00)	(-1.98)	(-2.04)	(-2.08)
TobinQ		0.60***	0.57***	0.60***	0.58***
		(9.20)	(8.74)	(9.68)	(9.17)
CashFlow		1.68*	1.78*	1.87*	1.92*
		(1.66)	(1.73)	(1.86)	(1.89)
LogAsset		-1.27***	-1.27***	-1.24***	-1.25***
		(-5.13)	(-5.04)	(-5.22)	(-5.15)
Leverage		-1.63**	-1.74**	-1.63**	-1.76**
		(-2.16)	(-2.25)	(-2.22)	(-2.35)
ROA		0.11***	0.11***	0.11***	0.11***
		(10.38)	(9.78)	(10.49)	(9.86)
Firm FE	YES	YES	YES	YES	YES
Year FE	YES	YES	NO	NO	NO
Year×Industry FE	NO	NO	YES	NO	YES
Year×Province FE	NO	NO	NO	YES	YES
Observations	12517	12318	12318	12318	12318
R-square	0.53	0.57	0.58	0.58	0.59

Table A9: Alternative Investment Measures

This table reports difference-in-differences estimates of the effect of mandatory power-rationing policies on firms' investment. The dependent variables include the year-over-year change in gross fixed assets (INV), the year-over-year change in total assets (IA), and the year-over-year change in gross fixed assets plus the decrease in accumulated depreciation, scaled by the average of beginning- and end-of-year net fixed assets (IK). $Treat$ equals one for a firm with at least one plant located in a treated city, and $Post$ equals one for 2021 and subsequent years. Control variables include Tobin's Q, cash flow from operations scaled by lagged total assets, log total assets, leverage, and return on assets. Odd-numbered columns report specifications with Firm and Year fixed effects but no controls. Even-numbered columns add the full set of controls and include Firm, Year×Industry, and Year×Province fixed effects. t-statistics based on standard errors clustered by firm are reported in parentheses. The sample period is 2017–2024. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent:	INV		IA		IK	
	(1)	(2)	(3)	(4)	(5)	(6)
$Treat \times Post$	-0.62**	-0.62**	-1.94**	-2.67***	-2.39**	-2.17*
	(-2.34)	(-2.28)	(-2.03)	(-2.83)	(-2.11)	(-1.89)
TobinQ		0.36***		3.06***		1.73***
		(5.08)		(10.87)		(5.67)
CashFlow		-1.31		25.95***		-5.01
		(-1.08)		(5.25)		(-1.02)
LogAsset		-1.21***		-19.81***		-2.84***
		(-4.75)		(-16.30)		(-2.63)
Leverage		0.43		-7.02**		-3.92
		(0.50)		(-2.30)		(-1.07)
ROA		0.09***		0.51***		0.47***
		(6.91)		(10.72)		(7.47)
Firm FE	YES	YES	YES	YES	YES	YES
Year FE	YES	NO	YES	NO	YES	NO
Year×Industry FE	NO	YES	NO	YES	NO	YES
Year×Province FE	NO	YES	NO	YES	NO	YES
Observations	14450	14250	14450	14250	14433	14233
R-square	0.26	0.30	0.28	0.44	0.23	0.28